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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

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The Engravings consist of 1. Dissections of Plants, illustrating the Growth of the Bud, by Mrs. A. Ibbetson. 2 Crystals of Allanite, by Dr. T. Thompson. 3. Instruments for measuring the Velocity of Rivers, by Mr. Regnier. 4. Method of covering a Roof with Flagstones, by R. L. Edgeworth, Esq. F. R. S. M. R. I. A. 5. A Ship's ordinary Boats converted into Lifeboats, by the Rev. J. Bremner. 6. A Barometer with an adjusting Scale, and 7. An Airpump for producing a perfect Vacuum, by a Correspondent. 8. Section of a Grapehouse, by T. A. Knight, Esq. F. R. L. and H. SS. 9. Figures to illustrate the Formation of the Leaves of Firs, and their Fructification, and the Motion of the Flower of the Barberry; delineated by Mrs. Agnes Ibbetson. 10. A Diagram illustrating the Radiation of Cold, by Marshall Hall, Esq. 11. Diagrams illustrative of the Motion of Rockets, by W. Moore, Esq. 12. A new Thrashing Machine, invented by H. P. Lee, Esq. 13. A Screw adjusting Plough, by Mr. Thomas Balls. 14. An improved Implement for extirpating Docks and Thistles, by Mr. J. Baker. 15. A pair of expanding Harrows, by Mr. Wm. Jeffery. 16. Mr. J. Davis's Fire-escape. 17. Mr. W. Moult's Filtering Apparatus. 18. Mr. B. Smith's Method of Relieving a Horfe, that has fallen in the Shafts of a loaded Cart. 19. Mr. J. Taylor's Extractor of foul Air from Mines, &c.

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SUPPLEMENT TO VOL. XXIX.

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MAY, 1811.

ARTICLE I.

On the Interior of Plants. Letter II. *By Mrs.* AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

I Shall now give a regular history of buds, and their manner of growing, as it has been hinted to me, that the sketch I gave in my last was not sufficiently explanatory and ample, considering the importance of the subject to botany, its novelty, and how little the real formation of the interior of plants is understood. It is certain, that the diligence of gardeners has far exceeded the labour of physiologists in this respect, and established first from accident, and then from practical experience, many rules, which should have been suggested and taught by the philosophers of botany; but I believe the scientific part of this science seldom travels as fast as the practical, and that it is usually left for the former to account for the reason why the process is good or bad, after it has been thoroughly established. But this may not always be the case; when once we have a thorough knowledge of the "interior formation of plants", the scientific may in its turn precede, and enforce rules for gardening.

There are three sorts of buds; flower buds, leaf and flower buds, and leaf buds only. The leaf bud I shall pass over for the present, as it differs so essentially from the rest, while the other two are so closely approximated to each other, and the alteration so trifling, that I shall consider them as one, and show at the end of this letter how they differ. Putting therefore the former distinction out of the question, we will establish another difference of buds, to make their history the more intelligible; dividing them into four parts.

The difference of buds.

1st, The buds of trees, shrubs, and some shrubs that are perennial; which plants have the line of life running into every part next the pith, and equally shooting; the bud on that line, whether in the middle stem, the branch, or the twig; and forming the bud by a knot, as soon as the plant has done seeding.

2d, The root buds, or those the buds of which reshoot from the root each year, as in all annuals or herbaceous plants, where the stem dies down.

3d, Palmate buds, that is, buds of palms, grasses, arums, &c., which shoot their buds just before flowering, and give therefore (preceding that time) no proof of possessing any buds, having no principle or middle stem.

4th, The buds which grow in bulbous roots, which shoot up when near the time of flowering, exactly like the last, except that they have a stem.

Formation of the buds of trees, &c.

To return to the explanation of the first species: I have mentioned, that in all trees and plants of this kind, the line of life invariably follows one course in every part of the plant; the difficulties buds have to encounter are therefore exactly proportioned to the situation in which they are found, they have hardly any wood to pass in fresh twigs; in the shoots of the preceding years, they have more; and in older branches still a larger proportion of wood to travel through; but in the trunk of the tree it becomes a very beautiful and amusing spectacle to behold the ease with which nature has arranged all for the perfect accomplishment of her work. Trees and shrubs prefer shooting their buds just opposite the leaf, that it may protect and support it in its cradle. In most trees the buds begin

begin to shoot as soon as the seed is perfected; it then forms a knot ou the line of life, breaking the two ends nearest the wood, each of these ends generally becomes a separate bud, and generates albumen all round it, while the old wood (as I have before shown) forms a vaulted passage for the buds to travel on to their different cradles in the bark.

The bud itself consists of the knot; a little albumen; two or three leaves, which may well be denominated cotyledons, as they are literally what cotyledons are in the seed, unformed leaves, covering the new and tender shoot, themselves distorted and hurt by confinement, and, if long retained by bad weather in the bud, the seminal leaves will increase as in the seed. Over these is a scale; and this is all I have ever been able to find in the bud of a tree, or shrub, before it arrives at its intended destination; afterward it gets leaves from the bark, and more scales from the rind to protect it from the cold.

The second sort of bud is that found in herbaceous and annual plants, as all sorts of culinary vegetables, &c. Here the line of life runs within the pith, and is not so easily traced; in many plants it crosses at every new shoot, and stops the pith; in others, it meanders within the pith in different branches, running up with each bud: but in all the knot for the bud is formed within the boundary of the pith: in some a number of buds follow in a string, in others, little bunches of buds shoot together. In the ranunculus, potentilla, tormentilla, &c., the contrivance is admirable; instead of generating a quantity of albumen to each bud, a large row of new wood is formed at the edge of the pith, in which all the buds are inflated, and then they can hardly be said to have any resting place or cradle: since they almost complete their form as they are pushing on to the exterior, and the wood being slight and made with divisions, which favour the exit of the buds, which are but formed in the root, till the growth of the branches transfers the line of life to a higher point: they then proceed from that part as in all other plants. 2d sort of bud.

The third are the palmate buds. This tribe of plants, from having no stem, naturally adopts another mode of growth; but it is simple and admirably accords with the others. 3d sort of bud.

others. The whole plant is formed of leaves, till flowering time, except the root, (which in every respect agrees with other plants, having the same divisions between the root and leaves, as I discovered between the root and stem in other plants, and which I called the grand obstruction): but when the time arrives for the plants to become prolific, there runs up from the root a slender thread, at first like pack-thread, within the axil of the leaf, and under the cuticle. This by degrees increases. It is composed of many little parcels of the germe of buds, in each of which is the knot of the line of life. As they rise, they enlarge, till, too much swelled for confinement, they burst forth into flowers; appearing to grow from the leaf: but they have in reality no connexion with it, except that of borrowing from it the spiral and nourishing vessels, which run into the corolla. I have traced the palm when just going to flower: for though from the want of air in the hothouse, it had never flowered, yet the buds were within. I found more in the root just ready to run up, and some half way; it is exactly made like the grasses, and like the arums, and every plant of that kind which has no stem; but in palms it is impossible to know all this without dissecting one. This order of plants flowers in various ways, but generally, at the top of the plant. The grasses carry up their diminutive buds through the flower stalk, one by one, with the line of life. In the arums it is very easy to trace the buds, but they increase more and in a quicker manner than in most of this class.

4th sort of bud. My fourth sort of bud is confined within a bulbous root. This explains itself; it begins at the root, and completes its form, more than is usual in any other plant, except those of the ranunculus tribe. Before it leaves the root all its parts are generally designed; they only enlarge, and the peduncle grows and raises them to view, having the bud at the top, hence the histories of the flowers to be found within the bulb. There are many however, that by no means resemble what they are to be, any more than many germes, when first concealed in the bud; because the parts are yet seldom proportioned to each other. The tulip and hyacinth are peculiarly perfect in the bulb.

I have now given a simple account, sufficient to make all

understand

understand the nature and progress of buds in all plants, except the cryptogamia and water-plants. These I am deeply studying; and I flatter myself it will not be long before I shall be able to complete my sketch in this respect.—I shall now endeavour to account for the cause of the succeeding of various means lately adopted in gardening, and show the reason that success attends such practices. All that Forsyth did to old trees, was to take from them the rotten part, which wholly checked the growth of the albumen, by soaking up the juices, which should have produced it; but the sap once returned to its usual office, forming the new wood, or albumen, gave an increased vigour to the line of life, which, when the rotten part was cut away, had room to shoot afresh, and by a quicker circulation of the sap renewed the vigour of every part of the plant. Scarcely has the year power to run its usual circle, before a tree so reanimated will begin to shoot at the very heart; the little pith to be found in so old a tree can hardly raise moisture enough for the innumerable buds, which form in every part, on the line of life; beginning at the dilapidated part, and soon communicating to all the rest. It is astonishing what good may be done, by thus now and then paring away a part, that appears to be beginning to decay: but then it must be cut with great care, and covered with the plaster ordered by Forsyth, which is excellent; and not left to contract the rot. The dried stump of a tree, or the remains of a broken limb, may in a very few years (by this management) be the source of new shoots, more than equal to those before lost; and though I believe with that philosophic observer Mr. Knight, that there is a term of life and vigour, beyond which a tree cannot pass: yet it is certain, that it is a very ancient time, and that almost all our trees die from careless inattention, and probably at half their proper age.

New practices in gardening.

Why Forsyth's method succeeds.

The preservative to the wound.

There is a strange idea universally spread among physiologists, arising (I must think) from our ignorance of the interior formation of plants, but universally received as a truth; that as wood grows old, it contracts in form, till all the passages of the fluids are stopped, and it remains in a kind of torpid state, till it dies. This idea appears to me to be so contradicted by all I have seen in nature, by my hourly

Sap in a tree never stagnates.

hourly study of the disorders in plants, and by the consideration of that sort of life which plants possess, that I am very anxious to show the fallacy of it. Wood once reduced to this state (it stands to reason) could never again recover; it could never throw out buds, it could never again be restored to a regular rising of the sap; for the vessels are so small, that, once choked, it would require a miracle indeed to open them; and yet it is well known, that a tree may be restored to almost pristine vigour, by a little cutting and care: and then, so far from being in a fixed state, every vessel of the wood must be moved out of its place, must bend in one way or other for the exit of the buds, the juices must be so plentiful (the sap in particular) as to form albumen to engender and accompany all those buds. Where then is its torpidity? It is true, that, the older wood grows, the more it is compressed; but it is the middle part between the vessels, that is reduced. A very simple proof may be given of this, by cutting the oldest piece of wood, that can be found in a living tree, and placing it in the fire: the quantity of sap, which runs from each separate vessel, bubbling and spouting out as soon as the heat acts, will quickly show how full of sap the oldest vessels are. But this very compression only more strongly proves the health and strength of the tree; it quickens the circulation of the juices by pressing the bastard pipes against the sap vessels, and thus gives increased vigour to the tree. I will be bound to say, that the passage of the vessels was never suspended in a plant, without causing the gangrene directly, and very soon death*. To prove, that I am not too hasty in this assertion, I will simply show the general manner of the death of trees, when they die naturally, and without accident. The first appearance of sickness is the hanging down of the branches and leaves: this is followed by a sweetness pervading all the different juices of the plant, attracting every species of insect, which soon cover and spoil the leaves with their filth: then little divisions of the wood (grown weaker than the rest) burst their vessels, and begin a sort of rot, which increases daily: the spiral wires, which attach the leaves to the stem, begin

Parts between the vessels compressed by health.

Death of a tree.

* This is so peculiarly the case, that almost all the disorders of trees arise from a stoppage of the circulation in different parts.

to

to grow brittle, and their cases to crack: the nourishing vessels round them decay, and the first wind, of course, takes off the leaves, and the next circumstance is generally the death of the line of life; which, when once it begins to be affected, soon bursts, turns black and dies: this spreads an increased sweetness over the plant, by the juices of the line; which, though often bitter, are luscious, and tempt the worm. From this time nothing can save the tree, though the bark and rind may still show some green; nay, I have known a fine day burst the leaf-buds, so little has the leaf to do with the plant, but they are soon gone; and the remainder sinks to torpidity and death. I have watched many trees from the first to the last in this way, and taken down their symptoms as they increased, by cutting branches, and thus judging of the progress of the evil; but if at the first appearance of it, care had been taken, the tree had been dug round, and a little dressing thrown with fresh earth; and if the disorder continued, and showed in any particular spot; had this been cut away, and managed as mentioned above, for to excite to fresh action is every thing in a plant, and air and light if possible let to it by cutting down any rubbish that impeded it, many trees might be saved, and much wood restoredto the country. Light is certainly the most necessary desideratum to plants. It is painful to see how trees will twist their branches in search of it, and perhaps be disappointed at last. A tree is therefore so far from dying by too much compression, that this is always a sign of health; as the spreading out and growing irregular in the branches is a sign of sickness. But I must dwell on this subject no longer.

To excite to action gives health to a plute.

I mentioned, that when a bud is protruded, a knot is formed on the line of life, which is broken, and the two ends form buds. All that is necessary therefore to form a bud is, to divide the line of life; this gardeners have learned to do, by cutting a gash in the place they mean to make prolific. They then not only divide the line, but they also separate the wood, which hastens the bud, as it has not to prepare the wood for its exit from the plant. This very much quickens the business; but then there is evident danger in the doing it. In the first place many buds may be destroyed in their

The breaking of the line of life,

way;

way: the finger should therefore be pressed all up the part, to be well assured that there is no branch on the point of shooting; the bud will be felt as soon as the bark and rind have made a socket or bed for its reception, which is done before the bud is half way on its journey: then a plaster should be prepared to cover the gash, without pressing it too close, but taking care to guard it well from the air, lest any should get in and cause a rot, more easily gained than cured. I have often found a bit of bladder, placed under the plaster, a better preservative than any thing else, if perfectly clean, and free from all grease.

forms buds.

Difference of the flower bud and mixed bud.

I promised at the conclusion of this letter to show the difference of the flower bud, and the leaf and flower bud, which is very trifling. They both come from the same place—the line of life—and both in the same manner. In the mixed bud, when arrived at its cradle, the rudiment of the flower stops while the leaf is weaving. The first has also some few leaves to complete, and many scales. The female or pistil of both was a rude mass containing the seeds, but now begins to take its proper form; while the males, all joined together, and proceeding from the wood, are completely fashioned. The scales in the mean time are growing to cover it thoroughly, and most buds have a quantity of their juices (that is the blood of the plant) lying between the several covers as a sort of resin, to protect it from the air and cold, of which it is now very susceptible. In the mixed bud, the leaves always are finished at the top, before the flower, even where the flower comes out first, to prevent the matter of the leaf, or mixing with the juices of the flower; a care which is peculiarly evinced throughout the whole formation of plants; and which it is wonderful to me physiologists have not observed, since their whole make is founded on this principle—the keeping all their juices perfectly separate. For this reason all the vegetable world is formed cylinder within cylinder; and, when there are holes, they are so contrived, that nothing but air can enter them. I shall soon exemplify this by delineations of the passage between the stem and the peduncle; which plainly show how strongly this principle is maintained in every instance, and how little therefore we can judge of the effect of the juices

Various juices of the tree always keep separate.

juices when we mix them all together. As to the leaf bud, I have in my last letter said, it is begun and finished in the bark. It is indeed a history in itself, and one of the most wonderful I know. There is so much pressing, rolling, and weaving, that I have constantly viewed it with fresh astonishment; for after being woven with all its parts loose and open, and all the ends hanging to it, like a piece of cloth fresh from the loom, it is folded anew, rolled in a particular manner, and laid in a liquid; then unrolled, and again folded in another manner, and pressed in the bud; and this is repeated several times, till by degrees losing all its ends, it is prepared for making the edges, which is the most curious part of all. I have already detailed this in my first letter, and shall not therefore repeat it, but only say, that the leaf buds of those plants, which have no stem, are formed within the bosom of the other leaves, joined to one end of the cuticle, not in the root. I have much to say on this subject, but it must be in another letter, and one which is restrained to leaves alone.

Formations of the leaf bud.

I must now say a few words on a subject I have long deferred touching upon, but which I have not the less studied; indeed I hardly know one that has lately engrossed so much of my thoughts; I mean, "whether there is, or is not, a circulation of sap through plants." After the most mature inquiry, the most exact research, I cannot discover the slightest reason for believing, that it takes place even in trees; on the contrary, the most potent arguments, drawn from the very nature of the vegetable tribe, militate against it. That there is a regular passage upwards for the rise of the sap, no one denies; but returning vessels from the head of the plant to the root I must think a fallacy; arising from that unfortunate comparison established between the animal and vegetable world, which was well enough in the first birth of both, but has been carried in my opinion to a false and blamable extent. Can any thing be more unlike animal life, than the shooting of the buds? This will, I think, more plainly appear, in drawing a comparison between the functions of both —in an animal constant motion is necessary to circulate the blood; its juices, formed in the body itself, from the different secretions I believe, (but I do not understand anatomy,)

Whether there are returning vessels for the sap or not?

Sap too much exhausted by its various formations.

tomy,) and constantly added to by food as solid as the flesh it creates, and as the blood it produces. There is no yearly extension of body, except a trifling increase at first, that could require the absorption of such a fund of matter as the whole blood of the animal to produce it: but in a tree each year creates almost a fifth of the weight, unless the tree is very large, in fruit, flowers, leaves, fresh branches, new radicles, and seeds. Whence then could the returning juices flow, exhausted as they must be? I have weighed the yearly increase in a small tree, and it far exceeded this calculation, Besides in an animal the blood is formed in it, whereas in a tree the sap is the juices of the earth. Nature would not therefore draw up more than is necessary for its various productions, merely to carry it down again. In animals the circulation, increased by exercise, occasions a constant dissipation of the several parts, which enter into its composition, and is therefore, I understand, productive of a thousand good consequences, without which the animal might become torpid and insensible, from the effects produced on the brain; but in a tree I see no end it can answer; nor could I ever find any returning vessels that would carry coloured juices the contrary way, though I have sought them in every part of the plant, As to the reason given, "that, if a deep piece of wood was cut out of a tree, a large portion of matter grew in the upper part of the wound, and none in the lower," it is easily to be accounted for. The moment a tree grows unhealthy, it gets full of these bunches; but such a cut must at once produce them. The first effect of such a dilapidation is to arrest the vigorous flow of the sap: much of this is therefore stopped, and often breaks some of the wood vessels: this forms little pools, which occasion the rot, while the other vessels, filled with air, are inflated and increased: in the mean time the line of life, which has been divided by the deep cut, shoots out many new germes, and forms new wood to engender them; and when you take off the lump so made, it is a mass of loose wood, of rotten albumen, and new shoots half alive, and half dead; while the under part, losing its sap by bleeding, which the other could not do, as the vessels could not discharge themselves backwards, is only dried up; and the buds, not being able to form for want of the sap, decay in their first

Wrongly accounted for.

first shooting, and of course the lower part of the wound is not at all increased in size. This appears to me perfectly the truth. I have dissected many pieces grown in this way, and they have always proved such as I have described above; it is therefore a proof which militates against the return of the sap vessels, rather than for it. The manner of the forming of the bud is also much against it, and I know not a single reason for it. Perfectly to understand the formation of the juices and to be able to separate their different parts and analyze them as they should be, is at present my most anxious wish. There are in all plants four different sorts of juices, which should with the greatest care be kept asunder; I have some curious details in this respect, though scarcely yet worthy to be laid before the public; but I hope my next experiments will be more exact, however I procured some very curious crystals of a peculiar shape, by means of subjecting the juices of the line of life to a very strong heat, and afterwards cooling it very gently; but I hope to procure better information; for I am but an indifferent chemist, though often dabbling in the science.

Four different sorts of juices in plants.

Though I have not in my last two letters taken notice of the foundation on which my studies rest, that for which I principally undertook to give the dissection of the interior of plants, that which appears to me to be the fundamental and systematic part of botany, "the natural affinity of plants to each other," yet I have not forgotten it. I continue to pursue it with the most exact care, noticing with attention each connexion; and strong lines indeed does the formation of buds draw between plant and plant, as I shall soon show; encouraging my hopes of finding something like a plan, on which a system may be discovered, without expanding into rules too diffuse to serve such a purpose. That the greatest, the most scientific botanist will ever be able to make one generally useful, and to supersede all artificial methods, I much doubt, when such a master as Jussieu has failed, but that one of more humble pretensions may be found; I am still most sanguine in my hopes, since the more I dissect, the more proofs of that system I find in nature.

Natural affinity of plants.

I am, Sir, your obliged humble servant,

AGNES IBBETSON.

Explanation of the Plate.

Plate I, fig. 1, A horizontal cutting of the red cabbage; to show the difference between the leaf bud and flower bud in annuals: the flower bud proceeding from the line of life, flowing within the pith, as at *a a*; the leaf bud generated within the rest of the leaves, as at *b b b*.

Fig. 2, The interior of the leaf bud, where many flowers grow in a bunch; the scales taken off. *c c c* new flower buds just generated on the line of life.

Fig. 3, A leaf bud, where no line of life is to be found.

Fig. 4, Mixed bud, of the apricot, in which the flower is completely separated from the leaf. *d*, the female; *e*, males; *f*, the line of life.

Fig. 5, The manner in which the leaf buds grow in the palmate buds. Each spiral turn makes a separate bud.

Fig. 6, A cutting of the potentilla; showing the circular line of albumen, in which the buds are formed. I have since found a vast number of annuals formed in this manner; that is, having the circular line of albumen within the line of the pith, in which the buds are very much formed.

Fig. 7, A sort of screws formed at the end of many new shoots, which are cradles for buds.

II.

Observations on the Hypothesis, which refers chemical Affinity to the electrical Energies of the Particles of Matter. By J. D. Maycock, *Esq. Communicated by the Author*.*

Mr. Davy's hypothesis of

Sect. I. FROM the consideration of an important, and interesting series of phenomena†, Mr. Davy has thrown

* This Essay, in very nearly its present form, was read to the Royal Medical Society of Edinburgh, on the 13th of March, in answer to a question proposed by the Society, and gained the gold medal. The question

† Phil. Trans. 1807: or Journ. vol. XVIII, p. 321, XIX, p. 37.

on

but a conjecture, that chemical affinity and electrical attraction are identical forces; and has very ingeniously endeavoured to point out the general application of the principle. This hypothesis, proposed by its author in the form of a question, has been too hastily admitted by some as an established doctrine; and speculations have been founded on it, which lead to the most extensive and unexpected conclusions.

chemical affinity.

The precipitate adoption of this hypothesis seems to have arisen principally from the imperfect and confused notions commonly prevailing in respect to electrical phenomena. I therefore deem it necessary, before proceeding to discuss the question proposed by Mr. Davy, to state such of the principal phenomena of electricity, as may unequivocally define in what sense we are to understand the terms electrical state, and electrical energy—terms which will often occur in the following pages; and which are to be esteemed synonimous; both being employed to denote a certain state of existence of bodies, in which peculiar phenomena are evinced.

Electrical state and electrical energy the same.

Bodies are said to be in different electrical states, or to have dissimilar electrical energies, when they attract each other: their electrical states or energies are said to be similar, when they repel each other. But we are to keep in mind, that these electrical attractions and repulsions are, in their effects, distinct from the attractions and repulsions, which bodies ordinarily evince. Two cork balls, suspended by silk lines, will indicate attraction or repulsion, accordingly as they may be in different or similar electrical states; and in either case, the motions arising in the balls will be in direct opposition to such as take place in consequence of the law of gravitation:—they will be diametrically contrary to those which appear in the action of the pendulum.

Different and similar states.

Actions produced by these.

question was: "Whether are the phenomena produced in the decomposition of bodies by galvanism capable of being explained on the usual principles of chemical attraction; or do they seem to establish the theory, that chemical phenomena depend entirely on the electrical energies of the particles of matter?"

When

When bodies are attracted, in consequence of a difference in their electrical states, and come into contact, or within a certain degree of proximity, each of them acquires a new electrical state, and the new electrical states are found to be similar: for between the bodies there is now exerted a repellent force. The operation, by which difference of electrical state is destroyed, is very frequently attended by the emission of light, a crackling noise, a peculiar smell, &c. The property, by which a body is brought to the same electrical state as that of surrounding bodies, is termed the conducting power, and is very various in different substances. Metals have the greatest, sealing wax and glass the least conducting force.

Vary in their force.

Bodies may, on account of their electrical states, attract or repel each other with various degrees of force; we therefore conclude, that various degrees of difference in the electrical states of attracting bodies exist; and that the electrical states of repellent bodies vary in different degrees from the electrical state of surrounding bodies.

The same operation produces opposite electrical effects.

When two dissimilar bodies are subjected to the same operation, the electrical state produced in the one is more or less different from that excited in the other. The same operation, indeed, not unfrequently appears to be the cause of diametrically opposite effects, when applied to dissimilar bodies. If a glass rod, and a rod of sealing wax, be excited by friction, and their electrical states be communicated to two insulated balls, which may be represented by the signs A and B: both these balls will exert an attractive force on the surrounding bodies; but A will more powerfully attract those bodies, which have been in contact with B; and *vice versa* B those, which have been in contact with A, than those which remain in their natural state. From this fact we learn, that the sealing wax and the glass differ less from surrounding bodies, in their electrical states, than they do, in this respect, from one another; and consequently, that the friction had produced opposite effects on them. In

Plus and minus electricity.

the theory of Dr. Franklin, an electric fluid is supposed to be accumulated in the glass, and dissipated in the sealing wax. Admitting the existence of an electric fluid, it would seem to follow, that, if it be accumulated in the glass,

glass, it must be dissipated in the sealing wax: but as far as my knowledge goes, it has never been determined, that it is in the glass, and not in the sealing wax, that the accumulation takes place. I mention so much of the theory of Dr. Franklin, not with an intention of entering into a defence or refutation of its principles, but rather to point out the origin of the terms positive and negative, plus and minus, as applied to the electrical states of bodies. I continue to employ these expressions, as it would be difficult at present to invent others freer than they are from hypothesis.

A body, to show signs of electricity, should be near another in a different state.

It is important to remark, that the phenomena, which have been enumerated, do not occur in every electrified body. That signs of electricity be evinced, it is essentially requisite, that the electrified body be in a state of proximity with other bodies electrified in a different manner.—I insulated one of Bennet's electrometers *, and, by a bent wire, connected the foot and the plate of the instrument. When I electrified this wire, a momentary and extremely trifling effect was produced on the gold leaves; but they returned to their natural position, although the whole apparatus was kept by one experiment in a state highly positive, by another in a state highly negative. The repulsion, when duly established, appeared to be equal between the gold leaves, and between the gold leaves and the tin foil. Had either the gold leaves or the tin foil been alone electrified, the effect, as is well known, would have been a separation of the gold leaves. From this experiment it also appears, that our Earth may *possibly* be very highly positive, or very highly negative, in relation to any other of the planets, without our instruments indicating such state; our Earth bearing the same relation to the bodies of the universe, that an insulated electrometer does to the various other bodies of our Earth.

The various bodies of our Earth naturally possess electricity.

That the various bodies of our Earth do naturally possess the quality we denominate electrical, is an opinion, not only probable from many general considerations, but one which admits of proof from the following

* I use the electrometer improved by Mr. Cuthbertson, but without the condenser.

statement

statement of fact. If two metallic balls, A and B, be placed near to each other, and to a small cork ball, suspended by silk, and positively or negatively electrified, which may be called C; and if A be connected with the Earth, and B be positively or negatively electrified in a greater degree than C; A and B will both attract C: but A will attract C with greater force after it has been in contact with B, than before; and the contact of C with A will augment the attraction between C and B. The effect is precisely the same as would have arisen, had A and B been both insulated, and differently electrified, and C connected with the Earth.

Circumstances may prevent the appearance of attraction.

It is also to be remarked, that, although two bodies, in different electrical states, be near to each other, it is very possible, that they may not indicate attraction. If, for instance, two fixed and insulated metallic balls be electrified, the one positively, the other negatively, and a small bit of cork, suspended by silk, be brought between them, the attraction of the cork for one metallic ball may be just sufficient to counteract its attraction for the other.

The preceding observations are unconnected with any hypothesis concerning the remote cause of electrical phenomena; and are, indeed, nothing more than a general statement of facts, established by experiment. Electricity is therefore a science, which has for its object phenomena produced in consequence of a difference in the electrical state of bodies, so situate, as to be within the sphere of action of each other; among which phenomena are certain modifications of the attractive and repulsive forces, that bodies ordinarily evince. Electrical state, or electrical energy, is the quality, to which such phenomena are referred. These conclusions are obviously deduced from the artificial electrical states; but, if they do not equally apply to the natural electrical states of bodies, I confess I have no idea of what is meant by this expression.

That difference of electrical state is the same with chemical affinity requires proof.

Sect. II. From a consideration of electrical phenomena in general, but more particularly of those which occur during decomposition by galvanism, Mr. Davy thinks it probable, that difference of electrical state is identical with chemical affinity, and an essential property of matter.

ter*. The important effects that such a principle, if adopted, would have on our chemical and physical reasonings, certainly require, that it should be established by the most satisfactory evidence. How far it is so will appear from the following observations and experiments.

Connexion of chemical action with gravitation,

Gravitation and chemical union are operations apparently dissimilar, and it is by no means surprising, that they should have been, for a considerable time, referred to the agency of different powers. At present we cannot but perceive, that gravitation is intimately connected with chemical action, by the various intermediate effects of the attraction of cohesion, of capillary attraction, and of hygrometric affinity. It has never, indeed, been demonstrated, that chemical affinity is identical with the attraction of gravitation; nor do I consider the opinion as admitting of such proof. Philosophy has in this instance done enough, and perhaps its utmost, in removing all objections to a general doctrine, which is recommended by strong and insuperable analogies. Now those who admit, that gravitation and chemical affinity depend on the same principle, cannot for a moment maintain, that chemical attraction and electrical attraction are identical: for it can be demonstrated, that the attraction of gravitation is not identical with electrical attraction.

which is not the same with electrical attraction,

for bodies in different electrical states do not gravitate differently to the centre;

In the first place, if gravitation depend on difference of electrical state, there must be some body at the centre of the Earth having an electrical state different from that of every body at the surface, since every body at the surface is apparently attracted to the centre. But as all bodies at the surface are supposed to have different electrical states, in respect to one another, there cannot exist the same degree of difference between the electrical state of any two dissimilar bodies, and that of the body at the centre, and consequently dissimilar bodies should be attracted to the centre with unequal degrees of force: a conclusion perfectly inconsistent with the principles established by Sir Isaac Newton's beautiful experiments with the pendulum,

* Philos. Trans. 1807, p 29: or Jour. vol. XIX, p. 50.

lum*, and on which is founded the whole system of natural philosophy.

and electrical attraction is proportional to the surface, gravitation to the mass.

In the second place the force of electrical attraction is, *cæteris paribus*, proportionate to the extent of the surfaces of the attracting bodies; gravitation, on the contrary, is proportionate to the quantity of matter reciprocally attracting, and has no dependance on the extent of surface. This essential difference between the two powers is peculiarly striking—A bit of gold leaf, of tin foil, or of sheet lead, will acquire a rapid motion through the air, when acted on by an excited electric, which would not sensibly affect an equal quantity of either of these metals in a globular form; and yet the gold leaf, the tin foil, or the sheet lead will have to overcome considerably more resistance in passing through the air, than will a globule of gold, of tin, or of lead. On the contrary a given quantity of gold, of tin, or of lead, will gravitate more rapidly, in the medium of our atmosphere, when in a globular form, than when beaten out into gold leaf, tin foil, or sheet lead: not that the force of attraction is diminished by the extension of the metals, but because in an extended form they have to overcome a greater degree of resistance from the elastic medium, through which they are to pass.

Thus, I conceive, it is demonstratively proved, that the attraction of gravitation is not identical with electrical attraction. Our views, therefore, of the phenomena of nature would not be rendered more simple by admitting, that chemical attraction and electrical attraction are identical; as this would draw a line of distinction between the principle of chemical attraction, and the principle of gravitation. Every one must determine for himself whether the hypothesis, proposed by Mr. Davy, be supported by arguments more plausible, than those, which can be adduced in favour of the identity of chemical attraction, and the attraction of gravitation.

Attraction considered as

Philosophers of the present day most commonly speak of attraction as an ultimate property of matter; not that they

* Newtoni Phil. Nat. Princip. Math. L. III, Prop. VI, Theor. VI.

conceive

conceive it to be really so, but that they are unwilling to add to the many vain speculations, which have been proposed to account for it. What indeed has been denominated electrical attraction, of which Mr. Davy considers chemical affinity to be a modification, is yet very generally referred to the agency of a subtile, and essentially fluid body. Repulsion is almost universally attributed to the action of such fluids. It is however to be remarked, that we have no direct evidence of the existence of these fluids, as they have never been the objects of sense. They are agents erected entirely by human ingenuity, for the purpose of explaining phenomena, which in the pride of speculation we are unwilling to admit are inexplicable. The whole of the modern doctrines, respecting light, caloric, and the electric fluid, are hypothetical, and allow only of such indirect evidence, as is derived from their capability of explaining the class of phenomena, on account of which they were assumed. That they do so to a certain extent cannot be questioned. It must, however, be admitted by every one who patiently investigates the subject, that there are phenomena, connected with the temperature and electrical state of bodies, which cannot satisfactorily be accounted for on the generally received opinions: and although there is, at present, no positive objection to the supposition, that light is a material substance; such may possibly arise in the progress of discovery. The hypothesis, therefore, which refers repulsion, or any modification of attraction, to subtile fluids, although it need not altogether be rejected, should be received with caution, and never made the basis of our general principles.

an ultimate property of matter.

In the present state of our knowledge, it would, perhaps, be most prudent to abstain from all speculations, concerning the cause of attraction and repulsion, and to consider them both as properties of matter, prevailing under different circumstances. It is at least certain, that we have as unquestionable experience of the particles of ponderable matter repelling, as of their attracting each other. Now it cannot be doubted, that attraction and repulsion are very much modified and affected, among other causes, by those which modify and affect the electrical state of bodies. Indeed

Attraction and repulsion.

Modified by causes affecting electricity.

such

such modifications of attraction and repulsion are among the most obvious phenomena of electricity, and those that first gave origin to this science. But after what has been said, it must appear impossible to consider difference of electrical state as identical with the principle of attraction. Neither do I think it could be seriously contended, that similarity of electrical state is identical with the principle of repulsion; as this would, at least, involve the opinion, that similarity of electrical state, whether positive, or negative, is identical with the cause of increased temperature; an opinion I, by no means, feel myself called upon to confute.

Supposition of minute differences in the electrical state of dissimilar particles.

Possibly, however, it was never meant, that difference of electrical state is identical with the principle of chemical attraction; but that there exist minute differences in the electrical states of the particles of dissimilar kinds of matter,—that such differences are not destroyed by contact,—and that, although they are not sufficient sensibly to affect the vibrations of the pendulum, they may yet so modify the principle of attraction, as to give rise to the phenomena which favour the idea of *elective* affinity. This is certainly the least objectionable form the hypothesis can assume. The supposition, however, that dissimilar bodies preserve different electrical states, is in opposition to analogy; since we invariably perceive a tendency in bodies to acquire similar electrical states, as far, at least, as our most delicate instruments inform us; and as this law holds to every measurable difference, it would surely be unphilosophical not to consider it as absolute, and without exception.

Particles might have different capacities for electricity.

Were we to adopt the theory of Dr. Franklin, it might appear to follow from many facts, that dissimilar bodies have different capacities for the electric fluid; but this would surely afford no stronger argument in favour of the opinion, that bodies exist in different electrical states, than might be drawn from their having different capacities for caloric, in support of an opinion, that they have naturally different temperatures. The hypothesis proposed by Mr. Davy cannot, therefore, be admitted, until it shall have been proved, that it is capable of explaining, in the most satisfactory manner, the phenomena on account of which it was assumed; and that these phenomena are inexplicable on

any

any known, and less exceptionable principle. I shall endeavour to determine how far Mr. Davy's proposition brings with it these recommendations; and would here observe, that the following arguments will equally apply, whether it be held, that the principle of chemical attraction is identical with difference of electrical state, or, that the principle of chemical attraction is modified by difference of electrical state.

Mr. Davy's speculation rests entirely on the correctness of his position, relative to the "changes and transitions by electricity." He states, not as an hypothesis, but as a general expression of fact, that "hidrogen, the alkaline substances, the metals, and certain metallic oxides, are attracted by negatively electrified metallic surfaces; and repelled by positively electrified metallic surfaces; and contrariwise, that oxigen and acid substances are attracted by positively electrified metallic surfaces, and repelled by negatively electrified metallic surfaces; and that these attractive and repulsive forces are sufficiently energetic, to destroy or suspend the usual operation of elective affinity*.

Certain substances said to be attracted by positive, others by negative electricity:

To determine, whether Mr. Davy's statement be correct, I selected one, from each of the classes of substances enumerated in the preceding paragraph: viz. boracic acid, barytes, and gold-leaf; and I found, that the metal and the earth were attracted as powerfully by an insulated metallic ball, electrified by glass, as by the same ball, electrified by sealing-wax. I also satisfied myself by experiment, that the acid is indifferently attracted by a positively or a negatively electrified metallic surface. It is impossible to operate on oxigen and hidrogen in their uncombined state, and thus to determine the truth of Mr. Davy's statement, as it relates to these substances. This circumstance is, however, the less to be regretted, as, when analogies are so forcible, and so obvious, as in the present instance, the conclusions, which are drawn from them, are received by the mind with a degree of certainty, little inferior to that, which is derived from demonstration.

but this not always the case.

Simple as these experiments may appear, they are decidedly adverse to Mr. Davy's hypothesis, the essential and

These facts

* Phil. Trans. 1807, p. 28: Journal, vol. XIX, p. 41.

indispensible

adverse to the hypothesis.

indispensible principle of which is, that particular substances have certain natural preferences and aversions to positively and to negatively electrified metallic surfaces; as they prove, that no such preferences and aversions are evident, while the substances acted on by the electrified surfaces remain in their natural electrical state. We cannot, indeed, by any means, infer from the result of these experiments, that bodies do not exist in different states of electricity; but we must feel satisfied, that an acid is not repelled by a negatively electrified metallic surface, or an earth or metal by a positively electrified metallic surface;—positions which form a very principal part of Mr. Davy's hypothesis.

Peculiarities of chemical action may be accounted for by the hypothesis; but equally on other grounds.

A supposition, that dissimilar bodies exist naturally in different electrical states, may possibly enable us to account for many of the peculiarities of chemical action; but I am inclined to think, that these peculiarities are explicable without the supposition, and that the philosophical labours of Berthollet have pointed out, with sufficient accuracy, the circumstances, which modify the principle of attraction, when excited on the minute particles of matter. The question, at present under consideration, did not, however, originate in the phenomena of chemical affinity, but was rather suggested to Mr. Davy, by the electro-motive property of bodies, and the truly valuable discoveries which have lately been effected by means of galvanism.

Bodies being in different electrical states after separation no proof, that they were so before.

If, after the contact and subsequent separation of two dissimilar bodies, they are found to be in different electrical states, in respect to one another, and to surrounding bodies, to what they were in before the contact, can we infer from this, that they must necessarily have existed in different electrical states, in respect to one another, previously to the experiment? Surely not. The electrical states, they now possess, have evidently been produced by contact, or subsequent separation. It may, indeed, be difficult to perceive the connexion between the effect and its cause; but this cannot warrant us in supposing, that bodies are in different electrical states, when our most delicate instruments assure us, that they are in similar electrical states. Was it, indeed, granted to us, that dissimilar bodies have naturally different electrical states, we could not, on this principle, consistently explain

explain their electro-motive property; since we set out with supposing, that they retain their particular electrical states, although contiguous with conductors.

Phenomena of galvanic decomposition,

Let us now turn our attention to the phenomena produced during decomposition by galvanism; and in the first place let us inquire, whether they can be accounted for on the principles proposed by Mr. Davy;—in the second place whether they cannot be accounted for on principles less objectionable.

Had it been proved, in the most unexceptionable manner, that the particles of dissimilar kinds of matter have different electrical states, and that the constituents of a compound retain their peculiar states while in composition, the rationale, Mr. Davy has offered of the phenomena of decomposition by galvanism, would yet be very far from being satisfactory.

Water taken as an example.

If we take water, *instar omnium*, and consider it as a compound of oxigen and hidrogen, and these substances as having, in respect to one another, the negative and positive states: it will by no means follow, that oxigen must be negative, or hidrogen positive, to every other body. In like manner, although the two wires of a galvanic battery be, respectively, the one positive, the other negative, yet the negative wire will be positive to a body more negative than itself, and the positive wire will be negative to a body more positive than itself. Now as far as we know from experience, a repellent force is not excited between electrified bodies, unless they be in precisely the same electrical state. If therefore the electrical state of oxigen and of hidrogen remain stationary, there will be only one point of positive electricity, at which the positive wire will repel hidrogen, and only one point of negative electricity, at which the negative wire will repel oxigen: and at all other points of excitement, the positive wire will attract hidrogen, and the negative wire will attract oxigen. Hence, as water is decomposed by the action of the two wires, when from the circumstances under which they are made to act, and from their effects on our instruments, we know, that they are in different degrees of positive and negative electricity, it becomes impossible to consider the repulsions, Mr. Davy speaks of, as essential to the decomposition, such repulsions being very rarely, if ever, exerted: but the

the whole decomposition must be referred to the unequal attractions of the two wires; for each wire will attract both oxigen and hidrogen, but with unequal degrees of force; and these attractions will be modified and counteracted by the attractions of the opposite wire. If, for example, the positive wire attract oxigen with a force equal to 20, and hidrogen with a force equal to 10; and *vice versa*, if the negative wire attract hidrogen with a force equal to 20, and oxigen with a force equal to 10; the efficient attraction between the positive wire and oxigen would be equal to 10, and that between the negative wire and hidrogen would be equal to 10, and consequently the power, tending to separate the oxigen and hidrogen, would be equal to 20. If, therefore, we keep in mind, that the effect of the two wires increases with the difference in their electrical state; we must, as might be shown by numerical calculation, suppose, that hidrogen is more positive than the positive wire, and oxigen more negative than the negative wire. On this supposition, and on no other, it will appear, that, as the excitement of the two wires is augmented, their action on water should be more powerful: for the nearer the electrical state of the positive wire comes to that of hidrogen, and the electrical state of the negative wire to that of oxigen, the stronger should be the efficient attraction of the positive wire for oxigen, and of the negative wire for hidrogen. The same reasoning must apply to the decomposition of all bodies, and the constituents of every body, decomposed by galvanism, must be considered as having electrical states more widely different, than are those of the positive and negative wires of the galvanic battery. But this is shown to be impossible by Sir Isaac Newton's experiment with the pendulum, and by every kind of experiment with the electrometer.

Why is not decomposition effected by a single wire?

Admitting, for a moment, that the attractive and repulsive forces of the minute particles of matter, and the action of galvanic wires on compound bodies, are really such as Mr. Davy supposes, it would, I think, be difficult to explain, why decomposition is never produced by a single wire, however powerful may be the battery, with which it is connected; why decomposition is never effected, either by common or galvanic electricity, except when *two* conductors,

tors, in different electricial states, are made to act on each other.

Experiment to show whether electricity be produced by contact or separation.

Sect. III. It is an established fact, that from the contact and separation of dissimilar and insulated metals there is produced such a change in the electrical state of each metal, that, after the separation the one is found to be positive, the other negative in relation to surrounding bodies; but it appeared to me, (not having in mind the experiments of Wilke and Œpinus,) a matter of some doubt, whether the alteration in electrical state is the effect of contact, or of separation. To determine this point, in place of the small plate which usually remains on my electrometer, I adapted a copper plate about 5 inches in diameter. It is evident, that when this apparatus is placed on a common table, the copper plate will be connected with the wire and gold leaves, but will in every other respect be perfectly insulated; and, consequently, that, whenever a state, different from that of surrounding bodies, is produced in the copper plate, it will be indicated by a divergence of the gold leaves.

The apparatus, above described, being so circumstanced, that the tin foil of the electrometer was connected with the Earth, while the copper plate, the wire, and the gold leaves were insulated, I brought, by means of an insulating handle, a zinc plate, also about 5 inches in diameter, into contact with the copper plate on the electrometer; but although they remained some time in contact; there was no visible divergence of the gold leaves. On separating the metals, the gold leaves immediately diverged; on again bringing them into contact, if the charge of the zinc plate had not been removed, the leaves returned to their natural position: on again separating the plates the divergence took place as before, and similar phenomena appeared, as often as the experiment was repeated. If the charge of the zinc plate had been removed after the separation, the succeeding contact did not reduce the gold leaves to their natural state; but left a slight divergence in them; and when the plates were again separated they diverged in a greater degree, than after the preceding separation. Thus, by repeating the experiment, and discharging the zinc plate after each separation, the divergence was considerably increased; not however beyond

yond certain limits, which apparently varied according to the state of the atmosphere as to moisture: and it is worthy of remark, that the manner in which the plates are separated materially effects the result of the separation. If one be slid along the other, neither will evince signs of electricity. The contact and separation of two copper plates produced no sensible effect on the gold leaves. From these, and the experiments of Wilke and Œpinus, I feel myself warranted in concluding, that the electrical states of dissimilar metals, and other dissimilar bodies, are not rendered different by the contact of these bodies with one another, but by their separation after contact*. I would also, from analogy, extend my conclusion to the minute particles of dissimilar kinds of matter; and would say, that when in contact, as in composition, they possess their natural, or, as I have endeavoured to show, in similar electrical states; but that on their separation, as in decomposition, they acquire electrical states different from what they had while in contact, and consequently different from their natural electrical states; and that from such change in the electrical states of the constituents of a compound, in consequence of separation, analogous to what takes place in respect to the voltaic plates, the one set of particles becomes relatively to the Earth and surrounding bodies positive, the other set negative.

Electrical states of bodies rendered different by their separation. This applicable to minute particles.

The experiments, to which I have just alluded, appear to me perfectly sufficient to point out the fallacy of the explanation, which is very generally received, of the excitement of the galvanic pile; the whole of which rests on the assumption, that dissimilar metals, while in contact, are in different electrical states, the one being relatively positive, the other negative; which has been shown to be perfectly untenable. The following also I consider as additional and weighty objections to the hypothesis. 1. The voltaic plates only act when applied to each other by extensive surfaces. In the present most improved galvanic troughs, the metals are connected together by comparatively few points, and the contrivance has not only rendered the apparatus more convenient for use, but also more powerful. 2. Volta's plates act

Supposition that the metals in the galvanic pile are in different states, erroneous.

* An account of the experiments of Wilke and of Œpinus will be found in Dr. Priestley's History of Electricity.

only

only when the polish of their surface is preserved; the copper and zinc plates of a galvanic battery are always very much tarnished. 3. The galvanic apparatus can only be excited by a decomposable fluid, and this fluid is always decomposed, when the apparatus is excited. From these considerations, I am inclined to conclude, that the principles, on which decomposable fluids act in producing their peculiar effects on the galvanic battery, have not yet been accurately determined.

It will be a general, and I think perfectly correct statement of the facts, relative to the decomposition of bodies by galvanism, to say, that hidrogen, the alkalis, the metals, and certain metallic oxides are, immediately after their separation by galvanism from oxigen, and from acids, found at the negative wire; and that oxigen and acids, after their separation from the first class of substances, appear at the positive wire. I trust, however, that the experiments and reasonings, which I have adduced, are sufficient to prove, that the particles of dissimilar kinds of matter do not exist in different electrical states while in composition, but that they acquire a difference of electrical state in the act of decomposition: this difference of electrical state is, therefore, not the cause, but the effect of decomposition.

The difference or state is owing to decomposition having taken place.

It yet remains to be determined, on what principle the opposite wires of a galvanic battery act, when their action occasions the separation of the constituents of compound bodies. To do this, I by no means conceive it necessary to enter into an investigation of the remote cause of electrical phenomena; on the contrary, I think the question may be decided by a reference to well known and undoubted facts.

How is decomposition effected by galvanism?

If two conducting bodies, in different electrical states, be brought near to each other, the difference will be destroyed; and if the difference between the electrical states of the conducting bodies be considerable, while the operation is going on by which it is removed, the conducting bodies will frequently be fused. This happens not only to bodies easily fused, but also to very refractory substances; as the alkalis, the earths, the metals. The fact divested of all hypothesis is, that the action of differently electrified conductors occasions a repulsive force to be exerted

By a repulsive force analogous to that of caloric.

erted between the particles of the conductors, and is, in this respect, precisely analogous to that power, which in the language of modern chemistry is denominated caloric.

Mode in which this is effected in the case of caloric,

It has been long known, that caloric, aided by the affinity of a substance for one of the elements of a compound, is sufficient to effect the decomposition of the compound: and this fact is particularly observable in the reduction of metallic oxides, by heating them with inflammables, or metals. By these means the French chemists have lately succeeded in their attempts to decompose the fixed alkalis, and have obtained, in an uncombined state, their constituent elements, which appear to be oxigen, and a metallic base. The rationale of these decompositions is sufficiently obvious. The repulsive force of caloric separates the constituent particles of the compound; at the same time, by diminishing the cohesion of the inflammable, or uncombined metal, it renders its attraction for oxigen efficient; and hence the separation of oxigen from the oxide, and its combination with the uncombined metal, or with the inflammable. The oxigen, entering into a new combination, is removed from the sphere of chemical action, and thus its reunion with the metal, from which it had been separated, is prevented.

and in that of galvanism.

The decompositions by galvanism will, I think, admit of explanation on similar principles. The action of the two wires of the galvanic battery occasions such a repulsion, at a certain number of points, as separates the constituents of the compound, which is made a part of the circuit, and which must possess a degree of conducting power. The separation of the particles of dissimilar kinds of matter, which had been in contact, produces different electrical states in them: the one set of particles is, consequently, attracted with greatest force by the positive wire, the other set of particles is attracted with greatest force by the negative wire; the separated particles are thus placed beyond the sphere of chemical action, and their reunion does not take place.

Having, then, considered at some length the question proposed by Mr. Davy, I am satisfied, that we cannot admit the hypothesis, which refers chemical phenomena to the electrical energies of the particles of matter. I am willing to allow, that it is highly ingenious, and that at first sight it has really

really the appearance of a simple generalization of facts: but I think it has been shown, that the assumption on which it rests is contrary to experiment and analogy; that the assumption is incapable of explaining the phenomena on account of which it was taken up; and that these phenomena can be explained on principles unconnected with any hypothesis, and which are the result of experiment and observation.

III.

Observations on the Igniting, or Wire-melting Power of the Voltaic Battery, as proportioned to the number of Plates employed; with an Account of some Experiments on this Subject, made in conjunction with Mr. JOHN CUTHBERTSON; *by Mr.* GEORGE JOHN SINGER, *Lecturer on Chemistry and Natural Philosophy. Communicated by Mr.* SINGER.

IN a lecture recently delivered at the Royal Institution, Dr. Davy detailed some experiments of the French Philosophers, made with the intention of ascertaining the proportion, in which water is decomposed by different Voltaic combinations, the *number* of plates being subjected to variation. After some observations on the probable source of inaccuracy in these experiments, he proceeded nearly as follows: "There is still another very interesting subject of "inquiry, *which has not yet been touched on*; I mean the "proportion the igniting power of the battery bears to the "number of plates employed." The Dr. then proceeded to exhibit some experiments on this subject; they were made with a new apparatus fitted up in troughs of Wedgwood ware; the size of the plates 11 inches, by $4\frac{1}{2}$ inches. The result of these experiments was very equivocal, *two* batteries ignited *four* times the length of wire ignited by *one* battery; but *six* batteries ignited little more than *twice* the length that *three* could ignite. Dr. Davy supposed, that the rate of ignition might vary in higher numbers, obeying a different law to that which obtains when a few plates

Inquiry concerning the ratio of the power of igniting wires to the number of plates by Mr. Davy.

Anomalies in the experiments.

plates only are employed. Every *practical* electrician would, however, I am convinced, refer the anomalous results of these experiments to some inaccuracy in the apparatus, or to a difference in the density of the fluid with which the batteries were charged; as the irregularities obtained were by far too considerable, to have been produced by a difference so trivial as that existing between the number of plates employed on this occasion. The authority of a philosopher, so highly and so justly celebrated as Dr. Davy, may give extensive and respectable circulation to even palpable errours; it is therefore the imperative duty of every genuine friend of science, to examine assertions flowing from such a source, and to give to the public any *facts* he may be acquainted with, that militate against, or contradict them.

Experiments on the subject made some years ago.

As early as the year 1804, direct experiments were made to ascertain the quantity of wire ignited by different numbers of plates. Of this I presume Dr. Davy was not aware, when he stated, that "the inquiry had not yet been touched on;" he may not have read the 18th volume of the Philosophical Magazine, or the 7th and 8th volume of Mr. Nicholson's Journal, or Cuthbertson's Practical Electricity, where an account of these experiments is published. In the 7th volume of this Journal, page 207, a series of experiments with large batteries of plates of 4 inches, and of 8 inches square, is detailed by Dr. Wilkinson: the results of these experiments prove, *that the power of ignition increases in direct proportion to the number of plates employed*; and this law of increase is uniform, whatever be the size of the plates. If a battery of any given size melt any determinate length of wire, *two* such batteries will melt *twice* the length, *three* such batteries will *treble* the effect, and by *four* it will be *quadrupled*, provided the acid with which they are charged is of equal strength.

The power of ignition in the direct ratio of the plates.

Other experiments gave similar results,

In the 8th volume of this Journal, pages 97 and 205, a very accurate series of experiments is given by Mr. Cuthbertson. By a variety of trials he proves, that double the quantity of plates burns twice the length of wire; and he points this out as a distinction between the action of common and Voltaic electricity, but concludes, that the difference arises from the imperfection of the Voltaic apparatus;

tus; as on one occasion he obtained a different result by employing very large plates arranged as a pile. In subsequent volumes of this Journal are several other papers on the same subject; but enough has been quoted to show, that the inquiry has not any claim to originality. The conclusions of the first experimenters are however at variance with those of Dr. Davy. Dr. Wilkinson and Mr. Cuthbertson suppose, that the igniting power of a battery composed of plates of any given size increases in direct proportion to the number of plates. Dr. Davy infers from his experiments, that, when a few plates are increased, the increase is as the square of the numbers; but in combinations of greater extent the effect does not increase so rapidly. The apparatus employed by Dr. Davy differs in structure from that of the earlier experimenters; and as this might occasion some slight difference in the results, I did not consider it justifiable to decide on the accuracy of either, without new trials. With the assistance of Mr. Cuthbertson the following experiments were made; their results furnish some useful practical information in addition to the ascertainment of the object, for which they were expressly instituted.

with one exception.

As the apparatus was not the same,

the experiments were repeated.

The acid mixture employed to charge the batteries was of the same strength in all the experiments, (being previously mixed in a large vessel for this purpose). It consisted of 10 gallons of water, 5 lbs. of strong nitrous acid, and half a lb. of muriatic acid. A mixture of this kind being the most effectual wire-melting charge. Ten batteries, each containing 10 pairs of four inch plates, fitted up in troughs of Wedgwood ware; and one battery, of 50 pairs of plates of the same size, fitted up in a wooden trough, with glass partitions, constituted the apparatus employed. The plates in the troughs of Wedgwood ware were new, but the glass partitioned battery had been frequently employed before.

The acid employed.

The apparatus.

Two of the Wedgwood batteries rendered nine inches of iron wire, $\frac{1}{120}$ of an inch diameter, faintly red hot, when the contact was first made. This effect continued but a very short time. When it had wholly ceased, an interval of one minute was suffered to elapse, and at the end of this time

Exp. 1.

time the contact was again made. Three inches of the same wire were now rendered red hot with the same appearance as the nine inches in the first experiment.

Exp. 2. Four Wedgwood batteries were next employed. At the first contact 18 inches of the same wire became slightly red hot; and the contact was preserved, till the effect of ignition entirely terminated. One minute was suffered to elapse, when, on removing the contact, 6 inches of wire were ignited in the same degree as in the preceding experiments.

Exp. 3. An interval of three minutes was suffered to pass without contact; at the end of this time, two batteries rendered six inches of the same wire red hot, and four batteries produced a similar effect on 12 inches.

Remarks. The uniform result of these experiments, in which the igniting power increases in the same proportion, however variable the action of the battery, renders it highly probable, that in Dr. Davy's experiments the batteries were accidentally charged with acid mixtures of variable strength, the increase in his first experiment being as the square of the numbers.

Exp. 4. To ascertain whether the ratio of increase continued the same when a larger combination is employed, ten batteries were charged with fresh acid, of the same strength. Five of these ignited at the first contact 18 inches of the same wire as that employed in the former experiments; and on repeating the experiment with ten batteries, an effect precisely similar was produced on 36 inches.

Exp. 5. A short interval was suffered to elapse, when five batteries ignited 15 inches of wire; and the same effect was produced on 30 inches by ten batteries.

Exp. 6. Platina wire, $\frac{1}{160}$th of an inch diameter, was taken. Ten batteries (in a diminished state of action) maintained a white heat in 5 inches of this wire. On repeating the experiment with five batteries a similar effect was produced on $2\frac{1}{2}$ inches of the same wire.

The power in direct proportion to the number. These experiments indicate, that the conclusions of Dr. Wilkinson and Mr. Cuthbertson are legitimate; and they prove also, that the *igniting power* not only *increases* in *exact proportion* to the *number* of plates; but that this ratio of increase

increase is *uniform*, however variable the action of the batteries may be.

The troughs of Wedgwood ware have the partitions, which form their cells, at a greater distance from each other, than that of the glass partitions in the wooden trough; they of course require more acid to excite a given quantity of plates; and it has been said, that this circumstance promotes the continuance of their action. The results of my experiments speak a different language. The continuance of the action is influenced much more by the nature and strength of the acid mixture; and I have not observed, that in any case the separation of the partitions to a greater distance than ¼ths of an inch is attended with any advantage in this respect.

The continuance of the action depends more on the acid than the size of the cells.

At the commencement of the preceding experiments, a glass partitioned battery, of 50 pairs of four-inch plates, was filled with the same acid mixture as that employed in the troughs of Wedgwood ware. Its action was greatly inferior, in consequence of the oxidated state of the plates from former operations; but the continuance of its action appeared precisely similar, and at the conclusion of the experiments the effects were so nearly alike, as to admit of no perceptible distinction. At the first contact 9 inches of wire were ignited, and by allowing an interval of five minutes a similar effect was produced by a second contact; a circumstance which proves, that the voltaic battery requires, like the electrical machine, time to produce its full effect. This fact, as indicated by the sensation produced on the animal organs by a series of 600 small plates, was noticed many years ago by Dr. Wilkinson.

Glass partitioned battery.

Batteries require time to produce their full effect.

The preceding are part only of a series of inquiries on this subject, which have long occupied my attention, and which I purpose to detail in future numbers of the Journal; anxious only, that in experimental science assertions be supported by accurate experiments; and that, in the progress of philosophical discovery, the merit of the first labourers be not forgotten amidst the achievements of their successors.

No. 3, Princes Street, Cavendish Square,
April the 13th, 1811.

IV.

On the different Forces with which Tubes, Bars, and Cylinders, adhere to a Magnet. In a Letter from Mr. E. LYDIATT.

To Mr. NICHOLSON.

SIR,

THROUGH the medium of your scientific Journal I am anxious to obtain information on some magnetic phenomena, which I have lately noticed to have taken place on applying different shaped conductors to connect the poles of a horseshoe magnet.

An iron tube connecting the poles of a horseshoe magnet adhered with great force.

In preparing the introductory course of lectures on the philosophy of the mechanic arts, which I have delivered this season at the Scientific Institution, I had occasion to make a few experiments on the magnetic property of iron and steel; in the course of which I happened to place a piece of iron tube in contact with the two poles of a horseshoe magnet composed of thin bars; and found to my surprise, when I attempted to remove it, that a considerably greater force was required, than that necessary to separate the conductor which belongs to the magnet, which, as usual, was a square piece of iron with a ring attached, and presenting a flat surface equal to the combined polar surfaces of the three bars composing the magnet. This striking singularity induced me to ascertain the relative force required, to overcome the different degree of attraction.

Its adhesion to that of a bar as 23 to 10.

I first applied the conductor belonging to the magnet, and, by suspending weights from the ring, found, that it separated with 5lb. I then supplied its place with the tube, which was a piece of gun-barrel about two inches in length, attached longitudinally from one pole to the other; and by passing a wire through it, and twisting the two extremities into a hook, I suspended the weights, and found that $11\frac{1}{2}$lb were requisite to separate it from the magnet. From this experiment it will be evident, that the relative degree of attractive force, exerted by the magnet on these two different conductors, is as 10 to 23. I repeated the experiment several times, and the results were invariably the same.

The

The line of contact of the tube, when the weights were first suspended, traversed the poles of the centre bar of the magnet only; but while they remained attached, I turned the tube till it stood in a diagonal direction with the extreme angles of the outside bars; but no difference of attraction was indicated, as it would not sustain more, or separate with less weight, than in its first position.

Change of position produced no alteration in the effect.

I then increased the width of the line of contact in the tube, with a file, to about $\frac{1}{15}$ of an inch, and found that it separated with nearly a pound less weight: I increased its width still more, and the attraction was proportionably less.

On increasing the contact by filing the surface, the attraction diminished:

This led me to suppose, that the extraordinary degree of attractive force, by which the tube was held to the magnet in the first instance, depended entirely on the minuteness of the line of contact; and of course, that a solid piece of sound iron of the same diameter, would be similarly attracted. To prove this however, I procured a solid cylinder of iron the same length and diameter as the tube; but upon applying the weights, I was surprised to find it separate with less than half what was necessary to displace the conductor belonging to the magnet.

but a solid cylinder adhered more feebly than a flat bar.

These hitherto unexplained, and probably unobserved, phenomena, are submitted for explanation to such of your philosophic readers, as may have paid more attention to this subject, than I have had an opportunity of doing; in hopes of being gratified with some communications, which will not fail to be interesting, while they elicit a more extensive inquiry into that mysterious and neglected principle of nature, magnetism.

Yours, &c.

London, April the 10*th*, 1811. E. LYDIATT.

V.

An Answer to Mr. MURRAY's *Observations on the Nature of Potassium and Sodium: by Mr.* JOHN DAVY.

To Mr. NICHOLSON,

SIR,

MR. Berthollet has estimated the proportion of water in common fused potash at 13·9 per cent; and Mr. Davy, from an

Quantity of water in potash.

an experiment on the action of silex on this hidrate, has concluded in his Bakerian lecture for 1809, that it contains, taking the potash formed by the combustion of potassium as a standard, about 16 or 17 per cent.

Mr. Davy's standard potash.

In the same lecture he has shown from the quantity of fused muriate, produced from a given weight of potassium in muriatic acid gas, that his standard potash has a much greater saturating power, than the hidrate of potash; that 100 of the former will neutralize the same quantity of acid as 120 of the latter.

Combined with boracic acid without evolution of water.

He has since ascertained, that, when potassium and powdered boracic acid glass are heated together in a tube of platina, both with and without red oxide of mercury, no water or inflammable gas is produced; and that the result is the same, when potash formed by the combustion of potassium is combined with boracic acid.

Common potash does not.

On the contrary, substituting the hidrate, or common fused potash, he has in one experiment actually collected about 15 per cent of water; and the loss of weight after the combination of the acid and alkali, in other similar experiments, indicated from 15 to 20 per cent.

Combustion of potassium in oximuriatic gas.

He has found too, that the only product of the combustion of potassium in oximuriatic gas is fused muriate of potash; that the same salt is formed; and oxigen gas evolved, without the least appearance of water, when potash from the combustion of potassium is used; and that water as well as oxigen is separated when hidrate of potash is employed.

Difference between the pure alkalis and their hidrates.

In addition to these circumstances, which are stated in Mr. Davy's last Bakerian lecture, a copy of which he has allowed me to peruse, there are physical properties also pointed out, distinguishing potash and soda from the hidrates; the former for instance require a much higher temperature for fusion than the latter, and possess greater hardness and apparently greater specific gravity.

Peroxides of potash and soda.

It is well known to those who have attended to the late progress of chemical discovery, that potash and soda are only to be procured by the rapid combustion of the alkaline metals, or by the after application of a red-heat; and that peroxides are formed when the combustion is feeble either in oxigen gas or common air. Messrs. Gay-Lussac and Thenard

nard first distinctly pointed out the nature of these peroxides, and described their properties. According to their statement, the peroxide of potassium contains three times the quantity of oxigen that exists in potash, and the peroxide of sodium half as much more as exists in soda*. These oxides have also been examined by Mr. Davy, and the general results of his experiments are conformable to those of the French chemists.

Trials of the saturating power of the alkalis.

Messrs. Gay-Lussac and Thenard, using the same test as Mr. Davy had before applied to their hypothesis, making comparative trials of the saturating powers of the alkalis formed from the metals and of the common hidrates, were convinced, that potassium and sodium are not hidrurets; and consequently they adopted Mr. Davy's opinion, that they are simple bodies†.

Mr. Murray controverts this opinion in his paper, published in the last number of your Journal: Finding that potash from the combustion of potassium, has much the same saturating power as hidrate of potash, he infers, that the metals of the fixed alkalis are compounds of unknown bases and hidrogen. As this gentleman does not describe the manner in which he formed his potash; there is every reason to conclude it must have been by combustion in the atmosphere, in which case, it would have been principally peroxide; and an equal weight of it ought to have less saturating power than an equal weight of common potash. Since, therefore, Mr. Murray's hypothesis appears to be unfounded, since it is contradicted by the ample statement of clear and decisive facts already made, I shall conclude without examining the speculations connected with it.

I am, Sir, with great respect,

Your humble servant,

London, March 14*th*, 1811. J. DAVY.

* Moniteur, July 5, 1810.

† At the end of this paper will be found a notice of these gentlemen's experiments; it is part of a Report of the Institute, published in the Moniteur already referred to.

Extract

Extract from the Moniteur of July the 5th, 1810, referred to in the preceding paper. Translated from the French by T. O. C.

Peroxides of the alkalis treated with acids.

"THESE oxides [the peroxides] present with some acid gasses phenomena worthy of attention. Messrs. Gay-Lussac and Thenard observed, that with carbonic acid gas the results were, an alkaline carbonate and an evolution of oxigen gas: that with sulphurous gas and oxide of potassium a sulphate and oxigen were obtained; and that with this gas and oxide of sodium the produce was only a great deal of sulphate and a little sulphuret: that not the slightest trace of moisture was given out in any case; and that the weight of the products obtained corresponded precisely to those of the oxide employed and the acid absorbed: Now as in the combustion of potassium and of sodium nothing is evolved, or no volatile product formed; we perceive, that, if these metals be hydrurets, it is a necessary consequence, that the sulphates and carbonates of potash and soda, and no doubt all the salts that have these alcalis for their base, contain as much water, as the hidrogen of these hidrurets can form by combining with oxigen, and that they retain it at a very high temperature; which is possible, but which nothing has hitherto proved. If it were so, a farther consequence would be, that potash and soda contain much more water, than Messrs. d'Arcet and Berthollet admit in them: for these alkalis would contain not only the water which is extricated on combining them with acids, but likewife that which the falt formed is capable of retaining. It was of fome use to determine directly the first of these two quantities of water; and this Messrs. Gay-Lussac and Thenard have done. For this purpose they converted into alkali, gradually and by means of humid air, feveral grammes of potassium and sodium, and saturated them with sulphuric acid diluted with water. On the other hand, having employed the same acid to saturate pure potash and soda that had been heated red hot; and having taken an account, in all the saturations, of the acid employed, as well as of the metal

Inferences.

Quantity of water in the alkalis examined.

metal or alkali employed also; it was easy for them to deduce the consequence they sought. Thus they found, that 100 parts of potash contain 20 of water, and that 100 of soda contain 24, supposing potassium and sodium to be simple substances. They have even verified this quantity of water with respect to soda, by treating over mercury in a curved jar a given quantity with a quantity, also given, of dry carbonic acid gas. The soda was placed on a small plate of platina, and gave out so much water the moment the temperature was raised, that this water trickled in abundance down the sides of the jar. We can even by these means, or by sulphurous acid gas, render the water sensible in 2 millig. [0·03 of a grain] of soda or of potash."

VI.

On the Nature of Oximuriatic Gas, in reply to Mr. MURRAY. *By Mr.* JOHN DAVY.

To Mr. NICHOLSON.

SIR,

MR. Murray, in his answer to the remarks which I ventured to make on his former paper, appears principally desirous of showing, that what my brother, Mr. Davy, has advanced as a theory respecting oximuriatic gas, is strictly an hypothesis. The conclusiveness therefore of Mr. Murray's answer depends on his success in proving Mr. Davy's views hypothetical; if he fails in this respect, he fails altogether, and the old hypothesis loses its asserted claims to attention.

Mr. Murray considers Mr. Davy's theory as hypothetical.

Mr. Murray first affirms, that Mr. Davy's theory is not a simple expression of facts, as I have represented it; that it is not a fact, that muriatic acid gas is a compound of oximuriatic gas and hidrogen, but an inference; and that the compositions of all the oximuriates are similar inferences. This I cannot admit. In the formation of muriatic acid gas, no substances, but those just mentioned, are concerned; the weight of the compound is the exact weight of

This not the fact.

of the two gasses employed—nothing ponderable escapes; muriatic acid gas consequently is not inferred, but is immediately perceived to be, a compound of oximuriatic gas and hidrogen, and all other cases are analogous.

Mr. Murray's illustration from the combination of oxide of mercury and muriatic acid.

Mr. Murray, to convince me of the errour of which he conceived me guilty, respecting the nature of Mr. Davy's theory, has recourse to particular instances to illustrate his argument. He says: "I combine oxide of mercury and muriatic acid, and form calomel. I conclude therefore, that calomel is a compound of oxide of mercury and muriatic acid. I combine muriatic acid and potash, and by dissipation of the water I obtain a solid product, which I consider as a compound of the muriatic acid and potash, and I perceive in these conclusions no supposition, but a simple expression of facts." If Mr. Murray can combine oxide of mercury and muriatic acid, and form calomel, I have no objection to his conclusions; if the above is a simple expression of facts, the theory which expresses those facts must be correct. But I have not been able to witness such facts. I have found, that, when muriatic acid gas is admitted into an exhausted retort, containing red precipitate, corrosive sublimate, and not calomel, is formed; that water in plenty is simultaneously produced; and that much heat is generated, sufficient indeed, when the experiment is made on a pretty large scale, to revive some mercury by the expulsion of its oxigen. Mr. Murray, not attending to all the phenomena, has formed a false theory. Stahl, finding sulphur produced by heating charcoal with sulphuric acid, asserted, that sulphur is a compound of sulphuric acid and phlogiston; and Mr. Murray, knowing that different metallic compounds may be procured by treating different oxides with muriatic acid, asserts, that these compounds consist of muriatic acid and metallic oxides. In Stahl's famous experiment, carbonic acid gas, not being then discovered, escaped his notice; but the same cannot be said of water, which Mr. Murray has thus neglected. The preceding illustration of Mr. Murray at once demonstrates the real difference between Mr. Davy's theory and the old hypothesis; and that the former is, as I have represented it, a simple a simple expression of facts, and the latter a series of suppositions.

I shall studiously avoid discussion in the remaining part of this paper: it is my intention to confine myself to facts, which speak for themselves, and are the only legitimate supports of a theory.

Water produced when oxide of manganese is heated in muriatic acid gas.

Mr. Murray asserts, that Mr. Davy is obliged to *suppose*, that water is produced in the common mode of making oximuriatic gas from muriatic acid, by means of the black oxide of manganese. Mr. Davy has ascertained the *fact*, that oximuriatic gas and water are produced, when black oxide of manganese is heated in muriatic acid gas.

Mr. Davy's theory more simple, than Mr. Murray supposes.

Mr. Murray imagines a great intricacy in some parts of Mr. Davy's theory, which does not really belong to it; for theory, being an expression of facts, must be as simple as the facts themselves. Mr. Murray, for instance, conceives, that, in the solution of muriate of potash in water, water is decomposed; and that it is recomposed at the moment of its expulsion by heat. These are conjectures. In Mr. Davy's theory, fused muriate of potash, I conceive, is a compound of oximuriatic acid and potassium; and the solution of muriate of potash is a compound of oximuriatic acid, potassium, oxigen, and hidrogen. The mutual decomposition of nitrate of mercury and common salt is another supposed complicated instance pointed out by Mr. Murray. It is this gentleman who imagines the changes complicated. The facts are merely these: sodium surrenders its oximuriatic acid to the mercury, and receives in return its oxigen and nitric acid, and thus calomel and nitrate of soda are very simply formed.

Things not accounted for not always anomalous.

Mr. Murray seems to consider every thing anomalous, that is not accounted for; thus the want of action between charcoal and oximuriatic gas is in his opinion an anomaly in Mr. Davy's theory. Can Mr. Murray account for the want of action between charcoal and nitrogen, and between the metals and nitrogen? and, if he cannot, does he consequently consider these facts anomalous?

Mercury decomposes muriatic acid gas and forms calomel.

Mr. Murray doubts what I have alleged to be fact; viz. that the composition of muriatic acid gas is uniformly the same. I do not pretend to account for the results of Dr. Henry's

Henry's experiments, on which he rests his doubts. I have observed, that, when muriatic acid gas is left in a jar over mercury, the acid will slowly disappear, calomel will be formed, and at length nothing but hidrogen will remain.

Carburetted hidrogen and oximuriatic gas do not form carbonic acid.

I have stated in my first paper the general result, that carbonic acid is not formed, when dried carburetted hidrogen is detonated over recently boiled mercury with an excess of oximuriatic gas. Mr. Murray wishes to know how I ascertained this fact.—I considered the precipitation of charcoal, and no cloudiness being produced on passing the residual gas through lime-water, sufficient evidences of this.

Detonation of hidrogen, oximuriatic gas, and carbonic oxide.

Mr. Murray objects to the mode in which his experiment on the detonation of a mixture of hidrogen gas, oximuriatic gas, and carbonic oxide, was repeated, and is not satisfied with the results which are in opposition to his own. I have assisted my brother in again making this experiment; Mr. Hatchett and Mr. Brande were present. A mixture, consisting of 14·6 measures of oximuriatic gas, of 4 measures of hidrogen gas, and of 10 measures of gaseous oxide of carbon, was inflamed by an electric spark over recently boiled mercury; a condensation of half a measure only was produced by the explosion. Pure ammoniacal gas was added in excess, and, after the admission of water, there remained 13 measures of unabsorbable gas. Eight measures of oxigen being introduced, the mixture was inflamed; there was a diminution equal to 4 measures, and 8 measures of the residue were absorbed by a strong solution of potash *.

Now the 8 measures of carbonic acid gas formed indicate 8 measures of residual carbonic oxide; and, when the common air present is taken into account, with the difficulty

* The oximuriatic gas was procured from a mixture of common salt, black oxide of manganese, and diluted sulphuric acid; the 14 measures employed were found by a comparative trial to be contaminated by 2 measures of common air. The other two gasses had been previously dried by potash. 11·5 measures of the carbonic oxide, detonated with 16·5 of oxigen, were immediately diminished to 19; and by agitation with a strong solution of potash, there was a farther diminution produced equal to 11 measures.

of

of effecting the entire exclusion of moisture, no result more satisfactorily conclusive, that no carbonic acid was formed, could be expected: and we obtained a similar result in another experiment, in which we employed a strong solution of potash instead of ammoniacal gas, for absorbing the acid gas formed.

Compound of oximuriatic gas and oxigen.

I mentioned in a note to my former paper the discovery made by Mr. Davy of a gaseous compound of oximuriatic gas and oxigen. I stated the method of procuring it, and the property which it has of converting carbonic oxide into carbonic acid. Mr. Murray appears to think very lightly of this compound. But I can assure this gentleman, "notwithstanding it is procured (as he justly remarks) from the same materials as oximuriatic gas, and by a process apparently not much different from that which is usually employed," that Mr. Davy has found it to possess very different properties. Copper leaf, arsenic, and the common metals, for instance, which instantly inflame in oximuriatic gas, remain untarnished in this gas. And, what is extraordinary, it is *oxigen* in union which prevents the combustion of the metals from taking place; for when the combination is broken by nitrous gas, or a gentle heat, the oximuriatic gas, set free, acts as usual. The decomposition too of this gas by heat is so rapid, that it produces a loud explosion; and, if the quantity is large, a dangerous one: and it is a very singular circumstance, that it is attended with the evolution of heat, and even of light, notwithstanding there is a very considerable increase of volume. Mr. Murray may have remarked the difference of colour between common oximuriatic gas and the gas from oximuriate of potash; it is owing to an admixture of the newly discovered gas. When this gentleman learns, that the pure gas contains about half its volume of oxigen, he will probably no longer doubt, that it may be able to convert carbonic oxide into carbonic acid; and since *oxigen* united to oximuriatic gas deprives the latter of all those properties, which it was supposed to owe to loosely combined oxigen, he will probably adopt the new idea, that oximuriatic gas is a simple body. But if on the contrary he should still prefer the old hypothesis—the consequence is inevitable—

Its singular properties.

he

he must account for muriatic acid being a supporter of combustion when combined with a *single* proportion of oxigen, and a nonsupporter when combined with a *double* proportion, and for a variety of other anomalies, which it is needless to mention.

I am, Sir, with great respect,

Your humble servant,

J. DAVY.

London, March the 15*th*, 1811.

VII.

An Attempt to answer the Queries proposed by F. D. *in the Journal for April last: by* William Crane, *Esq. F. R. M. S. Edinburgh.*

To Mr. NICHOLSON.

SIR,

Questions on the production of hyperoxi-muriate of potash.

A Correspondent, in your Journal for April, has in a paper on the production of hyperoximuriate of potash &c. pointed out some errours, into which Mr. Davy has fallen, in accounting for the formation of muriate and hyperoxi-muriate of potash; also respecting the formation of muriate of ammonia and oxide of tin, on the addition of water and ammonia to the fuming liquor of Libavius.

He observes, that, "when the oximuriatic acid comes into contact with the oxide of potassium, we must suppose, that part of it from superior affinity displaces *part* of the oxigen, and combines with the potassium". He then proposes the following questions:—"How shall we in the first place account for this partial action? If a superior affinity exist between part of the oximuriatic acid and part of the potassium; how is it, that it does not subsist between the whole? How is it, that the whole oxigen of the potash is not set free, and the combination consist of muriate of potash only?" In answer to these questions; it may be observed, that there are many phenomena in chemistry, where a partial decomposition only takes place, as has been noticed and explained by Berthollet in his Chemical Statics.

Partial decompositions take place in chemistry.

His

Farther questions.

His next questions are:—"But what becomes of that portion of oxigen which is liberated? Does it unite with the remainder of the oximuriatic acid, and so united, do they combine with the remaining oxide of potassium? or, is it attracted by the already saturated *oxide*, and that too in the face of a superior affinity?" According to the explanation which has been given by Mr. Davy, these objections certainly present themselves; but if we agree with Mr. Murray*, that potassium is the basis of the alkali united with hidrogen, a circumstance which I think that able chemist has proved from the experiments he has made, and from those of Gay-Lussac and Thenard, they are in a great measure removed. When hidrogen unites by combustion with oxigen, the product which is obtained is invariably water, which Mr. Davy supposes to be the union of these gasses in a neutralized state. Hence as the union of potassium with oxigen is always attended with combustion, there is great probability, that the hidrogen of the potassium unites with oxigen and forms water, and we obtain, instead of an oxide of potassium, as has been supposed, a hydrate; or pure alkali is the unknown base combined with water. That this is the case is also probable, from the very strong attraction alkali has for its water of crystallization, from which both Mr. Davy and Mr. Berthollet say it cannot be entirely freed at a very high temperature: after it has been freed from the water it holds in superabundance, I would suppose, it then requires the aid of a chemical agent, powerful enough to decompose the water it still retains, thus liberating the oxigen, whilst the hidrogen remains united to the unknown base, forming potassium. Again, as oximuriatic acid can unite with water, it requires no twisting of theory to suppose, that the hyperoximuriate of potash is a triple compound consisting of oximuriatic acid, water, and the unknown base, having, perhaps, by the combined affinity of the water and this base an excess of oximuriatic acid, and of course no evolution of gas would take place. This opinion might be extended a little farther, and we may account for the disengagement of oxigen from the hyper-

Answer on the supposition that potassium is united with hidrogen.

* See Mr. Murray's paper, Number for April.

oximuriate

oximuriate of potash upon the application of heat, by the combined affinity of the unknown base and oximuriatic acid for hidrogen being enabled to overcome, by the aid of heat, the affinity of the oxigen for the hidrogen, which neither of them can effect separately.

Composition of the two muriates

His next observation is, that, as muriate of potash is a compound of muriatic acid and potash. "We must now suppose, that, when the oximuriatic acid first enters the solution of potash, part of it attracts from the water of the solution, a portion of hidrogen; and, being thus changed to muriatic acid, combines with the potash to form muriate of potash. The oxigen thus liberated unites to the other portion of the oximuriatic acid and the hyperoximuriate of potash is formed," which, he says, is a direct contradiction to the theory advanced to account for the liberation of oximuriatic acid in the retort.

owing to the two acids coming over.

To account for the formation of the muriate of potash, there can be no occasion to have recourse to the decomposition of the water; for, as muriatic acid is extremely volatile, and as the action of the oxide of manganese is not instantaneous; it is evident, that part of the muriatic acid will rise and pass over with the oximuriatic acid, particularly in the first stages of the process, and hence we find both the muriate and oximuriate of potash.

Decomposition of the fuming liquor of Libavius.

Mr. Davy, in accounting for the production of water when muriatic acid is passed over litharge, says, it arises from the superior affinity, which exists between the oximuriatic acid and the lead, and the subsequent union of the hidrogen of the one and the oxigen of the other. Next, he accounts for the oxide of tin and muriate of ammonia, obtained by ammonia upon the addition of water to the fuming liquor of Libavius, as owing to the superior affinity between the oximuriatic acid and the hidrogen. Now your correspondent justly observes, that, "in the first place, water is *composed* because the affinity of oximuriatic acid for a *metal* is *greater* than the quiescent affinities, taken together, of oximuriatic acid for hidrogen and the metal for oxigen; and, in the second, water is *decomposed* because the affinity of oximuriatic acid for a metal is *less* than the now divellent affinities of oximuriatic acid for hidrogen and the metal

metal for oxigen". Supposing the compositions of the water in the first instance to take place according to Mr. Davy's views, then, in the second, the oximuriatic acid is attracted from the tin by the ammonia, at the same time it attracts, in its turn, the hidrogen of the water; and as by the attraction of the ammonia the affinity between the oximuriatic acid and tin is weakened, the tin by this being enabled to attract the oxigen of the water, and the oximuriatic acid attracting the hidrogen, the water is decomposed, and the oxide of tin and muriate of ammonia are formed.

I am, Sir,

Your humble servant,

Edinburgh, April the 9th, W. CRANE.
1811.

VIII.

Experiments on Allanite, a new Mineral from Greenland, by THOMAS THOMSON, *M. D. F. R. S. E. Fellow of the Imperial Chirurgo-Medical Academy of Petersburgh.**

ABOUT three years ago, a Danish vessel† was brought into Leith as a prize. Among other articles, she contained a small collection of minerals, which were purchased by Thomas Allan, Esq., and Colonel Imrie, both members of this society. The country from which these minerals had been brought was not known for certain; but as the collection abounded in cryolite, it was conjectured, with very considerable probability, that they had been collected in Greenland. Collection of minerals in a Danish prize.

Among the remarkable minerals in this collection there was one, which, from its correspondence with gadolinite, as described in the different mineralogical works, particularly attracted the attention of Mr. Allan. Confirmed in the idea of its being a variety of that mineral by the opinion of One of these supposed to be gadolinite.

* From the Transactions of the Royal Society of Edinburgh.

† Der Fruhling, Captain Jacob Ketelson, captured on her passage from Iceland to Copenhagen.

Count

Count Bournon, added to some experiments made by Dr. Wollaston, he was induced to give the description, which has since been published in a preceding part of the present volume.

About a year ago, Mr. Allan, who has greatly distinguished himself by his ardent zeal for the progress of mineralogy in all its branches, favoured me with some specimens of this curious mineral, and requested me to examine its composition; a request which I agreed to with pleasure, because I expected to obtain from it a quantity of *yttria*, an earth which I had been long anxious to examine, but had not been able to procure a sufficient quantity of the Swedish gadolinite for my purpose. The object of this paper is to communicate the result of my experiments to the Royal Society; experiments which cannot appear with such propriety any where as in their transactions, as they already contain a paper by Mr. Allan on the mineral in question.

Description of it.

Sect. 1. I am fortunately enabled to give a fuller and more accurate description of this mineral than that which formerly appeared, Mr. Allan having since that time discovered an additional quantity of it, among which he not only found fresher and better characterised fragments, but also some entire crystals. In its composition it approaches most nearly to cerite; but it differs from it so much in its external characters, that it must be considered as a distinct species. I have therefore taken the liberty to give it the name of Allanite, in honour of Mr. Allan, to whom we are in reality indebted for the discovery of its peculiar nature.

Allanite occurs massive and disseminated, in irregular masses, mixed with black mica and felspar; also crystallised; the varieties observed are,

1. A four-sided oblique prism, measuring 117° and 63°.

2. A six-sided prism, acuminated with pyramids of four sides, set on the two adjoining opposite planes. These last are so minute as to be incapable of measurement. But, as nearly as the eye can determine, the form resembles fig, 1, Pl. II; the prism of which has two right angles, and four measuring 135°.

3. A flat prism, with the acute angle of 63° replaced by one plane, and terminated by an acumination, having three principal

principal facettes set on the larger lateral planes, with which the centre one measures 125° and 55°. Of this specimen an engraving is given in the annexed plate, fig. 2.

Specific gravity, according to my experiments, 3·533. The specimen appears to be nearly, though not absolutely, pure. This substance, however, is so very much mixed with mica, that no reliance can be placed on any of the trials which have been made. Count Bournon, surprised at the low specific gravity noted by Mr. Allan, which was 3·480, broke down one of the specimens which had been sent him, in order to procure the substance in the purest state possible, and the result of four experiments was as follows.

4·001
3·797
3·654
3·119

In a subsequent experiment of Mr. Allan's, he found it 3·665. From these it appears, that the substance is not in a pure state. Its colour is so entirely the same with the mica, with which it is accompanied, that it is only by mechanical attrition that they can be separated.

Colour, brownish-black.

External lustre, dull; internal, shining and resinous, slightly inclining to metallic.

Fracture, small conchoidal.

Fragments, indeterminate, sharp-edged.

Opake.

Semihard in a high degree. Does not scratch quartz or felspar, but scratches hornblende and crown glass.

Brittle.

Easily frangible.

Powder, dark greenish-gray.

Before the blowpipe it froths, and melts imperfectly into a brown scoria.

Gelatinises in nitric acid. In a strong red heat it loses 3·98 *per cent* of its weight.

Experiments to ascertain its composition.

Sect. 2. My first experiments were made on the supposition, that the mineral was a variety of gadolinite, and were pretty much in the style of those previously made on that substance by Ekeberg, Klaproth, and Vauquelin.

Silex.

1. 100 grains of the mineral, previously reduced to a fine

powder in an agate mortar, were digested repeatedly on a sand bath in muriatic acid, till the liquid ceased to have any action on it. The undissolved residue was silica, mixed with some fragments of mica. When heated to redness, it weighed 33·4 grains.

Alumine.

2. The muriatic acid solution was evaporated almost to dryness, to get rid of the excess of acid, dissolved in a large quantity of water, mixed with a considerable excess of carbonate of ammonia, and boiled for a few minutes. By this treatment, the whole contents of the mineral were precipitated in the state of a yellowish powder, which was separated by the filter, and boiled, while still moist, in potash lie. A small portion of it only was dissolved. The potash lie was separated from the undissolved portion by the filtre, and mixed with a solution of sal ammoniac, by means of which a white powder precipitated from it. This white matter, being heated to redness, weighed 7·9 grains. It was digested in sulphuric acid, but 3·76 grains refused to dissolve. This portion possessed the properties of silica. The dissolved portion, being mixed with a few drops of sulphate of potash, shot into crystals of alum. It was therefore alumina, and amounted to 4·14 grains.

Metallic oxide.

3. The yellow matter, which refused to dissolve in the potash lie, was mixed with nitric acid. An effervescence took place, but the liquid remained muddy, till it was exposed to heat, when a clear reddish-brown solution was effected. This solution was evaporated to dryness, and kept for a few minutes in the temperature of about 400°, to peroxidize the iron, and render it insoluble. A sufficient quantity of water was then poured on it, and digested on it for half an hour, on the sand bath. The whole was then thrown upon a filter. The dark red matter, which remained on the filter, was drenched in oil, and heated to redness, in a covered crucible. It was then black, and attracted by the magnet; but had not exactly the appearance of oxide of iron. It weighed 42·4 grains.

Lime.

4. The liquid, which passed through the filter, had not the sweet taste which I expected, but a slightly bitter one, similar to a weak solution of nitrate of lime. Hence it was clear, that no yttria was present, as there ought to have been,

been, had the mineral contained that earth. This liquid being mixed with carbonate of ammonia, a white powder precipitated, which, after being dried in a red heat, weighed 17 grains. It dissolved in acids with effervescence; the solution was precipitated white by oxalate of ammonia, but not by pure ammonia. When dissolved in sulphuric acid, and evaporated to dryness, a light matter remained, tasteless, and hardly soluble in water. These properties indicate carbonate of lime. Now, 17 grains of carbonate of lime are equivalent to about 9·23 grains of lime.

Deductions.

5. From the preceding analysis, supposing it accurate, it followed, that the mineral was composed of

Silica,	37·16
Lime,	9·23
Alumina,	4·14
Oxide of iron,	42·40
Volatile matter,	3·98
	96·91
Loss,	3·09
	100·00

The oxide examined.

But the appearance of the supposed oxide of iron induced me to suspect, that it did not consist wholly of that metal. I thought it even conceivable, that the yttria, which the mineral contained, might have been rendered insoluble by the application of too much heat, and might have been concealed by the iron, with which it was mixed. A number of experiments, which it is needless to specify, soon convinced me, that, beside iron, there was likewise another substance present, which possessed properties different from any that I had been in the habit of examining. It possessed one property at least in common with yttria; its solution in acids had a sweet taste; but few of its other properties had any resemblance to those which the chemists, to whom we are indebted for our knowledge of yttria, have particularised. But as I had never myself made any experiments on yttria, I was rather at a loss what conclusion to draw. From this uncertainty I was relieved by Mr. Allan, who had the goodness to give me a small fragment of

 gadolinite,

gadolinite, which had been received directly from Mr. Ekeberg. From this I extracted about 10 grains of yttria; and upon comparing its properties with those of the substance in question, I found them quite different. Convinced by these experiments, that the mineral contained no yttria, but that one of its constituents was a substance with which I was still unacquainted, I had recourse to the following mode of analysis, in order to obtain this substance in a pure state.

Analysis.

Sect. III. 1. 100 grains of the mineral, previously reduced to a fine powder, were digested in hot nitric acid, till nothing more could be dissolved. The undissolved residue, which was silica, mixed with some scales of mica, weighed, after being heated to redness, 35·4 grains.

Silex.

Oxide of iron.

2. The nitric acid solution was transparent, and of a light brown colour. When strongly concentrated by evaporation, to get rid of the excess of acid, and set aside in an open capsule, it concreted into a whitish solid matter, consisting chiefly of soft crystals, nearly colourless, having only a slight tinge of yellow. These crystals, being left exposed to the air, became gradually moist, but did not speedily deliquesce. The whole was therefore dissolved in water, and the excess of acid, which was still present, carefully neutralised with ammonia. By this treatment the solution acquired a much deeper brown colour; but it still continued transparent. Succinate of ammonia was then dropped in with caution. A copious reddish-brown precipitate fell, which being washed, dried, and heated to redness in a covered crucible, weighed 25·4 grains. It possessed all the characters of black oxide of iron. For it was attracted by the magnet, completely soluble in muriatic acid, and the solution was not precipitated by oxalate of ammonia.

Another precipitate thrown down.

3. The liquid being still of a brown colour, I conceived it not to be completely free from iron. On this account, an additional quantity of succinate of ammonia was added. A new precipitate fell; but instead of the dark reddish-brown colour, which characterises succinate of iron, it had a beautiful flesh-red colour, which it retained after being dried in the open air. When heated to redness in a covered crucible,

crucible, it became black, and had some resemblance to gunpowder. It weighed 7·2 grains.

4. This substance attracted my peculiar attention, in consequence of its appearance. I found it to possess the following characters: This examined.

a. It was tasteless, and not in the least attracted by the magnet, except a few atoms, which were easily separated from the rest. Its characters.

b. It was insoluble in water, and not sensibly acted on when boiled in sulphuric, nitric, muriatic, or nitro-muriatic acid.

c. Before the blowpipe it melted with borax and microcosmic salt, and formed with both a colourless bead. With carbonate of soda it formed a dark-red opake bead.

d. When heated to redness with potash, and digested in water, snuff-coloured flocks remained undissolved, which gradually subsided to the bottom. The liquid being separated, and examined, was found to contain nothing but potash. When muriatic acid was poured upon the snuff-coloured flocks, a slight effervescence took place, and when heat was applied, the whole dissolved. The solution was transparent, and of a yellow colour, with a slight tint of green. When evaporated to dryness, to get rid of the excess of acid, a beautiful yellow matter gradually separated. Water boiled upon this matter dissolved the whole. The taste of the solution was astringent, with a slight metallic flavour, by no means unpleasant, and no sweetness was perceptible.

e. A portion of the black powder being exposed to a red heat for an hour, in an open crucible, became reddish-brown, and lost somewhat of its weight. In this altered state, it was soluble by means of heat, though with difficulty, both in nitric and sulphuric acids. The solutions had a reddish-brown colour, a slight metallic astringent taste, but no sweetness.

f. The solution of this matter in nitric and muriatic acid, when examined by reagents, exhibited the following phenomena: Action of reagents on its solution.

(1.) With prussiate of potash, it threw down a white precipitate in flocks. It soon subsided; readily dissolved in nitric acid; the solution was green.

(2.)

(2.) Prussiate of mercury. A light yellow precipitate, soluble in nitric acid.

(3.) Infusion of nut galls. No change.

(4.) Gallic acid. No change.

(5.) Oxalate of ammonia. No change.

(6.) Tartrate of potash. No change.

(7.) Phosphate of soda. No change.

(8.) Hydrosulphuret of ammonia. Copious black flocks. Liquor remains transparent.

(9.) Arseniate of potash. A white precipitate.

(10.) Potash. .... } Copious yellow-coloured
(11.) Carbonate of soda. } flocks; readily dissolved in
(12.) Carbonate of ammonia. } nitric acid.

(13.) Succinate of ammonia. A white precipitate.

(14.) Benzoate of potash. A white precipitate.

(15.) A plate of zinc, being put into the solution in muriatic acid, became black, and threw down a black powder, which was insoluble in sulphuric, nitric, muriatic, nitro-muriatic, acetic, and phosphoric acids, in every temperature, whether these acids were concentrated or diluted.

(16.) A plate of tin, put into the nitric solution, occasioned no change.

(17.) A portion being enclosed in a charcoal crucible, and exposed for an hour to the heat of a forge, was not reduced to a metallic button, nor could any trace of it be detected when the crucible was examined.

These properties aproached those of cerium,

These properties were all that the small quantity of the matter in my possession enabled me to ascertain. They unequivocally point out a metallic oxide. Upon comparing them with the properties of all the metallic oxides known, none will be found with which this matter exactly agrees. Cerium is the metal, the oxides of which approach the nearest. The colour is nearly the same, and both are precipitated white by prussiate of potash, succinate of ammonia,

but with some differences.

and benzoate of potash. But, in other respects, the two substances differ entirely. Oxide of cerium is precipitated white by oxalate of ammonia and tartrate of potash; our oxide is not precipitated at all: Oxide of cerium is precipitated white by hydrosulphuret of ammonia; while our oxide is precipitated black: Oxide of cerium is not precipitated

tated by zinc, while our oxide is thrown down black. There are other differences between the two, but those which I have just mentioned are the most striking.

These properties induced me to consider the substance which I had obtained from the Greenland mineral as the oxide of a metal hitherto unknown; and I proposed to distinguish it by the name of *junonium*. Supposed a new metal.

In the experiments above detailed, I had expended almost all the oxide of *junonium* which I had in my possession, taking it for granted, that I could easily procure more of it from the Greenland mineral. But, soon after, I was informed by Mr. Wollaston, to whom I had sent a specimen of the mineral, that he had not been able to obtain any of my supposed junonium in his trials. This induced me to repeat the analysis no less than three times, and in neither case was I able to procure any more of the substance, which I described above. Thus, it has been out of my power, to verify the preceding details, and to put the existence of a new metal in the mineral beyond doubt. At the same time I may be allowed to say, that the above experiments were made with every possible attention on my part, and most of them were repeated, at least a dozen times. I have no doubt myself of their accuracy; but think that the existence of a new metal can hardly be admitted, without stronger proofs than the solitary analysis which I have performed. Junonium.

5. The liquid, thus freed from iron and junonium, was supersaturated with pure ammonia. A grayish white gelatinous matter precipitated. It was separated by the filter, and became gradually darker coloured when drying. This matter, after being exposed to a red heat, weighed about 38 grains. When boiled in potash lie, 4·1 grains were dissolved, of a substance which, separated in the usual way, exhibited the properties of alumina. Alumine.

6. The remaining 33·9 grains were again dissolved in muriatic acid, and precipitated by pure ammonia. The precipitate was separated by the filter, and allowed to dry spontaneously in the open air. It assumed an appearance very much resembling gum arabic, being semitransparent, and of a brown colour. When dried upon the sand-bath, it became very dark brown, broke with a vitreous fracture, and still retained a small degree of transparency. It was tasteless, An oxide,

tasteless, felt gritty between the teeth, and was easily reduced to powder. It effervesced in sulphuric, nitric, muriatic, and acetic acids, and a solution of it was effected in each by means of heat, though not without considerable difficulty. The solutions had an austere, and slightly sweetish taste. When examined by reagents, they exhibited the following properties:

examined by reagents,

(1.) Prussiate of potash. A white precipitate.
(2.) Oxalate of ammonia. A white precipitate.
(3.) Tartrate of potash. A white precipitate.
(4.) Hydrosulphuret of potash. A white precipitate.
(5.) Phosphate of soda. A white precipitate.
(6.) Arseniate of potash. A white precipitate.
(7.) Potash and its carbonate. A white precipitate.
(8.) Carbonate of ammonia. A white precipitate.
(9.) Ammonia. A white gelatinous precipitate.
(10.) A plate of zinc. No change.

appeared to differ in some respects from that of cerium;

These properties indicated oxide of cerium. I was therefore disposed to consider the substance which I had obtained as oxide of cerium. But on perusing the accounts of that substance, given by the celebrated chemists to whose labours we are indebted for our knowledge of it, there were several circumstances of ambiguity which occurred. My powder was dissolved in acids with much greater difficulty than appeared to be the case with oxide of cerium. The colour of my oxide, when obtained from oxalate, by exposing it to a red heat, was much lighter, and more inclined to yellow, than the oxide of cerium.

but this owing to the method in which it was procured.

In this uncertainty, Dr. Wollaston, to whom I communicated my difficulties, offered to send me down a specimen of the mineral called *cerite*, that I might extract from it real oxide of cerium, and compare my oxide with it. This offer I thankfully accepted *; and upon comparing the properties of my oxide with those of oxide of cerium, extracted from *cerite*, I was fully satisfied that they were identical. The more

* The specimen of cerite, which I analysed, was so much mixed with actonolite, that the statement of the results which I obtained cannot be

more difficult solubility of mine was owing to the method I had employed to procure it, and to the strong heat to which I had subjected it; whereas the oxide of cerium from cerite had been examined in the state of carbonate.

7. In the many experiments made upon this powder, and upon oxide of cerium from cerite, I repeated every thing that had been established by Berzelius and Hisinger, Klaproth and Vauquelin, and had an opportunity of observing many particulars, which they have not noticed. It may be worth while, therefore, without repeating the details of these chemists, to mention a few circumstances, which will be found useful in examining this hitherto scarce oxide. Some particulars respecting it not before noticed.

a. The precipitate occasioned by the oxalate of ammonia is at first in white flocks, not unlike that of muriate of silver, but it soon assumes a pulverulent form. It dissolves readily in nitric acid, without the assistance of heat. The same remark applies to the precipitate thrown down by the tartrate of potash. But tartrate of cerium is much more soluble in acids than the oxalate.

b. The solution of cerium in acetic acid is precipitated gray by infusion of nut-galls. Cerium is precipitated likewise by the same reagent from other acids, provided the solution contains no excess of acid. This fact was first observed by Dr. Wollaston, who communicated it to me last summer. I immediately repeated his experiments with success.

c. Cerium is not precipitated from its solutions in acids by a plate of zinc. In some cases, indeed, I have obtained a yellowish-red powder, which was thrown down very slowly. But it proved, on examination, to consist almost entirely of red oxide of iron, and of course only appeared when the solution of cerium was contaminated with iron.

be of much importance. The specific gravity of the specimen was 4·149. I found it composed as follows:

A white powder, left by muriatic acid, and presumed to be silica,	47·3
Red oxide of cerium	44·
Iron	4·
Volatile matter	3·
Loss	1·7
	100·0

d. The

d. The solutions of cerium in acids have an astringent taste, with a perceptible sweetness, which, however, is different from the sweetness, which some of the solutions of iron in acids possess.

e. The muriate and sulphate of cerium readily crystallise; but I could not succeed in obtaining crystals of nitrate of cerium.

Best method of obtaining the oxide.

f. The best way of obtaining pure oxide of cerium is, to precipitate the solution by oxalate of ammonia, wash the precipitate well, and expose it to a red heat. The powder obtained by this process is always red: but it varies very much in its shade, and in its beauty, according to circumstances. This powder always contains carbonic acid.

Essential characters of cerium,

g. I consider the following as the essential characters of cerium. The solution has a sweet astringent taste. It is precipitated white by prussiate of potash, oxalate of ammonia, tartrate of potash, carbonate of potash, carbonate of ammonia, succinate of ammonia, benzoate of potash, and hydrosulphuret of ammonia. The precipitates are redissolved by nitric or muriatic acids. Ammonia throws it down in gelatinous flocks. Zinc does not precipitate it at all.

h. The white oxide of cerium, mentioned by Hisinger and Berzelius, and described by Vauquelin, did not present itself to me in any of my experiments: unless the white flocks precipitated by ammonia from the original solution be considered as white oxide. They became brown on drying, and, when heated to redness, were certainly converted into red oxide.

As cerium, as well as iron, is precipitated by succinate of ammonia, the preceding method of separating the two from each other was not unexceptionable. Accordingly, in some subsequent analyses, I separated the cerium by means of oxalate of ammonia, before I precipitated the iron. I found, that the proportions obtained by the analysis above described were so near accuracy, that no material alteration is necessary.

Lime.

8. The liquid, thus freed from iron, alumina, and cerium, was mixed with carbonate of soda. It precipitated a quantity of carbonate of lime, which amounted, as before, to about

about 17 grains, indicating 9·2 grains of lime.

From the preceding analysis, which was repeated no less than three times, a different method being employed in each, the constituents of allanite are as follows:

Component parts of allanite.

Silica	35·4
Lime	9·2
Alumina	4·1
Oxide of iron	25·4
Oxide of cerium	33·9
Volatile matter	4·
	112·0

I omit the 7 grains of junonium, because I only detected it in one specimen of allanite. The excess of weight in the preceding numbers is to be ascribed chiefly to the carbonic acid combined with the oxide of cerium, from which it was not completely freed by a red heat. I have reason to believe, too, that the proportion of iron is not quite so much as 25·5 grains. For, in another analysis, I obtained only 18 grains, and in a third 20 grains. Some of the cerium was perhaps precipitated along with it in the preceding analysis, and thus its weight was apparently increased.

The iron probably overrated.

IX.

Observations on Three Papers of Mr. Davy. *By Messrs.* Gay-Lussac *and* Thenard*.

Mr. Davy's observations on the researches of Gay-Lussac and Thenard

In the Annales de Chimie for September last are translations of three papers by Mr. Davy, sent to France by that gentleman, and entitled, 1. Observations on the Researches of Messrs. Gay-Lussac and Thenard relative to the Amalgam furnished by ammonia. 2. Examination of some Observations of Messrs. Gay-Lussac and Thenard on the Facts respecting the Metals of the Alkalis. 3. Reply to

* Abridged from the Annal. de Chim. vol. LXXV, p. 290.

Messrs.

answered.

Messrs. Gay-Lussac and Thenard's Answer to the Analytical Researches, &c. These are followed by Observations on them by Messrs. Gay-Lussac and Thenard, some extracts from which will no doubt be acceptable to our readers; though a translation of the whole would take up much room to little purpose, as most of the facts have come before them in a different form. The following is its exordium.

Introduction.

"The observations about to be read are divided into three parts. We shall merely relate our mode of viewing things, supporting it by reasons, which we believe to be preponderant. If by accident any expressions escape us liable to be misconstrued, we request our readers, and particularly Mr. Davy, not to do this. It is our intention, unquestionably, to combat some of his opinions, because we do not always think with him: but while we combat them we wish to employ the language suited to truth, and merit the esteem of the celebrated chemist, whose talents have justly entitled him to that of all Europe, and more particularly ours."

Amalgam of ammonia not acted on by the air or sulphuric acid.

On the first head these gentlemen say: "we have demonstrated, that the amalgam of ammonia has no action on the air, or on sulphuric acid; and it is totally impossible, that it should cover itself in the open air with a white powder of carbonate of ammonia." And again:

A compound of ammonia, hidrogen, and mercury.

"In fine, we believe we have fully demonstrated, that the ammoniacal amalgam is nothing but a compound of mercury, ammonia, and hidrogen: for Mr. Davy opposes nothing to us, but that it is impossible to dry this amalgam thoroughly with blotting paper; and that the water, which covers it, combines with the ammonium, and reforms ammonia. But we know very well, that it is difficult to dry the surface of this amalgam with paper: and accordingly we take only the centre, after having cooled it to zero [32°], to increase its consistency; we introduce it into a very dry jar with very dry mercury; and immediately the amalgam decomposing gives out ammoniacal and hidrogen gas. Certainly this experiment is unobjectionable.

"However, as this experiment has not convinced Mr. Davy; and as perhaps he will tell us, that there is a little water

water (which however cannot be) in the centre of this amalgam, we will relate another, to which we think he cannot reply. It is as follows.

"After having made a liquid amalgam of potassium, we poured it into a large cupel of moistened sal ammoniac, and obtained immediately, by the process for which we are indebted to Mr. Davy, a very bulky and very consistent compound of potassium and ammoniacal amalgam. Then, having removed with a knife all the upper part, we took out the interior parts with a very dry iron spoon, and immemediately put them into a tube almost full of mercury, which had been previously boiled. Afterward, having closed this tube, which was thus filled with mercury and the compound of ammoniacal amalgam with potassium, with a very dry stopple, we inverted it in mercury also well dried. The amalgam rose above the mercury, and was almost immediately decomposed, particularly by means of a slight agitation. But, in proportion as the decomposition went on, a pretty considerable quantity of gas was extricated; and this gas was always found to be a mixture of ammoniacal and hidrogen gas, in the proportion nearly of 2·5 to 1. Now will it be said, that the mercury or our vessels were humid? We can prove they were not; for, on pouring into them an amalgam of potassium, instead of a compound of potassium and ammoniacal amalgam, no gas was evolved. Or will it be said, that the interior of the ammoniacal amalgam with potassium contained a small quantity of water? But this is impossible, since water and potassium cannot exist together. Or, finally, will it be said, that we could not accurately remove with a knife the external portions of the compound of ammoniacal amalgam with potassium? But the experiment is so easily performed, that it can never fail.

Experiment to prove this.

"Thus the slightest objection cannot be made to this experiment, and it must be conclusive, even in the eyes of Mr. Davy. Besides, the result is easily understood: it is, the potassium, combining with a very large quantity of mercury, is so disseminated, that it can no longer act with sufficient force on the ammonia and hidrogen to unite them; so that the ammoniacal amalgam of potassium finds itself

The result explained.

itself in this case subjected to the same laws, as that which is formed solely of mercury, ammonia, and hidrogen, and which cannot exist, except under the electric influence.

Lightness of the amalgam accounted for.

"If Mr. Davy admit, that the ammoniacal amalgam is a compound of mercury, ammonia, and hidrogen, he must admit also our explanation of the phenomena exhibited by its formation, or of the cause of its being five or six times as bulky as the mercury it contains. This explanation is perfectly natural. In fact, since the hidrogen and ammonia are scarcely more condensed in this amalgam, than they are in the state of gas, which is proved by the facility with which they escape from it, they cannot but considerably diminish the specific gravity of the mercury. The property that mercury has of being about 34000 times as heavy as hidrogen gas; and that which gold has of losing its lustre and ductility, and becoming soluble in all the acids, by the addition of a few hundredths of oxigen gas, are facts as extraordinary."

Under the 2d head the French chemists observe:

Solid hydruret of potassium.

"Mr. Davy says, that he could never succeed in combining hidrogen gas with potassium, so as to form the solid hydruret of potassium; which we made known in 1808, No. 144 of the *Moniteur*, &c.; and on the preparation of which we gave some fresh information in No. 330 of the *Bibliothèque britannique*, in September, 1809. He imagines, that in our experiments we paid no attention either to the solution of potassium in hidrogen gas; a solution, which, according to him, occasioning probably a condensation of this gas, might have led us into an errour; or to the influence of the metal on glass; or to the circumstance, that, from his observations, very small quantities of air or water give rise to a grayish powder, similar to what we announce as the hydruret of potassium. Our answer to all these observations shall be very simple. Let a certain quantity of potassium, as was said, *Bib. brit.* No. 330, p. 47, and of very dry and very pure hidrogen gas, be heated in a curved glass jar, thoroughly freed from air and water, and with its extremity immersed in water, the mercury will soon be seen to ascend rapidly in the jar, and at the expiration of a certain

Mr. Davy's remarks answered.

tain time to be nearly stationary. At this period let the gaseous residuum be measured, and suppose it to be equal, for instance, to two thirds of the volume of hidrogen employed, it will be concluded, that one third of the hidrogen has been absorbed by the potassium. And, in fact, it may be expelled from it immediately, by heating the potassium sufficiently in the same jar in which the experiment has been made, and which is then full of mercury.

"Thus we find, that potassium absorbs a quantity of hidrogen, which is equivalent nearly to a fourth of what it gives out with water. We have repeated this experiment a great number of times, the result has always been the same. It is certain then, that a solid hydruret of potassium exists. The properties of this hydruret may be seen in the *Bib. brit.* as above quoted.*"

Potassium does not absorb more dry ammoniacal gas than moist.

"Mr. Davy says, that potassium absorbs more ammoniacal gas dried by lime, than of common ammoniacal gas, in the proportion of 16 to 12·5. We have always observed on the contrary, that the absorption of these two gasses is perceptibly the same with an equal quantity of potassium, if the temperature be the same; as we have already shown in the *Bib. brit.* What Mr. Davy considers as potash is already, according to us, an ammoniuret.

Ammoniuret of potassium gives out some ammonia undecomposed,

"Mr. Davy says, that the ammoniuret, made with ammoniacal gas and potassium, does not give out, as we have advanced, the 0·6 of ammoniacal gas it contains; namely, 0·4 not decomposed, and 0·2 decomposed; or at least that these

Properties of the hidruret of potassium.

* "It is in the form of a gray powder, which has not a metallic appearance. It effervesces briskly with water, and gives out about one fourth more of hidrogen, than the metal it contains is capable of giving out. Placed in contact with mercury in the cold, it is gradually decomposed; an amalgam of this metal is formed, and all the hidrogen, to which it owed its pulverulent state, is evolved. If heated, its decomposition by mercury is almost instantaneous, and no more hidrogen gas is evolved than when cold. In fine, heated to an obscure red, it resumes the metallic appearance, and also evolves all the hidrogen, which the metal had absorbed." Ann. de Chim. vol. LXXII, p 266.

† Messrs. Gay-Lussac and Thenard say, in the place referred to, that a temperature somewhat elevated expels a great deal of ammonia from the olive coloured substance; and hence the quantity of ammonia absorbed by the metal is very variable, according to the temperature employed.

results

results are obtained only so far as there is moisture in the vessels employed. On this point we cannot accede to the opinion of Mr. Davy: neither our gasses, nor our mercury, nor our vessels, contain water; and yet we always obtain from this ammoniuret the 0·4 of ammoniac without being decomposed. This difference between our results and those of Mr. Davy does not depend on water, as he supposes, but on the high temperature, to which he exposes the ammoniuret."

unless overheated.

Under the 3d head Messrs. Gay-Lussac, and Thenard say: "On treating the sulphurets and phosphurets of potassium with an acid, assisted by heat, as ought to be done, neither hidroguretted sulphur, nor hidroguretted phosphorus, is formed; and we always obtain even more phosphuretted hidrogen, than is requisite to represent the hidrogen of the potassium.

Sulphuret and phosphuret of potassium treated with an acid.

"Mr. Davy says, 1st, on treating the sulphuret of potassium with muriatic acid, he has obtained very variable quantities of sulphuretted hidrogen gas; and that in general less is evolved, than the potassium of this sulphuret would disengage of hidrogen from water: 2dly, that, on the contrary, on treating potassium with sulphuretted hidrogen gas, there is a greater quantity of hidrogen gas set free, than that which the potassium employed is capable of evolving in its contact with water.

"We have repeated more than fifty times our experiments on sulphur, sulphuretted hidrogen gas, and potassium: the sulphuret of potassium has always afforded us by acids a quantity of sulphuretted hidrogen gas, equal in volume to the hidrogen, that the potash was capable of evolving by its contact with water: and always too, on treating potassium with sulphuretted hidrogen gas, we have obtained as much hidrogen gas, as the potassium would have yielded with water.

"We affirm anew, that these results are certain.

"Mr. Davy considers it as probable, that, on heating potassium with sulphur, a portion of potassium remains in the centre of the sulphuret of this metal. If but little sulphur be employed, this does not take place: still less then can it when a great deal is used, as is done by Mr. Davy.

Potassium heated with sulphur.

"Mr.

"Mr. Davy says it is evident, that the method we employ in our endeavours to show, that his experiments on phosphorus and phosphuretted hidrogen are not accurate, do not apply to the case he has in contemplation. 'They have acted,' he adds, 'on the phosphuret of potassium with hot water, and thus they form phosphate of potash, and a large quantity of phosphuretted hidrogen gas; whereas, when strong muriatic acid is employed, the muriate of potassium is produced, and the oxigen is furnished solely, or principally to the potassium. We cannot form just conclusions, unless when the potassium alone is oxided; and my design in employing but a small quantity of acid was to oxide this substance alone."

Experiments on phosphorus and phosphuretted hidrogen.

"We shall observe, 1st, that we have treated the phosphuret of potassium not with hot water only, but with acids also; and that in every case we have proved, that more phosphuretted hidrogen gas was obtained, than was required to represent the hidrogen gas, that the potassium of this phosphuret was capable of furnishing with water; and that therefore Mr. Davy has nothing to object to the means we have employed to refute his opinion, or to demonstrate, that no oxigen exists either in phosphuretted hidrogen, or in phosphorus."

No oxigen exists in them.

"Mr. Davy accuses us of contradicting ourselves, as we have said, *Mém. d'Arc.*, vol. II, p. 304, that potassium, when heated in phosphuretted, sulphuretted, or arsenicated hidrogen, absorbs the phosphorus, sulphur, or arsenic, and a portion of hidrogen; and we say, *Jour. de Phys.* Dec. 1809, that potassium sets free all the hidrogen of phosphuretted or arsenicated hidrogen. In this there is nothing extraordinary. At first we employed an excess of potassium, and an absorption of hidrogen took place. But since, particularly when Mr. Davy had concluded from his experiments, that sulphur, phosphorus, and phosphuretted and sulphuretted hidrogen, contain oxigen, having examined anew the action of potassium on sulphuretted, phosphuretted, and arsenicated hidrogen gas; and for this having necessarily employed an excess of gas; we have seen, that, in this case, no portion of the hidrogen of the phosphuretted or arsenicated hidrogen is absorbed. Tus it appears, that we are perfectly consistent;

Potassium in excess absorbs hidrogen from phosphuretted, sulph. or ars., hidrogen; not otherwise.

since we can at pleasure cause the hidrogen of these gasses to be absorbed or not by the potassium.

Potassium absorbs phosph. hid. gas without flame,

"Mr. Davy observes, that we have said potassium absorbs phosphuretted hidrogen gas with flame; while on the contrary, as he has found, it absorbs it without flame. This is true, and the mistake has even occasioned us to make another, which Mr. Davy does not mention: it has led us to say, that potassium absorbs sulphuretted hidrogen gas without the emission of light. The fact is, these two experiments were made at the same time, and one was written down for the other. This may easily be conceived, for the phenomena are too visible not to be perceived. If we give this explanation however, it is not to exculpate ourselves from the mistake."

sulph. hid. with.

Arsenicated hidrogen contains oxigen.

"Mr. Davy complains of our having said, that, if he were acquainted with the action of arsenicated hidrogen gas on potassium, he would have inferred from it the existence of oxigen in this gas. We think the same still, because we do not obtain, on treating arsenic with water, a quantity of hidrogen gas representing that which potassium is capable of giving with water.

No oxigen in sulphur or phosphorus.

"Mr. Davy could have wished, that we had spoken of his experiments to demonstrate the existence of hidrogen in sulphur and phosphorus; and complains, that we have only endeavoured to point out errours. * * * * * But our only object was to inquire, whether these experiments demonstrated the existence of oxigen in these two substances: and, as no one of them proves this, and as the result of all are contrary to ours, we could not but draw inferences from them opposite to those of Mr. Davy."

Collection of Experiments.

"In a Collection of our Experiments, now in the press, we shall answer all Mr. Davy's objections, and endeavour to render him the completest justice."

X.

Observations respecting the Sensible Perspiration of the Dictamnus Albus, or Fraxinella. By Mr. ROBERT LYALL, *Surgeon, M. R. P. S. E. &c. Communicated by the Author.*

Assertion that the bastard dit.

IT has been said, that in calm summer evenings the dictamnus albus evolves hidrogen gas, or a highly odorous inflammable

inflammable effluvium, which explodes when brought into contact with the flame of a candle; an opinion that is maintained in the latest botanical publications I have seen. tany emits an inflammable gas.

When I first became acquainted with the above notion, my curiosity was excited, and I longed for an opportunity to make the experiment, which was not very long denied me. The result of my observations I shall now relate in order, that the subject may be more accurately investigated. Experiment made on it.

I need scarcely premise, that the peduncles, the calyx, the outside of the corolla, and especially the tops of the filaments, and the germen of the dictamnus, are covered with glands of an oblong form, many of them supported on little pedicles, all of them of a beautiful red colour, and containing a somewhat viscid fluid. Glands on it containing a fluid.

On the 10th of July, about ten in the evening, the weather fine, and the temperature 66, I commenced my experiments on the dictamnus. By holding a lighted candle at the bottom of a raceme of flowers, inconsiderable explosions, or rather a hissing noise was occasioned, accompanied by light-blue coloured flame, which proceeded along the course of the peduncles, &c., and ascended even higher than the top of the stem; a good deal resembling an amusing experiment sometimes practised in the theatre, and often by boys, by means of powdered resin and a burning candle, &c. Immediately after the combustion, the surrounding atmosphere became tainted with odoriferous effluvia, exactly similar to what the healthy flowers, though much stronger, emit. Exp. 1. July 13, I repeated this experiment, at the same hour as before. The evening was fine, but the plants were wet with the afternoon's rain. Scarcely any noise was produced; the experiment not succeeding as before. Exp. 2.

At another time I brought home a raceme of flowers, and after it had stood with its end placed in water for two hours, I approached a burning candle to it, and little explosions followed. I replaced the raceme in the water, and next morning darkened my room, and made the same experiment, but heard no explosions. Since the 13th of July, I have frequently repeated the first experiment, but never have succeeded nearly so well as at first; a little hissing noise, attended with a small flame, only occurring now Other experiments.

 and

and then, occasioned in consequence of the bursting, I imagine, of the glands of new flowers; which, from their not being before developed, remained uninjured, during former experiments.

The glands destroyed in these experiments.

On examining the plants after the combustion, I observed, that the glands were completely destroyed; and thus I was led to suppose, that the resinous fluid which they contained was burnt during the explosion; and not that hidrogen, or any inflammable vapour was exhaled. Since after-experiments never succeeded so well as the first; and because the smell of hidrogen was never present, either before or after the experiment, I think I am strengthened in my opinion. At the same time, however, I confess, that I am not completely satisfied with my own observations, and therefore wish that some one, who has convenience, would not only repeat the experiments, but communicate the result of them to the public, and thus either ascertain the truth of what I have reported, or annul it altogether.

and no smell of hidrogen ever perceived.

XI.

Description and Use of a Rheumameter, to estimate and compare the Velocity of the Current of Rivers; by Mr. REGNIER, *Conservator of the central Museum of Artillery* *.

Different means employed to measure the velocity of rivers.

FROM Mariotte to the present day men of the first eminence have employed different means to estimate the velocity and force of rivers; and their methods, more or less ingenious, seem to leave nothing to be desired. I may incur the imputation of temerity therefore in bringing forward another, perhaps not equally good; but as it is very simple, attended with little expense, and requires no calculation, it may suit a great many persons, who are desirous of erecting mills or other works on rivers, with the velocity of which they are unacquainted.

Dynamometer applied to this purpose.

Mr. Gauthey, inspector general of bridges and highways, first employed my spring powderproof in the shape of a

* Abridged from Sonnini's Biblioth. Physico-écon. March, 1810, p. 193.

steelyard,

steelyard, to ascertain the force of a stream on a given surface. His process is analogous to that of the bent lever balance, as described by Michelotti in his work on Experimental Hydraulics; but his method is not so simple, nor his apparatus so cheap and portable, as that of Mr. Gauthier.

Improvement in its application.

I have observed however, that the hand which holds the rod, to which the instrument is fixed, is liable unintentionally to give it an additional impulse. This inconvenience has led me to employ the steelyard in a different manner, which appears to me more convenient and more accurate, and affords the double advantage of measuring in distinct manners both the velocity of the current, and its absolute force on a given surface, so that the two modes of examination mutually check each other.

Apparatus described.

The apparatus consists of a cork log, or float, 10 cent. [4 inches] square, in the shape of a cube, so ballasted as just to sink to the level of the surface. A small reel, turning very freely, on which is wound a silk cord of a given length, to measure the distance the log should float. A small dynamometer, resembling that I constructed to measure the strength of threads of silk, cotton, or flax. With this apparatus, which may be carried in the pocket, the action of a current may easily be ascertained.

To the upper part of the log is fastened a silk cord, forming an acute angle, like the string of a kite; and to the point of the angle is hooked a red string two yards long, tied to a green string ten yards long, which is entirely rolled up on the reel. The other end of the green string is fastened to the reel, which the observer holds in his hand. A cord of two colours is used, to distinguish the part intended to measure the distance passed through from that which should be in the water with the log.

I have preferred a silk to a hempen cord, not only because silk is stronger and more pliable, but because it does not twist in the water, and retard the progress of the log. To satisfy myself of this, I have thrown into the water little pellets of paper, which floated freely by the side of the log, and the eye could perceive a sufficient uniformity in a distance of ten yards, the measure fixed on.

To

Method of using it.

To use it, a boat being anchored in the stream, the log is to be thrown into the water, and suffered to float away, till the whole of the green cord alone remains on the reel, which is stopped at this point by a catch. One person then looking at a seconds watch gives the signal, when the second hand begins its revolution, and instantly the other, who holds the reel, sets loose the catch; the log floats on, and the time it takes to run out the ten yards of line shows the velocity*.

To determine the absolute force of the current on the cube, slip the loop at the end of the cord off the knob on the reel, and hook it to the hole of the little dynamometer, and the number of degrees shown by the index will express the maximum of the action of the water on a surface of 16 square inches.

This action is not constantly the same, not only from the effect of the waves, but from the natural current, which appears not to be always regular. In fact we have observed in calm weather, without any apparent waves, that the force of impulse varied from one instant to another in the proportion of 6 to 8, or even more.

Experiments made with it

But the velocity has a great action, as will appear from a table of the experiments we made at Paris between the Pont des Arts and Pont-Royal, on the 20th of July, 1809. The weather was calm, and the Seine a little below its mean height, being at $1\frac{1}{2}$ met. [4 feet 11 in.] on the graduated scale of the Pont-Royal.

on the Seine.

First situation, 10 *yards from the side, opposite the wickets of the Louvre.*

Exp. 1. Veloc. in sec.,	25	Force in hectog. 2 to 3: in oz. avoird. 7 to $10\frac{1}{2}$
2	$26\frac{1}{2}$	
3	26	

* The person who holds the reel in his right hand might dispense with an assistant, by holding in his left a stop watch, stopped at the end of the revolution of the seconds hand. He would only have to set loose the stop with the forefinger of the left hand, at the instant he disengaged the catch with the right, and stop the watch again the moment the line was run off the reel. C.

Second

Second situation, in the middle of the stream.

1 .... $14\frac{1}{2}$ }
2 .... 14 } .... 6 to 9: .... 21 to $31\frac{1}{4}$
3 .... 14 }

Third situation, 15 *yards from the side, opposite the street des Saints-Pères.*

1 .... 28 }
2 .... 28 } .... 1 to 2: .... $3\frac{1}{2}$ to 7
3 .... 28 }

Though these data are not very ample, it is obvious, — General conclusions.

1st, That the water at the sides of rivers has but little velocity: and

2dly, That the velocity of the middle of the stream increases in an extraordinary degree the impulsive force; since the action produced on the log by a velocity of 10 met. [32f. 9 in.] in 14 seconds was from 21 ounces to $31\frac{3}{4}$; while by a velocity of 28 seconds it was only from $3\frac{1}{2}$ oz. to 7.

On comparing afterward our experiments with those of Mariotte, made about 1666 in the same place, we found a great deal of similarity in the results. By means of little balls of wax, ballasted so as to swim level with the surface, he estimated the velocity of the Seine, at its mean height, to be 150 feet in a minute, or 30 inches in a second. But when we made our experiments the Seine was only $4\frac{1}{2}$ feet high, and at the time of Mariotte's it was 5 feet; a difference in height answering to the difference of velocity. And hence we may infer, that a century and half has made no change in the current of the river at this part. — The experiments of Mariotte in the 17th century.

The same experiments led us to compare the velocity of the Danube with that of the Seine. In the Journal de Paris, of the 11th of July, 1809, is a note from Baron Pakali, who says, that the velocity of the Danube, at its mean height at Ebersdorf, is $4\frac{1}{2}$ feet in a second; so that we may consider it twice as rapid as the Seine at Paris. — Velocity of the Danube.

Explanation of the Plate.

Pl. II, fig. 3. *a*, a cube of cork, 4 inches square, bound round with packthread to strengthen it. — Explanation of the plate.

b,

b, a plate of lead, fastened to the bottom, to ballast the cube, so as to float level with the surface.

c c, knots from which proceeds a silk cord, forming an acute angle at the point *d*.

e, hook in the loop of the red cord about two yards long, tied to a green cord of ten yards, rolled up on the reel *f*, to measure the velocity.

g, a flat piece of hard wood forming a base to the reel, in the centre of which is a small rod of polished steel, on which, as an axis, the reel turns freely.

h, tail of the catch, on which the thumb rests, to let the reel move at the signal given.

Fig. 4. *i*, a small dynamometer, with an index, to mark on the arch the maximum of the impulse of the current.

Fig. 5. *k*, the log, floating in the stream.

l, the observer in a boat, holding in his hand the dynamometer, to estimate the force of the current, after having measured the velocity.

SCIENTIFIC NEWS.

French Institute.

French Institute.

AN analysis of the proceedings of the mathematical and physical class, during the year 1809, by Mr. Delambre, perp. sec., has just reached us.

Stability of the planetary system.

The question of the stability of the planetary system has been still farther pursued by Mr. Lagrange, who has examined it in a more general point of view, extending it to a system of bodies acting on each other in any manner whatever. He also purposes to investigate the relation of the planets round their centre of gravity, considering the deviation of their figure from a sphere, and the attraction the other planets exert on each of their particles.

Rotation of the Earth.

Mr. Poisson, as a continuation of his inquiry on the variations of the elements of the planets, has composed a paper on the rotation of the Earth. As Mr. Lagrange has noticed the extreme difficulty of this problem, we cannot be be surprised to find, that formulæ have occurred to Mr. Poisson, the absolute summing up of which appeared to him impracticable. His object was to examine the influence of the term of the second order in the expression of the velocity of the Earth's rotation. These terms arise from expanding into a series the function expressing the sum of the products of the mass of each body attracting by that of the body attracted, divided by the mutual distance of these bodies. As it is impossible to calculate all these terms, the object is to bring forward only those that merit attention. Mr. P. accordingly examines in the first place, whether even those that depend on the Sun might not be neglected: and he finds, that they are always in fact very small.

As to the figure of the Earth, Mr. P. supposes, that, without the action of the Sun and Moon, the Earth would turn precisely round one of its principal axes. This is justified by the physical state of things, since we do not perceive in the altitudes of the pole, observed at different places, any of the oscillations, that would result from a different hypothesis, and the duration of which would be about one year. By similar considerations he expunges the terms relative to the other two principal axes, which can never become sensible but on hypotheses of little probability, which would give to the rotary motion of the Earth periods of less than two years, which have never been observed. He afterwards shows, that the equations to be summed up in the successive approximations preserve the same form; whence he concludes, that the axis of rotation will always coincide nearly with the shortest of the Earth's principal axes, and that the poles will always answer to the same points on the surface.

May there not be irregularities in it?

But, though the latitudes may not vary so as to deserve any attention, or to be perceptible to astronomers, is the rotary motion so uniform, as has been supposed? If its inequalities be of a very short period, and not very perceptible, they may escape our notice, and yet in a certain degree affect all our observations, and the consequences deduced from them. Suppose, for example, that the pole, instead of the 360° of its circle, passes only through 350°; and that the latitude

latitude of Paris observed at a given period should appear, in consequence of an oscillation then at its maximum, too great by 1″, the errour being proportional to the cosine of 0; the year following at the same time it would be proportional only to the cosine of 350°, and so on, till at the end of 9 years it would be nothing. At the end of 18 years however it would be 1″ in the opposite direction, whence a difference of 2″ might appear in the altitude of the pole; but so small an inequality in so long a period would not be noticed. To show the probability of this we might say, that Bradley, from a number of observations of the polestar in 1753, found the latitude of Greenwich 51° 28′ 41·5″, though from a still greater number he had before found it only 51° 28′ 38″. We may suppose therefore an oscillation of 2″ with a short period; or a greater oscillation, of which only a part has been observed. The latitude of the observatory at Paris too was found to be 48° 50′ 10″ at one time, and 48° 50′ 14″ at other times, by Lacaille, Cagnoli, Mechain, and myself. These differences might be ascribed to oscillations of at least 2″, and a period of about 15 years, so that there would have been 2½ periods between Lacaille and Cagnoli, and one only between Cagnoli and us. But I must add, that, having examined at large the observations of Bradley for five successive years, I have perceived no trace of these oscillations; that if there were one of 2″, it might frequently be confounded with the errours of observation; and that the difference of 3·5″ between the two results of Bradley might arise from his having changed his quadrant in the interval, and particularly from the errour of collimation, which for his old quadrant was 1·74″, and for the other 8″, not being known with sufficient precision, of which there are many instances. Thus we may take it for granted for the present with Mr. P. and astronomers in general, that there is no oscillation, or a very minute one; but of this we have no demonstration, and it is a point of sufficient importance, to be worth ascertaining with an instrument, in which no errour in the collimation is to be apprehended. For this it would be sufficient to observe for some years with Borda's circle the meridian altitudes of the polestar above and below the pole during the

Arguments for these.

But probably there are none.

though this remains to be proved.

the months of December and January: an oscillation, were it but of 2″, could then scarcely escape observation; as we are indebted to Mr. P.'s analytical investigation for the knowledge, that its period cannot be a complete year, so that the latitude must undergo a gradual variation, if observed regularly at the same period.

Mr. Poisson has also investigated some other formulæ, with a view to simplify them, and render them of more easy application. The first object, to which he has applied them, is the motion of a point attracted toward a fixed centre, according to any given function of the distance: and the second is the rotary motion of a body subjected to no accelerating force. His paper terminates with the following remarkable conclusion. "The perturbations of the rotary motion of solid bodies of whatever figure, to whatever attractive forces they are owing, depend on the same equations as the perturbations of the motion of a point attracted toward a fixed centre. Thus the precession of the equinoxes, and the nutation of the Earth's axis, will be expressed by the same formulæ, as give the variations of the elliptical elements of the planets. Perturbations of revolving solids.

Mr. Legendre has given us some new theories in fluxions, and approximations of easy application. Theorems in fluxions.

Messrs. Laplace and Bouvard have each investigated the problem of the motion of the Moon being such as always to present nearly the same face to the Earth. Mr. Bouvard shows, that there is no need of recurring to approximations. His method, though different from mine [Delambre's], is equally precise and direct; and his results agree perfectly with those of Mayer, thus affording an additional proof of the ability of that great astronomer. Motion of the Moon.

Mr. Burckhardt has revised and enlarged a paper on the perturbations of the planets, which he composed in 1803, but had mislaid. Perturbations of the planets.

To this is added another paper, which will conclude the volume for 1808, now about to be published. Theory has not yet been able, or has not ventured, to undertake the calculations necessary for determining the coefficients of the different inequalities of the moon, and they have been taken from observation. The method followed in these re- Lunar tables.

searches

Lunar tables. searches is to leave in an indeterminate form, in the formula of the longitude or latitude of the Moon, all the unknown coefficients, multiplying them by the fraction which expresses the sine or cosine of the argument, on which the inequality depends. All the equations in which the same coefficient has the highest positive multipliers are brought together; another sum is made of those in which this coefficient has the highest negative cofactors; and from their comparison results the most probable value of the unknown coefficient, that which agrees the best with the observations. This method, which must have been followed by Mayer, has since by Masson and Buerg, and all who have calculated tables within these twenty years. This method is easy, and has no inconvenience but the length of the calculations when observations are taken by thousands; as must be done if we would determine the coefficients of those inequalities, which from their smallness have been neglected in the theory of the Moon: and Mr. B. now offers us a very simple method of abridging these calculations, since it dispenses with the calculating and summing up of all the sines of the argument.

Conceive a series of sines of arcs, forming a decreasing arithmetical progression from 90° to 90° minus a given limit y: Mr. B. has found, that we shall obtain with sufficient precision the value of the coefficient sought, by employing, instead of the mean arithmetical sine, the sine of y divided by the arc y. According to this idea he gives the rules to be followed in these researches, where we are liable to the vexation of finding after long calculations, that the inequality sought is null, or altogether imperceptible. As a trial of his method, Mr. B. has made a selection out of 1300 observations by Dr. Maskelyne, and proposed to determine an inequality, which should have for its argument the mean anomaly of the Moon, increased by the argument that regulates the inequality, the period of which is 180 years. Nine hundred observations gave him 4·7″ for the coefficient. He is desirous, that farther examination should be made of the goodness of an equation, which so well deserves to enter into the tables.

Mr. Burckhardt proposes some other calculations for the improvement

improvement of the lunar tables, which require only some one of sufficient courage to undertake the task.

Halley's comet.

In another paper the same astronomer has calculated the perturbations of Halley's comet, which reappeared in 1759, and is expected about 1835. He has found, that the attraction of the Earth will have altered the period of its revolution sixteen days.

Methods of determining azimuths.

Having formed the plan of a grand geodetic operation for joining observatories differing greatly in longitude, he was aware of the importance of an accurate determination of the azimuths to the success of his scheme, and in consequence examined the advantages and disadvantages attached to the different methods known.

Dip of the needle.

He has also determined the dip with two different needles, one of which gave 68° 47·1′, the other 68° 47·4′, on the 10th and 20th of August, 1809. Mr. Gay-Lussac had made similar observations with another compass about the same time; and as his dip differed some minutes from that of Mr. Burckhardt, these two gentlemen have agreed to repeat their trials, in order to ascertain, if possible, the cause of the difference.

Figure of the Earth.

Mr. Biot has read a note on the observations of the pendulum made at the two extremities of the meridian, namely at Formentera and Dunkirk, in company with Messrs. Arago and Mathieu, and on the oblateness of the Earth thence resulting. All these observations exhibit a surprising agreement with those made at Bourdeaux, Figeac, and Paris, by the same gentlemen and Borda; and give an oblateness differing very little from $\frac{1}{308}$, which I have deduced from a comparison of my arc with that of Peru.

Barometrical measurements.

Mr. de Prony having been of opinion, that Mr. Ramond's coefficient for barometrical measurements was too great for inconsiderable heights, and the original coefficient of Laplace better suited to them, Mr. Ramond has several times taken the height of various places near Clermond-Ferrand, by the barometer; and Mr. de Cournon measured the same heights trigonometrically. The heights were from 300 to 600 yards. The differences were from 1 yard to 0·05. Still the differences between the heights assigned to Mount Cenis by Mr. Ramond and Mr. de Prony remain

remain to be accounted for; since Mr. de Prony's barometrical measurement of that height is confirmed by the measurements of Mr. Daune, who had to take the levels during the construction of the road over it.

In order to introduce the use of the barometer in geodetical measurements, undertaken as preliminary operations in planning roads, and particularly for canals that have to traverse heights, which would be a considerable saving of time and expense, Mr. de Prony has undertaken a series of experiments at Paris and in its vicinity, to ascertain the coefficient best adapted to small heights. He verifies the barometrical heights by trigonometrical measurements with the repeating circle. Mr. Mathieu observes at the imperial observatory, and Mr. de Prony at the little observatory constructed for him over the pediment of the House of the Legislature. Mr. de Prony employs two micrometers, diametrically opposite, for adjusting the coincidence of the index with the tangent to the summit of the mercury, by means of which he makes this adjustment superior in accuracy to the measuring by the vernier.

Micrometers applied to the barometer.

Scarcely a private meeting passes without the class hearing some report on new machines or inventions, and on papers submitted to its examination by persons not yet members. As it is impossible to review all these, I shall only mention:

Propagation of light.

1. Researches on the velocity of light, by Mr. Arago, now member of the class, who has proved, that this velocity is the same, whether it come directly from the Sun or stars, or from a fire kindled on the Earth, or by reflection from the Earth, a planet, or any terrestrial body.

Fire-engine.

2. A fire-engine by Mr. Cagniard-Latour, who has made in it a very happy and inverse application of the screw of Archimedes.

Electrochemical inquiries.

In the physical department of the class the most prominent are the researches of Messrs. Gay-Lussac and Thenard in the brilliant career first opened by Mr. Davy; and though these gentlemen do not appear to contemplate every fact with the same eyes, the progress of science cannot fail to be promoted by the discussions that arise between them.

We

We are likewise indebted to Mr. Gay-Lussac for observations on the combinations of gaseous substances with each other, intended to show, that they always unite in simple ratios. Combinations of gasses.

These observations are followed by a separate paper on nitrous vapour, and on nitrous gas as an agent in eudiometry. In this we see clearly the influence of quantities on the result of combinations. If two parts of nitrous gas and two of oxigen be mixed, nitric acid is produced, and one part of oxigen remains free. If on the contrary four parts of nitrous gas and one of oxigen be mixed, nitrous acid is produced, and one part of nitrous gas is left free. And, as nitrous gas is composed of equal parts of oxigen and nitrogen, we know the constitutions of the two acids with precision. Compounds of nitrogen.

Mr. Guyton de Morveau, in a series of experiments on the diamond, and substances that contain carbon, found that water was decomposed by the diamond at a very high temperature, and carbonic acid produced. Water decomposed by the diamond.

Mr. Sage has imparted his researches on the revivification of silver by mercury in the nitrate of silver; on an acetate of ammonia obtained from wood by distillation; on the analysis of the calcareous stone named typographical; on the magnesia contained in shells, madrepores, limestone, and arragonite; on an arenaceous iron ore; on an unknown petrifaction; and on a cupreous and ferruginous petrified wood. Mr. Sage.

(To be continued.)

To Correspondents.

A Constant Reader may find many of the articles he mentions in vols. XVIII, XIX, XX, XXII, and XXIII; others will be inserted as opportunities occur. His concluding suggestion will be considered.

METEOROLOGICAL JOURNAL,

For APRIL, 1811,.

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of MAR.	THERMOMETER. 9 A. M.	9 P. M	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
29	46°	46°	55°	39°	30.55		Fair	Fair
30	40	47	56	47	·37		Ditto *	Ditto*
31	47	45	48	43	·19		Cloudy	Cloudy
APR.								
1	46	47	54	40·5	·08		Fair	Fair
2	45	52	58	44	29·87		Ditto	Ditto
3	50	55	61·5	48	·96		Ditto	Ditto
4	52	50	60	43	30·05		Ditto	Cloudy
5	44	42	49	36	·12		Ditto	Fair
6	39	50	55	39	29·92		Ditto	Cloudy†
7	40	38	41	34	·48	·075	Snow	Ditto
8	36	39	42	32	·54		Ditto	Ditto
9	37	41	48	32	·64		Fair	Ditto
10	38	43	51	39	·83		Ditto	Rain
11	41	42	49·5	38	30·00	·025	Ditto	Fair
12	46	46	49	39	·19		Cloudy	Cloudy†
13	50	55	59	52	29·98	·220	Fair	Ditto
14	55	58	63	52	30·18		Ditto	Ditto†
15	65	55	59	52·5	·18	·120	Ditto	Fair
16	56	59	59	47	29 96		Rain	Cloudy†
17	49	51	59	46	·85	·010	Fair	Ditto
18	51	50	58	47	·23	.160	Rain	Fair
19	50	53·5	59	48	·20	·055	Ditto	Cloudy
20	54	54	60·5	51	·30	·065	Fair	Ditto†
21	56	56	62	53	·42	·130	Ditto	Fair
22	56	58	65	55	·64		Showery	Cloudy
23	59	62	70·5	54	·60	·015	Fair	Fair§
24	58	60	68	53	·76		Ditto	Ditto§
25	57·5	56	62	52	·81		Ditto	Cloudy§
26	55	56	63·5	47	·76	·015	Ditto	Fair
27	56	57	65	44	·59		Ditto	Ditto

·890 Inch. since last Journ.

* Intervening Fogs. † Rain in the Night. ‡ Boisterous at 12 with rain. § Lightning, about 9 P. M.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1811.

ARTICLE I.

Description of a Method of Roofing Buildings securely with Flagstones. By RICHARD LOVELL EDGEWORTH, *Esq. F. R. S. M. R. I. A.*

To Mr. NICHOLSON.

SIR,

I Had occasion some time ago to roof a large building in an uncommon manner. I send you an account of it; as it has succeeded; and, as I believe, it may be useful in many places where slates and tiles are not to be had.

The gaol of Longford, in Ireland, which was built about twenty years ago, was covered with a circular arch of bricks; upon which broad flat stones, commonly called flags in this country, were laid with the best mortar that could be procured; these thin stones or flags were placed side by side; the lateral joints were filled up with mortar; and all the courses, as they descended, lapped over each other about two inches. After a short time the sun and frost cracked the mortar between the joints, and the rain found a way into every part of the building.

Gaol of Longford roofed with flags.

Penetrated by water.

Attempt to prevent it by a cement,

One of those *men of practice*, as they are called, from having been employed practically in building, undertook for forty or fifty pounds to remedy the evil, and by a *curious* cement to render the roof impervious to water. He laid on his hot cement of resin, and wax, and brick-dust, &c. The first summer shower passed off without penetrating through the joints, and the undertaker received his money; but in a short time things were as bad as ever, and the miferable creatures under confinement were drenched with rain and snow in every part of the prison.

did not succeed.

Expense of a lead or copper covering,

In the year 1809 the Grand Jury of the county of Longford desired, that I would endeavour to staunch this roof at any expense, that might be required. I received proposals for covering it with lead, and with copper: this could not be done for less than seven hundred pounds.

and of slating.

I then proposed to bolt down rafters upon the brick arch, so as to form a polygonous roof, upon which slates might be laid in the usual manner; but this I found would cost above four hundred pounds. It then occurred to me, that the flags on the roof might be so ordered, as to effect the intended purpose.

Management of the flags themselves to keep out the wet.

I took off a portion of the flags in fine weather, and without removing them from the top of the building I had them cut in the following manner; the flags (*a a*, Pl. III, fig. 3) were about three feet long, two feet or two feet six inches broad, and two inches and a half thick. The upper course was of fine even flags four feet broad, and each of considerable length, and under this course the roof was secure every where, except between the lateral joints. To prevent the rain or snow from penetrating between the upper and under courses or horizontal joints of the flags was the first object. For this purpose a groove was cut an inch deep in the surface of the upper part or top of those flags that were next the eaves; this groove was cut within one inch of the top of the flag. A similar groove was cut in the under side of the next course that lapped upon the lower course, and so on from the eave to the ridge; so that the upper flag or stone could hook upon the under one, as may be seen in the section, fig. 1. Pl. III.

The lateral

The next object was to secure the lateral joints. To effect

effect this purpose, grooves were cut into the upper surface along each side of every flag three quarters of an inch deep at one inch from the edge, see fig. 2 and 3, where a section or profileis given. To cover these lateral joints caps of lead were laid from the ridge to the eaves, a cap for each flag, or rather for every pair of flags. These caps, which had the appearance of a bead, were fastened over the rabbets or grooves of the flags by copper nails, *c*, driven through the caps into the juncture between the flags. These nails were made fast by slips of sheetlead *d d* fig. 3, put between the stones. A representation of the full size of the grooves in the stones of the lead cap and mushroom nailsis given, fig. 3. Joints covered with lead.

Where holes were made through the lead caps, the water might find a passage; but this was prevented by preparing the holes in the lead in such a manner, as to stop the water above the hole, and to turn it aside from the direction which might be hurtful. The caps before they were laid in their proper places were turned upside down, and where nails were to pass, a burr or button, *b*, *b*, was punched in the lead half an inch deep, and by a proper tool passing through the punch, a hole was made in the centre of the button. The cap, when put into its place, covered the ridges of the flags between the grooves, so that no water could find an entrance between the joints of the flags; nor could any water rise above the tops of these buttons, because the descent of the roof would carry it off. Besides, the button or burr was covered by a *mushroom-headed nail*, the rim of which entered a little into the lead round the burr and prevented small particles of snow from gaining admittance. This effectual.

The holes in these caps might have been closed by solder; but whenever any work that is of difficult access is to be performed, it is always advantageous to have it executed by some one workman, in a manner that requires no difficult art or complicated apparatus. And I find that not one drop of water has penetrated through these joints during the two winters that have passed since they were covered according to this plan. Simplicity advantageous.

At the commencement of the business many difficulties with respect to the scaffolding were started. Very long ladders were requisite. Cripples hung on iron staunchcons Difficulties with respect to the scaffolding in the wall were deemed insecure; and the country workmen trembled at the idea of being perched so high from the ground without any apparent protection. I constructed eight light ladders, each six feet long; these were wider at one end than at the other, so as to permit them to be joined together by small bolts passing through the ends of both ladders. The ladders, thus joined, applied themselves commodiously to the circular roof, they were hung across the top and fastened by ropes, passing over the ridge of the roof to the iron bars of the windows of the upper cells on the opposite side of the gaol, which happened to be empty. On these ladders movable cripples were placed wherever a scaffold was wanting: on these cripples, which extended six feet from the roof, strong planks were laid, with ledges to prevent their slipping sideways; round this scaffold a coarse substantial handrail was tied. The passage to this scaffold was through a large opening in the top of the roof whence the workmen descended down the ladders to the lower platform, and thence to any part of the roof*.

surmounted.

Expense.

The scaffolding of this work cost but fifteen pounds, and the repair of the roof, exclusive of some other work that was carried on at the same time, came within one hundred and forty pounds.

As I may not have an opportunity of mentioning it in another place, I hope that you will excuse me for inserting a circumstance relative to this gaol, which is certainly not connected with the immediate subject of this letter; but as it

* The moment the scaffold was finished, I went upon it myself, and from that time no objections were made. Notwithstanding all the precautions that had been taken, a fatal accident threatened the lives of the workmen, It has been said already, that the ropes which held the ladders were tied to the bars of the upper cells of the gaol. One morning, towards the close of the business, the principal workman found the ladders, and the scaffold that was attached to them, giving way. He had sufficient presence of mind to throw himself off the scaffold on the roof; as he was near the top, the slope of the roof was not sudden. He could therefore stick there till his companions relieved him.

The cause of this sudden failure it was impossible to foresee. A mad woman had been accidentally put for a single night into one of the upper cells; there by moonlight, with that mischievous alacrity which is often the accompaniment of insanity, she untied the cords, and left the scaffold without support.

it relates to public buildings of all sorts, it cannot be without some general interest.

Documents placed under the foundation for posterity.

In laying the first stone of the gaol of Longford twenty years ago, I placed in a cavity sunk in a large stone, under the S. W. corner of the building, several tiles, upon which, before they were baked, there were inscribed various memorandums for poſterity, the Greek and Roman alphabets, the latitudes and longitudes of Paris and London, the variations of the needle, the nature and dates of various inventions, of gunpowder, of printing, of the steam-engine, of iron bridges, of the balloon; some of the discoveries of chemistry, and several remarkable events, with the names of celebrated books, and of their authors.

This recommended on other occasions.

If this were done in various places in Europe, it might hereafter be not only gratifying to future curiosity; but might be useful to mankind. We have reason to believe, that fictile compositions are among the most durable substances that exist, and as we may, with the greatest ease, inscribe what we please on them before they are baked; it is but a small sacrifice to posterity, to give up an hour or two of leizure, from a hope, however feeble it may be, of preserving some of the discoveries, which have hitherto been made in art or science. Swift tells us, that a shrewd fellow inquired, why we did so much for posterity, when posterity has never done any thing for us. It is true, that posterity *has* never done any thing for us; but the idea of a posterity, that can bestow posthumous fame, has ever been and ever will be an excitement to present exertion. Our own immediate descendants reap the harvest which we sow, and nothing is more natural or more laudable than a wish to preserve our names among those who have been benefactors of society.

Posterity.

Edgeworthtown, Ireland,
the 17*th of April,* 1811.

II.

Method of making any Ship's Boat a Life boat, to preserve the Lives of the Crew in imminent danger; by the Rev. James Bremner, *Minister of Walls and Flota, Orkney Islands**.

Case of Shipwreck.

HAVING a great many years ago witnessed a melancholy scene of shipwreck, and seen men perishing at little more than the distance of one hundred yards from the shore, it forcibly struck me, that though there was no possibility of getting from the shore to them, yet there was a great probability that means might be found, by which those in such situations might with safety be enabled to effect their escape to the shore; and farther considering, that the very precarious aid of some accidental piece of wreck (under every disadvantage and in a tempestuous sea) sometimes serves to save life, I was confirmed in the opinion, that some method might be devised, which, upon good grounds, would hold forth the promising prospect of safety in all the common and general cases of shipwreck. Hence it was, that to devise such a scheme became the object of my research ever after.

Plans for saving persons shipwrecked.

The following plans (especially the first) are so simple, and the effect so obvious, that I cannot allow myself to think that any seaman can entertain the smallest doubt, but that a boat so prepared would live in any sea whatever, could neither sink nor overset, and could carry in safety a number of people, in proportion to her size, over a bar, or from the wreck to the shore through any surf.

Buoyancy of empty casks.

That empty casks must float, almost wholly above the surface of the water, is so clear, that no person can be so absurd as to question it; and it is equally certain, that every cask will support weight of any kind in proportion to its size. In order then to accomplish the end proposed, there is only one thing more wanted, and that is, by means of sufficient seizings or holdings, to secure the casks in their places. Were you to tell a seaman, that he is not master of

* Trans. of the Soc. of Arts, vol. XXVIII, p. 135. The silver medal of the Society and twenty guineas were voted to the author.

this

this mighty operation, it is easier to conceive than to express the contempt he would feel, and the energetic reply he would probably make to such a supposition. If then these are undeniable points, it must follow, that wherever the boat can be had recourse to, all that is contended for in the plan must be granted.

It no doubt has been upon these simple and obvious principles, that those corporate and public bodies, and hundreds of seamen to whom the plan has been communicated, have so readily and entirely approved of it. But however respectable and authentic these testimonies (afterward to be mentioned) may be, I lay no stress upon that point, neither do I ask any credit for it, but freely submit my statements to the great body of seamen in general, leaving them to be judged of, not with liberality only, but with severity, considering that it would be a crime of the first magnitude, to advance a single argument or suggestion, that could have the smallest tendency to mislead, in a matter so solemn and important as where life and death are concerned.

Were I to go back to cases that are well known to have happened, I could easily point out many, wherein had this plan been thought of, there can be no doubt but it would have been attended with the happiest consequences; and probably the recollection of many seamen may furnish cases of the same kind, which have happened within their own knowledge.

I shall only add, that I expect no benefit or advantage whatever to myself from my perseverance and labours on this subject, nor reimbursement for an expense of some hundred pounds which it has cost me in repeated journies to Edinburgh and London, as well as in experiments, which a living of less than seventy pounds a-year could very ill afford; but I shall nevertheless reckon myself amply rewarded, if what I have to propose shall at any time, or in any case, prove the means of relieving from the deepest distress, and of rescuing from otherwise inevitable death, even a few of those who have had the misfortune to be involved in all the horrours of shipwreck.

Mariners are unavoidably exposed to incomparably greater Hardships and

hardships

sufferings of mariners.

hardships and sufferings, than are to be met with in any other line in human life.

While the labours of all others are moderate, and find relief at stated intervals by day, and repose by night, the seaman must contend with the storm so long as it lasts, and encounter danger at a moment's warning, whether at mid-day or midnight. Whilst the tempest rages, no respite can be allowed him; he must keep his station without intermission, and after toiling above strength and above measure, it is often his hard fate to be shipwrecked at last.

The complicated distress attending this frequent and fatal disaster it would be in vain to attempt to describe in any words; nor is it possible to conjecture nearly the number, which is added annually to the innumerable multitude of dead which the ocean contains.

Sometimes several hundreds in one ship are involved in this direful calamity, where the misery of each sufferer is increased, in proportion to the accumulated wo that surrounds him; the cry of despair is heard on every side, and in distraction each exclaims, What shall we do?

Amidst overwhelming waves and wreck, the mariner suffers in his person all that a living man can undergo, and in his mind all the anguish that despondence can create, heightened by the agonizing thought, that he is never more to behold wife, child, family, or friend; still however amidst all his sufferings an ardent love of life prevails, and the hapless mariner, struggling hard to preserve it, clings to whatever seems to promise a momentary reprieve.

In the mean time the wreck is rapidly giving way, some are washed away in one place, and others in another; those who remain redouble their efforts for life; but alas! they strive in vain; one decisive blow has dashed their last and only support to pieces, and all are going down together—a general shriek is heard—to be heard no more! the melancholy scene has closed, and neither survivor nor wreck is left behind.

Any plan then that has for its object to afford relief in situations of such extreme distress, and which seeks to extend the same benefit to thousands of perishing men in future

future ages, will no doubt meet with a favourable reception from every humane and benevolent mind.

But humanity and true benevolence are not merely speculative, but active principles; and wherever they really exist, the helping hand is instantly stretched forth, to execute the dictates of the feeling heart.

True humanity an active principle.

As no subject can be more interesting to individuals than the present, or more important to society, may it not then be expected, that every friend to humanity and to his country will not only heartily wish success to the present plan, but also lend his best assistance to have it brought into all the practical effect, of which it may be found susceptible?

It is to be understood, that the plan is intended to apply to cases of shipwreck in general, and that it may very often succeed even in cases of extraordinary difficulty and peril.

This will comprehend the far greater number of all shipwrecks that happen, and the author thinks himself warranted to say, that no solid objection can be offered to the effectual operations of his plan to this extent, and that it will be found fitted to answer all the purposes of a life boat, by saving lives, where otherwise men must inevitably have perished.

At the same time he begs it may be understood, that he does not speak with this confidence from his own opinion only, however well-founded in principle and experiment it may be, but because the plan itself, after repeated investigation, has received the unanimous testimony and approbation of professional men, and of men too who must be allowed to be the most competent as well as the most respectable judges in the kingdom, namely, the Trinity House of Leith, in whose records a copy of it will be found.

The plan approved by competent judges.

The Report of the Highland Society of Scotland confirms, that in their Committee appointed to witness the experiment at Leith there were naval men of that number who were competent judges, and in whose skill they could confide, and for this reference is made to the Appendix of their second volume.

It has been repeatedly submitted to the Trinity House of London. It was first submitted to them by Lord Melville, the treasurer of the navy, and their answer under the hand of their

The plan approved by competent judges.

their secretary is inserted in the forementioned Appendix, signed James Court.

In the next place, the plan has been laid before the Royal Humane Society, and they, not being naval men, do submit every essay of that nature to the Elder Brethren of the Trinity; and in consequence of their approbation a premium of five guineas was given by the R. H. S., as appears from their printed Reports 1800 and 1801.

And to these attestations might be added the subscribed approbation of more than one hundred ship masters, whom the author had occasion to see only accidentally, and whose subscribed names are now in his possession.

It is under the sanction of such authorities and documents, that it is now offered to the public, and they are such as must be satisfactory to every impartial and candid mind.

They have been obtained without interest, favour, or friend, and small premiums have been given without the author's knowledge, till informed by letter that his plan had received this mark of approbation.

It is impossible therefore to ascribe so honourable testimonies and gratuitous bounties to any other motive than to the conviction of the utility and efficacy of the plan, and an ardent desire to promote an object so devoutly to be wished, as the preservation of lives in cases of shipwreck.

The inventor trusts his statements will show, that he is not unacquainted with his subject: and he shall only add, that he has had more than forty years experience in the use of boats, among dangerous tideways and rapid currents, such as the Pentland Frith, and all the other channels among the Orkney Islands; and that he has been several times at sea on shipboard, in storms that were attended with shipwrecks; and that from such experience he is perfectly convinced, that his plan is sound and unexceptionable, and is confident that the period is not very distant, when it will come into as great repute and general use as lifeboats, properly so called, are now known to be.

The plan may be executed upon boats of all dimensions, and the largest, provided they could be got out, would be found the most advantageous: but, all circumstances considered, the size deemed in general best adapted for the purpose

purpose would be any boat from sixteen to twenty feet in length, which is to be prepared as follows.

Reference to the Plan of the Rev. Mr. Bremmer's Preparation of Ship Boats as Lifeboats, Pl. III, *fig.* 4 *and* 5.

Two additional ring-bolts are to be fixed in the keel withinside of the boat. One to be placed one third of the boat's length from the stem. The other one third from the stern. Two auger bores are to be put through the keel withoutside, and close to the garboard stroke. One of these bores to be put about half way betwixt the ring in the stem, and that next to it in the keel. The other about half way betwixt the ring in the stern, and that next to it in the keel.

Preparation of a ship's boat to be used as a lifeboat.

Plugs may in ordinary be put into these bores, to be struck out, when occasion requires.

Those ring-bolts which are in ordinary in every ship's-boat, the two additional ring-bolts in the keel, and the two augur bores, are all intended as secure points of fixture, to which seizing ropes are afterwards to be attached.

Casks.

In the next place, two tight empty casks, (see fig. 4.) are to be provided, of such dimensions that their length may fit to the width of the boat, when laid athwart ship, and their diameters to be about three feet, and if larger so much the better.

Each cask must be furnished with a sling on each end, and each sling to have two eyes on it, about six inches asunder, and the slings so put on the cask as that the eyes may be on the upper side when laid into the boat, that the seizing rope may pass through those eyes, in their way from ring-bolt to ring-bolt.

One of these casks, so prepared, is to be laid in forward, and the other aft; and each cask so near its respective ring in the keel, as only to leave sufficient room for passing the seizing rope through the ring in the keel.

By this means the vacant space, to be then filled up with cork, will be left betwixt the cask and the bow forward, and betwixt the other cask and the stern aft.

Cork.

The requisite quantity of cork, according to the dimensions of the boat, and the quality of the cork, may be about

a hundred

a hundred and a half, or two hundred weight, for each end of the boat, and that for each end ought to be made up into two separate bundles, each bundle being fitted to the width of the boat, and the uppermost one forming an arch from gunwale to gunwale.

The cork is to be made up in canvas, done over with soft pitch for preservation, and each bundle marked and numbered according to its place.

The casks and cork being laid into the boat, seizing ropes are then to be applied for securing them in their places.

Method of securing the casks and cork.

Here it is to be observed, that the single turn of rope which is to go through the augur bore in the keel and round all, should be the first made fast, that the other seizing rope (which we shall suppose to have been made fast to the ring in the stem) may, in passing through the eyes on the sling, take in the surrounding rope betwixt the two eyes, which will thereby prevent the surrounding rope from slipping to either side of the cask.

The seizing rope, having passed through the eyes on the sling, is then to be passed on through the ring in the keel, and thence back again in the same manner, through the eyes on the sling on the other end of the cask, to the ring in the bow; and lastly, the seizing rope is to be brought directly from the ring in the stem to the ring in the keel, by which it will cross the cask at the bung or middle part of it: the other cask and cork aft are to be secured in the same manner.

The preparation will be completed by attaching a bar of lead or pig-iron, of about two hundred weight, to the keel within side, by means of the ring-bolts in the keel or otherwise.

Variation in the plan.

The same plan may be executed with equal effect, and nearly with the same expedition, by the following alteration and arrangement.

Instead of one large cask, two less ones may be used in each end of the boat.

These are to be laid in lengthwise, fore and aft, in the boat, alongside of each other, and both together ought to fill the width of the boat.

These must also be furnished with slings on each end, and

and with two eyes on each sling, and these eyes so placed as to be about two inches above the horizontal diameter of the cask, one eye being on each side of the cask when the sling is put on.

The seizing-rope, being now made fast to the ring in the stem, is to be passed through the eyes on the slings on one side of the cask, then through the ring in the keel, and so back again through the eyes on the slings on the other side of the same cask, to the ring in the stem. The rope is then continued on till it has passed in the same manner on both sides of the adjoining cask, and the last turn is to be made directly from ringbolt to ringbolt, passing over and above the surrounding rope, which will thereby be brought down in the middle betwixt the twô casks, and made closely to compress them on each side.

The same process is to be followed as to the casks aft, where the dimensions of the boat will admit of it, and where otherwise one large cask athwart ship may be used, as in the plate, fig. 5. It was in this manner that the experiment at Leith, hereafter to be detailed, was made, and all the cork that was used on that occasion was about one hundred weight put into the narrow part of the boat aft, in order to raise a common porter cask placed above it to a convenient height. The preparation of the cork bundles in this case will differ somewhat in their shape from those in the former plan, but as the purpose of them is the same, namely, to fill up the vacant spaces betwixt the cask and the boat, a particular description of them seems quite unnecessary; only it may be observed, that as the diameters of the casks forward are considerably less than that in the former plan, so much of the cork ought to be placed underneath, as may serve to raise the upper side of the casks about four inches above the gunwales, it being evident, that the higher they can be raised with sufficient security, the more effectually all possibility of overturning will be prevented.

The same quantity of ballast is to be used in this case as in the former, and is to be applied in the same manner.

With respect to boats of small vessels, a single cask forward and another aft, without any cork, will be sufficient. Boats of small vessels.

Each cask to be about the size of a hogshead, and to be set

set on end, or leaning obliquely towards the rings in the stem and stern, to which they are to be secured, and at the same time to two other rings placed in the keel, proper for that purpose: these casks, from their position and power, would effectually prevent sinking or upsetting: and as the crews of such vessels are few in number, their boats might support them safely through any breach into shallow water.

Advantages of timely preparation.

The foregoing plans are founded upon unquestionable principles, and constructed according to a regular method. They keep in view the difficulties to be encountered, and provide against them by making a few necessary preparations in due time. Were this attended to, all the confusion and embarrassment which arise from sudden alarm, and the distress that must attend a total want of suitable means, would be prevented, and an encouraging prospect of safety held out even in the most perilous situations.

The want of timely forecast, and the neglect of means that were in our power, never fail to occasion the bitterest self-reproach, and the most painful vexation, whenever we are overtaken by misfortunes, which a little prudence might have prevented.

A third plan suggested.

Having however but too much reason to apprehend, that such prudential provisions as have been stated will still be neglected, in spite of every suggestion and consideration that can be urged, I shall now propose a third plan. Though inferior to the former, as a ship with jury masts, torn sails, and a temporary rudder, is to one in perfect good condition; yet, considering that this inferior plan, like the disabled ship, may gain what was despaired of, and save what was given up for lost, I proceed to state it:

Casks alone.

This plan will consist in the application of casks only. These, if stowed closely and so as to fill up as well as possible one third part of the boat forward, and one third aft, would effectually prevent the boat from sinking or oversetting.

Upon this plan, in order the better to secure and combine the casks, the end of a sail should be in the first place thrown into the bottom of the boat, and the casks being stowed upon it, the other end of the sail should then be doubled over all: the seizings are then to be made through

holes

holes struck any where through the bottom and sides, wherever the passing of a rope may be found necessary, or of any use for confining the casks.

The constant and general idea, that the utility of every boat depends upon the tightness of her bottom, and her completely resisting the admission of water, opposes itself strongly and almost irresistibly to the directly opposite idea, that water freely admitted could do no injury; nay, so strong is the received opinion, that it may be very difficult to persuade some, that large openings in the bottom would prove a real advantage; it is however undoubtedly true, that in the present plan this would really be the case.

Holes in the bottom of a boat advantageous.

It is therefore very material to observe, that neither the number nor the size of the holes struck through is of any consequence, as to the water in the boat; on the contrary, they would be so far from being detrimental, that, to a certain extent, they would be of advantage, as they would serve to discharge, in proportion to the buoyancy contained, whatever top-water might be withinside, above the level without, and which the boat would otherwise retain as a load and dead weight, if she were every where perfectly tight: whereas, in proportion as the buoyant power operated in raising her, the top-water would instantly subside through the holes in the bottom, and thereby render her more lively, and to swim higher out of the water.

From not attending sufficiently to the fact now stated, it has probably happened, that the plan we are at present describing has never been attempted; but whoever will take the trouble to consider the matter a little may soon be convinced, that they may, *without scruple or hesitation, make as many holes, and of whatever size,* as they may judge necessary for passing ropes, wherever they can serve for effectually securing the casks in their places.

The only point chiefly to be attended to is never to attach ropes to any tender part of the boat, such as the gunwales or thwarts, but to such parts as possess the greatest strength, and in which entire confidence may be placed.

The fastenings to be applied to the strongest parts.

As the largest boats have strong timbers, this plan might probably succeed best if applied to launches and long-boats.

Small anchors that have iron stocks, and which could be laid

Ballast.

laid in the bottom of the boat, would serve for ballast, though probably ballast in large boats would not be very necessary.

Holes.

The holes to be struck through may be pierced with a marling-spike and mallet betwixt the timbers.

Buoyancy of casks.

The power and effect of empty casks is well known, the application of them being a common expedient, used almost every day for the purpose of floating stranded or bilged vessels of great burden. How easy then it must be, by the same means, to render a boat buoyant to any degree that could be wished, may be abundantly evident to every person not obstinately blind to undeniable fact.

The thing is so self-evident as to require no proof, that, if both ends of the boat be tolerably filled with empty casks, she will not only thereby be secured against upsetting or sinking, but will be rendered extremely buoyant, provided the casks be effectually secured in their places; and in full proof of this fact, the experiment hereafter to be narrated was made almost entirely with empty casks.

The inventor having little hope that the far better and more eligible plan by timely preparation will be adopted, is the more solicitous to gain attention to this third mode, by means of casks only, because necessity, which is often the mother of persuasion as well as of invention, may compel the unfortunate mariner to have recourse to it.

Seamen being above all others expert in the use of ropes, and expeditious in making secure seizings, which is the great and only thing wanted, the inventor begs leave confidently to affirm, that whenever it shall be tried it will be found perfectly safe and successful.

Let therefore no scruple or hesitation be made in striking holes through the boat, any where, and of any number or size that may be found necessary for passing ropes for the effectual confinement of the casks. This plan will apply *not to one boat only, but to every boat in the ship,* provided there be a sufficiency of casks on board.

If then the two great points upon which I set out, namely, the powerful buoyancy of casks, and the peculiar expertness of seamen in every operation where ropes are to be used, be duly considered, they will sufficiently vindicate and

and verify all that I have stated; and unless the one or the other, or both, (that is, the power of casks, and expertness of seamen) can be shown to be false assumptions, the conclusions which I have drawn can neither be denied nor resisted.

Observations and Remarks relative to the foregoing Plans.

1.—From the detail in the description it may be alleged, that the situation would not admit of so much time as the preparation would require. Observations. Time.

It is granted, that in some cases this might be true, if nothing had been done before-hand; but surely such neglect ought by no means to be imputed as any defect in the plan, but ought to be ascribed to its true cause, the remissness of those who would give themselves no trouble to avail themselves of it.

Slings fitted to the casks, two additional ring bolts, two auger bores, and the requisite quantity of cork, are all things so trivial and so easy to be provided, that to be without them must appear an unpardonable neglect; and, if these were in readiness, the short space of ten minutes would be quite sufficient for laying them in their places, and securing them.

It is evident to demonstration, or it might be easily proved by experiment, with respect to the first two methods stated, and where the necessary provisions had been made, that the whole could be executed in ten minutes, and therefore any objection in point of time can have no place.

2.—When there is a prospect of the ship holding together for some time, the boat may be kept in readiness and in reserve, or may be served on shore by a rope, and hauled off again, as often as occasion may require; and if to be hauled off, it might be a necessary precaution to pass a rope round her lengthways to assist the ring bolt in the bow, and in every case the attachment and connection of the boat with the vessel ought to be well secured till the moment she is to be cast off for the shore. The boat may be served on shore by a rope and hauled off again.

May be got into the water any how.

3.—It is of no consequence in what manner this boat is to be got into the water, whether after-end or side, by means of handspikes or otherwise, as no water can hurt her, though it might be more desirable, if it could be done without filling her in midships, as in that case she might be conducted through very heavy seas without filling at all, or receiving more water than might be easily baled out.

No matter how deep the men are in water.

4.—It is material to remark, as it may not generally be attended to, that the plan always supposes the midships to be full of water; but that the requisite buoyancy of the boat is not injured by that circumstance, nor will the addition of people, in so far as they are immersed in the water, prove any additional burden; this will be perfectly clear to all who understand this part of the subject, however improbable it may appear to others, and the remark serves to show, that it would be a good rule, in such circumstances, for the men to keep themselves immersed in the water in midships as far as possible.

The idea of placing men in the midships of the boat, while at the same time it was full of water, would probably startle a landsman not a little; such therefore may be told, that every lifeboat is supposed full of water, and that to imagine there could be any man in one with a dry thread about him would argue a total ignorance of the matter.

5.—It is to be kept in mind, that the danger is always supposed to be extreme, and that the present plan affords the only possible chance of saving life; therefore whatever hardship or difficulty there may be in putting it in execution is entirely out of the question; any other view of the subject is altogether foreign to the purpose.

Casks superior to cork.

6.—If any are of opinion, that cork ought alone to be used for buoyancy, there can be no doubt of its answering the purpose perfectly; at the same time the author is of opinion, that a combination of cork and casks would be found more convenient, and in some respects preferable.

Water casks would always be at hand, and, to save the expense of cork, might on that account be preferred by some; but independent of this consideration, casks are by more than one half lighter than their bulk of cork, and thereby

thereby more than a double advantage in favour of buoyancy is gained by using them.

There is but one objection to the use of casks, and that is, that they may be stove in; but if the great strength which they possess from their construction be considered, and at the same time that they are strongly defended by the boat, this objection must appear of no moment at all.

7.—Every boat, prepared as has been stated, is fit to carry men equal in weight to something more than one third of the boat's whole burden, and one of eighteen feet in length can carry from fourteen to sixteen people, and have sufficient room for working a pair of oars, which ought by all means to be short ones.

Proportion of men a boat will carry.

Oars, if any, should be short.

The disadvantage of working long oars upon a low gunwale, and in a high running sea, is too obvious to need any thing more than to be just mentioned.

8.—As all depends upon the points of fixture, too much attention cannot be paid to their sufficiency, and though those stated in the plan are judged to be perfectly adequate to the purpose, yet any person, wishing for more, *may add them at pleasure*, by rings of rope in the stem and stern-posts, as in the Greenland boats; by more rings in the keel; or, in addition to the seizing ropes, a netting of small rope may be made to cover the whole foreward, and another such may be applied in the same manner aft, and by these means every possible security that can be desired may be obtained.

The fixing should be secure.

9.—It is material to observe, that no dependence ought to be placed on seizings connected with the thwarts or gunwales, unless it were only as aids to the points of main dependence. The gunwales, more than any other part of the boat, are liable to damage, and may very possibly be injured in hoisting out, or before getting clear of the vessel.

The thwarts & gunwales unsafe for this.

The two auger bores in the keel are infallible holds; easy access may be had to them while the boat is on deck, and a rope may be passed through them in a moment. This seizing, beside the security it affords for confining the buoyancy, adds considerably to the strength of the boat, and

The auger holes in the keel secure.

and therefore ought to be preferred to any other mode of fixture.

10.—No rule can be laid down that will fit all boats, as to the precise quantity of cork, or size of casks, their shape and dimensions being so various; but from the general rule that has been stated, and the purpose to be served, every man may easily adjust his apparatus to his boat, or make such little alterations in the boat as may be found convenient or necessary.

A sail advantageous.

11.—No sail can hurt this boat, as it is supposed she has only to go right before the wind, and therefore a sail may be used with very great advantage. This would render oars unnecessary, and would be infinitely preferable. It is almost needless to add, that the boat could be steered in midships.

Disadvantages of the common lifeboat.

12.—The great benefit derived from the common lifeboats is well known, and universally acknowledged; but they are very far from being adequate to the calamity they are intended to remedy. Their number comparatively is very few, and the sphere of their operations extremely limited. In darkness by night, and in thick snow by day, when their aid is most wanted, they are of no avail. Storms may blow, and sometimes have blown so hard as to defeat their utmost exertions; and even in the most favourable cases, they require a considerable time before they can reach the wreck; in the mean time the vessel may be dashed to pieces, and all hands lost.

Superiority of the present.

The very preeminent advantage of the shipboat in these and several other respects is very conspicuous. This boat is wherever the ship is, and recourse may immediately be had to her; is of equal utility by night as by day, and in the thickest as well as in the clearest weather; and while the lifeboat, with extreme slow progress, must be impelled against wind and sea by a force superior to both, the shipboat has only to drift with ease before the storm.

The principle the same in both.

13.—As it may serve to gain confidence with those who are not otherwise qualified to judge of the plan, it may be observed, that the shipboat is prepared upon the very same principles as the lifeboat, and that these principles are applied to greater advantage in the former than in the latter.

The

The quantity of buoyancy in the shipboat, being considerably more in proportion to her size, and being carried to a greater height, gives more security against oversetting; and if to these advantages there be added the far greater one of having only to drift before wind and sea, no shadow of doubt remains of the success of the shipboat over that of the other.

Lastly.—This plan carries with it the very strong recommendation of private interest as well as of public utility.

The plan advantageous to private interest,

Suppose a ship to be riding in an open bay or roadstead, a storm comes on, and, if in winter, a long dark night is soon to follow. In this situation the mariners, being extremely doubtful whether the vessel could hold it out over the night, and terrified at the awful prospect of being thrown, as it were, blindfold into the most perilous of all situations, the determination would most undoubtedly be to cut and let the ship run on shore while there was light, as giving the only chance for saving life.

The same determination may be taken in hopes of escaping by favour of a falling tide, and in both cases lives, ship, and cargo may be all lost, as has certainly very frequently happened. Whereas could safety be ultimately relied upon from the boat, the ship would be allowed to ride so long as anchors and cables could hold her; and in the mean time the storm might abate, the wind might shift, or her tackling might prove sufficient to ride out the storm, and thus lives, ship and cargo would all be safe.

as it would encourage men to stay by a ship in danger.

In every situation the prospect of safety by means of the boat would prevent every precipitate measure, and encourage men to make those exertions for saving ship and cargo, which are not to be expected from men despairing of life.

In the foregoing plans there is nothing that can be reckoned complex, nothing that requires nice adjustment, or of doubtful and precarious effect. They are unquestionable in principle, simple and easy in execution, and absolute in security; and if the necessary previous preparation, which is very little, has been made, they will be found as expeditious as any emergency can require. They have been proved by experiment as far as circumstances would permit,

It is simple, & unobjectionable.

mit, and have received the unqualified approbation of naval men of the greatest experience, and of the first respectability.

These are the solid grounds upon which they are offered to the public in general, and most earnestly pressed upon the attention of seamen in particular.

The plan having been commmunicated to hundreds of seafaring men, they have always given it their ready and entire approbation; hence it is hoped, that every seaman from his own knowledge and experience, without any doubt whatever, will, upon considering the subject, be fully convinced in his own mind, that the scheme is perfectly practicable, and if adopted, would be attended with the happiest effect.

Substitutes and Expedients which may be found useful.

Substitutes. 1,—If so much cork was made up in canvas as would serve to go quite round the boat withoutside, and reach from the top of the gunwales to about fifteen inches downward, and of one foot in thickness; the same might be attached to the boat, and would render her extremely buoyant. This, together with ballast, and a small quantity of cork withinside, would produce a perfect lifeboat, upon almost the very same plan as the present lifeboats.

The cork might be made up in so many separate parcels, (netted in small rope,) as was found convenient to be attached to a strong rope going round the gunwale, and to another such which ought exactly to fit the girth of the boat where the cork reached to below.

As the cork would only press upwards, and always against the bottom and sides of the boat, it is evident, that, if the lower rope fitted tightly, the cork would keep its place; and in order to secure that point a few turns of rope passing from the lower edge on the one side over the keel to the lower edge on the other side would fix it completely. A very few seizings attached to the gunwale rope passing from the one side to the other would be quite sufficient.

The separate parcels must be furnished with loops or ends for attaching them to the main ropes, and to one another.

2.—Several

2.—Several ringbolts might be put into the keel withinside, and ropes, single, double, or treble, might be passed through these rings before laying in the buoyant materials, and then these ropes might be brought round the whole contents and made fast.

3.—It frequently happens that seamen, after they have gained the shore, find they have only escaped one death to perish by another still more miserable.

Dry clothing

Drenched in water, chilled to the heart with cold, worn out with fatigue, and exposed to all the severity of inclement weather, without shelter or succour, it is impossible but that the remains of life must soon be extinguished.

In this situation, and it is far from being uncommon, dry clothing would be as precious as life itself, and it might be had by the following expedient:—

Let a *leathern* bag be made for containing some shirts, a waistcoat or two, and two pair of drawers, all of flannel.

Let this bag be made of a length and size convenient for the purpose, and for tying round under the arm-pits.

made to answer the purpose of a cork jacket.

This would serve the purpose of a cork-jacket in the water, and prove a second time as life from the dead, by affording dry and warm clothing upon gaining the shore.

By this expedient every man may be made a swimmer, and sometimes one man by smimming has been the means of saving the whole ship's company.

It may be proper to observe, that the larger part of the bag should be placed high upon the breast, and the other or back part no higher than the armpits, as in the act of swimming the back part of the shoulders is little more than just covered with the water.

The bag must be perfectly water-tight, and only moderately filled; both ends of it may be left open to be closed with a tight seizing of small line.

The expense of this preparation would hardly be five shillings.

4.—If it were intended only for swimming, a neat and commodious preparation might be made with cork covered with thin leather*, to be applied in the manner which has just been described.

* I conceive flannel would be preferable. It would be less cold, and not so much affected by soaking in water. C.

It

It might be fitted on with clothes or without in half a minute, and made fast by a knot or clasp on the breast; three pounds of good cork would be sufficient to support any man, and the expense no more than in the former case.

A line might be conveyed on shore by a kite

5.—Another expedient bids fair for obtaining a speedy communication betwixt the ship and the shore, by means of a kite.

It is the property of this machine, to ascend in proportion as the cord is spared off.

To manage this, and to bring the line within reach on shore, let a piece of light wood, about the size of a small handspike, be attached to the line, about twelve fathoms from the kite; the line to be fixed to the forepart of the stick, and so as to pull only there, and then being slackly laid along the stick, made fast to the other end: by this means the kite would be prevented from rising higher, and would, at the same time bring the line to the shore from the ship, and by this small line a rope might be hauled from the ship by any spectator on the shore.

A silk handkerchief, and a piece of wooden hoop, might soon furnish a kite.

or by a spread ensign.

6.—Sometimes people are seen to perish, where those on shore, and those on the wreck, are almost within grasp of each other.

In this case, if there happened to be a mast standing, a common ensign made fast to a stick, just strong enough to keep it spread, and quickly spared off from the mast head, would probably reach the shore without touching the water, or at least drift on shore with a small line attached to it.

No experiments having been made upon these substitutes and expedients, they are barely mentioned, as things that might possibly succeed, and are thrown out as hints for others to improve upon, after much consideration on my part.

Invention of the life boat,

After this follow testimonies of approbation, from which it appears, that a boat prepared according to Mr. Bremner's plan, when overset by ropes applied for the purpose, righted herself immediately when these ropes were removed. The plan was not copied from that of the lifeboat, as it was communicated

communicated to Lord Melville, when Treasurer of the Navy, in 1792: whether the lifeboat were borrowed from it is more questionable.

Mr. Bremner also takes this opportunity of asserting his claim to the invention of applying locks to cannon, which he communicated to the late Sir Charles Douglas so long ago as the year 1768. and of locks to cannon.

III.

On the Scale of the Barometer, and the Construction of an Airpump for procuring a perfect Vacuum. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

I FEEL much obliged by the insertion of my paper on the Airpump, in your very valuable Journal. Should the following hint, respecting the construction of the barometer, (which is at least new to myself,) appear to be worthy the attention of your readers, it is much at your service.

In Mr. Dalton's Meteorological Observations, page 7, where he is speaking of the barometer, I find the following remark: "The scale in strictness ought not to be full "inches, but something less, owing to the rising and falling "of the surface of the reservoir. If the tube have a bulb, "then the area of the surface at the top of the column, di- "vided by the sum of the areas of the top and reservoir, "will give the part to be deducted; but if the tube be "straight, then the whole area of the reservoir, lessened by "the area of the glass annulus, made by a horizontal sec- "tion of the erected tube, must be used as the denomi- "nator of the fraction; hence, if the fraction be $\frac{1}{60}$, then "the scale of 3 inches must be diminished by half a tenth." At page 9 the following observation occurs. "With re- "spect to the barometers at Kendal and Keswick, they "were both clear of air and moisture, and exhibited the "electric light in the dark. The scales were both *full* "inches, and therefore the variations were somewhat greater "than Observations of the scale of the barometer.

"than the observations denote them. About $\frac{1}{10}$ should "have been allowed upon them." Not to mention the difficulty of obtaining the exact areas of the top of the column, and of the bulb, the latter of which is continually varying on account of its spherical form; it appears from Mr. Dalton's second observation, that philosophers do not always make the necessary corrections, even when they have sufficient data to do so.

The proper correction seldom made.

Barometer with two scales for this purpose.

The barometer which I generally make use of has a scale affixed to the bulb, as well as one at the top of the column. These scales are both divided into equal portions of an inch: in making an observation, therefore, with this barometer, I have nothing to do, but to take the height of the mercury in the column, and of that also in the bulb*, and by subtracting the latter from the former, the true altitude is immediately obtained. In the annexed figure, pl. IV, fig. 1, I have given a sketch of an improved construction of a barometer, upon the same principle.

The same effected by a single scale.

This barometer consists of a tube, bent into the form of a siphon, and hermetically sealed at the largest end, which must exceed 31 inches. The other end is open, and 4 or 5 inches will be a sufficient length for it. If both the legs of the siphon be of equal size, it is evident, that, when the mercury rises one inch in the largest leg, it will fall one inch in the shortest; and vice versa. The scale is to be 31 inches in length, and graduated in the usual manner at the top; but it must be movable, so as to slide freely, upwards and downwards, in a groove, which is to be set in the frame of the instrument, When we wish to take an observation, we have only to fix the bottom of the scale in the same horizontal line with the surface of the mercury in the open tube; and the height of the column may be instantly noted, as with the common barometer; only in this case, no correction will be necessary for the rising or falling of the surface of the reservoir. An index should be affixed to the lower extremity of the scale, to facilitate the adjustment of it to the height of the mercury; and another movable index, with a vernier scale, may be added to the

* Both the scales are graduated from the same point.

top,

top. When the mercury rises 1 inch in the common barometer, it will rise only ½ an inch in this, on account of the depression in the other end, which will also be equal to ½ an inch: it is however evident, that this diminution of range cannot at all affect the sensibility of the instrument; as it will be increased in an equal degree, in the opposite tube. But it is by no means necessary, that the legs of the siphon should be of equal diameter; although I have supposed them to be so, in the present instance, in order to make the action of the sliding scale more apparent. The scale may be affixed to any common barometer, having either a bulb, or an open reservoir; as will be evident, by inspecting the figure.

The note which is added to my paper on the airpump is perfectly just. I had however purposely avoided making use of the expression a "perfect *vacuum*"; and substituted that of *exhaustion*, in the room of it: meaning, that provided the construction of the pump were perfect, there would be *no limit* to the exhaustion it was capable of producing, i. e. that as long as any air remained in the receiver, a portion of it might still be expelled, by continuing the action of the pump; and although, strictly speaking, this is not producing a *perfect* exhaustion, yet it may be carried on *in infinitum**. I was so well convinced of the impossibility of obtaining a perfect vacuum, by this, or any other pump, that it was my intention to have added a few words on this subject, and on the only means, by which, I conceive, it may be procured. The vacuum of the barometer cannot be considered as perfect, even when air and moisture are entirely excluded; on account of the atmosphere, formed by the evaporation of the mercury itself, as was ascertained by Lavoisier, (Kerr's transl. p. 59, Ed. 3,) and also by Dr. Priestley. (See his Experiments Vol. 5, p. 225.)

Airpump.

Toricellian vacuum.

* Let us suppose the capacity of the barrel to be greater than that of the receiver. By the first stroke of the piston, a quantity of air greater than the half will be taken away: by the second stroke, more than half of the remainder will be removed, and so on: in this case, then, it is evident, that if the action of the pump be continued, there will at last remain a quantity less than any that can be assigned. This is approaching very near to perfection.

p. 225.) It may not be amiss to observe here, that it seems necessary, that a correction for this should be made, in taking observations with the barometer; as the mercurial atmosphere will react upon the surface of the top of the column, and prevent it from rising to the full height, to which it would otherwise attain.

Airpump for a perfect vacuum,

By the following means, a *perfect vacuum*, I believe, may be obtained. Let A B (fig. 2) be a tube of metal, ground so as to be perfectly cylindrical in the inside: let C C be the piston-rod; and D the piston, which is solid; and let *a* be a small metallic valve, opening outwards: also let the concave and convex surfaces of the barrel, and of the piston, be ground accurately to each other. Let us imagine the piston to be at the top of the barrel, and that all the air is expelled by means of the valve *a*: if the piston be now forced downwards, the space above it will be a *perfect vacuum*; at least with respect to air, and all evaporable fluids. The valve might be placed in the piston, as represented by the dotted lines at *b*; and in this case the one at *a* would be unnecessary. The action of this pump might be rendered more secure, by closing the bottom of the barrel, and inserting in it a metallic valve, opening outwards; and by making the piston rod to move in a collar of leathers: the piston also and the bottom of the barrel might be ground to each other. By these means, there would be but little danger of any air forcing itself into the vacuum, which would be *perfect* above the piston, and nearly, if not quite, so below it. This addition is shown by the dotted lines. If the barrel were made of glass, we should then have it in our power, to observe the appearance of the electric fluid, in a *perfect vacuum*; which, I believe, has never yet been the case. The bottom of the piston also might be formed of different metals, and exposed to the action of a burning glass, or of a galvanic battery. If any elastic fluid were generated by the process, it would be easy to collect and to ascertain its nature. Many other solid substances might be acted upon, by fixing them accurately into the bottom of the piston, so as to form a part of it; and by using the same agents, as in the former case.

in which some experiments might be made.

The principle upon which this pump is constructed I consider

consider as perfect; its application is by no means so: it may nevertheless be a useful hint to those, who are engaged in very refined experiments on the nature of the metals, &c. It would be less troublesome to construct, and perhaps more applicable to most purposes, if made after the following plan. Let A B be a glass cylinder, D the piston, and E E a plate of glass, large enough to cover the top of the barrel. When this instrument is to be made use of, let the piston be forced upwards, so as to project about a line or less above the cylinder; let the glass plate E E (which must be well ground to the top of the piston) be laid upon the piston, and let it then be drawn downwards; the plate will be kept in its place, by the pressure of the atmosphere, (or other means may be made use of to keep it more secure) and the vacuum will be perfect, as in the former case. The glass E E may be made of a piece of common plate glass, if truly ground; and the focus of the lens will easily be directed through it, so as to fall upon the bottom of the piston.

Another construction for the same purpose.

I am, Sir,

Your obliged and constant reader,

L. O. C.

IV.

A Description of a Forcing House for Grapes; with Observations on the best Method of constructing them for other Fruits. By T. A. KNIGHT, *Esq. F. R. S. &c.**

SO much difference of opinion prevails among gardeners respecting the proper forms of forcing houses, that two are rarely constructed quite alike, though intended for the same purposes; and every gardener is prepared to contend, that the form he prefers is the best, and to appeal to the test of successful experiment, in support of his opinion. And this he is generally enabled in some degree to do, because plants, when properly supplied with food, and water, and heat, will succeed in houses, the forms of which are very defective; and proper attention is not often paid

Construction of forcing houses for fruit.

* Trans. of the Horticultural Soc. Vol. I, p. 99.

by the gardener, when his prejudices satisfy him, that his labours cannot be successful. It is, however, sufficiently evident, that, when the same fruit is to be ripened in the same climate and season of the year, one peculiar form must be superior to every other, and that in our climate, where sunshine and natural heat do not abound, that form, which admits the greatest quantity of light through the least breadth of glass, and which affords the greatest regular heat with the least expenditure of fuel, must generally be the best: and if the truth of this position be admitted, it will be very easy to prove, that few of our forcing houses are at present ever moderately well constructed. I therefore think, that, if plans and descriptions of such forcing houses, as theory and practice combine to prove to have been properly constructed for the culture of every different species of fruit, were published by the Horticultural Society, much useful information might be conveyed to the practical gardener: and under these impressions I send the following description of a vinery, in which the most abundant crops of grapes have been perfectly ripened within less time, and with less expenditure of fuel, than I have witnessed in any other instance.

Best inclination of the glass.

It is well known that the sun operates most powerfully in the forcing house, when its rays fall most perpendicularly on the roof: because the quantity of light, that glances off without entering the house, is inversely proportionate to the degree of obliquity, with which it strikes upon the surface of the glass; and it is therefore important to every builder of a forcing house to know by what elevation of the roof, the greatest quantity of light can be made to pass through it. To ascertain this point, I have made many experiments, and the result of them has satisfied me that, in latitude 52, the best elevation is about that of 34 degrees: and relative to that elevation the position of the sun, in different parts of the year, will be nearly as represented in the annexed sketch, pl. IV, fig. 4, which is taken from the vinery I have mentioned. About the middle of *May*, the elevation of the sun will nearly correspond with that of the asterisk A, and in the beginning of June, and again early in July, it will be vertical at B, and at Midsummer it will at C be only six degrees from

from being vertical. The asterisk D points out its position at the equinoxes, and E its position in midwinter.

In this building, which is forty feet long, and is heated by a single fire place, the flue goes entirely round without touching the walls; and in the front a space of two feet is left between the flue and the wall, in the middle of which space the vines, which are trained to the roofs, about eleven inches from the glass, are planted; and as both the wall and flue are placed on arches, the vines are enabled to extend their roots in every direction, while, in the spring, their growth is greatly excited by the heat, which their roots and stems receive from the flue. Air is generally admitted at the ends only, where all the sashes are made to slide, to afford a free passage of air through the house, when necessary, to prevent the grapes becoming mouldy in damp seasons. About four feet of the upper end of every 3d light of the roof is made to lift up, (being attached by hinges to the wood-work on the top of the back-wall) to give air in the event of very hot and calm weather; for I prefer giving air by lifting up the lights, to letting them slide down, because when the former method is adopted, no additional shade is thrown on the plants.

Flue.

Place of the vines.

Air admitted at the ends,

and by lifting up some of the lights.

The preceding plan is here particularly recommended for a vinery only; but I am confident, that, by sinking the front wall below the level of the ground, and making a small change in the form of the bark-bed, the same elevation of roof may be made equally applicable to the pine stove, and that no upright front glass ought, in any case whatever, to be used; for light can always be more beneficially admitted by adding to the length of the roof, if that be properly elevated; and much expense may be saved both in the building, and in fuel. For forcing the peach or nectarine, I must, however, observe, that I think any house of the preceding dimensions wholly improper; and I purpose to submit a plan for the improved culture of those fruits to the Horticultural Society at a future opportunity.

The plan applicable to a pine stove.

Upright front glass injudicious.

This house not fitted to the peach or nectarine.

The vine often bleeds excessively when pruned in an improper season, or when accidentally wounded, and I believe no mode of stopping the flow of the sap is at present known to gardeners. I therefore mention the following, which I discovered

Composition to stop the bleeding of vines.

discovered many years ago, and have always practised with success: if to 4 parts of scraped cheese be added one part of calcined oyster shells, or other pure calcareons earth, and this composition be pressed strongly into the pores of the wood, the sap will instantly cease to flow; the largest branch may of course be taken off at any season with safety.

V.

On some of the Combinations of Oximuriatic Gas and Oxigen, and on the Chemical Relations of these Principles to Inflammable Bodies. By HUMPHREY DAVY, *Esq. LL. D. Sec. R. S. Prof. Chem. R. I. F. R. S. E.**

1. *Introduction.*

Oximuriatic acid gas a simple substance.

IN the last communication which I had the honour of presenting to the Royal Society, I stated a number of facts, which inclined me to believe, that the body improperly called in the modern nomenclature of chemistry *oximuriatic acid gas* has not as yet been decomposed; but that it is a peculiar substance, elementary as far as our knowledge extends, and analogous in many of its properties to oxigen gas.

My objects in the present lecture are to detail a number of experiments, which I have made for the purpose of illustrating more fully the nature, properties, and combinations of this substance, and its attractions for inflammable bodies, as compared with those of oxigen; and likewise to present some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they enter into union.

I have been almost constantly employed, since the last session of the society, upon these researches, yet this time has not been sufficient to enable me to approach to any thing complete in the investigation. But on subjects, important

* Phil. Trans. for 1811, P. 1.

both

both in their connexion with the higher departments of chemical philosophy, and with the œconomical applications of chemistry, I trust that even these imperfect labours will not be wholly unacceptable.

2. *On the Combinations of Oximuriatic Gas and Oxigen with the Metals from the fixed Alkalis.*

The intensity of the attraction of potassium for oximuriatic gas is shown by its spontaneous inflammation in this substance, and by the vividness of the combustion. I satisfied myself, by various minute experiments, that no water is separated in this operation, and that the proportions of the compound are such, that one grain of potassium absorbs about 1·1 cubical inch of oximuriatic gas at the mean temperature and pressure, and that they form a neutral compound, which undergoes no change by fusion. I used, in the experiments from which these conclusions are drawn, a tray of platina for receiving the potassium; the metal was heated in an exhausted vessel, to decompose any water absorbed by the crust of potash, which forms upon the potassium during its exposure to the atmosphere, and the gas was freed from vapour by muriate of lime. Large masses of potassium cannot be made to inflame, without heat, in oximuriatic gas. In all experiments in which I fused the potassium upon glass, the retorts broke in pieces, in consequence of the violence of the combustion, and even in two instances when I used the tray of platina. If oximuriatic gas be used not freed from vapour, or if the potassium has been previously exposed to the air, a little moisture always separates during the process of combustion. When pure potassium, and pure oximuriatic gas are used, the result, as I have stated, is a mere binary compound, the same as muriate of potash, that has undergone ignition.

Potassium inflames in oximuriatic gas,

and forms a neutral compound unalterable by fusion,

the same as muriate of potash

The combustion of potassium and sodium in oxigen gas is much less vivid than in oximuriatic gas. From this phenomenon, and from some others, I was inclined to believe, that the attraction of these metals for oxigen is feebler, than their attraction for oximuriatic gas. I made several experiments, which proved that this is the fact; but before I enter upon a detail of them, it will be necessary to discuss more

Potassium and sodium burn less vividly in oxigen than in oximuriatic gas.

more fully, than I have yet attempted, the nature of the combinations of potassium and sodium with oxigen, and of potash and soda with water.

When this is done on platina, the metal oxided.

I have stated in the last Bakerian Lecture, that potassium and sodium, when burnt in oxigen gas, produce potash and soda in a state of extreme dryness, and very difficult of fusion. In the experiments from which these conclusions are drawn, as I mentioned, I used trays of platina, and finding that this metal was oxidated in the operation, I heated the retort strongly, to expel any oxigen the platina might have absorbed, and, except in cases when this precaution was taken, I found the absorption of oxigen much greater than could be accounted for by the production of the alkalis. In all cases in which I burnt potassium or sodium in common air, applying only a gentle heat, I found that the first products were substances extremely fusible, and of a reddish brown colour, which copiously effervesced in water, and which became dry alkali, by being strongly heated upon platina in the air; phenomena, which, at an early period of the inquiry, induced me to suppose that they were protoxides of potassium and sodium. Finding, in subsequent experiments, however, that they deflagrated with iron filings, and rapidly oxidated platina and silver, I suspended my opinion on the subject, intending to investigate their nature more fully.

Potassium and sodium burned in common air produce brown, fusible substances,

which are peroxides.

Since that time, these oxides, as I find by a notice in the Moniteur for July 5th, 1810, have occupied the attention of Messrs. Gay-Lussac and Thenard; and these able chemists have discovered, that they are peroxides of potassium and sodium, the one containing, according to them, three times as much oxigen as potash, and the other 1·5 times as much as soda.

When they are formed on metallic substances these are always oxided.

I have been able to confirm in a general way these interesting results, though I have not found any means of ascertaining accurately the quantity of oxigen contained in these new oxides. When they are formed upon metallic substances, there is always a considerable oxidation of the metal, even though platina be employed. I have used a platina tray lined with muriate of potash, that had been fused; but in this case, though I am inclined to believe that

that some alkali was formed at the same time with the peroxides, yet I obtained an absorption of 2·6 cubical inches, in a case when 2 grains of potassium were employed, and of 1·63 cubical inches, in a case when a grain of sodium was used, but in this last instance the edge of the platina tray had been acted upon by the metal, and was oxidated*. The mercury in the barometer in these experiments stood at 30·12 inches, and that in the thermometer at 62° Fahrenheit.

When these peroxides were formed upon muriate of potash, the colour of that from potassium was of a bright orange; that from sodium of a darker orange tint. They gave off oxigen, as Messrs. Gay-Lussac and Thenard state, by the action of water or acids. They were converted into alkali, as the French chemists have stated, by being heated with any metallic or inflammable matter. They thickened fixed oils, forming a compound, that did not redden paper tinged with turmeric, without the addition of water. Their properties.

When potassium is brought into contact with fused nitre, in tubes of pure glass, there is a slight scintillation only, and the nitre becomes of a red brown colour. In this operation, nitrogen is produced, and the oxide of potassium formed. I thought that by ascertaining the quantity of nitrogen evolved by the action of a given weight of potassium, and comparing this with the quantity of oxigen disengaged from the oxide by water, I might be able to determine its composition accurately. A grain of potassium acting in this way, I found, produced only 0·16 of nitrogen; and the red oxide, by its action upon water, produced less than half a cubical inch of oxigen, so that it is probable, that potash as well as its peroxide is formed in the operation. Action of potassium on fused nitre.

Sodium, when brought into contact with fused nitre, Action of so-

* Messrs. Gay-Lussac and Thenard have stated in the paper above referred to, that common potash and barytes absorb oxigen when heated. It would seem, that the action of the fixed alkalis and of barytes on platina depends on the production of the peroxides. I have little doubt, but that these ingenious gentlemen will have anticipated this observation, in the detailed account of their experiments. Potash and barytes absorb oxigen when heated.

 produced

dium on fused nitre.

produced a violent deflagration. In two experiments in which I used a grain of the metal, the tube broke with the violence of the explosion. I succeeded in obtaining the solid results of the deflagration of ½ a grain of sodium; but it appeared, that no peroxide had formed, for the mass gave no oxigen by the action of water.

Potassium burned in a retort of pure glass,

When potassium is burnt in a retort of pure glass, the result is partly potash and partly peroxide, and by a long continued red heat the peroxide is entirely decomposed.

and in one of green glass containing oxigen.

A grain of potassium was gently heated in a small green glass retort containing oxigen; it burnt slowly, and with a feeble flame; a quantity of oxigen was absorbed equal to 0·9 of a cubical inch; by heating the retort to dull redness, oxigen was expelled equal to 0·38 of a cubical inch; the mercury in the thermometer in this experiment stood at 63° Fahrenheit, and that in the barometer at 30·1 inches.

Electrical decomposition of potash and soda.

In experiments on the electrical decomposition of potash and soda, when the Voltaic battery employed contains from 500 to 1000 series in full action; the metals burn at the moment of their production, and form the peroxides; and it is probable, from the observations of Mr. Ritter, that these bodies may be produced likewise in Voltaic operations on potash, at the positive surface.

Supposed protoxides.

In my early experiments on potassium and sodium, I regarded the fusible substances appearing at the negative surface, in the Voltaic circuit, as well as those produced by the exposure of the metals to heat and air, as protoxides, and as similar to the results obtained by heating the metals in contact with small quantities of alkali.

I have repeated these last operations, in which I conceived that protoxides were formed.

Potassium and sodium, when heated in glass tubes in contact with about half of their weight of potash and soda, that have been ignited, become first of a bright azure, then produce a considerable quantity of hidrogen, and at last form a gray coherent mass, not fusible at a dull red heat, and which gives hidrogen by the action of water.

Whether these are true protoxides, or merely mixtures of the alkaline metals with the alkalis, or with the alkalis and reduced

reduced silex from the glass, I shall not at present attempt to decide.

Potassium I find heated in a similar manner with fused potash, in a tube of platina, gives, after having been ignited, a dark mass that effervesces with water; but even in this case, it may be said, that the alloy of platina and potassium interferes, and that the substance is not a protoxide, but merely dry alkali mixed with this alloy.

As the pure alkalis were unknown, till the discovery of potassium and sodium*, and as their properties have never been described, it will perhaps be proper in this place to notice them briefly.

Description and properties of the pure alkalis.

When potassium and sodium are burnt in oxigen gas upon platina, and heated to redness to decompose the peroxide of potassium, the alkalis are of a grayish green colour. They are harder than common potash or soda, and, as well as I could determine by an imperfect trial, of greater specific gravity. They require a strong red heat for their perfect fluidity, and evaporate slowly, by a still farther increase of temperature. When small quantities of water are added to them, they heat violently, become white, and are converted into hydrats, and then are easily fusible and volatile.

When potassium or sodium is burnt on glass, freed from metallic oxides, and strongly heated, or when potash or soda is formed from the metals by the action of a minute quantity of water, their colour approaches to white; but in other sensible properties they resemble the alkalis formed upon metallic substances; and are distinguished in a marked

Stahl nearly discovered the pure alkalis.

* Stahl approached nearly to the discovery of the pure alkalis He cemented solid caustic potash with iron filings in a long continued heat, and states, that in this way an alkali "valde causticum" is produced. *Specim, Bech.* part ii, p. 255. He procured caustic alkali also, by decomposing nitre by the metals. Id. p. 253.

Affinity of potash for metallic oxides.

I find, that, when nitre is decomposed in a crucible of platina by a strong red heat, a yellow substance remains, which consists of potash and oxide of platina, apparently in chemical combination. The undecompounded potash, which comes over in the process for procuring potassium by the gunbarrel, is of an olive colour, and affords oxide of iron during its solution in water. Pure potash will probably be found to have an affinity for many metallic oxides.

manner

manner by their difficult fusibility from the potash and soda prepared by alcohol.

Water in the alkalis.

Mr. D'Arcet, and more distinctly Mr. Berthollet, have concluded, that the loss of weight of common fused potash and soda, during their combination with acids, depends upon the expulsion of water, which Mr. Berthollet has rated at 13·9 per cent for potash, and Mr. D'Arcet, at 27 or 28 for potash, and 28 or 29 for soda*.

I have stated in the last Bakerian lecture, that my own results led me to conclude, that fused potash contained about 16 or 17 parts in the 100 of water, taking the potash formed by adding oxigen to potassium as a standard.

The experiment, from which I drew my conclusions, was made on the action of silex and potash fused together, and I regarded the loss of weight as the indication of the quantity of moisture.

Water not yet collected from the ignited alkalis.

I am acquainted with no experiment on record, in which water has been actually collected from the ignited fixed alkalis, and this appeared necessary for the complete elucidation of the subject.

Experiment to effect this with potash.

I heated together, in a green glass retort, 40 grains of potash, (that had been ignited for several minutes), and 100 grains of boracic acid, which had been heated to whiteness for nearly an hour. The retort was carefully weighed, and connected with a small receiver, which was likewise weighed; the bulb of the retort was then gradually heated till it became of a cherry red; there was a violent effervescence in the retort, a fluid condensed in the neck, and passed into the receiver. When the process was completed, the whole of the retort was strongly heated; it was found to have lost $6\frac{1}{2}$ grains, and the receiver had gained 5·8 grains. The fluid that it contained was water, holding in solution a minute quantity of boracic acid, and when evaporated, it did not leave an appreciable quantity of residuum.

Water 0·17? or 0·19?

Water from soda 0·23.

A similar experiment made upon soda, heated to redness, but in which the water collected was not weighed, indicated 22·9 of water in 100 parts of soda.

* Annales de Chimie, tom. 68, page 190; or Journal, vol. XXVII, page 31.

It

It may be asked, whether part of the water evolved in these processes might not have been produced from the boracic acid, or formed in consequence of its agency; but the following experiments show, that this can not be the case in any sensible degree. None of the water from the boracic acid.

I heated 8 grains of potassium, with about 50 grains of boracic acid, to redness in a tube of platina, connected with a glass tube, kept very cool; but I found that no moisture whatever was separated in the process. I mixed a few grains of potassium with red oxide of mercury, and ignited the mixture in contact with boracic acid, but no elastic product, except mercury, was evolved. Proofs.

I made some potash by the combustion of potassium in a glass tube, and ignition of the peroxide; I added to it dry boracic acid, and heated the mixture to redness. Subborate of potash was formed, and there was not the slightest indications of the presence of moisture *.

It

* These processes must not however be considered as showing, that boracic acid that has been heated to whiteness is entirely free from water; they merely prove, that such an acid gives off no water by combination with pure potash at a *red heat*. I have found, that boracic acid in perfect fusion, and that has been long exposed to the blast of a forge, and that has long ceased to effervesce, gives globules of hidrogen, when dry iron filings are made to act upon it. I added to 54 grains of boracic acid in complete fusion, in a crucible of platina, 75 grains of flint glass that had been previously heated to whiteness, and immediately reduced into powder in a hot iron mortar; by raising the heat so as to produce combination, a copious effervescence was produced; and after intense ignition for half an hour, the mixture was found to have lost three grains and a quarter. Boracic acid heated to whiteness not free from water.

The combinations of boracic acid with potash and soda, that have been heated to redness, I find lose weight when their temperature is raised to a much higher degree. Thus, in an experiment made in the laboratory of my friend John George Children, Esq., and in which Mr. Children was so kind as to cooperate, 71 grains of hydrat of potash, mixed with 96 of boracic acid that had been heated as strongly as possible in a blast furnace, lost by fusion together in a red heat 11 grains, but on raising the temperature to whiteness the loss increased to above 13 grains. 55·5 grains of hydrat of soda, mixed with 80 of boracic acid, examined at intervals in a process of this kind, continued to lose weight for half an hour, during which time they were frequently heated to whiteness; at the end of this period the whole loss was 14 grains, of which at least one grain and a half

may

The common alkalis hydrats.

It is evident from this chain of facts, that common potash and soda are hydrats, and the bodies formed by the combustion of the alkaline metals are, as I have always stated, pure metallic oxides, (as far as our knowledge extends) free from water *.

I shall

may be referred to the acid. 95 grains of soda, ignited to whiteness in a platina crucible, with 140 of dry flint glass, lost 22·2 grains; 80 grains of boracic glass were added to this mixture; a fresh effervescence took place, and after intense ignition for a few minutes, there was an additional loss of weight of four grains and a half. The energy with which water adheres to certain bodies in other cases is shown by the experiments of Mr. Berthollet, *Mém. d'Arcueil*, tom. ii, page 47. Indeed it is impossible to say, that a neutral compound, or a fixed acid, is ever entirely free from water; it is only the first proportions that are easily separated. If the proportions of water in common potash and soda were to be judged of from their loss of weight, in combining with boracic acid, it would appear to be from 19 to 20 per cent in the first, and from 23 to 25 in the second.

Potassium and sodium not hydrurets.

* After the experiments detailed in my last two papers, it may perhaps appear unnecessary, at least to those enlightened British chemical philosophers, who have closely followed the progress of science, to offer any new evidences to prove, that potassium and sodium are not hydrurets of potash and soda; particularly as Messrs. Gay-Lussac and Thenard, the ingenious advocates of this notion, have acknowledged, in the Moniteur to which I have before referred, that it is not tenable; but on a subject so intimately connected with the most refined departments of chemical philosophy, and with so many new objects of research, additional facts cannot be wholly devoid of use and application.

Mr. Dalton, in the second volume of the work which he entitles "*A New System of Chemical Philosophy,*" of which he has had the goodness to send me a copy, has, I find in his first pages, adopted the idea, that potash and soda are metallic oxides; but in the latter pages has considered them as simple bodies, and the metals formed from them as compounds of potash and soda with hidrogen. He has given no facts in favour of this change in his opinion: his principal argument is founded upon the process in which I first obtained potassium. Common potash is a hydrat: when oxigen is procured from this by Voltaic electricity at one surface, and potassium at the other surface; Mr. Dalton, conceiving that this oxigen arises from the water, states, that the hidrogen of the water must combine with the potash to form potassium. It is evident, that, adopting such a plan of reasoning, lead and copper might be proved to be hydrurets of their oxides; for when these metals are revived from their aqueous acid solutions, oxigen is produced at the positive surface, and no hidrogen at the negative surface.

In

I shall now resume the detail of the experiments that I have made, on the relative attractions of oximuriatic gas and

Potassium burned in oxigen,

In my first experiments for producing potassium and sodium, I used a weak power; and in these instances, procuring the metals in very small quantities only, I perceived no effervescence. When from five hundred to one thousand plates are used for producing potassium, there is a violent effervescence, and a production of hidrogen, and sometimes of potassuretted hidrogen, connected with the formation of the metal.

In the production of potassium in quantity hidrogen evidently evolved,

Potassium, brought into contact with redhot hydrat of potash, disengages abundance of hidrogen, and the whole is converted into difficultly fusible potash.

as it is in other experiments.

327 grains of hydrat of potash, that had been ignited, were made to act in a gunbarrel on 745 grains of iron turnings heated to whiteness. Some hidrogen was lost, and some hydrat of potash remained undecompounded, yet 225 cubical inches of inflammable gas were collected, and 50 grains of potassium, and a large quantity of an alloy of potassium and iron formed; so that it is scarcely possible to doubt, that all the hidrogen produced from the decomposed hydrat of potash was liberated.

Mr. Dalton conceives, that there is an analogy between potassium and sodium, and the compounds of hidrogen with sulphur, phosphorus, and arsenic; but I am at a loss to trace any similarity between sulphuretted hidrogen, which is a gaseous body, soluble in water, and having acid properties, and a highly inflammable solid metal, which produces alkali by combustion. Potassium might as well be compared to carbonic acid. Mr. Dalton considers the volatility of potassium and sodium as favouring the idea of their containing hidrogen; but they are less volatile than antimony, arsenic, and tellurium, and much less volatile than mercury. He mentions their low specific gravity as a circumstance favourable to this idea. I have on a former occasion examined this argument, first brought forward by Mr. Ritter; but it may not be amiss to add, that, if potassium is a compound of hidrogen and potash, hydrat of potash must contain an equal quantity of hidrogen, with the addition of a light gaseous element, oxigen, which might be expected to diminish rather than to increase the specific gravity of the compound. Mr. Dalton states, p. 488, that potassium forms dry hydrat of potash, by decomposing nitrous gas and nitrous oxide; this is not the case: and he does not refer to experiment. I find by some very careful trials, that potassium attracts the oxigen and some of the nitrogen from these bodies, and forms a fusible compound which may be decomposed, giving off nitrogen and its excess of oxigen, by a red heat, and which becomes *potash*, and not dry hydrat of potash.

Observations on some opinions of Mr. Dalton's.

Messrs.

and oxigen for the metals of the fixed alkalis. I burnt a grain of potassium in oxigen gas, in a retort of green glass, furnished with a stopcock, and heated the oxide formed to redness, to convert it into potash: half a cubical inch of oxigen was absorbed. The retort was exhausted, and very pure oximuriatic gas admitted. The colour of the potash instantly became white; and by a gentle heat the whole was converted into muriate of potash: a cubical inch and $\frac{1}{4}$ of oximuriatic gas were absorbed, and exactly half a cubical inch of oxigen generated. The barometer during this operation was at 30·3, the thermometer at 62 Fahrenheit. I made several experiments of the same kind, but this is the only one on which I can place entire dependence. When I attempted to use larger quantities of potassium, the retort usually broke during the cooling of the glass, and it was not possible to gain any accurate results in employing metallic trays. The potassium was spread into a thin plate, and of course was much oxidated before its admission into the retort, which rendered the absorption of oxigen a little less than it ought to have been. In the process it was heated in vacuo before the combustion, to decompose the water in the crust of potash; for in cases when this precaution was not taken, I found that hydrat of potash sublimed, and lined the upper part of the retort, and from this the oximuriatic gas separated water as well as oxigen.

and oximuriatic gas admitted.

Muriate of potash formed, and all the oxigen given out.

Water separated from hydrat of potash by oximuriatic gas.

The phenomenon of the separation of water from hydrat of potash by oximuriatic gas was happily exemplified in an experiment in which I introduced oximuriatic gas to the peroxide of potassium, formed in a large retort, and in which the potassium had been covered with a considerable crust of hydrat of potash. The upper part of the retort and its neck contained a white sublimate of hydrat, which had risen in combustion, and which was perfectly opaque. As soon as the gas was admitted, it instantly became transparent from the evolution of water; and on heating the glass in

Messrs. Gay-Lussac and Thenard have convinced themselves, that potassium and sodium are not hydrurets of potash and soda, by a method similar to that which I adopted and published some months before, namely, by producing neutral salts from them.

in contact with the sublimate, its opacity was restored, and water driven off.

In various cases in which I heated dry potash, or mixtures of potash and the peroxide, in oximuriatic gas, there was no separation of moisture, except when the gas contained aqueous vapour; and the oxigen evolved in the process, when the heat was strongly raised, exactly corresponded to that absorbed by the potassium.

Decomposition of muriatic acid gas by potash,

When muriatic acid gas was introduced to potash formed from the combustion of potassium, water was instantly formed, and oximuriate of potassium *. I have made no accurate experiment on the proportions of muriatic acid gas decomposed by potash, but I made a very minute investigation of the nature of the mutual decomposition of this substance and hydrat of potash.

and by hydrat of potash.

Ten grains of hydrat of potash were heated to redness in a tray of platina, which was carefully weighed; it was introduced into a retort which was exhausted of air, and the retort was filled with muriatic acid gas. The hydrat of potash was heated by a spirit lamp; water instantly separated in great abundance, and muriate of potash formed. A strong heat was applied till the process was completed, when the tray was taken out and weighed; it had gained $2\frac{1}{1}\frac{3}{6}$ grains. A minute quantity of liquid muriatic acid was added to the muriate, to ensure a complete neutralization, and the tray heated to redness: there was no additional increase of weight.

Action of sodium and soda on oximuriatic gas.

In the few experiments which I have made on the action of sodium and soda on oximuriatic gas, the phenomena appeared precisely analogous; but sodium, as might have been expected, absorbed nearly twice as much oximuriatic gas as potassium.

Muriate of soda decomposed by potassium.

When common salt, that has been ignited, is heated with potassium, there is an immediate decomposition, and by giving the mixture a red heat, pure sodium is obtained; and this process affords an easy mode, and the one I have always lately adopted, for procuring that metal. No hidrogen is disengaged in this operation, and two parts of

* i. e. Muriate of potash.

potassium I find produce rather more than one of sodium.

The experiments agreeable to calculation.

From the series of proportions that I have communicated in my last paper, it is evident, that 1 grain of potassium ought to absorb 1·08 cubical inches of oximuriatic acid; and that the potash formed from one grain of potassium ought to decompose about 2·16 cubical inches of muriatic acid gas; and these estimations agree very nearly with the result of experiments.

The estimation of the composition of soda, as deduced from the experiments in the last Bakerian lecture, is 25·4 of oxigen to 74·6 of metal, and this would give the number representing the proportion in which sodium combines with bodies 22*; from which it is evident, that a grain of sodium ought

* Or, if soda be considered as deutoxide, which seems probable from the experiments detailed page 114, 44; and on this supposition, the salts of soda must be conceived to contain double proportions of acid. On either datum the proportion of oxigen in water must be taken as 7·5, and that of hidrogen as 1, though other numbers might be found as divisors or multiples of these, which would equally harmonise with the general doctrine of definite proportions. In my last communication to the Society, I have quoted Mr. Dalton as the original author of the hypothesis, that water consists of 1 particle of oxigen, and 1 of hidrogen; but I have since found, that this opinion is advanced in a work published in 1789, *A comparative View of the Phlogistic and Antiphlogistic Theories*, by William Higgins. In this elaborate and ingenious performance Mr Higgins has developed many happy sketches of the manner in which (on the corpuscular hypothesis) the particles or molecules of bodies may be conceived to combine; and some of his views, though formed at this early period of investigation, appear to me to be more defensible, assuming his data, than any which have been since advanced; for instance, he considered nitrous gas as composed of two particles of oxigen, and one of nitrogen. Mr. Higgins had likewise drawn the just conclusion respecting the constitution of sulphuretted hidrogen, from its electrical decomposition. As hidrogen is the substance which combines with other bodies in the smallest quantity, it is perhaps the most fitted to be represented by unity; and on this idea the proportions in ammonia will be 3 of hidrogen to 1 of nitrogen, and the number representing the smallest proportion in which nitrogen is known to combine will be 13·4. Mr. Dalton, *New System of Chemical Philosophy*, pages 323 and 436, has adopted 4·7 or 5·1, as the number representing the weight of the *atom* of nitrogen; and has quoted my experiment, *Researches, Chemical and Philosophical*, as authorising these numbers; but all the inquiries on nitric acid, nitrous gas, nitrous

Hypothesis of Mr. Higgins.

Remarks on some of Mr. Dalton's.

ought to absorb nearly 2 cubical inches of oximuriatic gas; and that the same quantity, converted into soda, would decompose nearly four cubical inches of muriatic gas. Muriate of soda ought on this idea to contain one proportion of sodium, 22, and one of oximuriatic gas 32·9; and this estimation is very near that which may be gained from Dr. Marcet's analysis of this substance. Hydrat of potash ought to consist of 1 proportion of potash, represented by 48, and one of water, represented by 8·5. This gives its composition as 15·1 of water, and 84·9 of potash. Hydrat of soda ought, according to theory, to contain 1 proportion of soda 29·5, and 1 of water 8·5, which will give in 100 parts 22·4 of

nitrous oxde, and on the decomposition of nitrat of ammonia stated in that work, conform much more nearly to the number 13·4.

According to Mr. Dalton, nitrate of ammonia contains one proportion of acid and one of alkali, and nitrate of potash two proportions of acid and one of alkali; but it is easy to see, that the reverse must be the case. Nitrate of ammonia is known to be an acid salt; and nitrate of potash a neutral salt; which harmonizes with the views above stated. Mr. Dalton estimates the quantity of water in nitric acid of specific gravity 1·54, at 27·5 per cent; and this, according to him, is a stronger acid than he obtained by decomposing fused nitre by sulphuric acid, which contained only 19 per cent of water; and one quantity of sulphuric acid, according to him, will produce from nitre more than an equal weight of nitric acid, and he supposes no water in nitre; so that his conclusion as to the quantity of water in liquid nitric acid on his own data must be incorrect. I find water in fused nitre, by decomposing it by boracic acid.

I shall enter no farther at present into an examination of the opinions, results, and conclusions of my learned friend; I am however obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments; and I trust to his judgment and candour for a correction of his views.

Hypothesis of definite proportions.

It is impossible not to admire the ingenuity and talent, with which Mr. Dalton has arranged, combined, weighed, measured, and figured his atoms; but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ulimately rest. It has a surer basis in the mutual decomposition of the neutral salts, observed by Richter and Guyton de Morveau, in the mutual decompositions of the compounds of hidrogen and nitrogen, of nitrogen and oxigen, of water and the oximuriatic compounds, in the multiples of oxigen in the nitrous compounds; and those of acids in salts, observed by Drs. Wollaston and Thomson; and above all, in the decompositions by the Voltaic apparatus, where oxigen and hidrogen, oxigen and inflammable bodies, acids and alkalis, &c. must separate in uniform ratios.

water;

water; and the experiments that I have detailed conform as well as can be expected with these conclusions.

The proportions of potash and soda indicated, in different neutral combinations, by these estimations, will be found to agree very nearly with those derived from the most accurate analyses, particularly those of Mr. Berthollet; or the differences are such as admit of an easy explanation.

Hyperoximuriate of potash.

I stated in my last communication the probability, that the oxigen in the hyperoximuriate of potash was intriple combination with the metal and oximuriatic gas; the new facts respecting the peroxide confirm this idea. Potassium, perfectly saturated with oxigen, would probably contain six proportions; for, according to Mr. Chenevix's analysis, which is confirmed by one made in the Laboratory of the Royal Institution by Mr. E. Davy, hyperoximuriate of potash must consist of 40·5 potassium, 32·9 oximuriatic gas, and 45 of oxigen.

I have mentioned, that by strongly heating the peroxide of potassium in oximuriatic acid, all the oxigen is expelled, and a mere combination of oximuriatic gas and potassium formed. I thought it possible, that at a low temperature a combination might be effected, and I have reason to believe, that this is the case. I made a peroxide of potassium, by heating potassium with about twice the quantity of nitre, and admitted oximuriatic gas, which was absorbed: some oxigen was expelled on the fusion of the peroxide, but a salt remained, which gave oximuriatic gas, as well as muriatic acid, by the action of sulphuric acid.

Its formation explained.

It seems evident, that in the formation of the hyperoximuriate of potash one quantity of potash is decomposed by the attraction of oximuriatic gas to form muriate of potash; but the oxigen, instead of being set free in the nascent state, enters into combination with another portion of potash, to form a peroxide, and with oximuriatic gas.

The proportions required for these changes may be easily deduced from the data which have been stated in the preceding pages. 5 proportions of potash, equal to 240 grains, must be decomposed, to form with an equal number of proportions of oximuriatic gas, equal to 164·5 grains, 5 proportions of muriate of potash equal to 367 grains; and 5 of oxigen,

oxigen, equal to 37·5 grains, combined with one of potash, equal to 48, must unite in triple union with one of oximuriatic gas equal to 32·9, to form one proportion, equal to 118·4 grains, of hyperoximuriate of potash.

(To be concluded in our next.)

VI.

Farther Account of a Mule Animal between the Male Ass and Female Zebra. In a Letter from THOMAS ANDREW KNIGHT, *Esq., F.R.S., &c.*

To W. NICHOLSON, Esq.

Dear Sir,

IN a former number of your Philosophical Journal* you have given an account of a mule animal between the male ass and female zebra, which was bred by the present Earl of Powis; and you have expressed a wish to obtain farther information respecting it: I in consequence send you the following particulars. Offspring of an ass and zebra.

You have justly stated, that the zebra would not admit the approach of the ass till his coat had been properly painted to resemble her own; which circumstance is curious, because it goes far to prove, that animals, in a state of nature, distinguish and select those of their own species, in part at least, by sight; while in a state of domestication, when their colours become varied by the influence of cultivation, they appear to be guided almost entirely by another sense. Wild animals distinguish their species by colour.

The animal, which I proceed to describe, like other mules, bore, externally, a greater resemblance to its male than to its female parent; and until by near approach its stripes, which were much less distinct than those of the zebra, became visible, it was not readily distinguishable from The animal more resembled the male than the female parent,

* Received from the Right Hon. Sir Joseph Banks, Bart., P. R. S.; and inserted Vol II, p. 267 of the quarto series.

a very

a very large and strong Spanish ass. I am ignorant whether nature has given to the zebra, as to the ass, the power of breathing through its mouth as well as through its nostrils; or whether the passage of the breath is confined to the nostrils only, as in the horse: but I observed, that the mule zebra uttered its cry, which a good deal resembled the braying of an ass, through its mouth; corresponding in this respect with the mule, which is obtained from the male ass and the mare, and differing from that which is derived from the horse and the female ass.

was intractable,

The temper of the mule zebra, as might have been expected from its parentage, was sullen, vindictive, and untractable. It was nevertheless sufficiently subdued to permit itself to be ridden; but a considerable time generally elapsed before the mule and the rider could agree about the direction in which they were to move; and when that point was in some degree settled, the labour, to the rider, of impelling and guiding his companion, was found so much to exceed that of walking on foot, that the services of the mule were not much in repute, or often called for.

and a complete mule.

Attempts were made to obtain offspring from it both by the female ass, and the mare; but neither were successful. It appeared to possess passions; but, like other mules, to be without powers. It met its death by an accident when rising four years old, and consequently before it had acquired its full growth and strength: but its size and form, at that age, indicated great powers of bearing weight and undergoing fatigue; and it would probably have been of great value both as a beast of burden and draught, had not its temper disqualified it for either office.

Died from an accident at four years old.

I am, dear Sir,

Your obedient servant,

THOMAS ANDREW KNIGHT.

Downton, April the 26*th*, 1811.

VII.

VII.

Remarks on Potassium, Sodium, &c.; in Reply to the Communications of JUSTUS. *By* JOHN DALTON.

To Mr. NICHOLSON,

SIR,

IN perusing the former of the two communications, purporting to be a reply to the remarks on potassium and sodium in my New System of Chemical Philosophy, (Journal, vol. 28, p. 67) I felt interested in various acute observations of your correspondent; but at p. 72, where he investigates the quantity of oxigen in a given volume of oximuriatic acid gas, I am quite at a loss to conceive how he had obtained so small a portion as 30·24 per cent, when I had found 50, (New System, p. 560) calculating from the best data I could procure, and which I was confident from my own experience could not be materially incorrect. Being at that time particularly engaged, I could not attend to the subject farther than to write a short note (Journal, p. 157) requesting an explanation. This was given in the ensuing number, (p. 219.) When I stated, that his *data* were *defective*, I did not mean *erroneous*; no mathematician would have understood me in that sense; I meant, that he had not given sufficient data, and consequently that he had made the problem an unlimited one. If I should propose the following question to your correspondent, namely, *How long would a body be in moving with a uniform velocity from the Earth to the Moon, or through a space of* 240,000 *miles*—would he not find it necessary, that the velocity should be given? Yet he has found means to answer a similar question without the requisite data. The accuracy of the answer then may well be suspected. It may be of service to your correspondent, and perhaps to others of your readers, if I make out this charge more particularly. According to Chenevix,

Quantity of oxigen in oximuriatic acid.

77·5 mur. acid + 22·5 oxigen = 100 oximur. acid, *by wt.*

then, by measure 77·5 mur. acid $\frac{1{\cdot}73}{1{\cdot}125} \times$ 22·5 oxi. = 112 measures;

 That

That is, 77·5 measures of muriatic acid + 34·5 measures of oxigen, together 112 measures, will, when chemically combined, be equal to x measures of oximuriatic acid gas. How your correspondent ascertains the value of x in the above equation to be 100, I know not. It may as easily and as probably be assumed 50 or 500. Surely he is not so ignorant of Mr. Davy's experience as not to know, that 77·5 measures of muriatic acid gas + 34·5 of oxigen, are far inferior in weight to 100 measures of oximuriatic acid. The truth is, the *specific gravity* of oximuriatic acid gas is a *datum* most obviously necessary in the estimation of the oxigen a given volume of it contains.

Potassium contains potash and hidrogen.

With regard to the facts and arguments respecting potassium and sodium, I can bring forward the following, namely, that fused hydrate of potash consists of potash and water, or potash, hidrogen, and oxigen; that in the decomposition of this article by Voltaic electricity, nothing but oxigen gas is evolved, and potassium remains; hence I conclude, that potassium contains, and probably consists of potash and hidrogen. If your correspondent is not satisfied with these facts, and this reasoning, I cannot convince him. The first fact I adopt from my own experience and that of others, the second from that of Mr. Davy; and I am not able to discover any flaw in the conclusive argument.

Heat separates water and potash, and evaporates them at a certain degree, whether chemically combined is not known.

As to the question, what is the power that produces the separation of water and potash? I answer, *heat*. When I say, that, by the application of heat to a certain degree, "the alkali and water both evaporate," no one has authority from me to add "in a state of chemical union," nor yet "in a separate state," though only one of the two ways is likely to be true. The fact was, I had not ascertained when I wrote that, nor indeed have I yet, which of the two is true. I am rather inclined to the latter; but as this is one of a large class of chemical facts, I wish to have more experience, and more time to reflect upon it, than at present I possess. It forms an important inquiry according to my views of chemistry, to ascertain the relation of water to the acids, alkalis, &c. in the very act of distillation; namely, whether the water in passing over is in a state of steam, such as we find it in the atmosphere, or in a gasiform state of of chemical union with the acid, &c. But, whichever be the case, it is true, "that the process cannot be used to expel the last portion of water from the alkali", when the object is to obtain a ponderable mass of alkali free from water.

Potash exposed to heat in an open vessel, and in a gunbarrel.

When fused potash is exposed to a red heat in an open vessel, white fumes are observed to play over it; these, no doubt, are the particles or small drops of the condensed liquid hydrate, similar to the visible mist or condensed steam over hot water. From this and the above observation, then, it is probable, that in the gun barrel experiment, not only particles or atoms of hydrate of potash, but also of potash, and of steam, may come into contact with the red-hot iron; hence may be explained the production of hydruret of potash or potassium, of oxide of iron, of hidrogen, and of the white amalgam or alloy of potash and iron. This last is easily exhibited by keeping carbonate of potash in fusion for some time in an iron spoon by an intense heat; after the potash is washed off, the whole surface of the spoon, which has been in contact with the fused carbonate, is white as if tinned, and may be acted upon by an acid without losing its colour*.

Alloy of potash and iron.

Complex nature of the decomposition of hydrate of potash.

As for the complex nature of the decomposition of hydrate of potash, I see no great reason to wonder at it. The article consists of 3 elementary principles; so does wood. Why, it may with equal propriety be asked, does wood, in its decomposition by heat, exhibit such a mixture of principles? Charcoal, water, carbonic acid, carbonic oxide, carburetted hidrogen, and hidrogen, are among the products of the destructive distillation of wood.

Levity of potassium.

I was surprised at your correspondent's observations on the argument I have drawn for potassium being a compound of hidrogen from its levity. I venture to say, that Mr. Davy will allow the argument to have some force. In the place referred to, Mr. Davy does not say a word about the notion, *that hidrogen united to potash ought to make a compound specifically lighter than potash.* His answer, which is pertinent and to the purpose, is to those who object to potassium and sodium being classed amongst the metals,

* On potassated iron see also Journal, Vol. XXV, p. 51.

 merely

merely on account of their levity. I feel no repugnance to call those new bodies metals, be they hydrurets or not; but I should be far from inferring, that the other metals are also hydrurets. With respect to the resemblances between the new metals and sulphuretted hidrogen, &c., they certainly are many and striking; so are their resemblances to the metals; I do not undertake to decide which are most numerous. I apprehend a piece of brass or other alloy has as many properties resembling the metals as potassium and sodium; yet no one allows the former to be simple substances. This argument is at least sufficient to show, that simple and compound bodies may have so many points of resemblance, as to be fairly arranged in the same class. I do not consider the discovery of the new metals less valuable and important for being compound rather than simple bodies; and though there may be several facts and experiments, which seem to point them out as simple substances, yet till the preceding facts are controverted, the others can do little more than excite doubts on the subject.

Simple bodies and compounds may have resemblances enough to be classed together.

Your correspondent, adverting to the combustion of potassium in muriatic acid, argues, that the hidrogen is derived from the acid, and not from the potassium; and as a support of the opinion adduces Mr. Davy's experiments, in which a mixture of equal parts of oximuriatic acid and hidrogen is by the electric srk converted into muriatic acid. Granting the truth of the last deduction, the argument amounts to this, that, if two bodies, one of which is known to contain hidrogen, by their mutual action develope that gas, it follows, that the other may not contain hidrogen. But the principal aim of introducing this subject into the discussion on potassium and sodium seems to have been, to defend the notion of oximuriatic acid being a simple substance, and muriatic acid a compound of it and hidrogen. It is to this object that his calculation is directed, on which I have animadverted at the commencement of this letter. The experiments alluded to on oximuriatic acid and hidrogen I consider of the most difficult execution, and if Mr. Davy has succeeded in obtaining tolerable approximations to accuracy in his first trials, great merit is undoubtedly his; still we want a more accurate and

Combustion of potassium in muriatic acid no proof, that it does not contain hidrogen.

The nature of oximuriatic acid not yet ascertained.

and trust-worthy table of the specific gravity of muriatic and oximuriatic acid gasses before the value of the experiment can be duly appreciated; and it should be farther ascertained what proportionate condensation of volume is produced upon muriatic acid gas by admitting to it $\frac{1}{3}$ of its weight of water. It is curious to observe, that your correspondent was fully persuaded, and probably continues to be, of the "incontrovertible argument" the above experiments afford to Mr. Davy's opinions in regard to oximuriatic acid, though he did not know at the time he wrote, whether the specific gravity of muriatic acid was 1·4, or 1·9, but took it at 1·7; and he now adopts 1·258; and of the specific gravity of oximuriatic acid he makes no mention whatever; neither of the condensation or contraction of volume which a very small portion of water produces on muriatic acid gas; yet it is impossible to ascertain the bearing of the experiments, till these three data are all of them pretty accurately investigated. If your correspondent wish to institute his calculus anew, I shall give him all the information I can respecting oximuriatic acid: its specific gravity by my own experience is 2·34; by Mr. Davy's, 2·45; by Thenard and Gay-Lussac's, 2·47; and by Dr. Thomson's, (in a letter to me) 2·71. If the last estimate should be true, he will find, that, adopting Mr. Davy's notions and estimate of muriatic acid, there should arise nearly $2\frac{1}{4}$ measures of muriatic acid gas from a mixture of 1 of oximuriatic acid and 1 of hidrogen.

There is one opinion on which we all concur; that it is very desirable the specific gravities of the various gasses should be ascertained within narrower limits. From what is stated above, it appears, we have a range from 1·3 to 1·9 for muriatic, and from 2·3 to 2·7 for oximuriatic acid. Would it not be a proper object for the Royal Society to depute a committee of its members to undertake the investigation? As long as it is left to individuals, each one finds a result differing from that of another; and one authority is deemed as good as another; so that it will, if no such step is taken, be a long time before a general agreement respecting these points is likely to be obtained.

Specific gravity of gasses still a desideratum of importance.

Manchester, I remain, Yours,
May the 11*th*, 1811. JOHN DALTON.

VIII.

VIII.

Table of the Rain, that fell in various Places in the Year 1810, by the Rev. J. BLANCHARD, *of Nottingham; with a Meteorological Table for the same Year, by Dr.* CLARKE, *of that Town.*

RAIN TABLE, by the Rev. J. BLANCHARD, of Nottingham.

1810.	Bristol.	Chichester.	London.	Chatsworth, Derbyshire.	Derby.	Horncastle, Lincolnshire.	Ferriby, Kingston upon Hull.	Heath, near Wakefield, Yorkshire.	Manchester.	Lancaster.	Dalton, Lancashire.	Kendal.	Feltfoot, near Milnthorpe, Westmoreland.	Carlisle.	Nottingham.
January - -	*no ac.*	0·28	0·26	0·58	1·10	1·14	0·64	0·89	1·39	2·17	2·85	2·68	4·87	1·84	1·05
February - -	0·90	2·90	1·44	1·15	1·84	1·64	1·10	1·95	2.57	1·91	2·54	4·15	3·11	1·22	1·03
March - - -	2·30	2·84	2·54	2·10	1·50	1·71	0·94	3·45	3·19	2·37	6·03	4·26	8·00	3·80	1·40
April - - -	1·68	1·61	1·70	1·92	1·33	0·82	1·54	1·91	1·92	0·37	1·12	1·03	2·30	1·04	1·00
May - - -	1·42	1·46	1·04	2·89	3·20	2·40	2·66	3·13	1·41	0·12	0·75	0·81	0·60	0·53	2·60
June - - -	2·59	0·49	0·56	0·87	1·42	1·54	1·27	1·90	1·90	1·47	1·87	2·10	1·92	1·60	1·18
July - - -	1·55	4·72	3·78	2·23	3·01	3·50	3·77	4·41	5·50	3·14	3·89	3·49	4·55	3·24	3·85
August - -	4·52	8·07	2·46	2·92	3·40	4·13	4·33	3·18	5·00	3·58	4·18	4·54	4·75	3·22	2·61
September -	2·66	1·95	1·98	2·13	1·85	0·10	0·58	2·10	1·90	2·58	2·62	2·07	2·60	1·70	0·62
October - -	2·66	3·31	1·92	1·73	2·52	2·40	2·21	1·88	4·68	4·00	4·70	3·97	5·43	3·12	2·72
November - -	3·45	11·[illegible]7	6·08	4·59	6·16	5·23	5·98	5·12	3·68	4·50	5·10	4·01	4·86	3·15	3·02
December - -	6·80	4·53	2·94	4·87	2·26	3·47	3·95	4·30	6·03	6·47	7·19	8·41	8·28	4·30	2·07
Total -	30·53	38·93	26·70	27·98	29·59	28·08	28·97	34·22	39·17	32·68	42·84	41·52	51·27	28.76	23·15

METEOROLOGICAL TABLE,

By Dr. Clarke, of Nottingham.

1810.	Thermom.				Barometer.				Weather.		Winds.			
MONTH.	Maximum.	Minimum.	Medium.	Greatest Variation in 24 hours.	Maximum.	Minimum.	Medium.	Greatest Variation in 24 hours.	Fair.	Wet.	N. and N. E.	E. and S. E.	S. and S. W.	W and N. W.
January....	53	18	36	10	30·36	29·75	30·05	0·29	26	5	5	9	17	8
February ..	54	14	37	16	30·34	28·73	29·75	0·68	19	9	7	4	21	7
March	59	30	43	10	30·10	28·88	29·62	0·41	19	12	17	9	14	11
April	70	32	47	9	30·18	29·27	29·76	0·33	24	6	14	7	18	2
May	68	29	47	15	30·33	29·05	29·86	1·05	23	8	26	4	7	6
June	78	38	57	10	30·35	29·72	30·38	0·35	28	2	16	7	15	4
July	77	42	57	15	29·95	29·40	29·75	0·31	12	19	9	4	12	5
August	80	40	57	10	30·43	29·39	29·79	0·52	21	10	1	6	18	6
September..	82	39	56	11	30·38	29·71	30·10	0·31	28	2	17	6	10	5
October....	68	24	45	8	30·30	29·03	29·86	0·54	23	8	15	11	8	6
November..	53	26	38	10	30·12	28·86	29·44	0·55	25	5	11	10	8	7
December ..	50	19	36	10	30·50	28·85	29·62	0·71	21	10	5	2	9	21

ANNUAL RESULTS AT NOTTINGHAM.

Thermometer. Wind.

Highest Observation, Sept. 2d, 82° E.
Lowest Observation, Feb. 20th, 14° NE.
Greatest Variation in 24 hours, February 19-20 .... 16°
Annual Mean................. 46°

Barometer. Wind.

Highest Observ. Dec. 31st, 30·50 NE.
Lowest Observ. Feb. 19th, 28·73 SW.
Greatest Variation in 24 hours, May 20th .... 1·05
Annual Mean .... 29·83

Weather.	*Days.*
Fair......	269
Wet......	96
	365

Winds.	*Times.*
N. & NE....	143
E. & SE....	79
S. & SW.....	157
W. & NW....	88
	467

Rain.	*Inches.*
Greatest Quantity in July	3·85
Smallest ditto in September ..	0·62
Total Quantity for the Year..	23·15

The Barometer is firmly fixed to a standard wall, on an elevation of 130 feet; and the Pluviameter is placed in a garden, 140 feet from the level of the sea.

IX.

On the Use of Iron Pipes for conveying Water, and Mode of securing their Joints. In a Letter from Mr. JOSEPH T. PRICE.

To W. NICHOLSON, Esq.

Esteemed Friend,

IF the enclosed facts should appear to thee likely to be of any service to those, who may want a supply of water conveyed from any distant source, thou art welcome to give them to the public in thy Journal.

I am,

Thine very sincerely,

Neath Abbey, J. T. PRICE.
14 *Feb.* 1811.

Water-pipe of iron,

Mr. H. B. Way, of Bridport, had occasion, in 1805, to lay down between eight and nine hundred feet of iron pipe, in lengths about 6 feet each, and 3 inches in the bore. At every 50 feet was a joint with flanches and screws; the other pieces were put together spigot and faucet fashion. To make these tight, he wrapped round the spigot end some canvas well saturated with white lead mixed with oil to a proper consistence, and drove it into the faucet end as tight as possible. When a length of 120 feet was laid down, the end was plugged up to try the joints; and it was found, that two thirds of them leaked considerably. Being informed by a neighbouring mason, that a linen manufacturer had completely stopped the leaks in his bleaching cisterns, in which lie both cold and boiling was used, by means of Parker and Wyatt's Roman cement, he procured some of this, and luted every joint with it. In 12 or 14 hours the pipe was tried, and found to be perfectly water-tight at the joints; but one of the pipes had a crack in it, which leaked, and this was as effectually stopped by the cement.

leaking at the joints,

secured with a luting of Roman cement,

which also stopped a crack in it,

and in a leaden pipe.

The lead pipe, used in the house, had also a leak in it, which

which was stopped in a very short space of time by the cement.

This work was done between the 10th and 24th of December, in very frosty weather; and the pipe was covered with earth before the end of that month. It is about two or three feet beneath the surface, in a loose, sandy soil; and was kept constantly full of water, without any appearance of leaking.

The water was so much discoloured by the iron for a week or two, and on standing deposited so much sediment, that it could not be used. To remedy this the same mason recommended to put some unslacked lime into the upper reservoir, or head of water, and open all the cocks below to give it a quick run; which he said would leave a coat of lime round the inside of the pipe, so as to prevent the rust from coming off. This was done, and for a few days after the water tasted very much of the lime; but the taste soon went off, and the water, which is very soft, was as good after it had passed through the pipe as at its source.

The water at first discoloured, but this remedied by lime.

In the following autumn the same gentleman superintended the laying down of 360 feet of similar iron pipe, with a fall between 20 and 30 feet, the joints of which were secured in the same way. The supply of water here was so copious, that it was obliged to be kept running all night and great part of the day. This soon cleared the pipe from rust, so that after a few days the water came through colourless, and consequently no lime was used.

Another water pipe with a nearly constant stream soon cleared itself.

In the summer of 1808, or 1809, two more pipes were laid down in the neighbourhood in a similar manner, extending together between two and three thousand feet; and with equal success.

Two more laid down.

These were all perfectly sound and secure in the month of February last; a little before which Mr. Way, having occasion to put a new leaden pipe in his yard from the iron one, found the latter, as far as it was examined, apparently as good as when laid down, and the cement as perfect, only seeming harder.

All continue sound.

X.

X.

On the Invention of the Economical Process for Evaporation ascribed to MONTGOLFIER. *In a Letter from Mr.* ST. AMAND.

To W. NICHOLSON, Esq.

SIR,

Economical process for evaporation said to be invented by Montgolfier.

THE supplement to the XXVIIIth vol. of your interesting and excellent Journal of Natural Philosophy, &c., just published, contains an account of a process, abridged from vol. LXXVI of the Annales de Chimie published at Paris in November last. This process *for procuring and accelerating the evaporation of fluids, without employing heat produced by the ignition of combustible substances*, is said to have been communicated in conversation by Mr. de Montgolfier. This declaration of Messrs. Desormes and Clément, authors of the article in the Ann. de Chim., is not translated in your Journal, which gives only an abridgment of it: but you may easily turn to it, and I beg you will have the goodness to satisfy yourself of the fact. I am neither jealous nor envious of the fame of any one; on the contrary I deem myself happy in having fallen on the same idea and the same means with a man of deserved celebrity: but truth and justice give me a right to claim at least a priority of date in the invention, and to this I shall confine myself. The following are incontestable proofs of it.

But Mr. St. Amand has a prior claim.

Proofs of this.

After the disastrous and bloody catastrophe of the 10th of August, 1792, I took refuge in England, whither I brought with me the same process, which I employed myself in developing and varying in several ways; giving it a greater extent, and applications more numerous, than those mentioned in the Ann. de Chim., or in your Journal. These developements were proposed and submitted to the British government about fifteen years ago. They were known, approved, and patronized by several persons, distinguished for

for their rank, knowledge, and situations; by ministers, peers, members of parliament, &c. Several learned societies, artists, and government contractors, whom the minister was desirous of exciting to carry it into execution, were acquainted with it. It is now nine years since the manuscripts, which contained a full account of the invention, with various other matters, and which were in the hands and under the care of government, were taken away by some treachery, respecting which there are only conjectures.

Papers stolen from a government office.

About that time, sir, I had the honour to request you to assist me with your knowledge and distinguished talents, in rendering them into the English language, with which I have but an imperfect acquaintance; and to show you authentic certificates of the experiments, that I had made several years before with an apparatus, to which I gave the name of a *polychrest machine*, on account of the variety and multiplicity of its applications. I appeal to your candour and impartiality, to confirm the proposal I had the honour to make you on this subject, and the production of the certificates, which are still in my possession, if you still remember the circumstance; which indeed was the occasion of my first having the honour of being known to you.

Farther proofs.

Polychrest machine.

The apparatus I have mentioned, for which I obtained several *caveats* in the patent office more than twelve years ago, was ordered by the nobleman who was then first Lord of the Admiralty, whose kindness and encouragement have supported me, and whose protection I have still the honour to enjoy. His zeal for the good of the public, and for the sciences, induced him to cause the apparatus to be constructed at his own expense, under my direction, as appears by the certificate of the experiments made in his presence, signed by himself some time after, and dated in 1798, which I have in my possession. After such authentic official testimonies, I presume I need not appeal to several others, which, though highly respectable in themselves, would add nothing to the validity of the proofs already adduced. All these persons, of whom I could give you a list, are still living; and as most of them are known to you, they would confirm, if requisite, the publicity, which I beg the

Caveats at the patent office.

An apparatus constructed here and tried in 1798.

favour

favour of you to give this letter in the next number of your Journal.

I have the honour to be,

With a just and high admiration of your talents,

Your very humble and very obedient servant,

ST. AMAND.

No. 25, *York Buildings, New Road,*
May the 15*th*, 1811.

XI.

On the Combustion of Ether, and of Metals, in Oximuriatic Gas: by Mr. VAN MEERTEN, *and Mr.* STRATINGH *.

Combustion of different substances in oximuriatic gas.

AS a proof of the property of sulphuric ether to burn with flame in oximuriatic acid gas, leaving a little oxide of carbon, Mr. Van Meerten points out the following experiment.

Ether.

Let a piece of the whitest possible sulphate of lime remain some time in ether. Set fire to this piece well soaked in ether, and introduce it under a jar filled with oximuriatic gas: the ether, or rather its hidrogen, will burn rapidly, and the surface of the gypsum will be covered with a coat of oxide of carbon.

Brass.

The combustion of brass and of tin is effected in this gas as easily as that of iron in oxigen. Take a slender brass wire, twisted into a spiral, and terminated by a piece of kindled charcoal; immerse it in a jar of oximuriatic gas; and it will burn rapidly and entirely, throwing out sparks. At the same time it may be seen, that the charcoal has not the property of burning in it, for it remains unaltered. A tin wire exhibited the same phenomena.

Tin.

* Ann. de Chimie, vol. LXXIII, p. 87. Translated from Trommsdorff's Journal der Pharmacie, by Mr. Vogel.

A copper

A copper wire does not burn in this gas, but becomes as soft as lead. Copper.

A brass wire not heated redhot does the same. Brass.

This gas has no action on lead-wire. Lead.

A wire of red French gold melted, without throwing out sparks. Gold.

Pure silver wire, and iron wire, were not altered in it. Silver.

Mr. Stratingh, in verifying the preceding experiments, prefers making the extremity of the brass wire red hot, to adding a burning coal to it. Brass. He could not succeed in burning tin wire. Tin.

He effected the combustion of a very slender copper wire, the extremity of which was pointed and red hot. The inside of the jar was covered with green oxide of copper. Copper.

A wire of ducat gold did not grow red, or melt, in the gas, but was slightly oxided. This difference probably arose from Mr. Van Meerten's French gold containing more copper. Gold.

Very slender silver wire melted, after its extremity had been made red hot. Silver.

Iron wire by itself was not altered: but on adding to its extremity a wire composed of an alloy of three parts of antimony with one of tin, which was heated a little before its immersion, the iron wire gave out much red vapour, and the inside of the jar was covered with a beautiful red oxide of iron. Iron.

Camphor alone does not burn in this gas: but if a piece be stuck in the end of a cleft stick, wrapped round with tin foil, and this powdered with metallic antimony, the camphor will begin to burn with a deep red flame. Camphor.

Oil of turpentine, or of cloves, poured into this gas, gives out some fumes, but very little light*. Essential oils.

* A rag wetted with oil of turpentine takes fire in oximuriatic gas. *Vogel.*

XII.

XII.

Observations in Illustration of Mr. HOWARD'S *Theory of Rain. In a Letter from* THOMAS FORSTER, *Esq.*

To W. NICHOLSON, Esq.

SIR,

AS the following observations may serve farther to illustrate Mr. Howard's ingenious Theory of Rain, (see his paper on the modification of clouds,) I shall request your insertion of them in your scientific Journal.

Appearance of clouds on the 18th,

On the 18th inst. the day was close and warm, in the afternoon I observed several different modifications of cloud dispersed about in the atmosphere at different altitudes. In some places *cirro-stratus* might be distinguished; in others, the clouds shewed a tendency to *cirro-cumulative* aggregation, *cumuli* increased in density, and cirrose fibres transversely crossed their summits, forming *cumulo-stratus*, which like mountains transfixed by the mighty shafts of giants appeared in the horizon, and represented a majestic appearance; while in other places the process of *nimbification* appeared going on rapidly, and distant thunder was heard. About six o'clock the sky, seen between the clouds under the descending sun, appeared of a very unusual brownish lake colour. As the evening advanced the mountainous clouds in the horizon appeared of a deep blackish blue colour, their edges as well as those of other detached clouds above them exhibiting a bright golden colour. Flocks of *cumulus* floated along in the wind, and refracted dark lake coloured light; by degrees all the clouds lost their distinctive characters as separate modifications, and became one dense mass, which ended in rain during the night.

19th,

On the 19th it rained all the morning, but held up in the evening; the continuous sheet of cloud however remained, notwithstanding a strong wind from the north.

and 20th of May.

Early on the morning of the 20th the same uniform sheet of cloud obscured the sky. As the day advanced it broke, and this dense sheet of *nimbus*, which had been originally formed by the collapse of several distinct modifications

cations, appeared to resolve itself into them again; as the sheet broke part of it seemed to mount up into a higher and comparatively calm region, and formed itself into *cirro-cumulus*, in some places disposed like windrows of hay, in others consisting of small roundish nubeculæ of various sizes; and into *cirro-stratus*, consisting either of flat sheets of thin vapour with dentated edges, or disposed in streaks: other parts of the once continuous sheet of *nimbus* descended into a lower region, and floated along in flocks of *cumulus*, with a strong wind, and the day became very fine. In the evening the distinct modifications again seemed lost in a general mistiness of the atmosphere, which as it became darker seemed very red coloured, and this vapour was seen to thicken in particular places which became dense *nimbi* again, and gave forth vivid flashes of lightning, and thunder storms continued through the night.

From the evident decrease in the quantity of cloud during the fine part of the day, it is evident, that, while part of the sheet of *nimbus*, which obscured the sky in the morning, divided itself again into the several modifications, the collision of which originally formed it; great part must have been absorbed by the air*: this is farther probable from the great transparency and dense blue colour of the sky between the clouds.

The insertion of these observations in your next number will, if convenient, oblige your constant reader,

Clapton, Hackney, THOMAS FORSTER.
May 21, 1811.

XIII.

Observations on Dr. Bostock's *Review of the Atomic Principles of Chemistry. By* John Dalton.

To Mr. NICHOLSON.

SIR,

Dr. Bostock's dissertation on the atomic system of chemistry in your Journal (Vol. XXVIII, page 280) may be divided

Dr. Bostock's remarks on the

* See Mr. Van Mons's paper in your Journal for September, 1809; also Rees's Cyclopedia, article *Cloud*.

atomic system of chemistry.

divided into two parts; one of which relates principally to the theory of chemical combinations, which I have embraced from an extensive comparison of facts and observations furnished by the writings of others, and from a careful and laborious train of experimental investigation of my own; the other relates to *his* application of the theory to the solution of a few of the more simple and common combinations. On the former of these parts I beg leave to make a few observations; on the latter I think it is altogether unnecessary to say any thing, unless it be to correct a misrepresentation or two which Dr. Bostock has accidentally introduced, namely, that oxigen and hidrogen in water are as 85·7 to 14·3 in weight, and that these numbers are as 7 to 1 nearly (page 285); and that I conceive *nitrous oxide* to be a *binary* compound (page 290). The weights of oxigen and hidrogen in water are stated (vid. New Syst. p. 275) to be 87·4 and 12·6 respectively; and it is *nitrous gas* which I maintain to be *binary*, and nitrous oxide and nitric acid to be *ternary* compounds.

Meaning of the terms, theory and hypothesis.

I do not exactly agree with Dr. Bostock as to his remarks on the difference between *theory* and *hypothesis*; these terms as far as I can learn from their common use differ only in degree. Theory is all or the greatest part of the facts reduced to regular laws; *hypothesis* is where only a few facts are reduced to laws, and the rest are either irreducible, or are yet only in a train, or have their accuracy suspected. I think no one would seriously advance any hypothesis on any subject, that had not some one or more facts previously established in its favour. What is *theory* to one man may be *hypothesis* to another. If Newton had lived in an age when no mathematician but himself existed, he might have established his beautiful theory of gravitation to his own satisfaction; but it must have been only an *hypothesis* to the rest of his contemporaries. These observations lead me to remark farther, that my chemical doctrine on combination is not, "altogether hypothetical," according to Dr. Bostock's own definition. I remember the strong impression which at a very early period of these inquiries was made by observing the proportion of oxigen to azote, as 1, 2 and 3, in nitrous

oxide,

oxide, nitrous gas, and nitric acid, according to the experiments of Davy. And Dr. B. must confess, that the greater part of the facts I have stated in my book, as the grounds from which I draw my conclusions, are not new; but facts that have been investigated by others before me, and often with the same results.

Dr. Bostock must be aware, that in writing my System of Chemistry, I have presumed all along, that the future readers of it would be tolerably acquainted with the various branches of the mechanical philosophy; otherwise I must have made a cyclopedia of it; one department must have treated of statics, another of dynamics, another of hydrodynamics, another of pneumatics, &c. This was not my design. If therefore I have announced certain rules as proper to be laid down, and have given no demonstration of them, it was because they were deemed obvious to the class of readers I expected, or otherwise were such as could not be demonstrated but by the gradual developement of the system itself in its progress.

Mechanical consistency of Mr. Dalton's first rule of combination.

I proceed now to point out the mechanical consistency of the 1st rule, which Dr. Bostock has quoted, page 283, namely, that "when only one combination of two bodies can be obtained, it must be presumed to be a *binary* one, unless some cause appear to the contrary"; and if this be established, the other three which he quotes may be considered as corollaries from it.

Instance in the composition of water.

Let us suppose a mixture, for instance, of hidrogenous and oxigenous gas, in such sort, that there are the same number of atoms of each gas; now as the gasses are uniformly diffused, each atom of hidrogen must have one of oxigen more immediately in its vicinity. The atoms of hidrogen are all repulsive of each other; so are those of oxigen: the atoms of hidrogen are all equally attractive of those of oxigen, and the attraction increases in some unknown ratio as the distance diminishes. Heat, or some other power prevents the union of the two elements, till by an electric spark, or some other stimulus, the equilibrium is disturbed, when the power of affinity is enabled to overcome the obstacles to its efficiency, and a chemical union of the elementary particles of hidrogen and oxigen ensues.

 Now

Instance in the composition of water.

Now the question is, whether, according to the received laws of motion, each one atom of oxigen should unite to the one of hidrogen next to it, or whether 7 atoms of oxigen should leap over all the more proximate atoms of hidrogen to another at a greater distance, and consequently less attractive, and that finally only $\frac{1}{7}$th of the number of atoms of hidrogen should be engaged by the oxigen and the rest remain in a state of freedom as before. The former is the conclusion I adopted, and thought it would scarcely require any elucidation; the latter is thought by Dr. Bostock equally plausible as the former (page 291). However till it can be shown, that a less force can overcome a greater, I must refuse my assent. Besides there is another consideration, that has no small weight with me; it is known, that the oxigen carries the greater part of its heat, and in all probability of its repulsion along with it in its combined state; it would therefore be an odd phenomenon, if it could be rendered visible, to see 7 atoms of oxigen surrounding 1 of hidrogen of equal size, all the atoms of oxigen repelling one another, but retained by an atom of hidrogen at the centre, whilst a number of atoms of hidrogen are around, all equally attractive of oxigen with the one engaged. But the difficulty does not end here: though I am persuaded the relative weights of the hidrogen and oxigen in water are nearly as 1 to 7, I by no means assert, that they are accurately so. Perhaps Dr. Bostock would prefer the ratio of 15 to 85; that is, in the smallest integers, 3 to 17. Now upon this view of the subject we must picture to ourselves 3 atoms of hidrogen surrounded by 17 of oxigen as constituting 1 atom of water; the remaining 14 atoms of hidrogen must be conceived to continue in their elastic state as spectators, and not to disturb the equilibrium of the atom of water. This *may* be the constitution of an atom of water; but it is wonderful, that in the decomposition of it by galvanism, nothing but hidrogen and oxigen should be produced, and never any new combination should arise out of so complex a system of particles as an atom of water exhibits to view. Would it not have been an improvement to have formed a set of atoms on purpose for water, by melting 3 of hidrogen into one, and 17 of oxigen into one?

Both you and your readers will probably think by this time,

time, that I have proceeded far enough in the developement of the truth of a proposition almost self evident; if not I may resume it on some future occasion.

The 2d, 3d, and 4th rules are necessarily consequent to the 1st. When an element A has an affinity for another B, I see no mechanical reason why it should not take as many atoms of B as are presented to it, and can possibly come into contact with it (which may probably be 12 in general), *except so far as the repulsion of the atoms of* B *among themselves are more than a match for the attraction of an atom of* A. Now this repulsion begins with 2 atoms of B to one of A, in which case the 2 atoms of B are diametrically opposed; it increases with 3 atoms of B to 1 of A, in which case the atoms of B are only 120° asunder; with 4 atoms of B it is still greater as the distance is then only 90°; and so on in proportion to the number of atoms*. It is evident then from these positions, that, as far as powers of attraction and repulsion are concerned, (and we know of no other in chemistry) *binary* compounds must first be formed in the ordinary course of things, then *ternary*, and so on, till the repulsion of the atoms of B (or A, whichever happens to be on the surface of the other), refuse to admit any more. 2d, 3d, and 4th rules.

I shall now proceed to the 5th, 6th and 7th, or remaining rules, which Dr. Bostock has not quoted, but of which he is equally in want of an explanation, or he would not have formed such conjectures as that "*it seems* the most natural to regard the sulphuric acid as the *binary* compound of sulphur and oxigen," and that carbonic acid is a *binary* and carbonic oxide a *ternary* compound, and that nitric acid is a *binary* compound of azote and oxigen and nitrous gas a ternary, and that nitrous oxide is *binary*, &c. (page 287, 5th, 6th, and 7th rules.

* I find from the principles of statics, that, upon the supposition of spherical atoms of equal size, and that the law of repulsion after chemical union is the same as before, namely, reciprocally as the central distance, the repulsion of any one atom of B upon another of B, to separate it from A, is a constant quantity, on whatever point of the surface of A it may be placed; so that when there are 3 atoms of B, the 3d atom is repelled twice as much by the other two as it would be by a single atom placed diametrically opposite. When there are 4 atoms, then the 4th is three times as much repelled, &c.

5th, 6th, and 7th rules. 287, 290. The 5th rule is "that a binary compound should always be specifically heavier than the mere mixture of its two ingredients." The principle on which this rule is founded is recognised by chemists as *general*, if not *universal*; namely, that condensation of volume is a necessary consequence of the expulsion of heat by the exertion of affinity. Thus, steam is specifically heavier than a mixture of 2 parts hidrogen and 1 oxigen; ammoniacal gas is in like manner heavier than 21 azote with 72 hidrogen. The 6th rule is that "a ternary compound should be specifically heavier than the mixture of a binary and a simple, which would, if combined, constitute it; and the 7th, that "the above rules and observations equally apply when two bodies, such as C and D, D and E, &c. are combined." These rules are founded on the same principle as the former, which principle entirely precludes the notions of nitrous oxide and nitric acid being binary compounds, and discountenances those of carbonic and sulphuric acid being binary compounds.

After making these observations on the general rules, I shall now advert to more particular objects. I have already remarked, that explanations and elucidations similar to the above were what I thought unnecessary to enter upon in the work alluded to: it is not improbable but I may have been mistaken in this respect, especially if such inquiries and observations as the following should be frequent. "When "bodies unite only in one proportion, whence do we learn "that the combination must be binary? Why is it not as "probable, that water is formed of two atoms of oxigen "and one of hidrogen, of two atoms of hidrogen and one "of oxigen, or in short of *any assignable number of atoms* "*of hidrogen and oxigen?* I do not perceive that Mr. "Dalton has given any reason in support of this binary "combination in preference to all the rest; and *I am una-* "*ble to conjecture what reason can be urged in its favour*," (page 283). I hope such remarks will be no more adduced; and farther, that if any one should inquire, for instance, why 1 part of carbone, which takes 1·28 of oxigen, or 2·56, does not also occasionally take 3·84 and 5·12 parts of oxigen, it will be understood, that the reason I should assign is, that

that in the state of carbonic acid there are two atoms of oxigen combined with one of carbon, and a third or fourth atom of oxigen, however it may be attracted by the carbon, cannot join it, without expelling one or more of the atoms of oxigen already in conjunction. The attraction of the carbon is able to restrain the mutual repulsion of two atoms of oxigen, but not of three or more.

The drift of Dr. Bostock's remarks and objections, in page 285, is quite beyond my comprehension. The *single object* I had in view in writing the paragraphs there quoted was, to find the relative weights of hidrogen and oxigen in a pound or any other given weight of water. I have deduced them as 1 to 7; whether right or wrong may be a question: but certainly I had no *other* object in view, and therefore I consider *that* as the only one to which any criticism can properly apply.

Looseness of quotation.

I must object to such loose quotations as the following; namely, that I have assumed, "that when only one combination of two elementary bodies can be obtained, it must be binary;" my language is, "it must be *presumed to be* a binary one *unless some cause appear to the contrary.*" Supposing for instance, that my hypothesis had been formed previously to the discovery of carbonic oxide, I must have concluded, according to Dr. Bostock's quotation, that carbonic acid was a binary compound; whereas I should have compared carbonic acid with the other acids, and found that like them it ought to contain at least two atoms of oxigen to one of base, and this with me would have appeared "some cause to the contrary." Again, "only one combination of oxigen and hidrogen, and only one combination of hidrogen and azote can exist," (page 284.) Knowing that I never entertained such ideas, I was curious to find out those passages in my book, which could possibly be so far misapprehended, and I think they must have been the following: "As only *one* compound of oxigen and hidrogen is certainly known," (page 275), and "only one compound of hidrogen and azote has yet been discovered," (page 415). These ideas however are repeatedly ascribed to me, and in the most express manner. "We have never yet

yet been able to produce more than one combination with each of these substances, therefore Mr. Dalton concludes, that only one combination can possibly exist," (page 286, see also note.)

Size of atoms not dependent on their weight.

Though I am fully persuaded we are in possession of data sufficient to decide upon the relative *weight* of atoms, we are not in regard to their *size*. This last is a matter of mere speculation. Dr. Bostock seems to think the *size* must be in direct proportion to the *weight*. I should however rather suppose, that atoms of different bodies may be made of matter of different *densities*, if the expression may be allowed; thus mercury, the atom of which weighs almost 170 times as much as that of hidrogen, I should conjecture was larger, but by no means in the proportion of the weights, which would require a diameter of five or six times the magnitude. Perhaps in a question of this sort Newton has a better claim to be heard than either of us; he says, (I think in the 31st query to his Optics) " God is able to create particles of matter of several *sizes* and *figures*, and in several proportions to the space they occupy, and perhaps of different *densities* and forces······at least I see nothing of contradiction in all this."

Knowing that Dr. Bostock had occasionally communicated several chemical essays through your Journal, I was curious to see whether he had not furnished me with some arguments in behalf of that doctrine, which he thinks " depends for its proof entirely upon *subsequent* observations and experiments." In the XIth vol. of this Journal, page 75, May 1805, he has given valuable analyses of the acetate and superacetate of lead. The results give the proportions of lead and acid as under:

Superacetate—Lead	6·12	or	100
Acid	3	··	49
Acetate—Lead	8·4	or	100
Acid	2	··	24

A number of such analyses as these would compel Dr. Bostock, and others of your chemical readers, to examine the theory of chemical combinations which I have offered to them

them with more attention, than I fear they do. The present state of chemical science imperiously demands it.

I remain, yours &c.

Manchester, JOHN DALTON.
May the 15*th,* 1811.

SCIENTIFIC NEWS.

Royal Society of Edinburgh.

ON the 4th of March, Mr. Allan read a paper on the rocks of the environs of Edinburgh, being the first of a series, which he proposes to read on this subject. The present embraced the rocks of St. Leonard's Hill and Salisbury Craig. The specimens illustrating the subject he presented to the Society, to be deposited in their cabinet. Rocks in the environs of Edinburgh.

On the 18th, Sir George Mackenzie read some geological remarks on the appearance presented by different rocks in Iceland; and showed their importance in connecting the phenomena of volcanoes with the principles of the Huttonian theory. Sir George brought forward the results of Sir James Hall's experiments on heat modified by compression, and successfully applied them to support his conclusions. The facts were explained in a satisfactory manner, and the whole paper was so important in a geological point of view, that we regret that it is not in our power to give an analysis of it. We understand, however, that it will form a part of the account of Iceland, which Sir George and his friends are about to publish, the work is now in the press. Huttonian theory. Account of Iceland.

On the 1st of April, Dr. Brewster read a description of a new instrument, for measuring capillary attraction, the instrument to be exhibited at a future meeting. Capillary attraction.

Prof. Playfair read a very interesting paper, being part of his new edition of his illustrations of the Huttonian theory, entitled Remarks on the natural History of Volcanoes. Professor Playfair's illustration of the Huttonian theory.

Royal

Royal Medical Society of Edinburgh.

The Society will give a set of books, or a medal of five guineas value, to the author of the the best experimental essay in answer to the following question.

Prize question. Does any decomposition of acids and alkalis take place on their uniting to form neutral salts, according to an opinion lately advanced by Mr. Davy in respect to muriates?

Honorary, extraordinary, and ordinary members of the Society are alone invited as candidates. The dissertations are to be written in English, Latin, or French, and to be delivered to the Secretary on or before the first Day of December, 1812. And the adjudication of the prize will take place in the last week of February following. To each dissertation is to be prefixed a motto; and this motto is to be written on the outside of a sealed packet, containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

Wernerian Natural History Society.

At the meeting on the 6th of April, Mr. William Elford Leach laid before this Society an arrangement of the natural tribe of diptera, eproboscidea of Latreille, with descriptions of the species, which he illustrated by drawings and specimens. At the same meeting Prof. Jameson read an account of the occurrence of coal in the first sandstone formation in Thuringia and Silesia; whence he inferred the possibility of coal existing in the extensive depositions of red sandstone in Scotland, in which that valuable mineral has not hitherto been discovered.

Diptera.

Coal in the first sandstone.

Society's Memoirs. The first volume of the Wernerian Society of Natural History has just been published.

India Maritime Directory. The 2d part of the Maritime Directory for navigating to, from, and between the Ports of India, China, &c., by James Horsburgh, Esq., F. R. S. is in the press, and is expected to be ready for publication in July.

Report

Report of the Proceedings of the Mathematical and Physical Class of the French Institute, continued from p. 79.

Mr. Vauquelin has analysed tobacco, with a view to detect the principles, that characterise this plant, and have occasioned it to be chosen for the uses for which it is employed: and to ascertain the changes produced in it by the preparations it undergoes for sale. It appears to contain an animal matter of the albuminous kind, malate of lime with excess of acid, acetic acid, nitrate and muriate of potash, a red matter the nature of which is unknown, muriate of ammonia, and finally an acrid and volatile principle apparently different from any other known in the vegetable kingdom. It is this principle, that imparts to tobacco its well known qualities; and it may be extracted from the plant by distillation, and employed separately. Prepared tobacco yielded, in addition to the matter above enumerated, carbonate of ammonia, and muriate of lime.

Analysis of tobacco.

Peculiar principle in it.

Mr. Vauquelin imagined, that the juice of belladonna, from its effects on the animal economy being analogous to those of tobacco, might contain the acrid principle he had discovered in the latter: but on analysing it he found only an animal matter, salts with base of potash, and a bitter substance, from which the juice of belladonna receives its narcotic properties.

Analysis of belladonna.

Mr. Chevreul presented to the class a very extensive series of experiments on vegetable matters. Te object o f some of these was the bitter principle produced by the action of nitric acid on organic matter containing nitrogen. He conceives it to be a compound of nitric acid and an oily or resinous vegetable matter: and he ascribes its detonating property to the decomposition of the nitric acid, the formation of ammoniacal gas, prussic acid, olefiant gas, &c. But with the amere is produced a resinous matter, and a volatile acid, on which Mr. C. has made many experiments; and which he considers as differing from the amere only by a small addition of nitric acid.

Production of amere

Another object of Mr. Chevreul was the substances formed by the action of nitric acid on carbonaceous or resinous matters, which have the property of precipitating gelatine

and of artificial tannin.

gelatine. Mr. C. does not agree with Mr. Hatchet, their discoverer, in considering them as similar to tannin. He thinks they differ not only from tannin, but from each other; and that their differences arise from the acid employed, the matter from which they are prepared, and the quantity of acid that enters into their decomposition.

Sulph. acid and camphor. Mr. C. has likewise examined the different compounds formed by the action of sulphuric acid on camphor.

Distillation of spirits. Not a year passes without presenting us with some happy application of chemistry to the arts, and thus affording us fresh proofs of the benefit, that our manufactories derive from the sciences. Thus Mr. Chaptal has made some interesting observations on the distillation of wine. The improvement of this process has gone hand in hand with that of chemistry. One of the principal distilleries in the South of France is nothing more than Woulfe's apparatus on a large scale.

Ancient colours. The same gentleman has analysed seven specimens of colours found at Pompeia.

Stucco, &c. Mr. Sage has examined the processes best adapted to the management of lime for making solid mortars; the nature of different kinds of stucco; the means of giving the polish of marble to artificial stones; and lastly a process for reducing white wax to a soap.

Zinc for roofs. He has also written a paper, and Messrs. Guyton and Vauquelin a report, on the advantages and disadvantages of roofing houses with zinc.

Injurious manufactories. The section of chemistry have also pointed out, at the desire of the minister, what manufactures may be injurious to those who live in the neighbourhood; and what measures should be adopted, to reconcile the interests of the manufactures with those of the public.

Indelible writing ink. A report has also been made on a paper of Mr. Tarry's respecting the composition and improvement of writing ink. The author has composed an ink, which is not destructible either by acids or alkalis; a great advantage in France, where the practice of altering title deeds has lately been very prevalent. It has the inconvenience however of letting fall its colouring matter too easily.

Artificial turquoises. Another report, on the artificial turquoises of Mr. Sauviac, gives

gives reason to hope, that the productions of art in this respect will soon imitate exactly those of nature, so as to afford us a new source of wealth.

A committee has also examined the late Mr. Bachelier' composition of a preservative mortar. Preservative plaster.

The progress of mineralogy has not been great. Mr. Guyton however has made known a new crystalline form of the diamond, and has made some valuable experiments on the tenacity of metals. New diamond crystal.

From the researches of Mr. Sage it would appear, that the chrysolite of volcanoes, when powdered, may be substituted for emery. All the artists that have used it have expressed themselves satisfied with it. Substitute for emery.

The observations from which geology can draw the most important conclusions are no doubt those relating to fossil animals, particularly such as have lived on the earth. Mr. Cuvier has continued his inquiries into this subject. Jointly with Mr. Brongniart he has concluded his mineralogical geography of the environs of Paris; and he has since examined the bony brecciæ of the coasts of the Mediterranean. These singular rocks, which are found at Gibraltar, near Terruel in Arragon, at Cette, at Antibes, at Nice, in Corsica, on the coasts of Dalmatia, and in the island of Cerigo, have been formed in fissures of compact limestone, which constitues the principal part of these countries, and are all composed of the same elements; which are numerous fragments of bones, and of the surrounding limestone, confusedly united together by a brick-coloured cement. All the bones belong to herbivorous animals, most of them known, and even still living in these places. These are mingled with freshwater shells; which lead to the supposition, that the brecciæ are posterior to the last abode of the sea on our continents, though very ancient with respect to us; since we have no indication of such brecciæ being formed in the present day, and some of them, as those of Corsica, include unknown animals. Fossil animals. Bony brecciæ.

Bones of animals of the order glires are contained also in alluvial soils. They have been found in the bogs of the valley of la Somme, with horns of stags, and heads of oxen; and in the vicinity of Azof, near the Black Sea. These bones belonged to animals of the genus castor; some much resembling those of the common beaver; others, which form Bones in alluvial soils.

form a complete head, from a larger species. Mr. Fischer, who discovered this animal, called it trogontherium, which M. Cuvier has adopted for its specific name.

Bones in schist.

Remains of glires have been found also in schists. Three species have been described, and Mr. Cuvier has seen the figure of one, which some authors consider as having belonged to a guineapig, others to a polecat. He could not determine the genus however, though it has the characters of the order glires.

Fossil bones of a species of elk.

Among the fossil bones of ruminants found in the loose soil, Mr. Cuvier has recognised a species of elk, different from that now known. Its remains have been collected in Ireland, in England, on the banks of the Rhine, and in the vicinity of Paris, in beds of marl of little depth, which appear to have been deposited in fresh water. Other horns, discovered in abundance, near Etampes, in sand underlying fresh water limestone, prove the existence of a small species of reindeer not now known. Mr. C. has also observed remains of the horns of the roebuck, fallow-deer, and stag, which do not appear to differ from those of the known species. None are more abundant than these.

Skulls of the aurochs, urus?

Among the fossil remains of ruminants with hollow horns he has recognised skulls of the aurochs, found on the banks of the Rhine and the Vistula, in the neighbourhood of Cracow, in Holland, and in North America. These skulls differ only in size from those of the present aurochs, and this Mr. C. ascribes to their more abundant nourishment in the vast forests and fat pastures of Germany and Gaul.

and of the original of our domestic cattle,

Fossil skulls are also found, that differ from those of our domestic cattle only in being larger, and having a different direction of the horns. These have been dug up in the valley of la Somme, in Suabia, in Prussia, in England, and in Italy. "If we call to mind," says Mr. C. "that the ancients distinguished two sorts of wild oxen in Gaul and Germany, the urus and the bison, shall we not be tempted to suppose, that one of them was that, which, after having furnished our domestic breed of cattle, has become extinct in the savage state; while the other, incapable of being tamed, still subsists in very small numbers only in the forests of Lithuania?"

the bison?

Bones of the horse and boar.

We find likewise in the loose soil bones of the horse and the

the boar. The former almost always accompany the fossil elephants, and are found with the mastodontes, tigers, hyenas, and other fossil bones, discovered in alluvial lands: but it has not been possible to determine, whether they belonged to a species different from our domestic horse. Those of the latter have been obtained chiefly from bogs, and exhibit no mark to distinguish them from those of the common boar.

New species of manatee.

Other bones have been found, which belonged, according to Mr. Cuvier, to a new species of manatee. They were in strata of a course marine limestone, on the banks of the Layon, near Angers; and were mingled with other bones, some of which appeared to have belonged to a large species of seal, others to a dolphin.

Three singular reptiles.

The skeletons of three fossil oviparous quadrupeds, found in calcareous schist, have likewise been examined by Mr. Cuvier. One was at Oeningen, on the right bank of the Rhine, at its efflux from the lake of Constance. It had been described and figured as the skeleton of an antediluvian man, an errour already refuted. Mr. C. has shown, that it was a reptile, somewhat resembling the salamanders, and belonging to the genus proteus.

The second, found at the same place, was of the toad genus, and approaching to the bufo calamita.

The third, and the most singular, discovered in the quarries of Altmuhl, near Eichstadt and Pappenheim in Franconia, had been described and figured by Colini in the memoirs of the Academy of Manheim. Mr. Cuvier considers it as having belonged to a species of *saurien.* The length of its neck and head, its long mouth armed with sharp teeth, and its long arms, indicate that it fed on insects which it caught flying; and the size of its orbits leads to the supposition, that it had very large eyes, and was a nocturnal animal. No reptile now known bears any resemblance to this inhabitant of the ancient world.

Ornitholite.

In a supplement to his fossils of Montmartre, Mr. Cuvier has given a figure and description of an ornitholite much more perfect than any before published. It appears to have been of the gallinaceous order, and to have come nearest in size to our common quail.

Mr.

Petrified fruits.

Mr. Sage has described some carpolites. One was a kernel of a walnut, found at Lons-le-Saulnier; another appeared to have been the fruit of the wild nutmeg, that grows at Madagascar and in some of the Molucca islands; and the third belonged apparently to a genus approaching the durio. The last was converted into jasper, the other two into limestone. To these observations Mr. S. adds some others, that had been made before, and concludes from them, that the petrified fruits found in our climates are exotic. He likewise enters into a chemical investigation of the means by which these petrifactions have been effected.

New order of plants.

Order and method will always be two objects of the greatest importance in natural history, and particularly in botany; and accordingly the most celebrated naturalists have made them their particular study. Mr. de Jussieu, who may justly be considered as the legislator of methods in botany, has formed a new order of plants under the name of monimiæ. The genera, of which he composes it, are ruizia, monimia, ambora, and perhaps citrosma, pavonia, and antherospermia. This order should be placed immediately before the family of utriceæ: but at the end of the monimiæ Mr. de J. places the calycanthus, hitherto united with the rosaceæ, which he considers as the type of a new order, that will serve as an intermediate link between the monimiæ and utriceæ.

Fructification of grasses.

Mr. Palissot-Beauvois has studied the organs of fructifications in grasses more accurately than had been before done; and on the structure of each part of these organs has founded characters, that distinguish them from each other; thus affording means of arranging the numerous species in genera much more natural than those hitherto adopted.

New plant of the palm kind.

Mr. Labillardiere has made known a new plant of the family of palms, of which he makes a genus under the name of ptychosperma, bordering on the elates and arecas. This plant was discovered by the author in New Ireland. It freqnently reaches the height of sixty feet, and yet its trunk is but two or three inches in diameter. From these proportions Mr. L. gives it the specific name of gracilis. It is astonishing, as he observes, that so slender a tree should support

support itself: but we know, that all the monocotyledonous plants have the hardest part of their wood externally; and this structure imparts to them a degree of strength, which they could not possess if their most solid fibres were in the centre.

Marine plants.

Mr. Lamouroux has presented to the class a very extensive work on marine plants. In forming one group of all these Mr. L. has made a useful innovation. The little progress that has been made in the study of seaweeds has prevented botanists from being agreed respecting the organs of fructification. Mr. L. not only embraces the opinion of the male and female organs being placed in tubercles at the extremity of their ramifications, but characterises the different parts of these organs with precision. He has farther observed, that the species growing on granite, on limestone, and on sand, are always different. As to their interior organization, Mr. Decandolle had observed, that it was destitute of vessels, and entirely formed of cellular texture. Mr. L. distinguishes two sorts of cells; one very long, and hexagonal, forming the stalks, and the ribs of the ramifications; the other also hexagons, but nearly equal sided, and constituting the membranous or foliaceous substance. The former he supposes are analogous to the vessels, and the latter to the cellular texture of the more perfect vegetables. His researches have also led him to form several new genera.

To CORRESPONDENTS.

On perusing Dr. Davy's paper in our present number, and the letter from Mr. J. Davy, in which he mentions the properties of Dr. Davy's zuthic acid, or compound of oximuriatic gas and oxigen, p. 43 of our last number, J. M. will probably perceive, that the objects of his obliging communication are there answered.

To some other correspondents a similar remark will apply.

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For MAY, 1811,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of APR.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
28	56°	55°	63°	47	29·60		Fair	Cloudy
29	53	50	55	46	·37	·080	Rain	Ditto
30	51·5	50·5	55·5	47	·77	·195	Ditto	Rain
MAY								
1	56	57	60	52	29·67	·055	Ditto	Ditto
2	56·5	52	59	47·5	·64	·140	Ditto	Fair
3	54	56	58	52	30·02	·040	Ditto	Ditto
4	55·5	57	61·5	53	29·89	·190	Ditto	Cloudy
5	56	51	56	44	·72	·200	Cloudy*	Ditto
6	48	50	52	47	30·07		Rain	Ditto
7	53	52	53	46	29·75	·320	Ditto	Ditto
8	49	54	56	48	·81	·160	Cloudy	Rain
9	52	51	54	47	·56	·110	Rain	Ditto
10	55	56	60	50·5	·74	·325	Ditto	Fair
11	56	59	62	55	·83	·070	Fair	Ditto†
12	59	68	72	60	·74		Ditto	Ditto
13	65	66	73	62	·53		Ditto	Ditto
14	65	57	65	52	·50		Ditto	Ditto
15	58	60	68	56	·78		Ditto	Rain
16	60	60	67	57	·79	·025	Rain‡	Fair
17	58	61	65	55	·90	·010	Fair	Ditto
18	60	64	67	58	·86	·030	Ditto	Cloudy
19	60	55	62	50	·95	·140	Rain	Ditto
20	55	57	70	54	·85	·260	Fair	Cloudy §
21	57	60	62·5	55	·68	·200	Cloudy	Fair
22	60	63	67	52	·70		Fair	Rain ‖
23	57	57	64	52·5	·81	·300	Ditto	Fair
24	58	62	66	57	·91		Fair	Cloudy ¶
25	61	64	69·5	58	·99	·090	Ditto	Ditto
26	65	68	74	62	30·04		Ditto	Ditto**

2·940 Inch. since last Journ.

* Boisterous day. † Lightning. ‡ Thunder, 3 P M.
§ Lightning—Tremendous Thunder and Lightning at 4 A. M. ‖ Ditto at 8 P. M.
¶ Lightning. ** Ditto.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1811.

ARTICLE I.

On the Manufacturing of Thread, and Articles resembling Flax, Hemp, Tow, and Cotton, from the Fibres of the common Nettle. By Mr. EDWARD SMITH, *of Brentwood, Essex**.

SIR,

I HAVE the honour to transmit to you a short memoir on that hitherto much neglected and despised vegetable the nettle, with the general useful purposes to which the produce thereof may be applied. If you think it will merit any claim to the attention of the Society, I request you will do me the favour to lay it before them.

Uses of the nettle.

My attention was first directed to this matter about the year 1793, but from many impediments no favourable opportunity presented itself for particular investigations till about the year 1800, since which time, I have annually selected a few of the nettle plants from their various situations at different periods, in order to ascertain the state most

* Trans of the Soc. of Arts, vol. XXVIII, p. 109. The silver medal was voted to Mr. Smith.

congenial to the process, and that most suitable to the different purposes to which I thought them applicable. The result of my experiments has deeply impressed upon my mind, that they may be made subservient to national utility, particularly at the present period, when our foreign commerce is so generally impeded, and in consequence our supplies of foreign hemp and flax nearly annihilated.

Its abundance.

I beg leave to observe, that the growth of nettles is general, in every country, particularly in strong fertile soils, that on every bank, ditch, and place, which cannot be brought to tillage, they are produced in such abundance, that the quantity, if collected, would be of great magnitude.

Places adapted to its growth.

The growth of them might be encouraged in such waste places, or a vast quantity of land of that description might, at a moderate expense, be made to produce a valuable crop of a useful article heretofore regarded as a nuisance. The shady places in woods, parks, and coppices, are particularly favourable to their growth; I have found them in such situations in the greatest perfection in point of length and fibre. The harl, or fibre of them, is very similar to that of hemp or flax, inclining to either according to the soil and different situations in which they grow. I have ascertained, as far as I have been able to proceed, that they may be substituted for every purpose for which hemp or flax is used, from cloth of the finest texture down to the coarsest quality, such as sailcloth, sacking, &c., and for cordage.

Answers every purpose of hemp or flax.

Material for paper.

Another very material use, the magnitude of which, I trust, will be duly estimated, is, that they may be applied to the manufactory of paper of various qualities. The impediments to foreign commerce have lately deprived us of a supply of linen rags, and occasioned a general use of cotton rags in the paper manufactory, which is injurious to the preservation of the most valuable works in literature, to the truth of which the observation of every one must bear testimony, who has attended to the depreciated quality of writing and printing papers.

That the produce of nettles, and the refuse of them from the manufactory, may easily be converted into writing, printing, and all inferior sorts of paper, I feel confidently assured.

assured. For the purpose of writing and printing paper they might be gathered twice in one season, as for these uses the length of staple is not required, and the fibre would be considerably increased in its fineness; and in point of colour, either in the refuse or unwrought state, the chemical process of bleaching now in practice would render them a delicate white.

I have in possession some samples, which have gone through a succession of processes similar to what are practised on hemp and flax; and I have, without the aid of any implements, brought them to a state of preparation ready for the hackle; but for want of that, and there being no flax or hemp manufactory in this neighbourhood, I have not been able to proceed farther, but I judge that they are sufficiently advanced so as amply to evince the practicability above referred to.

If you think proper, I will transmit the samples for the Society's inspection, and give any farther information in my power.

Permit me the honour to subscribe myself, Sir,

Your most humble servant,

March 24, 1809. EDWARD SMITH.

SIR,

Prize on the subject offered by the Society at Haërlem.

I am much obliged to the Society for their reference of my communication to one of their Committees. About ten years subsequent to my first observations, and three to my first experiments, I observed the following paragraph in the Chelmsford Chronicle, November 25, 1803. "The Society of Œconomy, at Haërlem, has offered prizes for the best memoir as to the particular species, the season for gathering, and the manipulation necessary in preparing nettles for use." This is the only account I have ever seen of them, and shows that such a matter was regarded as deserving the attention of that Society; but as I from the first had it in contemplation to present my observations on the subject to the Society of Arts &c., and thinking the matter of great consequence, and wishing my own country to be benefited by it, I declined answering the Haërlem advertisement.

My discovery of the properties of the nettle is original, and arose entirely from my own observations on the apparent resemblance to hemp and flax, which I remarked they had when growing. I now transmit to you some samples, in different states, for the Society's inspection.

I have the honour to be, with great respect,

Your most humble servant,

March 28, 1809. EDWARD SMITH.

SIR,

Coarse yarn & flax from nettles.

I have now the honour to transmit to the Society my farther progress, viz. A sample of yarn prepared from the coarsest part of the nettle produce, which I deemed less liable to be injured for want of knowledge in the manufacturing than the finer qualities. Since my former letters I have been bleaching some of the nettle flax, and have brought it to so good a colour, that a preparation from it would produce paper perfectly white, and I have caused a sample of yarn to be made from the nettle produce, both of which I have sent.

Paper from them.

I likewise enclose an improved specimen of paper made from the same substance; also a preparation for paper, a part of the same sample the enclosed was made from, which is, of course, much inferior to what would be done by a paper manufacturer. These samples having been made by such rough instruments as were constructed by my own hands, and which of course the Society will consider.

I remain respectfully, Sir,

Your obliged humble servant,

Nov. 18, 1809. EDWARD SMITH.

The following Specimens, produced from Nettles by Mr. Smith, are deposited in the Housekeeper's Office.

Articles produced from nettles.

Samples of the fibres, in their rough state, resembling different kinds of hemp and flax.

Samples of the fibres equal to the finest flax, and remarkably strong in texture.

Samples of very strong yarn, prepared from the coarsest fibres.

Samples

Samples of coarse paper, prepared from the rough refuse fibres.

Samples of the coarse fibres bleached white.

Samples of a coarse substance resembling cotton prepared from the bleached coarse fibres.

Samples of white paper prepared by him from the last-mentioned substance.

Mr. Smith's *Process for preparing various Articles from Nettles.*

The kind of nettle capable of being manufactured into cloth, &c., it is scarcely necessary to say, is that which in general is denominated the stinging nettle. The most valuable sort, which many years practical experience has furnished me with a knowledge of, in regard to length, suppleness, fineness of the lint, brittleness of the reed, which dresses most freely, with less waste of fibre, and yields the greatest produce of long and fine strong harl, I have found growing in the bottom of ditches among briars, and in shaded valleys, where the soil has been a blue clay or strong loam, but from which situations I have selected some which have measured more than twelve feet in height, and upwards of two inches in circumference. Plants growing in the situations above described are in general from five to nine feet in height, and those growing in patches on a good soil, standing thick, and in a favourable aspect, will average in height about five feet and a half, will work kindly, and the stems are thickly clothed with lint. Those that grow in poorer soils, and in less favourable situations, with rough and woody stems, and have many lateral branches, run much to seed, are stubborn, and work less kindly; they produce lint more coarse, harsh, and thin. In every situation and different soil I have experienced the most productive nettles to be those which have the smoothest and most concave tubes, the largest joints, the fewest leaves, and which produce the least quantity of seed.

The kind of nettle.

Best sort.

Worst nettles.

Marks of the best.

In gathering them, as they are perennial plants, I have preferred the mode of cutting them down, instead of pulling them up by the roots. This I recommend to be the practice, with a view to obtain a second crop where the situations will

They should be cut.

will allow of it, and to secure the propagation of them the subsequent year.

Time of gathering.

The most favourable time for collecting them is from the beginning of July to the end of August, but it may be continued even to the end of October, only the lint of those which remain growing to that time will be less supple, and will not work so freely; and if the season happens to be unfavourable, it is probable there would not be sufficient time to steep and grass them, in which case they should be dried by the heat of the atmosphere, or if the state of the weather would not permit of this, then by means of artificial heat; and when dried they should be housed or stacked till the spring, when they might successfully undergo the same operation of steeping as those of the first collection. Such as grow in grass fields, where the grass is intended for hay, should be cut when the hay is cut, in order to prevent their being spoiled by the cattle when feeding; the harls of which would be fine in quality, and well suited to be wrought up with the second crop, and which crop may be obtained after those of the first cutting, where the situation will admit of their being preserved. The fine quality of such I ascertained last autumn, and found the height of them to average three feet and a half; they were gathered the latter end of November. The following are the processes adopted by me.

Treatment after gathering.

After the nettles are gathered they should be exposed to the atmosphere till they gain some firmness, in order to prevent the skin from being damaged in the operations of dressing off the leaves, the lateral branches, and seeds. This should be done a handful at a time; and afterward they should be sorted, viz. those which are both long and fine by themselves, those which are both long and coarse by themselves, and those which are short and coarse by themselves; then made up into bundles as large as can be grasped with both hands, a convenient size for putting them into the water, and taking them out; a place for this purpose being previously prepared, either a pond or a pit free from mud, or a brook or river. The bundles should then be immersed, and placed aslant with the root end uppermost, and to prevent their floating on the surface some weight should be laid upon them.

The

The time required for steeping them is from five to eight days; but it is better they should remain rather too long in the water than too short a time, yet great care should be taken that they are not overdone. When the fibre approaches to a pulp, and will easily separate from the reed, and the reed becomes brittle and assumes a white appearance, this operation is finished. Steeping.

The bundles should then be taken out singly, very carefully, to avoid damaging the fibres, and be rinsed as they are taken out of the water to cleanse them from the filth they may have contracted; they must then be strewed very thin upon the grass, and be gently handled. When the surface of them is become sufficiently dry, and the harl has obtained a degree of firmness, they should be turned repeatedly, till they are sufficiently grassed; the time required is known only by experience, so much depends on the state of the weather during the process; when they are sufficiently done, the harl blisters, and the stems become brittle; they must then be taken up and made into bundles, and secured from the weather. Grassing.

The harl is now to be separated from the reed, after the manner practised on flax and hemp, either by manual labour or machinery now in use in those manufactories. This operation was performed in my experiments by hand, and with implements constructed by myself, but which I consider too simple here to describe. Separation of the harl.

The harl being separated from the reed, it requires next to be beaten, that it may become more ductile for the operation of dressing, which may be performed with such implements as are used for dressing flax or hemp. Dressing.

This operation being accomplished, the produce of the nettles is arrived at a state ready for spinning, and may be spun into various qualities of yarn, either by hand, or by machinery constructed for the purposes of spinning flax or hemp; and this yarn may be successfully substituted for the manufacturing every sort of cloth, cordage, rope, &c., which is usually made from hemp or flax, and is particularly calculated for making twine for fishing-nets equal to the Dutch twine imported for that purpose, the fibres of the Spinning. Twine for fishing nets.

nettles

nettles being stronger than those of flax, and not so harsh as the fibres of hemp.

Refuse.

In the course of my experiments on nettles it often occurred to me, that the refuse, and such parts as were damaged in the different processes, with the under-growth, might be applied to useful purposes, and in addition to the nettle manufactory, as applicable to the purposes for which hemp and flax are used. Another source of productive labour of great magnitude would be derived from a new substance, capable of being converted into so many beneficial uses, if my speculations should be finally accomplished. In contemplating these subjects, I was induced to believe the refuse and under-growth might be converted into paper of various sorts, according to the changes they might be made to undergo from the several operations necessary to reduce them to a proper state for this use; having frequently observed, with regret, the deterioration in the quality of writing and printing paper, occasioned by the use of cotton rags in the paper manufactory; which evinces itself even to the most superficial observer, who may only casually open many of the modern publications, and which must be admitted is of the utmost moment, as it endangers the preservation of works of literature. Being convinced of the superior strength of nettle substance, I thought, could my speculations be reduced successfully to practice, it would not only remedy this great evil, and operate as an antidote to the use of cotton rags in that part of the paper manufactory, but eventually effect a reduction in the prices of books, which for some years have been rapidly increasing, and are now become excessive, to the great obstruction of disseminating useful knowledge among mankind, and contribute to the diminution of our exports in that material branch of commerce.

Paper made from it.

Advantages of this.

In addition to the above incentives, the consideration of the high price of paper, chiefly occasioned, as I conclude, from the extravagant price of linen rags, and the impediments to the procuring a foreign supply of them, arising from the circumstances of the times; and seeing that the use of linen cloth is in a great measure superseded by the very general introduction

Farther motives.

introduction of cloth manufactured from cotton, which consequently must materially diminish the supply of linen rags, and probably, in process of time, from the increasing substitution of cotton cloth for linen, linen rags, particularly of the finer qualities, may be totally annihilated. Urged by all these considerations, which were forcibly impressed on my mind, and feeling assured of the practicability of reducing the substance of nettles to a state necessary to the production of paper, and confident in the superior strength of such paper, if it could be manufactured from a substance so substantial, I was most powerfully impelled to attempt to reduce to practice what in theory I had so warmly cherished. The attempt was arduous, not only from an entire want of knowledge of the manufactory, and of the necessary utensils, but I was destitute of any proper implement to engage in the undertaking with any probability of success; hoping however by perseverance to succeed, I proceeded, and found on my first rough trial my expectations realized.

Preparation of the lint for paper.

The most favourable condition of the lint, with a view to the paper manufactory, is to begin with it after it is hackled; in order that the fibres may be divested of the skins which enclose them, as, when it is intended to make white paper, having gone through that process, it would greatly facilitate the bleaching, and be the more easily disencumbered of the gross particles.

When I signify as my opinion, that the fibres of nettles should be dressed the same as for yarn, previous to their being prepared with a view to the making of paper, I wish not to be understood to convey the idea that the operation cannot be dispensed with; because I conceive, that, by the aid of such machinery as is in use with the paper manufacturers, or by some improvements therein, they might be brought to a pulp easily, even when the nettles are first gathered, should it, with a view to saving of labour, be deemed necessary; but the practicability of this I leave to the experience which time may hereafter afford.

Bleaching.

My operation of bleaching the fibres for paper was performed on the grass, which I deem preferable to the new mode of bleaching with water impregnated with air by means

means of oxigenated muriatic acid gas; because the old mode of bleaching on grass weakens the strength of the fibre, leaves it more flexible, and thereby expedites the maceration, which in some degree compensates for the time it requires longer than by the chemical process. But for bleaching of yarn or cloth made of whatever substance, the chemical process, if scientifically conducted, experience has convinced me is preeminently superior, as it gives additional strength to the yarn, greater firmnes to the texture of the cloth, and is an immense saving of time, labour, &c.

Subsequent management.

After the lint is bleached it should be reduced to a proper length for paper, and then macerated in water after the manner of rags, and undergo similar processes till the substance is converted into paper, which may be easily accomplished by manufacturers, and the substance of nettles made to produce paper of the first quality and the most substantial.

Mode in which specimens of paper were produced.

In my process the lint was reduced by scissors to particles as minute as was practicable with such an implement; then it was macerated in cold water about ten days, and brought as much to a pulp as could be effected without the aid of grinding, &c. Being a stranger to the composition used to procure the adhesion of the particles, if any is used for this purpose, I tried several glutinous substances, none of which answered so well as a solution of gum, but I am well aware this cannot be generally used, being too expensive.

After the pulp was impregnated with the solution, I then spread it thin on a wire frame of my own construction, which process, except drying it, with me was final. Not being possessed of the means of pressing the paper any more than grinding of the lint, and for want of the film which adheres to the lint being dressed off, I could not completely destroy the colour, so as to produce a clear white without picking out every discoloured particle, which I so well accomplished, that when I had reduced the staple in length, in this state it was perfectly free from colour; the deterioration which ensued when converted into paper was occasioned by the solution of gum.

My processes were the fruits of my own conceptions, and I desire it may not be understood, that I presume to recommend

mend them for practice, being conscious, that the manufacturers of paper, hemp, and flax, from analogy, are possessed of the knowledge of operations and means more consonant and infinitely superior.

These several manufactures from the new substance of nettles, patronized by the stimulating approbation and recommendation of the Society of Arts, &c., I with all due deference venture to predict will rapidly increase the capital of those individuals who engage therein, afford new employment to the poorer classes of society, and become a new source of wealth to the nation.

EDWARD SMITH.

April, 28, 1810,

II.

Description of an improved Reapinghook for Corn. By Mr. JOSEPH HUTTON, Jun. *of Ridgway, near Sheffield**.

SIR,

AT a time like the present, when all foreign supplies of grain are cut off, nothing can be more acceptable to the public than useful discoveries and improvements in agriculture. I am therefore anxious to contribute, in some degree, to this end, by sending some remarks on reaping the harvest, accompanied with my new-improved reaphook. Agricultural improvements important.

I have, for the last eight years, had an opportunity of inspecting the different modes of reaping the harvest in many parts of Great Britain, and I have also had information on the subject from various parts of Europe and America on respectable authorities. Reaping.

I will first endeavour to describe the different kinds of implements used for this purpose, some of them being employed in one part of the kingdom, and not in another.

* Trans. of the Society of Arts, vol. XXVIII, p. 54. The silver medal was voted to Mr. Hutton for this improvement.

The

Sickle. The sickle is of the greatest antiquity, though its use is now much upon the decline in England. It is almost in the form of a half circle, from twenty to thirty inches long, about three fourths of an inch broad, with teeth cut in the edge from twenty to thirty in an inch, inclining from the handle to the point.

Sithe. The sithe is an instrument so generally known, as to need no description, farther than that some are made longer, and others broader, as necessity or caprice requires.

Reapinghook. The common reap-hook is a half-circular piece of iron and steel, from twenty to thirty inches long, about one inch and a half broad, and has a smooth even edge, like that of a sithe.

Badginghook. The badging or baging-hook is broader than the common reap-hook, particularly at the point, where it is most used, and straighter than the sickle or reap-hooks generally are.

Use of the sickle. The reaping of wheat with the sickle is yet continued in Yorkshire, Durham, Westmoreland, Cumberland, Lancashire, Warwickshire, Leicestershire, Northamptonshire, Rutland, Nottinghamshire, and part of Lincolnshire; it is performed by putting the sickle into the corn with the right hand, meeting it with the left hand, gathering the corn into the elbow of the sickle near the right hand, holding the corn fast with the left hand until it is cut, then the person repeats his cutting until he has obtained a large handful, which is generally one third of a sheaf, which he lays in the straw binding ready prepared.

Use of the reapinghook. The common reap-hook is used in the manner above specified, but its effects are far different, the sickle, having a toothed edge, does not cut such stems as are not immediately collected into the left hand; for it is impossible to collect all where dispatch is required, particularly in thin straggling crops, for the teeth of the sickle being inclined, it is not so sharp in cutting from point to handle, as from handle to point, which is evident from a feel with the finger. The reap-hook, having a smooth even edge, cuts both ways alike, and cuts the straggled stems before they are collected in the gathering hand, consequently the loss of grain is great. The hook is allowed to perform its work with more ease

ease than the sickle, which perhaps accounts for its now being so general, nothing else being used for cutting wheat in the following counties:—Cornwall, Devon, Dorset, Somerset, Monmouth, South-Wales, Hereford, Wilts, Hants, Berkshire, and part of the adjoining counties; it is also much used in Norfolk and Lincolnshire, also Northumberland, Westmoreland, and South of Scotland. It has lately been introduced into the North of Ireland by Irishmen who have laboured in Scotland, likewise into the Indies for cutting rice.

Use of the badginghook.

The badging-hook is used about London, and in the West of England, its work is performed by the man holding the hook in his right hand, and while, with the left, he reclines the stems intended to be cut upon the standing corn, which supports it when cut, he repeats his cutting from his right to his left hand, and collects it from his left to his right, which is almost a sheaf.

This preferable to the sithe.

Badging is an expeditious mode of reaping; the corn is cut very low as if mown, and answers where straw is valuable. It may be said, that badging produces more manure, from the greater quantity of straw collected; but in stiff clay lands a longer stubble is perhaps necessary to be left, to render the land lighter for the following crop. The badging-hook is also used for cutting oats in Lincolnshire and Staffordshire, and where labourers can be procured, is preferable to the sithe, being expeditious in its work, and less loss attending its use, the corn is gathered in straight regular order, which is not the case with the sithe; for the sithe requires at least two persons to follow it to bind the corn in sheaves, besides raking the stubble. The corn after the sithe lies in very irregular order, and holds more moisture in wet weather; besides, the sithe is destructive to the ripe corn, for its heavy stroke strips from the entangled stems the best and ripest grain.

The labourer seldom considers the interest of his employer, but generally uses such a tool as will do the work with most ease to himself.

Improved reapinghook.

I offer my improved reap-hook to the public, with a view to prevent the loss of grain, and at the same time to be used with ease by the labourer. It has a smooth edge like the

reap-hook

reap-hook, from the handle of it towards its middle, where the corn is gathered; the other part has a toothed edge, like a sickle, and it will not scatter the corn so much as either of the other implements.

I shall furnish certificates to show, that I am the inventor of it, and that it has considerable advantages in general use, It is a great preserver of corn, in harvest, where it is straggled much from heavy rains.

I am, Sir,
Your obedient Servant,
JOSEPH HUTTON, Jun.

The following certificates were received.

Testimonies of its utility.

A certificate from Mr. J. Turner, of Ridgway, dated October 3, 1809, stating, that in the year 1805 he had made two dozen of improved reap-hooks by Mr. Hutton's instructions; that they were the first he ever knew to be made upon this plan, and that in the present year he and others have made thirty-five dozen for him.

A certificate from William Taylor, of Summit Lodge, in Yorkshire, bailiff to G. F. Burton, Esq., dated September 29, 1809, stating, that after a few seasons experience, he finds Mr. Hutton's reap-hook preferable to any other, from the nature of its edge; that the labourers under his superintendance used all of this sort the last season, and that it is found to be a great saver of corn.

A certificate from John Boothe, sithe, sickle, and reap-hook manufacturer, Ford Mills, near Sheffield, dated October 12, 1809, stating, that Mr. Hutton's reap-hook is certainly superior to the common one, and that public opinion confirms it as such, for there has been a great demand for them the last two harvests.

A certificate from Mr. Edmund Littlewood, of Dent Hall, near Dronfield, dated October 15, 1809, stating, that Mr. Hutton's reap-hook is superior to the common ones now in use, especially in the last harvest, in which the crops have been remarkably straggled, and bad to reap, owing to the heavy rains and winds. That the common reap-hooks cut

cut while putting in, before the gathering hand has collected the stems together, and consequently many drop and are lost; which is not the case with Mr. Hutton's new-invented reap-hook, which does not cut before the stems are collected together in the gathering hand.

III.

Report of Messrs. DE PRONY, CHARLES, MONTGOLFIER, *and* CARNOT, *to the French Institute, on the Invention of a new Engine, by Mr.* CAGNIARD-LATOUR, *formerly Pupil at the Polytechnic School*.*

IT is known, that all bodies immersed in a fluid lose a part of their weight equal to that of the fluid they displace. On this principle Mr. Cagniard's new engine is founded.

Principle of a new engine.

The first mover in this engine is not the vapour of boiling water, as in common steam engines, but a volume of air, which, being conveyed cold to the bottom of a vessel full of hot water, is there dilated; and, by the effort it then makes to rise to the surface, acts in the manner of a weight, but in a vertical direction, agreeably to the principle mentioned above.

First mover.

This mover, once discovered, may be employed in different ways: the following is that of Mr. Cagniard.

His machine, properly speaking, is composed of two others, which have perfectly distinct functions. The object of the first is to convey to the bottom of the vessel of hot water the volume of cold air necessary. That of the second, to apply the effort, which this air, once dilated by heat, makes to reach the upper surface of the fluid, to the effect required to be produced.

The machine composed of two, answering different purposes.

For the first purpose Mr. Cagniard employs the screw of Archimedes. If such a screw cause a fluid to ascend by turning it in one direction; it is obvious, that it will cause it to descend, if turned in the contrary direction. If then it be

The first the screw of Archimedes,

* Journal des Mines, vol. XXVI, p. 465.

be immersed in water, so that only the upper part of its spiral remains in the air, it ought, when turned in the contrary direction, to cause to descend to the bottom of this water the air that it takes into its upper part in every turn. This is precisely what Mr. Cagniard's machine does. The air he wants is first conveyed to the bottom of the reservoir of cold water, in which the screw is immersed; and thence it is conveyed by a pipe to the bottom of the vessel of hot water. The heat of this water immediately dilates it, and thus creates the new power, which is to act as the first mover. In this way the first object of the machine is accomplished.

which conveys air to the bottom of a reservoir of water,

The second, as we have said, is to apply this new mover to the effect to be produced. For this purpose the author employs a bucket wheel completely immersed in the vessel of hot water. The air, dilated and collected at the bottom of the vessel, finds a passage contrived so as to guide it under those buckets, which have their mouths downward. The ascensional force drives the water out of these buckets, and the side of the wheel on which they are being thus rendered lighter, the wheel turns continually like a common bucket wheel.

whence it rises into the inverted buckets of a bucket wheel in hot water,

This wheel, being set in motion, is capable of transmitting its action to any other movable machinery, either by a toothed wheel and pinion, or any other means. In Mr. Cagniard's machine the effect produced consists in raising, by means of a cord fixed to the axis of the wheel, a weight of fifteen pounds, with a uniform velocity of an inch in a second, while the moving power applied to the screw is only equal to three pounds with the same velocity. The effect of the heat therefore is to quintuple the natural effect of the moving power.

the motion of which is applied to the purpose wanted.

The effect of the first mover quintupled.

It may be conceived, that, the moving power being quintupled, we may take from this effect a sufficient momentum to supply the original power, and still there will remain at our disposal four times the original power. This in fact is done in Mr. Cagniard's machine. By means of a crank, he forms a communication between the axis of the wheel and that of the screw, so that this turns as if it were moved by an external agent, and consumes by its motion a fifth of the

Part of this effect taken to supply the place of the power that first sets it in motion.

the momentum of the moving power. The remainder serves to raise a weight of twelve pounds with a uniform velocity of an inch in a second: that is to say, the machine continually winds up itself, and leaves a disposable power, equal to four times what would be necessary in an external agent to keep the machine in motion.

It follows from what has been said, that, in the machine of Mr. Cagniard, the heat at least quintuples the volume of air employed in it; since it is evident, that the effect produced must be proportional to the volume of this air dilated. I have said *at least*, on account of the friction to be overcome: but this friction is very trifling, because both the screw and the wheel, being immersed in water, lose a considerable portion of their weight, and consequently press very little on their pivots. Besides, the movements are slow, and not alternative, and there is no jerk in them; so that this machine is free from those resistances, that commonly consume great part of the moving power in others, and accelerate their wear.

The friction very trifling,

and wear but small.

We do not look upon the machine of Mr. Cagniard as an object of curiosity merely: it may be useful under various circumstances. As it produces its effect in a body of water heated only to 75° [167°F.], or even less, it affords an opportunity of turning to account the hot water, that in various manufactories is thrown away, or runs to waste. In saltworks, for instance, the ebullition of the saline solution might be made, by means of Mr. Cagniard's machine, to work the pumps for filling the boilers: in ironworks the heat of the furnace might be made to work the bellows: in common steam-engines, which, like that at Chaillot, furnish a large quantity of very hot water, an action might be obtained equivalent to that of several men, or horses: in fine, in baths, distilleries, potteries, limekilns, glasshouses, founderies, and wherever there is a production of hot water, or of heat, advantage might be made of Mr. Cagniard's machine. This machine, which, as has been said, is liable to very little friction or want of repair, has also the advantage of being easily managed; and when its action is suspended for a time without extinguishing the fire, the heat is not lost:

It may be useful in practice in various situations.

lost: for, as the water is not boiling, the heat accumulates in it, and furnishes afterward a more powerful action.

The screw of Archimedes may be used in this way for blowing large fires with great advantage.

The screw of Archimedes, employed in this machine, produces the effect of a pair of bellows, and might be used as such in a foundery. It may even be considered as the best that is known, not only from its simplicity, solidity, and constant action, but from the saving of power in its use compared with any other machine employed for the purpose; for the screw becomes very light and very movable by its immersion in water, so that the friction of its pivots is next to nothing.

The machine applicable to raising water by means of mercury.

Mr. Cagniard has likewise applied the action of this machine to a body of mercury. As its mechanism requires two fluids of unequal densities, he has merely substituted mercury for water, and water for air, retaining the same construction as is mentioned above. The result is a very simple hydraulic machine, which, without valve, stoppage, or action of fire, being set in motion by any external agent, as a man or a stream of water, gives a continual flow of water at a height fourteen times as great as that of the column of mercury, in which the screw is immersed. This height may even be increased at pleasure, without altering that of the mercury, by combining the action of three fluids, mercury, water, and air. For this purpose, instead of raising a column of water alone a lighter column is formed by a mixture of water and air. This mixture is effected of itself, by disposing the lower part of the pipe that contains this column so as to leave its opening partly in water, partly in air, according as we would have more of one fluid than of the other, and consequently occasion the rise of the mixture to a greater or less height. It is obvious however, that this does not alter the momentum of the moving power, but that, when we would raise the water to a greater height, the machine yields a proportionally smaller quantity. This effect is analogous to that of the Seville pump.

General report.

The machine of Mr. Cagniard appears to us to include many new and ingenious ideas. Its application has been guided by sound theory and a thorough knowledge of the true laws of physics. It appears to us, that it may be useful to the arts on various occasions. We think therefore, that

that the author merits the encouragement of the class, and propose, that its approbation should be given to the machine.

IV.

Description of an Instrument for facilitating the Reduction of Plans; by Mr. DE LA CHABEAUSSIERE*.

I HAVE thought of an instrument for reducing plans, which is so simple, that I am surprised it was not invented by others long ago: but this simplicity, which I consider as an advantage, is probably the reason. I call it a *minudometer*, as its principal object is the reduction of plans; though it will answer equally well for enlarging them. Simple instrument for reducing plans.

This instrument is a wooden rule, with fiducial edges, at the extremity of which is a pivot; or a plate of metal with a hole, into which a pivot may be inserted at pleasure. This pivot is a piece of a needle, with a knob for a head. The minudometer described.

On this rule are marked two scales, one smaller than the other in any proportion you please. As my purpose in making it was chiefly for plans of mines, I took as a basis a scale of 3 lines to a fathom, the proportion generally used for such plans; and for the reduction I employed a scale of one line to a fathom. Such a scale diminishing the length and breadth of a plan two thirds each, all the parts will be brought sufficiently near to be considered at one view. Such a plan may be inferior in minuteness of detail and accuracy to a larger, but it has the advantage of being more portable, and will enable the manager to have a clear idea of the works under his direction. Particularly adapted to plans of mines.

Suppose then I would reduce a plan of three lines to the fathom to a third of this in all its dimensions. I take a rule of two feet long, which appears to me the most suitable length, and divide it into three parts, which makes eight inches, or 96 lines [of course in English measure 80 lines] to each part. On the first division, reckoning from the pivot, I trace the little scale of one line to a fathom, which gives me 96. From the extremity of the small scale I begin the division Method of making the instrument,

* Journal des Mines, Vol. XXVI, p. 461. Extracted from a paper sent to the Council of Mines.

of the larger, and the 192 lines remaining give me 64 fathoms each represented by 3 lines*.

I afterward subdivide each of these scales by three.

and using it.

I fix together the plan and the paper on which it is to be reduced, the latter being under the small scale; and place the rule so that it can traverse circularly as much of the large plan as its extent will admit. The rule being fixed on the large plan wherever it touches a point to be transferred to the paper, I note the number of toises on the large scale, and opposite the same number of toises on the small scale I make a mark with the point of a needle set in a handle, or merely with a fine lead pencil. Thus I set down all the parts of the plan one after another, which are found just and in due proportion.

If the plan to be reduced exceed the length of the rule, the instrument may be removed to another place. I need not mention the necessary precautions in this case for placing the minudometer properly†.

A different construction.

At first I placed my pivot between the two scales, counting the divisions in opposite directions; but as the plan was reversed in this case, I had not the advantage of comparing it readily with the original as I proceeded.

It is obvious, that, if we would have other divisions, we must have different rules, or trace these divisions on paper, and paste it on the same rule. The rule may be graduated also on both sides‡.

V.

* It might be supposed from the text, that Mr. de la Chabeaussiére began to count the divisions of the large scale from this point; but this would be obviously wrong: both scales must begin their count from the pivot, consequently the first division in the larger scale must be reckoned, in the instance before us, as 33, so that both scales will end with 96. It should have been said too, that the pivot, or the hole for it, must be placed in a line with the edge of the scale carrying the divisions. C.

† Though the divisions of the scale amount to 96 toises, there are only 64 that can in reality be used. Consequently it must be necessary to shift the minudometer and the paper once at least. C.

‡ If the edges were bevilled in opposite directions, and the rule were in two parts, made to fit into each other either way where the smaller scale terminates; and the units were of different lengths, though similarly divided; this would give four proportions for diminishing or enlarging. If for instance the principal divisions of one of the large scales were an inch, and

V.

On Mortars and Cements; Experiments that show the Cohesion which Lime contracts with Mineral, Vegetable, or Animal Substances; extracted from a Paper read to the French Institute the 17th of October, 1808, *by* B. G. SAGE*.

HAVING found, that an alkaline lixivial gas was evolved from a mixture of three parts of sand and two of lime slacked by immersion; and desirous of ascertaining, whether the products of the three kingdoms, mingled in the same proportions, would afford a similar gas; Mr. Sage made a number of experiments, which taught him, that the force of cohesion contracted by slacked lime was greater with metallic oxides in general, than with any other substance. These trials led him to new facts, which enabled him to discover mortars, or cements, at least as solid and impermeable as those made with the best puzzolana, which is of the greatest use, particularly in hydraulic structures.

Gas evolved from lime and sand.

Metallic oxides strengthen mortar.

The work we announce points out also a prompt and easy method of ascertaining the solidity and impermeability of mortars or cements, which cannot but be highly interesting to builders.

We must not always judge of the goodness of a cement from its having acquired a great deal of solidity in the open air, for it frequently loses this in water, in which it diffuses itself. Buildings made with such mortar soon tumble to pieces.

Mortar solid in the air may not stand water.

The necessity of a minute division of the substances, that enter into a cement, cannot be insisted on too strongly. They should first be mixed together uniformly while dry; and they must not be drowned in water, which must be added gradually, till the mixture is reduced to a soft paste.

Rules for making good mortar.

and of the other an inch and half; and those of the small scales, one half an inch, the other a quarter; we should get the proportions of a half, a third, a fourth, and a sixth. Two rules, with joints mutually fitting each other, would give 16 different proportions. If both edges be graduated, there must of course be a hole for a pivot at the extremity of each. C.

* Journal des Mines, vol. XXVI, p. 471. The above appears to be the title of a pamphlet, which Mr. Sage has published separately.

It

It is of the greatest importance to determine with precition the quantity of lime employed to obtain the most solid mortars or cements; and in general to use no lime but what has been made from pure limestone, and which has been kept well secured from the air after it is slacked.

Two parts of lime to three of other matter.

In the experiments of Mr. Sage he always employed two parts of lime to three of puzzolana, of sand, &c., which afforded him very hard and impermeable mortar: and he thinks this proportion of lime may even be lessened, when the architect is fully convinced of the impropriety of leaving the preparation of mortar to bricklayer's labourers, since the strength and solidity of hydraulic structures depends so much on it.

Mortars of lime and ashes.

The author has divided his experiments into five classes. 1. Mortars or cements made with substances, that have undergone the action of fire. The ashes of vegetables, whether lixiviated or not, being mixed with two thirds of lime slacked by immersion, forms one of the most solid and impermeable cements: a property which they appear to derive from the minutely divided quartz, which these ashes contain in the proportion of one fourth.

Lime and iron oxide.

2. Mortars or cements made with metallic substances. Iron adds to the hardness of all mortars; and of itself, in rusting, concurs in the agglutination of gravel and pebbles, as we see on the seashore. According to the state in which the iron is, that is combined with two parts of slacked lime, its force of cohesion is more or less considerable.

Iron alone a cement.

Lime and different stones.

3. Mortars or cements made with stones of different natures. Gæstein, chalcedony, sandstone, and gravel, form very hard and impermeable mortar with lime. Feldspar, better known by the name of petuntze, being mixed with two thirds of slacked lime, produces an impermeable and solid mortar.

Lime and mould.

4. Mortars or cements that alter in water. Vegetable earth, or mould, is essentially composed of minutely divided quartz, clay, and iron. Mixed with two parts of slacked lime, and water enough to form a soft paste, the brick produced from it, when dried, has some solidity, which it loses under water, where it cracks.

Lime and

5. Mortars or cements made with combustible substances. Mortar,

Mortar, or cement, made with sulphur and two parts of slacked lime, forms a hard and very sonorous brick, which is not altered under water; while mortars made with pulverised vegetable charcoal, or pitcoal, though they produce hard and sonorous bricks, soon fall to pieces in water; as do bricks made with sawdust, or raspings of ivory.

combustible substances.

VI.

Observations on the Alkaline Metalloids: *by Mr.* BUCHOLZ *.

THE quantity of metalloid substance obtained varies considerably. In an experiment made lately in my apparatus with three ounces of potash, six drachms of charcoal, and an ounce and half of iron, I obtained but one drachm of metalloid, divided into four or five pieces. In the tube were found thirty grains more of metalloid, clotty, and contaminated with charcoal; yet all the vessels had stood well, and remained impervious to air. The residuum, which furnished prussiate of potash, still contained however a large quantity of charcoal. It is clear therefore, from the small quantity of the product obtained, that it is not the whole of the charcoal, but perhaps only the hidrogen it contains, which concurs in the formation of the metalloid.

The quantity of metal obtained by means of iron varies.

Not being able to determine the specific gravity of the metalloid, as it alters so quickly in the air, I thought of composing an oil of the same density, in which it would neither sink to the bottom, nor float on the surface, and which consequently would be of the same specific gravity. This I did by mixing oil of petroleum and lard. The specific gravity of this mixture was 0·876.

Its specific gravity ascertained by a mixture of lard and oil of petroleum.

Twenty-five grains of the metalloid, converted into potash by water, and saturated with muriatic acid, produced 45 grains of fused muriate of potash, which, according to Rose's analysis, would contain 30 grains of potash and 15 of acid: but, as only 25 grains of the metalloid were employed,

Metalloid converted into potash increased in weight 0·2.

* Ann. de Chimie, vol. LXXIII, p. 78. Translated from Gehlen's Journal for May, 1808, by Mr. Tassaert.

ployed, there was an increase of 0·2, which favours the opinion of the alkalis being metallic oxides; otherwise we must suppose, that this increase of weight arises solely from the water of crystallization.

Its combustion rendered limewater turbid, owing probably to carbon from the adhering oil.

Into a well closed bottle, containing four ounces of limewater, above half a grain of the metalloid in several globules was introduced. The combustion was effected very speedily, and the water was rendered very turbid every time the globules sunk down, as Curaudau had observed. It might be presumed therefore, that the metalloid contained carbon; but, as it is very difficult to separate all the adhering oil, it may still be supposed, that the carbonic acid came from this oil. I thought I should obtain a much more certain result, by converting the metalloid into an amalgam with mercury, and thus immersing it in limewater, which would prevent the combustion of the oil. In this process the evolution of gas was very brisk, without the water becoming turbid; but the gas gradually ceased to be evolved, and the surface of the amalgam became covered with a light gray pellicle, which rendered the fluid turbid and gray, but not milky. A few drops of nitric acid did not make this cloudiness disappear, and the mixture acquired a metallic taste. I poured distilled water on the remaining amalgam, and the evolution of gas commenced anew with a great deal of energy; but no pellicle was formed, and the liquid did not become turbid. This result may be explained on the supposition, that the contact of limewater favours in some degree the oxidation of the mercury; though it is not easy to say why this should take place, as it does not with distilled water, and accordingly no pellicle is formed. As no trace of carbonate of lime appears, it may be concluded, that the metalloid contains no carbon; but it would be well to confirm this by fresh experiments.

The combustion of its amalgam did not.

Amalgam differs according to the proportion of mercury,

On triturating one part of metalloid of potash with thirty of mercury in a porcelain mortar, a pretty ductile amalgam was formed, resembling amalgam of tin: but with ten or twenty parts of mercury a gray pulverulent substance only was obtained, which assumed a metallic brilliancy by pressure. On continuing to bray this substance, it became moist,

moist, formed at length an alkaline liquid, and the mercury became fluid. The tendency of this amalgam to combine with other metals is surprising, it combines even with iron at the instant of contact, and extends on its surface; but after some time the metalloid returns to the state of potash, and the mercury separates from the iron.

& has a strong affinity for other metals.

Twenty-five grains of the metalloid of potash being heated red hot in a narrow-mouthed vessel, the small globules united into larger, which had a bright metallic lustre, that was a mean between that of tin and that of silver, and were very fluid. On cooling they assumed the appearance of a hard amalgam of tin. In the open air they became covered at first with a gray coat, which became blue in a greater heat, and the blue colour of which grew much deeper, when the gray pellicle was removed from the melted matter. On heating it more strongly all the colour disappeared, and the whole assumed a silver whiteness, with a metallic lustre, which became gray on cooling. A little of the fused matter, being brought into contact with the air, took fire, and gave out a white vapour, not alkaline, which deserves examination. On heating it to a cherry red, a liquid matter was produced of a yellow brown colour, and destitute of metallic lustre, which gradually became of a blue green, and comported itself as a siliceous compound that attracted moisture from the air. Potash therefore was formed without previous inflammation, and the metalloid of potash had attacked the glass, agreeably to the experiments of Mr. Davy.

Potassium heated red hot in a narrow mouthed vessel,

and afterward exposed to the action of air,

and of heat.

Some time ago I treated alkaline matter, from which I had failed to obtain metalloid, with linseed oil, according to Curaudau's process*. Having subjected it to a very violent heat, I could obtain no fluid metalloid in the receiver; but in the neck of the retort I found a portion in clots, mixed with carbonaceous matter, weighing about two drachms. On heating it, and straining it through a rag under heated petroleum, I obtained half a drachm of liquid metalloid.

Potassium obtained by means of linseed oil.

The residuum still comported itself like the pure metal-

* See Journal, vol. XXIV, p. 38.

loid

A detonating pyrophorous coal produced.

loid with water, mercury, and other substances. The coally matter, from which the metalloid had been separated, appeared to me a detonating pyrophoric product of a peculiar nature. It had the following properties. Its colour varied from deep black to brownish black, and black blue. It had a greater or less degree of cohesion, a pulverulent consistency, but requiring the stroke of a pestle to reduce it to powder. The pulverulent part inflamed with noise on the contact of air; but the large pieces did not take fire, till they had remained exposed to the air some time. They inflame more quickly when moisture is near. On triturating, striking, or pounding this matter with a solid body, it detonates with more or less noise, with flame, and with dispersion of the matter when the pieces are large. The noise resembled loud cracks of a whip. I have even observed, that this decomposition of the metalloid with noise takes place sometimes under water, and occasions a violent commotion in it. This detonating product was near occasioning me as disastrous an accident, as the metalloid did Mr. Gay-Lussac; for, in attempting to get all the matter out of the neck of the retort with a sharp-pointed iron wire, a portion detonated with a great deal of noise, and almost all the burning matter flew by my face. It is obvious therefore, that we cannot be too cautious in operating on this substance *.

Its properties.

Dangerous accidents.

Action of potassium on oil of turpentine.

On another occasion I observed a very violent action from some coally matter filled with metalloid. I poured about a drachm into an ounce of rectified oil of turpentine, and immediately perceived a very strong ebullition of the oil, part of which was volatilized in smoke. What remained had almost entirely lost its smell, but had acquired a striking smell of solution of camphor in oil of turpentine, yet I could not by any means discover the presence of camphor.

Detonating mixture.

* A mixture of sulphate of potash and vegetable charcoal, in a large proportion, produces a similar effect. *Collet-Descotils.*

VII.

VII.

Farther Observations and Experiments on Oximuriatic Acid, by J. Murray, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR,

In a former communication I had given an account of some experiments, which I regarded as subversive of Mr. Davy's lately proposed hypothesis on the nature of muriatic and oximuriatic acids. Of these some of the results were called in question by that gentleman, particularly that in which carbonic oxide, hidrogen, and oximuriatic acid gasses were subjected to mutual action, either at a low temperature or by detonation. The production of carbonic acid in this experiment he appeared to have considered as arising from the operation of the water introduced with the view of examining the product; he employed therefore dry ammoniacal gas, and with this variation he stated, that there is no conversion of carbonic oxide into carbonic acid. Though satisfied, that there is little probability in the supposition of any fallacy from this source, I thought it right to repeat the experiment so as to exclude its operation, and having lately done so, I beg leave to communicate the result.

Result of the action of carbonic oxide, hidrogen, and oximuriatic acid, questioned.

I may previously remark, that I had objected to the imperfect manner in which Mr. Davy's experiment was executed; no attempt apparently having been made to discover if carbonic acid were formed, but its nonformation having been inferred merely from the residual gas burning with the same coloured flame as carbonic oxide. This has since been attended to, and the experiment performed with a more strict examination of the result. An account is given by Mr. J. Davy in his last communication of this repetition of the experiment. A mixture of 10 measures of carbonic oxide, 4 measures of hidrogen, and 14·6 measures of oximuriatic acid gas contaminated with 2 of common air, was inflamed by the electric spark; the residual air being detonated with oxigen was found to contain 8 measures only of carbonic oxide; 2 measures of this gas therefore had disappeared,

Mr Davy's experiments on the subject

appeared, and it appears to be admitted in the statement of the experiment had been converted into carbonic acid, as indeed no other conclusion could be drawn. But this is ascribed to the action of the common air, or of moisture in the gasses; and it is inferred, that, when the action of these is taken into account, "no result more satisfactorily con- "clusive that no carbonic acid was formed could be ex- "pected."

produced carbonic acid, It is at least established, that in this experiment, when the results are submitted to accurate examination (even with the precaution, which Mr. Davy deems so essential, of substituting ammonia for water), there is a conversion of carbonic oxide into carbonic acid. The *fact* therefore is admitted, which I had asserted, and which had been before denied. The *suppositions* by which it is now attempted to be accounted for I regard as unsatisfactory, no proof being given, either that the causes assumed did operate, or were adequate to the production of the effect. With regard to the *supposed* operation of the atmospheric air mingled with the oximuriatic gas, it is not probable, that, diluted as it must be by the large intermixture of elastic fluid, its oxigen would combine with the carbonic oxide in the feeble inflammation, which from the small portion of hidrogen employed would take place in the experiment. And even if it had combined, the quantity of it was not sufficient to have converted into carbonic acid half the quantity of carbonic oxide which disappeared. With regard to the *supposed* effect from moisture, as the carbonic oxide and hidrogen gasses were previously dried, it can scarcely be assumed to have been present to the extent which it is necessary to suppose, allowing even that it could operate in the momentary action from the detonation. And if there were grounds for supposing, that these circumstances were of any importance in producing the result, why were they allowed to operate? It is easy to obtain oximuriatic gas without such an intermixture of common air as 2 measures in 14; it can also be dried by submitting it to the action of substances which abstract water. When they could thus have been excluded, the only reason that could justify this admission was the belief, that their influence was so unim- portant

not satisfactorily accounted for.

portant that it might be disregarded. But to admit them, and at the same time to assume that their operation had given rise to the result, the possibility of obtaining which independent of such circumstances is the very question at issue, appears to be making by choice an ambiguous instead of a decisive experiment. I am satisfied however, that these circumstances had no important effect. And when we have the actual formation of carbonic acid, and only such modes of accounting for it to avoid the conclusion, that oxigen is communicated from oximuriatic acid, I cannot but regard the result as being in conformity with that which I have always stated to be obtained.

The proportion of hidrogen in them too small.

One other observation with regard to this experiment I find it necessary to make. In employing hidrogen gas to promote the action of oximuriatic acid on carbonic oxide, the proportion I used was equal volumes of the hidrogen and carbonic oxide, and in the repetition of the experiment with the view of ascertaining if the result I had stated were accurate it was to be expected, that the same proportion would have been observed. Mr. Davy in his former experiment used the proportion of 8 parts of hidrogen to 10 of carbonic oxide, a deviation of no great importance, and of which therefore I did not think it necessary to take notice. But he has now employed the proportion of only 4 measures of hidrogen to 10 of carbonic oxide. I know not what may have been the reason for this change of proportion, but it is obvious what effect is to be expected from it. I had found, that dry carbonic oxide gas, and oximuriatic acid gas, do not act on each other; and I had affirmed, that they do act, and that there is a production of carbonic acid, when a portion of hidrogen is added. According to the view with which that hidrogen was added, that of affording a certain portion of water necessary to the constitution of muriatic acid gas, the larger the quantity used, the conversion of carbonic oxide into carbonic acid by the oximuriatic acid might be expected to be more complete. Mr. Davy repeats the experiment with the view of disproving the result I had affirmed to be obtained; but he reduces the proportion of hidrogen more than one half; and from not attending to the effect of it, he withdraws as far as possible the

the very circumstance held to be essential to its success. And still with this variation part of the carbonic oxide is converted into carbonic acid.

Farther experiments substituting ammonia for water.

I have now to state the results of the experiments I have performed, substituting ammonia for water, in examining the product of the mutual action of the gasses. I was assisted as before in making these experiments by my friend Mr. Ellis, and the results were witnessed by some other friends.

Exp. 1.

Ten measures of carbonic oxide gas, and 10 measures of hidrogen gas, each of which had been previously dried by exposure to lime, and 20 measures of oximuriatic acid gas, obtained from a mixture of muriate of soda, black oxide of manganese, and diluted sulphuric acid, and which had been kept in contact with muriate of lime, were mixed together in an apparatus fitted with stopcocks, so that the gasses could be transferred and mingled without the intervention of water or of quicksilver. The mixture was exposed to light, excluding the direct action of the solar rays, for about 36 hours. At the end of that time, the apparatus being opened under dry quicksilver, a small quantity only entered, indicating a very inconsiderable diminution of volume; and the quicksilver acquired a slight tarnish, a proof of the presence of a small portion of oximuriatic acid. The gas was transferred through dry quicksilver into an inverted jar; and ammoniacal gas, which had been previously dried by exposure to lime, was added to it. Dense white vapours were abundantly produced, and the introduction of the ammonia was renewed from time to time, until their production had ceased. A little water was then introduced to absorb the excess of ammonia, and dissolve the concrete salt that had condensed. The solution was rendered turbid by the test of muriate of barytes, indicating the production of carbonic acid*.

Carbonic acid apparently produced.

I soon

Presence of carbonic acid not always immediately perceptible.

* In some experiments this result was not obtained, or the transparency of the solution was at least little impaired. To discover the cause of this I had dissolved small portions of muriate and carbonate of ammonia in water, thus preparing a solution similar to that which I supposed to be formed in the experiment; but on adding to it muriate of barytes there

I soon ascertained this in a manner altogether unequivocal. The concrete salt, condensed on the sides of the jar when the action of the ammonia had ceased, being collected, on dropping it into dilute muriatic acid, a sensible effervescence was observed, especially when it had been taken from the upper part of the inverted jar. This latter circumstance appeared to indicate, that the ammonical gas, when introduced to the elastic fluid remaining after the mutual action of the three gasses, had combined first with the muriatic acid, and afterward more slowly with the carbonic acid that had been formed, so that the product of this latter combination had been deposited principally towards the head of the jar; a result which might indeed be expected from the more powerful action of muriatic acid, than of carbonic acid, on ammonia. This afforded a mode of obtaining the two products in a great measure separate. On adding the first portion of ammonia, the white vapours were allowed to condense, the residual gas was transferred into another jar, and a fresh portion of ammonia added. The salt obtained from the sides of the first jar was principally muriate of ammonia, that from the second was carbonate, and when dropped on a dilute acid effervesced as strongly as pure carbonate of ammonia did. The production of carbonic acid was established therefore beyond the possibility of doubt: it farther appears, that the conclusion I had drawn from my former experiments was correct, and that there is no fallacy in the introduction of water after the mutual action of the gasses to examine the product, the result being equally decisive when ammonia is employed.

Formation of carbonic acid proved.

Exp. 2.

The residual gas in these experiments was found to be a mixture of hydrogen and carbonic oxide, with a little nitrogen

Residual gas.

there was no precipitation; and I farther found, that the transparency of a solution of pure carbonate of ammonia is not immediately impaired by this test. This may be ascribed partly perhaps to the action of ammonia counteracting the formation of carbonate of barytes, but principally to the excess of carbonic acid in the carbonate of ammonia, which contributes to retain the barytes dissolved. Hence subcarbonate of ammonia gives a precipitate with muriate of barytes, and in the above experiment the solution became turbid on the addition of the muriate only when an *excess* of ammonia had been added to the elastic fluid formed by the mutual action of the gasses.

trogen

trogen arising from the action of the oximuriatic acid on the ammonia. When mixed with atmospheric air and kindled, it burned not with the blue lambent flame of carbonic oxide, but with the quick flame of hidrogen, and afforded by its combustion only a small quantity of carbonic acid. This residue of inflammable gas, while there also remained a small excess of oximuriatic acid, is probably to be ascribed to imperfect exposure to light.

The slow action preferable to detonation,

In performing these experiments I preferred the method of submitting the gasses to slow mutual action at natural temperatures to that of promoting it by detonation, both as capable of being conducted with more accuracy, and in itself more conclusive. In the mode by detonation it is necessary to operate over quicksilver, and from the action of the oximuriatic acid on the quicksilver it is more difficult to observe the phenomena of the experiment, and to estimate the results. In the slow action this may be avoided. We farther avoid any fallacy which may be supposed to arise from the high temperature in favouring the decomposition of any water that may be present. And the mutual action, from its continuance, appears to be more complete. I confirmed however the preceding results to a certain extent, by performing the experiment by detonation, the test of muriate of barytes indicating the presence of carbonic acid in the solution formed by the introduction of water after the ammonia.

but the latter gave similar results.

Experiments of Mr. Cruickshanks

Mr. J. Davy in his first reply to my observations on this subject stated, that he had repeated some of Cruickshank's experiments on the production of carbonic acid by the action of oximuriatic acid on the carburetted hidrogen gasses. When the experiment is made over water, some ambiguity may be supposed to arise from its influence. But even when it is excluded, a portion of carbonic acid ought to be formed from the agency of the hidrogen similar to that in the preceding experiments: and I did not make the experiment to ascertain this only from the uncertainty with regard to the existence of oxigen in the composition of these gasses, which, if carbonic acid were formed, it might be contended contributed to its formation. Mr. J. Davy however considering this source of fallacy as of little importance, performed

repeated by Mr. Davy.

formed the experiment, excluding water, and stated, that he "never obtained carbonic acid gas, though oximuriatic gas in great excess was employed." I alluded briefly in my reply to the source of errour whence this observation I conceived had arisen, and I now find my conjecture to have been just. I had found in the case of the production of carbonic acid from the mutual action of oximuriatic acid, hidrogen, and carbonic oxide, that no milkiness is apparent on the first or even the second transmission of the gas through lime water, the small portion of remaining muriatic or oximuriatic acid preventing the formation of carbonate of lime. I had no doubt that this had operated in Mr. Davy's experiment, especially as he laid stress on the very circumstance which would give rise to it, the great excess of oximuriatic acid employed; and I have found, that this is the case. One measure of carburetted hidrogen gas obtained by passing watery vapour over ignited charcoal, freed from any intermixture of carbonic acid by careful agitation with lime water, and afterward dried, was mixed with a measure and a half of oximuriatic acid gas passed over dry muriate of lime; the mixture was inflamed over dry quicksilver by the electric spark; the residual gas was transmitted first through water, and afterward through lime water; no milkiness was apparent in the latter on the first or second transmission, but on the third the surface became milky, the whole became turbid on agitation, and this was repeated on two or three subsequent transmissions. The production of carbonic acid was therefore not in the least doubtful*.

Mr. Davy.

Source of errour.

The experiment repeated,

and carbonic acid produced.

* The residual gas in this experiment burned with the blue lambent flame of carbonic oxide, and gave carbonic acid in its combustion. If it were found to be carbonic oxide, it would prove, on the supposition that the gas from humid charcoal after careful washing with water is a binary compound of carbon and hidrogen, that still more oxigen had been communicated from the oximuriatic acid, than had gone to the formation of the carbonic acid; or, if this were not admitted, the result would throw some light on the disputed question with regard to what are named the carburetted hidrogen gasses, whether oxigen exists in their composition; as it would render probable the opinion, that this gas at least is a ternary compound of carbon, hidrogen, and oxigen, in opposition to the opinion, that it is a binary compound of carbon and hidrogen.

Residual gas.

All the results confirm the former statement.

The results of all these experiments then, instead of invalidating, confirm what I have before stated. The sources of fallacy supposed to exist have been found to have no effect; and the more accurately the results have been examined, the more strict has been the coincidence with that statement. In all of them carbonic acid has been found to be formed, and Messrs. Davys appeared not to have obtained it in their experiments, because they did not look for it with sufficient care, or were not sufficiently aware of the fallacies, by which its production might be concealed.

On the other topics of this discussion I am pleased to find, that it is not necessary for me to enlarge; as, with regard to those of any importance, Mr. J. Davy has in his last communication either attempted no reply to my observations on his former statements, or the reply is in general such, that, with a few remarks, I willingly leave the decision to the judgment of those, who have given attention to the question.

Remark on the assertion, that Mr. Davy's statement is not hypothetical.

He still for example professes to maintain, that the proposition "muriatic acid gas is a compound of oximuriatic acid and hidrogen" is not an inference from the fact, that this gas is obtained from the mutual action of these two substances, but is the expression of the fact itself; that because they are the only substances concerned in the experiment, and it is equal in weight to the weight of them employed, "muriatic acid gas is not inferred, but immediately perceived to be a compound of oximuriatic gas and hidrogen, and that all the other cases are analogous." His brother's views therefore he contends are not hypothetical; and, if I fail in proving them such, I fail, he adds, altogether. I confess I have felt surprised, that this ground of defence ever has been assumed, and that Mr. H. Davy should have remarked, that I have mistaken his views in supposing them to be hypothetical, adding, that "he merely stated what he had seen, and what he had found." And although Mr. J. Davy might at first have adopted these sentiments, I had hoped, that the observations in my former paper would have convinced him, that this view was a hasty one, that these pretensions were too high, and that the subject might be presented under a very different aspect. If I have failed in in this, I must despair of being more successful by any farther illustrations, and I feel indeed no desire to add illustrations on what appears to me too obvious to bear a moment's reasoning. I shall only present the subject under one other light; and beg to remind him, that the very possibility of the proposition being called in question without any doubt being expressed of the accuracy of the experiment on which it rests is a sufficient proof, that it is not a simple expression of the fact, as he and his brother suppose, but an inference from the fact. I should not involve myself in the absurdity, or rather in the palpable contradiction of denying, that muriatic acid gas is *obtained* from the mutual action of oximuriatic acid and hidrogen, and is the only sensible product of that action, while I did not call in question the accuracy of the experiment of which this is stated to be the result; though I feel no hesitation in denying (equally admitting the experiment) that muriatic acid is a compound of oximuriatic acid and hidrogen. I perceive an essential difference between these two propositions; the one (supposing the experiment accurate) is a simple expression of a fact; it will for ever remain true, be the progress of the science what it may, and no one who understands the terms in which it is expressed will call it in question; the other is an inference from the fact, which may be questioned, and may prove to be false. If Mr. Davy however can perceive no difference between them, he is right in maintaining, that his brother's opinion is a genuine theory. I trust I need not add, to avoid misconception, that I have admitted, that, were our induction to be restricted to this fact, the conclusion drawn by Mr. Davy, as it is the most direct, would be the most probable one; it is only when connected with the other phenomena to which it is related, that it becomes more doubtful; it then comes in contact with a different conclusion, which may be drawn, and which in relation to some of these phenomena has in its turn the advantage of being more directly inferred; the two are to be compared in their whole extent, and the one which in its application to all the phenomena shall appear most probable is to be preferred. It is altogether a limited view, to look only to the experiment of the production of muriatic acid gas from the mutual

Remark on the assertion, that Mr. Davy's statement is not hypothetical.

mutual action of oximuriatic acid and hidrogen; for, to draw the conclusion from that experiment, we must previously know what is the constitution of muriatic acid gas, and what the constitution of oximuriatic acid; and the most probable inferences with regard to these must regulate the conclusion that ought to be drawn. If there is reason to believe, that the former is the real acid, and that the latter is a simple substance, it may be inferred, that muriatic acid gas is a compound of oximuriatic acid and hidrogen. But if there are facts whence it can be inferred, that muriatic acid gas contains water, or that oximuriatic acid contains oxigen, the theory of the experiment must be given in conformity to these—the oxigen of the oximuriatic acid combining with the hydrogen, and forming water, which the muriatic acid holds combined with it in the elastic form. There are facts from which these are the most *direct* and *probable* conclusions; and these conclusions are avoided by less direct, and more complicated and hypothetical assumptions. And it is merely an errour in logical deduction to suppose, that such assumptions require no independent proof, but are established because they would follow if the inference were admitted, that muriatic acid is a compound of oximuriatic acid and hidrogen.

Most obvious conclusion not always just.

To some of the examples which I had given, illustrating the general proposition, that the most obvious conclusion from an experiment is not always the just one, Mr. J. Davy has stated some objections, which perhaps it is superfluous to notice; for, were even the illustrations incorrect, the proposition itself cannot be denied, and it might be easily illustrated by other examples. The truth however is, that the examples I have given remain in full force. To one of them indeed, that from the production of dry muriate of potash, no objection has been offered. With regard to the other, that of the production of calomel by combining muriatic acid and oxide of mercury, there may be, as he supposes, a production also of water, (though this remains to be proved) yet still the most direct inference from the experiment is, that calomel is a compound of the oxide and acid; for it is a more simple conclusion, that this water had been deposited from the acid, than that it had been formed by the

Production of calomel.

the oxigen of the oxide combining with hidrogen from the acid, while the oximuriatic acid combines with the metallic mercury to form the calomel. Mr. Davy seems to doubt indeed if calomel can be formed by presenting muriatic acid to oxide of mercury. If the metal is highly oxidated, corrosive sublimate, it has long been known, is formed; but it is equally true, that calomel is the product of the mutual action of muriatic acid and mercury in a low state of oxidation.

Production of oximuriatic acid from muriatic acid and oxide of manganese.

I had given as an example of hypothetical assumption in Mr. Davy's system, the explanation of the production of oximuriatic acid by distilling muriatic acid from oxide of manganese; the explanation *supposing*, that the oxigen of the oxide combines with the hydrogen of the acid and *forms* water, while the oximuriatic acid is set free. To this Mr. J. Davy replies: "Mr. M. asserts, that Mr. Davy is obliged to *suppose*, that water is produced in the common mode of making oximuriatic gas from muriatic acid by means of the black oxide of manganese. Mr. Davy has ascertained the *fact*, that oximuriatic gas and water are produced, when black oxide of manganese is heated in muriatic acid gas." It is almost superfluous to remark, that here the leading term in the proposition, and on which the whole discussion rests, is changed. I had asserted, that Mr. Davy is obliged to suppose, that in this experiment water is *formed*: and the assertion is strictly correct. To say, that Mr. Davy has ascertained the fact, that water is *produced*, is saying nothing to the point. The *production* of water in an experiment is not its *formation*, nor is it a proof of it; it is as probable *a priori*, that it is deposited, as that it is formed: unless there be particular evidence indeed for the latter conclusion, the former is to be preferred as more simple and direct; and though water is produced, in other words becomes sensible, when muriatic acid gas acts on black oxide of manganese, I repeat, that Mr. Davy is obliged to suppose it is formed; and that he has no other proof of its formation than the supposed truth of his hypothesis, which is of course assuming the point in dispute.

Distinction between pro-

In a different part of his reply Mr. J. Davy, from not attending to this distinction between the production of water and duction and formation.

and the formation of water, has supposed, that I have neglected its agency in an experiment, when I only suppose the most direct conclusion, and the one most strictly analogous to that which would be formed in similar cases, to be drawn. If muriatic acid gas in acting on a metallic oxide disappears, forming a solid product, while water is also produced, the most obvious and direct conclusion, and the one most conformable to a very extensive analogy is, that the acid has combined with the oxide, and that the water had been previously combined with the acid, but does not enter into the new combination. If nitric acid vapour, or sulphuric acid vapour, were transmitted over a metallic oxide, with similar results; the disappearance of the acid, the formation of a solid product, and the production of water; this is the very conclusion which I suppose Mr. J. Davy would consider as the legitimate one; and it may be well for him to consider a little further the grounds on which he violates this mode of induction, in refusing to draw a similar conclusion with regard to muriatic acid.

Hypothetical explanations in Mr Davy's system.

I had given some examples of hypothetical explanations in Mr. Davy's system, which I regarded as much more complicated than any of those which are given in the opposite opinion. Mr. J. Davy has endeavoured to render them more simple, but I fear with little success. Dry muriate of potash is regarded as a compound of oximuriatic acid and potassium. On dissolving it in water, I conceived, that, in conformity to the system, it was supposed to be converted into a compound of muriatic acid and potash, a portion of the water being decomposed, its oxigen communicated to the potassium, and its hidrogen to the oximuriatic acid: and that again in expelling the water from this solution, and obtaining the dry salt, the hidrogen of the acid and the oxigen of the potash combine, forming water, while the oximuriatic acid and the potassium enter into union. In giving these as the explanations which are conformable to Mr. Davy's system, I believe I have done it justice; and that, though sufficiently hypothetical and complicated, they are the most probable of which it admits, and are in conformity to his own statements: "the action of water on these compounds which have been usually considered as muriates,

muriates, or as dry muriates, but which are properly combinations of oximuriatic acid with inflammable bases," being stated to be exactly that which I have described; oxigen being supposed to be communicated to the base, and hidrogen to the oximuriatic acid. Mr. J. Davy rightly remarks, that they are *conjectures*: and to avoid them, he satisfies himself with remarking, that "fused muriate of potash is a compound of oximuriatic acid and potassium; and the solution of muriate of potash is a compound of oximuriatic acid, potassium, oxigen, and hidrogen." If fused muriate of potash is a compound of oximuriatic acid and potassium, how can it be obtained from the watery solution formed by uniting potash and muriatic acid, by the evaporation of the water, without the very changes taking place, which I have stated? With regard to the changes that occur on dissolving it in water, Mr. Davy gives a view indeed somewhat different from that which I had stated, but not much to the advantage of his hypothesis. It is not merely it seems a portion of water that is decomposed so as to form muriatic acid and potash; but the whole water, if the above statement of Mr. Davy with regard to the nature of this solution have any distinct meaning, is decomposed; and the solution is a quaternary compound of potassium, oximuriatic acid, oxigen, and hidrogen, in which neither potash, muriatic acid, nor water exists. If this is simplifying chemical theory, and rendering it more probable, we have hitherto been much mistaken in our notions of simplicity and probability. I know not if Mr. J. Davy is prepared to extend the same view to other analogous cases, and to say for example, that, when we dissolve sulphate of soda in water, we form a compound of sodium, sulphur, oxigen, and hidrogen. It is needless to analyse the other example of the mutual action of nitrate of mercury and muriate of soda, as the same remarks nearly apply to it.

Nonaction of oximuriatic acid on charcoal.

I had stated the fact of charcoal not being acted on by oximuriatic acid as presenting an anomaly in Mr. Davy's hypothesis; for, since oximuriatic gas is held to be a principle similar to oxigen in its general chemical agencies, it ought like oxigen to combine with charcoal, and still more so, since it combines with all other inflammable and metallic

tallic substances. Of this a satisfactory explanation may be given in conformity to the opinion, that oximuriatic acid acts on inflammables by imparting oxigen; while it remains an anomaly in the opposite opinion. To this the reply is made by Mr. J. Davy, that I seem to consider every thing anomalous, that is not accounted for; and the query is added, "Can Mr. M. account for the want of action between charcoal and nitrogen, and between the metals and nitrogen? and, if he cannot, does he consequently consider these facts anomalous?" The fallacy of this reasoning I should scarcely have supposed could have escaped observation. The anomaly with regard to charcoal is not simply, that it is not acted on by oximuriatic acid, as it is not acted on by nitrogen; but that, being an inflammable substance, and every other inflammable being acted on by oximuriatic acid, it is not. Inflammable substances are not acted on by nitrogen, we have therefore no reason to expect any action to be exerted by it on charcoal; while there is reason to expect, that charcoal, in common with other inflammable substances, should be acted on by oximuriatic acid; in the one case there is no general result, to which an exception occurs; in the other there is, and there is therefore an anomaly. Of this singularity with regard to charcoal, the explanation which may be given in conformity to the common opinion is so satisfactory, as to afford even a presumptive proof of the truth of that opinion, the fact being precisely what might be expected to occur. On Mr. Davy's hypothesis it is confessedly incapable of being accounted for.

New gas observed by Mr. Davy.

With regard to the new gas, which Mr. Davy has observed, a compound, as he regards it, of oximuriatic acid and oxigen, I have little to say. Without speaking lightly of it, as Mr. J. Davy imagines; or without doubting, that it may be able to convert carbonic oxide into carbonic acid; I may simply remark, that I have no reason to believe, that it operated in my first experiments; it no doubt was excluded in the repetition of the experiment by Mr. H. Davy, in which, as has already been remarked, carbonic acid is formed; and I have farther avoided it in the experiments stated in this communication, without finding any differ-

ence

ence in the results. The difficulties which Mr. Davy has supposed attend the common opinion from the comparative inactivity of this gas, though it contains more oxigen than oximuriatic acid, and which he imagines will probably lead me to "adopt the new idea, that oximuriatic gas is a simple body," appear to me of no weight. The powerful action of oximuriatic acid does not depend merely on the *quantity* of oxigen it contains, but on the state of combination of that element, and the disposing affinity exerted by the muriatic acid; and I can easily suppose the quantity of oxigen to be increased without any augmentation, or even with a diminution of power. It will be time enough however to explain this to Mr. J. Davy, when the properties and composition of this new compound are more fully detailed.

I have now given all the attention to this controversy, which it appears to me to claim; and the progress of it has, I trust, shown more clearly, that the common opinion of the relation between muriatic and oximuriatic acids is still the most probable one, inferred by the most simple and direct induction, and in strict conformity with the established theory of acidity, and the chemical agencies and combinations of acids; while the opinion of Mr. Davy, instead of being, as it has been contended, a simple expression of facts, is an hypothesis, involving assumptions gratuitous and complicated, and at variance with extensive and well established analogies. The experimental proof I have brought forward, and which I consider as sufficiently confirmed, is still farther, it appears to me, conclusive in support of the opinion I have maintained. I regard the discussion on my part as closed, and I shall not be disposed to resume it, unless some new facts or arguments are adduced sufficiently important to demand consideration.

I am, with much respect,

Your most obedient servant,

Edinburgh, June the 7th, 1811. J. MURRAY.

P. S. Mr. J. Davy, in a communication in the number of your Journal for May, has stated a series of facts, from which

On the nature of the metalloids.

which he has inferred, that the opinion I had advanced with regard to the nature of potassium is unfounded. He will have observed, that, in my second paper, published in the supplement to your last volume, which accompanied that number, I have taken notice of the greater number of these facts; and, that I had given them due consideration both in conducting the additional experiments of which I have given an account, and in forming the conclusions I had drawn. It is therefore unnecessary for me to make any observations on his statements in their present form. The whole subject, from the difficulties which attend it, I consider as open to farther investigation, though I may add, that, without placing any undue confidence in my own experiments, I do not consider their results as invalidated; and, that I still regard the view I have given of the nature of the metalloids as the one which is most probable, nor shall I have any hesitation in engaging in the more minute discussion of the grounds on which it rests.

VII.

Description of Firs, illustrated by Dissections. By Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Arrangement of the fir tribe.

I AM now to give a description of the fir tribe of plants, seldom, I believe, studied, though well worthy of attention, as differing more in many important particulars than any natural order of plants I am acquainted with. Though seldom interfering in the arrangements of botany, I have ventured to place the thujas with the cypresses, allotting the cedars to the genera they appear to belong to. For they have been hitherto placed without the least regard to their flower or fruit; else could the white cedar be called a cedar, or the balm of gilead fir a pine? I shall divide them into three sorts, the pine, the cypress, and the cedar, placing the various species according to their fructification.

Firs

Firs differ from plants in general in having no spiral wire; for these vessels are absolutely only to be found where the leaves require turning, and not when so fastened on the main stem, as to be incapable of changing their position: an arrangement that might have been expected, since to turn the leaves as habit requires, to open and shut the flower, are the real offices of the spiral wire. They have no spiral.

The fir tribe differ also in forming their bark and rind by leaves; for, while in common plants the juices with the thread vessels of the bark form together the upper covering of the tree, in the firs they form leaves alone; and with these the tree is covered. The leaves of the pines are more simple in their formation than leaves having the spiral wire, all that rolling and pressing is not used in any of the fir tribe, though the buds are more difficult to be understood in their general arrangement. To comprehend a leaf bud when forming, you must take it out of the interior of the "leaf calyx", within which, and next the stem, it will be found. They also form their bark and rind from leaves.

The leaf bud consists of several pairs of calyxes, having a bundle of leaves weaving: as at fig. 1, Pl. V. Take one of these, and in the solar miscroscope it will show a very curiously worked wood, vessels ready formed; as a middle to the leaf, and a parcel of threads weaving the sides of the leaf by passing backward and forward: see fig. 2, where *a a* are the sides, and *b b* the middle, through which the threads pass. When this is done, the pabulum or blood of the plant coagulates, and settles on the threads, forming a mass both thick and durable; while the cobweb skin, which is woven with the calyxes, fastens on it, and covers the whole. Now the edges of the leaf begin to shoot, while threads of singular fineness and beauty appear; but scarcely have you time to admire the various prismatic colours they reflect in the sun, ere they are covered by the same cobweb skin, which makes of these apparent glass rings (for such they seem) one regular circular vessel; bordering the leaf, and fastening down the upper surface: the next appearance of the leaves is at the top of a bud, their form is then complete, though extremely small. In this bud you see the first starting of the flower bud from the line of life as at fig. 3; where *c c* are the female buds, *d d* the leaves. No sooner has the flower The leaf bud.

flower bud got its scales and clothing to fit it for the cold it may encounter, than the stem will lengthen, and leave the female bud on each side of the stalk; carrying its leaves (still covered by a scale) on the top; when growing in length, and having now acquired a proper height, for the last time the stem begins to shoot, and the leaves push off the scale as they increase, depositing their proper number at each point of the stalk, according to the species. But the calyx still remains stationary, so that the length of the stalk, with the number of leaves contained in each bud, is easily known.

Peculiar bud in the pines.

There is in the pines a peculiar sort of bud, that must catch the attention of the most careless. In the shape, and with the appearance of a bud, it is in reality the spring shoot, showing itself in May or June, just after the leaf buds have made their spring increase, and when their feathery tops display such beatiful green plumes. It is also that peculiar thing, which serves to show the height the tree gains each year, and proves, that the leaves alone form its covering. It is the increase of the stem without the wood; that is, the bark and inner bark forming their shoots, while all around the sides, closely imbedded, are found buds of leaves, serving, as the stem increases, for the future covering of the tree. As soon as this is finished, the wood, line of life, and pith shoot up in the middle, and then the stem is completed.

Female bud.

But this does not happen till the female bud is formed at the top of this new shoot. At first the line of life runs up through it, and may be seen as a few green threads, followed by some wood vessels. The female flower is then protruded; and the rest of the wood begins to grow. This is an uncommonly curious process, as plainly proving two things: 1st, That the bark, inner bark, and leaves, want little assistance from the wood: 2d, That as soon as the pistil and stamens begin to grow, the line of life is their first accompaniment, and then the wood. The bud, when the female cone appears at top, is near a foot long, and often more in the Scotch fir, in the spruce still more, and in the silver fir less. Still it is the same thing, though rather different in appearance.

There

There is a peculiarity in the Scotch fir, and Weymouth pine, not to be found in any of the firs, I mean the beautiful matter, which resembles the bloom of a plum, and which, like that, is a cryptogamian plant of an elegant kind; and though its extreme thickness grows only in spots, yet it is spread in a less degree over all the back of the leaf. It comes not till the leaf is fully formed; and disappears with age and sickness.

Peculiar bloom in the Scotch fir and Weymouth pine, caused by a cryptogamous plant.

The Scotch fir is very different from the other pines in growth. If not in perfect health, and in a soil exactly suited to it, it is but too apt to grow squalid and ugly. Indeed no trees so directly show sickness as the firs. As soon as the stem of the side bough ceases to be on an even line with the branches that proceed from it, especially at its termination, and as soon as it stands much above them, it begins to mark a disordered frame, and its future symptoms of decay are as regular as the seasons. For years, the tree will continue growing more unsightly, though it may require a century to kill it. But when in perfection it is a beautiful tree, and less formal than other firs. The pinus latifolia is a variety of this species.

Scotch fir.

Marks of disease in it.

The stem of the pine I have in part described, the leaves standing instead of bark and inner bark; the scales instead of rind. But next to the bark is a matter in all firs, which has hitherto been called by that appellation, though differing entirely from it. To inquire into the nature of this substance, its use, and why placed there, may be worth the trouble. On examining all those trees which have hitherto yielded the tanning principle, I find they have invariably this substance placed next the bark, and joining the albumen; although it is found in no other trees. On farther examination it appears to be allotted to them in a degree of thickness very nearly proportioned to the strength we have found in this same tanning principle, in each tree. Thus in the sumach it is composed of about 8 or 10 rows in thickness, in the oak of 6 or 7; in the willow of 5 or 6: and so on. Now on placing a piece of this matter in the solar microscope; I find, instead of being bark, it is wood formed exactly the same as the wood on the other side of the albumen. But so altered, so changed in its appearance and feel

Peculiar matter in all firs, and in all trees that yield tannin.

feel, that a large magnifier alone could prove it the same: for instead of that hard and harsh substance, it is soft, smooth, and pleasant to a great degree. But when I came to dissect the firs, instead of finding a few rows of this matter, there were 40 or 50, making two or three tenths of an inch in thickness; it was become so soft in every respect, that it serves for bread in some countries. Though so thick it will turn round the finger with the utmost ease, and is far more succulent, more oily, and of a more beautiful white colour, than this matter in any other trees I have mentioned.

Use of this matter.

From all those observations, I think I may notice the conclusions I have drawn from these data, without being accused of giving way to imagination. I am persuaded, that this matter, placed in this situation in the tree, is intended to guard the albumen from being steeped in this softening liquid, and therefore never gaining the strength requisite to it: that the matter thus placed shows the effect of this tanning principle by the extraordinary changes of its appearance: and that the conclusion naturally to be drawn from the whole is, how much stronger must the tanning principle be in the firs, when nature is forced to have recourse to such an expedient, in such a treble guard: and how strong must the juices be, which have produced so astonishing an alteration, for the wood can only be compared to beautiful white leather. Why this matter should tear off with the bark, and leave the wood, is easily explained, as is also the reason why at this time the bark comes off at all. It is in the spring and fall, that the new albumen shoots; and it is then so soft and watery, and its vessels, if formed at all, so weak, that the smallest effort separates them. Indeed, at first it is only a collection of the sap to form the albumen; and they of course then fall apart.

Wood in other trees.

As to the wood of the pines, it is nearly the same as in any other trees; composed from the depositions of a new row each year.

Fructification of the pines.

I shall now show the fructification of the pines. There is perhaps no seed, where nature so plainly and openly exposes her whole process, as in this tribe of plants. So evidently indeed does she develope them to the view of the attentive

tentive physiologist, that even dissection is unnecessary. I shall also, in describing the seed, prove the truth of all I have hitherto advanced on this subject, and shall continue to take my specimens of the pines from the Scotch fir. There is a curious particular concerning them, yet unknown I believe. The cones of the present year are not impregnated till the following; nor are they fit for planting, or will they come off the tree, till the succeeding season. When they are first seen on the new shoot, the stamens have already exhausted all their powder: besides, the cones have at that time no seed within them. But the following May, as soon as the stamens make their appearance, the cones, if watched, will exhibit a beautiful sight. On each squama will be seen two brilliant drops of liquid, the juice of the pistil, appearing toward noon, and subsiding in the evening. For a little time it will continue thus, till the stamen has risen out of its calyx, and each anther hangs like a basket of gold dust, ready to disperse in air. In a short time the drops on each pistil get saturated, and pass down to the seed, which they impregnate; running the line of life, filled with the mixed liquor, into each seed, and forming the corculum. As soon as the heart is perfected, the same line shoots lower, and produces the pocket, which is the outward cuticle of the embryo, and the cotyledons,. When the pocket is large enough it joins to the heart, and the cotyledons begin to grow; and this is a long process in the fir tribe of plants, where there are from 5 to 10 in each seed. I know no plants so capable of proving the mistake into which most botanists have fallen, "in supposing the cotyledons nourish the embryo"; for though these seeds, like all others, have the 8 parts perfect; yet, being of the foliferous kind, they are so very diminutive, a large magnifier is required to see them. Would then most nourishment be formed, where there was hardly any embryo to feed? Besides, as I have before observed, the cotyledons are a part of the embryo; it would therefore be nourishing one part with the other; an idea not to be supported. In the firs also the nourishing vessels are so very plain, that all must see them. See fig. 4 and 5.

The cotyledons do not nourish the embryo.

Having now explained the Scotch firs, as an example of all

The cypress kind,

all the pines, which they in fructification and habit closely resemble, I shall turn to the cypress kind; including only those, the fruit of which bear a strict analogy to the cypress; as the white cedar, the balsamea, the arbor vitæ, and others, too many to name; taking the white cedar as an example of all the rest.

instanced in the white cedar.

The young shoot.

The young shoot of the cypress kind is curious. It so much resembles the juniper, that the most knowing gardener would be deceived. This is caused by the first shooting of the leaf bud in the axil of the leaf; which necessarily throws it from the stalk, to which at every other time it cleaves most closely: for they have inbricate leaves, with the leafing branches quadrangular, which makes them take a pyramidical form, The Bermudas cedar is only a variety of the cupressus sempervirens, expands more in its branches, grows larger in size, and is that species from which the wood is taken, so remarkable for its resistance to the insect tribe. The pinus balsamea, with its brown and woody corollas, has the same fructification, though the cone is in the former more expanded. In the arbor vitæ it differs little; though this has generally been supposed to carry its male and female flowers on different trees. But this I conceive a great mistake; I have repeatedly drawn them from the same plant, as well as in the balsamea.

Bermudas cedar.

Balm of Gilead fir.

Thuja.

Stems of the young branches.

The stems of the young branches of all these of the cypress kind are more formed like leaves than stems, only that they are so thick as to have no edges. They are almost wholly composed of pabulum, having very few regular vessels. A quantity of smaller bubbles of resinous matter, surrounded by a net work enclosing now and then a larger circular bleb. Thus net on net appears to form both the minor branches and leaves: but the principal stems are composed as those of firs in general; except, that in the larger stem of all firs there is a peculiarity not yet noticed. In showing the interior formation of trees, I mentioned the grand obstruction, and the middle*. This last was the stoppage of the pith at the commencement of each branch. Now

Principal leaves.

Peculiarity of the firs.

* See Journal, vol. XXVIII, p 259, 260.

when

when a branch is divided in the firs, the wood as usual is perceived to supply the place of the pith; but in the middle of the wood is a square of pith proportioned to the size of the branches, which is seen in the firs only. In all firs there is very little pith: possibly therefore it may be intended to supply the moisture necessary to raise the wood for the passing of the buds: for in the firs almost all the buds may be seen passing from the line of life to the exterior in this very place; and perhaps no plants give more complete conviction to the mind respecting that important point, namely, whence the flower buds proceed, than the firs; for they are seen proceeding in every direction from the interior, and throwing off their female cones as the stem increases.

The leaves are formed with a *large bladder* in the middle, and a thorn at top. The leaves.

The fructification is very different from that of the pines. Fig. 7 is a single squama of the cone of the cedar thyoides; fig. 8 is a squama dissected; and fig. 1, Pl. VI, is the male ament. The fructification.

I now turn to the real cedars, at the head of which may be placed the cedar of Lebanon. With these I have joined the larch, and all those the leaves of which grow in bundles. In fructification they much resemble the pines; but their nature agrees not together; and if any should be separated beside the cypress, it should certainly be these. They are hardy, and brave every climate, from the hot Bermudas to the moist Barbadoes, and the cold New England, and grow in perfection in all. They grow also in the bogs of America, and on the mountains of Asia. The cedar we have from Jamaica is a spurious sort; and the wood so porous, that wine soaks through it; while that of Carolina (probably a true cedar) is so firm and close, that it often preserves the strongest spirits in vigour. In this country none of these firs have any scale, or covering to their leaf buds; and they are also perfectly alike in their manner of forming their leaves. It is curious, that in the pines, where the leaves are few, or in pairs, they weave in bundles; and in the cedar, larch, &c., where they come out in bundles, they weave singly. There is no apparent leaf bud; the whole work is formed within. Each separate little calyx has a bundle of threads, which it winds round the long vessels, Cedars. Leaves.

vessels, working them in and out like basket work, thus binding them to the middle wood vessel. But no sooner are the leaves formed, though ever so diminutive, than the stalk shoots, carrying up the leaves with it, and another general calyx forms round the parcel of leaves; the single calyxes remaining to bring out fresh ones, and to serve to cover the new stalk. The edges of the leaves are formed very differently from those of the pines. A parcel of threads, very clear, and apparently full of water, are found shooting by the side, and binding themselves to the leaf by a single thread. In some of the real cedars there are two, in some three, and in the larch four of these vessels.

Peculiarity in the cedar of Lebanon.

There is a peculiarity in the cedar of Lebanon so very extraordinary it must not be passed over. The upper covering of the trunk of the tree seems as if too long for it, and sits in high ridges all the way, appearing as though, if stretched out in length, it would be as long again. It would be very instructive to know whether this is the case in its native land. I have long been seeking for the ball and socket found in some plants, and peculiarly marked in some firs, where the branches have missed. In the cedar I was much struck with this appearance, and resolved to try whether I could find the ball. On cutting round it, it moved under my hand, and I found it was easily taken out. I have now procured ten of them, some formed like a pointed top, some merely circular, but the bark and rind, instead of being, like that of the rest of the tree, formed of thickened leaves, are divided into narrow slips of bark and rind, rolled, and covering it like basket work.

Another peculiarity.

There is also another peculiarity never seen in our forest trees, and which appears to belong only to the exotic trees: a projection round the part where the branches first shoot. If they have it not in their own climate, it may be an increase to strengthen them, weakened by growing in a foreign country.

Fructification.

I shall not occupy your pages with describing at length the fructification of the cedar, as its process very nearly resembles the account already given; but mention only, that its cone is extremely large and solid, and appears to contain a greater quantity of the tanning principle than any other

Abundance of tannin.

other part. The seeds are not only full of it, but are covered without by bladders filled with the same juices.

Wood

As I have now concluded my account of the firs, I shall finish with a few words respecting wood in general, as one of the most important subjects in the botanical world. Some of our best physiologists have made a strange mistake, if I may venture to say so, in supposing it impossible that the wood can convey sap, because the wood can be torn to atoms. Look in the microscope at one of these shreds, and it will be found pointed, not a sap vessel, but a fragment. The sap vessels are round, but the wood has besides the bastard pipes, pieces of thin flimsy texture, which fill up all the places between the sap vessels, and are very large in young wood, and will divide into hairs; which are often taken for important vessels. The sap vessels also will separate, but I cannot conceive their being thus flexible, and easily torn, lessens their power of conveying sap when perfect and whole. But is it not an easy thing to prove, that they are the real sap vessels? since they are the only pipes yet found in plants, that will convey coloured infusions, as all acknowledge. I have repeatedly taken a branch three or four feet long, and though I could not make the coloured mixture rise the whole space at once; yet by cutting a little below where it stopped, I have made it by degrees rise the whole length, and thus proved, that there is no real stoppage in the vessels; but that the sap is capable of flowing in one even current from the bottom to the top of the tree; and the only reason we cannot make coloured liquids rise with the same ease and quickness as the sap is, that our mixtures are not so well tempered as Nature's: there is always some dust, some matter to choak these little pipes. I once made some very curious experiments on capillary attraction, in very diminutive glass pipes, which rendered this most evident, not only between the liquids, but between the pipes which we make when compared to the perfect works of Nature.

capable of conveying sap.

Flower buds from the interior of the tree.

As I cannot believe, that any one can strip off the bark of a tree, and yet be doubtful whether the flower buds come from the interior of the wood, I am very anxious to persuade the physiologist to study at this season the tree newly barked. There he will not only see the buds just breaking through, but the variation in each different tree in this respect—the manner in which each point of the compass is marked by its growth, by the scarcely undulating line of the sap vessels in the north, and by their never ending half circles in the south.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

Explanation of the Plate.

Pl. V, fig. 1. A bundle of leaves taken out of the inner leaf bud of the Scotch fir, while weaving; with their calyxes.

Fig. 2. A single leaf much magnified, and showing the manner of forming all the leaves of the pines.

Fig. 3. A sort of general or mixed bud in the Scotch fir, when the leaves, *d d*, are completely formed, and they are discovered at the top of a bud; while the female cones, *c c*, are shooting from the line of life, though not one in ten lives to come out of the cradle in the bark, *p p*.

Fig. 4. The squama taken out of the Scotch fir. *a a* the pistil: *o o* the two drops: *b b* the line of life running to the seed, and entering it, to form the heart at *f*: *c* the nourishing vessels entering the seed at *d d*: and fastened to the cone at *e e*.

Fig. 5. The male collection of stamens or catkin.

Fig. 6. A single stamen with its scale.

Fig. 7. A squama of the cypress kind, taken from the white cedar.

Fig. 8. The same dissected: *h* the pistil: *i i* the drops appearing to catch the powder: *k k*, the line of life passing into the seed at *r r*. *l* the nourishing vessels passing into the seeds at *n n*, and then joining the cone at *m m*.

Pl. VI, fig. 1. The male catkin of the cypress kind.

IX.

IX.

On the Motion of the Flower of the Barberry. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

AS the berberis has been the subject of a letter from one of your correspondents, I have waited till the flower was in full beauty, to send you a sketch of the manner in which the whole motion is managed by the spiral wire. Dr. Smith has most properly observed, that it is a contraction of the stem (for it can hardly be called a filament) of the stamen: it is so; for the contraction is in the spiral wire within this stamen stalk, which is gathered up, as may be plainly seen, when put into the solar microscope. Pl. VI, fig. 3, *b b* is the corolla, *a a* the stamen fastened to it—it has also a fastening to the pistil, which, crossing from each side to the pistil and round it to the other stamen, makes a general communication, but not a very sensible one. The strong spiral wire, *o o*, that manages the flower, runs through the middle of the stamen *c c*, with two joining from the sides *d d*, and running into the nectaries, *f f*, in which they are fastened at *g g**. So uncommonly strong is this spiral wire, that it is larger than that which manages many flowers of three times the size. There are many cross spirals, for it is rather a complicated management; but this will at least account for the contraction that takes place from *a* to *a*, and is plainly to be seen. The same thing happens in the stalk of many plants when in bud, which alter their position when full blown. On placing a bunch of these flowers under water, it is very difficult to make the water get to them; but if they are once thoroughly wet, they move no more. However I again repeat what I have said in my last letter, that I cannot say I am myself convinced what is the

Motion of the flower of the barberry.

The flowers will not move under water when thoroughly wet.

* Pl. VI, fig. 2, *a a*, shows the stem of the stamen of the barberry in its perfect state; and fig. 3, *a a*, the stem cut open to expose the cases of the spiral wire.

power,

power, which rules the spiral wire: but that this wire is the cause of the motion, whatever may be the superior cause that regulates it, I am hourly more and more convinced. The berberis is curious on another account; its corolla is very peculiarly made, something like the watery corolla, but not quite; no one can look at it, and not see that it is water, which causes all the beauty of its light and sparkling appearance.

Structure of its corolla

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

X.

An improved Method of cultivating the Alpine Strawberry. By THOMAS ANDREW KNIGHT, *Esq. F. R. S., &c.**

Culture of the Alpine strawberry.

THE Strawberry is a fruit, which is agreeable to the palates of so many persons, and which disagrees with the constitutions of so few, that any means of improving the culture of it, and of prolonging the season of its maturity and perfection, will probaby be acceptable to the Horticultural Society: I am therefore induced to send an account of an improved method of cultivating the Alpine strawberry, that is, I believe, little, if at all, known, and that I have practised with the best possible success.

Valued as an autumnal crop.

Experiments.

Though the flavour of the Alpine varieties is generally approved, they are not much thought of, while the larger varieties continue in perfection, and are valued only as an autumnal crop. I was therefore led to try several different methods of culture, with a view to obtain plants that would just begin to blossom at the period when the other varieties cease; conceiving, that such plants, not having expended either themselves or the virtue of the soil in a previous crop of fruit, would afford the best and most abundant autumnal produce. Under this impression I sowed the seeds of

* Trans. of the Horticultural Soc. vol. I, p. 159.

the

the best alpine variety, that I had ever been able to obtain, in pots of mould, in the beginning of August, the seeds of the preceding year having been preserved to that period; and the plants these afforded were placed, in the end of March, in beds to produce fruit. Seeds sowed in August.

This experiment succeeded tolerably well; but I was not quite satisfied with it; for though my plants produced an abundant autumnal crop of fruit, they began to blossom somewhat earlier than I wished, and before they were perfectly well rooted in the soil. I therefore tried the experiment of sowing some seeds of the same variety early in the spring in pots, which I placed in a hotbed of moderate strength in the beginning of April, and the plants thus raised were removed to the beds in which they were to remain in the open ground, as soon as they had acquired a sufficient size. They began to blossom soon after Midsummer, and to ripen their fruit towards the end of July, affording a most abundant autumnal crop of very fine fruit; and even so late as the second week in December I have rarely seen a more abundant profusion of blossoms and immature fruit than the beds presented. The powers of life in plants thus raised, being young and energetic, operate much more powerfully than in the humours of older plants, or even in plants raised from seeds in the preceding year; and therefore I think the Alpine strawberry ought always to be treated as an annual plant.

The plants blossom rather too early.

Seeds sowed in spring

yielded fruit very late.

Should always be treated as an annual.

XI.

On the Nature of Heat. By MARSHALL HALL, *Esq. In a Letter from the Author.*

To W. NICHOLSON, Esq.

SIR,

THE nature of caloric has long been a subjeet of inquiry in chemical philosophy. The first conjecture on this matter, which deserves attention, is that of Lord Bacon; his opinion has, however, been in a great measure superseded by

Nature of caloric long questioned.

Hypothesis of Bacon.

by the hypothesis of Homberg and Boerhaave. It may indeed be observed, that while the opinion of the materiality of caloric has had many adherents, and received much consideration, the hypothesis of Bacon has probably been too much neglected; nay, it has even been held up to ridicule and contempt, as a " delusive dream," or as a " labyrinth of perplexities." Probably the reason of this censure has been just given; had the opinion obtained the consideration which it merits, it would possibly have long since ceased to be this labyrinth of perplexity.

I trust therefore, that a few observations on this subject will not be altogether unwelcome: no apology can be required for their imperfections. I shall commence with a few remarks on the prevailing opinion, and shall then give a concise view of what may be termed the hypothesis of vibration.

1. *Sources of caloric.*

Sources of heat.

Scarcely any circumstance can afford more forcible objection to the hypothesis of material caloric, than some of the means of producing an increase of temperature. Every one is acquainted with the important researches of Boyle, Romford, and Davy, on this subject. Their experiments prove, that heat may be produced by friction in circumstances, where no source of it, considered as a material agent, can be discovered or suspected.

But these facts have been so often urged as imcompatible with the supposition of material caloric, that it is needless to enter farther into the discussion of this point. I wish rather to avail myself of this opportunity, to consider other parts of the doctrine.

Under this head it may indeed be added, that the excitation of heat, in the operations of electricity and galvanism, has not been explained. There is also much difficulty in accounting for the production of heat in some instances of combustion *, and of other chemical actions.

* Thomson, vol. I, p. 575 et seq. 3d ed.

2. *Motion*

2. *Motion of caloric.*

It is observed, that the best conductors of heat receive and deliver it most easily and rapidly; those bodies, which absorb heat with most avidity, are also such as radiate most copiously. The first part of these operations might be ascribed to the attraction exerted between the particles of the body and of caloric; but the second phenomena are directly adverse to such an explanation. Considering caloric as matter, and subject to attraction like other matter, the circumstances above related appear to require explanation. Motion of heat.

In the *radiation* of heat many phenomena occur, which have not been satisfactorily explained: and first, the remarkable difference between solar and culinary heat does not appear to be by any means understood *. The first is transmissible through and refrangible by glass, and other transparent media; the second is in a large proportion intercepted by every solid body: the first is reflected in circumstances, in which the second is absorbed †: culinary heat, unlike the solar ray, suffers a considerable aberration in its reflection: and lastly, the absorption of culinary heat is not affected by the colour of the absorbing surface, in the same manner as that of solar heat ‡. What can be the cause—what the rationale of these differences? Radiation of heat. Difference between that of the sun & fire.

It is however in the radiation of *cold,* I conceive, that we have the most forcible and direct objection to the hypothesis of material caloric, and the most certain indication of the real nature of this principle. It is scarcely necessary to say, that no unexceptionable explanation of this phenomenon has been proposed. According to Prevost's supposition, the effect of radiation from a cold surface ought in reality to be that of heating, and not of cooling the opposed thermometer; this will be rendered evident by the assistance of a diagram. Radiation of cold.

Pl. VI, fig. 4, *a b, c d,* are two concave mirrors. T *a c* I, T *b d* I are rays of heat issuing from the thermometer, I *c a* T, I *d b* T, are rays *also of heat,* issuing from the ice;

* Nicholson's Journal, vol. viii, p. 297.
† Leslie's Inq. p. 83, et seq. ‡ Ibid, p. 97.

for,

for, according to the hypothesis, both the thermometer and the ice radiate heat. The thermometer T however radiates to the ice I more than the latter does to the thermometer; the ice therefore receives caloric, and will be dissolved: but the ice also radiates caloric; this will be reflected and conveyed to the thermometer, which will consequently maintain a higher temperature, than if there were no ice, from which it could receive caloric.

Count Rumford ascribes heat to undulations.

"Count Rumford, not admitting the existence of caloric as a distinct matter, endeavours to explain the phenomena of radiant heat from the hypothesis of undulations excited by bodies at a high temperature in an etherial medium."—The Stahlian theory accounted for the phenomena of oxidation, while philosophers neglected the agency of the atmospheric air in the operation; and in a similar manner the hypothesis of Count Rumford might explain the radiation of heat and cold, could we forget the manifest influence of the "*ambient air.*" Other difficulties in this hypothesis would occur, in applying it to explain the differences between solar and culinary heat; and in accounting for the partial interception and partial transmission of culinary heat by transparent media.

Difficulties in this hypothesis.

This partial transmission of culinary heat, and its distribution in the prismatic spectrum, do not appear to admit of explanation on the ingenious hypothesis of Mr. Leslie.

With regard to other attempts, which have been made to explain the radiation of cold, and to reconcile it to the general theory, complete satisfaction may be obtained from consulting Mr. Murray's work.

3. *Effects of caloric.*

Effects of heat.

The opinion respecting the mixture of material heat arises chiefly from the consideration of the effects, which the communication of temperature occasions on the bulk and form of bodies submitted to its action. The explanation, which the hypothesis affords, of the immediate effects of heat, is indeed often satisfactory; yet, although it applies in many cases, it fails altogether in others; and cannot, I conceive, bear the test of a strict examination.

Expands bodies generally,

1. If the hypothesis were true, expansion ought invariably

bly to attend an increase of temperature, and contraction ought constantly to accompany its diminution.

It is scarcely necessary to mention the contradictions to this law, observed in our operations on water, iron, and some saline solutions, while they retain their fluidity; on water, iron, bismuth, antimony, sulphur, the saline bodies, &c. during their transition from a solid to a fluid form; and on argil at high temperatures. but not always.

In the liquefaction of ice, iron, sulphur, &c., the contraction in bulk is very considerable; yet during the operation, from the temperature acquired, and especially from the increase of capacity, a very great quantity of caloric is supposed to be absorbed.

2dly, The degree of expansion ought, ceteris paribus, to be in direct proportion to the quantity of caloric absorbed. Expansion not in proportion to the heat supposed to be absorbed.

Now as in changes of temperature those bodies, which have the greatest capacity for caloric, absorb the greatest quantity, it follows, that their expansibility ought to be proportionate to their capacity. This is however by no means the case, as will be observed by the inspection of the following table, in which the expansibility and the capacity of several of the metals are compared.

	Capacity.	Expansibility.
Iron	·98982	100126
Copper	·98823	100170
Zinc	·64699	100296
Antimony	·43292	100109
Lead	·39959	100287

Of these metals, iron and lead occupy the extremes in capacity; iron having the largest and lead the least capacity for caloric; yet lead is the most, iron the least expansible by heat: that metal, therefore, which absorbs the most caloric, expands the least; and, on the other hand, that which absorbs the least of this repulsive fluid, expands the most! The same discrepancy is observed in other parts of the table; antimony and lead have a capacity nearly equal, yet they occupy the extremes, in the scale of expansibility.

Aware, however, that the expansibility of any body might be regulated altogether by the degree of cohesion between its This apparently not owing to cohesion.

its particles, I examined the same circumstance in those bodies, the form of which precludes the operation of this case, at least to any very considerable extent. The following table will show the result.

	Capacity.	
Oxigen gas	5·238147	Expansibility equal.
Hidrogen gas	1·80402*	
Atmospheric air	1·79*	
Carbonic acid gas	1·5681	
Nitrogen gas	·78169	

All gasses then are found to expand in an equal degree by the same change in temperature; yet how widely different are the respective quantities of caloric absorbed! From the above table it appears, that, during a given increase of temperature, carbonic acid gas absorbs a quantity of caloric more than twice as great as the same bulk of nitrogen gas; atmospheric air, and hidrogen gas absorb a quantity still more considerable; and oxigen gas actually absorbs more than $6\frac{1}{2}$ times this quantity; and still the expansibility is precisely the same in all.

It is certainly needless to add any remarks on facts such as these; they are indeed truly important. One portion of caloric, the principle of repulsion in these operations, occasions an expansion in one case equal to that which $6\frac{1}{2}$ times this quantity does in another: this effect too takes place, when no cause occurs, to regulate or influence it.

4. *Capacity for Caloric.*

Capacity for heat.

The phenomena of the capacity of bodies for caloric appear to me, to be adverse to the opinion of its materiality. Caloric is the supposed cause of temperature and of expansion; yet we communicate caloric to ice, at 32° Fahr., without an increase of its temperature, and with an actual diminution of its bulk. Here then our material agent has forgotten its functions, and we are obliged to resort to a

* This statement from calculation agrees nearly with the results of Mr. Leslie's experiments on these two gasses. The same may be said of the oxigen and nitrogen gasses; $\frac{\cdot 78169 \times 3 + 5\cdot 238147}{4} = 1\cdot 8958$ which, all circumstances considered, is wonderfully exact.

new

new hypothesis, to reconcile the contradiction: for I can regard the doctrine of capacity in no other light, than in that of a second hypothesis, adopted to obviate the imbecility of a former one.

The objections afforded by this part of the subject, to the general theory, are too palpable to need to be insisted on. If it be argued, that our notion of capacity supposes a power of counteracting the usual properties of caloric; as the properties of acid and alkali mutually neutralize each other; there are facts not less in contradiction to this supposition. Thus the caloric communicated to boiling water is expanded in satisfying its increased capacity; nevertheless the expansion occasioned is prodigiously great.

It has indeed been asserted, that there are *direct proofs* of the existence of material caloric; it is therefore proper, that we should consider these.

Communication of heat through a vacuum not proved.

1st, "The communication of caloric through a vacuum, has been regarded as such a proof." In opposition to this argument it is sufficient to state, that no absolute vacuum, as far as we know, has ever been effected. Cavallo could never render the sound of a bell even perfectly inaudible, although he employed an air pump of the best construction. And in the Torricellian vacuum it is well known, that an atmosphere of mercurial vapour is formed. Pictet has even observed the condensation of this vapour. By this vapour therefore may the heat be communicated, although not material; its tenuity affords no objection; the conducting power of bodies does not observe the ratio of their density.

Radiation of heat facilitated by the air.

It is observed, that the conducting power of the Torricellian vacuum is to that of the atmospheric air as 100 to 605. Now this presents a fact, which it is not easy to reconcile to the material theory. According to this theory, the radiating power of any body must depend on its own nature and power; it cannot be assisted, it may be opposed, by surrounding bodies; but the fact just stated, and the experiments of Mr. Leslie, prove, that radiation is in reality facilitated by the surrounding air.

Radiation of heat

2dly, "The radiation of caloric appears to be another unequivocal

quivocal proof of its materiality. A matter is thrown from heated bodies, which moves in right lines, with velocity, raises the temperature of any body, on which it falls; and which, in every state, preserves the properties of caloric."

no proof of its materiality.

But are these proofs of materiality? By no means. If every thing were material, of which these properties could be predicated, then should we have proofs of something substantial in *sound* and *cold*. Thus sound is thrown from surrounding bodies, in right lines, with velocity, is capable of reflection and of condensation, occasions sound in some bodies on which it falls, and, in every state, preserves the properties of sound. Cold also moves in right lines, with velocity, suffers reflection and condensation, lowers the temperature of bodies, and is always and absolutely cold.

Heat of the Sun distinct from light.

"Lastly," it is said, that "the existence of caloric, in the rays of the sun, apart from visible light, adds to the proof, that a peculiar matter exists, possessed of the properties of caloric, and distinct from every other."

This will be considered hereafter.

It is sufficient to have mentioned this last alleged proof of the existence of material caloric; its validity rests entirely on the supposition, that no other explanation can be given of the phenomenon; and it will consequently fall to be considered, in the second division of our subject.

(To be concluded in our next.)

XII.

On some of the Combinations of Oximuriatic Gas and Oxigen, and on the Chemical Relations of these Principles to inflammable Bodies. By HUMPHRY DAVY, *Esq. LL. D. Sec. R. S. Prof. Chem. R. I. F. R. S. E.*

(Concluded from p. 127.)

3. *On the Combinations of the Metals of the Earths with Oxigen and Oximuriatic Gas.*

Muriates of the earths not

THE muriates of baryta, lime, and strontia, after being a long time in a white heat, are not decomposable by any simple

simple attractions: thus, they are not altered by boracic acid, though, when water is added to them, they readily afford muriatic acid and their peculiar earths.

decomposable without water.

From this circumstance, I was induced to believe, that these three compounds consist merely of the peculiar metallic bases, which I have named barium, strontium, and calcium, and oximuriatic gas; and such experiments as I have been able to make, confirm the conclusion.

Compounds of metals with oximuriatic gas.

When baryta, strontia, or lime, is heated in oximuriatic gas to redness, a body precisely the same as a dry muriate is formed, and oxigen is expelled from the earth. I have never been able to effect so complete a decomposition of these earths by oximuriatic gas, as to ascertain the quantity of oxigen produced from a given quantity of earth. But in three experiments made with great care I found, that one of oxigen was evolved for every two in volume of oximuriatic gas absorbed.

The earths evolve one part of oxigen for two of oximuriatic gas.

I have not yet tried the experiment of acting upon oximuriatic gas by the bases of the alkaline earths; but I have not the least doubt, that these bodies would combine directly with that substance, and form dry muriates.

Direct union not yet tried.

In the last experiment that I made on the metallization of the earths by amalgamation, I paid particular attention to the state of the products formed by exposing the residuum of amalgams to the air. I found, that baryta formed in this way was not fusible at an intense white heat, and that strontia and lime so formed gave off no water when ignited. Baryta made from crystals of the earth, as Mr. Berthollet has shown, is a fusible hydrate; and I found, that this earth gave moisture when decomposed by oximuriatic gas; and the lime, in hydrate of lime, was much more rapidly decomposed by oximuriatic gas than quicklime, its oxigen being rapidly expelled with the water.

Earths produced from their metallic bases,

not hydrates as the common earths.

Some dry quicklime was heated in a retort, filled with muriatic acid gas: water was instantly formed in great abundance, and it can hardly be doubted, that this arose from the hidrogen of the acid combining with the oxigen of the lime.

Dry quicklime heated in muriatic gas.

As potassium so readily decomposes common salt, I thought it might possibly decompose muriate of lime, and

Action of potassium on the

thus

muriates of the earths.

thus afford easy means of procuring calcium. The rapidity with which muriate of lime absorbs water, and the difficulty of freeing it even by a white heat from the last portions, rendered the circumstances of the experiments unfavourable. I found, however, that by heating potassium strongly, in contact with the salt, in a retort of difficultly fusible glass, I obtained a dark coloured matter, diffused through a vitreous mass, which effervesced strongly with water. The potassium had all disappeared, and the retort had received a heat at which potassium entirely volatilizes. I had similar results with muriate of strontia, and (though less distinct, more potassium distilling off unaltered) with muriate of baryta. Either the bases of the earths were wholly or partially deprived of oximuriatic gas in these processess, or the potassium had entered into triple combination with the muriates. I hope on a future occasion to be able to decide this point.

Combination of magnesia, alumine, and silex, with muriatic gas.

Combinations of muriatic acid gas with magnesia, alumine, and silex, are all decomposed by heat, the acid being driven off, and the earth remaining free. I conjectured from this circumstance, that oximuriatic gas would not expel oxigen from these earths, and the suspicion was confirmed by experiments. I heated magnesia*, alumine, and silex to redness in oximuriatic gas, but no change took place.

Barytes absorbs oxigen.

Messrs. Gay-Lussac and Thenard have shown, that baryta is capable of absorbing oxigen; and it seems likely, (as, according to Mr. Chenevix's experiments, most of the earths are capable of becoming hyperoximuriates) that peroxides of their bases must exist.

Lime apparently not.

I endeavoured to combine lime with more oxigen, by heating it in hyperoximuriate of potash, but without success, at least after this process it gave off no oxigen in combining with water.

Oximuriate.

The salt, called oximuriate of lime, made for the use of the bleachers, I found gave off oxigen by heat, and formed muriate of lime.

Oxigen produced from oximuriatic gas and magnesia.

* From some experiments of Messrs Gay-Lussac and Thenard, Bullet. de la Societ. Phil. Mai, 1810, it appears, that oxigen is procured by passing oximuriatic gas over magnesia at a high temperature, and that a muriate indecomposable by heat is produced. They attribute the presence of this oxigen to the decomposition of the acid; but, according to all analogies, it must arise from the decomposition of the earth.

From

Component parts of the earthy muriates.

From the proportions which I have given in the last Bakerian lecture, but which were calculated from the analyses of sulphates, it follows, that, if the muriate of baryta, strontia, and lime, be regarded as containing one proportion of oximuriatic gas, and one of metal, then they would consist of 71* barium, 46 strontium, and 21 calcium, to 32·9 of oximuriatic gas.

To determine how far these numbers are accurate, 50 grains of each of these muriates, that had been heated to whiteness, were decomposed by nitrate of silver, the precipitate was collected, washed, heated, and weighed.

The muriate of baryta, treated in this way, afforded 68 grains of horn-silver.

The muriate of strontia 85 grains.

The muriate of lime 125 grains.

Horn silver.

From experiments to be detailed in the next section, it appears, that horn-silver consists of 12 of silver to 3·9 of oximuriatic gas, and consequently, that barium should be represented by 65·1, strontium by 46·1, and calcium by 20·8.

4. *On the Combinations of the Common Metals with Oxigen and Oximuriatic Gas.*

Combinations of oximuriatic gas with metals.

In the limits which it is usual to adopt in this lecture, it will not be possible for me to give more than an outline of the numerous experiments, that I have made on the combinations of oximuriatic gas with metals; I must confine myself to a general statement of the mode of operating, and the results. I used in all cases small retorts of green glass, containing from 3 to 6 cubical inches, furnished with stopcocks. The metallic substances were introduced, the retort exhausted and filled with the gas to be acted upon, heat was applied by means of a spirit lamp, and after cooling, the results were examined, and the residual gas analysed.

Metals heated in it.

All the metals that I tried, except silver, lead, nickel, cobalt, and gold, when heated, burnt in the oximuriatic gas, and the volatile metals with flame. Arsenic, anti-

* If Mr. James Thompson's analysis of sulphate of barytes be made the basis of calculation, sulphuric acid being estimated as 36, then the number representing barium will be about 65·5.

mony, tellurium, and zinc with a white flame, mercury with a red flame. Tin became ignited to whiteness, and iron and copper to redness; tungsten and manganese to dull redness; platina was scarcely acted upon at the heat of fusion of the glass.

Product from arsenic,

The product from arsenic was butter of arsenic; a dense, limpid, highly volatile fluid, a nonconductor of electricity, and of high specific gravity, and which, when decomposed by water, gave oxide of arsenic and muriatic acid. That from antimony was butter of antimony, an easily fusible and volatile solid, of the colour of horn-silver, of great density, crystallizing on cooling in hexaedral plates, and giving, by its decomposition by water, white oxide.

antimony,

tellurium,

The product from tellurium, in its sensible qualities, resembled that from antimony, and gave when acted on by water white oxide.

mercury, zinc,

The product from mercury was corrosive sublimate. That from zinc was similar in colour to that from antimony, but was much less volatile.

iron,

The combination of oximuriatic gas and iron was of a bright brown; but having a lustre approaching to the metallic, and was iridescent like the Elba iron ore. It volatilized at a moderate heat, filling the vessel with beautiful minute crystals of extraordinary splendour, and collecting in brilliant plates, the form of which I could not determine. When acted on by water, it gave red muriate of iron.

copper,

Copper formed a bright red brown substance, fusible at a heat below redness, and becoming crystalline and semitransparent on cooling, and which gave a green fluid, and a green precipitate by the action of water*.

manganese,

The substance from manganese was not volatile at a dull red heat; it was of a deep brown colour, and by the action of water became of a brighter brown; a muriate of manga-

* It is worth inquiry, whether the precipitate from oximuriate of copper by water is not a hydrated submuriate, analogous in its composition to the crystallized muriate of Peru. This last I find affords muriatic acid and water by heat.

Resin of copper.

The resin of copper discovered by Boyle, formed by heating copper with corrosive sublimate, probably contains only 1 proportion of oximuriatic gas, while that above referred to must contain 2.

nese,

nese, which did not redden litmus, remained in solution; and an insoluble matter remained of a chocolate colour*.

Tungsten afforded a deep orange sublimate, which, when decomposed by water, afforded muriatic acid, and the yellow oxide of tungsten. tungsten,

Tin afforded Libavius's liquor, which gave a muriate by the action of water containing the oxide of tin, at the maximum of oxidation. tin,

Silver and lead produced horn-silver and horn-lead, and bismuth, butter of bismuth. The absorption of oximuriatic gas was in the following proportions for two grains of each of the metals; for arsenic 3·6 cubical inches, for antimony 3·1, for tellurium 2·4, for mercury 1·05†, for zinc 3·2, for iron 5·8, for tin 4, for bismuth 1·5, for copper 3·4, for lead ·9; for silver, the absorption of volume was 0·9, and the increase of weight of the silver was equivalent to 0·6 of a grain‡. silver lead, and bismuth. Properties of gas absorbed.

In acting upon metallic oxides by oximuriatic gas, I found that those of lead, silver, tin, copper, antimony, bismuth, and tellurium, were decomposed in a heat below redness, but the oxides of the volatile metals more readily Action of oximuriatic gas on oxides.

* When muriate of manganese is made by solution of its oxide in muriatic acid, a neutral combination is obtained, but this is decomposed by heat; muriatic gas flies off, and brown oxide of manganese remains. In this respect manganese appears as a link between the ancient metals and the newly discovered ones. Its muriate is decomposed like that of magnesia; and its oxide is the only one amongst those long known, as far as my experiments have gone, which neutralizes the acid energy of muriatic acid gas, so as to prevent it in solution from affecting vegetable blues. Effect of oxide of manganese on muriatic acid.

† The gas in these experiments was not freed from aqueous vapour, and as stopcoks of brass were used, a little gas might have been absorbed by the surface of this metal, so that the processes offer only approximations to the composition of the oximuriates. The processes on lead, tellurium, iron, antimony, copper, tin, mercury, and arsenic, were carried on in three successive days, during which the height of the mercury in the barometer varied from 30·26 inches to 30·15, and the height of that in the thermometer from 63·5 to 61 Fahrenheit.

The experiment on silver was made at the temperature of 52 Fahrenheit, and under a pressure equal to that of 29·9 inches.

‡ This agrees nearly with another experiment made by my brother, Mr. John Davy, in which 12 grains of silver increased to 15·9 during their conversion into horn-silver.

 than

than those of the fixed ones. The oxides of cobalt and nickel were scarcely acted upon at a dull red heat. The red oxide of iron was not affected at a strong red heat, while the black oxide was rapidly decomposed at a much lower temperature; arsenical acid underwent no change at the greatest heat that could be given it in the glass retort, while the white oxide readily decomposed.

Oxigen given off as much as the metal absorbed.

In cases where oxigen was given off, it was found exactly the same in quantity as that which had been absorbed by the metal. Thus 2 grains of red oxide of mercury absorbed 0·9 of a cubical inch of oximuriatic gas, and afforded 0·45 of oxigen*. Two grains of dark olive oxide, from calomel decomposed by potash, absorbed about 0·94 of oximuriatic gas, and afforded 0·24 of oxigen, and corrosive sublimate was produced in both cases.

Analysis of corrosive sublimate and of calomel.

* I have made two analyses of corrosive sublimate and calomel, with considerable care. I decomposed 100 grains of corrosive sublimate by 90 grains of hydrat of potash. This afforded 79·5 grains of orange coloured oxide of mercury, 40 grains of which afforded 9·15 cubical inches of oxigen gas; the muriate of silver formed from the 100 grains was 102·5.

100 grains of calomel, decomposed by 90 grains of potash, afforded 82 grains of olive coloured oxide of mercury, of which 40 grains gave by decomposition by heat 4·8 cubical inches of oxigen. The quantity of horn-silver formed from the 100 grains was 58·75 grains.

In the second analysis, the quantity of oxide obtained from corrosive sublimate was 78·7; the quantity of muriate of silver formed was 103·4; the oxide produced from calomel weighed 83 grains; the horn-silver formed was 57½ grains. I am inclined to put most confidence in the last analyses; but the tenor of both is to show, that the quantity of oximuriatic gas in corrosive sublimate is exactly double that in calomel, and that the orange oxide contains twice as much oxigen as the black, the mercury being considered as the same in all. The olive colour of the oxide formed from calomel is owing to a slight admixture of orange oxide, formed by the oxigen of the water used in precipitation; the tint I find is almost black, when a boiling solution of potash is used; and trituration with a little orange oxide brings the tint to olive. It has been stated, that the olive oxide thrown down from calomel by potash is a submuriate; but I have never been able to find a vestige of muriatic acid in it when well washed. It is not easy to obtain perfect precision in analyses of the oxides of mercury; water adheres to the oxides, which cannot be entirely driven off without the expulsion of some oxigen. In all my experiments, though the oxides had been heated to a temperature above 212, a little dew collected in the neck of the retort, so that the 40 grains must have been overrated.

In

Oxides of zinc, iron, and arsenic.

In the decomposition of the white oxide of zinc, oxigen was expelled exactly equal to half the volume of the oximuriatic acid absorbed. In the case of the decomposition of the black oxide of iron, and the white oxide of arsenic, the changes that occurred were of a very beautiful kind; no oxigen was given off in either case, but butter of arsenic, and arsenical acid formed in one instance, and the ferruginous sublimate, and red oxide of iron in the other.

Two grains of white oxide of arsenic absorbed 0·8 of oximuriatic gas*.

I doubt not that the same phenomena will be found to occur in other instances, in which the metal has comparatively a slight attraction only for oximuriatic gas, and when it is susceptible of different degrees of oxidation, and in which the peroxide is used.

Oxide of tin.

The only instance in which I tried to decompose a common metallic oxide, by muriatic acid, was in that of the fawn coloured oxide of tin; a compound of water and Libavius's liquor separated.

One part of metal combines with one, two, or three of oximuriatic gas.

From the proportions which may be gained in considering the volumes of oximuriatic gas absorbed by the different metals, in their relations to the quantity of oxigen which would be required to convert them into oxides, it would appear, that in the experiments to which I have referred, either one, two, or three proportions of oximuriatic gas combine with one of metal, and consequently, from the composition of the muriates, it will be easy to obtain the numbers representing the proportions in which these metals may be conceived to enter into other compounds†.

* A singular instance of the tendency of the oxide of arsenic to become arsenical acid occurs in its action on fused hydrat of potash, the water in the hydrat is rapidly decomposed, and arseniuretted hidrogen evolved, and arseniate of potash formed.

† From the experiments detailed in the note in the opposite page, it would appear that the number representing the proportion in which mercury combines must be about 200. That of silver, as would appear from the results, page 227, about 100. The numbers of other metals may be learnt from the data in the same page, but, from what has been stated, these data cannot be considered as very correct.

5. *General Conclusions and Observations, illustrated by Experiments.*

Former inferences confirmed.

All the conclusions, which I ventured to draw in my last communication to the Society, will, I trust, be found to be confirmed by the whole series of these new inquiries.

Oximuriatic gas combines with inflammable bodies.

Oximuriatic gas combines with inflammable bodies, to form simple binary compounds; and in these cases, when it acts upon oxides, it either produces the expulsion of their oxigen, or causes it to enter into new combinations.

The oxigen not from its decomposition.

If it be said, that the oxigen arises from the decomposition of the oximuriatic gas, and not from the oxides; it may be asked, why it is always the quantity contained in the oxide; and why in some cases, as those of the peroxides of potassium and sodium, it bears no relation to the quantity of gas.

No acid matter in it.

If there existed any acid matter in oximuriatic gas, combined with oxigen, it ought to be exhibited in the fluid compound of one proportion of phosphorus, and two of oximuriatic gas; for this, on such an assumption, should consist of muriatic acid (on the old hypothesis, free from water) and phosphorous acid; but this substance has no effect on litmus paper, and does not act, under common circumstances, on fixed alkaline bases, such as dry lime or magnesia. Oximuriatic gas, like oxigen, must be combined in large quantity with peculiar inflammable matter, to form acid matter. In its union with hidrogen, it instantly reddens the driest litmus paper, though a gaseous body. Contrary to acids, it expels oxigen from protoxides, and combines with peroxides.

Decomposition of potash by it.

When potassium is burnt in oximuriatic gas, a dry compound is obtained. If potassium combined with oxigen is employed, the whole of the oxigen is expelled, and the same compound formed. It is contrary to sound logic to say, that this exact quantity of oxigen is given off from a body not known to be compound, when we are certain of its existence in another; and all the cases are parallel.

Production of oximuriatic gas from muriatic & oxide of manganese.

An argument in favour of the existence of oxigen in oximuriatic gas may be derived by some persons from the circumstances of its formation, by the action of muriatic acid on on peroxides, or on hyperoximuriate of potash; but a minute investigation of the subject will, I doubt not, show, that the phenomena of this action are entirely consistent with the views I have brought forward. By heating muriatic acid gas in contact with dry peroxide of manganese, water I found was rapidly formed, and oximuriatic gas produced, and the peroxide rendered brown. Now as muriatic acid gas is known to consist of oximuriatic gas and hidrogen, there is no simple explanation of the result, except by saying, that the hidrogen of the muriatic acid combined with oxigen from the peroxide to produce water.

Scheele explained the bleaching powers of the oximuriatic gas by supposing, that it destroyed colours by combining with phlogiston. Berthollet considered it as acting by supplying oxigen. I have made an experiment, which seems to prove, that the pure gas is incapable of altering vegetable colours; and that its operation in bleaching depends entirely upon its property of decomposing water, and liberating its oxigen.

Its bleaching effect

owing to the decomposition of water.

I filled a glass globe, containing dry powdered muriate of lime, with oximuriatic gas. I introduced some dry paper tinged with litmus, that had been just heated, into another globe containing dry muriate of lime; after some time this globe was exhausted, and then connected with the globe containing the oximuriatic gas, and by an appropriate set of stopcocks, the paper was exposed to the action of the gas. No change of colour took place, and after two days there was scarcely a perceptible alteration.

Litmus paper not affected by dry oximuriatic gas,

Some similar paper dried, introduced into gas that had not been exposed to muriate of lime, was instantly rendered white*.

whitened by moist.

Paper that had not been previously dried, brought into contact with dried gas, underwent the same change, but more slowly.

Moist paper changed more slowly.

The hyperoximuriates seem to owe their bleaching powers entirely to their loosely combined oxigen; there is a strong

Hyperoximuriates act by

* The last experiments were made in the laboratory of the Dublin Society; most of the preceding ones in the laboratory of the Royal Institution; and I have been permitted to refer to them by the Managers of that useful public establishment.

tendency

loosely combined oxigen.

tendency in the metal of those in common use, to form simple combinations with oximuriatic gas, and the oxigen is easily expelled or attracted from them.

Oximuriatic gas not condensed and crystallized by cold.

It is generally stated in chemical books, that oximuriatic gas is capable of being condensed and crystallised at a low temperature; I have found by several experiments, that this is not the case. The solution of oximuriatic gas in water freezes more readily than pure water, but the pure gas dried by muriate of lime undergoes no change whatever, at a temperature of 40 below 0° of Fahrenheit. The mistake seems to have arisen from the exposure of the gas to cold in bottles containing moisture.

Boracium, phosphorus, iron, and arsenic, attract oxigen more strongly;

I attempted to decompose boracic and phosphoric acids by oximuriatic gas, but without success: from which it seems probable, that the attractions of boracium and phosphorus for oxigen are stronger than for oximuriatic gas. And from the experiments I have already detailed, iron and arsenic are analogous in this respect, and probably some other metals.

some other substances oximuriatic gas.

Potassium, sodium, calcium, strontium, barium, zinc, mercury, tin, lead, and probably silver, antimony, and gold, seem to have a stronger attraction for oximuriatic gas than for oxigen.

Combinations of oximuriatic compounds.

I have as yet been able to make very few experiments on the combinations of the oximuriatic compounds with each other, or with oxides. The liquor from arsenic, and that from tin, mix, producing an increase of temperature; and the phosphuretted, and the sulphuretted liquors unite with each other, and with the liquor of Libavius, but without any remarkable phenomena.

Oximuriates of phosphorus and lime.

I heated lime gently in a green glass tube, and passed the phosphoric sublimate, the saturated oximuriate of phosphorus through it, in vapour; there was a violent action with the production of heat and light, and a gray fused mass was formed, which afforded, by the action of water, muriate and phosphate of lime,

I introduced some vapour from the heated phosphoric sublimate into an exhausted retort containing dry paper tinged with litmus; the colour slowly changed to pale red.

Indications of

This fact seems in favour of the idea, that the substance is an an acid; but as some minute quantity of aqueous vapour might have been present in the receiver, the experiment cannot be regarded as decisive; the strength of its attraction for ammonia is perhaps likewise in favour of this opinion. All the oximuriates that I have tried, indeed, form triple compounds with this alkali; but the phosphorus is expelled by a gentle heat from the other compounds of oximuriatic gas and phosphorus with ammonia, and the substance remaining in combination is the phosphoric sublimate. its acidity.

6. *Some Reflections on the Nomenclature of the Oximuriatic Compounds.*

Nomenclature of oximuriatic gas.

To call a body which is not known to contain oxigen, and which cannot contain muriatic acid, oximuriatic acid, is contrary to the principles of that nomenclature in which it is adopted; and an alteration of it seems necessary to assist the progress of discussion, and to diffuse just ideas on the subject. If the great discoverer of this substance had signified it by any simple name, it would have been proper to have recurred to it; but dephlogisticated marine acid is a term, which can hardly be adopted in the present advanced æra of the science.

Chlorine, or chloric gas.

After consulting some of the most eminent chemical philosophers in this country, it has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties—its colour, and to call it *chlorine*, or *chloric* gas*.

Should it hereafter be discovered to be a compound, and even to contain oxigen, this name can imply no errour, and cannot necessarily require a change.

Salts improperly called muriates.

Most of the salts, which have been called muriates, are not known to contain any muriatic acid, or any oxigen. Thus Libavius's liquor, though converted into a muriate by water, contains only tin and oximuriatic gas, and horn-silver seems incapable of being converted into a true muriate.

Compounds of oximuriatic gas.

I venture to propose for the compounds of oximuriatic gas and inflammable matter the name of their bases, with the termination *ane*. Thus argentane may signify horn-

* From χλωρος.

silver

silver; stannane, Libavius's liquor; antimonane, butter of antimony; sulphurane, Dr. Thomson's sulphuretted liquor; and so on for the rest.

In cases when the proportion is one quantity of oximuriatic gas and one of inflammable matter, this nomenclature will be competent to express the class to which the body belongs, and its constitution. In cases when two or more proportions of inflammable matter combine with one of gas; or two or more of gas, with one of inflammable matter; it may be convenient to signify the proportions by affixing vowels before the name, when the inflammable matter predominates, and after the name, when the gas is in excess; and in the order of the alphabet, *a* signifying two, *e* three, *i* four, and so on.

Muriates. The name muriatic acid, as applied to the compound of hidrogen and oximuriatic gas, there seems to be no reason for altering. And the compounds of this body with oxides should be characterised in the usual manner, and as the other neutral salts.

Thus muriate of ammonia and muriate of magnesia are perfectly correct expressions.

I shall not dwell any longer at present upon this subject. —What I have advanced, I advance merely as suggestion, and principally for the purpose of calling the attention of philosophers to it*. As chemistry improves, many other alterations

* It may be conceived, that a name may be found for oximuriatic gas in some modification of its present appellation, which may harmonize with the new views, and which may yet signify its relation to the muriatic acid, such as demuriatic gas, or oximuric gas; but in this case it would be necessary to call the muriatic acid, hydrogenated muriatic acid, or hydromuriatic acid; and the salts which contain it hydrogenated muriates or hydromuriates; and on such a plan, the compounds of oximuriatic gas must be called demuriates or oximuriates, which I conceive would create more complexity and difficulty in unfolding just ideas on this department of chemical knowledge, than the methods which I have ventured to propose. It may however be right, considering the infant state of the investigation, to suspend for a time the adoption of any new terms for these compounds. It is possible, that oximuriatic gas may be compound, and that this body and oxigen may contain some common principle; but at present we have no more right to say that oximuriatic gas contains oxigen, than to say that tin contains hidrogen; and names should

alterations will be necessary; and it is to be hoped, that, whenever they take place, they will be made independent

should express things, and not opinions; and till a body is decomposed, it should be considered as simple.

In the last number of Mr. Nicholson's Journal, which appeared February 1st, while this sheet was correcting for the press, I have seen an ingenious paper, by Mr. Murray, of Edinburgh, in which he has attempted to show, that oximuriatic gas contains oxigen. His methods are, by detonating oximuriatic gas in excess with a mixture of hidrogen, and gaseous oxide of carbon, when he *supposes* carbonic acid is formed; and by mixing oximuriatic gas in excess with sulphuretted hidrogen, when he *supposes* sulphuric acid, or sulphureous acid is formed. In some experiments, in which my brother, Mr. John Davy, was so good as to cooperate, made over boiled mercury, we found, that 7 parts of hidrogen, 8 parts of gaseous oxide of carbon, and 20 parts of oximuriatic gas, exploded by the electric spark, diminished to about 30 measures; and calomel was formed on the sides of the tube. On adding dry ammonia in excess, and exposing the remainder to water, a gas remained, which equalled more than 9 measures, and which was gaseous oxide of carbon, with no more impurity than might be expected from the air in the gasses, and the nitrogen expelled from the ammonia; so that the oxigen in Mr. Murray's carbonic acid, it seems, was obtained from *water*, or from the carbonic oxide. Sulphuretted hidrogen, added, over dry mercury, to oximuriatic gas in excess, inflamed in two or three experiments; muriatic acid gas, containing the vapour of oximuriate of sulphur, was formed, which, when neutralized by ammonia, gave muriate of ammonia, and a combination of ammonia and oximuriate of sulphur.

Supposed decomposition of oximuriatic gas.

When a mixture of oximuriatic gas in excess, and sulphuretted hidrogen, was suffered to pass into the atmosphere, the smell was that of oximuriate of sulphur; there was not the slightest indication of the presence of any sulphuric or sulphureous acid. If Mr. Murray had used ammonia, instead of water, for analyzing his results, I do not think he would have concluded, that oximuriatic gas is capable of decomposition by such methods.

I shall not, at present, enter upon a detail of other experiments, which I have made on this subject, in cooperation with my brother, as it is his intention to refer to them, in an answer to Mr. Murray's paper.

I shall conclude, by saying, that this ingenious chemist has mistaken my views, in supposing them hypothetical; I merely state what I have seen, and what I have found. There *may* be oxigen in oximuriatic gas; but I can find none. I repeated Mr. Murray's experiments with great interest; and their results, when *water* is excluded, entirely confirm all my ideas on the subject, and afford no support to the hypothetical ideas, which he has laboured so zealously to defend.

of

of all speculative views, that new names will be derived from some simple and invariable property, and that mere arbitrary designations will be employed, to signify the class to which compounds or simple bodies belong.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Iceland crystal.

AT the meeting of this Society on the 27th of April, Professor Jameson read a paper concerning the geognostic relations of the Iceland doubly-refracting crystal. The secretary communicated an account of the habits of the colymbus immer, or ember-goose, by Dr. Edmonston of Lerwick. And Dr. Gordon read an interesting paper, consisting of observations and experiments on the qualities of sensation of sound; on the different modes in which sonorous vibrations are communicated to the auditory nerve; on the ideas of the distance, and of the angular position of sounding bodies with respect to the ear, which are associated, by experience, with the different qualities of sounds; and on some of the more remarkable differences in the *sense of hearing*, both original and accidental, which are occasionally observed among individuals, and, in particular, on the musical ear.

Ember-goose.

Qualities of sound.

Report of the Proceedings of the Mathematical and Physical Class of the French Institute, continued from p. 159.

Albumen of seeds affords nutriment to the plant.

Mr. Mirbel has continued his researches into the physiology of plants. Hitherto it had been acknowledged indeed, that the albumen of seeds commonly served to nourish the young plant after germination; but this opinion required the support of positive observations, and Mr. Mirbel appears to have removed all doubts respecting it by an experiment as simple as ingenious. The embryo in the seed of the onion bends as it unfolds itself, so as to form an elbow that rises out of the ground, while the plumula and radicle remain concealed in it. If at this stage of vegetation a mark

mark of any kind be made at equal heights on the two branches of the germe, the mark nearest the radicle will rise alone, if the plant received no aliment but from the juices of the Earth; on the contrary, if it were nourished solely by the albumen of the seed, the mark on the plumule would rise above the other: and lastly, the marks would rise pretty equally, if both the ground and the seed concurred in the developement of the germe. It is the latter phenomenon that takes place; and it ceases when the albumen is entirely absorbed: the young plant has then strength enough, to derive from the ground or the atmosphere the nourishment it thenceforward requires.

Germination of asparagus,

This paper is accompanied with interesting observations on the germination of asparagus, and on the manner in which the leaves of this plant, at first ensheathed like all those of the monocotyledons, become, by the growth of the stalk, lateral and opposite, and afterward lateral and alternate.

and of the water lily.

In another paper Mr. Mirbel has examined the germination of the nelumbium. Botanists were not unanimous respecting the class, to which this plant should be referred, or the nature of the two fleshy lobes, from between which it springs. Some, observing no radicles developed in the germination of this plant, suppose it to be destitute of them: some consider these lobes as roots; others as peculiar organs analogous to the vitellus. Mr. M. has endeavoured to remove these doubts by his dissections. In the first place he finds in the nelumbium all the characters of a plant with more than one cotyledon; he next finds in the lobes vessels analogous to those of cotyledons; and at the juncture of the lobes he observes other vessels, uniting in the same manner as those that are characteristic of the radicles in embryos furnished with them. Hence he concludes, that the water lily does not differ essentially from the other plants of its class.

The lobss of the water lily analogous to the vitellus.

Mr. Correa, while he agrees with Mr. M. in considering the nelumbium as a dicotyledon, differs from him on the nature of the lobes. He thinks, with Gaertner, that they have a great analogy to the vitellus, and he compares them with the fleshy tubercles of the roots of orchis. Plants he

observes

observes have a double organization, relating, on the one hand, to the earth, in which they spread their roots; on the other, to the air, in which their leaves are expanded. The roots are destined to the ascending vegetation; the leaves, to the descending; and it is at the point where these two systems unite, that the cotyledons are usually placed. But the lobes of the nelumbium are at the lowest part of the plant, and consequently belong to the roots. The example of many other plants destitute of cotyledons shows, that they are not essential to vegetation; and that the characters derived from them to arrange the vegetable kingdom in three divisions are insufficient, and should be replaced by those arising from the direction of the vessels and medullary radii.

Germination of grasses.

Mr. Poiteau has examined the germination of grasses. The part of the seed, which ought to be considered as the cotyledon, is yet questioned among botanists. Mr. P., observing that the scutum, which Gaertner took for a vitellus, and Mr. Richard for the body of the radicle, was placed at the point where the plumula and radicle separate, deems this a true cotyledon. Mr. P. has observed too, that, the moment when the radicle of a grass is unfolded, it assumes the figure of a cone, and represents the taproot of other plants; but as soon as the lateral roots have acquired a certain growth, this cone is obliterated, so that no plant of this family has a taproot. And as Mr. P. has made the same observation on several other monocotyledons, this substitution of numerous secondary roots for one principal root takes place; because each bundle of fibres of the monocotyledons has its peculiar root.

Amphibious mammaliæ.

The researches of Mr. Cuvier concerning fossil animals have commonly led him to discussions respecting the species admitted by naturalists, tending generally to the advancement of the science of zoology. Thus in considering the organization of the amphibious mammaliæ, he has been led to separate from the seals and morses, the Indian wallrus, the manatees, and the species described by Steller. These three genera form one family, distinguished by the absence of the posterior extremities, and by herbivorous teeth.

In

In another paper, on the genus felis, he gives the osteological characters of the head in the principal species; and has made known a species not distinguished by modern naturalists. To this he has given the name of leopard, which had become synonimous with panther, for want of being able to apply it with precision. It differs from the latter in being of a smaller size, and having a greater number of spots.

Feline genus.

Leopard.

Mr. Geoffroy long ago made a particular division, under the name of ateles, of the apes without thumbs, which had been confounded with the sapajous, from the prehensile tail common to both. He has now added two new species to those he had already made known, and given figures and descriptions of them. One of them, which he names arachnoides, had been merely mentioned by Edwards and Brown. The other, which he terms *encadreé* is altogether new. It is black, with white hairs round the face.

Classification of apes.

Two new species.

The same gentleman has described two birds; one imperfectly known, the other new. The latter has some resemblance both to the corvus nudus and the c. calvus; but there are sufficient differences between them to form three distinct genera, under the names of cephalopterus for the new species, gymnoderus for the c. nudus, and gymnocephalus for the c. calvus.

Ornithology.

The cephalopterus is black, with a very high crest, which falls forward on the beak, and a kind of dewlap also covered with feathers. Each of these is of a metallic violet colour.

Cephalopterus.

The other bird, which had been imperfectly described by Marcgrave under the name of cariama, Mr. G. had considered from his description as approaching to the trumpeter; but now he has seen it in the Museum of Natural History he classes it as a separate genus under the name of microdactylus.

Microdactylus.

The tortoises have furnished Mr. G. with the subject of another interesting paper. Having seen, while in Egypt, the tortoise of the Nile, mentioned by Forskaol, he was led to form a particular genus of all those, which like it have the extremities of the ribs separate, and a soft shell. He names it trionix, and has added to it several new species.

Tortoises.

Mr. Brougniart, in his elegant general treatise on reptiles, had

had classed these with his emydes; noticing at the same time the characters, that distinguish them from all the other species, the shell of which is complete and hard. The large softshelled tortoise of Bartram Mr. G. places in the genus chelys of Duméril.

Monography of tortoises.

Twenty years ago scarcely thirty species of tortoises were known, but nearly twice as many are accurately described by Mr. Sweiger in his general monography of tortoises. In this work a copious list of synonimes is given, and it is illustrated with figures carefully engraved.

Icthyology.

The class of fishes too has been enriched with many new species. Mr. Risseau and Mr. Delaroche have communicated the observations they made on this subject, the former in the Gulf of Nice, the latter in the sea round the Balearic Islands. It has been supposed, that fishes had their peculiar climates, but Mr. R. has found in the Mediterranean fishes considered as peculiar to the East Indies, and others known only in the northern seas. Mr. Delaroche made some interesting observations on the depth at which different fishes habitually live, the manner of catching them, and their airbladders.

Respiration of the crocodile.

Notwithstanding the difficulty of physiological experiments, and the nicety required in them, Mr. von Humboldt made many during his dangerous and toilsome travels. He has communicated his experiments on the respiration of the sharpnosed crocodile of America. He found, that, notwithstanding the volume of its bronchiæ, and the structure of its pulmonary cells, it suffers greatly without a supply of fresh air; its breathing is very slow; and a young one a foot long deprived the air of scarcely 12 cubic inches of oxigen in an hour and forty three minutes.

To be concluded in our next.

An accident has rendered us unable to insert our usual Meteorological Journal this month.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1811.

ARTICLE I.

On the Motion of Rockets both in Nonresisting and Resisting Mediums. By W. MOORE, *Esq.*

(Continued from Vol. XXVIII, *p.* 169.*)*

To Mr. W. NICHOLSON.

SIR,

THE following is a farther extension of my essay concerning the motion of rockets in different mediums; which, if worthy acceptance, is quite at your service. The last two propositions are given as preparatory to my next inquiry, which is that of the several effects of the wind upon the first motion of these machines, when it is blowing in any given direction and velocity; which I will communicate to you as soon as time will allow me properly to prepare a paper of them.

From the results of the propositions that here follow, some very curious and important facts are ascertained; as that the motion of a *rocket* can never become uniform throughout the time of its burning under any law of resistance whatever; that bodies projected into resisting me- Curious facts ascertained in the following propositions.

diums cannot, independent of gravity, describe certain finite spaces but in infinite times, let the velocity of projection be what it may, great or small; the ratio of the resistance of a sphere to that of its circumscribing cylinder, when this moves in a direction perpendicular to its axis; and many other very curious particulars, as the person who shall read this paper will find. The investigations of the resistance to cylinders moving in fluids in directions different from that of their axes are new, as far as I know. No work, that I am acquainted with, contains a solution to this problem generally, but merely of the common particular case, where the solid is supposed to move in the direction of its axis; and perhaps the flight of rockets is one out of but very few cases in which the subject is at all applicable.

With thanks for the attention which you have hitherto paid to my communications, and respect for that impartiality and ability, with which your Journal is conducted.

I am, Sir,

Your most obedient servant,

W. MOORE.

Royal Academy,
June 1811.

Prop. 6.

The motion of a rocket can never become uniform.

To determine whether the Motion of a Rocket ascending vertically in the Atmosphere can ever become uniform: the law of resistance being directly as the square of the velocity, as before.

When the motion of a body becomes uniform, or the velocity a maximum, the accelerative force is then nothing: therefore putting $\frac{(sned^2 b^2 - Rv^2)a}{(am - ct).b^2} - 1$ the accelerative force (see the last Prop.) $= o$, and reducing the equation, we have $v = b.\left(\frac{sned^2 a - am + ct}{Ra}\right)^{\frac{1}{2}}$. Whence it appears, that the velocity, and consequently the motion of the rocket can never become equable; being in terms of t the time of its burning; but will be greater and greater unto the end of the time t, when the velocity will continually decrease till the whole is destroyed by the retardive force of gravity.

gravity. And it is moreover evident, that the motion of a rocket can never become uniform under any law of resistance whatever.

Prop. 7.

All things remaining as in the 5th Proposition: to find the Velocity and Space described by the Rocket, when it is influenced only by the impelling Force of the Composition and the Resistance of the Medium.

Velocity and space described by a rocket from the impulse of its composition and resistance of the medium.

Here, gravity not acting, the accelerative force of the rocket at the end of the time t will be $\frac{(s n e d^2 b^2 - R v^2)\, a}{(a m - c t)\, b^2}$ as determined in Prop. 5. Therefore $\dot{v} = 2 g f \dot{t} = \frac{(s n e d^2 b^2 - R v^2) . 2 a g \dot{t}}{(a m - c t) . b^2} =$ (putting $h = 2 a g s n e d^2 b^2$, $k = 2 a g R$, $l = a m b^2$, and $p = c b^2$) $\frac{h \dot{t} - k v^2 \dot{t}}{l - p t}$; and $\frac{\dot{v}}{h - k v^2} = \frac{\dot{t}}{l - p t}$, whereof the fluent is $\frac{1}{2 . (h k)^{\frac{1}{2}}}$. hyp. log.

$$\frac{\left(\frac{h}{k}\right)^{\frac{1}{2}} + v}{\left(\frac{h}{k}\right)^{\frac{1}{2}} - v} = - \frac{1}{p} . \text{ hyp. log.} \left(\frac{l}{p} - t\right)$$ which, when

$v = 0$ and $t = 0$, is $o = - \frac{1}{p}$. hyp. log. $\frac{l}{p}$: therefore the

correct fluent is $\frac{1}{2\,(h k)^{\frac{1}{2}}}$. hyp. log. $\frac{\left(\frac{h}{k}\right)^{\frac{1}{2}} + v}{\left(\frac{h}{k}\right)^{\frac{1}{2}} - v} = \frac{1}{p}$

$. \left\{\text{hyp. log. } \frac{l}{p} - \text{hyp. log.} \left(\frac{l}{p} - t\right)\right\} = \frac{1}{p}$. hyp. log. $\frac{l}{l - p t}$: and hence by the nature of logs.

Velocity and space described by a rocket from the impulse of its composition and resistance of the medium.

$$\left(\frac{\left(\frac{h}{k}\right)^{\frac{1}{2}}+v}{\left(\frac{h}{k}\right)^{\frac{1}{2}}-v}\right)^{\frac{p}{2(hk)^{\frac{1}{2}}}} = \frac{l}{l-pt}$$: or, putting $\left(\frac{h}{k}\right)^{\frac{1}{2}} = j$ and $\frac{(hk)^{\frac{1}{2}}}{p} = w$, we shall have

$\frac{j+v}{j-v} = \frac{l^w}{(l-pt)^w}$; and by reducing this equa. $v = \frac{jl^w - j(l-pt)^w}{l^w + (l-pt)^w}$; which, when $t = a$, is $v = \frac{jl^w - j(l-pa)^w}{l^w + (l-pt)^w}$ the velocity of the rocket when it just ceases burning. Or, restoring the values of j, w, l, h, &c., the velocity of the rocket in thiscase will be expressed by

$$\frac{db.\left(\frac{sne}{R}\right)^{\frac{1}{2}}.\left\{(amb^2)^{\frac{4agd(sneR)^{\frac{1}{2}}}{cb}} - (amb^2-acb^2)^{\frac{4agd(sneR)^{\frac{1}{2}}}{cb}}\right\}}{(amb^2)^{\frac{4agd(sneR)^{\frac{1}{2}}}{cb}} + (amb^2-acb^2)^{\frac{4agd(sneR)^{\frac{1}{2}}}{cb}}}$$

Now to determine what this velocity is, we must first find the value of R for the given case of velocity b. Now under the conditions, that the particles of the medium are perfectly nonelastic, and that the medium is infinitely compressed and affords no resistance to the motion of the rocket but what arises from the inertia of its particles, (which is the ground of our hypotheses concerning the law of resistance), we shall, putting r for the radius of the rocket's base or of the head of the rocket; $f =$ the sine of the angle, which the slant side of the head, (supposing it conical) makes with the axis; $p = 3{\cdot}1416$; $S =$ the specific gravity of the medium, which is here considered as the atmosphere; and $g = 16$ feet, (omitting the $\frac{1}{12}$) have $R = \frac{pSr^2b^2f^2}{4g}$ (investigated in most works of fluxions and mechanics).

Let $b = 1$, in order to render the expression as simple as possible,

possible; and the angle, the sine of which is f, 30 degrees; then $f = \cdot 5$ or $\frac{1}{2}$ (to rad. 1): and taking the specific gravity of air at a medium, or $S = 1\frac{2}{9}$, R will be found $= \cdot 0002343$ ounces; which is the absolute resistance the rocket suffers when moving with a velocity of 1 foot per second. Hence the expression above for v will become - - - - - - - -

$$\frac{d\left(\frac{sne}{\cdot 0002343}\right)^{\frac{1}{2}}\left\{(am)^{\frac{4agd}{c}(\cdot 0002343\,sne)^{\frac{1}{2}}} - (am - ac)^{\frac{4agd}{c}(\cdot 0002343\,sne)^{\frac{1}{2}}}\right\}}{(am)^{\frac{4agd}{c}(\cdot 0002343\,sne)^{\frac{1}{2}}} + (am - ac)^{\frac{4agd}{c}(\cdot 0002343\,sne)^{\frac{1}{2}}}}$$

and substituting the values for a, c, d, &c., which are as follow: namely,

$$\begin{aligned}
s &= 1000 \\
n &= 230 \text{ ozs.} \\
w &= 18 \text{ lbs.} = 288 \text{ ozs.} \\
c &= 10 \text{ lbs.} = 160 \text{ ozs.} \\
m &= w + c = 448 \text{ ozs.} \\
a &= 3 \text{ sec.} \\
d &= \tfrac{1}{4} \text{ ft.} \\
g &= 16 \text{ ft.} \\
e &= \cdot 7854
\end{aligned}$$

it is $v = \dfrac{6941\cdot 575\left(1344^{1\cdot 95171} - 864^{1\cdot 95171}\right)}{1344^{1\cdot 95171} + 864^{1\cdot 95171}}$

$= \dfrac{6941\cdot 575 \times 737094}{1814186} = 2820\cdot 325$ feet: which is therefore the greatest velocity the rocket can acquire, and which it does acquire at the end of its burning. Velocity.

It is somewhat remarkable, that the whole resistance of the air to the rocket, on the supposition that gravity does not act, should so nearly approximate to the effect of this force (considered as constant) when there is no consideration of any resistance from the former; the deviation causing no more than $(2896\cdot 9895 - 2820\cdot 325 =)$ $76\cdot 6645$ feet per second

Velocity.

second difference in the greatest velocity of the rocket on the side of gravity.

To find the space described. By the theory of variable motions $\dot{x} = v\dot{t} = \frac{jl^w\dot{t} - j\dot{t}\,(l - pt)^w}{l^w + (l - pt)^w} = j\dot{t} - \frac{2j\dot{t}\,(l - pt)^w}{l^w + (l - pt)^w}$. Put $l - pt = T$; then $\dot{T} = -p\dot{t}$, and $\dot{t} = \frac{-\dot{T}}{p}$. Whence $\dot{x} = \frac{-j\dot{T}}{p} + \frac{2j}{p}.\frac{T^w\dot{T}}{l^w + T^w}$ = (by expanding $\frac{T^w\dot{T}}{l^w + T^w}$ in a series) $-\frac{j\dot{T}}{p} + \frac{2j}{p}$ $-\left(\frac{T^w\dot{T}}{l^w} - \frac{T^{2w}\dot{T}}{l^{2w}} + \frac{T^{3w}\dot{T}}{l^{3w}} - \frac{T^{4w}\dot{T}}{l^{4w}} + \&c.\right)$; the fluent of which is

$$x = \frac{-jT}{p} + \frac{2j}{p}\left(\frac{T^{w+1}}{(w+1)\,l^w} - \frac{T^{2w+1}}{(2w+1).\,l^{2w}} + \frac{T^{3w+1}}{(3w+1)\,l^{3w}} - \frac{T^{4w+1}}{(4w+1).\,l^{4w}} + \&c.\right)$$

$$= \frac{-jT}{p} + \frac{2jT^{w+1}}{pl^w}.\left(\frac{1}{w+1} - \frac{T^w}{(2w+1)\,l^w} + \frac{T^{2w}}{(3w+1)\,l^{2w}} - \frac{T^{3w}}{(3w+1)\,l^{3w}} + \&c.\right) = \frac{j}{p}.$$

$$\left\{-(l - pt) + \frac{2\,(l - pt)^{w+1}}{l^w}\left(\frac{1}{w+1} - \frac{(l - pt)^w}{(2w+1).\,l^w} + \frac{(l - pt)^{2w}}{(3w+1)\,l^{2w}} - \frac{(l - pt)^{3w}}{(4w+1).\,l^{3w}} + \&c.\right)\right\};$$

and the fluent corrected

$$x = \frac{j}{p}\left\{l - 2l.\left(\frac{1}{w+1} - \frac{1}{2w+1} + \frac{1}{3w+1} - \frac{1}{4w+1} + \&c.\right)\right\} + \frac{j}{p}.$$

$$\left\{-(l - pt) + \frac{2\,(l - pt)^{w+1}}{l^w}.\left(\frac{1}{w+1} - \frac{(l - pt)^w}{(2w+1).\,l^w} + \frac{(l - pt)^{2w}}{(3w+1)\,l^{2w}} - \frac{(l - pt)^{3w}}{(4w+1).\,l^{3w}} + \right.\right.$$

$$\left.\left.\&c.\right)\right\} = (\text{when } t = a)\; j. \left\{ a + \frac{2\,(l - ap)^{w}}{l^{w}} . \left(\frac{1}{w+1} - \frac{(l - ap)^{w}}{(2w+1).\,l^{w}} + \frac{(l - ap)^{2w}}{(3w+1).\,l^{2w}} - \frac{(l - ap)^{3w}}{(3w+1).\,l^{3w}} + \&c. \right) - \frac{2\,l}{p} . \left(\frac{1}{w+1} - \frac{1}{2w+1} + \frac{1}{3w+1} - \&c. \right) \right\}$$

for the space described by the rocket at the end of the time t.

Now to determine how far the rocket will farther move before its motion is wholly destroyed. Put a = the velocity at the end of its burning = 2820·325 feet per second, and v any variable velocity corresponding to the space x; w = weight of the rocket = 448 ozs., and R = ·0002343 ounces, the resistance of the medium to the rocket when moving with a velocity of 1 foot per second. Then Rv^2 will be the resistance to velocity v, and $\frac{Rv^2}{w}$ the force by which the rocket is retarded by the fluid. Hence $\dot{x} = \frac{-v\dot{v}}{2fg} = -\frac{w\dot{v}}{2gRv}$, and $x = \frac{-w}{2gR}$. hyp. log. v; and the fluent corrected $x = \frac{w}{2gR}$. hyp. log. a. Which by substitution of numbers is = 305170·3 feet.

Space described.

Hence it appears, that, after the burning of the rocket ceases, it will move to a distance of 305170·3 feet, or nearly 58 miles, before all its motion is destroyed, when it will remain at rest in the medium; there being no force to influence it in any manner or direction whatever, and having no power to create motion in itself.

Time of moving through it.

As to the time that the rocket would be in moving through this space, it will be had as follows. The same substitution as above being retained; the general fluxional expression for the time $(\dot{t})$ namely $\frac{-\dot{v}}{2gf}$ will be found $= \frac{-\dot{v}}{2gRv^2}$

=

$= \frac{-1}{2gR} \cdot \frac{\dot{v}}{v^2}$ (substituting $\frac{Rv^2}{w}$ for f as before) the fluent of which is $t = \frac{1}{2gRv}$. Now when $t = 0, v = a$, therefore the correct fluent of the time is $t = \frac{1}{2gRv} - \frac{1}{2gRa}$ which, on v becoming nothing, will be infinite. So that it appears, that the rocket will not describe the above space but in an infinite time.

Suppose $v = 1$ foot; then $t = \frac{a-1}{2gRa} = 133{\cdot}344$ seconds or 2 min. 13 seconds. That is, the rocket will only have been in motion 2 min. 13 sec. after it has acquired the greatest velocity from its burning before the celerity of its motion will be reduced to 1 foot per second; and yet, notwithstanding this great annihilation of velocity in so short a time, the remaining small part will not in any finite time be destroyed, though we know the limit at which the rocket would attain a state of quiescence.

A projected body cannot lose all its motion in any finite time.

And from the result here determined we conclude, that into whatever medium a body is projected with any given velocity, great or small, it will in no finite time lose all its motion. So that, if the planetary bodies were moving in a resisting medium, and gravity should suddenly be destroyed; the bodies would all pursue rectilinear paths (that would be tangents to their orbits) to certain finite distances, which would not be wholly described by them but in infinite times.

Lemma I.

Resistance to a cylinder moving perpendicularly through a fluid.

To determine the Resistance a Cylinder meets with in a Fluid when moving in a Direction perpendicular to its Axis.

It is universally allowed, and indeed is evident, that the resistance to a body moving through an infinite fluid at rest (such as is here supposed) is the same in effect as the force of the fluid in motion with equal velocity on the body at rest: therefore, as it will be somewhat more convenient to consider the fluid in motion, and the body quiescent, we shall pursue the solution of the problem upon this hypothesis.

Let

Resistance to a cylinder moving perpendicularly through a fluid.

Let A B C D (Pl. VII, fig. 1) be the cylinder, and E T F any section parallel to the base. Let a particle strike this section at T in the direction P T, perpendicular, by supposition, to BD; and draw T O to the centre O: draw also the tangent T Q to the circle E T F or cylinder at T, upon which let fall the perpendicular P Q, and let fall the perpendicular Q R upon T P.

Then, considering P T to represent the full force of a particle of the fluid, P R will denote that part only, which has effect in moving the cylinder in the direction P R. For, on account of the obliquity of the surface of the solid, the stroke of the particle will also be oblique; and therefore, resolving T P into the two forces P Q and T Q, the force T Q only will be effective, which, in the direction P R, will be as P R, or the sine of the angle P Q R, or P T Q; as is evident by considering P Q resolved into the two forces Q R, R P; whereof the former, being parallel to the cylinder, has no effect in moving it in a perpendicular direction thereto.

Now by the nature of fluids, the force with which a particle strikes a body perpendicularly is equal to the weight of a line of such particles, the height of which is equal to that which is due to the velocity of its motion, or through which a body must fall to acquire that velocity; therefore, calling n the density of the particles or fluid; $\frac{n v^2}{4g}$ (where v denotes the velocity, and $g = 16$ feet) will be the absolute force of a particle moving with the velocity v. And this is represented above by the line P T; therefore, since rad. (1) : T P :: sin. $\angle$ P T Q (s) : P Q, the force denoted by P Q will be $\frac{n v^2 s}{4g}$, and that by P R $\frac{n v^2 s^2}{4g}$.

Put S L $= x$, L T $= y$, and S T $= z$; also let T n ($= \dot{z}$) express the fluxion of the course S T; then, because of the inclination of this line to the direction of the fluid, the number of particles striking it will be diminished in the ratio of T n to no, or of radius to the sine of the angle o T n; consequently the fluxion of the force of the fluid against S T, which would otherwise have been $\frac{n v^2 s^2 \dot{z}}{4g}$, will be $\frac{n v^2 s^3 \dot{z}}{4g}$.

Now

Resistance to a cylinder moving perpendicularly through a fluid.

Now $\dot{z} = (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$; and $y = (2rx - x^2)^{\frac{1}{2}}$ by the property of the circle; consequently $\dot{y} = \frac{r\dot{x} - x\dot{x}}{(2rx - x^2)^{\frac{1}{2}}}$ and $\dot{z} = (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}} = \frac{r\dot{x}}{(2rx - x^2)^{\frac{1}{2}}}$ (r being the rad. of the cir. E S F). Also, by reason of similar triangles, $\frac{QP}{TP} = \frac{LT}{OT} = \frac{y}{r}$: whence s, being $= \frac{QP}{TP}$, will also be equal to $\frac{y}{r}$. Therefore by substitution $\frac{nv^2 s^3 \dot{z}}{4g} = \frac{nv^2}{4g} \cdot \frac{y^3}{r^3} \cdot \frac{r\dot{x}}{(2rx - x^2)^{\frac{1}{2}}} = \frac{nv^2}{4g} \cdot \frac{(2rx - x^2)^{\frac{3}{2}}}{r^3} \cdot \frac{r\dot{x}}{(2rx - x^2)^{\frac{1}{2}}} = \frac{nv^2}{4gr} \cdot (2rx\dot{x} - x^2\dot{x})$; of which the fluent is $\frac{nv^2}{4gr^2} \cdot \left(\frac{3rx^2 - x^3}{3}\right)$ wanting no correction; so that when $x = 2r$ the fluent will be $\frac{nv^2 r}{3g}$; which is the effective force of the fluid on the semicircumference of a section of the cylinder parallel to the base. Consequently $\frac{nv^2 r}{3g}$ into the height of the cylinder (h) $= \frac{nv^2 rh}{3g}$ will be the resistance, that the whole cylinder suffers when it moves in a direction perpendicular to its axis with the velocity v.

Cor. Because it is found, that a sphere, the radius of which is r, moving in a fluid of the density n, with the velocity v, is $\frac{pnv^2r^2}{8g}$; we shall have the resistance of the sphere to the resistance of its circumscribing cylinder as $\frac{pnv^2r^2}{8g}$ to $\frac{2nv^2r^2}{3g}$, or as 1 to $\frac{16}{3p}$ (where $p = 3{\cdot}1416$); the latter therefore being resisted more than the former by about ·69829 of the former. Whence, the resistance to a sphere being given, the resistance to its circumscribing cylinder will be had by multiplying the former by 1·69829.

LEMMA

LEMMA II.

To determine the same as in the last, when the Cylinder moves in any Direction oblique to its Axis.

Resistance to a cylinder moving obliquely.

Let T P (Pl. VII, fig. 2) be the direction of the cylinder moving in the fluid, or P T that of the fluid against the cylinder. Let a particle strike the solid at T, at which point draw the tangent T n to the section E F T, which is parallel to the base C D: draw L T Q perp. to the diameter V O S, which is at right angles to the axis X Y, and P Q and Q R perp. to T Q and T P respectively. Then, denoting the force of a particle of the fluid when in motion by P T, and supposing this to be resolved into the two forces P Q, Q T, the latter only, Q T, which varies as the sine of the angle T P Q, will have effect in moving the cylinder; which, in the direction P T, will be as R T, or the sine of the angle T Q R, or S P Q. Now the effective force of a particle in the direction Q T has been shown in the preceding lemma to be equal to $\frac{n v^2 s^2}{4 g}$ when the whole force of a particle is represented by Q T: but in the case before us, putting f for the sine of the angle Q P T, or of the angle of incidence of the impingeing fluid against the solid, the efficacy of Q T will consequently be $= \frac{n v^2 s^2 f}{4 g}$ (where s = sin. of the angle Q T n) and therefore the effect of a particle to move the cylinder in the direction P T will be $\frac{n v^2 s^2 f^2}{4 g}$.

Put c = sin. of the angle P T Q, the cosin. being f

r = rad. of the base of the cylinder

x = O L

y = T L

Then, by reason of the similitude of the triangles O L T, T n K, we obviously obtain $s = \frac{y}{r} = \frac{(r^2 - x^2)^{\frac{1}{2}}}{r}$, and the

Resistance to a cylinder moving obliquely.

the cosine of the angle K T $n = \frac{x}{r}$; (radius being unity in all cases). Now let $z =$ F T and $\dot z$ the fluxion of the same, then the fluxion of the force of the fluid on F T will be $= \frac{n v^2 s^2 \int^2 \dot z}{4g}$ multiplied by the sine of the angle P T n, whereof the angle P T n being composed of the two angles P T Q, Q T n, the natural sines and cosines of which are represented above; its sine by trig. will be expressed by $\frac{cx}{r} + \frac{\int (r^2 - x^2)^{\frac{1}{2}}}{r} = \frac{cx + \int (r^2 - x^2)^{\frac{1}{2}}}{r}$; also $\dot z$

$= \frac{r \dot x}{(r^2 - x^2)^{\frac{1}{2}}}$. Therefore

$$\frac{n v^2 s^2 \int^2 \dot z}{4g} \text{. sine angle P T } n =$$

$$\frac{n v \int^2 {}^2}{4g} \cdot \frac{r^2 - x^2}{r^2} \cdot \frac{r \dot x}{(r^2 - x^2)^{\frac{1}{3}}} \cdot \frac{cx + \int (r^2 - x^2)^{\frac{1}{2}}}{r}$$

$$= \frac{n v^2 \int^2}{4 g r^2} \cdot \frac{\left\{ cx\,(r^2 - x^2) + \int (r^2 - x^2)^{\frac{3}{2}} \right\} \dot x}{(r^2 - x^2)^{\frac{1}{2}}}$$

$$= \frac{n v^2 \int^2}{4 g r^2} \cdot \left\{ c x \dot x\,(r^2 - x^2)^{\frac{1}{2}} + \int r^2 \dot x - \int x^2 \dot x \right\};$$

the fluent of which is

$$\frac{n v^2 \int^2}{4 g r^2} \cdot \left\{ -\frac{1}{3} c\,(r^2 - x^2)^{\frac{3}{2}} + \int r^2 x - \frac{1}{3} \int x^3 \right\}$$

which corrected will, in the ultimate case, where $x = r$, be

$$\frac{n v^2 \int^2}{4 g r^2} \cdot \left(\frac{1}{3} c r^3 + \frac{2}{3} \int r^3 \right) = \frac{n v^2 \int^2 r}{12 g} \cdot \left(c + 2 \int \right),$$

which is therefore the effective force of the fluid on the quadrantal arch F T S. Hence $\frac{n v^2 \int^2 r}{6 g} \cdot (c + 2\int)$ will be the force on the semicircum. V F S; and $\frac{n v^2 \int^2 r h}{6 g} \cdot (c + 2\int)$ the force on the whole semicylindric surface m D v r B s; or or the resistance to the cylinder when moving in the fluid at rest, so far as relates to that surface.

Resistance to a cylinder moving obliquely.

To determine what farther resistance is opposed to the cylinder by the fluid acting against the top A s B r. Let us suppose AV BT (fig. 3) to be the head of the cylinder, and a particle striking it at T; also let A B be a diameter of the circle perp. to the axis, and draw T Q parallel to A B, and P Q and Q R perp. to T Q and T P respectively. Then P T being considered the representative of the full force of a particle, and to be resolved into the two forces P Q, T Q; the force T Q, being parallel to the plane A B V, has no effect in causing it to move; but only the force denoted by P Q, which is as the sine (c) of the angle P T Q. Therefore the effective force of a particle in this case will be $\frac{nv^2c^2}{4g}$; and that of the fluid on the whole circular plane $\frac{nv^2c^5pr^2}{4g}$ (p being $= 3{\cdot}1416$). Hence the whole resistance to the cylinder is

$$\frac{nv^2 f^2 rh}{6g} \cdot (c + 2f) + \frac{nv^2 c^3 pr^2}{4g}$$

Cor. 1. When the angle T P Q (fig. 2) is 90°, or the solid moves in a direction perp. to its axis; then f becoming 1 and c nothing, the resistance to the cylinder will be $\frac{nv^2rh}{3g}$ as determined in the first lemma.

Cor. 2. The resistance to the cylinder moving in the direction T P estimated in the direction Q T is $\frac{nv^2 f rh}{6g}$ $(c + 2f)$, being that arising only from the action of the fluid upon the semisurface of the solid; that on the head or top of the cylinder having no effect to move it in this direction, but in the direction of its axis.

For an example to this proposition in numbers, when the medium is supposed to be that of our atmosphere. Let the angle T P Q (the sine of which is f) $= 60°$; and consequently the angle P T Q (the sine of which is c) $= 30°$.

Then

Then we have $f = {\cdot}866$
$c = {\cdot}5$
Let $r = 6$ in. $= \frac{1}{2}$ foot
$v = 1$ ft.
$h = 3$ ft.
$g = 16$ ft, (omitting the $\frac{1}{12}$th)
$n = 1\frac{1}{9}$

$$\text{Hence } \frac{nv^2 f^2 rh}{6g} \cdot (c + 2f) + \frac{nv^2 c^3 pr^2}{4g} =$$

${\cdot}03196687 + {\cdot}00187486 = {\cdot}03384173$ ounces for the resistance to a cylinder of the above dimensions, when moving with the velocity of 1 foot per second. And therefore, as the resistance to the same cylinder varies as the square of the velocity, the resistance corresponding to any other velocity will be had by multiplying the above by the velocity (in feet) squared.

II.

On the Defective Algorithm of Imaginary Quantities. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

A quadratic equation apparently with three roots.

IN a mathematical investigation, in which I was lately engaged, I fell upon a very singular anomaly in the theory of equations, which is nothing less than a *quadratic equation* having (at least to all appearance) *three roots*, all different from each other; whereas, according to received principles, it can have only two. As this is a very ſtrange deviation from what has been hitherto considered as a well established theory, I am induced to request the publication of it in your Journal, in hopes that some of your mathematical correspondents may undertake to explain the difficulty, and rescue the theory of equations, and the present algorithm of imaginary quantities, from the danger to which such anomalies muſt necessarily expose them; particularly as there are

are some among us, who wish to cramp the power of analysis, by rejecting in that science every species of quantity coming under an imaginary form. I think I can perceive where the mystery lies; but still I should be glad to see the opinion of more able analysts on this apparent incongruity; if however no such should appear, I will, through the medium of your Journal, publish my ideas on the subject.

The equation to which I have alluded is this:

$$x^2 + x = 2$$

and the *three roots* of it are the following,

$$\begin{aligned} &\text{1st root } x = 1 \\ &\text{2d root } x = -2 \\ &\text{3d root } x = \sqrt[3]{\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}} + \sqrt[3]{\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}}} \end{aligned}$$

The two first of which evidently answer the conditions of the equation, and with the third I proceed as follows.

$$x^2 = \left(\sqrt[3]{\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}} + \sqrt[3]{\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}}}\right)^2, \text{ or}$$

$$x^2 = \sqrt[3]{(\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}})^2} + 2\sqrt[3]{(\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}) \times (\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}})}$$
$$+ \sqrt[3]{(\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}})^2}$$

And now in order that I may be certain of my results, I multiply these quantities under the radicals at full length, as follows; viz.

$$\left.\begin{array}{l} \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} \\ \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} \end{array}\right\} \text{to find the square of } \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}$$
$$\begin{array}{l} \tfrac{1}{4} + \tfrac{1}{2}\sqrt{-\tfrac{3}{4}} \\ \quad\;\; \tfrac{1}{2}\sqrt{-\tfrac{3}{4}} - \tfrac{3}{4} \\ \hline \tfrac{1}{4} + \sqrt{-\tfrac{3}{4}} - \tfrac{3}{4} = -\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} = -(\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}}) \end{array}$$

$$\left.\begin{array}{l} \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} \\ \tfrac{1}{2} - \sqrt{-\tfrac{3}{4}} \end{array}\right\} \text{for the product } (\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}) \times (\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}})$$
$$\begin{array}{l} \tfrac{1}{4} + \tfrac{1}{2}\sqrt{-\tfrac{3}{4}} \\ \quad - \tfrac{1}{2}\sqrt{-\tfrac{3}{4}} - -\tfrac{3}{4} \\ \hline \tfrac{1}{4} \quad * \quad + \tfrac{3}{4} = 1 \end{array}$$

$$\left.\begin{array}{l}\frac{1}{2}-\sqrt{-\frac{3}{4}}\\ \frac{1}{2}-\sqrt{-\frac{3}{4}}\end{array}\right\}\text{ to find the square of } \frac{1}{2}-\sqrt{-\frac{3}{4}}$$

$$\begin{array}{l}\frac{1}{4}-\frac{1}{2}\sqrt{-\frac{3}{4}}\\ \quad-\frac{1}{2}\sqrt{-\frac{3}{4}}-\frac{3}{4}\\ \hline \frac{1}{4}-\sqrt{-\frac{3}{4}}-\frac{3}{4}=-\frac{1}{2}-\sqrt{-\frac{3}{4}}=-\left(\frac{1}{2}+\sqrt{-\frac{3}{4}}\right)\end{array}$$

Therefore

$$x^2=\sqrt[3]{-\left(\frac{1}{2}-\sqrt{-\frac{3}{4}}\right)}+2+\sqrt[3]{-\left(\frac{1}{2}+\sqrt{-\frac{3}{4}}\right)} \text{ or}$$

$$x^2=2-\sqrt[3]{\frac{1}{2}-\sqrt{-\frac{3}{4}}}-\sqrt[3]{\frac{1}{2}+\sqrt{-\frac{3}{4}}} \text{ or}$$

$$x^2=2-\left(\sqrt[3]{\frac{1}{2}+\sqrt{-\frac{3}{4}}}+\sqrt[3]{\frac{1}{2}-\sqrt{-\frac{3}{4}}}\right) \text{ but}$$

$$x=\left(\sqrt[3]{\frac{1}{2}+\sqrt{-\frac{3}{4}}}+\sqrt[3]{\frac{1}{2}-\sqrt{-\frac{3}{4}}}\right)$$

and therefore by addition, we have

$$x^2+x=2$$

Now if this be a legitimate result, I see no reason why this value of x should not be considered as a root of the proposed equation as well as the other two; and if it be admitted as such, then I can find any number of other roots at pleasure; which will totally destroy the established theory of equations; but if, on the contrary, this cannot be admitted as a root, then it necessarily follows, that the present algorithm of imaginary quantities is defective, or otherwise that I have deviated from that algorithm in the preceding operation. In order to discover the errour wherever it may lie, and that the connection of it may be made public, I am induced to request the publication of this paper in your Journal; which, if you should think proper to comply with, will much oblige

Yours &c.

MATHEMATICUS.

III.

III.

On the Nature of Heat. *By* MARSHALL HALL, *Esq.* *In a Letter from the Author.*

(Concluded from p. 222.)

IN appreciating the merit of any hypothesis, we ought certainly to consider, what assumptions are inseparable from the subject itself; and what suppositions are necessary, to constitute the particular hypothesis proposed.

Some assumptions may be inseparable from a subject, others connected with a particular view of it.

To apply this to our subject; it appears to me, that, whatever may be our notion concerning the ultimate nature of caloric, one postulate must necessarily be made; the existence of a channel for this agent between the Sun and the Earth must unavoidably be assumed.

Suppositions connected with the materiality, and immateriality of heat.

If we embrace the opinion of the materiality of caloric, we suppose, that this matter emanates constantly from the sun's surface; and penetrates space. On the other hand, in adopting the opposite opinion, we necessarily suppose the existence of a fluid, naturally pervading the universe in a state of quiescence; but ready to be impressed by external causes. This is indeed the great difficulty; and a difficulty, which no one will pretend to obviate. It may diminish the objection, which is thus afforded to the hypothesis, to observe, that on either side of the question the difficulty is nearly the same; or, if there be any difference, it is in favour of the hypothesis of vibration. For what is the great difference, between the assumption of a material agent, which, being impelled, penetrates space with rapid motion; and that of a quiescent fluid pervading space, and subject to certain impressions?

Most assumptions in the material hypothesis.

But, if we consider this circumstance farther, we shall observe, that, in the material theory, the assumption of one fluid only does not suffice. According to this opinion, the sun-beam must consist of at least three; or, if we consider the compound nature of light, of no less than of nine distinct fluids. Many persons however will be willing to grant all these to modern theorists, who would refuse to Huygens and Euler their ethereal medium; although both suppositions are equally hypothetical.

Neglect of induction

It must be acknowledged, that in investigating the nature of caloric, we subject ourselves to the imputation of false philosophy. We neglect the method of induction, and seek, as the ancients did, occult causes. So much are we involved in the trammels of theory, that we are scarcely able, in some sciences, to express a single fact, without implying the existence of something perfectly hypothetical; this is very much the case with our present subject, heat, and with the science of electricity. We are educated in the belief of such hypotheses, and do not doubt of their truth, until a considerable progress has been made in the study of them. It is not one of the least of the uses of investigations like the present, to teach us how very little all hypotheses ought to be relied on, and how very much and how constantly they ought to be distrusted.

and recourse to hypothesis too common.

The Nature of the Vibration of Heat.

Nature of the vibration of heat.

Heat may possibly depend, not on the presence of any material fluid in the interstices of bodies, but on a state of intimate vibration of their particles. The temperature or degree of heat may be greater or less, according as these vibrations may be more or less frequent in any given time; or, as it may be expressed, according to the intensity of the vibration. By this term I wish only to express the relative state of the vibration; that vibration I suppose to be the most intense, which occasions the highest temperature.

Objection to the hypothesis.

An objection which has always been urged to the hypothesis of vibration is, that the propagation of heat does not obey the established laws of motion. "Were they the same, its propagation ought to be momentary through elastic bodies, and should be more or less rapid through others, according to their elasticity."

Answer to the first part of it.

To the first part of this objection, it may be answered, that the propagation of heat through elastic bodies is indeed momentary; for this is the radiation of caloric. Previously to considering the second part of the objection, it will be necessary to consider somewhat of the *nature* of the vibrations,

tions, which we have supposed to constitute heat; and to show in what respect they differ from other vibrations.

The important and manifest difference between these vibrations, of sound, for example, and of heat, is, that in the former the *mass*, in the latter, the *particles* only of that mass, vibrate; and this distinction is sufficient to explain the necessary and consequent difference in the laws observed by these vibrations. The facility with which the mass of any body vibrates will be proportionate to the elasticity of the body; but it is plain, that the vibration of the particles of the mass will obey laws as different as the vibrations themselves are different: accordingly, who, after this consideration, would expect that the elasticity of any body should regulate the vibration of its particles only? It is argued indeed, that the vibration of the mass of any body must ultimately be referred to the condition of its particles; this I readily admit: yet it proves nothing; it does not prove that the converse of this is true; namely, that the vibration of the particles must be determined by the condition of the mass.

Difference between the vibrations of heat and sound.

Perhaps it was the want of considering the necessary difference between the vibrations of heat and of sound, that has led to some other objections to this theory. It has been said, that no body could communicate heat to another, (if heat were vibration), unless the second made a sort of concord with the first. Another objection is still more futile; the vibrations, if such constituted heat, would, it is said, "gradually relax and die away."

Other objections.

Sources of Caloric.

For the same reason, that this part of the subject was treated with brevity in the former part of this discussion, I may be equally concise in this place. The intimate connection between motion, friction, percussion, &c. and heat, has lately been so much attended to, and so satisfactorily explained by the theory of vibration, that nothing scarcely remains to be added on this point.

Sources of heat.

The light and heat produced by the transition of the electric fluid from one body to another is extremely analogous to the sound produced by the motion of air through tubes.

If the transition be sudden, powerful light and heat are occasioned; if it be slow and equable, a continual spring of light and heat is formed. In the same manner, if the motion of air be rapid, a sound is produced as powerful as the heat and light, in the first instance; if the motion of the air be slow and equable, the sound produced is smooth and uninterrupted.

Motion of Caloric.

Motion of heat.

It has been said, that the best conductors of caloric receive and part with this power the most rapidly. This is precisely what our hypothesis would have led us to expect, *a priori*. It is to be remarked, that the action of hot and cold bodies upon each other is *reciprocal*. The heating and cooling of bodies is, according to our opinion, the same operation; both are reducible to the effecting a change in the state of vibration; and different substances are susceptible of this change in different degrees; those which are most so are the most easily heated, and the most readily cooled.

This explanation applies equally well to the absorption and radiation of heat and cold; which are perhaps greater difficulties in the opposite opinion, than even the circumstance with respect to conducting power.

Radiant heat.

It was formerly stated, that radiant heat is extremely different, according as it comes from the sun, or from a source of heat upon Earth; I wish however to state this difference somewhat more distinctly.

From the sun.

That the heat of the sun is transmissible through and refrangible by transparent media, is abundantly proved; the refrangibility of the heat accompanying the coloured part of the prismatic spectrum, and of the invisible rays of solar heat, is shown in the 13th and 17th experiments of Dr. Herschel *; and the familiar use of burning lenses demonstrates the refraction of the caloric of the undecomposed solar ray.

From a fire.

On the other hand, that culinary heat is not transmissible through any solid body is very decisively proved by the valuable experiments contained in Mr. Leslie's third chapter: see the Inquiry.

* Phil. Trans. for 1800: or Journal, 4to Series, Vol. IV, p. 34, 366.

The

The latter statement requires however some qualification and restriction; for I must now observe, that, although Mr. Leslie's experiments prove decidedly the difference between solar and culinary heat in this respect, yet he has I believe proceeded too far, in asserting, that the latter is not *at all* transmissible through transparent media. That the heat of a candle is in some degree refracted by glass is proved by the 13th experiment of Dr. Herschel; the heat of a common fire was transmitted and refracted in the 14th and 16th; the heat of redhot iron was refracted in the 15th; and invisible culinary heat was refracted in the 19th and 20th experiments*. The heat emanating from a candle, from a boiling mixture of sulphuric acid and water, and from boiling water, was transmitted through glass, in some experiments performed by my friend Mr. Maycock †. The latter in some degree transmissible,

The whole of these experiments concur in establishing a remarkable difference, between the transmission of radiant culinary and solar heat. Solar heat is scarcely if at all impeded, culinary heat almost entirely intercepted by transparent media! but not equally to solar heat.

But this is not the only difference between solar and culinary heat; another distinction is observed in their reflection. "Cover each ball of a differential thermometer with a coat "of tinfoil, and rub that one below which the scale is affixed gently with sand paper; or it may be rubbed before "it is applied to the glass. Placing the instrument now in "the sun, the liquor will visibly rise, perhaps 5 or 10 degrees." "Set this differential thermometer now directly "opposite the fire, and about two or three feet distant from "it. In this situation a very remarkable depression will "quickly take place, equal perhaps to 30 or 40 degrees." "This beautiful experiment likewise indicates clearly the "distinction between the solar rays and culinary heat ‡." Difference in the reflection of solar and culinary heat.

The explanation of this phenomenon, which follows its relation, will not, I conceive be readily acquiesced in; "the "light from the fire, has," it is said, "some tendency, to Attempt to account for this.

* Phil. Trans. 1800; or Journal as above.

† Phil. Journal, Vol. XXVI, p. 75.

‡ Inquiry, p. 83, et seq.

"counteract

"counteract or diminish in a certain measure the peculiar "effect of the heat emitted from the same source."

Other differences.

Another difference still between the two kinds of heat was discovered by Mr. Leslie. A very considerable aberration takes place in the reflection of culinary heat, which is not I believe the case with the solar rays. Nor is the effect of colour, in absorbing the two kinds of heat, the same.—"Stained paper has very nearly the same action as white "paper, and it is only when covered by a pigment super-"induced, that the diversity of effect becomes conspicu-"ous *."

Why solar heat passes through transparent bodies wholly;

I shall now attempt to explain this remarkable difference between solar and culinary heat. Solar heat may consist of vibrations in that medium or fluid, which we suppose to fill space. This fluid is one of extreme tenuity, and pervades all bodies without exception; vibration therefore, which subsists in this fluid, does and ought to pass through such bodies as are transparent, with little or no interruption.

other heat only in part.

Radiation from other bodies, that is radiant culinary heat, is very different: the radiator is in a state of vibration; this vibration is communicated to all surrounding bodies, the most important of which is the atmosphere; the subtile fluid too must be taken into consideration; these, with other bodies, which are within the vicinity of the source of heat, take on vibration, and convey it to distant surfaces. In as much as the vibration subsists in the more subtile medium, it will, as it did in the case of solar heat, pervade transparent bodies; but the chief conductor of heat in this operation is the "ambient air;" this fluid does not pervade transparent or other bodies, its vibrations will therefore be intercepted by them.

Other differences.

It is easy to conceive, that, as the two kinds of radiant heat are so extremely different, the laws which are observed in their other motions shall be very different; a difference in their reflection and absorption is what might have been naurally expected.

Opaque bodies.

From some cause, the pervading fluid of certain bodies does not propagate its vibrations; these bodies are therefore

* Inquiry, p. 94.

opaque

opaque *. It is from this circumstance, that opaque bodies only are heated by the sun's rays; in intercepting them, they receive the heating power, the vibration of these rays. Such bodies act upon solar heat somewhat in the same manner as all bodies act upon culinary heat.

Air a quick conductor of heat.

From this view of the subject it appears, that air is a very quick conductor of heat; Berthollet has remarked this fact †: nevertheless air is employed in the arts as a bad conductor: this circumstance requires some explanation.

Conducting power of bodies.

Different bodies are susceptible, in different degrees, of undergoing a change in their vibration; and, having suffered a change in their vibration, they convey this change to distant parts with different degrees of celerity. Cæteris paribus, those bodies, which are most susceptible of change in vibration, induce the least change in other bodies; and, cæteris paribus, those bodies, which convey the changes they may have suffered with most celerity, produce the greatest change in other bodies. Thus the conductor, which occasions the greatest change in temperature, is that which unites the properties of celerity of conducting power and little susceptibility of change in vibration: and thus, although air conducts vibration with much celerity, yet, from its high susceptibility of change in vibration, its effect in augmenting or reducing the temperature of bodies is by no means great. It appears that the terms good and bad conductors are involved in some ambiguity.

Radiation of Cold.

Radiation of cold.

This phenomenon appears to me to be the most decisive in demonstrating the true nature of caloric; it deserves perhaps the appellation of experimentum crucis.

Effect of a concave mirror.

A concave mirror has the property of concentrating the rays of vibration proceeding from a source properly opposed to it. In a similar manner the vibrations of air constituting sound are converged in an eliptical chamber. The operation of mirrors does not however increase the *intensity* of the

* It is necessary to remark, that I have considered the theory of light of Huygens and Euler as the most probable; a few observations on this subject may probably at some future time be transmitted to the Philosophical Journal.

† Murray, Vol. I, p. 274.

vibration

vibration of rays, it merely causes them to converge, collects and unites their effect. The intensity of vibration in the focus of the mirror is not greater than that of each of the rays before they converged; but as the force of all the rays is concentrated in the focus, the heating effect will be greater there; that is, a body in the focus will be heated much sooner than by the operation of a single ray, but will never attain to an intensity of vibration greater than that of a single ray. In like manner, although the force of rays of sound be accumulated in the focus of an elliptical chamber, yet the note, or pitch of the sound, i. e. its intensity of vibration, remains the same.

Now vibrations of a certain intensity occasion the sensation and phenomena of cold; the accumulation of rays of vibration of this intensity by means of a concave mirror, as before, does not alter their intensity, but merely converges and collects their force, and thus increases the effect of producing cold; and this it does, to the very same extent, provided all circumstances be equal, as the effect of producing heat was increased by converging the rays of heat. I have endeavoured to ascertain this by experiment. The temperature of the atmosphere was 60°. Two mirrors were properly opposed to each other; in the focus of one was placed a thermometer, in that of the other a cubical canister, one side of which, (namely, that opposed to the mirror), was blackened. The canister was now filled with water at 90°. The effect on the thermometer in time and extent was marked: the canister was then removed, and its place supplied by a similar one containing a saline solution at 30°. The effect on the thermometer was opposite, but equal in time and in degree, to that of the former experiment.

Experiment.

This fact is of an importance not to be easily appreciated; it appears to me to identify heat with vibration.

Effects of Heat.

Effects of heat.

In the former part of my paper I have related some facts, which are not only inexplicable on the theory of repulsive caloric, but which appear to afford some degree of contradiction to it; it will therefore appear, that an explanation of of these facts must necessarily proceed on some other principle.

Attraction and repulsion.

It is certain, that two energetic, but opposite powers, are constantly active, in all the operations of chemistry; these are attraction and repulsion. The nature however of these powers is still very uncertain: the effects of heat are all referrible to changes in their condition; but from our ignorance of their nature, it must be extremely difficult, to ascertain with precision the cause and nature of the changes they undergo.

Accounted for by polarity of particles.

The phenomena which I have mentioned as contradictory to the theory of repulsive caloric have been ascribed to the agency of a certain polarity in the particles of the body; by means of this polarity the particles are opposed to each other in a particular manner; and the state of attraction and repulsion is influenced or regulated by this state of apposition. The change in temperature is the cause of the change in the apposition of the particles; and this change of apposition of the particles proves the cause of the change in the state of attraction and repulsion, and consequently of the bulk of the body.

Change of bulk from temperature.

This explanation is probably correct; and if it apply in one instance of changes produced by temperature, why not in all? The greatest density of water seems to be about the temperature of 38°. If its temperature suffer any change from this point, expansion occurs; and for any given number of degrees above or below this temperature, the expansion is the same, if the water retain the fluid force. Here therefore, the effects are precisely similar, but, according to the theory, they are ascribed to *causes* that are different; which in itself appears to me contrary to the true laws of philosophising. This opinion therefore, and the objections which I have mentioned to the usual explanation, have induced me to refer the changes of bulk from temperature, in every case, to the same cause; whatever the cause may be.

Circumstances in favour of the hypothesis of polarity.

There are many circumstances, which tend to corroborate the idea of polarity. The appearances of crystallization appear to depend on its agency. The state of fluidity of bodies must also be referred to the "particular situation" of their particles.

particles. And I can conceive, that the agency of attraction, whether of aggregation or of composition, may in every case be influenced or regulated by the particular state of apposition of particles.

It will readily be acknowledged, that much difficulty and uncertainty still exist in this question; but I conceive, that the difficulties are incomparably greater in relation to the theory of repulsive caloric, than in the view of the subject, which has been given. If we could explain the cause of the expansion of water, cooled from 40° to 10°, we should probably find little difficulty in understanding the similar and precisely equal expansion, when the same water is raised in its temperature from 40° to 70°.

Expansion of water.

Capacity for Caloric.

Capacity for heat.

It would be extraordinary indeed, if all bodies were equally susceptible of vibration; no property of matter is equally possessed by all the innumerable substances, which nature presents to our attention; gravity, hardness, elasticity, &c. are possessed in an equal degree by no two bodies with which we are acquainted: such is the diversity in Nature's works! Nor are all bodies equally susceptible of change in the state of their vibration. This proposition is sufficient to account for the variety in the capacity of different bodies, and of the same body under different forms, for heat. Mercury is more susceptible of vibration than water; solids than fluids; fluids than gasses: the quantities, for comparison, being ascertained by weight*.

Mercury and water.

Let mercury at 40° be mixed with an equal weight of water at 80°; mercury is more susceptible of change in the state of its vibration than water, and will consequently suffer more change; its intensity of vibration will pass more nearly to that of the water, than the intensity of vibration in the latter will to that of the mercury: the resulting temperature will therefore be above the mean; i. e. more nearly that of the water than the mean. If the experiment be reversed, the effect will also be reversed.

Change in capacity.

If during the time of the change in the susceptibility of any body for vibration (this change being to diminish its

* In speaking formerly of the high susceptibility of air for change in vibration its quantity was considered by bulk, not by weight.

susceptibility

susceptibility) heat be communicated, its temperature may remain unaltered; a greater power, or longer application of vibration, being now necessary to occasion a temperature, which, before the susceptibility for vibration was diminished, was produced by a power much smaller, or an application much shorter. Hence steam is no higher in temperature than boiling water. If, during this change of susceptibility for vibration, no farther application of heat be made, it follows, that the temperature must fall: hence arise the effects of freezing mixtures.

Steam.

Freezing mixtures.

It scarcely need be added, that the converse of all this will take place, if the susceptibility for vibration be increased, and no abstraction of heat be made. The temperature then must rise; for the body contains within itself what may be termed the power of vibration; a given quantity of which produces a greater intensity of vibration in any body, according to the susceptibility of that body for vibration.

Converse of this.

Such is an imperfect sketch of the hypothesis of vibration, which I proposed to give. Many circumstances, which would have elucidated, and perhaps have confirmed the opinions, have been necessarily omitted; and here the greatest candour of your readers will be constantly required.

It may be useful in concluding, to present a summary of the circumstances which have been considered; and thus to institute a comparison between the two hypotheses.

Summary.

1st, The firft principles of each opinion are equally hypothetical.

The two hypotheses compared.

2dly, The production of heat by friction is explained by the hypothesis we propose; but not, satisfactorily at least, by the other.

3dly, Certain facts have been related, under the head of the effects of heat, which appear to afford fome degree of contradiction to the hypothesis of material caloric; and although they may not be easily explained on the opposite principle, yet they do not by any means appear contradictory to it.

The advantages of our theory appear most conspicuous in the following particulars; for

4thly, The properties of good conductors, and of good radiators of caloric, are explained by it alone.

5thly,

5thly, The same observation applies to the difference of solar and culinary heat;

6thly, And in particular to the radiation of cold.

7thly, The opinion of capacity for caloric is hypothetical; that of the difference in susceptibility for vibration is in conformity to the usual order of nature, in dispensing the other properties of matter.

I remain, Sir,
Your very obedient,
MARSHALL HALL.

Edinburgh, June 8th, 1811.

IV.

On a Combination of Oximuriatic Gas and Oxigen Gas. By HUMPHRY DAVY, *Esq. LL. D. Sec. R. S. Prof. Chem. R. I.**

Compound of oxigen and oximuriatic gas.

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

Oximuriatic gas differs when differently prepared.

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

Its properties when procured by means of manganese.

The oximuriatic gas prepared from manganese, either by mixing it with a muriate and acting upon it by sulphuric acid, or by mixing it with muriatic acid, is, when the oxide of manganese is pure, and whether collected over water or mercury, uniform in its properties; its colour is a pale yellowish green; water takes up about twice its volume, and scarcely gains any colour; the metals burn in it readily; it

* Phil. Trans. for 1811, p. 155.

combines

combines with hidrogen without any deposition of moisture: it does not act on nitrous gas, or muriatic acid, or carbonic oxide, or sulphureous gasses, when they have been carefully dried. It is the substance which I employed in all the experiments on the combinations of oximuriatic gas described in my last two papers.

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

Varies when procured from hyperoximuriates.

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

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

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

It is a compound of oximuriatic gas and oxigen, mixed with some oximuriatic gas. This is proved by the results of its spontaneous explosion. It gives off, in this process, from $\frac{1}{6}$ to $\frac{2}{5}$ its volume of oxigen, loses its vivid colour, and becomes common oximuriatic gas.

A compound.

I attempted to obtain the explosive gas in a pure form, by applying heat to a solution of it in water; but in this case, there was a partial decomposition; and some oxigen

Attempts to obtain it pure.

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

was

was disengaged, and some oximuriatic gas formed. Finding that, in the cases when it was most pure, it scarcely acted upon mercury, I attempted to separate the oximuriatic gas with which it is mixed, by agitation in a tube with this metal; corosive sublimate formed, and an elastic fluid was obtained, which was almost entirely absorbed by $\frac{1}{4}$ of its volume of water.

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

In one set of experiments upon it, a jar of strong glass, containing 40 cubical inches, exploded in my hands, with a loud report, producing light; the vessel was broken, and fragments of it were thrown to a considerable distance.

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

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

50 parts of the detonating gas, by decomposition, expanded so as to become 60 parts, The oxigen, remaining after the absorption of the oximuriatic gas, was about 20 parts. Several other experiments were made, with similar results. So that it may be inferred, that it consists of 2 in volume of oximuriatic gas, and 1 in volume of oxigen; and the oxigen in the gas is condensed to half its volume. Circumstances conformable to the laws of combination of gaseous fluids, so ably illustrated by Mr. Gay-Lussac, and to the theory of definite proportions.

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

The

* In page 245 of the Phil. Trans. for 1810, (Journal, vol. XXVII, p. 333,) I have mentioned, that the specific gravity of oximuriatic gas is between 74 and 75 grains per 100 cubical inches. The gas, that I weighed, was collected over water, and procured from hyperoximuriate of potash, and at that time I conceived, that this elastic fluid did not differ from the oximuriatic gas from mangenese, except in being purer. It

Spec grav. of probably contained some of the new gas; for I find, that the specific gravity

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

When the explosive gas was detonated with hidrogen equal to twice its volume, there was a great absorption, to more than $\frac{1}{3}$, and solution of muriatic acid was formed; when the explosive gas was in excess, oxigen was always expelled, a fact demonstrating the stronger attraction of hidrogen for oximuriatic gas than for oxigen. Attraction of oximuriatic gas for hidrogen.

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

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

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

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

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

Charcoal introduced into it ignited, produced a brilliant flash of light, and burnt with a dull red light, doubtless charcoal,

vity of pure oximuriatic gas from manganese and muriatic acid is to that of common air, as 244 to 100. Taking this estimation, the specific gravity of the new gas will be about 238, and the number representing the proportion in which oximuriatic gas combines, from this estimation, will be rather higher than is stated above. oximuriatic gas.

owing

owing to its action upon the oxigen mixed with the oximuriatic gas.

nitrous gas,

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

muriatic acid gas.

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

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

Why the compound was not before observed.

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

Hyperoximuriatic acid of Mr. Chevenix.

This substance produces the phenomena, which Mr. Chenevix, in his able paper on oximuriatic acid, referred to the hyperoxigenised muriatic acid; and they prove the truth of his ideas respecting the possible existence of a compound of oximuriatic gas and oxigen in a separate state.

Explosions from hyperoximuriates.

The explosions produced in attempts to procure the products of hyperoximuriate of potash by acids are evidently owing to the decomposition of this new and extraordinary substance.

All the facts confirm the simple nature of oximuriatic gas.

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

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

If the power of bodies to burn in oximuriatic gas depended upon the presence of oxigen, they all ought to burn with much more energy in the new compound; but copper, and antimony, and mercury, and arsenic, and iron, and sulphur have

have no action upon it, till it is decomposed; and they act then according to their relative attractions on the oxigen, or on the oximuriatic gas.

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

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

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

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

That oximuriatic gas and oxigen combine and separate from each other with such peculiar phenomena, appears strongly in favour of the idea of their being distinct, though analogous species of matter. It is certainly possible to defend the hypothesis, that oximuriatic gas consists of oxigen united to an unknown basis; but it would be possible likewise to defend the speculation, that it contains hidrogen. Oximuriatic gas apparently simple, and of the same nature with oxigen.

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

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

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

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

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

V.

Description of a new Thrashing Machine, invented by H. P. Lee, *Esq. of Maidenhead Thicket**.

SIR,

Inducements to the invention. I BEG leave to state to the Society of Arts &c. the following particulars, relative to my attempts to improve the thrashing machine for corn, and of my success therein.

Being largely concerned in agriculture, and having 800 acres of arable land, I found, that a thrashing machine or two became absolutely necessary for the continuance of my occupations. I accordingly erected one of the kind recommended to me; but from the complication of its structure, its being frequently out of order, and from its bad performance of the work at all times, I resolved to try to have a thrashing machine made under my own directions, more simple in its construction, and more efficacious in its operations. Its success. With this view I have continued my experiments

* Trans. of the Soc. of Arts, Vol. XXVIII, p. 25. The premium of the gold medal, offered by the Society, was adjudged to Mr. Lee, for this machine.

for

for nearly three years, at an expense of about three hundred pounds, and have, at last, brought my machine to a degree of perfection, which is satisfactory. Many gentlemen and farmers, who have seen it and its operations, give it a decided preference to any they have seen, for the simplicity of its construction, for the cleanness of its thrashing, and for the quantity of corn thrashed by it, in proportion to the power applied.

I have no doubt but that the result of my original thoughts and experiments on this subject will be of great advantage in this highly useful agricultural implement, and I have sent a model of the machine for the Society's inspection.

I am, Sir,

Your very obedient Servant,

Maidenhead Thicket, H. P. LEE.

Dec. 27, 1809.

Testimonies of its excellence.

Certificates from Mr. Edward Green, of Bowlney, in Oxfordshire, and Mr. Thomas Michlem, of Hurley, in Berkshire, stated, that they are largely concerned in the agricultural line; that they have seen a variety of thrashing machines, but give the preference to those on Mr. Lee's principle, for the simplicity of their construction; that they highly approve of the manner, in which they perform their work; and that they consider them as calculated to thrash more corn, in proportion to the power applied, than any other they have seen.

Effect produced by it on barley,

Certificates from William Hubbeard, of Maidenhead Thicket; Thomas Williams, of Feeres Farm, in White Waltham; Joseph Lee, of White Waltham; and Richard Silver, of Maidenham Thicket; testified, that, on the 27th of February, 1810, Mr. Lee's thrashing machine did thrash in one hour and fifty-five minutes, eight quarters and three bushels and a half of barley; that the straw was thrashed clean, and not broken; and the work was in all respects performed in a workmanlike manner.

and on oats.

A certificate from James Willis, foreman to Mr. Lee, stated, that, on the 27th and 28th of February, 1810, he

 did

did thrash thirty quarters of oats with Mr. Lee's machine, at Highway Farm, in the parish of Cookham, Berkshire.

SIR,

After inspecting several Thrashing Machines by a variety of makers, I saw and examined yours, at Highway Farm, and was impressed with its superiority over every other that I had seen, both on account of its simplicity and effect. I applied to Wright, your builder, who has erected one for me upon your improved principle, which effectually thrashes wheat and barley clean, without injuring the straw, and very much to my satisfaction. I have not hitherto had an opportunity of ascertaining its powers with other grain, but am happy to assure you, that I consider your improvements to constitute a material step toward perfecting an instrument of the first consequence to the agricultural interests of this kingdom, and highly deserving our warmest acknowledgments.

Thrashes wheat and barley clean without injury to the straw.

I have the honour to subscribe myself, Sir,

Your obliged humble Servant,

SAMUEL NICHOLLS, M. D.

Hinton House, Twyford, Berks,
March 1, 1810.

Farther testimonies.

A certificate from Mr. G. H. Crutchley, of Sunning Hill Park, Berks, dated March 3, 1810, stated, that he had seen Mr. Lee's thrashing machine at work; that it thrashed clean, and pleased him so well, that he had ordered one on the same principle.

By subsequent letters received from Dr. Nicholls and Mr. G. H. Crutchley, the above certificates were confirmed by them, with additional testimonies in favour of Mr. Lee's machines.

Account of the machine and its working.

Mr. Lee, in his attendance on the Committee appointed by the Society for the examination of the merits of his machine, stated, That his machine requires no rollers for entering the corn to be thrashed.

That

That it is about three feet diameter, and about two feet six inches in length.

That two horses are quite sufficient to work it; that from half past seven to two o'clock they will, without fatigue, thrash two loads of wheat, each of forty bushels.

That he thinks the straw is not so much broken as with other machines.

That the vanes within the cylinder turn from one hundred to one hundred and twenty times round for one round of the horses, in a space of twenty-two feet diameter.

That there are four vanes within the drum or cylinder, each vane one inch and a half thick, and enclosed to within about three inches of their exterior edges; that the drum or cylinder, within which the vanes turn, is close-fluted with wood of about an inch thick, and is in movable parts, so as to admit of being placed nearer to, or farther from, the vanes, as the corn to be thrashed may require.

That he has erected two of these machines on his estate, and has used them for three years.

A note sent to the Society by William Wright, of Henley upon Thames, Oxfordshire, the maker, states, that the price of a thrashing machine on this principle, including the horse-wheel, is forty-eight pounds, at his manufactory there. Price.

Reference to the Engraving of Mr. LEE'S *Thrashing Machine, Pl.* VII, *Fig.* 4 *and* 5.

Fig. 4 and 5 are a side and end-view of the machine; A, in both figures, represents the framing of the machine; B is the shaft of a cog-wheel C, which is turned by cog-wheels, from the great horse-wheel, in the same manner as the ordinary thrashing mill; the cog-wheel C turns a small pinion D, to which it gives a rapid revolution; on the axis of the pinion, the beaters EE are fixed, and revolve with it, within a segment or drum, formed of iron plates, grooved or ribbed, parallel to the axis, as the figure represents, and connected together by wooden curbs FF, to which they are screwed. *a a* is the feeding board upon which the corn is placed to enter the machine. The end of this board is fixed very near to the four vanes, or beaters, *b b b b*; as these revolve rapidly they strike the heads of the corn upwards, with such a jerk Explanation of the plate.

as

as to beat out all the corn from those ears which they meet fairly; but if any escape they are drawn in, together with the straw, and rubbed round by the beaters against the inside of the ribbed drum, or cylinder, F, so as to open the ears and let out the corn, though the ears come in any position whatever. At H is a grating, upon which the beaters deliver the corn, chaff, and straw all together; the two former fall through upon the ground at X, and the latter slides down on the grate; the corn is afterward to be dressed in a winnowing machine, which separates the light and heavy corn from the chaff. The curbs F are fixed by screws, which can be adjusted so as to bring the cylinder nearer, or farther from, the beaters, to adapt the machine for thrashing different kinds of grain; for it is evident, that large corn, as pease, beans, &c., must require more space to rub them in than the smaller grain, as wheat and barley. L, fig. 4, is one of the uprights of the frame which supports the bearing for the axis B of the cog-wheel; and M is an oblique brace, which strengthens the frame. N is the stage on which the man who feeds the machine stands.

VI.

Account of a Substitute for Hemp, prepared from Bean Stalks. By the Rev. JAMES HALL, *of Chesnut Walk, Walthamstow**.

Fibres in the stalk of the bean

THOUGH it has not been attended to, or, so far as I know, ever been mentioned by any one, yet it is certain, that, according to its size, every bean plant contains from 20 to 35 filaments, or fibres, running up on the outside, under a thin membrane, from the root to the very top all around, the one at each of the four corners being *rather thicker*, and stronger than the rest. It is also certain, that, next to Chinese, or sea-grass, in other words, the material with which hooks are sometimes fixed to the end of fishing lines, the filaments, or hempen particles of the bean plant,

exceedingly strong.

* Trans of the Soc. of Arts, vol. XXVII, p. 57. The silver medal was voted to Mr. Hall.

are

Method of separating them.

are among the strongest yet discovered. These, with a little beating, rubbing, and shaking, are easily separated from the strawy part, when the plant has been steeped 10 or 12 days in water: or is damp, and in a state approaching to fermentation, or what is commonly called rotting. Washing and pulling it through hackles, or iron combs, first coarse, and then finer, is necessary to the dressing of bean-hemp; and so far as I have yet discovered, the easiest way of separating the filaments from the thin membrane that surrounds them.

An acre yields about 2 cwt.

From carefully observing the medium number of bean-plants in a square yard, in a variety of fields on both sides the Tweed, as well as in Ireland, and multiplying them by 4840, the number of square yards in an acre, and then weighing the hemp, or filaments of a certain number of these stalks, I find, that there are at a medium about 2cwt. of hemp, or these filaments, in every acre, admirably calculated for being converted into a thousand articles, where strength and durability is of importance, as well as, with a little preparation, into paper of all kinds; even that of the most delicate texture.

About 20000 tuns might be procured annually in the United Kingdom.

Now since there are, at least 200000 acres of ticks, horse, and other beans planted in Great Britain and Ireland; and since, where there is not machinery for the purpose, the poor, both young and old, females as well as males, belonging to each of the 9700 parishes in England, &c. where beans are raised; might (hemp having risen of late from 60 to 120 pounds per tun), be advantageously employed in peeling, or otherwise separating these filaments from the strawy part of the plant, after the beans have been threshed out; I leave it to the feelings of the Society for the Encouragement of Arts &c. to judge of the importance of the idea held out here, not only to the poor, but to the landholders, and the community at large.

The hemp stands exposure to air,

It is nearly twelve months since, by analyzing its component parts, I discovered hemp in the bean plant. I would have written to you then, Sir, on the subject, and sent a specimen, but that I was trying experiments with other plants, as I am during my leisure hours doing at present; and I wished to ascertain in what degree this species of hemp is liable to injury from different situations, and the changes

changes of the atmosphere. With a view to this, I exposed one parcel, nearly 12 months, to all the varieties of the air within doors, and kept another nearly as long *constantly* under water, and find them not in the least injured. The chief difference I perceive is, that the one kept constantly under water, namely the *whitest* of the specimens sent you, has assumed a rich silky gloss, and a much more agreeable colour than it had before.

and to water with exclusion of air, without injury.

But though this is the case with bean-hemp *after* it is cleaned and dressed, and which, though stiff and hard when dry, is pliable and easily managed when rather damp or wet, it seems otherwise with it *previous* to its being separated from the straw. If bean-straw be kept for years under water, or quite dry, it produces I find hemp as good and fresh as at first. But, if the straw be sometimes wet, and sometimes dry, the filaments or fibres are apt to be injured. The specimen of bean-hemp accompanying this letter, in the form of oakum for caulking ships, having been long exposed to the varieties of the weather, previous to being separated from the straw, is a proof of its being considerably injured. If the straw of the bean was scattered thin on the ground, and exposed to the weather for two or three months, I have uniformly found that the hemp, or fibres, are loosened, and easily separated from the strawy part, without any other process than *merely* beating, rubbing, and shaking them, and perhaps this is the easiest way of obtaining bean-hemp; but then, from being thus exposed, and the fermentation that takes place in the strawy part, which is of a spungy nature, communicating itself to the fibres, or hemp, I find that these are generally less or more injured, though not so much so, in my opinion, as to prevent them from being excellent materials for making paper.

Before it is dressed it is injured by the alternate action of air and moisture, but is still fit for paper.

I have also found, and the importance of the idea will, I hope, be an excuse for mentioning it here, that, though the water, in which bean-straw has been put to steep, in a few days generally acquires a black colour, a blue scum, and a peculiar taste, yet cattle drink it greedily, and seemed fattened by it. But my experiments have hitherto been on too limited a scale to be able, in a satisfactory manner, to ascertain this last circumstance. When the water, in which bean

The water in which it is steeped perhaps rather beneficial than injurious to cattle.

bean straw has been put to steep, becomes fœtid, which I find it is *scarcely* more apt to become than common stagnant water, on being stirred by driving horses or cattle through it, by a stick, or in any other way set in motion, (as is the case with all putrid water, even the ocean itself,) the fetid particles fly off, and the effluvia die away.

When straw is to be steeped for bean hemp, the beans are to be thrashed in a mill: the beans should be put to the mill, not at *right angles*, but on a *parallel*, or nearly so with the rollers, else the straw, particularly if the beans are very dry, is apt to be much cut. If the straw is *not* to be steeped, on putting the beans to be thrashed at right angles, or nearly so, with the rollers of the mill, a certain proportion of the fibres, or hemp, may easily be got from the straw, these being in general not so much cut as the straw; but often found torn off and hanging about it like fine sewing threads. The hemp thus taken off, though its lying under water for months would do it no harm, requires only to be steeped a few minutes, drawn through a hackle, and washed, previous to its being laid up for use. If the hemp or fibres, collected in this way (which is a fine light business for children, and such as are not able for hard work, and which requires no ingenuity,) are intended only for making paper, they require neither steeping nor hacklings, but only to be put into parcels and kept dry till sent off to the manufacturer.

Mode of thrashing the beans.

The straw of beans contains a saccharine juice, and is highly nutritive, perhaps more so than any other; and like clover, the prunings of the vine, the loppings of the fig-tree, &c., produces a *rich* infusion, and uncommonly fine table-beer, as well as an *excellent* spirit by distillation. It is the hemp, or fibres, that prevents cattle from eating it. These, like hairs in human food, make cattle dislike it. The collecting of it therefore should never be neglected, nor the boys and girls in workhouses and other places be permitted to be idle, while business of this kind would evidently tend both to their own and their employers' advantage.

Bean straw nutritive, and capable of producing a fermentable liquor.

It is a fact, that about the generality of mills for beating and dressing hemp and flax, a large proportion, in some inland places both of Great Britain and Ireland amounting nearly

Refuse of hemp and flax a valuable material for paper.

nearly to one half of what is carried thither, is either left there to rot, under the name of refuse, or thrown away as of no use, because too rough and short for being spun and converted into cloth. Now, from the experiments I have tried, and caused to be tried, I have uniformly found, that, though too rough and short for being converted into cloth, even of the coarsest kind, the refuse of hemp and flax, on being beat and shaken, so as to separate the strawy from the stringy particles, which can be done in a few minutes by a mill or hand-labour, as is most convenient, becomes thereby as soft and pliable, and as useful for making-paper, as the longest, and what is reckoned the most valuable part of the plant, after it has been converted into cloth and worn for years.

May be made very white.

In its natural state, it is true, the refuse of hemp and flax is generally of a brown and somewhat dark colour. But what of that? By the application of muriatic acid, oil of vitriol, or other cheap ingredient, well known to the chemists, as well as to every bleacher, such refuse, without being *in the least* injured for making paper, can, in a few hours, if necessary, be made as white as the finest cambric.

Number of newspapers published in London.

There are, at a medium, published in London, every morning, 16000 newspapers, and every evening about 14000. Of those published every other day there are about 10000. The Sunday's newspapers amount to about 25000; and there are *nearly* 20000 other weekly papers, making in all the enormous sum of 245000 per week. At a medium 20 newspapers are equal to one pound—hence the whole amount to about 3 tuns per week, or 260 tuns per annum. But though this, perhaps, is not one half of the paper expended in London on periodical publications, and what may be called fugacious literature; and not one fourth part of what is otherwise consumed in printing-houses in the country at large; yet there are materials enough in the refuse of the hemp and flax raised in Britain and Ireland for all this and much more.

These consume 260 tuns of paper annually.

Hopbines contain hemp.

Nor is this all, for as the bine or straw of hops, a circumstance well known to the Society, contains an excellent hemp for making many articles, so also will it prove a most excellent material for making all kinds of paper. And it is a fact, a fact, that, were even the one half of the bine of hops raised in the counties of Kent, Sussex, and Worcester, instead of being thrown away, or burnt, after the hops are picked, as is commonly done, steeped for ten or twelve days in water, and beaten in the same way as is done with hemp and flax, independent of what might be got from bean-hemp, and a variety of articles well-known to the Society, there would be found annually materials enough for three times the quantity of paper used in the British dominions.

I have the honour to be,
with much respect,
Sir,
Your most humble servant,
JAMES HALL.

Streatham, Jan. 9,
1809.

Certificates of the Truth of the foregoing Statement.

Testimonies of the use of hemp from beanhaulm.

We, the undersigned, do hereby certify, that the specimens of hemp enclosed and sealed up by us, addressed to Dr. Taylor, Secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce, Adelphi, Strand, are the produce of common bean straw:—That we never saw or heard of bean hemp till lately; when the Rev. James Hall, who resides here at present, was trying experiments respecting it at Mr. Adams's farm, Mount Nod, and other parts of this parish:—That, in the present obstructed state of commerce with the Continent, it appears to us the discovery of bean hemp may be extremely useful to the manufacture of canvas, ropes, paper, &c.;—And that, as it affords a new and important prospect of employment for the poor, we think Mr. Hall, the discoverer, is deserving of the approbation of the public. We shall only add, that as the Society for the Encouragement of Arts, Manufactures, and Commerce, have contributed so often in a high degree to the exertion of genius, the improvement of the arts, and the public good, we have no doubt but they will not only take the proper steps to prosecute the discovery and encourage the manufacture of bean hemp, but also, by some mark of their favour, show their approbation of Mr. Hall's merit in the

the discovery he has made, as well as of his high public spirit and liberality in communicating the discovery to the public without reserve.

WILLIAM ADAMS, Mount Nod.
EDWARD BULLOCK, Curate.
Streatham, Surry, WM. GARDNER, Surgeon.
Jan. 9, 1809.

These are to certify to the Secretary of the Society for the Encouragement of Arts &c., London, and all whom it may concern, that having seen (at first to our astonishment) the Rev. James Hall, who has resided here for some time past, procuring hemp from common bean straw, steeped some days in water, we steeped some also, and easily got hemp from it; there being no mystery in the matter more than *merely* steeping the straw, peeling off the hemp, and then washing and cleaning it, by pulling it through a hackle or comb.

It answers extremely well for sewing shoes.

These are also to certify, that having tried bean hemp, and found it to take both wax and resin, we have sewed with it, and find the fibres of which it consists in general so strong, that the leather never failed to give way sooner than the seam. We have only to add, that as hemp has of late become uncommonly dear, while much of it is bad, we anxiously wish the prosecution of the discovery, and the appearance of bean hemp in the market; and shall, so soon as we hear of its being spun and on sale, be among the first to purchase and use it.

JOHN HOUNE, Shoemaker.
THOMAS ALFORD, Shoemaker.

Letter from Mr. HUME, *of Long Acre, to the Reverend James Hall.*

SIR,

It bears bleaching extremely well.

I enclose a specimen of the bean filaments, or thread, which have been submitted to the bleaching process. The texture and strength seem not in the least to have been impaired, but retain the primitive tenacity; and I am persuaded this substance will prove an excellent substitute for hemp and flax, for the manufacture of various kinds of paper,

per, cordage, and other materials. I did not find more difficulty in accomplishing the bleaching of this than in other vegetables which I have occasionally tried, and I believe this article is susceptible of a still greater degree of whiteness.

I remain, Sir,

Your very obedient servant,

Long Acre, Feb. 24, 1807. JOS. HUME.

Letter from Mr. H. Davy to the Rev. James Hall.

SIR,

I shall enclose in this paper a small quantity of the bean fibre, rendered as white as possible by chemical means.

It seems to bear bleaching very well, and, as to chemical properties, differs very little from hemp.

The question, whether it is likely to be of useful application, is a *mechanical* one, and must be solved by experiments on its comparative strength.

I am, Sir,

Your obedient humble servant,

H. DAVY.

VII.

A Chemical Analysis of Sodalite, a new Mineral from Greenland. By THOMAS THOMSON, *M. D. F. R. S. E., Fellow of the Imperial Chirurgo-Medical Academy of Petersburgh* *.

THE mineral, to which I have given the name of *sodalite*, was also put into my hands by Mr. Allan †. In the Greenland collection which he purchased, there were several specimens of a rock, obviously primitive. In the composition of these the substance of which I am about to treat formed a constituent,

Sodalite, a new mineral, in the composition of a primitive rock.

* From the Transactions of the Royal Society of Edinburgh.

† See p. 47.

constituent, and, at first appearance, was taken for felspar, to which it bears a very striking resemblance.

Composition of this rock.

This rock is composed of no less than five different fossils, namely, garnet, hornblende, augite, and two others, which form the paste of the mass. These are evidently different minerals; but in some specimens are so intimately blended, that it required the skill of Count Bournon to make the discrimination, and ascertain their real nature. Even this distinguished mineralogist was at first deceived by the external aspect, and considered the paste as common lamellated felspar, of a greenish colour. But a peculiarity, which presented itself to Mr. Allan in one of the minerals, induced him to call the attention of Count Bournon more particularly to its construction.

Crystals of sahlite,

On a closer examination of the mineral, Mr. de Bournon found, that some small fragments, which he had detached, presented rectangular prisms, terminated by planes, measuring, with the sides of the prism, 110° and 70° or nearly so,—a form which belongs to a rare mineral, known by the name of sahlite, from Sweden. He farther observed, intermixed along with this, another mineral; and after some trouble, succeeded in detaching a mass, presenting a regular rhomboidal dodecahedron. It was to this form that Mr. Allan had previously requested his attention.

and of another mineral

resembling the Swedish natrolite of Dr. Wollaston.

Some time before this investigation, Mr. de Bournon had examined a mineral from Sweden, of a lamellated structure, and a greenish colour, which, he found, indicated the same form. From this circumstance, together with some external resemblance, which struck him, he was induced to conclude, that our mineral was a variety of that substance.

To that substance the name of Swedish *natrolite* had been given, in consequence of the investigation of Dr. Wollaston, who found that it contained a large proportion of soda.

Natrolite of Klaproth very different.

There are few minerals, however, that are so totally distinct in their external characters as the natrolite of Klaproth, and the substance we are now treating of. The mineral examined by Klaproth occurs at Roegan *, on the Lake of Constance, in porphyry-slate, coating the sides of veins and cavi-

* It has been observed also by Professor Jameson, in the flœtz trap rocks behind Burntisland.

ties

ties in a mamellated form, the texture of which is compact, fibrous, and radiated; the colour pale yellow, in some places passing into white, and marked with brown zones. Hitherto it had never been found in a state sufficiently perfect to afford any indications of form. Lately, however, Mr. de Bournon was so fortunate as to procure some of it, presenting very delicate needleform crystals, which, by means of a strong magnifier, he was able to ascertain presented flat rectangular prisms, terminated by planes, which, he thought, might form angles of 60° and 120 with the sides of the prism. With this neither our mineral nor the Swedish can have any connection, farther than some analogy which may exist in their composition.

Concerning the Swedish mineral I have not been able to obtain much satisfactory information. There is a specimen of it in Mr. Allan's cabinet, which he received directly from Sweden, sent by a gentleman who had just before been in London, and was well acquainted with the collections of that city, from which it is inferred, that the specimen in question is the same as that examined by Count Bournon and Dr. Wollaston.

Fettstein

Werner has lately admitted into his system a new mineral species, which he distinguishes by the name of Fettstein. Of this I have seen two descriptions; one by Haüy, in his Tableau Comparatif, published last year; and another by Count Dunin Borkowski, published in the 69th volume of the Journal de Physique, and translated in Nicholson's Journal, (Vol. XXVI, p. 384). The specimen, called Swedish natrolite, in Mr. Allan's possession, agrees with these descriptions in every particular, excepting that its specific gravity is a little higher. Borkowski states the specific gravity of fettstein at 2·563; Haüy at 2·6138; while I found the specific gravity of Mr. Allan's specimen to be 2·779, and, when in small fragments, to be as high as 2·790. This very near agreement in the properties of the Swedish natrolite with the characters of the fettstein leads me to suppose it the substance, to which Werner has given that name. This opinion is strengthened, by a fact mentioned by Haüy, that fettstein had been at first considered as a variety of wernerite. For the specimen sent to Mr. Allan, under the name of compact

apparently the same with the Swedish natrolite.

wernerite,

wernerite, is obviously the very same with the supposed natrolite of Sweden. Now, if this identity be admitted, it will follow, that our mineral constitutes a species apart. It bears, indeed, a considerable resemblance to it; but neither the crystalline form, nor the constituents of fettstein, as stated by Haüy, are similar to those of the mineral to which I have given the name of sodalite. The constituents of fettstein, as ascertained by Vauquelin, are as follows:

Constituents of fettstein.

Silica	44·00
Alumina	34·00
Oxide of iron	4·00
Lime	0·12
Potash and soda	16·50
Loss	1·38
	100·00

Description of sodalite.

Sodalite, as has been already mentioned, occurs in a primitive rock, mixed with sahlite, augite *, hornblende, and garnet †.

It occurs massive; and crystallised, in rhomboidal dodecahedrons, which, in some cases, are lengthened, forming six-sided prisms, terminated by trihedral pyramids.

Its colour is intermediate between celandine and mountain green, varying in intensity in different specimens. In some cases it seems intimately mixed with particles of sahlite, which doubtless modify the colour.

External lustre glimmering, internal shining, in one direction vitreous, in another resinous.

Fracture foliated, with at least a double cleavage; cross fracture conchoidal.

Fragments indeterminate; usually sharp-edged.

Translucent.

Hardness equal to that of felspar. Iron scratches it with difficulty.

* This situation of the augite deserves attention. Hitherto it has been, with a few exceptions, found only in flœtz trap rocks.

† The particular colour and appearance of this garnet shows, that the rock came from Greenland: for similar garnet has never been observed, except in specimens from Greenland.

Brittle.

Brittle.

Easily frangible.

Specific gravity, at the temperature of 60°, 2·378. The specimen was not absolutely free from sahlite.

When heated to redness, does not decrepitate, nor fall to powder, but becomes dark gray, and assumes very nearly the appearance of the Swedish natrolite of Mr. Allan, which I consider as fettstein. If any particles of sahlite be mixed with it, they become very conspicuous, by acquiring a white colour, and the opacity and appearance of chalk. The loss of weight was 2·1 per cent. I was not able to melt it before the blow-pipe.

1. A hundred grains of the mineral, reduced to a fine powder, were mixed with 200 grains of pure soda, and exposed for an hour to a strong red heat, in a platinum crucible. The mixture melted, and assumed, when cold, a beautiful grass-green colour. When softened with water, the portion adhering to the sides of the crucible acquired a fine brownish-yellow. Nitric acid being poured upon it, a complete solution was obtained. Chemical analysis.

2. Suspecting, from the appearance which the fused mass assumed, that it might contain chromium, I neutralised the solution, as nearly as possible, with ammonia, and then poured into it a recently prepared nitrate of mercury. A white precipitate fell, which being dried, and exposed to a heat rather under redness, was all dissipated, except a small portion of gray matter, not weighing quite 0·1 grain. This matter was insoluble in acids, but became white. With potash it fused into a colourless glass. Hence I consider it as silica. This experiment shows, that no chromium was present. I was at a loss to account for the precipitate thrown down by the nitrate of mercury. But Mr. Allan having shown me a letter from Ekeberg, in which he mentions, that he had detected muriatic acid in sodalite, it was easy to see that the white precipitate was calomel. The white powder weighed 26 grains, indicating, according to the analysis of Chenevix, about three grains of muriatic acid. Silex. Muriatic acid.

3. The solution, thus freed from muriatic acid, being concentrated by evaporation, gelatinised. It was evaporated nearly to dryness; the dry mass digested in hot water aci- Silex.

dulated with nitric acid, and poured upon the filter. The powder retained upon the filter was washed, dried, and heated to redness. It weighed 37·2 grains, and was silica.

4. The liquor which had passed through the filter was supersaturated with carbonate of potash, and the copious white precipitate which fell collected by the filter, and boiled while yet moist in potash-lie. The bulk diminished greatly, and the undissolved portion assumed a black colour, owing to some oxide of mercury with which it was contaminated.

5. The potash-lie being passed through the filter, to free it from the undissolved matter, was mixed with a sufficient quantity of sal-ammoniac. A copious white precipitate fell, which being collected, washed, dried, and heated to redness, weighed 27·7 grains. This powder, being digested in sulphuric acid, dissolved, except 0·22 of a grain of silica. Sulphate of potash being added, and the solution set aside, it yielded
Alumine. alum crystals to the very last drop. Hence the 27·48 grains of dissolved powder were alumina.

6. The black residue, which the potash-lie had not taken up, was dissolved in diluted sulphuric acid. The solution being evaporated to dryness, and the residue digested in hot
Lime. water, a white soft powder remained, which, heated to redness, weighed 3·6 grains, and was sulphate of lime, equivalent to about 2 grains of lime.

7. The liquid from which the sulphate of lime was separated, being exactly neutralised by ammonia, succinate of
Oxide of iron. ammonia was dropped in; a brownish red precipitate fell, which, being heated to redness in a covered crucible, weighed one grain, and was black oxide of iron.

8. The residual liquor being now examined by different reagents, nothing farther could be precipitated from it.

9. The liquid (No. 4.) from which the alumina, lime, and iron had been ſeparated by carbonate of potash, being boiled for some time, let fall a small quantity of yellow-coloured matter. This matter being digested in diluted sulphuric acid, partly dissolved, with effervescence; but a portion remained undissolved, weighing 1 grain. It was insoluble in acids, and with potash melted into a colourless
Silex. glass. It was therefore ſilica. The sulphuric acid solution being

being evaporated to dryness, left a residue, which possessed the properties of sulphate of lime, and which weighed 1·2 grains, equivalent to about 0·7 of a grain of lime. Lime.

10. The constituents obtained by the preceding analysis being obviously defective, it remained to examine whether the mineral, according to the conjecture of Bournon, contained an alkali. For this purpose, 100 grains of it, reduced to a fine powder, and mixed with 500 grains of nitrate of barytes, were exposed for an hour to a red heat, in a porcelain crucible. The fused mass was softened with water, and treated with muriatic acid. The whole dissolved, except 25 grains of a white powder, which proved on examination to be silica. The muriatic acid solution was mixed with sulphuric acid, evaporated to dryness; the residue, digested in hot water, and filtered, to separate the sulphate of barytes. The liquid was now mixed with an excess of carbonate of ammonia, boiled for an instant or two, and then filtered, to separate the earth and iron precipitated by the ammonia. The liquid was evaporated to dryness, and the dry mass obtained exposed to a red heat in a silver crucible. The residue was dissolved in water, and exposed in the open air to spontaneous evaporation. The whole gradually shot into regular crystals of sulphate of soda. This salt, being exposed to a strong red heat, weighed 50 grains, indicating, according to Berthollet's late analysis, 23·5 grains of pure soda. It deserves to be mentioned, that during this process the silver crucible was acted on, and a small portion of it was afterward found among the sulphate of soda. This portion was separated before the sulphate of soda was weighed.

Analysed in a different way.

Soda.

The preceding analysis gives us the constituents of sodalite as follows:

Constituents of sodalite.

Silica,	38·52
Alumina,	27·48
Lime,	2·70
Oxide of iron,	1·00
Soda,	23·50
Muriatic acid,	3·00
Volatile matter,	2·10
Loss,	1·70
	100·00

Mr.

Analysis by Mr. Ekeberg.

Mr. Allan sent a specimen of this mineral to Mr. Ekeberg, who analysed it in the course of last summer. The constituents which he obtained, as he states them in a letter to Mr. Allan, are as follows:

Silica,	36·
Alumina,	32·
Soda,	25.
Muriatic acid,	6·75
Oxide of iron,	0·25
	100·00

This result does not differ much from mine. The quantity of muriatic acid is much greater than mine. The lime and the volatile matter, which I obtained, escaped his notice altogether. If we were to add them to the alumina; it would make the two analyses almost the fame. No mineral has hitherto been found containing nearly so much *soda* as this. Hence the reason of the name by which I have distinguished it.

VIII.

Account of a Primitive Gypsum. By Mr. Daubuisson, *Mine Engineer**.

Stratum of primitive gypsum.

IN a visit I have just made to the mine of Cogne, I had an opportunity of observing a mineralogical fact, that may be thought not uninteresting, the existence of a stratum of primitive gypsum, intercalated in the mass of the Upper Alps. Mineralogists have hitherto noticed only a single instance of such gypsum, discovered by Mr. Friesleben at the southern foot of St. Gothard in a micaceous schist; and some doubts have been started respecting the period assigned to the formation of this rock. I trust the particulars I shall relate respecting the situation of that at Cogne will evince the existence of really primitive gypsums; accord-

Only one previous instance, and this doubted.

* Journal des Mines, vol. XXII, p. 161.

ingly

ingly I shall begin with a few words on the mineral constitution of the country around.

Southern declivity of the Alps.

The southern declivity of the Alps, from Mount Blanc to Mount Rose, belongs almost wholly to the micaceous schist formation. Here, as in other places, this schist frequently includes strata of primitive limestone, serpentine, chlorite, oxidulated iron, &c. Sometimes it passes into argillaceous schist, as at the col de l'Allée Blanche for instance; but still more frequently into gneiſs and granite.

Mountain near Cogne.

About 15000 met. [9 miles] south of the town of Aoste, and to the east of the village of Cogne, which is at its foot, rises a mountain, that forms part of the chain separating the valley of Cogne from that of Fenis. It is terminated by a sharp ridge at least 700 met. [765 yds.] above the bottom of the valley. Its absolute height appears to me nearly to equal that of the passage of the Great St. Bernard, or 2400 met. [2623 yds.] above the level of the sea. It probably rests on the granite, that shows itself on the surface 2 or 3 kilom, [10 or 15 furl.] to the north. It is composed of micaceous schist, in strata very slightly inclined, so that they may be considered in general as horizontal. In its upper part the micaceous schist becomes loaded with limestone, so that in some little places it ends with being nothing but a white granular limestone containing merely a few spangles of mica. It includes also considerable strata of serpentine, in one of which is the celebrated iron mine of Cogne*.

Stratum of gypsum.

The stratum of gypsum is found 20 met. [22 yds.] below the highest point of the ridge. It is exposed only to the length of 7 or 8 met. and 1 met. thick. Throughout the rest of its extent it is concealed by the numerous fragments of stone, that have fallen from the summit, and cover the sides of the mountain in this part: so that I can say nothing

Rich iron mine.

* This mine, perhaps the richest in the world, exhibits the appearance of an iron quarry, which is worked in open day. The ore is oxidulated iron, in some places pure. It is in very small grains, and sometimes wholly compact. It forms a mass, that appeared to me to be a very short and thick bed. Where it is worked it is more than 25 met. [27 yds] thick.

of

of its length, thickness, or the circumstances of its superposition. However at more than 50 met. beyond the place where it has been laid open for working I have seen indications of its existence. Its thickness cannot be great, for the rock appears in its natural position a few yards below the place where it is worked. The rock at this place, as well as above the stratum, is a micaceous and calcareous schist, of a deep gray, with plane laminæ, traversed by numerous filaments of calcareous spar, and including some veins and nodules of quartz. In getting out the gypsum the workmen have advanced about two yards under the schist, so that this rock forms a projecting roof, under which they work. In this place we see in the most distinct manner, that the schist overlies the gypsum: both are stratified: their strata are perfectly parallel, and dip only a few degrees to the south-east. The strata of gypsum are a few centim. [the cent. is near 4 lines] thick, and frequently separated from each other by a greenish talcy coat.

The gypsum described.

This gypsum is of a fine white colour, with sometimes a slight rosy tinge. Its grain is very fine crystalline, similar to that of the beautiful Carrara marble. It is very translucid, and very soft. If pieces of any size, and exempt from fissures, could be got from the quarry, it would form a very fine alabaster. It is used however for building, and makes good plaster.

Talc contained in it.

It contains a great deal of talc in detached particles, generally lenticular, and varying in size from that of a lentil to that of a walnut. These almost always lie flat, and arranged in lines parallel to each other, and to the stratification. Their colour is a very pleasing green. Sometimes the laminæ of talc are so close together as to produce a kind of steatite; sometimes they are very narrow, resembling fibres, and forming together little masses, exhibiting a pleasing variety of fibrous talc. Pretty frequently these fibres are disseminated in small groupes through the gypsum, are of a delicate light green, and might be taken at first sight for amianthus, of which they have all the appearance. Martial pyrites also is seen in the gypsum, and particularly in the small masses of talc. It is sometimes in rounded grains, sometimes in little striated cubes.

What

What I have said, particularly on the parallel stratification of the gypsum and micaceous schist, as well as on the presence of the talcy or steatitic matter in these two mineral masses, evidently shows, that they are of the same formation, that is, formed at the same period. The nearly horizontal position of the strata from the foot of the mountain to its summit; the identity of the rock, that forms the roof and the wall of the strata of gypsum, &c.; all militate against the idea of a transposition, that might have covered a secondary gypsum, deposited on the mountain subsequent to its formation, with a block of schist. Here the gypsum is really a component part of the mountain; it is one of the courses, that form the building; and it has ever been placed before several others, those that are at the summit. Now the mountain of Cogne itself makes part of that portion of the Alps, *Grandi Alpi* of the Italians, which extends from Mount Blanc to Mount Rose: and it is of the same nature, as we may be satisfied by reading what Saussure has said of that country in his journey to Mount Cervin. Thus we have a gypsum of the same formation with those lofty mountains, which have always been considered as primitive, or anterior to the existence of organized beings, and which every thing still indicates to be so.

Of the same date with the other Alpine rocks.

IX.

Farther Observations on the Fructification of the Firs. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

MY endeavour to contract my subject has made me leave out much respecting the firs, I think of high consequence to them: I shall therefore add this short letter respecting the cones, that I may not be misunderstood.

Seeds of the firs

I have said that the seed of the fir is not impregnated the first year, and this is certainly *true* with respect to the *pines*, and all those *firs*, the male flower or catkin of which so much *precedes*

not impregnated the first year.

precedes the female cone, as to disappear wholly before the pistil is scarcely visible. The Scotch fir will serve as a proper example. The female cone for the present year came out in June, 1811. In May, 1811, all the powder of the stamens had disappeared; besides that the cone shows no seed till full three months after its first coming, of course these seeds could not be impregnated. Next year, May 1812, the cone will show (by many outward signs) that the seed is ready to receive the line of life; the pistils in the cone will be elongated; the drops ready to be saturated with the powder of the stamen as soon as it is fit. The pistil is then in the exact situation in which I drew it in my last letter; and the impregnating and nourishing vessels distended in a manner they never are but at this time. As soon as the drops are saturated, the pistil draws in, and all is complete for the year, except that the cones continue to increase and alter their form by degrees. The following year, March 1813, they will begin to swell about the points, and in a few months to open; and this is the time the cones in England are obliged to be gathered, or they are very apt to shed their seed. Still they are *far too much attached* to their stalks; and are often greatly hurt by this early plucking. I believe it will be acknowledged that *now*, as well as in Evelyn's time, the seed of all pines are *far better* coming from abroad, than that shed in our own country: and the reason is plain: they are able (from the shade either of the mountain or the forest where they grow) or from the northern climate, to remain on the trees *till ripe*; so that they are not gathered till the fourth or fifth season. That is, if appearing in 1811, they are not sent to this country till 1814 or 1815: by which means, the integuments will be completely loosened from the tree, (not torn as ours are), their seed will be perfected, and full of moisture; and if in taking out the seed *some injury* is done to the vessels, it signifies *little*, as the process is completed: but in ours, when the seed is but half formed, to injure the vessels is to stop the completion of the seed, and this is the exact difference between the two sorts when examined: the work of Nature is finished in the foreign seed; in ours it is not perfected. No person, who is not a *dissector*,

Scotch fir.

Foreign seed of pines best.

dissector, *can conceive* the mischief done to such seeds, in thus tearing the chief vessels in the cone.

In the pines it is most easy to know the cone of the year for several years back, as they are always found on the year's shoot to which they belong. By tracing each shoot the tree has made a few years back, this will be found *never to vary*. The first year's cone is *white* and close; the second year's is green and close; the third brown; the fourth brown and open; and each falls back one year's shoot.

Cones of the pine of different years growth described.

With respect to the larch, it is very different; in *habit*, *nature*, and *appearance*, no trees can differ more. The larch gives her fruit in an irregular manner, equally on the old as on the new wood. It has also its female catkin appearing before the male, and so much preceding it, that the seed is ready for impregnation, ere the powder of the stamen is ripe: this is easily known, by dissecting the cone of the *last year*, and comparing it with the present. The distended vessels, which are most observable at the back; the opening of the nourishing vessels, and above all the bubble, if watched for in April or May, prove this early impregnation. I doubt not it is also the case with the cedar of Lebanon, though the cones of *this tree* hang afterwards till the fourth or fifth season, as their appearance testifies. The quantity of tannin (or of that juice which appears to contain it), is excessive, and seems nearly as much as is contained in the bark; for the cone part (when the seeds are taken out), if magnified, shows nothing but bladders of this juice.

Fructification of the larch.

Cedar of Lebanon.

Tannin.

As to the cypresses, and those I have ventured to rank with them, (like the pines), they are too late in the year for impregnation; beside that the seed is not formed, or the cone opened, till late in the autumn.

Cypresses.

Since I have turned my mind to remark the quantity of tannin found in trees, I have observed how much more is found in many, than in the *oak*. In the betula alnus of this country there is certainly a very great quantity, though not so much as in the firs. The men's hands who bark it are always so stained, that they find it very difficult to obliterate it; which is not the case when they strip the oak. After studying the firs, my hands were so stained, I had great trouble to take it from them; and yet the *guarding*

Quantity of tannin in different trees.

wood

wood of the firs is of a beautiful yellow white, till exposed to the air, when it becomes a deep brick red.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

X.

Description of a Screw adjusting Plough, invented by Mr. THOMAS BALLS, *of Saxlingham, near Holt, Norfolk* *.

SIR,

Plough on a new construction.

I HUMBLY offer, for the inspection of the Society, the model of a plough, constructed upon a principle on which I have made several.

Sir Jacob Astley, Bart., has seen two at work on my farm, which I have constantly used, in different kinds of ploughing, for three years, and which, excepting in the share, have not cost me a shilling in repairs. Sir Jacob has ordered one to be made; and he being desirous, that the plough should be more generally known, expressed a wish that I would send a model to the Society. If its mechanical principle proves to be of real utility to agriculturists, and superior to the ploughs in general use, I shall be highly gratified in my endeavours to promote the liberal views of the Society.

I am, Sir,

Your most obedient humble Servant,

Saxlingham, April 5, 1810. THOMAS BALLS.

Certificate from Sir JACOB HENRY ASTLEY, Bart.

DEAR SIR,

Testimony of its utility.

I have seen Mr. Ball's plough worked against the common Norfolk plough, and find it much superior. It laid the furrow much better, more equal, and with much less draught

* Trans. of the Soc. of Arts, Vol. XXVIII, p. 45. The silver medal was voted to Mr. Balls for this invention.

to

to the horses, and has not wanted the usual repairs, which the common ploughs are subject to. I make this observation from having had one in use for more than a year; and I find this plough much approved of by the farmers in this neighbourhood.

I remain, Dear Sir,

Your most obedient Servant,

Milton-Constable, May 3, 1810. J. H. ASTLEY.

SIR,

The enclosed certificates will, I hope, be satisfactory to the Society respecting my plough. It is a material improvement over the wheel-plough in common use in Norfolk, as it works with greater ease to the horses, on account of the line of draught being on a line with the angle of the horse's shoulders. It lays the furrow-slice particularly level, and cuts an even bottom-furrow. It is less liable to wear, on account of having less friction on the ground irons. It is particularly well calculated for breaking up stiff old land, and less liable to be put out of order than any plough generally used. By the adjusting screw, the furrow may be set from one to nine inches in depth, and secured by a lock to any of those intermediate depths with the greatest exactness. It may be easily converted into a swing-plough, by disengaging the axle-tree and wheels. Its beam may be made particularly light, on account of the line of draught lying so near the heel. I beg leave to inform the Society, that the Earl of Thanet, in the year 1807, ordered two of these ploughs, and in 1809 six more of them. Mr. Burroughs, of Weasenham, intends to have all his ploughs on this plan; also Mr. Wall, of Bayfield-lodge; Mr. Cobon, of Leatheringsett, will have two ploughs; and the Rev. T. Munnings has given orders for some to be made.

Advantages of this plough.

Its use adopted by several.

If I had not been so limited in time, I could have sent you many more certificates.

I am, Sir,

Your most obedient humble Servant,

THOMAS BALLS.

Saxlingham, May 6, 1810.

Certificates

Farther testimonials.

Certificates were received from the following persons: viz. Mr. Robert Wright, of Great Snoring, stating, that he has three of Mr. Ball's ploughs, which he conceives to be much superior to the common plough, both in the execution of the work and easiness of draught.

Mr. Mark Barret, farming steward to Sir George Chad, stating that he has three of Mr. Ball's ploughs; that they are the best he has ever made use of, and answer every purpose, both as a swing and wheel-plough.

Mr. Thomas Hurrell, of Saxlingham, stating his opinion, that Mr. Ball's plough will come into extensive use, being an excellent plough for general purposes.

Mr. Henry May Waller, farming steward to Sir Jacob Henry Astley, Bart., stating, that he has two of Mr. Ball's ploughs in constant use; that he thinks them well calculated for strong work; and that they may be converted into a swing-plough, by disengaging the wheels.

Reference to the Drawing of Mr. BALL'S *Plough, Fig.* 1, *Pl.* VIII.

Description of the plough.

A is the beam of the plough carrying the coulter B, share D, and handle E; F is the mould board; the draught of the plough is taken by two iron rods G, connected at one end with a hook *a* in the beam A; and at the other with an iron bridle H by a swivel-bolt; this iron bridle has several notches to receive the draught-chain I, by means of which the point of traction is adjusted sideways; the adjustment for height, and in which the improvement consists, is made by an iron frame K, at the top of which a nut is placed acting upon a screw *d* fixed into the beam A; the axletree *e* of the wheels *f f* is connected with the iron rods G, by a single bolt or pivot projecting from the end of them, which passes through the axletree; by these means the wheels always apply themselves to the inequalities of the ground without influencing the motion of the plough. The nut of the screw *d*, being turned, raises or lowers the iron rods G, and elevates or depresses the point of traction, so that the plough will cut a greater or less depth of furrow.

XI.

XI.

An improved Implement for extirpating Docks and Thistles; by Mr. J. BAKER, *of West-Coker, near Yeovil, in Somersetshire**.

SIR,

I HAVE sent to the Society an implement of my invention for destroying thistles and docks, which are two very injurious weeds to agriculturists. Implement for destroying thistles and docks.

The implement is so contrived, that, if the root breaks in the claw, in attempting to draw it, you may, by turning the instrument, cut the root so far below the turf as to prevent its growth.

I am, Sir,
Your obedient Servant,
JOHN BAKER.

West-Croker, Oct. 31, 1809.

Certificate.

We do hereby testify, that the instrument made by Mr. John Baker for destroying docks and thistles has been used to great advantage, and is likely to come into general use.— Edward Guppy, Nathaniel Bartlett, Thomas Sandford, Edward Penny. Testimonies of its utility.

Description of the Implement.

Fig. 2 of Pl. VIII represents Mr. Baker's thistle-extirpator. A is the handle; B the claws, between which the thistle is received; the curved iron C is the fulcrum, over which the purchase to extract the weed is obtained; D is an iron rod, or bar, upon which the foot is placed to thrust the claws into the ground. In case the root of the weed breaks in endeavouring to extract it, the curved blade E, which has a sharp end like a chissel, is thrust into the ground to cut off the root of the thistle some inches below the surface, and prevent its vegetation. The implement described.

* Trans. of Soc. of Arts, vol. XXVIII, p. 50. The gold medal was voted to Mr. Baker for this invention.

Description

XII.

Description of a Pair of Expanding Harrows, applicable both for cleaning foul Land, and harrowing in Seeds. By Mr. WILLIAM JEFFERY, *of Cotton-End, Northampton**.

SIR,

New invented harrows.

I HAVE sent, for the Society's inspection, a model of a pair of harrows of my own invention, made to a scale of one inch and a half to a foot, and which are allowed to be a great improvement in these implements.

The improvement stated.

The improvement consists in their power of contraction or expansion, so as to cover an extent of land from five feet to ten feet; their teeth may be set at twelve different distances between them, and their tracks will always be at equal distances, according to the state of the land; they will either serve for harrowing in seeds, or cleaning foul land.

For cleaning foul land this harrow far exceeds any other yet made; for in such land the teeth ought to be at a greater distance in the first harrowing, and at the subsequent harrowings to be brought nearer together by degrees, till the teeth are brought very near together by contracting them. One pair of my harrows answer the purposes of three or more pairs made upon the old construction with fixed teeth.

My harrows are so constructed as to be contracted, or expanded, in two or three minutes; and the teeth, which are thirty-four in number, set at any equal distances required, having only two screws to confine them. This implement is more durable than other harrows, as there are no mortices or tenons in them to weaken the wood-work, or admit the rain. They are put together with iron nuts and screws.

They are also easier conveyed from field to field than other harrows, and when not in use will fold up in a small compass. I hope they will meet the Society's approbation, and be rewarded according to their merit.

I remain, Sir,

Your humble Servant,

WILLIAM JEFFERY.

Cotton-End, Northampton,
June 8, 1807.

* Trans. of Soc. of Arts, vol. XXVIII, p. 51. The silver medal was voted to Mr. Jeffery.

On

On the 31st of March, 1810, certificates were received from Mr. John Rice, Cotton-End; Mr. J. Hawkins, Castle-Ashby; and Mr. William Shaw, Hardingstone, to the following effect: viz.—That they had purchased of Mr. William Jeffery, and made perfect trials of his newly-invented expanding harrows, and find them to be upon a much superior principle to any they have seen, or made use of before. Testimonies of their use.

Reference to Mr. W. Jeffery's Expanding Harrows.

Fig. 3, Pl. VIII, represents Mr. Jeffery's expanding harrow. It consists of two sets of movable bars of wood, connected by hooks in one set, and eyes in the other. Each set is composed of four bars of wood, A B C D, furnished with teeth; these are connected, and held parallel to each other by three other bars, or braces, E F G, united to the former by screw bolts; the iron loops H I are the points for the chains, by which they are drawn; K are two iron braces, joined to the bars E at one of their ends, and have a number of holes, any of which can be put over screw-pins fixed upon the middle bar F, provided with nuts; when these nuts are removed, and the iron braces detached from their pins, the frames may be either closed up, or extended, so as to bring the teeth of the harrow nearer together, or remove them farther asunder, and they can be fastened at any point by the different holes in the iron braces, so as to be worked with the teeth at any requisite degree of extent. The harrow described.

XIII.

Observations on an occasional Increase and Decrease of Bulk in the Hair of the Head. In a Letter from Thomas Forster, *Esq.*

To Wm. NICHOLSON, Esq.

SIR,

IT has always appeared to me, that the best means to get at a correct knowledge of any intricate subject is, to excite the Knowledge gained by exciting attention to a subject.

the attention of others toward it; particularly of those who may have more extensive opportunities, as well as more capacity for accurate observation than myself. In conformity with this view of the subject, I request your insertion of the following observations.

Sympathy between the skin and stomach.

The sympathies between the skin and the stomach have been frequently adverted to by physiologists; the skin has been found to be alternately *hot and dry, hot and moist, cold and dry,* and *cold and moist*; and these varieties have been attributed to variations in the state of the stomach, between which and the skin a very direct sympathy is believed to exist. But the variations in the appearances of the hair do not appear to be duly noticed.

Variations in the appearance of the hair.

I have remarked, that people of what is usually called nervous and susceptible constitutions appear at times to have but half the quantity of hair on their heads, that they have at others, though they have assured me none had been cut or combed off.

Causes of apparent increase of quantity.

On minute examination I have found, that the apparent increase in quantity at certain times was occasioned by the following circumstances: the shafts themselves were found to be specifically larger, and more tense or elastic, at the same time that they did not lie in such close contact.

The body of the hair enlarged.

The apparent diminution in quantity, at other times, I found to result from a specific decrease in the size of the shafts, which also lay in closer contact than ordinary, and were more flaccid, and generally more dry. Considering the considerable influence which the atmosphere exercises on our bodies, I was once induced to attribute the *closer contact* of the shafts to a diminution in their *electricity*, by which they would become less *mutually repulsive*; this however does not seem calculated to account for their increase in size.

What is the cause of this?

May the shaft be considered to be organized throughout, and its enlargement to be caused by an increased action of its vessels? or, Is there an aëriform perspiration into the cavity of the shaft, on an increase of which it becomes distended? or may the increased tension and size of the shaft be considered as resulting from the cooperation of these two causes?

Apparently connected

The strength and tension of the hair appears generally to accompany health, while the weakness, close contact, and flaccidity

flaccidity of it denote disorder. I have observed also, that small doses of mercury have changed the appearance of the hair very soon after their administration. From being flaccid, dry, and small, it has become tense, strong, and moister; at the same time more tension and solidity has appeared in the muscles, and the countenance has displayed a more healthy appearance. Now mercury may increase an aëriform perspiration, (if such a one exist) into the shaft; it may also set the digestive organs to rights, thereby cause a more healthy action of the vessels in general, and of those of the shaft among the rest. I cannot help observing, that there is no objection to supposing hairs organized, because we cancannot discover their vessels. On this subject we may, I think, be allowed to reason thus: If all nourishment be performed by the action of vessels, either vascularity must extend itself *ad infinitum*, or there must be certain small vessels not nourished at all. Can we demonstrate those small arteries, which ramify in the coats of and nourish the smallest *vasa vasorum*? Such considerations as these ought to prevent our denying organization to any part of an animal body, even to the cuticle and the enamel of the teeth.

with health and disease. Mercury soon restores the healthy appearance.

Organization probably extends much farther than is generally supposed.

I shall be much obliged to any of your correspondents, who may have noticed any connection between the varieties in the appearance of the hair and any peculiarities in the state of the body, &c., to communicate them in your scientific journal; and I remain, Sir,

Your constant reader,

THOMAS FORSTER.

XIV.

On the Prevention of Damage by Lightning. In a Letter from Mr. B. Cook.

To Mr. NICHOLSON.

MY DEAR SIR,

I HAVE read with much concern almost every week for some time past accounts of some damage of one kind or other done to buildings, trees, and cattle, or in the loss of lives by lightning;

Annual damage done in this country by lightning.

lightning; indeed every year this country suffers very much, either by the destruction of trees, houses, and cattle, and what is far more distressing, the loss of so many lives by the electric fluid. I have endeavoured to form an idea of the loss sustained on an average; and I find upon a moderate calculation, it cannot be far short per annum of 40 to 50 thousand pounds, and the loss of lives from 20 to 30. It is of so serious a nature, that I wonder no effort has been made to remove, if not wholly, at least a part of the evil. Looking at it in this light, and conceiving it to be the duty of every man to endeavour to propose some remedy, I have taken the liberty to hand you what follows for your consideration; if you think it worth inserting in your valuable Journal.

Our kingdom from its high and rocky nature, from its bowels containing such vast masses of iron, copper, and other ores, all conductors of lightning, and also from its situation in the midst of the waves, itself becomes a conductor also; all these circumstances conspire to collect the electric fluid together around us. If it was possible to find out means to carry off this very destructive element without danger, the country would experience a great and invaluable benefit from it. The loss of so many lives is a very serious consideration, and ought to engage the studies of the philosopher and philanthropist to propose some remedy, if only for their sakes, and if it is impossible to remove the evil wholly, at least it is possible to remove it partially.

Plan proposed for preventing it.

The plan I with much deference propose, and I feel satisfaction in proposing it first to you for your consideration, because if you do not approve of it, it will not meet the eye of the world, for no man is more competent to decide upon its merits than yourself. The plan is to erect at different stations conductors throughout the kingdom, at 5 or 6 miles distant, or in some instance nearer, according to the nature of the ground, on the most elevated parts, so that wherever the clouds moved, surcharged with the electric fluid, the conductors would carry it down, so that it would be next to an impossibility for a collection of electric fire to accumulate, so as to produce a destructive discharge. I have very little

little doubt, but that all, or nearly all of the fluid would be carried off by these conductors, and little or no damage, or death would ever be occasioned by the lightning.

The expense of erecting conductors at different stations throughout the kingdom would be saved in a few years, and the safety of men's lives would be of more value than any expense that could be incurred. If every parish would agree throughout the kingdom to appropriate a part of the rate for the ereetion of 4, 6, or more conductors, according to the size of the parish, on the different parts that are most elevated, the expense would not be felt—indeed it would not be worth naming. If the different noblemen, gentlemen, &c., of the different parishes were to take it into consideration, first considering the certain security it would provide for their cattle, buildings, and the lives of themselves and servants; and secondly, when they estimate the very small expense these conductors might be erected for; I do think every parish would instantly be induced to adopt the plan.

Faults of the common iron conductors.

But there are several great imperfections and objections against the present iron conductors.

The first is, the very short time they stand without being deeply corroded with rust, and when first put up the iron is so very irregular on the surface, that it is a great hindrance to the descent of the electric fluid, and calculated in a great measure to cause it to fly off to any other conducting substance in its way or near to it; and when up for a few years it becomes still worse, and so incrusted with rust, that the irregularity and imperfections of the conductor are increased. Another fault is, that the tops of the conductors are not raised high enough above the building they are placed to protect; the point of the rod is in general placed just above the chimney. The rod ought to rise 6 or 8 feet above the top of the house or building, and to end in a single point only. If conductors are used, in every instance the best materials should be used to make them. Iron is the very worst material, and yet all conductors are made of iron; but this arises from the cheapness of the article.

Conducting power of different metals.

According to the experiments of Mr. Henly, (published in Dr. Rees's Cyclopedia, under the article Conductors), to to prove the best conductors, he found the same charge from an electric machine melted 4 inches of gold wire, 6 inches of brass wire, 8 inches of silvered copper, 10 inches of silver, and 10 inches of iron *wire*; so that gold is the best conductor, and iron the worst. Brass stands next to gold in the quality of a conductor. Cavallo says, that copper and brass are the best conductors, and also that they never rust; but to make them of copper or brass would be a very great expense, and then, if not drawn through plates, they would be very uneven on the surface, which is a defect in electric rods.

Iron tubes plated with brass.

I had in prospect the making of conductors on an improved plan, so that they would be equal to solid brass in their use, and come as cheap or cheaper than wrought iron, in a patent I have very recently obtained for combining different sorts of metals, particularly brass or copper, with iron. By this method we can plate or cover tubes of iron 15 or 16 feet long, of any diameter, with a coat of brass, from $\frac{1}{16}$ of an inch, to any thickness; and so connected with the iron, by compression, that, when so combined, it appears a solid piece of brass, but being hollow, is very light and portable, and the method used in making them being by drawing them through a polished draw plate, all the surfaces are as smooth and uniform as it is possible to make them. Being made in convenient lengths, they may be sent to any part of the kingdom, and put up in a very short time, as one piece screws into another, so that, when screwed in, both edges of the brass meet, and join together. Conductors of this kind would never rust, as what is presented to the atmosphere is brass only. They would be $\frac{3}{4}$ lighter than iron rods, would be put up in a very short time, would be quite as cheap as iron, and furthermore would be the best conductors you can possibly make. But as I said before that I had given the subject a good deal of thought, especially the probability of drawing off the electric matter by conductors, so as to prevent its getting to a head and causing by its discharge so many accidents; when I considered the manner of the iron rods and their great defects, it set me a thinking how I could contrive a better conductor than iron, and I flatter myself I have succeeded. Therefore I leave it to every man to judge, whether

whether what I have asserted is true, namely, the great damage done yearly by lightning, and also the great necessity of providing, if it is possible, some remedy; and if conductors are the only means that promise a remedy, those conductors which afford the most beneficial and lasting results will certainly be chosen. *This is, Sir, very much like recommending my own invention*: but if my rods are the best, which I leave to every candid man to judge; and if society is benefitted, I see no reason why I should not be benefitted also. The present conductors on shipboard, where any are used, are I believe constructed of chains, which are the worst of all conductors, as the lightning has to run down the most irregular of surfaces, besides their being so clumsy. But my brass rods might be so attached to the mainmast, and the collecting point raised above the top; and where the joints of the mast are, there might be a round universal joint, that would bend in every direction with the mast. The rod might be carried down thus into the sea, and the expense of them would be so trifling, that one would hardly think any vessel would be without one, especially when it is considered, they would be made of a metal allowed by all who have written on electricity to be the best conductor of lightning.

Conductors for ships.

I am, Dear Sir,

B. COOK.

Annotation. W. N.

The subject of conductors for lightning being still obscure, I have with pleasure inserted Mr. Cook's communication without considering, as at all needful, that an acquiescence in its contents should be implied throughout on my part. Being founded on the generally admitted doctrine, it is in many respects entitled to consideration, and, like all other ingenious researches, is calculated to excite investigation. On the present occasion I would remark, that the course, disposition, and striking places of thunder clouds appear to be governed in a very great measure by certain

Conductors for lightning.

Conductors for lightning.

certain conducting parts lying along or within the earth either as ridges or internal masses, and that the stroke from a stratum of clouds, many miles in length, seems to be determined by an action which extends far beyond the influence of any metallic rod,—even supposing this last to be inserted into the conducting mass itself: that the whole process of atmospheric evaporation and condensation appear to be accompanied with electric phenomena upon a very extended scale, but most strikingly manifest when the changes are rapid; this last being the only difference between thunder storms and common squalls or showers: and that it does not seem probable, that our rods can essentially modify the course of these effects. Other more remote considerations would offer, if they could; such as the possibility of an interruption of the ordinary course and frequency of showers, which Darwin thought within the reach of human power, and the greater probability that the atmospheric electricity of a whole country would soon destroy any series of conductors: but the affair of the poor-house at Heckingham *, in Norfolk, which, about thirty years ago, was struck and set on fire by lightning without touching any one of eight elevated metallic conductors attached to the building, has been considered as a proof of the very limited influence of these rods, and that their power of protecting a single edifice requires the condition, that all the conductors should be connected together, and with the metallic parts of the house.

XV.

Extract of a Letter from Mr. CORDIER, *Mine Engineer, on Mount Mezin* †.

Mount Cenis.

Alternation of gypsum and micaceous schist.

THE passage of Mount Cenis has been laid open to view by the new road. We see there vast strata of gypsum, which alternate with the rocks of micaceous schist, compose nearly a twentieth of the mass of the mountains, and show them-

* See Philos. Trans. of that time.

† Journal des Mines, Vol. XXVI, p. 239.

selves

selves equally in the lowest and in the highest parts of the mountains. Saussure had supposed this gypsum to be superposed, but I easily satisfied myself, that it is in reality intercalated.

Extinct volcanoes in France.

I have revisited almost all the extinct volcanoes in the interior of France. My object was to verify many of my descriptions, and to make new ones, wherever I could find situations truly classical, that is, capable of being cited as exhibiting a complete and perfectly circumscribed geological phenomenon.

Mount Mezin.

I have paid much attention to Mezin, which is a volcanic system analogous to Puy-de-Dôme and Mont-d'Or, but much better characterized. We see there two orders of volcanic substances; those that were anterior to the last period of deluge, and those that have been thrown out subsequent to all the revolutions the Earth has undergone. The mass of the mountains is composed almost wholly of primitive formations. Considered as a whole, it is a frustum of a very obtuse cone of ten leagues radius. I find, with Mr. Ramond, that it is 1774 met. [1939 yds] above the level of the sea, and about 800 met. [874 yds] above the granitic flat on which it rests. It exhibits the ruins of a volcanic colossus, unquestionably much loftier and more extensive. We find in it this very remarkable peculiarity, that most of the incoherent matters thrown out have undergone no alteration, and have not been changed either into tufs or breccias. The red scoriæ in fragments, and the black stony scoriæ, appear with all the characters impressed on them by the fire. Add to this, all the currents, or segments of currents, are accompanied with scorified crusts above and below. The interior of these currents presents only lithoid lavas, from the basaltic porphyry to the compact earthy, or fine-grained granular porphyry with base of feldspar. The three varieties with base of feldspar are frequently found in the same current, and thus exhibit the transition of the three pretended species, domit, the base of graystone, and clinkstone.

Two orders of volcanic substances.

The mountain described.

The volcanic substances unchanged.

Transition.

The later lavas.

The modern lavas are not very numerous at Mezin. They are all formed of porphyritic basaltes with fine crystals of peridot, and pyroxene, mixed with nodules of granular peridot,

peridot. The same nodules and the same crystals are found in the scoriæ that compose the craters, whence these lavas issued. The modern currents having almost all flowed through narrow and deep valleys, the torrents have resumed their beds, by hollowing out vast furrows in the lava. Hence result sections admirable for their height, which sometimes reaches to 200 French feet; for the regularity and dimensions of the basaltic columns; or for their extent, as they frequently reach whole leagues. These superb curtains are ornamented with scoriæ at top and bottom. The decomposition of the lower scoriæ gives rise in certain places to a curious phenomenon. The tuf, or wacke, resulting from it, mixes with the river-mud or sand, which the lava had covered, and these places exhibit a transition of the sort that Werner admits: that of sand, or clay, to basaltes! The modern basaltic columns of Mezin are unquestionably the finest ever yet observed.

Transition.

New kind of granite.

The whole system of Mezin rests on a new kind of granite, into which pinit enters in the proportion of a twentieth, a tenth, and even a third. This rock occupies a space of more than 250 square leagues, and extends to what was formerly Forêt, where it serves as a matrix to the substance that was taken for emerald, but is only a translucid pinit. Of this I satisfied myself on the spot.

SCIENTIFIC NEWS.

Report of the Proceedings of the Mathematical and Physical Class of the French Institute.

(Concluded from p. 240.)

Respiration of fishes.

SINCE Mr. von Humboldt's return to France, he has made many experiments on the respiration of fishes, in concert with Mr. Provençal. Spallanzani and Sylvestre had shown, that fishes do not breathe by decomposing water, as some had supposed, but by water obstructing the oxigen dissolved in it, or by coming to the surface to collect oxi-

gen

gen directly from the atmosphere. The experiments of Messrs. von H. and P. have gone farther. Seven tenches were placed under a jar filled with river water, containing 4000 cent. cub. [243·6 cub. inch.]. After living in it eight hours and a half, the analysis of the air still found in the water showed, that the fishes had absorbed in this time 145·4 [8·85 cub. in.] of oxigen, and 57·6 [3·5] of nitrogen, and that 132 [8] of carbonic acid had been produced.

Effects of different gasses on them.

In water deprived of air the fishes were uneasy, and in about twenty minutes fell motionless to the bottom. In pure oxigen they appeared to respire eagerly, and spread their gills more. In nitrogen and hidrogen they kept their gills closed, seemed to dread the contact of these gasses, and died soon after they were put into the water containing them. Carbonic acid too kills them in a few minutes. But it is not by their gills alone that fishes absorb oxigen and nitrogen, the whole surface of their bodies has the faculty of acting on these gasses. After the fishes were taken out of the water containing the deleterious gasses, a small portion of carbonic acid was found in it, exhaled probably from their bodies.

Respiration of animals after the 8th pair of nerves was divided.

Mr. Provençal has also made some experiments on the respiration of mammalia after the eighth pair of nerves had been divided. The animals gradually absorbed less oxigen, and produced less carbonic acid, after the operation. At first their respiration was not apparently weakened; but it soon became feeble; and at length ceased altogether: probably from the cessation of the mechanical functions of the thorax. The heat of the animal diminished soon after the division of the nerves, and proportionably with the respiration.

Airbladder of fishes.

With the functions of the airbladder of fishes we are not yet well acquainted. In some it has a duct communicating with the stomach. In others this duct is wanting, and it contains a peculiar organ of a red colour, and a laminated structure. In some both this organ and the duct are found, and in a few this bladder has muscles. The air contained in this bladder is a mixture of oxigen and nitrogen, the former being in greater quantity in proportion to the depth at which the fish lives in the water. Its absence does not

appear

appear injurious to respiration, though it does to the production of carbonic acid. Tenches from which it has been removed swim, dive, and rise to the surface, with as much ease as others. Such are the principal results of the different inquiries of Messrs. Duvenoy, Delaroche, von Humboldt, Provençal, and Cuvier.

Poison of the upas.

Drs. Magendie and Delisle have made many experiments on animals, chiefly dogs, with the poison of the upas. Whether introduced into the system by the bloodvessels or lymphatics, by the way of the intestines, or by a wound, the animals died universally convulsed. It appears particularly to affect the spinal marrow, and to enter the system only by means of the circulation. It seems to act but very indirectly on the brain, thus showing an independence between it and the spinal marrow, that is not indicated by anatomy.

Juice of deadly nightshade.

Mr. Vauquelin has found, that the juice of belladonna, when swallowed by animals, produced in them a delirium exactly similar to that of opium. Some experiments of Mr. Sage confirm the action of this juice on the nervous system.

Gasses injected into the bloodvessels.

Dr. Nysten has examined the effect of different gasses injected into the bloodvessels. Atmospheric air, oxigen, nitrous oxide, carbonic acid, carbonic oxide, sulphuretted hidrogen, &c., were not deleterious. Oximuriatic, ammoniacal and nitrous acid gasses appeared to act by irritating very violently the right auricle and pulmonary ventricle. Sulphuretted hidrogen, nitric oxide, and nitrogen were injurious to the contractibility of these parts. Some others so changed the nature of the blood, that respiration was unable to convert it from venous to arterial.

Stings of insects and fishes.

In a paper on the means of remedying the sting of the weever, trachinus draco L.; and on the effects of the poison of the tarantula, with the mode of cure used in Spain; Mr. Sage recommends the internal and external use of the volatile alkali.

Rotations of crops.

In a report on the Means of improving Agriculture by Rotations of green crops by Mr. Yvart, the committee recommends it as answering the important purpose of showing how land may be rendered constantly productive without exhausting it.

Mr.

Mr. de Cubière read a paper on the cultivation of the bald cypress (*le cyprès-chauve*), showing the advantages of this fine tree. Culture of the cypress.

Mr. Leblanc, who has resided several years in America, communicated his ideas of the ease with which the vicugna might be domesticated in the Alps and Pyrenees. Vicugna.

Mr. Poyfère-de-Ceré read an account of the mode of washing superfine wool in Spain. Wool.

Mr. Percy related some curious observations on the fabrications of the jars and alcarazas, which the Spaniards employ for preserving liquors, and for cooling them. Alcarazas.

In the report of the Class of History and ancient Literature, a paper by Mr. Grégoire is mentioned, containing a description of a singular ancient bell, from the convent of Bobbis, in Piedmont. This bell is about 9 dec. [35·4 inches] in diameter, and of a spherical shape: one hemisphere being complete; the other formed of ten branches, broad at the base where they join the upper half, and tapering to a point*. Its sound is much louder than that of a bell of the common form of the same weight. A small portion, taken from the ear, was analysed by Mr. Vauquelin, and found to consist of 76 parts copper, 20 tin, and 4 lead. Mr. Vauquelin was satisfied, that these were the only metals present; though, from the smallness of the quantity analysed, the proportions may not be strictly accurate. He supposes, that the lead was an adulteration of the tin, though advantageous to the sound. Messrs. Molard and Montgolfier have cast four other bells of the same form and size, before they had a knowledge of Mr. Vauquelin's analysis, using different compositions. That which came nearest in sound to the original was a mixture of equal parts of copper, brass, and tin. Ancient bell of very loud tone.

Royal Society of Sciences at Harlem.

The prize for the question concerning the insects most injurious to fruit trees, their natural history, and the means Insects injurious to fruit trees.

* Nothing is said of the thickness of the metal, or of the space left between the points of the branches, which appear from the description not to be united. C.

of

of destroying them, was awarded to Mr. Fred. W. Freyer, councellor of the court and of the regency of Saxe Hilburghausen.

Prize questions.

The following questions, having received no satisfactory answer, are repeated,

Graduation houses for making salt.

1. May graduation houses, for making salt from seawater be established with advantage on the coast of Holland; n d how may they be best conducted, considering the circumstance of the country?

Effects of manures.

2. From the process lately made in the physiology of plants how far do we know in what way vegetation is promoted by different manures in various soils; and what indications way we deduce from the knowledge we have acquired, with respect to the fertilization of uncultivated and barren land?

Ancient topography of Holland.

3. How far can the study of ancient authors, the examination of antiquities, and observations made on the spot, serve to determine with certainty what the face of this conntry was formerly, particularly under the dominion of the Romans, including the course of the rivers, and extent of the lakes, and what changes they have successively undergone?

Changes on the coasts of Holland.

4. What do historical accounts of acknowledged authenthenticity teach us of the changes, that have taken place on the coasts of Holland, the islands, and the arms of the sea that separate them? and what useful information may be derived from it.

Ancient and present height of tides.

5. Do the tides on our coasts rise higher than in former ages, and fall proportionably less low?—If so, how far can we determine the quantity of this difference in ages more or less remote, and what are the causes of the changes? Do they arise from gradual alterations in the outlets of the waters, or do they depend on external and more remote causes?

Renovation of the oxigen of the atmosphere.

6. As the experiments and observations of philosophers have shown of late, that the quantity of oxigen gas emitted by plants is by no means sufficient to supply to the atmosphere what is consumed by the respiration of animals, combustion, absorption, &c., by what other means is the due proportion between the component parts of the atmosphere continually preserved?

7. How

7. How far has chemistry made known the component parts and principles, both proximate and remote, of plants, particularly of those employed as food? and how far can we deduce from what is known, or what may be discovered by experiments, combined with the physiology of the human frame, what vegetables are best adapted to our use in a state of health, and in certain diseases. Immediate and remote principles of plants.

8. What is the cause of the phosphorescence of the water in the seas and inlets in and around this kingdom? Does the phenomenon depend on the presence of living animalcules? If so, what are they, and are they capable of imparting to the atmosphere any injurious properties?—They who purpose to answer this question are requested to consult the most recent and accurate writers on this subject, particularly Viviani, Genoa, 1805, and to examine how far this phosphorescence, which is very remarkable on some parts of our coasts, is connected with the prevailing diseases in unhealthy seasons. Phosphorescence of the sea.

The following new questions are also proposed.

9. As the secretion of milk in cows appears to be increased, when they are fed in stables on potatoes, carrots, or beetroots, it is required to show by experiments and observations, whether the milk of cows be really increased by these articles of food, and under what circumstances; in what way they can be given with most advantage; whether the quality of the milk be altered by this feeding; and, if so, what this alteration is, particularly with regard to the quality and quantity of cream and butter produced. Milk of stall fed cows.

10. As the antiseptic quality of common salt appears not to depend solely on the muriate of soda, but also on the muriate of magnesia, which adheres to common salt, it is required to determine by experiments the comparative proportions of the antiseptic quality of these two salts; in what proportions they should be mixed, to prevent putrefaction as long as possible, without the taste of the substances we would preserve becoming less agreeable; and whether it would be advantageous to use muriate of magnesia alone, particularly in voyages to hot climates. Antiseptic quality of the muriates of soda and magnesia.

11. What

Shell lime.

11. What is the chemical reason why stone lime makes on the whole more firm and durable buildings than shell lime, and how may the latter be improved in this respect?

Nitre beds.

12. May nitre beds be profitably formed in this country, particularly in places where the water is impregnated with several substances produced by the putrefaction of animal matters? and what rules should be observed respecting them?

Nature of luminous meteors.

13. What do we know, from incontestible observations, of the nature of luminous meteors, or those that have the appearance of fire, lightning excepted, which occasionally appear in our atmosphere? how far can they be explained by known experiments? and how much is there still gratuitous or doubtful in what the philosophers of the present day have asserted respecting them?

Metals from the alkalis.

14. Can it be demonstrated by incontrovertible experiments, that the substances which have the appearance of metals, produced from alkaline salts, are real metals? or are there sufficient reasons to maintain, that they are hydrurets, produced by the combination of hidrogen with the alkalis? What is the most certain and convenient mode of producing these substances from the alkaline salts in pretty considerable quantity by means of a high temperature?

An omnia ab ovo?

14. How far may we still maintain the doctrine of Harvey, that animals are born in general from preexisting eggs, and that plants spring only from seeds? and on the contrary what are the principal observations that show, that there are animals and plants, which are produced in a different mode?

Chemical explanations of electricity.

16. What judgment is to be formed of the chemical explanations attempted to be given of electrical phenomena? Are there any founded on sufficient experiments, or that may be proved by new ones? Or are they to be considered hitherto as hypotheses by no means proved, or advanced without valid reasons?

The time for answering all these questions is previous to the 1st of January, 1812. Beside the usual medal, value 30 duc. [£13 : 17 : 6], 30 ducats in addition will be given to those who answer questions 2, 3, 4, 7, 10, 11, 13, 14, and 15; and 50 duc. [£23 : 2 : 6] in addition to questions 1 and 5.

Academical

Academical Society of Sciences, at Paris.

At the meeting in September, 1809, Mr. Nauche communicated some experiments he had made on the contraction of the muscles in frogs. The object of these was to show, in opposition to the assertions of prof. Richerand and Bichat, that the contraction of the muscles may take place independently of nervous influence, or the influx of the blood. We learn from comparative anatomy, that this is the fact in thousands of animals, which have neither blood-vessels nor a nervous system, yet this faculty is excited in them with great intensity. It may even be affirmed, that it is more active, stable, and independent of life, in proportion as the animal is less perfect, and lower in the zoological scale. These facts appear to confirm the opinions advanced by Mr. Dubuisson in his Essay on the Properties of the Vital Power in Vegetables, that contractibility, or irritability, is inherent in the muscular fibre, and peculiar to it; as sensibility is to the medullary substance, renitence and elasticity to the albugineous fibre, and tone to the cellular texture.

Contraction of the muscles independent of nervous influence and the blood.

Society of Encouragement at Paris.

A report on the thread stockings manufactured by Mr. Detrey, sen. says, that they combine fineness, strength, and beauty. The evenness of the thread, its lustre, and accurate spinning are remarkable. They are three thread. They are not dear, as the price is 15 fr. [12*s.* 6*d.*] a pair; and when it is considered, that cotton stockings are manufactured in France as high as 48 fr. [£2.], not exceeding them in beauty, and inferior in strength, they must be esteemed a public benefit.

Thread stockings.

The gold and silver medals, on count Rumford's donation, have been adjudged by the president and council of the Royal Society to Mr. Malus, for his discoveries of certain new properties of reflected light, published in the 2d vol. of Mémoires d'Arcueil, which we shall insert the earliest opportunity.

Count Rumford's medals.

The

Jacksonian premium.

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

Cotton manufactory in Italy.

The cultivation of cotton, and its manufacture, are said to be carrying on to a considerable extent in Italy.

Vessel of a new construction.

Mr. Daubusson de la Feuillade has exhibited on a canal near Paris a model of a vessel of his invention. The model was 25 feet long, 52 inches broad, and did not draw 5 inches of water. The inventor proposes to build one 200 feet long, and 50 feet beam*, which is to carry 66 guns. It is to have four masts, be deep waisted, and with two thousand men, and provision for 50 days, would draw only 9½ feet of water. It will sail faster, and lie nearer the wind, than any other vessel. Its sails are to turn quite round, and either end may be made the stem or stern at pleasure, so as to render tacking unnecessary†. In a dead calm a *rame aspirante* is to supply the want of wind. Cork sheathing, and airvessels of copper, are to be employed, to render it incapable of being sunk. Its intention is to surprise an enemy's harbour.

Valuable writing ink.

An inkmaker at Paris professes to have discovered a vegetable fluid ink, which never thickens or loses in any degree its fluidity by evaporation, is always free from sediment, and never occasions iron moulds, or in any way injures linen or clothes, that may be accidentally soiled with it. He adds, that what is written with it never becomes yellow by age. Sonnini says, that he has long used this new ink made by his neighbour, Mr. Alphonsus Wée, and that it possesses the qualities ascribed to it.

* This is not proportional to the model. C.

† The inventor appears from this to have but little knowledge of navigating a ship. C.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXIX.

ARTICLE I.

Method of assisting the Escape of Persons, and the Removal of Property from Houses on Fire: by Mr. JOHN DAVIS, *No. 7, John Street, Spitalfields* *.

SIR,

I BEG you will have the goodness to lay before the Society of Arts, &c. a machine, which I have invented, for more effectually saving persons and property from fire.

Machine for saving persons and property from fire.

It appearing to me a desirable object, that the public should be in possession of an apparatus better adapted for the above purpose than any now in use, I have endeavoured to strike out an entire new plan of a machine calculated for the use of a parish, which can be easily removed and adjusted to any window, with a convenient apparatus or box, movable up or down, so as to receive persons or property. I have completed such a machine, which has answered my expectations, and been approved by several gentlemen who have seen it in action.

* Trans. of the Soc. of Arts, vol. xxviii, p. 175. Fifty guineas were voted to Mr. Davis for this invention.

The machine is at Mr. James Bevan's mahogany-yard, City road, where it shall be exhibited to a committee appointed by the society whenever they please.

I am, Sir,

Your obedient humble servant,

JOHN DAVIS.

No 7, *John Street*, *Spitalfields*,
Jan. 10, 1809.

Reference to the Engraving of Mr. John Davis's Fire-Escape, Pl. IX.

Description of the machine.

The plan of my fire-escape is calculated for the use of a parish; its principle consists in three ladders, ABC, applied to each other by four clasp irons on the top of each of the two lowermost, which are so contrived that each ladder may slide into the one beneath it; on the top of the lowermost ladder A two pullies are fixed on the inside, over which two ropes *a a* pass, and situate between the lower ladder A, and the middle one B. The ropes are made fast to the bottom of the middle one on each side in a proper direction with the pullies on the top. The upper ladder C is attached to the middle one in the same manner, and on the top it carries two horn pieces, D, made of iron, and turned off at each end similar to two horns, which are four feet wide; their ends are sharp to pitch on each side of a window, and with its points hold the ladders steady. The three ladders when shut down are about fifteen feet in height. They are placed perpendicularly in the middle of a framed carriage, EF, of nine feet six inches long, and five feet six inches wide, mounted upon four wheels F. On each side of the carriage a windlass is placed; that marked G on the right side of the carriage is for the four ropes *a a* and *b b*, fixed two to each ladder AB. By turning this windlass the ladders may be wound out from their standing height of fifteen feet to forty. Over this windlass is a screw turned by the winch *d*, by turning which the ladders may be inclined against the house with all imaginable ease. On the top of the upper ladder C on the outside, are two pullies, over which two chains are conducted to the windlass H on the

the left side for the purpose of carrying up a box I; two of which travel with the fire-escape, so that in the event of one being filled with small valuables, it may be unhooked, and the other K put on, which will save time. The whole apparatus may be drawn by one horse, or six men, and when arrived at the scene of danger may be adjusted in two minutes. If every parish would provide one of these escapes, and keep it where it might be brought out on the first alarm, I feel persuaded it would lessen the many accidents, which occur by fire in the metropolis.

I have the honour to be, Sir,

Your obedient humble servant,

JOHN DAVIS.

No. 7, *John Street*, *Spitalfields*,
May 14, 1810.

Farther Observations from Mr. Davis on his Fire-Escape.

SIR,

I BEG leave to return you, and the gentlemen of the committee, my sincere thanks, for the kind attention I have experienced; and should you think the following hints likely to give any additional information on the subject of my fire-escape, you will have the goodness to submit them to the consideration of the committee.

Hints with regard to escape from fires.

Certain it is, that, however good any principle may be, the practice must also be so to be effectual; therefore it is my opinion, that every parish should be provided with a machine on my principle, to be kept in some convenient place, easy of access. The key should be kept at the watch-house by night, and by day at the nearest public-house; if this, which ought to be, were the uniform custom, it would soon become familiar, and be attended with no expense. On the alarm of fire, I would have the machine brought out directly, as I consider it an improvident method, when a house has been on fire some time, and some unfortunate sufferer should appear in need of prompt assistance, to have to search about for the keys of a churchyard, or some other obscure place to bring the fire-ladders; which, when brought,

 if

if not exactly the right height, are useless; and when this, which is not unfrequent, is the case, the remedy is almost as bad as the disease, witness Mrs. Smith, having fallen off a parish ladder, at Chelmsford, while endeavouring to save herself in that dreadful fire, in March, 1808. It would be needless for me to enumerate instances, where a well-timed outward apparatus would have been of essential service—the thing is self-evident, and the occasions for their use have also been many. I would also propose, that a board should be put up, offering a reward sufficient to stimulate persons to bring the machine—for example, ten pounds for every life saved by it. I think no person would think it too much, who had been saved. This would have the good effect of having it always in time, which is most essential, as twenty shillings are not sufficient to induce men to the necessary trouble attending such labour.

Having thus offered my sentiments, respecting the good effects which may be derived were certain regulations put in force,

I remain, with great feeling for suffering humanity,

Sir,

Your most obedient and humble servant,

JOHN DAVIS.

II.

New Method of applying the Filtering Stone for purifying Water: by Mr. William Moult, *No.* 37, *Bedford Sqaare* *.

SIR,

Inconveniences in the common mode of using filtering stones.

IF you think the following information, relative to a new method of filtering water, is deserving of the attention of the Society of Arts &c., I wish you would lay it before them.

* Trans. of the Soc. of Arts, vol. xxviii, p. 212. The silver medal was voted to Mr. Moult.

My

My objections to the old method of filtering by putting water into the filtering stone are, that the dirt falls to the bottom, and fills up, or chokes the pores of the filtering-stone, so that the stone requires frequently to be cleaned with a brush and sponge to allow the water to pass, after which the water passes through the stone in a muddy state for two or three days; it likewise requires to be frequently filled, and as it empties less water comes into contact with the stone, and therefore a smaller quantity, in such a state, can only pass through. Likewise a filtering stone used in the common way soon becomes useless, from the filth insinuating itself into the internal parts of the stone, out of the reach of the brush.

Improved method.

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

I am, Sir,

Your humble servant,

WILLIAM MOULT.

No. 37, Bedford Square,
April 18, 1810.

CERTIFICATES.

Testimonies of its advantages.

We, the undersigned, having inspected and examined a new mode of employing the ordinary filtering-stone, discovered by William Moult, are of opinion that its superiority over the customary method is so great as to entitle it to particular notice.

That it not only supplies an infinitely greater quantity of purified and limpid water, but is capable of preserving its porosity free and pervious for years together, by an occasional self-operation.

That by this valuable process the principal objections to drip-stones is removed, viz. the constant labour they require

to

to keep them clean by means of brushes, without eventually producing the intended effect, and without preventing their being finally rendered useless.

D'Arcy Preston, captain in the Royal Navy;
Charles Gower, M. D.;
Thomas Pitt, Esq. V. P. Wimpole street;
Richard Davenport, Esq. Wimpole street.

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

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

III.

Method of raising a loaded Cart, when the Horse in the Shafts has fallen: by Mr. BENJAMIN SMITH, *No.* 11, *Turnham place, Curtain road, Shoreditch**.

SIR,

I HAVE taken the liberty of sending you a model, with a brief explanation of the utility of my invention, in order that it may be laid before the Society instituted for the Encouragement of Arts &c., to whose comprehensive judgment and abilities I with great deference submit it for their determination, whether they think it likely to be attended

* Trans. of Soc. of Arts, vol. xxviii, p. 215. Fifteen guineas were voted to Mr. Smith.

with

with the success and utility which I flatter myself it deserves. From the simplicity of the construction and the trivial expense attending it, I presume there will be no bar to its universal adoption. I respectfully submit it to the discernment and decision of the society, who will, I am convinced, give it all the merit and approbation it may deserve.

Horses falling in the shafts of loaded carts.

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

Much injured.

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

Method of relieving them.

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

I am, Sir,

Your most obedient servant,

BENJAMIN SMITH.

No. 11, Turnham place, Curtain road, Shoreditch, London, Dec. 13, 1809.

Advantages

Advantages derivable from this Invention:

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

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

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

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

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

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

Reference

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

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

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

Certificate.—Mr. William Whitehead, jun., of Cadogan place, Sloane street, certified, that he had attended experiments made to ascertain the efficacy of Mr. Smith's invention; that a cart weighing twenty three hundred-weight, loaded with one tun of stones, was raised by means of Mr. Smith's apparatus with ease by one horse. Testimonial.

That he very much approves of Mr. Smith's invention, and

and thinks it likely to be of great service in general practice, more especially on account of the business being effected with little expense. That many carts are already so formed, that very little additional apparatus will be required to complete them for the purpose.

IV.

Method of Ventilating Mines, or Hospitals, by extracting the foul Air from them: by Mr. JOHN TAYLOR, *of Holwell, near Tavistock* *.

SIR,

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

I am, Sir,

Your obedient servant,

JOHN TAYLOR.

Holwell, April 9, 1810.

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

Importance of ventilating mines.

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

* Trans. Soc. of Arts, vol. xxviii, p. 219. The silver medal was voted to Mr. Taylor.

their

their employers are put to considerable additional expense by the unavoidable hinderance and the waste of candles and other materials.

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

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

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

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

several hundred pounds; or in sinking a shaft it may be necessary, at an expense not much less, to drive a level to it from some other for this purpose alone.

Attempts to avoid this by a double shaft:

To avoid this, recourse has been had to dividing the shaft or level into two distinct parts, communicating near the part intended to be ventilated, so that a current may be produced in opposite directions on each side the partition; and this, where room is to be spared for it, is often effectual to a certain extent. It is found however to have its limits at no very great distance, and the current at best is but a feeble one, from the nearly equal states of heat in the air on each side. The only scheme beside these, that I know of, has hitherto been to force down a volume of purer air, through a system of pipes placed for the purpose, and a variety of contrivances have been devised for effecting this; most of them are so old that they may be found described in Agricola's work *De Re metallicâ*. The most common are by bellows worked by hand; by boxes or cylinders of various forms placed on the surface with a large opening against the wind, and a smaller one communicating with the air-pipes by a cylinder and piston working in it, which, when driven by a sufficient force, has great power; but the cheapest and most effectual scheme for this purpose, where circumstances will admit of its being applied, is one which I adopted some time since in the tunnel of the Tavistock canal. It is by applying the fall of a stream of water for this purpose, and it has been long known that a blast of considerable strength may be obtained in this manner, which has the advantage of being constant and self-acting. The stream being turned down a perpendicular column of pipes, and dashing in at a vessel so contrived as to let off the water one way, with an opening at another part for the air, which being pressed into it by the falling water, may be conveyed in any direction, and will pass through air-pipes with a strong current, which will be found efficacious in ventilating mines in many instances, as it has likewise, in some cases, been sufficient for urging the intensity of fires for the purposes of the forge. It is easily procured where a sufficient fall is to be had, and the perpendicular column can be so fixed, as that the water from the bottom may pass off, while

or by forcing down air.

Common method of doing this.

Cheaper and more efficacious method.

while the air is forced into a pipe branching from the air-vessel, and which is to be continued to the part of the mine where the supply of fresh air is required.

This an imperfect remedy.

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

Pumping out the vitiated air preferable.

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

Modes suggested.

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

Machine for the purpose described.

I at last erected the machine, of which the annexed is a drawing, which, while it is so simple in construction, and requires so small an expense of power, is so complete in its operation, and its parts are so little liable to be injured by wear, that, as far as I can imagine, nothing more can be desired,

desired, where such a one is applied. This engine bears considerable resemblance to Mr. Pepys's gazometer, though this did not occur to me until after it was put to work. It will readily be understood by an inspection of the drawing, Pl. x, fig. 3, where the shaft of the mine is represented at A; and it may here be observed, that the machine may be as well placed at the bottom of the shaft as at the top, and that in either case it is proper to fix it upon a floor, which may prevent the return of the foul air into the mine, after being discharged from the exhauster; this floor may be furnished with a trap-door to be opened occasionally for the passage of buckets through it.

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

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

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

It's mode of action.

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

The quantity of air exhausted is calculated of course from the area of the bore of the cylinder, and the length of the stroke.

Dimensions of one for large works.

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

The bore of the exhausting cylinder two feet.

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

The

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

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

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

Small engine.

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

Ventilator applied to the tunnel of the Tavistock canal.

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

Its action.

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

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

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

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

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

Its application to various purposes.

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

Thus, if it were required to ventilate a shaft of a mine, or a single level, which is most frequently the case, where three men are at work at one time, and we allow that those three men vitiate each twenty-seven cubic inches and a half of air per minute, (as determined by the experiments of Messrs.

Messrs. Allen and Pepys); and allowing farther, that their candles vitiate as much as the men, there will be six times twenty-seven cubic inches and a half of air to be drawn out in a minute, equal to one hundred and sixty-five.

Now a cylinder five inches in diameter, working with a stroke of nine inches, will effect this by one stroke in a minute, though it would certainly be advisable to make it larger.

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

Its application to collieries; and to fire-damp, or choke-damp:

In workhouses, hospitals, manufactories, &c., it is always easy to calculate the quantity of air contained in any room, or number of rooms, and easy to estimate how often it is desirable to change this in a certain number of hours, and to adjust the size and velocity of the engine accordingly. Where this change of foul air for pure is to take place in the

to workhouses, hospitals, manufactories, &c.

the night, means for working the machine may be provided by pumping up a quantity of water into a reservoir of sufficient height to admit of its flowing out during the night in a small stream, with sufficient fall, so as to give motion to the engine; or by winding up a weight of sufficient size; or by many other means, which are easily devised.

If, for instance, a room in which fifty persons slept was eighty feet long, twenty wide, and ten high, it would contain 16000 cubic feet of air; and if this was to be removed twice in eight hours, it would require a cylinder of thirty inches diameter, working with a four-foot stroke four times in a minute, to do it; or nearly that. Such a cylinder could be worked by the descent of ten gallons of water ten feet in a minute; or, for the whole time, by eighty hogsheads falling the same height.

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

JOHN TAYLOR.

Holwell, near Tavistock,
Feb. 7, 1810.

CERTIFICATE.

Testimony of its efficacy.

An extract from the Report of the Committee of Management of the Tavistock Canal, to the General Meeting of Proprietors, held in August 1808, stating, that great impediments had arisen from the want of good air in the tunnel when distant from a shaft, then adds—"For the purpose of rendering the ventilation in the tunnel completely good, and of doing it in a manner that may be applied to very considerable lengths in driving, the engineer has erected machines, acting upon the simplest principle, and without friction, which exhaust from the very place in which the men are working a continued volume of vitiated air; the place of which, of course, is as constantly supplied with fresh air, by the pressure of the atmosphere, and thus all difficulty on this head completely ceases."

V. *On*

V.

On the Processes employed to cause Writing to disappear from Paper, to detect the Writing that has been substituted, and to revive that which has been made to disappear; Improvement of common Ink; a Notice of a new Ink, that resists the Action of chemical Agents: by B. H. Tarry, *M. D.* *.

Indications of writing having been scratched out.

WRITING is removed either by scraping with a knife, or by means of acids. When writing has been scratched out, commonly pounce, or size, is applied to the paper, that the ink afterward used may not run. If pounce have been employed, the strokes of the same pen will appear more slender, if size more full, than on other parts of the paper. Immersion in warm water for a few minutes will dissolve and wash away size: alcohol will have the same effect on pounce. After the paper is taken out, it should be dried slowly; at first in the shade, till three parts dry, and afterward between the leaves of a book, or a quire of paper. While it is drying the ink last used will spread and sink into the paper more or less. Generally indeed close inspection with a good lens will show where any writing has been scratched out, by the appearance of some loose or torn filaments.

If all the iron have been removed the writing cannot be restored.

If the means employed to obliterate writing have been such as to remove the whole of the iron from the paper, every attempt to restore the writing must be vain. If some ferruginous compound remain, the characters may be reproduced in their original form; though the colour will vary, according to the nature of the compound in which the iron is concealed, and of the reagent employed.

Sometimes it may by the gallic acid,

In some cases the gallic acid is capable of recomposing the writing, that has been made to disappear by chemical means; but its attraction for the oxide of iron is not so strong as is commonly supposed. The red or brown oxide of iron, obtained from the sulphate or nitrate by means of

* Abridged from the Ann. de Chim. vol. lxxiv, p. 153; and from the report made to the Institute by Berthollet, Vauquelin, and Deyeux, ib. vol. lxxv, p. 194.

 alkaline

alkaline carbonates, cannot combine with the gallic acid to form ink, unless the carbonic acid have been expelled from the oxide of iron by some more potent acid. It is the same with respect to the oxalic acid, and acidulous oxalate of potash: when this acid or this acidulous salt has seized the oxide of iron, the gallic acid cannot destroy the combination, because it has an inferior attraction for the oxide of iron.

If the writing have been destroyed by nitric or oximuriatic acid, the gallic acid in tincture, infusion, or decoction of galls will revive it.

prussiate of lime or potash,

Liquid prussiate of lime or potash is a good reagent, to detect the presence of iron. If the ink have disappeared in consequence of the decomposition of gallic acid, as when oximuriatic acid has been employed, either of these will render it legible, causing it to appear of a light greenish blue while wet. If oxalic acid have been employed to obliterate the writing, the prussiates will restore it of a reddish brown colour. If nitric or sulphuric acid have been employed, the prussiate of lime will show this by staining the paper blue, but it cannot reproduce the writing.

hidroguretted alkaline sulphurets.

Hidroguretted sulphurets of the alkalis, or of the alkaline earths, are very prompt and powerful tests of ferruginous salts. The alkali, or earth, combines with the acid; and the sulphuretted hidrogen with the oxide of iron, forming an hidroguretted sulphuret of iron. Iron in the state of red oxide is partly disoxidated by the hidrogen, water is formed, and the iron passes to the state of black oxide. This is the case with writing turned rusty: these reagents immediately change it to a green black, much deeper than gallic acid would give. A solution of sulphate of iron mixed with an hidroguretted sulphuret produces a very deep green black ink.

The same attractions are exerted when the hidroguretted tests are applied where writing has been obliterated by the oxalic acidule or the oximuriatic or nitric acid. If the oxalic acidule were employed, the characters will reappear of a green black or brown red. If the oximuriatic acid, of a green black or pale rust colour. The less the revived writing approaches a black, the more the iron was oxided in in the metallic salt decomposed, or the less the iron was disoxided by hidrogen. The writing on which nitric acid has acted strongly cannot be reproduced: but on passing sulphuretted hidrogen over the paper where it was, waving lines of a green black will be formed on the remotest parts to which the sulphuretted hidrogen penetrates. These lines may be produced in great number, and in different directions. They are owing to the sulphuretted hidrogen combining with the oxide of the ferruginous nitrate. If the undulating lines, or the letters that have been restored, should disappear, they may be reproduced by dipping the paper into cold water. Beside the traces of writing, and the undulating lines just mentioned, the paper takes a yellow colour when it is not impregnated with an acid, and a green more or less deep when it is. The green colour will be deeper, in proportion as the acid was stronger, or in larger quantity. In all cases the paper retains the colour of fresh butter after it is dry. The hidroguretted sulphurets should be diluted with half or two thirds their quantity of water before they are used, as in their ordinary state they are too strong.

Indications of writing obliterated by nitric acid.

From what has been said, we may hope to restore writing, that has been obliterated by any agent except the nitric acid: and if this have been employed only in small quantity, without the assistance of any other acid, and its action has not been too long continued, on holding the paper to the fire the writing will reappear of a rust colour.

Method of restoring it when practicable.

With regard to the improvement of ink, little progress has been made since the time of Lewis. Inks made by infusion, and with green sulphate of iron, are of a Prussian blue colour, light, pale when written with, but growing black as they dry on the paper. Those made by decoction are blacker, thicker, and form a more copious sediment, which is of a dirty Prussian blue colour. Decoction extracts from galls all the soluble parts; infusion takes up chiefly the gallic acid, and mucilage, with a little extract and tannin. In the decoction the iron of the green sulphate becomes more oxided, and the extract and tannin acquire oxigen, by absorption from the atmosphere; and the iron in a higher state of oxidation, and the oxigenized extract, produce a deeper black with the gallic acid and tannin.

Improvement of ink.

The

The more abundant sediment is owing to a larger quantity of extract and tannate of iron. In inks made by infusion, the oxide of iron, extract, and tannin, increase their oxigenation very little, till they come to dry on paper. Nitric acid immediately obliterates writing with ink made by infusion, but that which has been made by decoction resists its action much longer, on account of the larger quantity of extract in it.

Infusion or decoction of galls should be kept some time.

In proportion as the infusion or decoction of galls grows old, its surface is covered with mother, which is the mucilaginous principle separated. This mother ceases to form in about a year, during which the pellicle produced on the surface should be removed three or four times. The infusion or decoction of galls grows brown as it becomes oxigenized, takes an amber colour, and emits a pleasing smell; and, when combined with green sulphate of iron, no longer produces a Prussian blue, but a green black. The amber colour of this infusion or decoction is owing to the oxigenized extract and tannin. The green colour of the ink arises from the mixture of the black of the gallate of iron with the fawn colour of the oxigenized tannin, which in this state can no longer combine with the oxide of iron. If the tannin be separated from the infusion or decoction by means of an alkali, the green or red sulphate of iron forms with it a very black and purer ink; and the alkali in the solution facilitates the union of the oxide of iron with the gallic acid, by combining with the sulphuric acid of the sulphate. The oxigenized extract concurs in rendering the ink blacker, as does the oxide of iron more highly oxided.

Infusion preferable.

Infusion of galls is preferable to the decoction, as it dissolves the principle, that is essential to the composition, and very little of those that are foreign to it. Logwood browns the ink, and loads it with its colour; it is better therefore, to use in its stead a small quantity of galls in addition to that directed by Lewis. The following is the composition of a good ink.

Receipt for good ink.

Infuse in one litre [a wine quart] of rain or river water 125 gram. [4 oz. troy] of bruised galls, letting them stand in the sun four hours in summer, or six hours in winter. This infusion may be used immediately after straining; but it it is better to let it stand four or six months, removing the mother that forms on the top now and then, and finally separating by filtration both this and the tannin that has fallen to the bottom. In this dissolve 32 gr. [a troy ounce] of powdered gum arabic; then add the same weight of finely powdered sulphate of iron, superoxigenized by calcining it till it grows reddish; and continue shaking the mixture till this is completely dissolved. The ink thus made is fine, light, and of a purple tinge, but black when dried on the paper. It is nearly, if not precisely, the composition of Guyot's ink.

Dr. Tarry next proceeds to his indelible ink, the composition of which however he does not disclose. He says only, that it contains neither galls, nor logwood, nor brazil, nor gum, nor any preparation of iron; that it is entirely vegetable; and that it resists the action of the most powerful acids, of alkaline solutions in their most concentrated state, and of all solvents. He sells it in a solid form; and for use it is to be mixed accurately in a mortar with eight parts of water, and then put into a bottle left at least one third empty, for the purpose of shaking it, which is to be done every six or eight hours for a couple of days. It soon soften quills, but metallic pens are well adapted to it, as it contains no acid. There is no danger from putting the pen into the mouth, as it contains nothing deleterious. Indelible ink.

Nitric acid has very little action on this ink. Oximuriatic acid only changes it to the colour of goose dung. After it has been acted on by this acid, caustic alkaline solutions give it the colour of carburet of iron. The letters however still remain unchangod in form, and these effects require a long maceration for their production. Action of acids and alkalis on it.

From the report of the committee it appears, that the ink of Dr. Tarry possesses the properties he ascribes to it; but they add, it has one of the faults common to all the indelible inks proposed, that of pretty quickly forming a considerable sediment, which deprives the supernatant fluid of its properties, so that it requires to be shaken every time it is used. Report of the committee.

VI. *On*

VI.

On the Sense of Smell in Fishes: By M. C. Duméril *.

Holes in the heads of fishes called nostrils.

ALMOST all the fishes hitherto observed have nostrils †. At least this name is given to two deep holes, which are generally found in the heads of these animals between their eyes and lips. These cavities have a single slender orifice; and within they are lined with a mucous membrane, having numerous folds. The first pair of nerves from the brain enter into the substance of this membrane, ramify in it, and there terminate. Analogy therefore seems to indicate, that the nostrils of fishes are particularly intended for the organ of smell, as in all other animals with vertebræ. Against this opinion however, adopted by all naturalists and physiologists, I have some facts and reflections to offer, which perhaps will seem more consistent with our knowledge in comparative anatomy and physiology.

Supposed to be the organ of smell:

but this questionable.

Fundamental propositions.

I propose to show, that the organ of smell does not and cannot exist in the mouths of fishes, from their manner of breathing: that the organs, hitherto considered as adapted to the sense of smell in these animals, are intended for the perception of a sensation analogous to that of taste: and that there can be no true smell for an animal habitually immersed in a fluid.

Nerves of sight, hearing, and smell, distinguished;

but not those of taste.

Different nerves lead to it.

In animals with vertebræ, anatomy easily distinguishes among the nerves, that lead to the organs of sight, hearing, and smell, the trunks of those peculiarly intended to transmit the sensation: but it is not the same with the organ of taste. We know indeed, that, at least among the mammalia, the gustatory faculty resides in the surface of the tongue: but, as this fleshy substance has other functions, and as its movements are particularly connected with the organs of speech and deglutition, it receives several nerves, and these greatly ramified, proceeding from three different regions of the brain. Hence anatomists have not been able precisely to

* Mag. Enc. Sept. 1807, p. 99. Read to the Institute, August, the 24th.

† Except the cyclostomes, as the lampreys and sphagobranchiæ, which are not real fishes, as I shall show elsewhere.

determine,

determine, whether the sensation be imparted through the medium of the lingual branch of the fifth pair, that of the glossopharyngean, or that of the great hypoglossal nerve.

The general opinion in favour of a branch of the lower maxillary nerve.

It is true the majority agree in considering the lingual branch of the inferior maxillary nerve as the only one capable of transmitting the sensation of taste; and most of them adduce in support of their opinion the observation of Colombo, who did not find this branch in a man destitute of the sense of taste. Soemmering, however, questions the circumstances of this fact, as well as of a similar one cited by Rolfink.

Others for the great hypoglossal nerve.

On the other hand some physiologists, at the head of whom is the great Boerhaave, have ascribed the gustatory faculty to the great hypoglossal nerve. These too rest their opinion on some anatomical observations, particularly on a case in pathology quoted by Hevermann, where the sense of taste was destroyed on the extirpation of a gland, with which the nerves, called at that time the great gustatory, or ninth pair, were removed.

The particular subject of physiology and comparative anatomy before us, therefore, may throw some light on a question not yet completely resolved.

The sense of taste necessary to animals:

Though the sense of taste is essentially necessary to animals, and must be the last obliterated, since on its decisions depend their preservation, by instructing them in the nature of the substances proper for their food, and the selection of them; at first sight, however, it would appear, that fish are destitute of it, if we seek for this organ in the parts where it is commonly seated.

but fishes apparently destitute of it;

In fact the inside of the mouth in fishes is lined with a thick, smooth, and polished membrane; of a very close texture, resembling that of the skin; and most commonly of the same colour with it. Sometimes this membrane is completely detached from the bones of the palate, or retained merely by a few vessels; as I have observed in the cod, frogfish, bullhead, ray, and shark: and I have never seen in it papillæ, or salivary glands.

as it cannot reside in their mouth,

The tongue of fishes is seldom movable. A bone supports it throughout its whole length. Its point can neither turn backward, nor toward the sides. In general the lips, palate,

or tongue.

palate, tongue, and branchiostegous rays are covered with bony points, or laminæ of different forms, which prevent the intimate contact of substances taken into the mouth. It is true in the muscles of the hyoides and of the branchiostegous rays, placed at the lower part of the mouth, we find all the ramifications of the nerves of the fifth pair, as well as those of the indeterminate nerve, which evidently has the place of the glossopharyngean. Yet neither I nor Mr. Cuvier could ever meet with the great hypoglossal nerve in fishes, notwithstanding our most attentive searches, when I enjoyed the advantage of editing his lectures on comparative anatomy. Besides, as this fact was of great importance to the subject of the present paper, I think it proper to add, that I have again satisfied myself of it by fresh anatomical researches.

The great hypoglossal nerve wanting in them.

It is easy to imagine, that the water, by its continual entrance into the mouth, and the compression it there undergoes, as often as the fish exerts on it the action of deglutition necessary to force it through the gills, must exert a friction so often repeated, as to deaden all the sensibility of these parts.

The sensation of the mouth deadened,

Now since the integuments of the inside of the mouth are coriaceous, destitute of salivary glands, and frequently roughened with teeth or horny points; the tongue adherent, bony, and immovable; the great hypoglossal nerve wanting; and water continually exerting a friction on it: it is very probable, that the organ of taste cannot exist there. This was the first point I proposed to examine.

and the organ of taste cannot exist in it.

As the organ of taste appears not to reside in the mouths of fishes, and this sense is indispensable to animals, we must meet with it elsewhere: and since tastes in general bear a considerable analogy to smells, let us inquire whether the sense of smell be not to a certain degree converted into that of taste. But, before we enter on this investigation, let us examine the nature of these two sensations.

Probably it is in some other part.

Natural philosophers, chemists, and subsequently physiologists, have generally attached to the idea of smell, that of the sensible existence of corporeal atoms of extreme minuteness. Though art has not yet been able to imitate an instrument so perfect as that met with at the entrance of the respiratory

Nature of smells.

respiratory organ in animals that live in the air, we have some means of proving chemically the material existence of those smells, the nature of which is best known. Thus the exhalations from nitrous gas, volatile oils, and ether, for example, may be destroyed by the combination of some of their principles with oxigen; and muriatic acid gas renders sensible to the eye the particles of ammonia, which cease to be odorate the moment this acid combines with them in the open air.

Proofs of their materiality.

The most perfect animals, those that possess all the five senses, are so organized as to perceive the principal modifications of the bodies surrounding them. They have sight, to enjoy the effects of light; feeling, to appreciate the solidity of palpable objects; hearing, to distinguish the vibrations of elastic bodies; taste, to discriminate the qualities of bodies capable of becoming liquid; and lastly smell, to collect the emanations of substances, that have the properties of a gas.

Senses of perfect animals.

Light exerts its action only on the eye; not on the tongue, nostrils, ears, or skin. It is the same with most smells, which do not act on the sight, taste, hearing, or touch. Each of the organs of sense then has its particular function, fixed and determined beforehand by the arrangement of its apparatus: for the sentient principle appears to be identical, and placed, as we may say, on the watch on the inside of each instrument, in order to collect and transmit the slightest modifications in the qualities of bodies.

Each sense has its peculiar object, dependent on the organ, as the sentient principle is one.

The sensations of smell and taste however, are most analogous, both in respect to the mode of action on our bodies, and to the apparent end at least for which nature seems to have given us organs to perceive them. The odorate and sapid particles are conveyed either by the airs that serve for respiration, or the solid and liquid aliment that must enter the stomach. Stopped on their passage through the nostrils or the mouth, these particles touch the nerves distributed on those parts, and thus give notice of their presence. The nerves immediately excite the ideas of the sensations they perceive, and excite us to admit or reject the air or food, according as the impression produced on the organ is agreeable or not. The sapid and odorate qualities of bodies then

Smell and taste have considerable analogy.

are

are discriminated by the tongue, when they are contained in a liquid; and by the pituitary membrane, when they are suspended in a gas.

Smell peculiar to the state of gas,

From these general considerations of the nature of smells and tastes, it appears, that liquids cannot intrinsically possess smell, since this quality of bodies is inherent in their state of gas, or vapour. We are justified therefore in presuming, that an animal, which from its nature must be immersed in a liquid all its life, does not possess a sense of which it can make no use: and this is the case with cetaceous animals, fishes, most of the mollusca, a great number of crustaceous animals and worms, and all the zoophytes.

and cannot be perceived in a liquid.

Cetaceous tribe analogous to fishes in their mode of respiration;

In a former paper I have pointed out the analogy between fishes and cetaceous animals, with regard to the mechanism of respiration*. It is in consequence of this mode of respiration, if I may so say, and of their necessary abode in water, that the organ of smell appears to be annihilated in these animals; for as Daniel Major and John Hunter first observed, though only in a few species, and Cuvier has since shown generally and more at large, there are no olfactory nerves, and no ethmoidal foramina, in the cetaceous animals. The pituitary membrane, that lines their nostrils, is smooth, dry, and coriaceous: it appears to have become insensible from the constant friction on it ocsasioned by the rapid and violent action of the water, that pervades the cavity of the nostrils. It appears however, that the organ of taste here supplies the place of that of smell; for, by a slight modification of the organs, the olfactory nerves of fishes may have another use, and be destined to make them sensible of tastes.

and want the olfactory nerves.

These nerves have another use in fishes.

Though fishes cannot smell,

From the ideas we have formed of the nature of smells, it necessarily follows, that fishes cannot receive impressions similar to those they occasion in animals that breathe air. Yet we know, that fishes are attracted by the emanations, that escape from several substances immersed in water, as is demonstrated by various baits employed in fishing; the salted roes of cod and mackarel, the broiled or stinking flesh of certain animals, old cheese, and many other things of strong smell.

they are sensible of emanations from substances.

* See Journal, vol. xxviii, p. 355.

Aristotle

Aristotle was acquainted with most of these facts, and even recites them at large in his History of Animals: yet he says positively, "fishes have no distinct organ of smell, for there is but one orifice to the apertures they have in the place of the nostrils." And elsewhere, "we see in them no external organ of hearing or smell, not even an aperture." Mr. Schneider, in his Synonimes of Artedi's Fishes, reproaches Aristotle with entertaining this opinion, after having so well described the olfactory organ and nerves in these animals. It is in some measure therefore a defence of Aristotle's opinion, if I endeavour to show, that every emanation in water must produce on the nerves, with which it comes into contact, a sensation analogous to that of taste.

This known to Aristotle, whose opinion is here defended.

Since there are no real smells in water, the exhalations, that escape from bodies immersed in it, either rise to the surface in the form of gas, and consequently do not remain in the liquid; or they are suspended in it or combined with it, and they participate in all the properties of liquids. If however the qualities of these particles, thus dissolved, be perceptible, they necessarily come under the same circumstances as sapid bodies; and therefore it would be useless for fishes, which live habitually in water, to be endowed with the organ of smell.

The organ of smell would be useless to fishes.

To prove the accuracy of this reasoning, it is necessary to investigate the use of the nervous apparatus, which has hitherto been supposed to be intended for the perception of smells: and to this I shall proceed, treating it more minutely than in the beginning of this paper.

Use of the nervous apparatus supposed to be intended for smelling.

The cavities termed nasal are always situate before the eyes, in the space between the nasal bones and those of the upper lip. Sometimes they are in the substance of the bones of the nose themselves, or between these and the pieces which Artedi has called hypophthalmic. The heterosome fishes, as the pleuronectes, the only animals with vertebræ that are not symmetrical, are the only ones that have both nostrils on one side of the body, in some on the right, in others on the left, and unequal. Lastly, though most of these species have these cavities on the top of the head, in the forehead; they are found beneath, and most frequently

The nasal cavities described.

frequently communicating with the mouth, in all the plagiostomes, as the ray, the shark, &c.

In all fishes these cavities present a kind of sinus, or cul-de-sac with a narrow opening; most commonly divided into two portions, sometimes into three, as in the eel, by a membranous septum, variously convoluted, which icthyologists have frequently noticed as characteristic of species.

We know from the observations of Monro, that these valves or curtains may be moved according to the will of the animal; and that under certain circumstances the orifice may be nearly covered by the septum. It is easy to observe this in live fishes, as I have seen in the goldfish and stickleback. It is then apparent, that the motion of the septum seems to be the consequence of the protraction of the lips; since at each inspiration the cavity opens and dilates, while it contracts and is covered as often as the mouth is closed: whence it seems to follow, that at every inspiration the fish causes a small quantity of water to enter on each side, which it may be said thus to analyse.

The first pair of nerves and part of the 5th spread on the membrane within them.

Each of these perforations exhibits within a cavity, very spacious in proportion to its orifice; and on this is spread the sentient membrane covered with mucus, in the substance of which is expended the whole of the first pair of cerebral nerves, and one or more large branches of the fifth pair, according to the observation of Collins quoted and corrected by Cuvier.

These cavities are always separated from the respiratory canal.

Nor must I forget to remark, as a circumstance particularly deserving notice, that these pretended nasal cavities are always separated from the canal of respiration; and that it is only in the rays, and some neighbouring genera, which have spiracles, that they are observed almost in the mouth. In fact it is to be presumed, that the liquid, in traversing them, would have deadened the sensibility of their surface by the rapidity of its motion, and the friction of its particles.

What are the inferences from this structure?

Now are these peculiarities of structure, which I have mentioned, of such a kind as to lead us to abandon our first opinion, deduced from the knowledge of physics, that smells cannot be perceived in water? or is not this supposed organ of smell in fishes better adapted to excite in them the sensation

sensation of tastes? These questions I shall proceed to examine.

Tastes and smells are nearly of the same nature: both sensations are produced by the physical and chemical qualities of bodies. We know, in fact, that very minute particles are continually separating from certain substances, which, without being decomposed, come to act immediately upon animals at that point of their surface alone, where they can manifest their presence. This phenomenon is effected by the intervention of a fluid medium, and a sort of contact*.

Tastes and smells analogous.

All the conditions necessary for the impression or sensation of taste are united therefore in the organ under examination, and the nature of the substances that may produce it. First, the organ is placed secure in a cavity: it opens and shuts at the will of the animal, it admits or rejects emanations at pleasure. Secondly, the sentient surface receives numerous and bulky nerves from the fifth pair; it is soft, moist, and mucous; and it presents a great surface in a large space. Thirdly, it appears in a certain degree to supply the place of the organ of taste, which cannot exist in the mouth of fishes from the very mechanism of their respiration.

The organs in question perfectly adapted to the sense of taste.

It seems to follow then from all these circumstances, that the organ of taste in fishes does not reside in the mouth: that the sensation of taste is probably imparted to them by the apparatus, which has hitherto been considered as adapted to perceive the emanations of odorate bodies: and lastly, that no real smell can be perceived in water.

General conclusions.

* I have already had occasion to enlarge on these general ideas in a paper on the organ of smell in insects, which I published ten years ago, and which may be found in the second volume of the Magazin Encyclopédique, p. 435.

VII. *On*

VII.

On the Alum Mines of Aubin in the Department of the Aveyron; by Mr. L. Cordier, *Engineer in chief, &c.* *

Alum mines of Aubin.

THE alum mines of the country of Aubin differ from those of the same nature worked in other places: their existence is wholly contingent. The periods of their formation are known, and are very recent. They occupy no considerable space of ground, and cannot extend much farther. Lastly their duration must be very limited, whether they be worked or not. These mines are nothing but coalpits, that have taken fire within a certain distance of time, and in which the fire is still daily exercising its ravages. There are four of them; those of Lassale, Fontaines, Buégne, and Bourlhones. To give an idea of their situation, that of the coal in the country must be known.

from coal-pits that have taken fire.

Description of the coal country.

The territory of Aubin is very hilly, and intersected by deep narrow passes. The part to the north-east of the town consists entirely of coal country, and is the least elevated, being nearly in the form of an elliptic basin, the great axis of which is north and south, and the surface of which exceeds a square myriametre [24676 acres]. This space is skirted and overtopped on all sides by the primitive soil; and is occupied by a pretty considerable number of oblong hills, intersected in every direction, and crowded together. The highest, for they are unequal in this respect, are two or three hundred metres above the valleys.

Its structure.

The arrangement of the strata throughout the basin exhibits nothing constant, or continued. Independent of the interruptions occasioned by the narrow passes and valleys, the direction, inclination, and order of the strata vary from one hill to another; so that to depict the present state of the soil, it is sufficient to say, that it appears to be the result of a complete disruption. We can merely perceive, that the directions more frequently approach the meridian than any other line, and that the prolongation of the

* Abridged from the Journal des Mines, vol. xxvi, p. 401. From the Report made to the council of Mines in 1807.

strata

strata is almost always in the longitudinal direction of the hills. As to their dip the strata are generally set on edge: they hang in all directions, and at every possible angle from perpendicular to horizontal: the strata of two neighbouring hills are seldom seen to incline the same way; and, when this does occur, it is at different angles. The hills nearest together offer striking varieties, and frequently singular for the nature, order, and thickness of the strata. It is even in vain to seek for some similarity of structure in certain places, where the strata that skirt a valley are placed so, that they would come to rest against the strata on the other side, if both were sufficiently prolonged. Hence it may be conjectured, not only that the surface has been completely broken up, but that it has experienced considerable degradations subsequent to this disruption.

Strata.

The coal ground is almost wholly formed of a gray sandstone, commoly fine-grained, and composed of feldspar, quartz, and some particles of mica. The mean thickness of the strata is about a yard: some are found, that are more than ten yards thick, others not a tenth of a yard. In the midst of these sandstones are seen thick strata of puddingstones with granitic fragments; and strata, generally thin, of gray or blackish argillaceous schist exhibiting some impressions of vegetables.

The coal.

Coal is found throughout almost the whole of the basin. The outcroppings are very numerous, and occur indiscriminately at the foot, on the acclivity, toward the summit, or on the ridges of the hills. The number of the seams, their thickness, and their distance from each other, vary in every hill. They are almost all thick enough to be worked. There are seldom more than four in one hill. Their mean thickness is in general from two to six yards; but in some places it is truly astonishing, and hitherto unexampled. The vertical seam now working at Lassalle is 103 met. [338 feet]. Its course is perfectly regular, and known, for it is worked by means of levels extending from the roof to the wall.

Management of it.

From what has been said it is obvious, that the coal of this country is as easy to extract as it is abundant. It is worked in fact in a number of places, and almost every where by means of levels. The coal is embarked on the river Lot, which runs near the mines. But this union of natural advantages, far from being turned to profit by good management, has hitherto given rise to various abuses; though I shall only point out that, which relates to the subject of the present paper. From time immemorial every landholder has been at liberty to dig in his own ground, get out the coal without order or method, and dispose of it as he could. Hence the number of pits opened has had no reference to the demand, and frequently individuals have been obliged to relinquish their works for want of a sale for their produce. Now from causes which it is useless to discuss here *, the works that are thus given up are liable to take fire spontaneously, even when carefully watched. The fire communicates very rapidly every where; and if the greatest exertion be not made to stop it in the beginning, it becomes afterward impossible to check its ravages, and the work is destroyed. It appears, that this misfortune happened very often formerly; for, on going over the surface of the ground occupied by the mines, at almost every step we meet with very evident traces of subterraneous fires now extinct. Accidents of this kind are now more rare, either from the people having learned how to prevent them, or knowing how to check them: yet seven or eight works are still burning at this moment.

The works that have been relinquished take fire spontaneously.

Four of these only deserve notice.

Among these works that have caught fire, those called Lassalle, Fontaines, Buégne, and Bourlhones, are the only ones remarkable, either on account of the intensity of the fire and the space it occupies, the disruption and torrefaction of the earth as far as the surface, or the daily pro-

Spontaneous combustion of the coal.

* In general only the purest coal is got out of the works. That which is mixed with schist, being of no value, it is used with other matters to fill up the vacuities made. Now whether this be frequently accompanied with iron pyrites disseminated in it, or perhaps even contain sulphur in combination, the fact is, that moisture renders it a very active pyrophorus, in all parts of the mines where the circulation of the air is stopped. The miners of the country have but one opinion on the subject: they all agree, that the spontaneous inflammation of the works is owing to the action of stagnant water on the refuse left in them; and that the fire manifests itself the more speedily, in proportion as the circulation of the air is more slow.

duction

duction of a considerable quantity of aluminous salts amid the torrefied rocks.

The burning pits, whence the alum works originate, must be considered as totally destroyed; but the alum produced will more than compensate for the loss of the coal. It is known too, that the fire will go out of itself, as soon as it has consumed all the masses of fuel, that have been exposed by the levels. It has long been ascertained, that the fire does not extend more than a yard into the coal left untouched in depth. This is so certain, that the extraction of the coal from beneath the works burned has been resumed at Lassalle and Fontaines.

The pits destroyed, but compensated by the alum.

The fire does not burn more than a yard deep.

The effects of the spontaneous combustion of the coal are the same in the four alum works. To judge from the state of the surface of the earth, the fire has not extended beyond the space that had been worked. The surface has sunk, cracked, and been deranged, in the manner of volcanic solfaterras. It emits a gentle heat, incessantly renewed; it is bestrewed with very curious productions of fusion and torrefaction; the crevices emit burning fumes of sulphurous acid, bitumen, and water; and even flames continually arise when the fire is consuming a stratum near the surface. The sandstones and schists, that accompany the burning seams of coals, are either simply torrefied, or changed into red, light, and rugged scoriæ, or violet-coloured, bluish, gray, and often striped, enamels. The acidosulphurous vapours attack, deprive of colour, and decompose part of these products, and frequently reduce them to powder; and at their expense are formed the vitriolic saline substances, that are found in such great abundance, either in the cavities of the masses, or amid the earth resulting from their decomposition, or on the surface of the ground. The simple or alkaline * sulphate of alumine constitute almost the whole of these saline substances. They exhibit themselves in all forms; in disseminated particles, discoverable only by their acerb and styptic taste, in whitish efflorescences, and filamentous and

Effects of the combustion.

* The alkali is probably furnished either by the combustion of the coal, or the decomposition of the feldspar, which abounds in the rocks affected by the acidosulphurous vapours.

silky masses, in yellowish mamillary incrustations, or in confused masses, friable, cavernous, white, gray, yellow, red, or a mixture of all these colours. It may not be superfluous to add, that the last variety is sometimes met with in blocks or incrustations weighing several pounds.

Such are the general characters of these alum mines, but there are particular ones, which ought to be noticed.

The mine of Lassalle.

The alum mine of Lassalle is in the bottom of a valley, at the foot of the hill of the same name, two miles north by west from the town of Aubin. The surface it occupies on a slope of about 45°, does not amount to 2 hect. [247 acres]. The subterranean fire has not exceeded the limits of the coal-pit: it occupies the length of 250 met. [273 yards] at the foot of the mountain, and extends nearly 70 met. [76 yards] into it. It has attacked nothing below the level of the brook, that flows through the valley.

Burning these twenty years.

This pit took fire spontaneously about twenty years ago. The stratum of coal, which feeds it, was three or four yards thick, and worked by means of levels. Attempts were made to extinguish the fire at the time, but in vain. The inclination of the strata in this part of the mountain is about 8° or 10° W. N. W.; or contrary to the slope of the mountain.

The fire now abating.

The activity of the fire has decreased greatly within these few years. It appears to be drawing to an end; or that the accumulation of torrefied and decomposed substances, that cover the surface, has retarded its ravages. The effect of the exavations made within these six months seems to confirm the latter opinion. Vapours now issue out abundantly by all the new vents they have been able to make, and the saline efflorescences increase more rapidly.

This mine has not been worked above nine months.

Mine of Fontaines.

The alum mine of Fontaines is at the bottom of the cul-de-sac, that terminates the valley of Lassalle, and at the foot of the mountain 2500 met. [2732 yards] N. E. of Aubin. It takes its name from a hamlet directly above it. Its surface is nearly square, and may be 3 hect. [370 acres]. The foot of the mountain at this place has a slope of about 50°.

Has burned these 80 years in different seams.

The fire commenced here eighty years ago. Several seams of coal were then working, one over another, and inclining

inclining 35° or 40° W. S. W. It was got out by means of levels, and with so much the more ease as the mountain slopes to the north. Each seam having been worked in several places, and to some distance, as 80 or 100 met., the fire has made more ravage, than at either of the other three places. Notwithstanding the length of time, the activity of the fire has not abated, at least in the higher parts. In fact we see there the sunken surface of the earth intersected by long and deep fissures, the sides of which are in the highest state of incandescence, and from which flames, accompanied with suffocating vapours, are continually escaping. In a word, the solfaterra of Fontaines presents the most curious combination imaginable of all the phenomena above described*.

The vitrified, scorified, and decomposed matters, that fill the space occupied or traversed by the fire, are very rich in aluminous salts.

Mine of Buégne.

The alum mine of Buégne is at the top and on the back of the hill of Buégne to the east. It is about 2 kilom. [$1\frac{1}{4}$ mile] west of Aubin. It is the result of the spontaneous combustion of a single seam of coal, which commenced twenty years ago, and has lost nothing of its activity. This seam is several yards thick, and runs east and west, as the ridge of the hill does. Its dip is about 45° south, and consequently opposite to the slope of the hill. It is easy to distinguish the outcrops of this seam on the parts of

Difference between these and volcanic fires.

* The aspect of the alum mine of Fontaines, the desolation and broken state of the ground, at first view suggest the idea of volcanic phenomena. But on a more attentive examination we perceive, that the earth has been deranged only by sinking in; that there is no fissure which has any resemblance to the mouth of a crater; that the scorification and vitrification have been effected on the spot; that the products of these two operations do not resemble lavas; that the vapours always very evidently contain bitumen, and never muriate of ammonia; that the salts formed are sulphates; that besides no detonation is ever heard, and the ground experiences no commotion that can be compared to an earthquake: in short, if we set aside the heat and light produced by the combustion of the coal, and the aqueous and acidosulphurous vapours emitted, nothing similar to volcanic phenomena ever takes place.

of the hill uninjured. They run horizontally about a third of the way down the hill. The works had not been carried very deep before the fire, but they occupied a considerable length on the outcrop.

The space deranged and altered by the fire exhibits nearly an oval figure. The shorter axis does not exceed 70 met. [76 yards]; but the greater, which is horizontal, may be 150 met. [164 yards]. The surface cannot be estimated at more than 60 ares [148 acres]. The whole of it has ceased to form a continued plane with the slope of the mountain, which is about 40°; and exhibits a depression pretty exactly resembling in figure the stern of a boat. Part of this surface is covered with solid aluminous incrustations, which resist the action of the rain in some degree, or are reproduced immediately after. It must be a rich mine, though not at present worked.

Mine of Bourlhones.

The mine of Bourlhones is the least of the four. It is half way up the hill that faces the mine of Buégne, and consequently in the same valley. Their distance from each other in a straight line is about 500 met. [546 yards.]

The fire that formed it has not continued above ten years. It is fed at the expense of a single seam of coal several yards thick, and inclining 30° or 40° east, consequently opposite to the slope of the hill.

The coal had not been worked to any extent, when it took fire. The combustion has not yet reached its highest degree of activity. The surface of the ground is partly covered with grass, partly sunk down, cracked, and torrefied. Copious vapours of water, sulphur, and bitumen, issue from it. Its shape is nearly circular, and it may be estimated at 30 ares [74 acres]. The aluminous salts are very abundant, but only in certain places; though by proper management their formation may be accelerated in others. No attempt has yet been made to work it.

Produce of the mines.

From the two mines, that are worked by two companies of adventurers, near 17000 myriagr. [167 tuns] of alum were made in 1809, which sold for about 120000 fr. [£ 5000].

VII. *The*

VIII.

The Croonian Lecture, on some Physiological Researches, respecting the Influence of the Brain on the Action of the Heart, and on the Generation of animal Heat. By Mr. B. C. Brodie, *F. R. S.* *

Influence of the brain on the action of the heart.

HAVING had the honour of being appointed, by the president of the Royal Society, to give the Croonian lecture, I trust, that the following facts and observations will be considered as tending sufficiently to promote the objects, for which the lecture was instituted. They appear to throw some light on the mode, in which the influence of the brain is necessary to the continuance of the action of the heart; and on the effect which the changes produced on the blood in respiration have on the heat of the animal body.

Not directly necessary to it.

In making experiments on animals to ascertain how far the influence of the brain is necessary to the action of the heart, I found, that, when an animal was pithed by dividing the spinal marrow on the upper part of the neck, respiration was immediately destroyed, but the heart still continued to contract circulating dark coloured blood; and that in some instances from ten to fifteen minutes elapsed, before its action had entirely ceased. I farther found, that, when the head was removed, the divided blood vessels being secured by a ligature, the circulation still continued, apparently unaffected by the entire separation of the brain. These experiments confirmed the observation of Mr. Cruickshank † and Mr. Bichat ‡, that the brain is not directly necessary to the action of the heart; and that, when the functions of the brain are destroyed, the circulation ceases only in consequence of the suspension of the respiration. This led me to conclude, that, if respiration was produced artificially, the heart would continue to contract for a still longer period of time after the removal of the brain.

* Philos. Trans. for 1811, p. 36.
† Philosophical Transactions 1795.
‡ Récherches Physiologiques sur la Vie et la Mort.

The

The truth of this conclusion was ascertained by the following experiment.

Exp. 1. On a rabbit. Communication cut off, and respiration continued artificially.

Exp. 1. I divided the spinal marrow of a rabbit in the space between the occiput and atlas, and having made an opening into the trachea, fitted into it a tube of elastic gum, to which was connected a small pair of bellows, so constructed, that the lungs might be inflated, and then allowed to empty themselves. By repeating this process once in five seconds, the lungs being each time fully inflated with fresh atmospheric air, an artificial respiration was kept up. I then secured the blood vessels in the neck, and removed the head, by cutting through the soft parts above the ligature, and separating the occiput from the atlas. The heart continued to contract, apparently with as much strength and frequency as in a living animal. I examined the blood in the different sets of vessels, and found it dark coloured in the venæ cavæ and pulmonary artery, and of the usual florid red colour in the pulmonary veins and aorta. At the end of twenty-five minutes from the time of the spinal marrow being divided, the action of the heart became fainter, and the experiment was put an end to.

No urine secreted.

With a view to promote the inquiry instituted by the society for promoting the knowledge of animal chemistry respecting the influence of the nerves on the secretions *, I endeavoured to ascertain, whether they continued after the influence of the brain was removed. In the commencement of the experiment I emptied the bladder of its contents by pressure; at the end of the experiment the bladder continued empty.

This experiment led me to conclude, that the action of the heart might be made to continue after the brain was removed, by means of artificial respiration, but that under these circumstances the secretion of urine did not take place. It appeared, however, desirable to repeat the experiment on a larger and less delicate animal; and that, in doing so, it would be right to ascertain whether under these circumstances the animal heat was kept up to the natural standard.

* Philosophical Transactions for 1809. Journal vol. xxvi, p. 136.

Exp.

Exp. 2. I repeated the experiment on a middle sized dog. The temperature of the room was 63° of Fahrenheit's thermometer. By having previously secured the carotid and vertebral arteries, I was enabled to remove the head with little or no hæmorrhage. The artificial respirations were made about twenty-four times in a minute. The heart acted with regularity and strength. Exp. 2. On a dog.

At the end of 30 minutes from the time of the spinal marrow being divided, the heart was felt through the ribs contracting 76 times in a minute. Action of the heart.

At 35 minutes the pulse had risen to 84 in a minute.

At one hour and 30 minutes the pulse had risen to 88 in a minute.

At the end of two hours it had fallen to 70, and at the end of two hours and a half to 35 in a minute, and the artificial respiration was no longer continued.

By means of a small thermometer with an exposed bulb, I measured the animal heat at different periods. Animal heat.

At the end of an hour the thermometer in the rectum had fallen from 100° to 94°.

At the end of two hours a small opening being made in the parietes of the thorax, and the ball of the thermometer placed in contact with the heart, the mercury fell to 86°, and half an hour afterward in the same situation it fell to 78°.

In the beginning of the experiment I made an opening into the abdomen; and, having passed a ligature round each ureter about two inches below the kidney, brought the edges of the wound in the abdomen together by means of sutures. At the end of the experiment no urine was collected in the ureters above the ligatures. No urine secreted.

On examining the blood in the different vessels, it was found of a florid red colour in the arteries, and of a dark colour in the veins, as under ordinary circumstances. Blood.

During the first hour and a half of the experiment there were constant and powerful contractions of the muscles of the trunk and extremities, so that the body of the animal was moved in a very remarkable manner, on the table, on which it lay, and twice there was a copious evacuation of fæces. Muscular contractions.

Exp.

Exp. 3. On a rabbit. Action of the heart.

Exp. 3. The experiment was repeated on a rabbit. The temperature of the room was 60°. The respirations were made from 30 to 35 in a minute. The actions of the heart at first were strong and frequent: but at the end of one hour 40 minutes the pulse had fallen to 24 in a minute.

Blood.

The blood in the arteries was seen of a florid red, and that in the veins of a dark colour.

A small opening was made in the abdominal muscles, through which the thermometer was introduced into the abdomen, and allowed to remain among the viscera.

Animal heat.

At the end of an hour the heat in the abdomen had fallen from 100° to 89°. At the end of an hour and forty minutes in the same situation the heat had fallen to 85°, and when the bulb of the thermometer was placed in the thorax in contact with the lungs the mercury fell to 82°.

Seemingly not dependent on chemical changes of the blood in respiration.

It has been a very generally received opinion, that the heat of warm blooded animals is dependent on the chemical changes produced on the blood by the air in respiration. In the two last experiments the animals cooled very rapidly, notwithstanding the blood appeared to undergo the usual changes in the lungs; and I was therefore induced to doubt whether the above mentioned opinion respecting the source of animal heat is correct. No positive conclusions however could be deduced from these experiments. If animal heat depends on the changes produced on the blood by the air in respiration, its being kept up to the natural standard, or otherwise, must depend on the quantity of air inspired, and on the quantity of blood passing through the lungs in a given space of time: in other words, it must be in proportion to the fulness and frequency of the pulse, and the fulness and frequency of the inspirations. It therefore became necessary to pay particular attention to these circumstances.

Exp. 4. On a small dog.

Exp. 4. The experiment was repeated on a dog of a small size, whose pulse was from 130 to 140 in a minute, and whose respirations, as far as I could judge, were performed from 30 to 35 times in a minute.

The temperature of the room was 63°. The heat in the rectum of the animal at the commencement of the experiment

ment was 99°. The artificial inspirations were made to correspond as nearly as possible to the natural inspirations both in fulness and frequency.

At 20 minutes from the time of the dog being pithed, the heart acted 140 times in a minute with as much strength and regularity as before: the heat in the rectum had fallen to $96\frac{1}{2}$.

At 40 minutes the pulse was still 140 in a minute: the heat in the rectum $92\frac{1}{2}$.

At 55 minutes the pulse was 112, and the heat in the rectum 90°.

At one hour and 10 minutes the pulse beat ninety in a minute, and the heat in the rectum was 88°.

At one hour and 25 minutes the pulse had sunk to 30, and the heat in the rectum was 85°. The bulb of the thermometer being placed in the bag of the pericardium, the mercury stood at 85°, but among the viscera of the abdomen it rose to $87\frac{1}{2}$.

During the experiment there were frequent and violent contractions of the voluntary muscles, and an hour after the experiment was begun, there was an evacuation of fæces.

Exp. 5. On a rabbit.

Exp. 5. The experiment was repeated on a rabbit, whose respirations, as far as I could judge, were from 30 to 40 in a minute, and whose pulse varied from 130 to 140 in a minute. The temperature of the room was 57°. The heat in the rectum, at the commencement of the experiment, was $101\frac{1}{2}$. The artificial respirations were made to resemble the natural respirations as much as possible, both in fulness and frequency.

At 15 minutes from the time of the spinal marrow being divided, the heat in the rectum had fallen to $98\frac{1}{2}$°.

At the end of half an hour the heart was felt through the ribs, acting strongly 140 times in a minute.

At 45 minutes the pulse was still 140; the heat in the rectum was 94°.

At the end of an hour the pulse continued 140 in a minute; the heat in the rectum was 92°; among the viscera of the abdomen 94°; in the thorax, between the lungs and pericardium, 92°.

During

During the experiment, the blood in the femoral artery was seen to be of a bright florid colour, and that in the femoral vein of a dark colour, as usual.

The rabbit voided urine at the commencement of the experiment; at the end of the experiment no urine was found in the bladder.

Exp. 6. On a large rabbit.

Exp. 6. I procured two rabbits of the same colour, but one of them was about one fifth smaller than the other. I divided the spinal marrow of the larger rabbit between the occiput and atlas. Having secured the vessels in the neck, and removed the head, I kept up the circulation by means of artificial respiration as in the former experiments. The respirations were made as nearly as possible similar to natural respirations.

In 23 minutes after the spinal marrow was divided, the pulse was strong, and 130 in a minute: the ball of the thermometer being placed among the viscera of the abdomen, the mercury stood at 96°.

At 34 minutes the pulse was 120 in a minute: the heat in the abdomen was 95°.

At the end of an hour the pulse could not be felt, but on opening the thorax the heart was found acting, but slowly and feebly. The heat in the abdomen was 91°; and between the lobes of the right lung 88°.

During the experiment, the blood in the arteries and veins was seen to have its usual colour.

Comparative experiment on a smaller.

In this therefore, as in the preceding experiments, the heat of the animal sunk rapidly, notwithstanding the continuance of the respiration. In order to ascertain whether any heat at all was generated by this process, I made the following comparative experiment. The temperature of the room being the same, I killed the smaller rabbit by dividing the spinal marrow between the occiput and atlas. In consequence of the difference of size, *cæteris paribus*, the heat in this rabbit ought to diminish more rapidly than in the other; and I therefore examined its temperature at the end of 52 minutes, considering that this would be at least equivalent to examining that of the larger rabbit at the end of an hour. At 52 minutes from the time of the smaller rabbit being killed, the heat among the viscera of the abdomen

domen was 92°, and between the lobes of the right lung it was 91°. From this experiment, therefore, it appeared, not only that no heat was generated in the rabbit, in which the circulation was maintained by artificial respiration, but that it even cooled more rapidly than the dead rabbit.

No heat generated by artificial respiration.

At the suggestion of professor Davy, who took an interest in the inquiry, I repeated the foregoing experiment on two animals, taking pains to procure them more nearly of the same size and colour.

Exp. 7. I procured two large full grown rabbits of the same colour, and so nearly equal in size, that no difference could be detected by the eye.

Exp. 7. Two large rabbits of equal size: in one artificial respiration kept up, in the other not.

The temperature of the room was 57°, and the heat in the rectum of each rabbit previous to the experiment was 100½.

I divided the spinal marrow in one of them, produced artificial respiration, and removed the head after having secured the vessels in the neck. The artificial respirations were made about 35 times in a minute.

During the first hour, the heart contracted 144 times in a minute.

At the end of an hour and a quarter the pulse had fallen to 136 in a minute, and it continued the same at the end of an hour and a half. At the end of an hour and forty minutes the pulse had fallen to 90° in a minute, and the artificial respiration was not continued after this period.

Half an hour after the spinal marrow was divided, the heat in the rectum had fallen to 97°.

At 45 minutes the heat was 95½.

At the end of an hour the heat in the rectum was 94°.

At an hour and a quarter it was 92°.

At an hour and a half it was 91°.

At an hour and forty minutes, the heat in the rectum was 90½, and in the thorax, within the bag of the pericardium, the heat was 87½.

The temperature of the room being the same, the second rabbit was killed by dividing the spinal marrow, and the temperature was examined at corresponding periods.

Half an hour after the rabbit was killed, the heat in the rectum was 99°.

At

At 45 minutes it had fallen to 98°.

At the end of an hour the heat in the rectum was 96½.

At an hour and a quarter it was 95°.

At an hour and a half it was 94°.

At an hour and forty minutes the heat in the rectum was 93°, and in the bag of the pericardium 90½.

The following table will shew the comparative temperature of the two animals at corresponding periods.

Table of their comparative temperatures.

Time.	Rabbit with artificial respiration.		Dead Rabbit.	
	Therm. in the Rectum.	Therm. in the Pericardium.	Therm. in the Rectum.	Therm. in the Pericardium.
Before the experiment.	100½		100½	
30 min. aft.	97		99	
45 ——	95½		98	
60 ——	94		96½	
75 ——	92		95	
90 ——	91		94	
100 ——	90½	87½	93	90½

The production of animal heat seems not to depend on respiration.

In this experiment, the thorax, even in the dead animal, cooled more rapidly than the abdomen. This is to be explained by the difference in the bulk of these two parts. The rabbit in which the circulation was maintained by artificial respiration cooled more rapidly than the dead rabbit: but the difference was more perceptible in the thorax than in the rectum. This is what might be expected, if the production of animal heat does not depend on respiration; since the cold air, by which the lungs were inflated, must necessarily have abstracted a certain quantity of heat, particularly as its influence was communicated to all parts of the body, in consequence of the continuance of respiration.

Objection.

It was suggested that some animal heat might have been generated, though so small in quantity as not to counterbalance the cooling powers of the air thrown into the lungs. It is difficult, or impossible, to ascertain with perfect accuracy, what effect cold air thrown into the lungs would have

have on the temperature of an animal under the circumstances of the last experiment, independently of any chemical action on the blood: since, if no chemical changes were produced, the circulation could not be maintained, and if the circulation ceased, the cooling properties of the air must be more confined to the thorax, and not communicated in an equal degree to the more distant parts. The following experiment, however, was instituted as likely to afford a nearer approximation to the truth, than any other that could be devised.

Exp. 8. Attempt to remove this.

Exp. 8. I procured two rabbits of the same size and colour: the temperature of the room was 64°. I killed one of them by dividing the spinal marrow, and immediately, having made an opening into the left side of the thorax, I tied a ligature round the base of the heart, so as to stop the circulation. The wound in the skin was closed by a suture. An opening was then made into the trachea, and the apparatus for artificial respiration being fitted into it, the lungs were inflated, and then allowed to collapse as in the former experiment, about 36 times in a minute. This was continued for an hour and a half, and the temperature was examined at different periods. The temperature of the room being the same, I killed the second rabbit in the same manner, and measured the temperature at corresponding periods. The comparative temperature of the two dead animals, under these circumstances, will be seen in the following table.

Tabulated results.

Time.	Dead Rabbit whose lungs were inflated.		Dead Rabbit whose lungs were not inflated.	
	Therm. in the Rectum.	Therm. in the Thorax.	Therm. in the Rectum.	Therm. in the Thorax.
Before the experiment.	100		100	
30 min. aft.	97		98	
45 ———	$95\frac{1}{2}$		96	
60 ———	94		$94\frac{1}{2}$	
75 ———	$92\frac{1}{2}$		93	
90 ———	91	86	$91\frac{1}{2}$	$88\frac{1}{2}$

In

No animal heat apparently produced by respiration.

In this last experiment, as may be seen from the above table, the difference in the temperature of the two rabbits, at the end of an hour and a half in the rectum, was half a degree, and in the thorax two degrees and a half; whereas, in the preceding experiment, at the end of an hour and forty minutes, the difference in the rectum was $2\frac{1}{2}$ degrees, and in the thorax 3 degrees. It appears, therefore, that the rabbit in which the circulation was maintained by artificial respiration cooled more rapidly on the whole, than the rabbit whose lungs were inflated in the same manner after the circulation had ceased. This is what might be expected if no heat was produced by the chemical action of the air on the blood; since in the last case the cold air was always applied to the same surface, but in the former it was applied always to fresh portions of blood, by which its cooling powers were communicated to the more distant parts of the body.

In the course of the experiments which I have related, I was much indebted to several members of the Society for promoting the Knowledge of Animal Chemistry, for many important suggestions, which have assisted me in prosecuting the inquiry. Mr. Home, at my request, was present at the seventh experiment. Dr. E. N. Bancroft was present at, and assisted me in the second experiment: and Mr. William Brande lent me his assistance in the greater part of those which were made. I have been farther assisted in making the experiments by Mr. Broughton, surgeon of the Dorsetshire regiment of militia, and Mr. Richard Rawlins, and Mr Robert Gatcombe, students in surgery.

Many other experiments gave similar results.

I have selected the above from a great number of similar experiments, which it would be needless to detail. It is sufficient to state, that the general results were always the same; and that, whether the pulse was frequent or slow, full, or small, or whether the respirations were frequent or otherwise, there was no perceptible difference in the cooling of the animal.

General conclusions.

From the whole we may deduce the following conclusions:

1. The influence of the brain is not directly necessary to the action of the heart.

2. When

2. When the brain is injured or removed, the action of the heart ceases, only because respiration is under its influence, and if under these circumstances respiration is arficially produced, the circulation will still continue.

3. When the influence of the brain is cut off, the secretion of urine appears to cease, and no heat is generated; notwithstanding the functions of respiration and the circulation of the blood continue to be performed, and the usual changes in the appearance of the blood are produced in the lungs.

4. When the air respired is colder than the natural temperature of the animal, the effect of respiration is not to generate, but to diminish animal heat.

Addition to the Croonian Lecture for the Year 1810. (*P.* 207.)

In the experiments above detailed, where the circulation was maintained by means of artificial respiration after the head was removed, I observed that the blood, in its passage through the lungs, was altered from a dark to a scarlet colour; and hence I was led to conclude, that the action of the air produced in it changes analogous to those, which occur under ordinary circumstances. I have lately, with the assistance of my friend Mr. W. Brande, made the following experiment, which appears to confirm the truth of this conclusion. Artificial respiration produces similar changes on the blood with natural.

An elastic gum bottle, having a tube and stop-cock connected with it, was filled with about a pint of oxigen gas. The spinal marrow was divided in the neck of a young rabbit, and, the blood vessels having been secured, the head was removed, and the circulation was maintained by inflating the lungs with atmospheric air for five minutes, at the end of which time the tube of the gum bottle was inserted into the trachea, and carefully secured by a ligature, so that no air might escape. By making pressure on the gum bottle, the gas was made to pass and repass into and from the lungs about thirty times in a minute. At first, the heart acted one hundred and twenty times in a minute, with regularity and strength; the thermometer, in the rectum, rose to 100°. At the end of an hour, the heart acted as frequently as before, but more feebly; the blood Experiment to show this.

 in

in the arteries was very little more florid than that in the veins; the thermometer in the rectum had fallen to 93°. The gum bottle was then removed. On causing a stream of the gas, which it contained, to pass through lime water, the presence of carbonic acid was indicated by the liquid being instantly rendered turbid. The proportion of carbonic acid was not accurately determined; but it appeared to form about one half of the quantity of gas in the bottle.

B. C. BRODIE.

IX.

Notes by Mr. J. H. HASSENFRATZ *on the Disoxidation of Oxide of Iron by Hidrogen Gas* *.

Disoxidation of iron by hidrogen gas.

DESIROUS of repeating the experiment of Messrs. Priestley, Chaussier, and Amadeus Berthollet, on the disoxidation of iron by hidrogen gas, I last year employed Mr. Charbaut, then a pupil of the School of Mining, to make the experiment in my presence. He proceeded in two ways: in one the iron was disoxidated by hidrogen, in the other by oil and charcoal. In the latter experiment the metal was fused by increasing the temperature, so as to obtain a button of iron.

More weight lost than in the reduction by oil and charcoal.

On comparing these two modes, I was astonished to find, that the diminution of weight of the oxidule of iron by hidrogen was always greater than that effected by oil and charcoal. The perplexity into which I was thrown by these results induced me to repeat the experiment anew. Accordingly this year I employed at the Practical School of Mining the pupil Desroches, of whose sagacity and precision I was previously satisfied, to decompose by the action of hidrogen gas oxidules of iron from the valley of Aoste, and specimens of oligist iron from Elba, while other pupils assayed the same minerals before me in the dry way. The results obtained agreed precisely with those of last year;

The experiments repeated.

* Ann. de Chim. vol. lxxiii, p. 147.

Finally

Finally, at my departure from Moutiers, I requested the pupil Desroches to make fresh experiments on the decomposition of the oxidule of iron of Cogne, and oligist iron of Elba. The official statement of these experiments, certified by engineer Leboullenger, I shall proceed to lay before the public.

Experiments on the Disoxidation of Oxide and Oxidule of Iron.

It has been said, that all metals are capable of being disoxidated by heat, and that the temperature required for their reduction is much higher than that of their oxidation. It is easy to conceive, that, if the tendency to take the aeriform state be less powerful than the attraction of the oxigen by the metal, the oxigen will be solidified, and an oxide formed: but if the elasticity be superior to the attraction, no combination, or oxidation, will take place. This occurs in the manufacture of minium: too strong a fire produces massicot, and sometimes reduces the oxide entirely. It is observable too, that, beyond a certain temperature, the time required for oxidation is in the inverse ratio of the heat. This I had an opportuity of observing in the oxidation of iron by heat last year. Having taken some pure filings of good iron, and exposed them to a graduated heat, I obtained in a very little time an addition of 32 per cent: I increased the heat and the current of air, but it was a long while before I gained 40 per cent: and it was not without a great deal of trouble, and a very long time, that I obtained the known result of 45 per cent, which I could not exceed.

Disoxidation of metals by heat.

Oxidation by heat.

But is heat alone capable of reducing all metals? This question is already decided with respect to some, which have but a feeble attraction for oxigen. As to those which retain it forcibly, it may be, that the heat requisite for their disoxidation is superior, or at least equal to that necessary for their fusion; and then it would be impossible to separate the gas from the metal.

Are all metals reducible by heat?

But if a powerful disoxidizer be employed in conjunction with caloric, so great a heat will not be required to reduce the metal: this no doubt induced the younger Mr. Ber-

 thollet

thollet to employ hidrogen gas in his experiments, which I repeated as follows.

Two specimens of native oxide of iron exposed to the action of hidrogen gas.

I took 5 gram. [77·23 grs.] of oxidulated iron of Cogne, and a similar quantity of oligist iron of Elba, and placed them in a semicircular tube with two compartments, intended each to hold one of the oxides. This tube, furnished with a long stem curved at one end, was placed in a gunbarrel open at both ends, previously cleaned, and coated externally with loam, to preserve it from oxidation. At the curved end of the stem, which answered to one of the ends of the gunbarrel, a curved tube was luted, terminating under water, and intended to afford a passage to the superfluous hidrogen gas and the vapours of the apparatus, which were collected in bottles filled with water, and resting on a perforated test, underneath which the tube opened. The gunbarrel was placed 4 inches from the grate in a furnace, the opening of which was 8 in. [8·5 Eng.] wide, and 12 [12·8] high from the grate, which rested immediately on the nozzle of a pair of forge bellows. To the other end of the gunbarrel was fitted a tube, curved likewise, communicating with a cock placed under a jar completely immersed in a tub of water, the pressure of which was intended to force out the hidrogen gas, with which the jar was kept constantly filled.

All the parts of the apparatus being securely fixed and luted, it was found to be air tight, by passing a measured portion of air from the jar into the receivers at the other extremity.

Hidrogen gas was then prepared from iron filings and diluted sulphuric acid; the furnace was filled with charcoal; the fire was kindled, and blown gently. When the gunbarrel was redhot, which might easily be seen through the glass tubes at its two extremities, the cock was closed, and the jar filled with hidrogen gas. This gas was then passed through the apparatus, by opening the cock a little. Part of the gas was absorbed; and the remainder, which was received in the bottles with the aqueous vapour that condensed in them, was returned into the jar. In this process the oxidule and oligist iron at this temperature, presenting to the gas a porous mass, which it could easily traverse, each particle

particle was surrounded with hidrogen, gave up its oxigen, and formed vapour of water, which was perceived to condense in the curved tube at the extremity of the barrel; and which, at the close of the operation, when the heat was excessive, traversed all the water in the tube and the bottles, producing wreaths of white vapour, similar to those issuing from rockets.

Care was taken to keep in the tub a sufficient quantity of water to cover the jar; and also such a quantity of gas in the jar, that the pressure should be always nearly the same, and the passage of the gas consequently uniform. The fire was gradually increased; but absorption still taking place, it was stopped when it appeared to be at a maximum. I then thought I observed, that the fire was not stronger than might have been produced in a common furnace, simply supplied with the current of air passing through the ash-hole, so that the bellows were useless. This however I mention only as a conjecture, more decisive proofs being necessary to ascertain it.

We were employed in the fatiguing operations of supplying fuel, filling the jar with hidrogen, emptying under it the bottles containing the hidrogen that had passed through the apparatus, preparing others to receive that which was constantly issuing from it, and keeping up the level of the water in the two tubs, for four hours and a half. At the expiration of this time the iron oxides having absorbed the eight bottles of hidrogen gas that had been prepared, it was necessary to put an end to the experiment: and for my own satisfaction, I dilated the end of the gunbarrel that contained the plate iron stem of the tube, and the curved end of this stem enabled me to draw out the tube with an iron wire. I weighed the iron immediately: that of Cogne weighed 4·19 gr. [64·72 grs.]; that of Elba, 3·77 gr. [58·23 grs.]

Their weight after the experiment,

The oxidule of Cogne had become altogether stony, and of a yellowish gray. Many pieces of the oligist iron had lost their metallic lustre, having turned yellowish, and acquired a duller lustre like that of silver: but I was not certain, that this iron was reduced, since no superfluous hidrogen gas had passed over.

which was perhaps incomplete,

and therefore repeated.

This induced me to continue the experiment. The apparatus was fitted up again as before; and, after I had made a considerable quantity of hidrogen gas, and taken the precautions abovementioned, the fire was kindled, and gas was passed over, till no sensible absorption took place. All the fuel remaining in the furnace was then consumed, by continuing the action of the bellows; and during this combustion gas was still passed over, that no water might introduce itself into the gunbarrel during its cooling, which was thus effected very gradually. The jar was cooled full of gas, and the apparatus taken to pieces as before.

Weights.

The oxidule of Cogne now weighed 3·69 gr. [56·99 grs.] and the oligist iron of Elba 3·32 gr. [51·28 grs.]

State of the iron of Cogne,

The oxidulated iron of Cogne had altogether lost its metallic lustre: its yellowish aspect exhibited spots separable from the yellowish gray ground, which, examined with a lens, exhibited a sort of metallic arborizations of the colour of cast iron. On hammering it acquired lustre, and flattened, but at length broke (owing, no doubt, to the impurities of the ore). Its fracture was then very brilliant, and resembling that of iron.

and of that of Elba.

The iron of Elba had likewise lost its metallic lustre, but had assumed a duller, resembling that of silver. Some parts had the appearance of a sponge, coloured superficially with a fugitive tint, varying from yellow to that of coarse Prussian blue, and thence to violet. All its species were malleable, and were reduced thinner under the hammer than the iron of Cogne before they broke. After the experiment the specimens were analysed, to determine exactly the quantity of iron they contained.

The iron of Cogne analysed.

The 3·69 gram. [56·99 grs.] of iron of Cogne were treated with nitromuriatic acid. A large quantity of nitrous acid was evolved in red fumes, which proved the great disoxidation of the oxidule. That nothing might be lost, it was not levigated; which did not prevent the action from being brisk, and completed in a few hours, even without heating. This was necessarily the case; for, the iron having been rendered very porous by the process of disoxidation, every particle of the metal was separated, as it were from the rest, and from the earthy particles, so that the acid

acid could act on them with facility. Having evaporated to dryness, water was added, and a little muriatic acid, to take up the oxide of iron separated by drying. A whitish granular precipitate was obtained; which, collected on a filter, washed and calcined, became very white, and weighed 0·36 of a gram. [5·56 grs.] This was silex. The iron of Cogne analysed.

The solution, of a fine orange yellow colour, was saturated by ammonia; only taking care to leave a slight excess of acid, to hold in solution all the earths, that might have fallen down with the oxide of iron. This oxide was collected on a filter; and the liquor assayed by carbonate and oxalate of ammonia to detect the presence of alumine and lime. No precipitate being thrown down, the liquor was evaporated to dryness; and the muriates, oxalates, and carbonates of ammonia and magnesia (for, if there were any earth present, it could only be magnesia) were afterward calcined. The ammoniacal salts were volatilized; and a substance was left (it was an oxalate), which, having been again calcined on a porcelain test, became white, and weighed 0·31 of a gr. [4·79 grs.] It was magnesia.

As the oxide of iron remaining on the filter might still contain other metals and earths, it was treated by acetic acid, and heated to dryness. Water was then added, and it was heated to dryness again. Lastly, after having added more water, cleaned the capsule, and heated a little; the solution was filtered, evaporated to dryness, and the residuum calcined on a porcelain test. The whole was volatilized, except a blackish, alkaline substance, incapable of being weighed, which was presumed to be lime (proceeding from the filtres) contaminated by the carbon of the decomposed acetic acid.

The iron left on the filtre was treated with muriatic acid, because it was suspected to contain silex; for the nitromuriatic acid might have dissolved a portion of this earth in its state of disintegration, and the ammonia would have precipitated the silex with the iron. This in fact was the case: for, after having filtered the solution of iron, there was a residuum, which, when washed and calcined, became very white, and weighed 0·2 of a gram. [3·09 grs.]; and this was silex.

The

The iron was precipitated by ammonia, which was boiled on it repeatedly to remove the acid; and, after calcination in the open air, 4·08 gr. [63·02 grs.] of fine red oxide of iron were obtained.

Results.

Thus the oxidulated iron of Cogne yielded

Red oxide of iron -	4·07 *	grammes	= 62·86 grains
Silex - - -	0·56		= 8·65
Magnesia - -	0·31		= 4·79

Accordingly it contained 0·87 of a gr. [13·44 grs.] of earth: and consequently of the 5 gr. [77·23 grs.] employed only 4·13 gr. [63·75 grs.] were oxidule. Now in the experiment of the disoxidation the 5 gr. [77·23 grs.] were reduced to 3·69 [56·99 grs.]: 4·13 gr. [63·75 grs.] of oxidule therefore contained 1·31 gr. [20·23 grs.] of oxigen (lost in the experiment;) and consequently the oxidulated iron of Cogne is at $\frac{13100}{413}$ per cent, or 31·72 per cent.

Results of the analysis of the iron of Elba.

In like manner the oligist iron of the isle of Elba yielded by analysis

Red oxide of iron -	4·4	grammes	= 67·96 grains
Silex - - -	0·25		= 3·86.

Thus, as there were 0·25 of a gr. [3·86 grs.] of earth, there were only 4·75 gr. [73·37 grs.] of oxide in the substance employed: and, as the 5 gr. [77·23 grs.] were reduced in the experiment to 3·32 gr. [51·28 grs.], they had lost 1·68 gr. [25·95 grs.]; consequently there were 1·68 gr. [25·95 grs.] of oxigen to 4·75 gr. [73·37 grs.] of oxide. The oligist iron of the isle of Elba therefore has $\frac{16800}{475}$ per cent of oxigen, or 35·37 per cent nearly.

If we may be allowed to depend on these results, we may conclude, that the oxidulated iron of Cogne contains 32 of oxigen in 100 of the oxidule; and that the oligist iron of Elba contains 35 of oxigen in 100 of oxide.

Other Results.

Results from the iron of Cogne,

It has been seen, that there were 4·13 gr. [63·75 grs.] of oxidule in the iron of Cogne, and that this iron was oxidized in the proportion of 31·72 per cent. It has appeared too, that the 3·69 gr. [56·99 grs.] of iron of Cogne

* Just above it is said 4·08 gram. C.

obtained

obtained by disoxidation contained 0·87 [13·44 grs.] of earth; consequently there were 3·69 − 0·87 = 2·82 gr. [43·56 grs.] of pure iron. In the analysis of this iron 4·07 gr. [62·86 grs.] of red oxide were obtained: the red oxide therefore contained 4·07 − 2·82 = 1·25 gr. [19·31 grs.] of oxigen; and consequently was at $\frac{12500}{282}$ per cent, or 44 per cent and upward, (allowing for any trifling errour).

As to the iron of Elba, we find by calculation, that the red oxide obtained was at 43 per cent and upward, allowing likewise for any trifling errour; and if we take the mean of the two results, admitting decimals and allowing for any little errour, we shall find, that the red oxide is at 44 per cent.

and that of Elba.

In some troublesome experiments, which I shall not describe, I was employed to obtain hidrogen by the decomposition of water. For this purpose I took some very fine iron wire, which I weighed and introduced into a gunbarrel, adapted to this a retort filled with water, and proceeded in the usual way. After the process I had a wire extremely increased in size, consisting of an assemblage of octaedral crystals so small as to be visible only by a lens, and forming a fragile wire oxided in all parts. I weighed it, and as there were still some parts that had been less heated, and not perfectly oxided, I pulverised the oxidule, subtracted the iron thus separated, and on calculation found I had an oxidule of 32 per cent and upward.

Hidrogen obtained by passing water over iron wire.

State of the iron.

DESROCHES.

This is to certify, that these experiments were made at the laboratory of the School of Mines in the month of August, 1809.

LE BOULLENGER.

Observations by Mr. HASSENFRATZ.

It follows from the experiments of Mr. Desroches, that the oxidule of Cogne lost 0·317 of oxigen, which amounts to 46 parts to 100 of iron; and that the oligist iron of Elba lost 0·3537, which would make more than 54 to 100 of iron.

Observations by Mr. Hassenfratz.

The

The oxidule of Cogne, treated with charcoal, in one experiment yielded from 5 gr. [77·23 grs.) a button containing 3·42 gr. [52·82 grs.] of iron, and 0·66 of a gr. [10·19 grs.] of scoriæ, which would make the loss about 27 to 100 of iron; and in another experiment the 5 gr. yielded a button containing 3·38 gr. [52·21 grs.] of iron, and 0·78 [12·05 grs.] of scoriæ, making the loss 25 to 100 of iron. We will take the highest term, 27.

The oligist iron of Elba yielded from 5 gram. a button of iron weighing 3.6 [55·6 grs.] and 0·1 [1·54 gr.] of scoriæ; which would make the loss 30 to 100 of iron.

More loss in the reduction by hidrogen than in that by charcoal.

Thus the difference of loss in the two modes of reducing the oxide of iron would be for the oxidule of iron of Cogne 46 by hidrogen, and 27 by charcoal; and for the oligist iron of Elba 54 by hidrogen and 30 by charcoal.

Possible causes of the difference.

With regard to the causes, that may produce this difference, we may distinguish three: 1, the charcoal, that combines with the iron, when the metal is fused with this combustible: 2, the oxigen, that may remain combined with the iron in the metallic button obtained: 3, the action of the hidrogen on the iron, the gas dissolving and carrying off some of the metal.

Addition to the iron by carbon,

Desirous of knowing what might be the influence of each of these causes, I fused in a crucible lined with charcoal 5 gr. of iron wire previously soaked in oil, and obtained a button weighing 5·13. Hence it follows, that somewhat less than 0·03 of carbon was combined with it.

and by carbon and oxigen.

I afterward dissolved 5 gr. of iron in nitric acid, in order to oxidate the metal to a maximum; moistened the oxide with oil; placed it in a crucible lined with charcoal to fuse it; and obtained a button weighing 5·2: consequently 0·04 of carbon and oxigen had combined with the iron.

Supposing, that 0·04 of carbon and oxigen remained in the buttons obtained from the oxidule of Cogne and the oligist iron of Elba, it would follow, that the oxidule of Cogne had lost near 32 per cent of oxigen, and the iron of Elba near 36.

Difference between the two ores.

These two results agree in placing the oxidule of Cogne in the rank of black oxides obtained by the decomposition of water over iron; for this proportion of 32 is nearly what

what I have deduced from the experiments of several able chemists on the composition and decomposition of oxidules of iron. It is also the same as Mr. Desroches has deduced from the experiments he made this year at Moutiers.

It follows too from these experiments, that the oligist iron is more oxided than the oxidule, as the learned Mr. Haüy had concluded from the colour of these two ores when powdered.

Loss in the reduction by hidrogen not thus accounted for.

But when we have taken account of the carbon and oxigen combined in the metallic button obtained from the disoxidation of oxides of iron by charcoal, it appears, that the loss they undergo in their reduction is still less than that which occurs when they are disoxided by hidrogen; since in the latter case the oxidule of Cogne lost 46 to 100 of iron, while it lost but 32 in the reduction by charcoal; and the iron of Elba lost 54 with hidrogen, and only 36 with charcoal.

Iron apparently dissolved and carried off by the hidrogen.

Is this difference ascribable to the solvent action of hidrogen? Some observations seem to warrant this conclusion. 1, When the hidrogen gas obtained by the decomposition of water passed over iron, or by dissolving this metal in acids, or otherwise, is preserved in jars over water, the interior of the jars sometimes becomes coated with a slight stratum of oxide of iron. 2, At the end of the account of his experiments Mr. Desroches had added the following note. "A great deal of ferruginous hidrogen gas was evolved, as I found by its smell; so that probably some iron was lost in the passage of the hidrogen gas through it."

This deserves inquiry.

I do not think however, as Mr. Desroches observes, that we should hastily conclude hidrogen to have a solvent action on iron from his experiments alone. They should be repeated and varied in several ways, before we decide on a fact of such importance. It is sufficient for me at present to have called the attention of chemists to a result, that is worthy their consideration.

X. *Determination*

X.

Determination of the Quantity of Hidrogen and of Ammonia contained in the Amalgam of Ammonia: by Messrs. Gay-Lussac and Thenard*.

Quantity of hidrogen contained in amalgam of ammonia.

We took 3·069 gr. [47·403 grs.] of mercury, placed them in a small cupel of sal ammoniac at the negative pole, and, when their bulk was about quintupled, threw them into a conical glass filled with water, in which was previously placed a small jar also filled. The bubbles of air, that might have been adherent to the button of amalgam, were at first suffered to escape, by keeping the jar close to the sides of the glass; after which the jar was raised, so as to let the button fall to the bottom, and all the hidrogen gas arising from it was collected gradually in the upper part of the jar. Six buttons of amalgam, each made with a similar quantity of mercury, and treated in this manner successively, produced such a quantity of hidrogen, that the mercury had absorbed 3·47 times its bulk of this gas in passing to the state of soft amalgam. To avoid every source of errour, the bulk of the mercury employed and of the hidrogen collected was measured in the same tube, which was accurately graduated.

A second experiment, made also with six buttons of soft amalgam, having afforded results scarcely differing from the preceding, they may be considered as exact, or at least as approaching very nearly to the truth. It may happen however, that, on a repetition of these experiments, other numbers than ours may be found; and this must necessarily be the case, if the amalgam were not made so as to obtain it soft, or so that the mercury entering into it should have its bulk at least quintupled.

Quantity of ammonia contained in amalgam of ammonia.

We imagined at first, that by amalgamating a given quantity of mercury and deducting the known weight of the mercury and the hidrogen it contained, we should find exactly the quantity of ammonia entering into the amalgam. But we soon

* Annal. de Chim. vol. lxxiii, p. 209. Extracted from a paper read to the Institute, September, 1809.

found

found, that this mode of analysis was very inaccurate: 1st, because the amalgam is half destroyed before it is well dried: 2dly, because this amalgam displaces a volume of air, of which it is difficult to take account: 3dly, and lastly, because, on introducing it into the phial, the hidrogen and ammoniacal gas evolved take the place of a quantity of air, which cannot be estimated, and must necessarily occasion great errours in the results. Hence the weights of all differed from one another. One gave us on 3·069 gr. of mercury an augmentation of 0·002; another, an increase of 0·003; a third, of 0·0045; and a fourth, of 0·001 only. It is even possible, that a loss of weight might appear, since the air of the phial is replaced by hidrogen and ammoniacal gas. Such no doubt were the causes of Mr. Davy's mistake, when he found that mercury, in forming an amalgam, was increased only a twelve-thousandth of its weight.

Mode of analysis employed.

Impelled by these reasons to reject this mode of analysis, we employed the following, which we consider as very exact. Knowing the quantity of hidrogen contained in the ammoniacal amalgam; and not doubting, that the hidrogen and ammonia were in a uniform proportion to each other in this amalgam, we had recourse to this proportion, to determine the whole quantity of the ammonia it contained. For this purpose we converted into amalgam 3·069 gr. [47·403 grs.] of mercury; after the amalgam was well dried with blotting paper, we introduced it immediately into a small jar very dry, and a quarter filled with mercury; and immediately too clapping a finger on the mouth of the jar, we shook the whole together for a few minutes. In this way the portion of amalgam that still subsisted was decomposed, the hidrogen and ammonia it contained returning to the state of gas; for the moment the little jar was immersed in mercury and unstopped, the mercury was seen to sink. Three other similar experiments were made, in order to obtain more decisive results; and after each experiment the gasses were passed into one and the same very dry tube filled with mercury. When they were thus all collected in the tube, the quantity of ammonia they contained was determined by agitating them with water. Then,

to

to know exactly the quantity of hidrogen present, which, in this residuum, was mingled with a great deal of common air, it was burnt in Volta's eudiometer, with an addition of hidrogen and oxigen in known quantities, in order to render the combustion complete and more easy.

Results.

Thus we found, that in these gasses the ammonia was to the hidrogen as 28 to 23. But as we knew, that the mercury absorbed 3·47 times its bulk of hidrogen in passing to the state of soft amalgam, it follows, that, in acquiring this state, it absorbs at the same time 4·22 times its bulk of ammoniacal gas: and consequently the mercury, in passing to the state of amalgam, is increased in weight about 0·0007; while from the experiments of Mr. Davy it is increased only a twelve-thousandth. Our increase too is a minimum; for it is very possible, that a part of the ammonia is absorbed in the course of our experiment. Though this increase is very small, it would appear sufficient to explain the formation of the amalgam, if it be considered, that hidrogen and ammonia are very light substances; and that, being retained in this amalgam by a very weak affinity, they are scarcely more condensed than in the free state.

XI.

On the Decomposition of some vegetable or animal Substances subjected to the Action of Heat: by Mr. Gay-Lussac*.

Some substances partly decomposed by heat, partly volatilized.

WHEN certain substances belonging to the vegetable or animal kingdom, as oxalic acid, indigo, &c., are subjected to distillation, part is decomposed, and part is volatilized without alteration. To prove, that this is not owing to the impurity of these substances, we have only to distil anew what was volatized, and we shall find as much in proportion decomposed as the first time; so that, if the process be frequently repeated, we shall obtain a complete decomposition. These facts, though very remarkable, have

* Ann. de Chim. vol. lxxiv, p. 189. Communicated to the Society of Arcueil, November, 1809.

not

not sufficiently engaged the attention of chemists: I will therefore endeavour to explain them from the principles I have laid down in a paper on the volatilization of substances, printed in the first volume of the Society of Arcueil. The question to be solved is this: Why, when certain substances of the vegetable or animal kind are distilled, is part decomposed, and part volatilized? Why are they not entirely volatilized, or entirely decomposed?

Why?

The substances, that present to us this kind of alteration are volatile, and at the same time capable of being decomposed by heat. Farther, a substance cannot be volatilized below the point at which its vapour has a degree of elasticity sufficient to overcome the weight of the atmosphere, unless this vapour can mix with the air, or some other elastic fluid.

Volatilization effected by heat alone,

or assisted by gas.

Now if a substance, that is both volatile and capable of being decomposed, be subjected to the action of heat, it may happen either that it will be completely volatilized, before it experiences a sufficient degree of heat to decompose it; or that it will be decomposed, before its vapour has acquired a sufficient elasticity, to overcome the pressure of the atmosphere.

A substance may be volatilized without decomposition, or the contrary.

In the first case there is no difficulty: it is that of the distillation of acetic acid, alcohol, ether, volatile oils, &c. As to the substances included in the second, as indigo, the oxalic, gallic, and succinic acids, wax, suet, fixed oils, &c. they begin to be decomposed, before they are volatilized: but, as their decomposition produces gasses, these gasses will cause the volatilization of the part not decomposed, in the same manner as the air causes that of water below its boiling point.

First case.

Second.

Cause of a partial volatilization:

Since the gasses that result from the decomposition of a substance are the cause of its volatilization, and withdraw it from complete destruction; and as all elastic fluids possess the same properties in this respect; it is easy completely to volatilize indigo, several vegetable acids, and many other substances, without their undergoing any alteration. It is sufficient, to keep their temperature a little below that at which they are decomposed, and to cause a current of some elastic fluid, that has no chemical action on them, to pass through them.

which may be prevented.

These

These observations will be found, no doubt, capable of frequent application. It was from not being acquainted with them, that Mr. Chevreul has given an explanation of the action of caloric on indigo, which is by no means satisfactory.

XII.

Remark on Mr. MOORE'S *Paper on the Motion of Rockets. In a Letter from a* CORRESPONDENT.

To MR. NICHOLSON.

SIR,

Remark on Mr. Moore's paper on rockets.

YOUR readers undoubtedly feel much indebted to Mr. Moore for some ingenious papers upon the motion of rockets. As the subject is an important and curious one, it is highly deserving of accurate investigation. On this account I am desirous of pointing out to Mr. Moore's notice, as early as possible, an errour into which he has inadvertently fallen. In his investigation of the resistance opposed to a cylinder moving in a fluid, in a direction inclined to the axis, he expresses the sine of the angle P T *n* (see fig. 2, Plate vii,) in terms of the sines and cosines of P T Q, and Q T *n*; forgetting that the three angles are in *different planes*, and consequently that the trigonometrical formula, to which he refers, will not apply.

I am, Sir,

Yours, &c.

ZENO.

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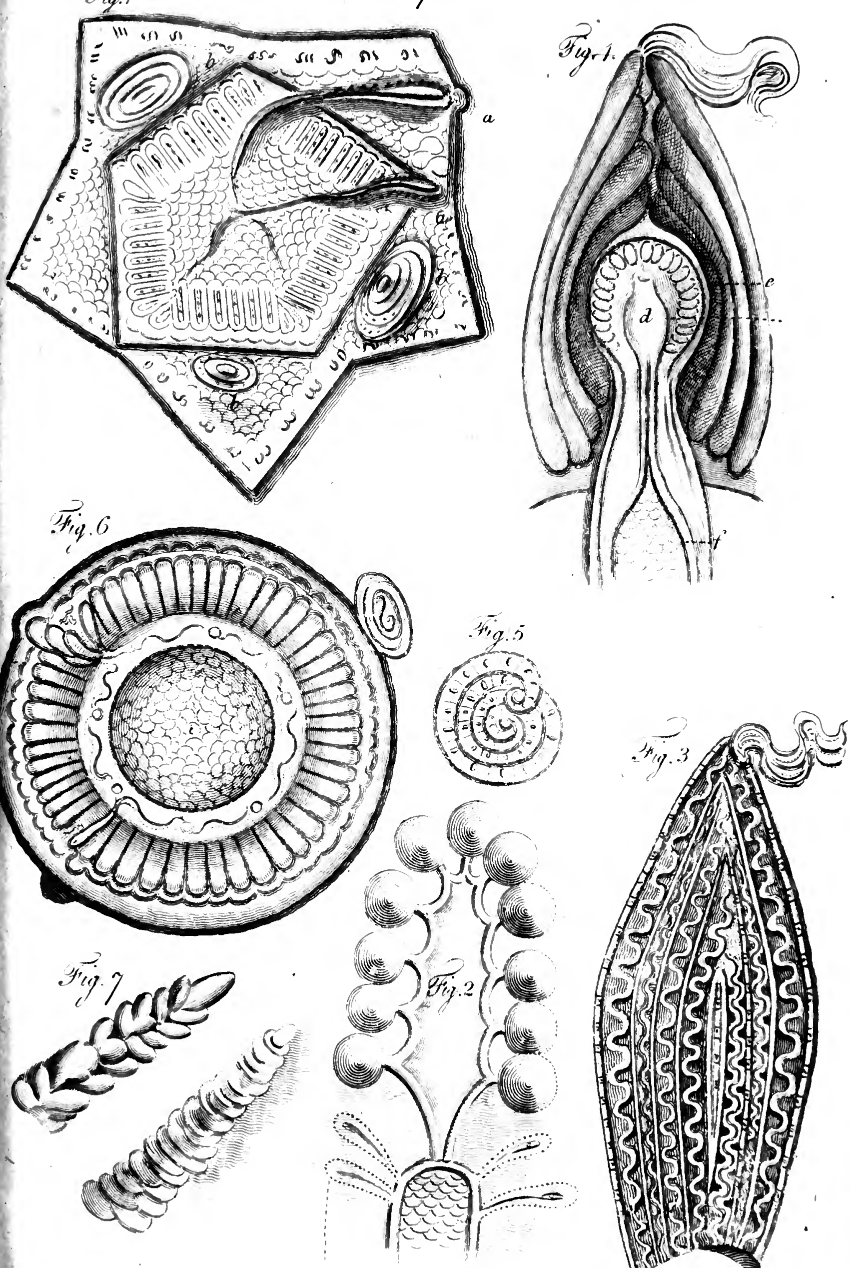
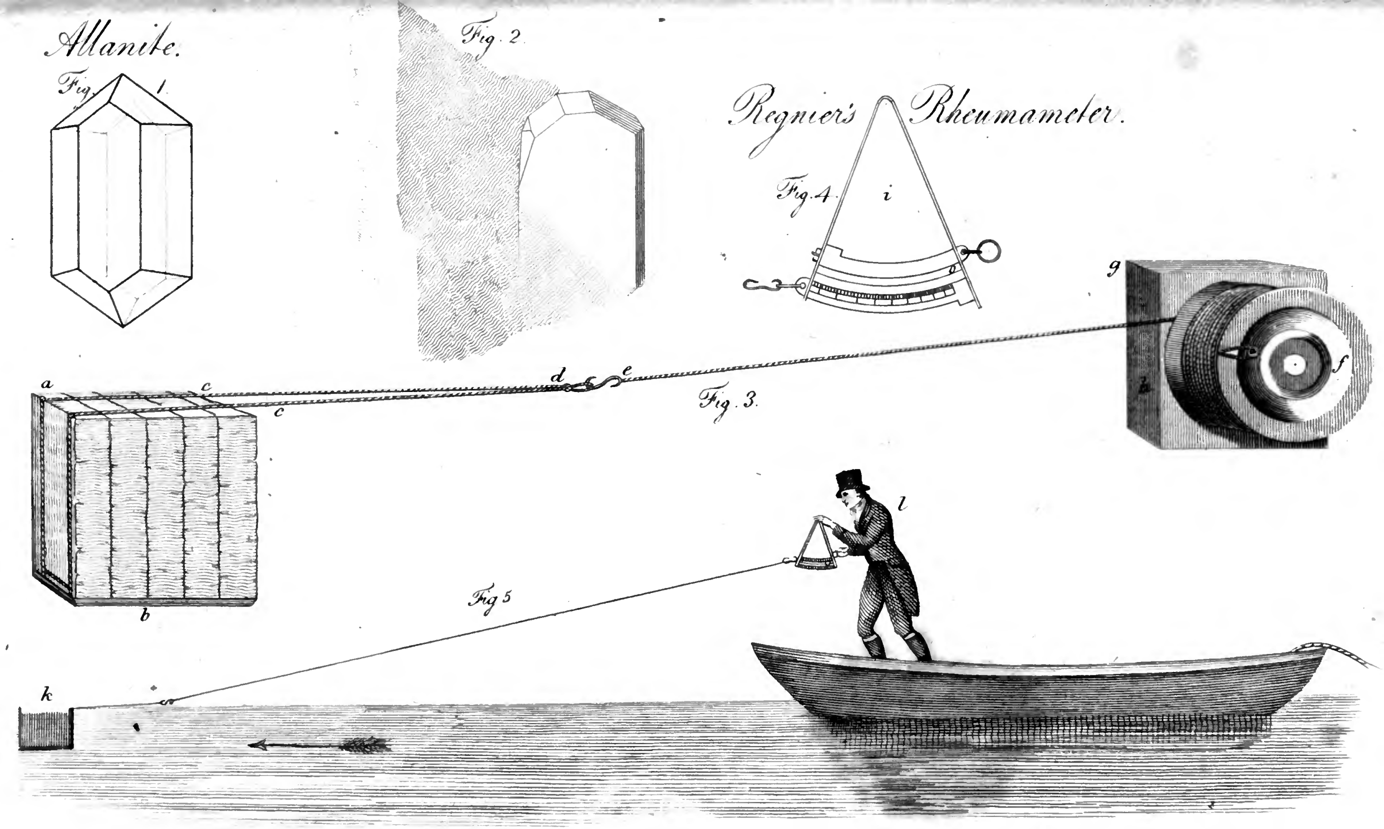
Structure of Plants.




Allanite.
Fig. 1
Fig. 2
Regnier's Rheumameter.
Fig. 4
i
o
g
f
a
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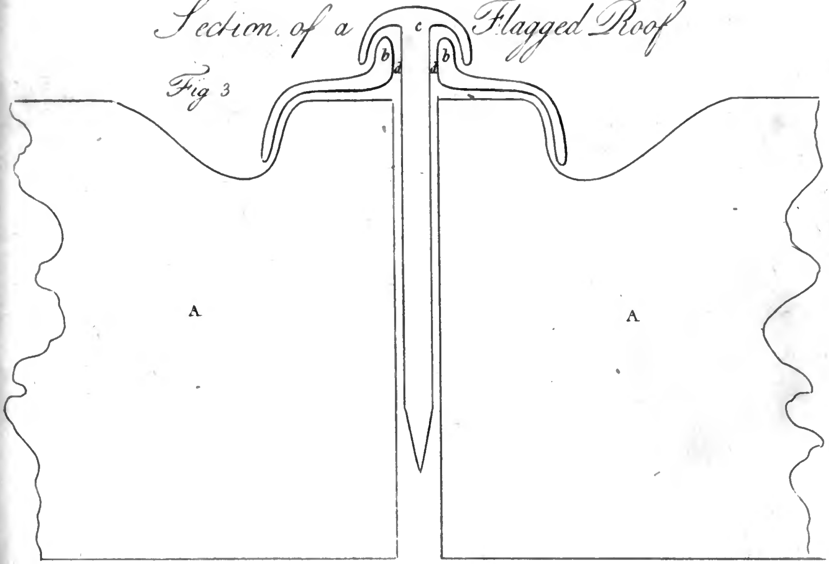
c
d
e
Fig. 3
b
l
Fig 5
k

Section of a Flagged Roof

Fig 3



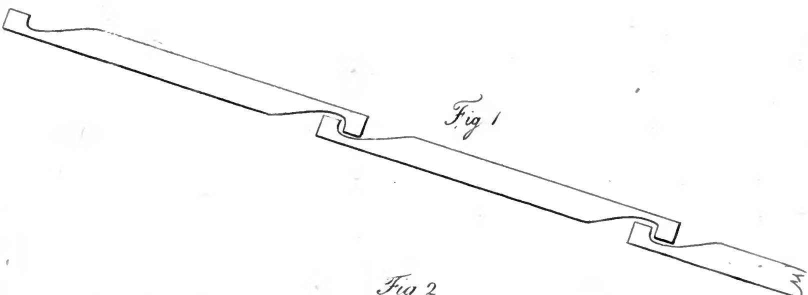


Fig 1

Fig 2

Fig 4

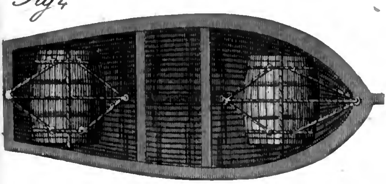
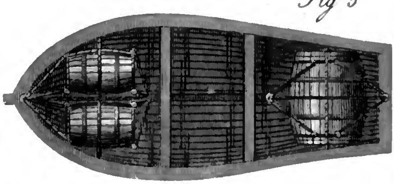
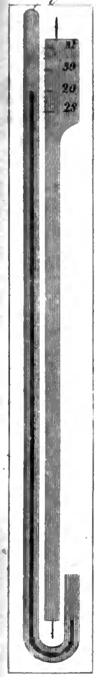


Fig 5



Revd Mr Bremners Life Boat

Fig 1



Barometer

Fig 2 Airpump

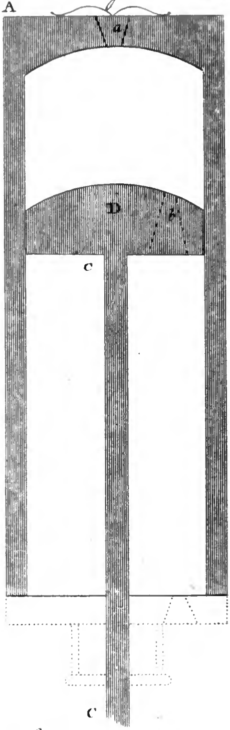
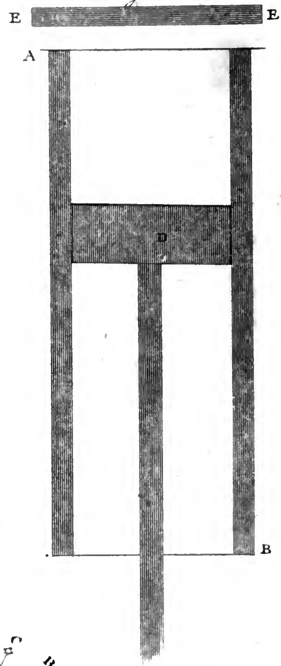
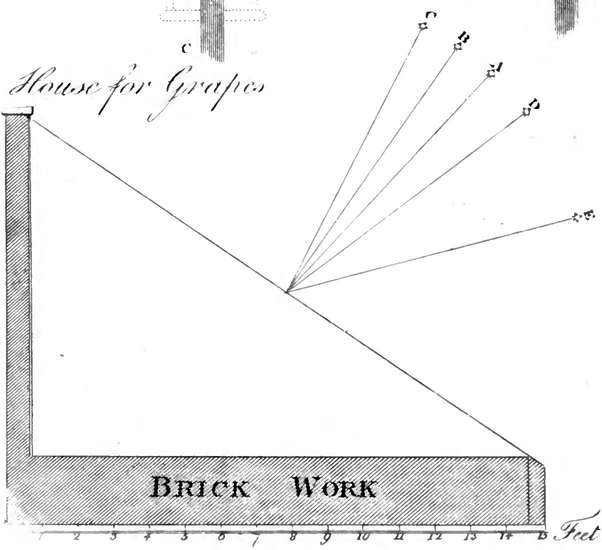


Fig 3



Forcing House for Grapes

Fig 4



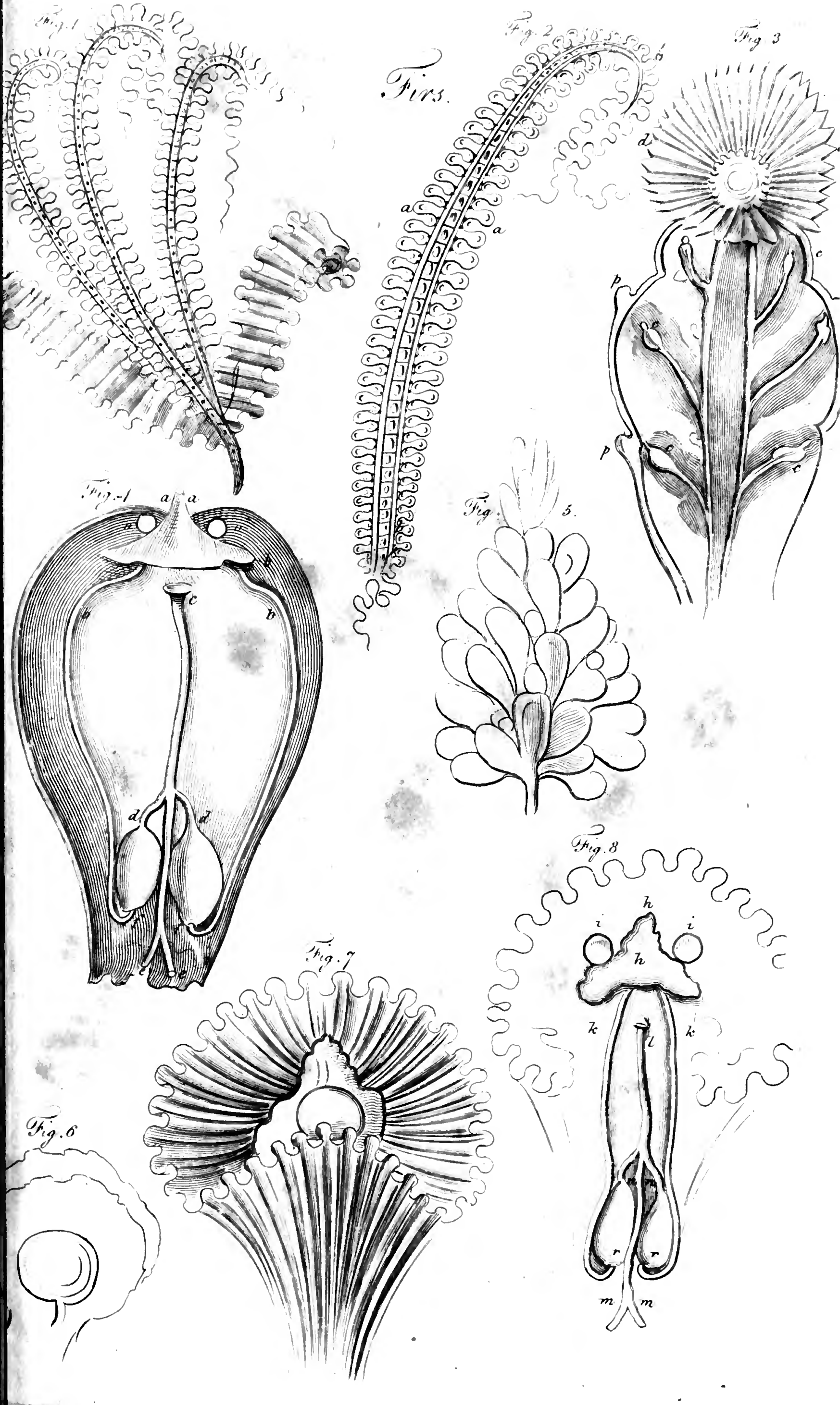

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Fig. 2
Fig. 3
Fig. 4
Fig. 5.
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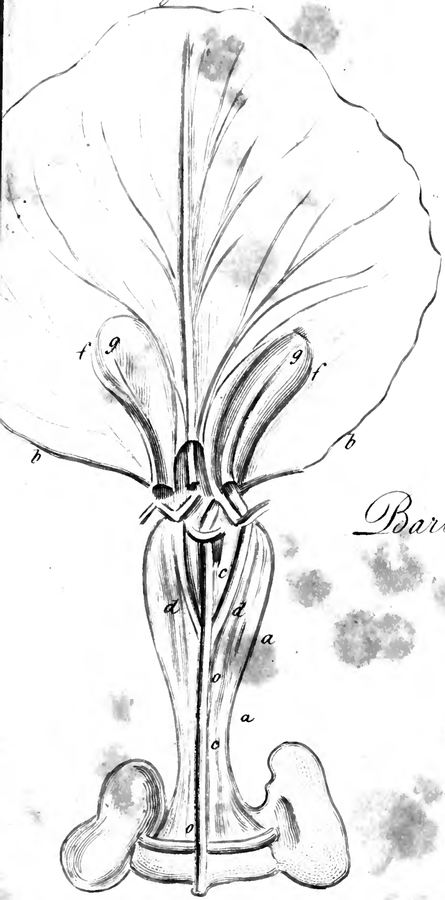
Fig. 8

Fig 3



Cypreſs Catkin.

Fig 1

Barberry.

Fig 2

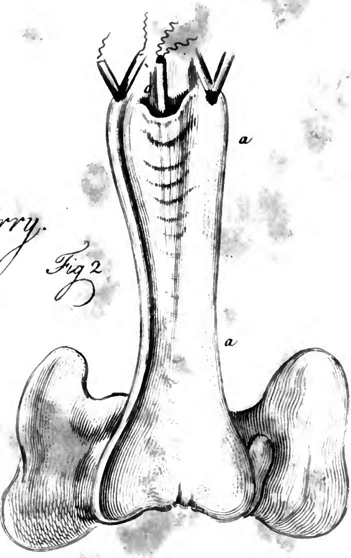
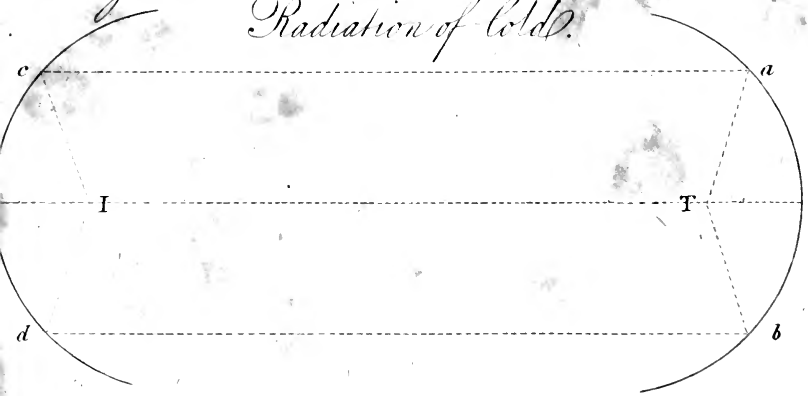
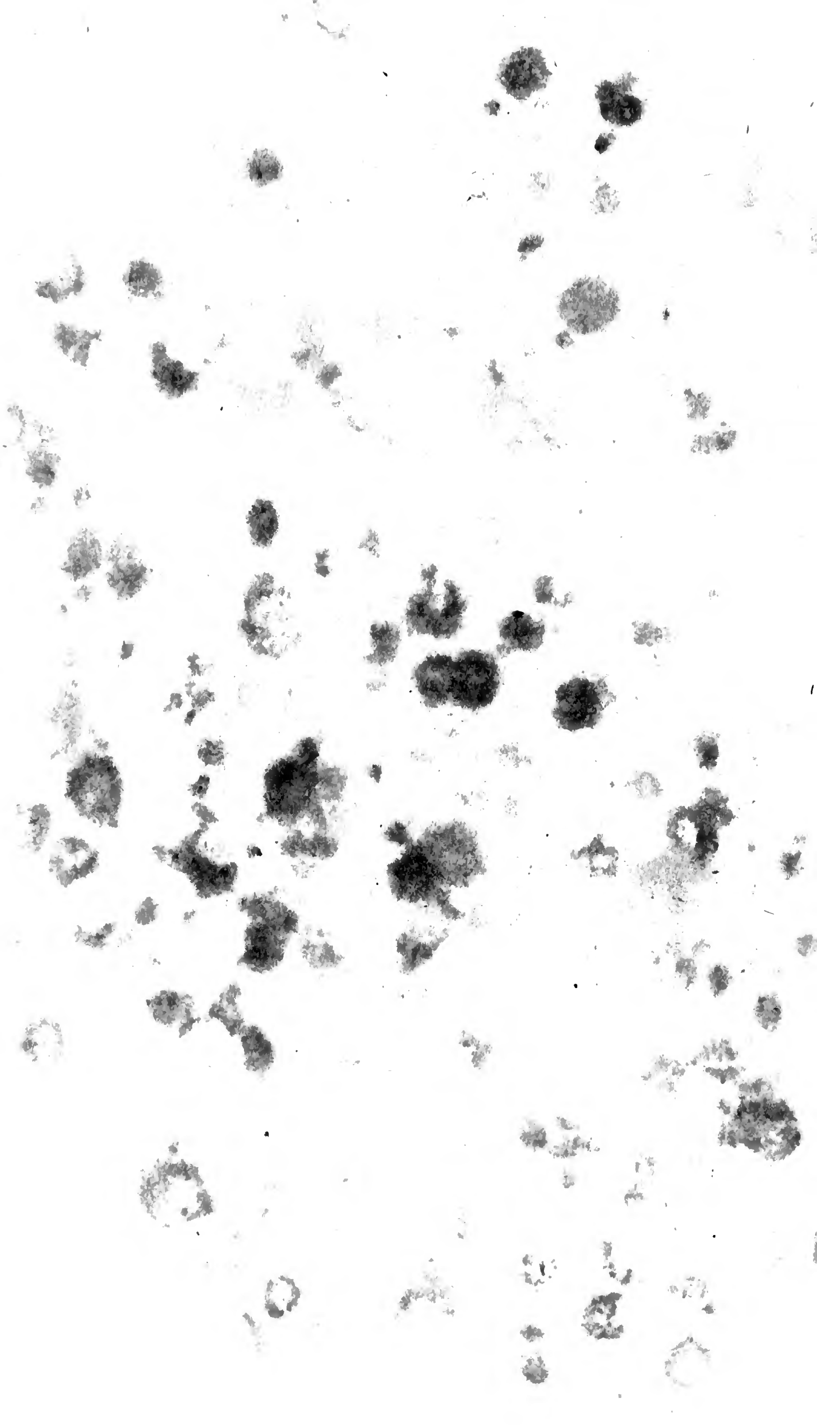


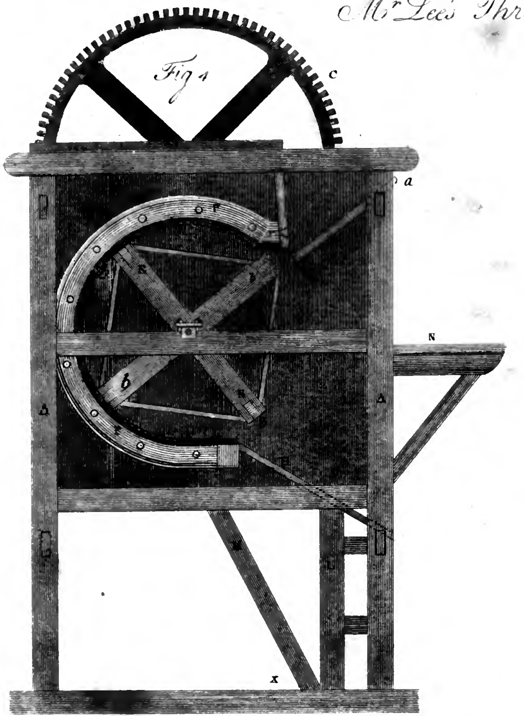
Fig. 4

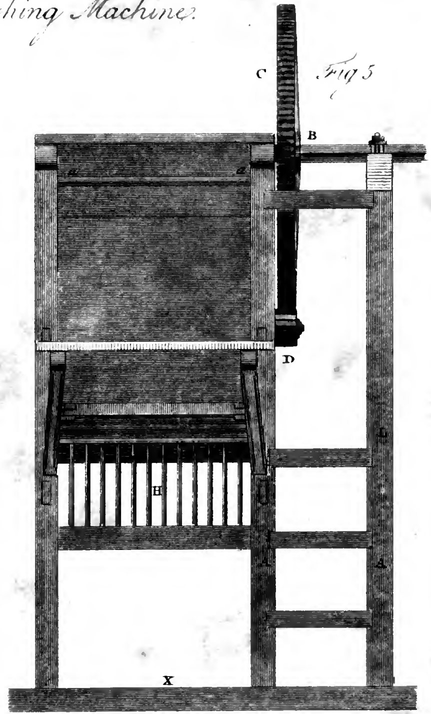
Radiation of Cold.



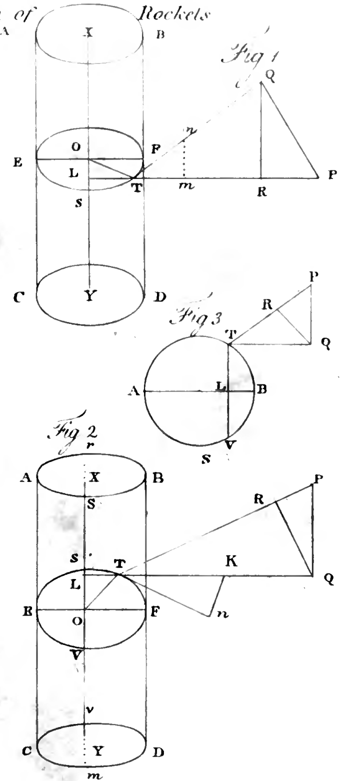


Mr Lee's Thrashing Machine.





Motion of Rockets



Mr. F. Ball's Screw adjusting Plough.

Fig. 1.

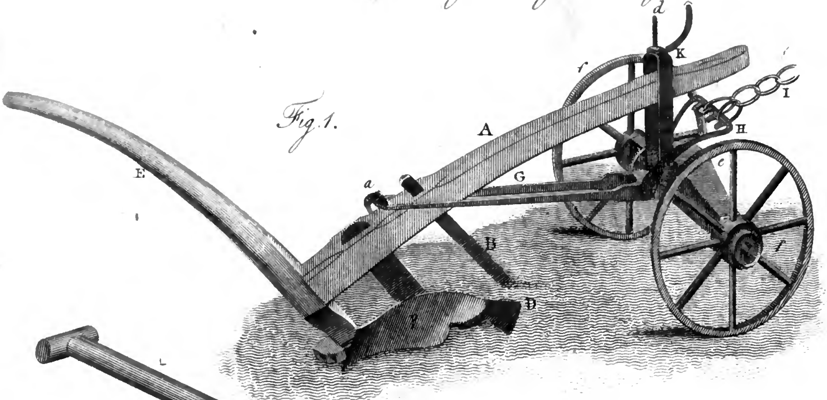
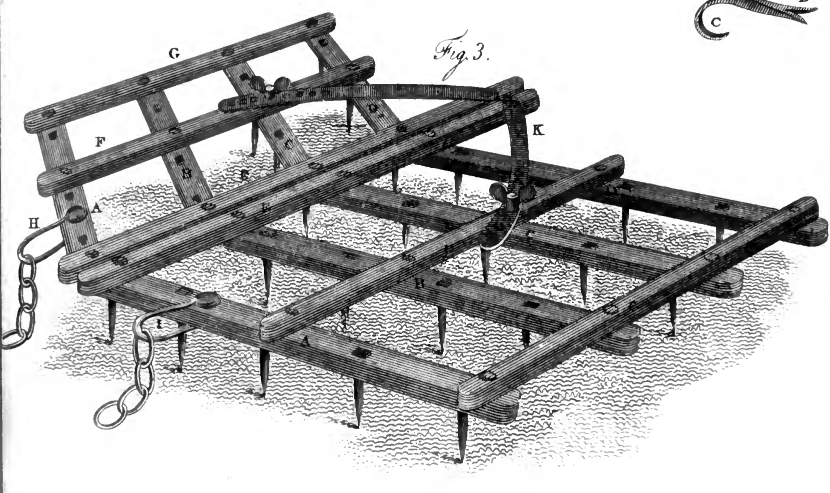


Fig. 2.

Mr. Baker's Thistle Extirpator.

Mr. Jeffery's Expanding Harrows

Fig. 3.



Mr. J. Davis's Fire Escape

Mr. Moults Filtering Apparatus.

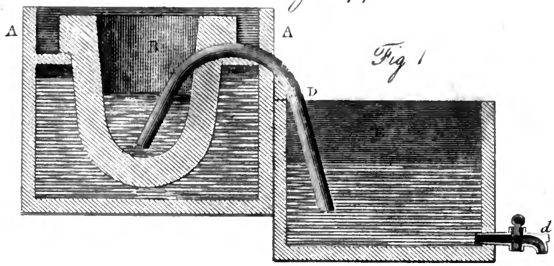


Fig. 1

Mr. Smiths method of relieving a Horse which has fallen in the shaft of a loaded Cart

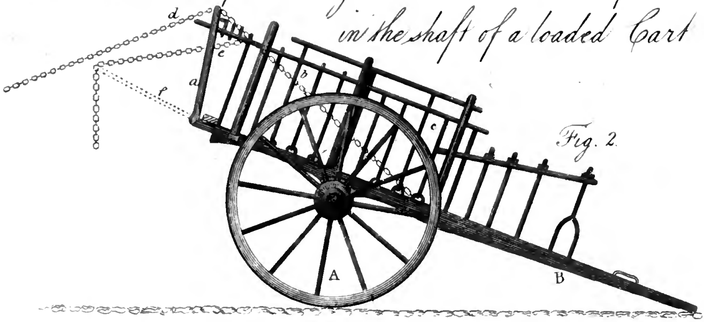
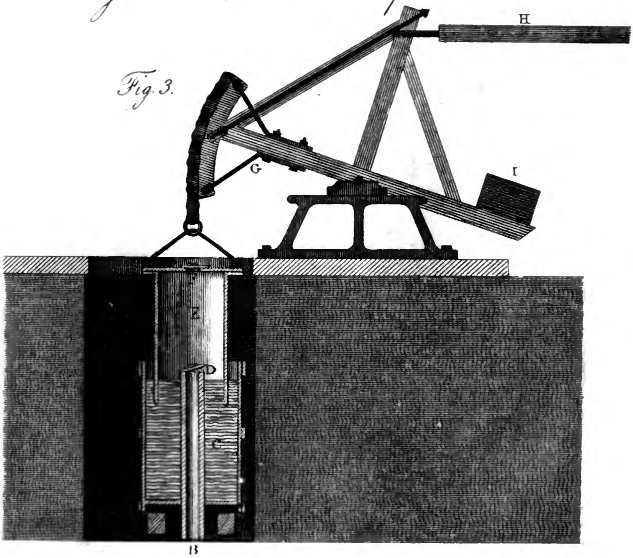


Fig. 2.

Mr. Taylors Air Exhauster for Mines.

Fig. 3.



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END OF THE TWENTY-NINTH VOLUME.

Stratford, Printer, Crown Court, Temple Bar.