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CHEMISTRY,
AND
THE ARTS.

VOL. XVIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, Aletes; William Ramsey; John Gough, Esq.; Hydrophilus; James Parkinson; R. B.; Tyro; Apsophus; Sir George Cayley, Bart.; Mr. Robert Harrup; O. N.; Thomas Young, M. D. F. R. S.

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And of British Memoirs abridged or extracted, John Bostock, M D.; Mr. Argand; Thomas Young, M. D.; John Maher, F. H. S.; Sir Joseph Banks, Bart. K. B. P. R. S.; Thomas Andrew Knight, Esq. F. R. S.; Mr. R. Salmon; Mr. John Prior; Rev. James Hall; Humphry Davy, Esq. F. R. S. M. R. I. A.; Mr. Benjamin Stott.

The Engravings consist of 1. Capillary Action of Fluids, by Aletes; 2. Boullay's Apparatus for Phosphoric Ether; 3. Mr. Gough's Chamber Barometer; 4. The Proteus Auguinus; 5. Mr. Maher's Blanching Pot; 6. Three Representations of the Sacro Catino; 7. Universal Tide Table; 8. General Table of Lunations; 9. Mr. R. Salmon's Geometrical Quadrant and Staff; 10. Mr. John Prior's Larum for Pocket Watches; 11. Structure of Covered Ways; 12. Sir George Cayley's Expansion Air Engine; 13. Mr. Stott's Engine for splitting Sheep Skins.

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OCTOBER, 1807.

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A Duplicate of the Plate of the Tide and Lunar Tables is given, that it may be cut out and put together, either as it is, or mounted on pasteboard. without rendering the Work imperfect.

NOVEMBER.

NOVEMBER, 1807.

Engravings of the following Objects: 1. Mr. Salmon's Geometrical Quadrant and Staff: 2. Mr. John Prior's Larum for Pocket Watches.

DECEMBER 1807.

SUPPLEMENT

SUPPLEMENT TO VOL. XVIII.

Engraving of the following Object: Mr. Stott's Engine for splitting Sheep Skins.

Capillary Actions of Fluids.

Fig. 1.

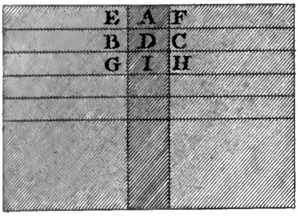
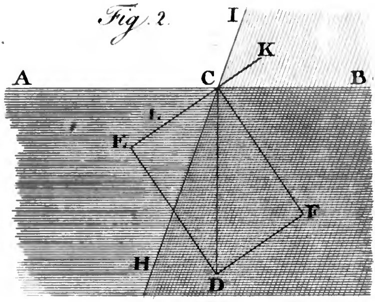


Fig. 2.



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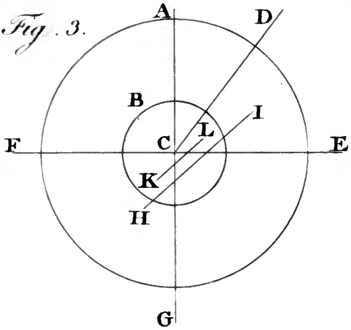

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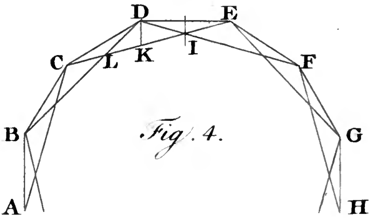

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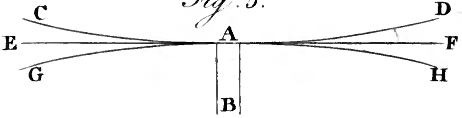

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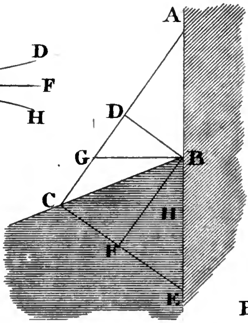
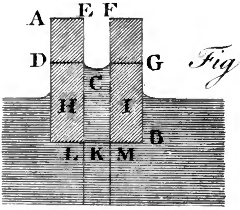


Fig. 7.



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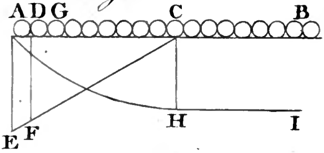
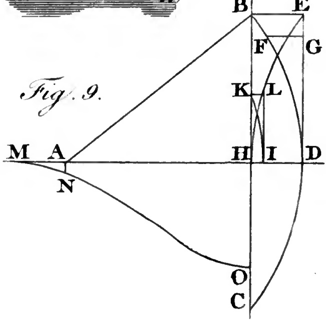


Fig. 9.



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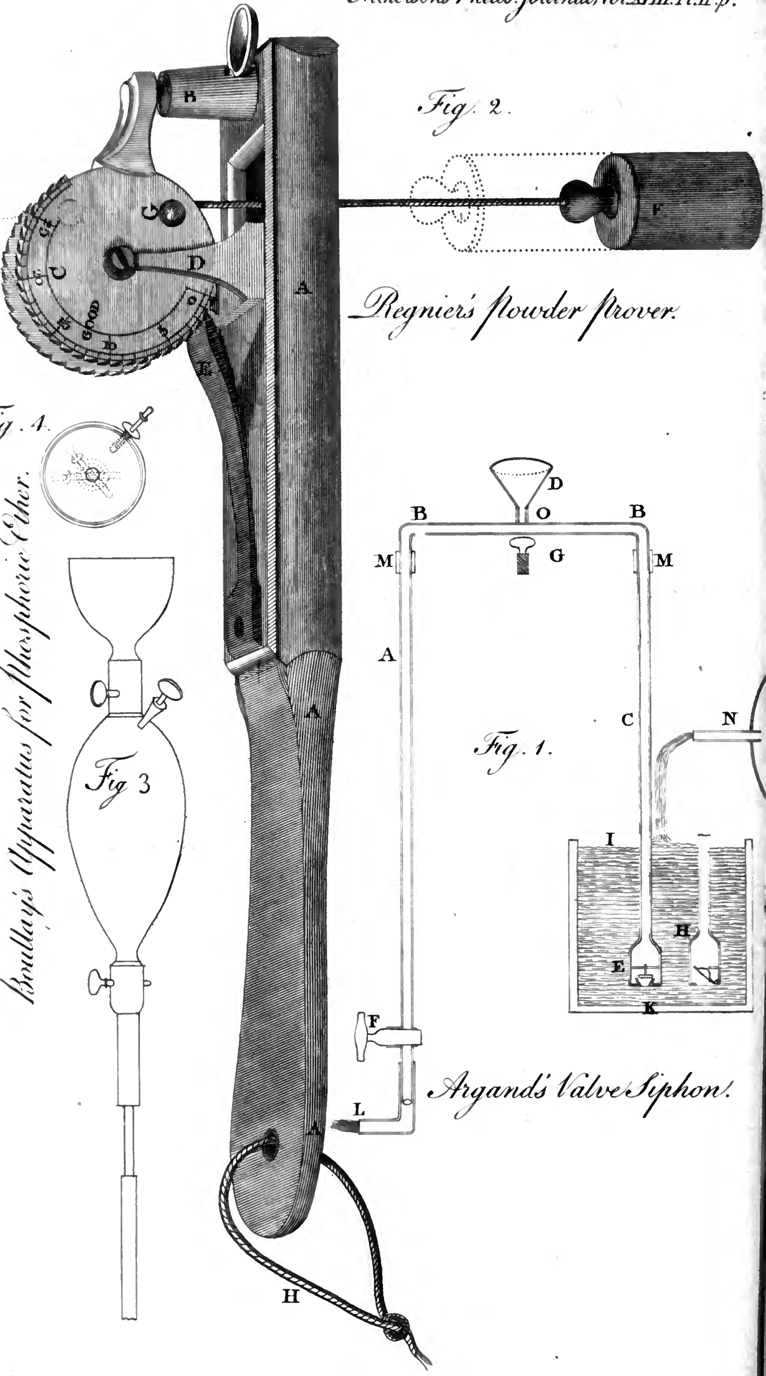

Regnier's powder prover.

Fig. 4.

Fig 3

Boullay's Apparatus for Phosphoric Ether.

Fig. 1.

Argand's Valve Siphon.

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SEPTEMBER, 1807.

ARTICLE I.

Remarks on some Difficulties which occur in the Investigation of the Capillary Actions of Fluids.

To Mr. NICHOLSON.

SIR,

THE capillary actions of liquids have lately been minutely investigated, both in this country and in France, and several essays on the subject have been inserted in your Journal: but there appears to me to be still some deficiency in all the modes of demonstration which have been employed. Mr. Laplace's first method leads to erroneous conclusions, respecting the angle of contact of a solid and a fluid: his second is less exceptionable, but it is still defective in omitting the consideration of the force of repulsion; for it cannot be denied, that this force is equally indispensable with that of cohesion to the existence of all material bodies in the state of solids or of liquids; and every theory of the mutual actions of the particles of such bodies, which does not comprehend the consideration of both these forces, must necessarily be imperfect. Dr. T. Young's reasoning, although built on more probable suppositions respecting the mutual actions of the particles, does not seem to be mathematically

Capillary action of fluids not yet completely investigated.

Laplace defective.

Force of repulsion necessary to be considered.

Dr. T. Young.

matically conclusive, so far as it relates to the physical foundation of the general law of an equal tension of the surface of any given liquid.

In the first place it appears, that Mr. Laplace's conclusion respecting the attraction, which he supposes to be exerted by a liquid, terminated by a plane surface, on any imaginary column within it, may be confuted on every supposition that can be formed, respecting the nature of the forces concerned. For the force which tends to draw every such column downwards into the liquid, can only be derived from the actions of the neighbouring columns, and must therefore tend in an equal degree to elevate them: so that the parts of each column which are nearest to the surface are urged downwards, and the remoter parts upwards, by equal forces; and the result is merely a general attraction of the whole stratum for the stratum next below, which of course must be completely counteracted by the repulsive force, whatever its nature may be. Thus the portion A (Pl. I. Fig. 1) is urged downwards by the attractions of the portions B and C, while D is urged upwards by those of E and F; and in the same manner D is urged downwards by G and H, while I is urged upwards by B and C. And thus, by continually adding to the substance any number of successive strata, we shall still find, that the general effect of the whole body on the column A I will retain it in equilibrium, whatever may be the depth. It is true, that according to Mr. Laplace's own principles, the attraction of any limited number of strata, on a column passing through them, must disappear, the force of the lower surface, which he supposes to be directed upwards, counteracting that of the upper in a contrary direction; but this consideration, although it may lead to a correct result with respect to the actions of fluids only, is not applicable to those of solids, or to the mutual actions of solids and fluids.

Force that tends to draw a column of fluid downward, tends to draw the neighbouring columns upward.

Counteracted by the repulsive force.

Thus equilibrium maintained to any depth.

Laplace's principles not applicable to solids, or solids and fluids.

In the second place, Mr. Laplace's determination of the attractive powers of a wedge of any kind, as proportional to its chord, must necessarily lead, as Dr. Young has already observed, to a proposition respecting the equilibrium of the surface of a fluid with a solid, which Mr. Laplace will not justify, and which he has silently abandoned; that is, that

His determination of the attraction of a wedge leads to erroneous principles.

that no angular termination of a fluid in contact with a solid can remain at rest, unless the density of the solid be precisely half that of the fluid. Thus if A B (Fig. 2) be the surface of a fluid retained in a horizontal situation by the vertical force C D, resulting from the joint actions C E and C F of the wedges A C H and B C H, if we add a wedge B C I opposite and similar to A C H, we shall have a straight line H I, and the action C K of this wedge reducing C E to C L, that of the wedge B C H must be reduced in the same proportion, in order that the result may remain in the direction C D, and the density of B C H must be made equal to the difference of the densities of B C I and A C H; or, if H I be the termination of a single solid, that solid must be of half the density of the fluid. It was perhaps in order to avoid this inference from his first theory, that Mr. Laplace adopted afterwards a different mode of reasoning.

Angular termination of a fluid in contact with a solid cannot remain at rest, unless the density of the solid be to that of the fluid exactly as 1 to 2.

I shall now examine the consequences of the supposition of a repulsive force extending its action to all particles within a certain very small distance of each other. Since it is certain, that the particles of all bodies in the state of gas repel each other, without any thing like the actual contact of impenetrable atoms; and since it may be shown by experiment, that many solid bodies exert repulsive powers on each other at sensible distances; it is natural to imagine, that the repulsive force, acting on any given particle, is derived from the joint effect of a considerable number of other particles at different distances from it, in the same manner as the force of cohesion is conceived to be derived from the joint actions of a great number of particles cooperating with each other; although the repulsive force may naturally be supposed, to consist principally in the stronger action of a smaller number of particles. Now if the circle A (Fig. 3) represent the limits of the force of cohesion, and B those of the force of repulsion acting on the central particle C, it is evident, that, if the substance be divided into any number of wedges meeting in the point C, the two forces exerted by any one of these D C E, upon any other F C G, must be equal, since the segments are in the same proportions as the whole circles; and the effects of the whole circles are equal:

Consequences of a repulsive force.

Particles of gas repel at sensible distances, as do many solids,

therefore repulsion the joint effect of many particles,

and probably from the stronger action of fewer particles than operate in cohesion.

This applied to sectors of circles.

equal: for, if an imaginary separation be made in the substance in any direction A C, it is evident, that the cohesive force, tending to bring all the particles of the two segments together, must be equal to the repulsive force, which prevents their nearer approach; and, into whatever portions the cohesive forces of the wedges be supposed to be divided, it is obvious, that for each of these, as for example, the mutual actions of the particles situate at H and I, an equivalent may be found in the repulsive force K L, exerted between the particles which are similarly situate within the sphere of repulsion: consequently, the whole result must be not only equal, but also parallel: so that, if the wedge F C G be considered as the termination of a vertical column, the effect of the wedge D C E, or of D C G, will have no tendency either to elevate or to depress that column. The only want of perfect counteraction will be, that the parts nearest the wedge will be urged more downwards by the repulsive force, and the remoter parts more upwards by the cohesive force. In order to understand the effect of a combination of such actions where the surface is curved, let us suppose the superficial particles to be situate at the angles of a polygon, A B C D E F G H, (Fig. 4) and the repulsive force to extend only to the two nearest particles, one on each side, while the cohesive force is so distributed, as to have its general result directed to the next particle but one: it will then be necessary, in order that there may be an equilibrium between the forces tending to separate and to unite any two particles D and E in the direction of the surface, that the cohesive forces in the directions D F, E C, be represented by D I and I K, while D E represents the repulsive force: then the forces acting on D being represented by C D, E D, L D, and I D, it is evident, that the parts of these forces which tend to urge the particle D to and from the line C E, are precisely equal, so that this particle will remain perfectly in equilibrium, without occasioning any pressure on the stratum within it. It is supposed in Dr. Young's reasoning on this subject, that the repulsive and cohesive forces acting on each particle are either accurately or very nearly equal; but this supposition, although it appears

The counteraction not absolutely perfect.

Effect of a combination of such actions.

Dr Young's supposition.

pears at first sight unexceptionable, is in fact inconsistent with the general principles of the theory.

Repulsive force therefore necessary to the solution; but the force deducible from experiment not adequate to the effect.

It appears therefore on one hand, that the consideration of a repulsive force is indispensably necessary to the perfect solution of the problem; and on the other, that such a force as there is reason to infer from experiment is not capable of producing effects similar to those of the capillary affections of liquids. There appears to be only one way of avoiding these difficulties, which is, to suppose that a *part of the force of repulsion* only is concerned in that action which is observed at sensible distances, while another part is so confined to the particles in immediate contact with each other, that if we suppose a liquid to be divided by any imaginary plane surface, the particles on one side of this surface can only act on the particles on the other side in a direction perpendicular to it, leaving them completely at liberty to move without resistance in the direction of the surface itself. This hypothesis has been tacitly assumed by Mr. Laplace, with respect to *the whole* force of repulsion; but in any shape it is still an *hypothesis* only, and the reasoning founded on it can only be considered as demonstrative, so far as its results are justified by a coincidence with facts and experiments.

Part of this force only supposed to act at sensible distances.

Supposition of a column terminating in a concave surface.

Suppose a column or stratum A B (Fig. 5) terminating in a curved surface C D, to be contained between two parallel planes perpendicular to the tangent E F; then the action of the particles below E F will have no power to move the column in a vertical direction; but the portion of the substance included between the curve and its tangent will tend to elevate it, and the more in proportion as the curvature is greater; the number of particles within a very minute distance from the column being directly as the curvature, or, where the surface has a double curvature, as the sum of the two curvatures in directions perpendicular to each other. And if the line G H be every where as much below E F as C D is above it, the action of the particles, cut off by this line on the column A B, will be equal to that of the particles above E F, and will produce an equal force tending to raise it; hence, if all these particles be removed, the remaining parts of the substance below G H will attract the column with the same force as was before counteracted by

Surface of double curvature.

Pressure proportional to the curvature.

by that of the parts removed; and the pressure will therefore in every case be proportional to the curvature.

A drop of fluid being perfecty insulated,

Hence if we imagine a drop of a fluid to be perfectly insulated, it is evident, that the superficial parts on one side of the drop must press the included fluid towards the other side, and must consequently be pressed back in an equal degree, so that at the circumference of the circle supposed to divide the drop, the surface must be *stretched* by the whole of this force, reduced only to a single direction; and there must therefore be *a uniform tension* of the surface.

the tension of its surface will be uniform

The only case which can be supposed to afford an exception to this demonstration, is that of the surface of a liquid terminated on each side by a solid of precisely half the density: but it is of little consequence what may be the result of such a combination, since it is scarcely possible, that it should ever be observed in nature.

Exception where the surface of the fluid is bounded on each side by a solid of half the density.

If it were not true, that the surfaces of liquids are stretched by a uniform force, it would follow, that a cork, wetted on one side and greased on the other, would continue for ever to move, on the surface of a large reservoir, towards the wetted side.

Tension of fluid surfaces must be equable.

Hence the angle of contact of a solid and a fluid may be deduced.

The angle of contact of a solid and a fluid, of given density, may be deduced from the law of equable tension, when once established, in a very satisfactory manner. Conceive a body, of the density of the solid only, to extend through the substance of the solid and fluid A B C (Fig. 6); the attraction of its surface will then urge the angular particle in the direction B D, with a force which is to the whole tension as B D to half A B; then a substance equal in density to the difference of the solid and the fluid, being superadded to the wedge C B E, will draw the particle in the direction B F with a force B F: now in order that the forces in the directions B D and B F may produce a result B G, capable of being completely counteracted by the perpendicular attraction of the surface A E, they must be proportional to B D and D G, and the density of the additional portion C B E must be to that of the solid as D G to B F, or to D C or A D, and the whole density of the liquid C B E to that of the solid as A G to A D, that is, by similar triangles, as A E to A H, which is the versed sine of the angle A B C, A E being the diameter.

Mr.

Laplace's second method ingenious, but requires the same hypothesis of repulsion

Mr. Laplace's second method of considering the effects of capillary action, though not wholly new, is ingenious and satisfactory; but it requires the assistance of the same hypothesis respecting repulsion, as is necessary to his first theory. The attraction of a capillary tube A B (Fig. 7) on the column C consists of two equal parts, one of which is derived from the action of the part D E F G on the upper portion of the fluid at C, the other from that of the end of the tube at H I upon the portion below at K; and these two forces are opposed by the attraction of L M, the part of the fluid forming a continuation of the solid, which draws the column downwards in the same manner as each of the other forces draws it upwards: so that the weight of the fluid elevated must be proportional to the excess of twice the density of the solid above that of the fluid. Supposing the fluid to be elevated in a very narrow space of a given breadth, the half of this breadth being the radius, the secant of the angle of contact will become equal to the radius of curvature of the surface, which is always inversely as the height of the elevated column; hence the cosine of the angle of contact will be directly as the height, that is, as the difference between the density of the fluid and twice that of the solid, the whole density of the fluid being represented by the radius; and this determination agrees precisely with the former.

Laplace's observation on unequal elevations and depressions of a fluid round floating bodies just.

Mr. Laplace has very justly observed, that where two floating bodies are surrounded by an elevation and a depression which are unequal in height, their repulsion will become a maximum at a certain distance, and upon a still nearer approach will be changed into an attraction. When the distance is very small, the height of the fluid elevated between the bodies is the mean of the heights to which it would be raised between two similar portions of the respective substances, and hence the magnitude of the force may be readily determined. Dr. Young seems to have considered only the case of an equal depression and elevation.

This not considered by Dr. Young.

Combined effects of cohesion and repulsion in the constitution of na-

As an illustration of the combined effects of the forces of cohesion and repulsion in the constitution of natural bodies, I shall subjoin a short investigation of the magnitude of the attractive power which retains the particles in solids and fluids tural bodies.

fluids in their situation, upon the simple, although perhaps inadequate supposition of a congeries of incompressible particles in contact with each other, actuated by a cohesive force, which extends, without diminution of its intensity, to a certain small distance from each particle.

In the series of single particles A B (Fig. 8) the particle A, being attracted by all the particles between A and C, the limit of the cohesive force, presses on the next particle D with the whole of this force, which may be represented by the line A E: but the pressure occasioned by the cohesion of the particle D is only proportional to the line D F, which is to A E as D C to A C, because the mutual action of D and A takes from the whole cohesive force a part which is equivalent to the action of the particle next beyond C: hence D presses on G with a force represented by the sum of A E and D F; and in the same manner it may be shown, that the whole mutual pressure of the particles at or beyond C is expressed by the area of the triangle A E C; and in general, that it may every where be represented by the ordinates of the parabolic curve A H, or of the mixtilinear figure A H I. The same may be inferred from considering the whole force resisting the division of the series between any two of its particles.

Magnitude of the force of attraction.

Suppose now that a single particle A (Fig. 9) is placed beyond the limit B C of an attractive body; it is required to determine the magnitude of the whole force with which it is attracted. The force of the particles situate upon the arc B D, when reduced to the direction A D, is represented by the line D E, since the number of particles in any small portion B F is as much greater than in E G, as A H is less than A B; and in the same manner the force of the particles in the line or narrow ring I K is represented by the line I L; hence the attraction of the whole segment B D H will be represented by the area D H E, I L being always equal to H K, and the curve H E being a hyperbola, which, when A comes into contact with H, becomes a right line. But when B C is considered as representing the surface of a solid, the measure of the attraction is the hyperbolic conoid, or the cone described by the revolution of the line H E on H D as an axis; and hence the attraction of the solid on a

particle

particle at H is precisely half as great, as if all the particles within the hemisphere of cohesion were situate in the line H D. If we take H M = A D, and make the ordinate A N every where proportional to the content of the conoid corresponding to the distance A H, the curve M N will approach at M infinitely near to a parabola, and at O will become parallel to A H; and the area A M N will express the sum of the attractions of a series of particles extending from M to A, and consequently the proportional attractive force of two solids situate at the distance A H. It is easy to show, by a fluxional calculation, that the area H M O is half of the rectangle M H O, and consequently, that the mutual action of the substances when in contact, is half as great, as if all the particles of the one body within reach of the cohesive force of the other, were situate immediately at its surface.

If one of the bodies be equal in thickness to the distance to which the cohesion extends, it will still be attracted by the whole force of the other: but if its thickness be less, and equal, for example, to A H only, the attraction will be expressed by the area A H O N only, which is ultimately to the whole area H M O as twice A H to M H. This is perhaps the reason, why the superficial particles of liquids are easily detached by the action of heat, in the process of slow evaporation.

This accounts for the process of slow evaporation.

I am, Sir,
Your very obedient servant,

20 *July*, 1807. ALETES.

II.

On the Solubility of some of the Earths by means of Sugar.

By Mr. William Ramsay.

SIR, *Glasgow, July* 14, 1807.

SHOULD you think the following experiments, on the solubility of some of the earths by means of sugar, worthy of of notice, you are at liberty to publish them in your valuable Journal.

I am, Sir,
Your most humble servant,
WILLIAM RAMSAY.

Wm. Nicholson, Esq.
London.

Quicklime gave a caustic taste to solution of sugar.

BEING employed in making experiments on sugar, and happening to put some quicklime into a cold solution of it, I noticed, that it had acquired an uncommon caustic taste.

Sugar dissolved in limewater is different.

Uncertain whether sugar dissolved in common lime water might not have the same taste, I prepared a small quantity, and added sugar to it; but the taste of the solution was very little different from that of sugar dissolved in water. On adding diluted sulphuric acid to the former, a copious precipitation of sulphate of lime took place, while the latter scarcely showed any traces of the presence of lime by the same agent.

Sugar dissolves lime.

Hence I concluded, that sugar possesses the property of dissolving a certain proportion of lime; and in order to ascertain its capacity in this respect, the following experiments were made upon this earth, together with barites, strontites, magnesia, &c.

Solution of sugar, at 50°,

One pound avoirdupois of common unrefined sugar was dissolved in rain water, and the solution filtered; the specific gravity at the temperature of 50 degrees of Fahrenheit's thermometer was found to be 1040. This solution was used in all the following experiments as a standard, to which the earths were added at first at the temperature of 50 degrees.

dissolves lime in

A portion of the saccharine solution was taken, quicklime was added to it in superabundance, repeatedly filtered, and fresh portions of the earth given to it, until the solution was evidently saturated; when the specific gravity was found to be

Solution of sugar	1020
Increased sp. grav. from lime in solution	40
	1060

Consequently

Consequently sugar dissolved in water at the temperature of 50 degrees is capable of dissolving one half of its weight of lime.

the proportion of 1 part to 2 of sugar.

Heat does not much increase its solvent power.

As most salts combine with greater facility, and in general in larger proportion by the medium of heat, it was thought, that the action of the sugar on the lime might be increased, and a greater quantity of it dissolved, at a higher temperature. Fresh quicklime was boiled in the solution of sugar. On filtering and cooling the liquid, it was found, that very little of the earth was dissolved. On the addition of dilute sulphuric acid, a slight cloudiness only appeared; but by the addition of oxalic acid to another portion of the liquid, a precipitation of oxalate of lime took place, which was estimated at about twice the quantity of lime that would have been precipitated from common limewater by the same agent.

Chemical properties.

The solution of lime in sugar is of a beautiful white wine colour, and has the smell of fresh slacked quicklime.

It is precipitated from the solution, by the carbonic, citric, tartarous, sulphuric, and oxalic acids. And it is decomposed by double affinity, by caustic and carbonated potash and soda, the citrate, tartrite, and oxalate of potash, &c.

Sugar in solution dissolves 1-4th of strontites at 50°,

and an equal weight at 212°, which it retains at 50°.

Having ascertained the quantity of lime that is dissolved in a given quantity of sugar, I next tried it as a solvent of strontites. Two ounces of this earth were taken, and the carbonic acid expelled by dilute nitric acid; the mixture was evaporated to dryness, and then put into a crucible in a red heat, until the nitric acid was decomposed. A portion of the solution of sugar was taken, and the earth added to it in the cold state; the specific gravity was increased to 1050. This solution was boiled on a fresh portion of earth, and the liquid immediately filtered; for some time it remained of a pure white wine colour, but as the liquid cooled, it gradually deposited a number of gray coloured crystals, which are soluble in water, and have the same taste as the saccharine solution of strontites. At the temperature of 50 degrees, the specific gravity of this solution was as under.

Solution of sugar	1040
Increased sp. grav. from strontites in solution	40
	1080

Consequently

Consequently an equal weight of strontites with the sugar employed is capable of being dissolved at the temperature of 212 degrees; and of being retained in solution by the sugar at 50 degrees of Fahrenheit. On exposing the crystals, which had fallen down during the cooling of the liquid to the air of the atmosphere, they attracted carbonic acid and effloresced.

Chemical properties.

The solution of strontites in sugar is of a fine white wine colour, and like that of lime has a peculiar caustic smell. This earth is precipitated by caustic and carbonated potash and soda; also by the carbonic, citric, tartarous, sulphuric, and oxalic acids. And it is decomposed, by compound affinity, by the carbonates of potash and soda; also by the citrate, tartrite, and oxalate of potash.

Sugar and barytes.

Judging from the greater solubility of strontites when compared with lime in the saccharine solution, that this might proceed from its superior affinity for this substance; it was thought, that a greater proportion of barytes would be dissolved than of either of the former earths. Two ounces of the carbonate of barytes were taken, and treated in the same manner as has been narrated in the preparation of the strontian earth, by expelling the carbonic acid by dilute nitric acid, evaporating to dryness, and then igniting the mixture until the nitric acid was destroyed. The prepared earth was put into the saccharine solution in the cold state, and frequently agitated; the liquid assumed a dull greenish appearance, and the smell of carbonated hidrogen gas was sensibly felt. After 24 hours rest the solution had lost its green colour, and was of the same colour as the original solution of sugar; and a black matter was found precipitated to the bottom of the glass jar. On trying the specific gravity of the solution it was not increased. The liquid was then taken and boiled on a fresh portion of the barytic earth, then filtered; on being cooled to the temperature of 50 degrees, the specific gravity was

None dissolved cold,

and but little hot,

Solution of sugar	1040
Barytes in solution	6
	1046

From

The experiment repeated.

The barytes acquired carbonic acid.

Repeated with the same earth.

From the result of this experiment being so very different from those preceding it on lime and strontites, I supposed, that some change had taken place either in the sugar or barytes; the experiment was therefore repeated several times, but always with the same result. The barytic earth, which was left on the filtering paper, was put into dilute nitric acid, and a violent disengagement of carbonic acid gas took place, although the whole of this gas was apparently expelled before the earth was introduced into the saccharine solution. The same portion of earth was treated in the same manner thrice, and on expelling the carbonic acid and introducing it into the saccharine liquor, the specific gravity was not farther increased; the liquid always assumed the greenish appearance before noticed, and when in this state carbonated hidrogen gas was evidently disengaged, and a black flocculent matter always subsided before the liquid became transparent.

Probably the barytes decomposed the sugar by its affinity to carbon.

From these appearances one would be almost ready to conclude, " That barytes, by its superior affinity with the " carbon of the sugar, is capable of decomposing it; that " part of the carbon, in union with the barytes, is precipi- " tated along with the earth in its carbonated state; and " that the oxigen of the sugar, being set at liberty, unites " with the hidrogen and another portion of the carbon, and " is disengaged in the state of carbonated hidrogen gas." But as I cannot say, that the earth and the nitric acid were in a state of absolute purity, on this account I dare not trust entirely to this explanation, and only state what took place during the course of making these experiments.

Magnesia very little soluble in it.

I next tried the power of the solution of sugar on magnesia. One half ounce of this earth calcined was added to the saccharine solution in the cold state, without the specific gravity being perceptibly increased; the mixture was then boiled, when on filtering and cooling the liquid to 50 degrees, the specific gravity was

Solution of sugar	1040.000
Magnesia in solution	3.050
	1043.050

The

Chemical properties.

The solution of magnesia, like those of lime and strontites, was of a pure white wine colour, and had no sensible variation in smell or taste from the common solution of sugar; farther than that the sweet seemed much improved, and was softer and more agreeable on the palate, as if it were entirely freed from the earthy taste, which unrefined sugar frequently has. On its remaining at rest for some months in a bottle well corked, the magnesia appears to be entirely separated.

Alumine but little soluble in sugar.

Very little alumine is dissolved by a solution of sugar, when fresh precipitated earth is presented to it either in the cold or hot state. Neither does it seem capable of holding it in solution, when sulphate of alumine is decomposed by saccharite of lime in the way of double decomposition: both the lime and alumine are precipitated together. But when fresh precipitated earth of alum is boiled for some time in the saccharine solution, it seems capable of attracting the colouring matter of the sugar, and the liquid, when the earth has precipitated, is in a purer state than before. Perhaps with certain modifications this agent might be of service in the refining of sugar.

Perhaps it might be useful in refining it.

Alkaline carbonates separate the earth from sugar.

The union of sugar with the alkalis has been long known; but this is rendered more strikingly evident, by carbonated potash or soda (for instance) decomposing the solutions of lime and strontites in sugar by double affinity.

Weak sugars contain lime.

In making solutions of unrefined sugar for culinary purposes, a gray coloured substance is found frequently precipitated. It is probable, that this proceeds from a superabundance of lime, which has been used in clarifying the juice of the sugar cane at the plantations abroad. Sugar with this imperfection is known among the refiners of this article by the name of *weak*. And it is justly termed so, the precipitated matter being nothing but lime which has attracted carbonic acid from the sugar, (of which there is a great probability) or from the air of the atmosphere. A bottle in which I had kept a solution of lime in sugar for at least four years closely corked, was entirely encrusted with a yellowish coloured matter, which on examination was found to be entirely carbonate of lime.

Lime separates from sugar in time in the state of a carbonate.

III.

III.

Inquiries concerning the assimilating Power in Vegetables; by Mr. HENRY BRACONNOT: *read at the Academical Society of Sciences of Nanci, November the 22d,* 1806*.

PHYTOLOGISTS for a long time imagined, that vegetables were nourished by certain juices, which they extracted ready formed from the earth. Van Helmont in great measure refuted this by his celebrated experiment. In a box containing 100 lbs. of earth, and covered with lead, he planted a willow, weighing 50 lbs. This he watered with distilled water, and in five years it had acquired an addition to its weight of 119 lbs. 3 oz. without any perceptible diminution of the earth. The experiments of Boyle with earth baked in an oven, and those of Duhamel and Bonnet with moss†, prove the same thing.

Supposition that vegetables extracted nutritious juices from the earth refuted by Van Helmont, Boyle, and others.

Other natural philosophers have pursued the same inquiry: Tillet, in particular, made a number of experiments, to ascertain whether water and air were the only substances necessary for the growth of plants. He filled several pots with different earthy matters, some with old plaster, others with pure river sand, fragments of stone broken to powder, &c.; buried them partly in the ground, to retain the moisture; and sowed wheat in them. The wheat produced very fine ears; and the grains, being sown, produced other fine plants.

Tillet showed air and water alone to be necessary to their growth.

From the infant state of chemistry, at the time, however, none of the plants produced by means of air and water alone were analysed. This indeed has since been done; and it has been advanced, that plants growing in such a manner as to have been nourished by water alone, did not furnish as much

None of these plants analysed

Since said, that these plants contained less

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* Abridged from the Annales de Chemie, Vol. LXI. p. 187. Feb. 1807.

† Mr. Procopius Densidoff of Moscow, who has a very fine botanic garden, raises all sorts of plants by a peculiar method. He sows the seeds in moss, where they germinate, and then plants them in pots. In this way he loses very few seeds of those that grow with most difficulty. *Note of Prof. Willmett.*

Seeds most difficult to germinate succeed in moss.

carbon than their seeds.

carbon as was contained in the seeds from which they sprung; while those in mould were much more vigorous, in consequence of the carbon with which it furnished their roots. But these experiments were on too small a scale to furnish satisfactory results; and I have therefore attempted to investigate the subject anew, in order to ascertain, how far this opinion is well founded.

Mould first analysed.

As a preliminary step, I conceived it necessary to analyse vegetable mould in a state of perfect decomposition. For this purpose I selected perfectly pure, black, pulverulent mould, from among the hollow roots of a very old tree.

Contained nothing soluble in water.

Distilled water, in which it was boiled, remained colourless after filtration, and on being evaporated left no sign of any soluble matter.

Very retentive of moisture.

A hundred grammes [3½ oz.] were reduced by dessication to 20, which shows it to be extremely retentive of water.

Its products.

These 20 gr., distilled at a red heat, gave out 4 of water, that powerfully reddened infusion of litmus; and contained empyreumatic acetous acid, partly saturated with ammonia; 2 of an acrid oil, that congealed on cooling, and was of a dark brown colour; 89 cubic inches of oily hidrogen gas, burning with a blue flame; and 34 cubic inches of carbonic acid absorbed by lime.

Residuum.

The coally residuum weighed 8·5 grammes, which were reduced by incineration to 2·4 gr. of light yellow ashes.

Ashes.

Boiling distilled water digested on these ashes acquired no peculiar taste, did not turn sirup of violets green, and was barely rendered turbid by the addition of a few drops of oxalic acid, which seemed to indicate, that a few particles of lime had been set free by the calcination. The nitrates of barytes and of silver produced no change in it. On farther analysis these ashes afforded 1·3 of a gr. of silex, ·2 of oxide of iron containing a little oxide of manganese, ·25 of phosphate of lime, ·2 of lime, and some traces of magnesia.

The mould boiled in solution of potash.

I boiled 6 gr. [92 grains] of the same mould for an hour, in a strong solution of caustic potash, when it became glutinous, and swelled up. I then diluted it with water filtered, and obtained a very deep brown liquor. This mixed with the lixiviating waters gave on the addition of an acid a precipitate, that weighed 1 gr. when dried. It was of a very

very black colour, and in little shining scales. Scarcely any vapour arose from it when thrown on burning coals, and I consider it as charcoal well saturated with hidrogen. Art may imitate this substance, by effecting by fire what nature does by time. If we deprive a vegetable substance of almost all its oxigen, and a small quantity of its hidrogen, by exposing it to a certain temperature, the result will be a hidroguretted charcoal, partly soluble in potash, as I have found by experience.

The residuum resembled pitcoal.

That part of the mould, which had resisted the action of potash, weighed when dried 5 gr. It had no longer the physical characters of mould; was in pieces that were tolerably hard and brittle; and had a striking resemblance to pitcoal, which led me to produce it in larger quantity. In this way it had still such a resemblance to coal, that I could compare it to nothing else.

It affords no soluble matter to plants.

From this examination of mould it appears, that it cannot supply plants with any soluble matter for their nutrition, since boiling water has no action on it. It would be superfluous to say, that seeds sowed in it vegetated with extraordinary vigour; but I must not omit to mention the presence of a large quantity of potash in the plants, though I could not detect any in the mould in which they grew, by the most strict researches.

Manure therefore seems to be of use only by retaining moisture.

These observations appear to corroborate the opinion of Tillet and Parmentier, who consider manure as useful only by retaining moisture, and keeping strong soils open for some time, so as to allow the roots of plants to penetrate them. But if water and air be the only sources of the food of plants, any insoluble matter, sufficient in quantity, and duly watered, must be adequate to the purpose of their growth. This I endeavoured to ascertain by experiments.

Mustard seed sown in litharge.

Exp. I. In a box filled with pure litharge I sowed 400 seeds of white mustard, weighing 2·2 gr. These I watered frequently and carefully with distilled water. The box was placed in a good aspect, and a glass was hung over it to keep out the dust. The plants throve very well, and produced perfect pods. I collected all the seminal leaves as well as the rest that dropped off; and when the vegetation was at its height, pulled up the plants. Having well washed the

the roots, to remove any portions of oxide of lead, and wiped them dry, the whole weighed 264 gr. After they were dried, the weight was 34·2 gr.

The produce analysed.

These yielded 12 gr. of coal, which by incineration were reduced to 4·2 gr. of ashes. These lost by lixiviation 2·2 gr.; of which ·59 gr. were sulphate of potash, ·69 gr. potash. The insoluble residuum afforded ·4 gr. of silex; ·45 gr. of oxide of iron, alumine, and phosphate of lime, the proportions of which were not determined; ·57 gr. of oxide of iron; and a very small portion of magnesia.

Mustard seed sown in flowers of sulphur.

Exp. II. In a very large, deep, and perforated bowl of stone ware, filled with well washed flowers of sulphur, 250 seeds of white mustard were sown. The whole was covered with a large bell glass, allowing free access to the air and light. The plants grew vigorously, being frequently watered with distilled water; as sulphur, having little affinity with water, parts with it very easily, and dries on the surface. They produced flowers in tolerable abundance, and the seeds produced plants in common ground. The weight of the fresh plants, with the fallen leaves, was 108 gr.; and when dried 18·6 gr. Their coal weighed 7·8 gr. and left 1·55 gr. of whitish ashes, which afforded by lixiviation ·6 gr. of carbonate and sulphate of potash. The insoluble part was similar to that of the former.

The produce analysed.

Mustard seed sown in small shot.

Exp. III. A hundred seeds of white mustard were sown in twenty pounds of very small unglazed shot, on the 9th of July. On the 28th of August they began to flower, and afforded small pods. All these plants were slender, and had but few and small leaves. When fresh they weighed 10 gr. and after being dried 2·3 gr. they yielded very little coal, but more than the weight of the seed. The weight of the shot appeared to oppose too great an obstacle to the young roots, as most of them spread over the surface, without being able to penetrate it. The little affinity of the lead for water was another reason of the feeble growth of the plants; and hence I have found, that plants thrive less in metallic powders, than in their oxides.

Radish seed sown in a heap of sand.

Exp. IV. On a flat stone pavement a bed was formed, about a yard high, of fine sand, taken immediately from the bed of the river, and well washed. In this were sown seeds

seeds of the common radish *(raphanus sativus)*, which were frequently watered with perfectly pure rain water, and the plants grew with as much vigour as they would have done in any ground. The greater part of the radishes were brought to table, and were of a very delicate flavour, without any of the disagreeable earthy taste they have sometimes. Some of them were left to seed; and most of these grew to the height of 2 feet or 2½. It was observed, that those at the top of the heap were much larger and stronger than those near the bottom*.

The produce analysed.

Sixty three of these plants when dried weighed 372 gr. Incinerated they left 54·2 gr. of gray ashes. These afforded by lixiviation 18·6 of very fine potash. From this I am inclined to think, that the radish might be cultivated with advantage on wet sandy places by the sea shore, for the purpose of fabricating potash†. These 18·6 gr. being farther analysed,

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Asparagus and potatoes grow well in sand.

* A skilful gardener informs me, that asparagus too will succeed very well in pure river sand. Potatoes also grow well in sand, and are said to be better tasted.

Potash abounds in tetradynamic plants.

† It appears, that potash abounds in all the plants of the class tetradynamia, and the ashes of some of the species were long in use for making soap and glass, before the introduction of soda as an article of trade. According to Bomare, the bunias cakile, sea rocket, was much employed for these purposes.

Acrid and bitter plants generally abound in potash, frequently combined with nitric acid.

I must here add an observation, which appears to me pretty general, and which I made in examining the acrid and bitter properties of plants. One or other of these principles I have almost always found in conjunction with a large quantity of potash, which was frequently saturated with nitric acid. Thus among the cruciferous plants, which are all more or less acrid, the sisymbrium nasturtium, common water-cress, afforded me a great deal of alkaline matter after incineration; and when fresh I found in it nitrate of potash. I have observed the nitre melt on incinerating cabbages and turnips; and Mr. Delaville found this salt in large quantity in the sap of these plants. Mr. Bouillon-Lagrange found a large quantity of potash in the ashes of the erigeron canadense, Canada fleabane, which is acrid. The ashes of tobacco, the acrimony of which is well known, yield 40 per cent of potash. Among the bitter plants I have examined, I found nitrate of potash in the fumitory, 100 parts of the ashes of which contain more than 36 soluble in water, according to Wiegleb and Rukert. The common centaury, marsh and Siberian trefoil, and different species of the genus centaurea, which are very bitter, afford ashes in which potash abounds. Whether in these plants it be saturated analysed, were found to contain 6·7 of pure potash; 7·35 of sulphate of potash; a small quantity of phosphate of lime; and the rest was carbonic acid.

Residuum contained sulphur.

The residuum left after lixiviating the ashes appeared to contain sulphur, as on pouring nitric acid over it sulphuretted hidrogen was given out; but I could not find any phosphoric acid in it. I did not examine it for the earths, as these might have been said to have been taken up from the sand.

Compared with other plants from garden mould.

Having thus examined these plants, I thought it might not be amiss to compare their produce with that of some others, which had grown in common garden mould. Of these dried 372 gr. afforded but 34 of ashes, which it is true were very saline, and yielded 16 gr. of saline matter, consisting of carbonate and sulphate of potash. In the incineration of these plants too I observed a very copious production of ammonia, on pouring a little water on their ashes while still hot.

Whence these substances found in plants nourished only with water?

Are they all formed ftom oxigen and hidrogen?

But whence come these earths, alkalis, acids, metals, sulphur, phosphorus, found in plants, that have had no aliment but pure water? Can vitality, in conjunction with light and heat, determine certain quantities of oxigen and hidrogen to form by peculiar condensations those substances which have been considered as simple? this might put us on examining in a new point of view all those substances, that chemistry has not yet been able to decompose, and thus perhaps the conjectures, that have been advanced by some, may be verified.

Is it the same with animals?

We may even extend these remarks to animalization, supported by the well-known experiment of Rondeletius, who

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saturated with nitric acid I have not ascertained. I must observe, however, that I have found no nitric acid in wormwood, 100 parts of the ashes of which afford nearly 75 of saline matter, according to Wiegleb. This large quantity of alkali has appeared to me to be partly saturated with a peculiar matter, which is deposited by a decoction of the fresh plant, and may be precipitated abundantly by nitrate of lead. This matter dissolves very well in alkalies neutralizing part of their properties: it is the same that Mr. Vauquelin has found in some species of cinchona. Does exist in all bitter plants? and is it this which in cinchona and wormwood cures intermittent and low fevers?

The alkali partly saturated with a peculiar matter:

is it cinchonin?

kept a fish in pure water, till it grew too large for the vessel containing it, and by other similar experiments on different animals. It would even seem, that food acts on the stomach in a great measure as mould does on the roots of plants, merely retaining water in such a state of division, as to fit it for absorption and assimilation.

Does food act on the stomach merely as mould on the roots of plants?

From what has been said it appears, that foreign matters dissolved in water only check the progress of vegetation; but that the vital powers can sometimes surmount these obstacles, appropriating only the pure water, that held these matters in solution.

Matters dissolved in water injurious to vegetation.

If experiments founded on long practice were still necessary to prove, that the soil is so much the more proper for vegetation in proportion as it is deprived of soluble foreign matter, I would mention the practice of paring and burning wastes, used chiefly in England. Lands thus treated remain in heart a long time; the parts where the heaps of surface mould were burned are most fertile; and manure even appears to be injurious, by causing the wheat to run chiefly to straw, with thin ears, and those of bad quality.

Absence of soluble matter advantageous, as appears from the effect of paring and burning;

This extraordinary effect of torrefaction on the soil appears to me attributable to the combustion of those excrementitious matters, which are ejected by the roots of plants. When the soil is impregnated with these matters, which are eminently injurious to vegetation, the perfect developement of plants is prevented. This excretion from the roots is evident from the surrounding earth, which becomes unctuous, and sometimes of a darker colour. In several of the euphorbiums and cicoraceous plants it is very perceptible, and milky. It may be observed too, that roots, when they multiply under water, become covered with a glairy matter, which deserves to be examined; and which no doubt the earth would have absorbed, had the roots remained buried in it. It is to these excretions from the roots perhaps we must frequently ascribe that kind of antipathy between certain plants, which are never found together. Thus the thistle is injurious to oats, euphorbium and scabious to flax, elecampane to carrots, fleabane and darnel to wheat, &c.

which probably acts by destroying the matter excretted from the roots of plants.

This excretion an obvious fact;

and probably the cause why some plants particularly injurious to others.

It would certainly be wrong, to ascribe the fertility of land pared and burned to the charcoal produced in this operation; for

Effects of paring and burning cannot be

owing to charcoal.

for Mr. Chaptal has shown, that dry charcoal, alone or mixed with earths of little solubility, does not penetrate into the vessels of vegetables.

Plants do not draw charcoal from the earth.

To add to the proofs, that vegetables have no need of drawing carbon from the earth, I might mention high trees, loaded with fruit, that grow and thrive on rocks or old walls, totally destitute of vegetable mould; and those vast forests, the soil of which is pure sand extending far beyond the roots.

Opinion that they derive it from the atmosphere.

Saussure's experiment.

The plants killed by the vapour of the lime.

I have now to examine the opinion, that vegetables absorb their carbon from the small quantity of carbonic acid contained in the astmosphere. Sennebier first announced this decomposition; and T. Saussure afterward endeavoured to prove, that this very small quantity would be sufficient for all the vegetables that exist. But though this philosopher was persuaded of the utility of carbonic acid in vegetation, he satisfied himself, that plants could grow in an atmosphere freed from it; and he ascribed this growth to the carbonic acid produced by the plants themselves, which they decomposed after having formed it*. To prove this he exposed to the sun closed receivers, in which plants were growing, and suspended quicklime to the upper part of them. The plants soon grew yellow, and at the expiration of five days gave no signs of vegetation; whence he inferred, that the absorption of carbonic acid by the lime was the cause of their death, and that the elaboration of this acid was necessary to vegetation in the sun. But I cannot be of his opinion. I have examined the experiment carefully, and satisfied myself, that the death of the plants was not owing to the privation of carbonic acid alone, but to the lime itself in vapour.

The following experiments convinced me of the volatility of lime.

Proofs that lime is in some measure volatile.

1. Paper tinged by repeated immersion in infusion of litmus, then reddened, and afterward washed in water to remove its excess of acid, was suspended in a stopped phial,

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* It is obvious, that the carbonic acid formed by the plants could not furnish them with more of its base than it had previously taken from them. Tr.

into

into which I had put with great caution some lime, that was slaked, and suitably moistened with water. It was not long before the red colour of the paper was changed to blue. This effect was not unknown to Fourcroy.

Distilled with water.

2. Into a retort I put with all possible precaution a certain quantity of lime and water, and by distillation I obtained a liquor impregnated with an intolerable smell of lime. This liquor left a disagreeable impression on the palate, and had manifestly alkaline properties.

Still more rises with spirit.

Alcohol by its volatility carries up in vapour a much larger quantity of lime, as appears from an experiment of Proust. In order to obtain spirit free from acetic acid, he distilled 25lbs. of red wine with a handful of quicklime. The product was so much impregnated with the taste and smell of the lime, that he was surprised. When redistilled it had the same taste, precipitated metallic solutions and oxalic acid, and restored the blue of litmus.

Other fixed alkalis rise at a low heat.

Lime is not the only fixed alkali, that shows a disposition to rise at a pretty low temperature.

Potash distilled.

A solution of potash, subjected to distillation, afforded me a water with a strong lixivial smell. This water redistilled retained the same smell, and gave with nitrate of lead a white flocculent precipitate, which was completely soluble in distilled vinegar.

Other substances partially volatile.

But there are other substances beside alkalis, the volatility of which is so little apparent in the temperature of the atmosphere, that it is discoverable only from its effects on organized beings.

Quicksilver.

Some Dutch chemists set plants in water, by the side of which they placed a small bottle of mercury, and covered the whole with a jar standing in water. On the third day the plants were covered with black spots, and on the fourth, the fifth, or at latest the sixth, they were entirely black. The effects were the same when the jar rested on pieces of cork on a table. Other plants lived a long time under similar circumstances except the presence of mercury.

Other vapours injurious to vegetation.

Sennebier and Hubert too have shown, that the vapour of sulphuric ether prevents germination from taking place, without altering the quantity of the air. Camphor, oil of turpentine, assafetida, vinegar, ammonia, bodies in a state of of putrefaction, &c., have the same effect. Hence we may infer, that all those matters, which are injurious to animals, sensibly affect vegetables likewise.

Carbonic acid not useful,

We cannot therefore lay much stress on Saussure's experiments to show the utility of carbonic acid in vegetation, particularly when we recollect an experiment of Priestley's, which proved, that an atmosphere with an eighth part of carbonic acid was sufficient to kill two plants of mint, though this small quantity of acid was in contact with a large surface of water.

but injurious in no very large proportion.

Seeds germinate in oxide of lead,

Having found by experiment, that seeds germinate very well in oxides of lead, which are known to be greedy of carbonic acid, I conceived, that these might contribute to elucidate the question respecting the utility of carbonic acid in vegetation. In consequence I moistened with distilled water some recently prepared oxide of lead in the first stage of oxidation. This mixture I introduced speedily into a flint glass bottle: and though the disagreeable and as it were alkaline smell that arose from it, led me to doubt the success of my experiment, I sowed some mustard seed in this oxide, and corked the bottle tight. As I foresaw, no germination took place: but what I was far from expecting, and to my great surprise, part of the oxide of lead in the water was reduced by the seeds, each of which was enveloped by a shining coat of metallic lead. This appeared to me to be very probably owing to a production of water by the union of the oxigen of the oxide with the large quantity of hidrogen, that is condensed in this oily seed, which after the reduction was more or less carbonized.

but not if recently prepared,

and included in close bottles.

The lead partly reduced.

If the oxide have been exposed to the air, the seeds grow.

If the oxide of lead be left exposed to the air for some time after it is made, and then put into a bottle with water and seeds, no reduction of the metal will be effected, but germination will take place.

Oxide of lead easily reduced unless carbonic acid prevent it.

These experiments show the extreme facility, with which the oxides of lead are reduced, and the obstacle that carbonic acid opposes to this reduction.

As these first attempts did not afford me the result I sought, I availed myself of an old experiment of Huyghens, who, in 1672, put some earth into a bottle, corked it up, and found it produce such a quantity of plants, as almost to fill the

Seeds sown in siliceous earth, in corked bottles, the bottle, without having had any fresh air admitted to it. Accordingly I procured six large flint glass bottles, most of which were square: filled them in part with very fine white sand, which I deprived of all calcareous earth by washing with weak muriatic acid; and moistened this with distilled water. The remainder of the bottle was filled with atmospheric air freed from carbonic acid.

without the presence of carbonic acid.

In these bottles having sowed 460 seeds of white mustard, I closed them very accurately, and placed them a few inches deep in a moist soil. Vegetation soon commenced, and considerable verdure was produced*. After six weeks growth my plants were liberated from their prisons, washed with great care, and dried. In this state they weighed 9 gram. [140 grs.] I filled a phial with them, which terminated in a narrow tube, and exposed it gradually to a strong heat. Thus I obtained 4 gr. 8 dec. [74 grs.] of coal. But as I supposed this coal might still contain a little sand, I incinerated it, and found 3 gr. 3 dec. [51 grs.] of very alkaline ashes. Consequently there was 1½ gr. [23 grs.] of pure carbon.

They grew,

and afforded carbon,

In a very small vessel I distilled 460 white mustard seeds, and from this highly hidrogenated seed I obtained only 4 gr. [62 grs.] of coal, which lost near half its weight by calcination. Hence it follows, that 460 mustard seeds acquired in close vessels upwards of a gramme [15½ grs.] of pure carbon,

in larger quantities than the seed.

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* It may be supposed, that these seeds did not germinate with as much vigour, as if in the open air. This however I do not think must be ascribed to the want of oxigen; for by trial of the air with a sulphuret before and after the experiment, its proportions appeared to be nearly the same. This is agreeable to the experiments of Hassenfratz, who convinced himself, that plants do not diminish the quantity of oxigen in a confined atmosphere: and I am inclined to think, that oxigen acts on plants merely as a stimulant, which is not indispensable, for Homberg found different seeds germinate in the vacuum of an airpump. The principal cause, that prevents the complete developement of plants in close vessels, appears to me to be owing to their abundant perspiration, which throws out the excrementitious fluids, that are so fatal to them even in the open air, as to render a certain space among their neighbours necessary to their vigorous growth.

The plants grew weakly, but not from want of oxigen.

Probably from their perspiration not being carried off.

which

which appeared evidently to have been formed at the expense of water, and probably of light*.

Arguments from geology that the carbon of plants does not come from the atmosphere.

Geological facts too seem to shake that theory, which ascribes the carbon found in vegetables to the small quantity of carbonic acid contained in the atmosphere. How indeed can so small a portion of this acid, scarcely amounting to a ten thousandth part of the air, explain the formation of those vast mines of pitcoal, which still retains the marks of those organized substances whence it originated, and the organic origin of which is sufficiently announced by its composition of carbon, hidrogen, oxigen, and azote? But without appealing to these ancient productions of the vegetable kingdom, buried in the earth in such abundance, we need only cast an eye on its surface, to satisfy ourselves that nature must have taken other steps to produce carbon.

Charcoal produced from water, and therefore contains hidrogen.

On the other hand, if, in the silent progress of vegetation, the elements of water concur with the solar light to produce charcoal by intimate combinations unknown to us, charcoal ought to contain hidrogen likewise; and this is confirmed by experience.

Proofs of this.

If charcoal strongly calcined be urged in the fire with a substance that has an affinity for hidrogen, the charcoal is partly decomposed, and hidroguretted products are obtained.

Light necessary to the production of carbon in plants.

* To satisfy myself, that plants can appropriate to themselves the elements of water, so as to constitute their different materials, only by their own organic action combined with that of light, I caused a given quantity of seed to grow in complete darkness, and at the common temperature of the air. They shot out long white filaments, at the extremity of which were the two seminal leaves; but nothing more appeared. After desiccation these plants weighed less than the seeds whence they sprung: which appeared to be owing to their having lost carbon in this languishing state, instead of acquiring it.

Apparently it combines with them.

But the mode of action of light on vegetables remains yet to be known. It appears however, that it enters into combination with them, and that to this combination is owing the green colour of their leaves, and the variety of hues admired in their flowers. Yet Mr. Humboldt has found green plants growing in deep and dark mines, the atmosphere of which contained a great deal of hidrogen. Does not this fact indicate something common between hidrogen and light, particularly when we observe, that these two fluids, the lightest in nature, seem likewise to produce analogous effects on some metallic oxides and salt?

Perhaps analogous with hidrogen.

Mr. Berthollet mixed 30 gr. [463 grs.] of charcoal calcined in a forge fire with 20 gr. [309 grs.] of sulphur, and by distillation in a porcelain retort obtained more than 100 cubic centim. [391 lines] of sulphuretted hidrogen gas: and it appears to me to be probable, that, if the experiment were frequently repeated with the same charcoal, it might be totally decomposed, a fact that it would be interesting to verify.

If oxigen in the state of gas be presented to the charcoal instead of sulphur, water is formed, as is proved by the experiments of Lavoisier on the combustion of charcoal, as well as by those of Mr. Hassenfratz: and analogous results are obtainable with metallic oxides, according to the observations of Cruikshank.

The diamond too contains hidrogen.

It even appears from the nice investigations of Messrs. Biot and Arrago on the refractive power of bodies, that the diamond, which has hitherto been considered as pure carbon, must contain a large quantity of hidrogen, which has the greatest refractive power of any substance yet observed in nature. These gentlemen intend to verify their conjecture by direct experiments, from which very interesting results may be expected. The existence of hidrogen in the diamond has been announced from other facts by Mr. Winterl.

From the chief facts that have been here mentioned it follows:

General conclusions.

1. That vegetables find in pure water every thing necessary for them to assimilate.
2. That vegetable mould in a state of complete decay contains nothing soluble, and can only supply plants with water, which it retains abundantly in a certain state of division adapted to their nourishment.
3. That vegetables can grow in any substance, provided it have no action on them, and be perfectly insoluble in water.
4. That the organic powers, assisted by the solar light, developes in plants substances that have been deemed simple, as earths, alkalis, metals, sulphur, phosphorus, charcoal, and perhaps too nitrogen, that probably will no longer continue to be the limits at which chemical analysis will stop.
5. That oxigen, hidrogen, and fire appear to be the only elemen-

elementary substances, that serve to constitute the universe.

6. Lastly that nature, in its simple course, produces the most various effects by the slightest modifications in the means it employs.

IV.

On Vegetable Mucilages; by John Bostock, M. D. *of Liverpool.*

Vegetable mucilage a vague term.

The author's object.

THE term mucilage is employed, in rather a vague manner, to designate a class of vegetable productions, which, although they agree in many of their properties, are in other respects considerably dissimilar. My object in the following course of experiments was to obtain a more accurate knowledge of their peculiar properties, and to discover tests by which their presence may be detected, without having recourse to those methods of analysis, in which they are resolved into their component elements.

Solution of gum arabic.

Examined with different reagents.

Their effects.

Gum arabic, when dissolved in water, exhibits all the properties of a vegetable mucilage in the most complete form. I prepared a solution, in the proportion of ten parts of water to one of gum, and to portions of this the following reagents were respectively added; in general one drachm of the solution was mixed with ten drops of the reagent, except in the case of alcohol and the infusion of galls, when equal parts were employed. 1. Acetate of lead, 2. superacetate of lead*, 3. nitro-muriate of tin, 4. nitro-muriate of gold, 5. nitrate of mercury, 6. oxysulphate of iron, 7. silicated potash, 8. alcohol, and 9. infusion of galls. In No. 1, there was a copious, dense, white precipitate. No effect was produced in Nos. 2, 3, and 4. In No. 5 a white precipitate appeared, which was dissolved by agitation, but was reproduced by the addition of water, and in a few hours it assumed a light pink colour. In No. 6 an orange coloured precipitate was formed, at first in small quantity, but in 24

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* For the difference between these two salts see Nicholson's Journal, XI, 75; and Thomson's Chemistry, III, 262, (3d Edit.)

hours the whole became opake. In No. 7 an immediate opacity was produced, and after some time a precipitate fell down. There was an immediate precipitate in No. 8: but in No. 9 there was no effect produced. If the oxysulphate of iron be added to a solution containing $\frac{1}{5}$ of its weight of gum, the whole is immediately converted into a solid, transparent, orange coloured jelly. When the solution is so far diluted as to contain only a thousandth of its weight of gum, alcohol no longer produces any visible effect; while a strong solution is immediately converted into a white, and perfectly opake fluid.

Cherry tree gum affected differently by reagents.

A substance which, in its physical properties, bears a strong resemblance to gum arabic, is the gum which exudes from the cherry tree; but I found the effects of reagents upon it to be considerably different. When the acetate of lead is added to a mucilage of cherry gum, there is no precipitate thrown down, but there appears a slight tendency to coagulation, and in the space of 24 hours the gum appears to be separated from its solvent in the form of fine filaments. The nitro-muriate of tin converts the mucilage into a solid jelly of a light yellow colour; the oxysulphate of iron causes no precipitation or coagulation, but changes the colour to a blackish brown; the nitro-muriate of gold causes an immediate opacity, and changes its colour to a light brown, but there is no precipitate thrown down; the superacetate of lead and the nitrate of mercury produce no effect. When alcohol is added to a strong solution of cherry gum, a number of filaments are formed, but the greatest part of the mucilage seems to incorporate with the alcohol without undergoing any alteration; the solid gum is not, however, in the slightest degree soluble in boiling alcohol. The infusion of galls produces no effect upon the mucilage of cherry gum. The cherry gum, when first dissolved in water, forms a uniform and transparent solution, but after being kept for some days in a warm atmosphere, it gradually exhibits a tendency to separation, a number of dark films are formed in it, which rise to the surface, and the whole becomes slightly turbid*.

Part of its solution incorporates with alcohol.

Separates from water by standing.

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* The result of my experiments on cherry gum will be found to differ very considerably from those of Dr. Thomson. *Chem.* V, 48.

Tragacanth.

When softened by boiling in water, may be dissolved by trituration.

1 part to 100: equal to 10 of gum arabic.

Effects of reagents on the solution.

A greater portion of fluid would not in

Tragacanth is, in many of its physical properties, considerably different from gum arabic, and its habitudes with the chemical reagents appear to be no less dissimilar. Water dissolves this substance with so much difficulty, that it has been said to be absolutely insoluble[†]. When tragacanth is digested in water, it absorbs a large quantity of the fluid, and is greatly increased in bulk, but even after being kept for some time at the boiling temperature, no proper solution seems to be produced. If, however, the tragacanth in this softened state be strongly rubbed in a mortar with an additional quantity of fluid, a real combination appears to be effected; a mucilage is formed, which possesses a homogeneous consistence, and retains the same state for several weeks, without manifesting any tendency to subsidence. A mucilage of this kind was made with 1 part of tragacanth to 100 parts of water; it was of about the same consistence with that composed of 1 part of gum arabic to 10 parts of water. To this mucilage the same 9 reagents were added as in the former experiments. In No. 1 a copious dense precipitate was instantly produced. In No. 2 there was a slight degree of coagulation, and a precipitate, which was increased in the space of 24 hours. In No. 3 a firm coagulum was instantly formed. No effect was produced in No. 4, except the mucilage was extremely dense, when its colour was changed to a dusky gray, and afterward to a blackish purple; but without any precipitation or coagulation. In the same manner the oxysulphate of iron produced no effect, except the mucilage was of a very strong consistence, when its colour was changed to a deep brown; but there was no precipitate or coagulum. The nitrate of mercury threw down a slight precipitate of a reddish tinge. Silicated potash produced no effect, except a very strong solution was employed, when there was a degree of opacity produced; but the same was observable upon the addition of caustic potash. It was difficult to ascertain the effect of alcohol upon tragacanth, because, however carefully the mucilage was prepared, any farther addition of fluid would not incorporate with it, but produced an appearance of precipitation.

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[*] Duncan's Dispensatory, p. 183. Thomson's Chemistry, V. 46.

In this case, however, I conceive that a proper precipitate was formed, because a greater degree of opacity was perceptible, upon the addition of alcohol to the mucilage, than from an equal quantity of water; and after remaining for some time, the effect was evidently increased, the solid matter being separated in a flocculent form. A turbidness was produced upon adding the infusion of galls to the mucilage of tragacanth, but the same difficulty occurred in determining whether there was any specific effect produced by the presence of the tan. I did not find tragacanth to be precipitated by the sulphate of copper, as stated by Dr. Duncan*.

corporate with it.

Mucilage of linseed

A well known vegetable mucilage is extracted from linseed. By adding a quantity of the seeds to 10 times their weight of water, a fluid was procured of about the consistence of the abumen ovi; when poured from one vessel to another it showed the same tenacity with the mucilage of gum arabic, and it also resembled gum in being indefinitely soluble in water, and in immediately incorporating itself with any additional quantity of fluid. Its chemical properties are, however, considerably different from those of gum. Upon the addition of the acetate of lead a copious, dense precipitate was immediately thrown down; with the superacetate of lead, and the nitro-muriate of tin, there was a considerable opacity; with the nitrate of mercury a slight precipitate only was formed; while the nitro-muriate of gold, the oxysulphate of iron, and silicated potash, produced no effect. When equal parts of the mucilage and alcohol were mixed, the fluid became slightly turbid, a degree of coagulation was produced, and at length the solid matter was separated in a flocculent form. Linseed mucilage is not precipitated by tan.

incorporates with water like gum;

but differs in its chemical properties.

Effects of the reagents.

Mucilage of quinceseed.

A substance, which in its physical properties, and in the manner in which it is procured, bears a strong analogy to the linseed mucilage, is that derived from the seeds of the quince. A quantity of these seeds, boiled for a few minutes in 40 times their weight of water, produced a fluid of about the same consistence with the linseed mucilage employed

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* Ed. Dispensatory, p. 183.

ployed in the last experiments. When poured from one vessel to another, it exhibited the same degree of tenacity; when recent it was perfectly homogeneous, but after being kept for some time, it showed a tendency to coagulation. It was submitted to the same reagents as in the former experiments; and with all of them, except the silicated potash and the galls, the effect produced was a greater or less degree of coagulation. With the acetate of lead the coagulation was so considerable, that the solid matter was instantly precipitated in the form of dense white flakes; the effect was equally rapid with the nitro-muriate of tin, but the coagulum was not in quite so dense a state. With the superacetate of lead, the nitro-muriate of gold, the nitrate of mercury, and the oxysulphate of iron, the coagulation was less complete, although sufficiently apparent. As the quince mucilage is coagulated by the mere addition of an acid, it is probable, that the effect produced in these cases may depend in some degree upon the acid, which enters into the composition of these metallic salts; yet the action of the acetate of lead and the nitro-muriate of tin were so remarkable, as to indicate the operation of a specific affinity. It may be farther observed, that a greater effect is produced by the acetate of lead, than by the super-acetate, although in this latter salt there is a portion of uncombined acid. The effect of alcohol upon the quince mucilage was exactly similar to that upon the linseed.

Coagulated by most of the reagents.

Coagulable by acids:

but this does not account for the effects.

Hyacinth roots.

The bulbous roots of many vegetables are composed in a great measure of mucilage, upon which I proposed to make my next experiments, and for this purpose I selected the hyacinth. Some of the roots of the common blue bell*, were bruised in an earthen mortar, and afterward rubbed with four times their weight of water. The whole was converted into a pulpy mass, which was strained through linen, and then appeared homogeneous, although opake. In a few hours a substance of a farinaceous appearance separated from it, and left the mucilage more transparent. It was now filtered through paper, and was in appearance and consistence very similar to the mucilage of linseed; although it

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* Hyacinthus non scriptus of Withering, scilla nutans of Smith.

differed

differed from it in soon showing a tendency to putridity, when it exhaled a very nauseous odour. The reagents used in the former experiments were employed in this case. The acetate of lead formed a dense precipitate, composed of white films and flakes; the super-acetate of lead threw down a precipitate in moderate quantity; the nitrate of mercury a precipitate of a light pink colour; the nitro-muriate of tin a copious white precipitate; the nitro-muriate of gold a light brown precipitate; the oxysulphate of iron a brown precipitate; the infusion of galls also formed a precipitate; while the silicated potash produced no effect*.

Their mucilage tends to putrefaction.

Tested with the reagents.

After having examined the properties of six species of mucilage, I next turned my attention to some bodies, which although obviously different from the mucilages, yet seem to have a close connection with them in their origin and constitution. The first of these is starch. A quantity of it was boiled in water, until it had acquired that state of half solution, of which it is alone capable, and in this form it was subjected to the usual trials. With the acetate of lead an immediate and very dense precipitate was thrown down, and so intimate a combination formed between the lead and the starch, that the water was separated from them nearly in a limpid state. By the nitro-muriate of tin a very copious precipitate was also thrown down, although less dense than the former; but no effect appeared to be produced by any other of the reagents. It is not without considerable diffidence, that I venture to dissent from the opinion of Dr. Thomson, on the subject of the action which takes place between tan and starch. When equal quantities of the mucilage of starch and the infusion of tan are mixed together, a precipitate is produced, which slowly subsides; it disappears by heating the fluid, and again becomes visible as it cools. This process is described as a characteristic property of tan; but I have observed the same appearance to ensue, if an equal quantity of water be added to the starch mucilage. It appears to depend merely upon the insolubility of starch in cold water.

Experiments on analogous substances.

Starch.

Effects of infusion of tan owing to the water.

The effects produced by the reagents upon paste, made

Flour paste.

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* These results differ from those of le Roux. Ann. Chim. 145 & seq.

by boiling flour in water, were somewhat different from those upon starch. The acetate of lead and the nitro-muriate of tin produced, as in the former case, very copious precipitates; the nitrate of mercury caused the fluid to assume a pink colour, and the nitro-muriate of gold a dusky gray, but without any precipitate; the super-acetate of lead, the oxysulphate of iron, the silicated potash, and the infusion of galls had no visible effect. It is necessary to observe, that in the experiments upon starch and paste, the substances themselves being opake, it is difficult to ascertain the effect of reagents upon them, unless it be considerable and immediate.

The effects of reagents not easily observable.

As starch and paste differ from each other principally in consequence of a quantity of gluten which exists in the latter, I thought it necessary to obtain this substance in a separate state, in order that its properties might be examined with more accuracy. I accordingly procured it in the usual manner, and digesting a quantity of it for some days in water, it exhibited marks of partial solution. The fluid was then filtered, and appeared homogeneous, although somewhat opake; in this state it was submitted to the reagents. Precipitates were thrown down by the acetate of lead, super-acetate of lead, and the nitro-muriate of tin. With the nitrate of mercury a precipitate was produced in moderate quantity, which very quickly subsided, while the fluid assumed a beautiful pink hue. With the oxysulphate of iron, and the nitro-muriate of gold, precipitates were also thrown down, the latter of a light brown colour. No effect was produced by silicated potash; but a very copious precipitate ensued upon the addition of the infusion of galls.

Gluten.

Partially dissolved.

Effects of the reagents.

I next wished to ascertain the properties of vegetable jelly, and for this purpose I procured a quantity of it from the pulp of the gooseberry. I could not, however, succeed in freeing it from the acid which it contains, and was, on this account, prevented from observing the operation of the metallic salts and the silicated potash. I found that a precipitate was formed by adding the infusion of gall to it.

Vegetable jelly.
From the gooseberry.
Not freed from acid.
Precipitated by galls.

I next submitted a strong solution of sugar to the action of the different reagents, but I found that no effect was produced in any of them, except the acetate of lead, which after some

Sugar.

some time became opake; but as this effect is produced merely by adding it to water, when exposed to the atmosphere, I was inclined to suppose, that the effect was produced independently of the sugar.

General conclusions.

These being all the substances to which I had an opportunity of extending my experiments, I must now inquire whether any general conclusions can be deduced from them. And first, as to the value of the different reagents employed as tests. The acetate of lead is by far the most delicate and copious in its effects, but it can be of little value in discriminating the different species from each other, because it produces its operation almost equally on all of them. With respect to the super-acetate of lead, we may in the first place remark how materially it differs from the acetate, with which, until lately, it was confounded. The acetate of lead was affected by all the substances to which it was applied, except sugar; while the super-acetate* produced no change upon gum arabic and starch, nor had it any specific effect upon quince mucilage; it was affected only in a slight degree by tragacanth, while with the linseed and hyacinth mucilage, and with gluten, tolerably copious precipitates were thrown down. The nitro-muriate of tin does not act upon gum arabic, but is more or less affected by every other kind of mucilage, particularly by those of cherry gum, tragacanth, and quince, and by starch of gluten. We have seen in how peculiar a manner the oxysulphate of iron acts upon gum arabic; it produces a brown colour in a strong solution of tragacanth; and it forms a precipitate with the hyacinth and the gluten; the quince is, as usual, coagulated; while no change is effected on the linseed or the starch. The nitro-muriate of gold is precipitated by hyacinth mucilage and by gluten; the quince, as in other cases, is coagulated by it, but its most remarkable effect is upon the tragacanth, the colour of which it converts to a deep blackish purple.

Acetate of lead the most active test, but not discriminative.

The superacetate acted very differently.

Nitro-muriate of tin.

Oxysulphate of iron.

Nitro muriate of gold.

Nitrate of mercury.

The nitrate of mercury throws down a precipitate of a singular nature from gum arabic, and tinges the fluid of a pink colour; the same shade is produced in the tragacanth, the

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* Dr Thomson must, no doubt, have employed this salt, when he states, that the acetate of lead does not precipitate gum.

hyacinth mucilage, and in the flour paste, and still more remarkably in the gluten; there is a slight precipitate in the linseed, and a coagulation in the quince, but no change of colour. Silicated potash. The silicated potash acts only upon gum arabic, and points out its presence when it exists only in a very minute quantity. Tan. Tan does not act upon any of the mucilages, except in a slight degree upon tragacanth; it is copiously precipitated by gluten, and also by vegetable jelly. Alcohol. The effects of alcohol have been fully stated. Gum arabic it precipitates from the water in such a manner as to render the fluid completely opake; whereas in the linseed, quince, and tragacanth mucilages, the solid matter was separated in a fibrous form; with the hyacinth mucilage both the pulverulent and fibrous kinds of precipitate were produced; while the cherry gum was only slightly affected by it. Alcohol precipitated starch in the same manner that it did gum arabic.

Nitric acid. The action of the nitric acid on the different mucilages was so similar, as not to exhibit any phenomena, which can assist us in distinguishing them from each other. According to the circumstances of the process, either the saclactic acid, or a mixture of this acid and the oxalic was produced, attended with the usual disengagement of gas. Sulphuric acid. It did not appear, that any use can be made of the sulphuric acid as a test of the different mucilages; I put in practice the process mentioned by Hermbstaedt, for separating gum from mucilage, but in no instance did I perceive the coagulation which he describes. The acid, in a concentrated state, slowly dissolves the different mucilages, and forms with them a thick, black fluid. Neutral salts. I did not observe any effect to be produced by the addition of the neutral salts, except that many of them coagulated the quince mucilage. Alkalis. The pure alkalis generally rendered the mucilages more fluid, but they did not exhibit any specific or discriminating effect.

Arrangement of the vegetable mucilages. Before we attempt to make any arrangements of the vegetable mucilages, it is necessary to inquire, whether the different varieties are to be considered as all of them homogeneous, or whether at least some of them ought not to be regarded as compounds of two or more of the primary mucilages. Some of them compounds. Although I am not acquainted with any method, by which the constituent parts of the compounds can be separated

rated from each other, yet I am inclined to believe, that this is the case; and we may conjecture, with some degree of plausibility, that those species are the most simple, that are acted upon by the fewest reagents. In the first place I consider gum arabic to be a homogeneous substance; and we may properly assign to it the specific name of *gum*, to which it is entitled by long usage and general consent. Its precipitation by silicated potash, and the orange coloured jelly which it forms with the oxysulphate of iron, are sufficient to constitute its essential characters; to which may be added the milky precipitate which is formed by the addition of alcohol to it, and the negative circumstance of its not being affected by the nitro-muriate of tin. I am disposed to regard as pure *vegetable mucus* the substance procured from linseed. It is sufficiently characterized by the effect of the superacetate of lead and the nitro-muriate of tin, and by the manner in which it is precipitated by alcohol from its aqueous solution; these circumstances, as well as the negative operation of the oxysulphate of iron and the silicated potash, serve to mark an obvious distinction between this substance and gum. A third vegetable principle, which is possessed of pecular physical properties, is *starch*. In its relations to the different chemical reagents it strongly resembles mucus, although it differs from it in the manner in which it is precipitated by alcohol. *Gluten* is a fourth substance, in every respect essentially different from any which we have hitherto examined. The most remarkable effect produced upon it, by any of the chemical reagents, is the change of colour induced by the nitrate of mercury, and the copious precipitate by the addition of tan.

Gum arabic homogeneous, and gum by way of eminence.

Its characters.

Vegetable *mucus* from linseed.

Starch.

Gluten.

In endeavouring to form an arrangement of vegetable mucilages, and to assign definite characters for the primary substances, which enter into the composition, we must consider to what degree of minuteness our subdivisions ought to be extended. If, for instance, we meet with a body, agreeing in every physical and chemical property with gum arabic, except that it is not precipitated by silicated potash, are we to regard this as a distinct vegetable principle, or simply as a variety of gum? This latter opinion I should certainly be inclined to adopt; for by indefinitely multiplying our

Extent of subdivision.

our principles, we should defeat the very end of arrangement. Hence, in the formation of our essential characters, we must endeavour to adopt the due medium between the extremes of minuteness and laxity. In each particular instance we must be guided by those properties, which are the most distinctly recognized, and the most readily ascertained, and which exhibit the closest analogy to each other. Under this impression I shall not think it desirable, in the present state of our knowledge, to proceed any farther in proposing an additional number of primary vegetable compounds; but I shall offer a few remarks upon the different mucilages, that have been made the subject of experiment.

Generic characters of gum.

Gum I consider as a generic term, which may be defined a transparent, brittle, insipid substance, indefinitely soluble in water, with which it forms a mucilage; the mucilage is precipitated by alcohol, in such a manner as to render the fluid perfectly opake; it is also precipitated by the acetate of lead in dense flakes. Under this genus we can, at present, rank only one species, viz. gum arabic, to which the following specific character may be applied. A gum, the mucilage of which may be precipitated by silicated potash; forms with the oxysulphate of iron a solid jelly, with the nitrate of mercury a precipitate of a pink colour; and is not acted upon by the superacetate of lead, the nitro-muriate of tin, or the nitro-muriate of gold.

Species and characters.

Generic characters of mucus.

The second genus is mucus, a substance seldom found in a separate state, but forming a frequent constituent of the seeds, roots, leaves, and other parts of vegetables. It is indefinitely soluble in water, and forms with it a mucilage; this is precipitated by alcohol in a fibrous form, without rendering the fluid opake; it is also precipitated by the acetate of lead, the super-acetate of lead, and the nitro-muriate of tin. Under the genus of mucus we may enumerate three species, that of linseed, of quinceseed, and of the hyacinth. To the first of these the generic character strictly applies; and to the second we may add the specific character of being coagulated by the addition of any acid, neutral, earthy, or metallic salt. The hyacinth mucilage I have also classed as a mucus, because its leading properties are such as point out its relation to the substances of this genus. In the state, however,

Three species.

Specific characters.

That of the hyacinth a compound.

however, in which it is usually procured, it seems to be a compound of two or more of the vegetable principles. A quantity of starch is obviously mixed with it; and I am inclined to think, that it also contains gluten, a supposition which will account for all the phenomena it exhibits with the chemical reagents.

Cherry gum.

There are two substances, the characters of which still remain to be ascertained, cherry gum and tragacanth. From its physical properties we should be disposed to place cherry gum in the same class with gum arabic; but it is so differently affected by the chemical reagents, as absolutely to prevent us from considering it in this point of view. Its properties are not more characteristic of mucus; nor does it afford any indications of being a compound substance, so that it may probably be necessary to consider it as a distinct vegetable principle. The action of the acetate of lead and of alcohol upon cherry gum would induce us to suppose, that it bore an analogy to sugar, rather than to the gums or mucuses; but the effect of the nitro-muriate of tin is not favourable to this supposition; nor is that of the nitric acid, which I found, by the usual process, converted it principally into the saclactic acid. If we are to bestow a new name upon it, we might denominate it cerasin.

Perhaps a distinct principle.

Cerasin.

Tragacanth.

I am equally unable to determine in what class tragacanth ought to be placed. It has obviously no relation to gum, either in its physical or chemical properties; and it differs very considerably from what has been laid down as the generic character of mucus. Its properties are the most similar to those of starch and gluten; particularly to that form of starch, which is prepared by first forming it into a mucilage with hot water, and then evaporating it to dryness, when it becomes transparent and brittle, but almost insoluble in water. The effect of the nitro-muriate of gold forms a remarkable character of tragacanth mucilage; it seems to depend upon a partial reduction of the oxide, at the same time that it unites with the tragacanth. The effect was very evident when the solid matter composed 1-50 part of the fluid, but if much more diluted, it was not perceptible; a slight degree of the same effect is produced when the nitro-muriate of gold is added to flour paste.

I am

I am fully sensible, that I have by no means executed the task, which I proposed to myself, of characterizing and arranging the vegetable mucilages; but a particular circumstance having, for the present, put a stop to my experiments, I was induced to publish this very imperfect attempt, in hopes, that the difficulties being pointed out, some more able hand may endeavour to remove them.

Liverpool, Aug. 12, 1807.

V.

Observations on Sulphurous Mineral Waters; by Mr. WESTRUMB*.

Sulphurous waters contain hidrosulphuret of lime.

MR. Westrumb has examined various sulphurous waters, and lately those of Eilsen in the county of Schaumbourg. One of the most interesting facts he has observed is, that all sulphurous waters contain more or less hidrosulphuret of lime.

The gasses being expelled by boiling, this is shown by sulphuric, nitric, or oxalic acid.

To detect this he boiled the mineral water, excluding the contact of atmospheric air, to expel the sulphuretted hidrogen gas and carbonic acid. Into the water thus boiled he poured sulphuric acid, when more sulphuretted hidrogen gas was evolved, and sulphate of lime was thrown down: fuming nitric acid, which separated from it sulphur: and oxalic acid, which expelled sulphuretted hidrogen, and formed oxalate of lime. The water evaporated in open vessels let fall sulphate of lime, and gave out sulphuretted hidrogen gas.

Method of ascertaining the quantity of sulphuretted hidrogen and carbonic acid gas in them.

To ascertain the quantity of sulphuretted hidrogen gas and carbonic acid, Mr. Westrumb proceeded as follows. He introduced the sulphurous water into a matrass, till it was filled to a certain point, which he marked; fitted to it a curved tube, which terminated in a long cylinder; filled this cylinder with limewater for the one experiment, and with acetate of lead with excess of acid for the other; luted the apparatus; the

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* Originally published in Gehlen's new Journal of Chemistry, and abridged by Vogel, Annales de Chimie, vol. lxii, p. 183, May, 1807.

and boiled the water till no more gas was expelled. When the limewater is used carbonate of lime is precipitated in the proportion of 20 grains to every 10 cubic inches of carbonic acid gas; when the solution of acetate, hidrosulphuret of lead is thrown down in the proportion of 19 grains to 10 cubic inches of sulphuretted hidrogen gas.

Sulphuretted nitrogen gas

Another observation, not less remarkable, relates to sulphuretted nitrogen gas.

in the waters of Aix-la-chapelle and Nenndorf. Its characters.

It is known that Dr. Gimbernat, a Spanish chemist, asserts, that the thermal waters of Aix-la-Chapelle contain sulphuretted nitrogen gas. Mr. Schaub too says, that he has obtained it from the sulphurous waters of Nenndorf in Hesse. The following characters are ascribed to this gas. 1. In smell it resembles sulphuretted hidrogen. 2. It is not decomposable by carbonic acid. 3. It is not inflammable. 4. It will not maintain combustion. 5. It is not decomposable by nitrous acid. 6. It is not decomposable by concentrated nitric acid, which separates from it sulphur. 7. It decomposes metallic solutions, and forms sulphurets. 8. It has a great affinity for water, from which it is separable only by long boiling.

Sulphuretted hidrogen gas has the same properties imparted to it by lime.

But Mr. Westrumb has found, that sulphuretted hidrogen gas, when washed with milk of lime, or passed through lime diluted with water, acquires all the properties here mentioned. Whether the sulphuretted hidrogen gas be obtained from sulphurous waters, or prepared artifically, the same phenomena take place. If the milk of lime be taken from it by an acid, sulphuretted hidrogen is disengaged, which is inflammable, and possesses the usual properties. Sulphuretted nitrogen gas therefore is a product of the operation.

Whether a product or an educt doubted.

Mr. Westrumb however is in doubt, whether this new gas be produced by the action of quicklime on sulphuretted hidrogen, or whether the sulphuretted hidrogen gas contain sulphuretted nitrogen.

Carbon in sulphurous waters.

A third observation, not less interesting, is the presence of carbon and carburetted substances in sulphurous mineral waters.

A fetid resin of sulphur found in them.

Mr. Westrumb has discovered in them a new principle, a fetid resin of sulphur *(stinkendes schwefelharz)*. To obtain this, the sulphurous water must be evaporated in open vessels

vessels, and the residuum dissolved in alcohol, which takes up this resin and the earthy muriates. By evaporating the alcohol, this substance appears at first as a yellowish fat, which gradually assumes a brown colour, and becomes resinous. By repeated solutions in alcohol and evaporations it is decomposed into sulphur and a blackish brown resin. It emits a garlic smell; which becomes very strong, and similar to that of assafetida, if water be poured into the alcoholic solution. Its solution acts as an acid.

Smells like garlic.

The resin is soluble in ammonia, and communicates to it a yellow colour. This liquid comports itself like that of Beguin. With limewater a hidrosulphuret is formed. All these solutions act on metallic compounds in the same manner as sulphuretted hidrogen.

Soluble in ammonia.

Forms hidrosulphurets.

As sulphurous mineral waters arise from strata of pitcoal, perhaps the source of this bituminous principle may be traced to the coal itself.

Perhaps from the bitumen of coal.

Round the baths of Eilsen, as round those of St. Amand, a mud accumulates, which in time grows darker coloured, and ultimately black. From this are obtained, on analyzing it, fetid sulphurous resin, hidrosulphuret of lime, sulphur, lime, alumine, magnesia, charcoal, and sand, with some fibrous substances; and a little sulphuretted hidrogen gas, and carbonic acid gas.

Black mud of the baths of Eilsen.

Whatever may be the origin of the bituminous principle in the sulphurous waters, Mr. Westrumb, assisted by Mr. Basse, has been able to produce charcoal and the fetid resin from pure sulphur. For this purpose he has digested in alcohol sulphur precipitated from sulphuretted hidrogen by an acid. On distilling off part of the alcohol, sulphur is separated in yellow crystals, or in a yellowish gray powder. The fetid resin is then completely formed in the supernatant liquor, and possesses all the properties mentioned above.

Charcoal and the fetid resin produced from sulphur.

Its formation may be ascribed to the concurrence of the alcohol; particularly as after its separation from the residuum left on evaporating sulphurous water, the pungent smell is manifested on its being taken up by alcohol. But several observations lead Mr. Westrumb to believe, that alcohol does not contribute to the formation of this substance,

Not formed by the alcohol.

stance, but rather that it derives its origin from the sulphur itself.

Messrs. Westrumb and Basse intend to pursue the inquiry, and promise to make known the results.

The inquiry will be pursued.

VI.

Report on a Memoir of Mr. BERTHOLLET, *Jun., entitled: Inquiries concerning the reciprocal Action of Sulphur and Charcoal; by Messrs.* FOURCROY, DEYEUX, *and* VAUQUELIN *.

Lampadius's sulphur alcohol,

IN 1796 prof. Lampadius, of Freyberg, endeavouring to ascertain how much sulphur a martial pyrites mixed with charcoal would furnish when acted upon by fire, obtained a very volatile sulphurous liquid, which he suspected to be a compound of sulphur and hidrogen, and to which he afterward gave the name of sulphur alcohol.

hidroguretted sulphur.

Carburetted sulphur of Clement and Desormes.

Clement and Desormes, unacquainted with this fact, when afterward endeavouring to demonstrate, that there is no hidrogen in well burned charcoal, by passing sulphur in vapour over very hot charcoal, obtained a product exactly similar to that of Lampadius. Having remarked, that the charcoal in this operation was destroyed without evolving any gas; and that the liquid product, when burned, left some slight black spots on the vessel that contained it; they thought, that this liquor was formed by the combination of sulphur with charcoal, and consequently termed it carburetted sulphur.

Examined by Berthollet, jun.

This difference of opinion respecting its composition induced Mr. A. B. Berthollet, to examine experimentally this question, with which many interesting points in chemistry are connected.

Description of his apparatus.

His apparatus, which does not differ much from that of Clement and Desormes, was constructed in the following manner. A straight glass tube, about a yard long, was

placed

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* Annales de Chemie, Vol. LXI. p. 127. Feb. 1807.

placed in a reverberatory furnace, so that one end projected a little more than four inches beyond the wall of the furnace; and the other end, which sloped gently upward, about eighteen inches. The part within the furnace was coated with a lute capable of sustaining a very strong heat. To the lower end was fitted an adopter, terminating in a small tubulated receiver, from which a curved tube passed into water contained in a two-necked bottle. Another tube with two bendings conveyed the gas from this bottle into a pneumatic apparatus.

Charcoal included in the tube, with sulphur above it, and heat applied.

Charcoal being included in that part of the tube that was luted, and sulphur introduced into the empty part, its upper end was hermetically sealed; the tube was gradually heated to incandescence; and the sulphur as it melted flowed down into the charcoal. When these came into contact, bubbles of gas were extricated, that succeeded each other rapidly, and were accompanied with white vapours, which, condensing in the adopter, passed into the receiver, and sunk to the bottom of the water in the form of a white or sometimes yellowish oil.

Products.

These liable to vary.

Mr. Berthollet however observes, that the results of this operation vary according to a number of circumstances, which he has carefully described; and the chemist not having it always in his power to render these circumstances perfectly similar, different products are frequently obtained.

Circumstances that affect them.

If for instance the extrication of gas and condensation of liquid slacken, the sulphur must be heated, that more may pass down; and if this do not accelerate the operation, the temperature of the charcoal must be increased. When the operator is desirous of producing much of the liquid, it is necessary to raise the temperature of the charcoal a little above a cherry red, and to allow only a slight excess of sulphur to pass down. Too little of the sulphur produces only gasses, and a few drops of a liquid lighter than water, which in the course of the process resumes the state of gas. On the contrary, if the sulphur be too abundant, nothing is formed but gasses, and solid hidroguretted sulphur, which was mistaken for carburetted sulphur by Clement and Desormes. It is always advantageous, to keep the vessels, in which

The receivers

which the liquid is to be condensed, in a refrigerating mixture.

to be kept very cool.

Two stages in the process.

In conducting the operation with these precautions, the extrication of gas will not take place after a certain time, unless both the temperature of the tube and the quantity of sulphur be augmented.

Necessary precautions.

The charcoal used by Mr. Berthollet was always previously heated for half an hour, to expel the water and gasses that yield to simple heat. When the operation was finished, he kept up the temperature of the tube, that the nature of the residuum might not be altered by the sulphur in contact with it; and for the same reason he prevented all access of air to the apparatus, by turning a cock adapted to the tubulure of the bottle.

Products of the first stage.

On examining the products of the operation, stopped at the end of the first stage, he found: 1. That the water in the bottle, which was milky, had the smell and all the properties of sulphuretted hidrogen water. 2. That the gas itself had a similar smell, dissolved in water by agitation or long contact, and communicated to it all the characters of sulphuretted hidrogen. 3. That this gas burned with a blue flame, and diffusing a smell of sulphurous acid. 4. That when mixed with oxigen gas it detonated briskly with the electric spark, sometimes without rendering limewater turbid, but more frequently producing a slight precipitate, and depositing sulphur.

Evidently sulphuretted hidrogen, though little soluble in water.

From these characters every one must recognise in it sulphuretted hidrogen; though Mr. Berthollet found, that it was less soluble in water than what is obtained by the ordinary means. Scheele, Kirwan, and others, however, have mentioned combinations of sulphur and hidrogen, which were little, if at all, soluble in water.

A liquid like that of Lampadius.

The liquid collected under the water in the receiver, and in the bottle, had perfectly similar properties to those described by Lampadius, and by Clement and Desormes: that is, it was as transparent as water; it emitted a smell resembling that of sulphuretted hidrogen, but more lively and pungent; shaken in a phial with water, it adhered to the glass, and rendered it greasy like an oil; and it burned rapidly, with a blue flame, and smell of sulphurous acid.

Left no charcoal when burned.

He did not obtain any charcoal, however, as a residuum of its combustion; the circumstance on which Clement and Desormes founded their opinion: for it burned entirely away, or, if the combustion were stopped before it was completed, left nothing but sulphur.

Its characters.

This liquid, being very volatile, produces on the skin a sensation of great cold. It dissolves in the air, greatly increasing its volume; and then burns calmly with a blue flame, and does not detonate by the electric spark. If water be admitted to the air thus expanded, the air returns to its original bulk, and the water acquires the properties of sulphuretted hidrogen. This Mr. Berthollet observes would of itself be sufficient to prove the existence of hidrogen in the liquid.

Evidently contains hidrogen.

Not entirely volatile, sulphur being left.

However transparent it was, he could never volatilise it entirely. Whether he left it exposed to the air, or assisted its evaporation by heat, he had always a residuum of sulphur, which he could sublime completely, without perceiving any vestige of charcoal.

The gasses it produce afforded no indications of charcoal.

The residuum of this liquid affording Mr. Berthollet no charcoal, he examined the gasses it produced, to ascertain its existence. But neither its combustion with oxigen gas in vessels placed over water, nor the action of oxigenized muriatic acid, nor that of alkalis, produced any indications of charcoal, or of carbonic acid. In the first case the product of its combustion did not render limewater turbid; in the second nothing was found but sulphuric acid mixed with muriatic; and in the third a combination was obtained, which comported itself like the hidroguretted sulphurets.

It is therefore a compound of sulphur and hidrogen.

From all these facts the author concludes, that the liquid produced by the reciprocal action of incandescent charcoal and sulphur is formed of sulphur and hidrogen, as Lampadius announced; and, contrary to the assertion of Clement and Desormes, contains no charcoal. These facts at the same time show, that sulphur and hidrogen, like many other substances, are capable of uniting in various proportions, according to circumstances; and that the predominating ingredient always communicates some of its properties to the compound. In the present case for instance, if the sulphur be

These unite in various proportions,

be very abundant, the compound takes the solid form: if the proportion of hidrogen be increased, the attraction of its particles is diminished, and it resolves itself into a liquid: if a still greater quantity of hidrogen be present, the compound expands, and the result is a gas.

which alter the characters of the compound.

Mr. Berthollet has made a very interesting experiment, which greatly confirms this. The liquid in question, distilled with water at a temperature of 36° [113° F.] afforded him a gas, that had the smell of sulphuretted hidrogen, burned with a blue flame, detonated briskly with oxigen when fired, and combined readily with water, which it turned milky, communicating to it the properties of sulphurous water. After this gas a transparent liquor came over, swimming on water, and which, as it evaporated on the contact of air, precipitated to the bottom, or disappeared entirely, leaving only some slight traces of sulphur on the water. At 45° [133°] the extrication of gas ceased, and a liquid heavier than water succeeded. The colour and consistency of this liquid increased, as the distillation proceeded.

The liquid distilled with water at a low heat gave a gas,

a light liquid,

a heavy liquid,

On stopping the process when the temperature had been kept some time at 45° [133°], what remains in the retort becomes solid by cooling, and prismatic crystals are distinguished in the mass. If a sufficient quantity of sulphur be not melted down on the charcoal, liquids of different densities are likewise obtained: the heaviest condense in the receiver; the lighter do not condense, till they reach the bottle, where they rise to the surface of the water; and lastly others, carried off by the gasses, reach the pneumatic apparatus.

and a solid remained.

When the sulphur is deficient liquids of different densities obtained.

It is evident, that the efficient cause of these different modifications, depending on the respective qualities of the elements that combine, is the difference of temperature, which disengages first the most expansible bodies. This is not peculiar to the compound of sulphur and hidrogen; and the effect is the more obvious, the greater the difference in the expansive force of the substances.

These modifications owing to difference of temperature.

The sulphur that flows into the adopter during the operation contains a certain quantity of hidrogen, which gives it a laminated texture, an inferior density, and in particular a very decided smell of sulphuretted hidrogen, a small quantity

The sulphur that flows by the charcoal contains hidrogen, tity of which Mr. Berthollet obtained from it by means of a gentle heat.

but no charcoal. Some manganese and iron.

But by no method could he discover in it charcoal; though he imagined he discerned some very slight traces of manganese and of iron, arising from the charcoal, or from the sulphur itself.

The same products when hidroguretted sulphurets are decomposed by acids.

A fact long known, that confirms the conclusions of Mr. Berthollet, is, that the same products may be obtained by decomposing hidroguretted sulphurets by means of acids, as by distilling sulphur over charcoal, namely, sulphuretted hidrogen in the state of gas, liquid hidroguretted sulphur, and solid hidroguretted sulphur; and in all these substances there is no charcoal.

The charcoal not altered in appearance, but combined with sulphur.

On examining the charcoal remaining in the apparatus after having been long exposed to the heat, Mr. Berthollet found no external appearance of alteration. It retained sulphur in actual combination, which heat could not separate, but which might be dissolved by an alkali, or burned by heating in contact with air. The charcoal is then very light and friable, leaves fine black traces on paper, and burns with difficulty. Charcoal therefore can combine with sulphur; but this compound assumes neither the liquid nor the gaseous state.

Charcoal contains hidrogen;

All the facts adduced by Mr. Berthollet clearly demonstrate the presence of hidrogen in charcoal, from which it is inseparable by any heat we have yet been able to produce.

which sulphur aided by heat, takes from it.

If sulphur take it from charcoal, it is by combining its chemical action with that of heat: and perhaps this may be a means of depriving charcoal entirely of hidrogen, and obtaining it in a state of purity, so as to describe its properties, which from this observation may be yet unknown to us.

The whole of the charcoal may be made to disappear;

Mr. Berthollet has remarked however, that, when all the phenomena already described have taken place, if the temperature be strongly raised, and a great deal of sulphur caused to pass, the extrication of gasses recommences, and the charcoal may be made to disappear entirely. On stopping the process before this arrives, pieces of charcoal exhibiting evident marks of erosion will be found in the tube. The little liquid obtained in this second stage of the process is so volatile, that it soon reassumes the state of gas. The sulphur that flows into the adopter contains no more charcoal,

coal, than what passed at the commencement, but it contains hidrogen. An experiment made on a gramme [15½ grs.] of charcoal, previously heated for an hour in a forge fire, continued five or six hours, and produced four or five litres [or wine quarts] of gas.

and a gaseous compound of carbon, hidrogen, and sulphur, is formed.

This gas resembled sulphuretted hidrogen in its smell, manner of burning, solubility in water, and the properties it communicated to water. But water did not absorb it completely; and the product of its combustion rendered limewater considerably turbid. In this gas therefore, analogous probably to that which Clement and Desormes termed gaseous carburet of sulphur, the carbon subjected to the experiment is found. It is in fact a triple compound of carbon, hidrogen, and sulphur; requires for its combustion nearly an equal bulk of oxigen gas; and at the moment of taking fire has its volume increased at least tenfold.

With respect to the doubts, that may arise respecting the nature of the precipitates formed in limewater in these experiments, Mr. Berthollet gives means of ascertaining, whether they be owing to sulphurous acid or carbonic.

Some hidrogen essential to charcoal.

Reflecting on the complete destruction of charcoal by the action of sulphur, and the nature of the products it furnishes, we are tempted to believe, that charcoal is inseparable from a certain quantity of hidrogen; and that, at a high temperature, the sulphur in contact with it occasions a new order of compounds, which assume the elastic state.

The sulphur too probably contains some.

But on considering the quantity of gas obtained, and the property sulphur has of retaining hidrogen in the solid state, Mr. Berthollet suspected, that possibly the sulphur itself furnished a certain quantity of the gas. To verify this ingenious suggestion, he passed sulphur through a glass tube, coated, and brought to a white heat; adapted to it a tube, for collecting the gas; and obtained some very slight indications of sulphuretted hidrogen.

Experiments to prove this.

From the formation of metallic sulphurets.

On the other hand, on forming metallic sulphurets in earthen retorts, after taking all possible precautions to remove every source of uncertainty, he obtained sufficient sulphuretted hidrogen gas to precipitate the solution of lead, and to be set on fire. In these experiments he employed iron

iron prepared expressly for the purpose, copper, and mercury. The last metal afforded him the most.

Aqueous vapour passed over melted sulphur takes up hidrogen, without being decomposed.

On this occasion too he repeated an experiment of Priestley's, who produced sulphuretted hidrogen gas, by passing the vapour of water over melted sulphur: and he found, that the water was not decomposed, for he could discover no trace of sulphuric acid; it only served therefore, to disengage the sulphuretted hidrogen. Many other facts in confirmation of these experiments might be adduced if necessary.

General conclusions.

From the experiments of Mr. Berthollet we may conclude:

1. That charcoal contains hidrogen, which the most intense heat we can produce will not completely expel.

2. That sulphur at a red heat acts upon hidrogen, and forms compounds in very different proportions, on which their properties depend.

3. That charcoal deprived of hidrogen, or at least nearly so, forms with sulphur a solid compound, into which the sulphur enters in a small proportion.

4. That at a high temperature sulphur, carbon, and hidrogen unite into a compound, which assumes the state of gas.

5. And lastly, that sulphur contains hidrogen.

Experiments by Robiquet confirm those of Berthollet.

While Mr. A. B. Berthollet was examining the nature of this compound, Mr. Robiquet, apothecary to the hospital of Val-de-grace, was likewise making experiments on it, at the suggestion of Mr. Vauquelin, to whom Mr. Berthollet's intentions were unknown. These led to similar conclusions; but were discontinued, as soon as Mr. Vauquelin was acquainted with the labours of Mr. Berthollet. Mr. Vauquelin had likewise given a tolerable quantity of the liquid to Mr. Biot, that he might ascertain, if possible, from its refractive power, the proportion of hidrogen it contains.

Biot supposes sulphur to contain hidrogen.

We may add, that Mr. Biot had already inferred the presence of hidrogen in sulphur, during the course of his experiments on refraction.

VII.

Account of the Metallurgic Treatment of Pyritous Copper at the Mines of Chessy and Sainbel, in the Department of the Rhone: by Mr. GUENIVEAU*.

Products of the works analysed to explain the theory of smelting copper pyrites, and the effect of quartz;

THIS paper contains several results of the analysis of the metallurgic products of the works at Chessy, which appear calculated to serve as bases for the theory of smelting copper pyrites, and particularly to show the effect of quartz added to the ore in this operation. The fifth part is employed in describing those chemical operations, that afforded new results; and the remainder in deriving from those results the most remarkable consequences, and pointing out their application.

in concert with Hassenfratz and Descotils.

The chemical experiments were made in part at the laboratory of the School of Mining of Mont Blanc, under the inspection of Mr. Hassenfratz, and partly at the laboratory of the Council of Mines, under Mr. Descotils.

They were the scoriæ of the ore, and of the matts.

Characters of the 1st,

of the 2d,

common to both.

The metallurgic products subjected to analysis were scoriæ from the pyritous copper, taken from the basin of the fore hearth during the operation. Of these there are two sorts, one formed during the fusion of the roasted ore, the other during that of the roasted matts. The first sort is of a tolerably brilliant metallic gray colour; exhibits laminæ, indicating a crystallization; and may easily be confounded with certain ores of oxidulous iron. The second has no metallic lustre, is of a brown colour, and of a fibrous fracture. The characters common to both are, they are tolerably compact, without any vitreous appearance, and almost without blebs; attractable by the magnet; melt before the blowpipe without addition, sometimes emitting a slight smell of sulphur, and with borax exhibiting the same characters as iron ores; yielding iron when assayed in the dry way, and sometimes traces of copper; and forming a jelly with acids with great facility.

General mode of analysis.

The following is a general description of the mode in which they were analysed. After they were powdered, they

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* Abridged from the Journal des Mines, Oct. 1806, Vol. xx, p. 245.

were treated with concentrated muriatic acid, mixed with a little nitric acid. With this they formed a tolerably tenacious jelly, which was diluted by adding water, and boiling it. The insoluble part, become very white, was separated, dried, weighed, and then heated red hot, to expel the sulphur. The residuum had all the characters of silex. The muriatic solution contained no sulphuric acid. To this ammonia was added in excess, which gave indications of copper and dissolved the zinc. The precipitated oxide of iron was treated while wet with a solution of caustic potash, to separate the alumine. Into the ammoniacal liquor oxalate of ammonia was poured, to separate the lime: after which an excess of sulphuric acid was added, in order to precipitate the copper by a slip of iron. By a large quantity of ammonia the zinc was separated from the iron introduced; and the proportion of this metal was determined by converting it into a prussiate.

The first precipitate of oxide of iron was examined anew, in the hope of finding in it portions of copper and of lime, which might have escaped the first operations. The sulphur, copper, and zinc, were examined by separate experiments.

Component parts of the scoriæ No. 1.

From 100 parts of the scoriæ of the roasted ore were obtained silex 31, red oxide of iron 75, metallic zinc 2. Beside which there were some indications of copper and sulphur, and an atom of lime.

No. 2.

From 100 parts of the scoriæ of the roasted matt were obtained silex 22, red oxide of iron 90, lime 3, sulphur 3; beside a trace of copper and of zinc.

Assay gave 50 of iron.

The assay in the dry way, with Guyton's flux, gave 50 per cent of iron.

The iron slightly oxided.

The iron in these scoriæ is very little oxided, for the weight of all the products of the analysis exceeds that of the substance employed, and ammonia precipitates the muriatic solutions of a green colour*. It is observable too, that the second kind of scoriæ contained much more iron than the first. Some

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State of the iron doubtful.

* The state of the iron in this combination appears to me very doubtful: for, if we consider it as black oxide, that is with 27 per cent of oxigen, it is not easy to explain the effervescence, that the powdered scoriæ

Some time afterward Mr. Gueniveau examined a specimen of scoriæ of the first kind, obtained in like manner from the fusion of the ore, which was in the collection of the Council of Mines. His principal objects were to ascertain whether it contained manganese, and whether the insoluble residuum had a mixture of sulphate of barytes: but as the component parts of this specimen differed from those of the former, he gives the following account of his analysis. Another specimen, No. 3, analysed for manganese and sulphate of barytes.

He treated 5 gram. [77grs.] of the scoriæ, thoroughly pulverised, with ten of caustic potash, in a silver crucible. The fusion was complete; and on its being suffered to grow cold, and a large quantity of water being added, no green colour appeared, which evinced, that no manganese was present. Analysis. No manganese.

Another portion he treated with concentrated muriatic acid mixed with a little nitric, to detect the alkalis. Having separated the insoluble residuum, he treated the solution with ammonia, which separated a part of the earths and metals. The copper and zinc dissolved were then precipitated by sulphuretted hidrogen; the excess of this gas was expelled by ebullition; the lime was thrown down by oxalate of ammonia; and the liquor was evaporated to dryness. The ammoniacal salts were then driven off by bringing the residuum to a red heat in a platina crucible, and a pretty considerable quantity of fixed salts remained. Having dissolved these in a very small quantity of water, he added to the solution muriate of platina, which occasioned no precipitate; and endeavoured, but in vain, to obtain crystals by cautious evaporation and cooling; which convinced him, that neither potash nor soda was present. Examined for alkalis. Neither potash nor soda.

By various trials he found, that the fixed residuum consisted of the muriates of barytes and magnesia. Barytes and magnesia.

Lead too was an object of his particular research; but he could not discover an atom. No lead.

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scoriæ make with sulphuric acid a little concentrated, and with dilute muriatic acid; an effervescence owing to the decomposition of water, since the gas has the smell of hidrogen set free by iron. The same phenomenon has been observed in scoriæ from sparry iron ores, which were not attractable by the magnet, and contained a great deal of manganese, and but little iron.

From these data Mr. Gueniveau undertook a complete analysis of this specimen of the scoriæ of Chessy.

Its external characters.

Assay.

Its external characters were the same as those of the scoriæ No. 1. Its specific gravity was 3·61. Assayed with borax it gave 23 per cent of iron, without any signs of copper.

Analysis.

Treated with concentrated muriatic acid, and a little nitric.

Five grammes, well powdered, were treated with concentrated muriatic acid, mixed with a little nitric. The whole coagulated into a jelly. On this water was poured, it was boiled, and fresh acid was added; by which means a very white residuum was obtained. The decanted liquors were mixed, and evaporated to dryness, in order to separate the silex completely from the muriatic acid. Water being added, and filtered, a residuum was collected, which contained no sulphur, and weighed 1·75 gr. after having been exposed to a red heat. This was fused with very pure caustic potash; and the whole of the compound being soluble in muriatic acid, Mr. Gueniveau inferred, that the residuum was very pure silex, without any mixture of sulphate of barytes.

Residuum pure silex.

Muriatic solution precipitated with sulphuric acid,

and hidrosulphuret of ammonia.

The muriatic solution gave with sulphuric acid a precipitate of 0·90 gr. of sulphate of barytes, with which he ascertained no sulphate of lime was mixed, and which was heated red hot before it was weighed. The same solution was treated with hidrosulphuret of ammonia, which precipitated the metals and alumine, leaving in solution lime and magnesia. The sulphurets were roasted, and afterward dissolved by nitro-muriatic acid: and lastly, the sulphur and sulphuretted hidrogen were separated from the preceding liquor. He had then two solutions; one, A, containing lime and magnesia; the other, B, containing the metals and alumine.

Lime thrown down by oxalate of ammonia,

and magnesia by potash.

From the solution A the lime was thrown down by oxalate of ammonia; and the precipitate, being strongly calcined, left 0·18 gr. of caustic lime. The magnesia, which remained, was precipitated by potash; and, after being calcined, weighed 0·1 gr.

Iron and alumine precipitated by ammonia, and separated by potash.

The solution B was treated by ammonia, an excess of which dissolved the zinc and copper. The oxide of iron and the alumine, which fell down together, were separated by liquid caustic potash, assisted by heat. The red oxide of iron after calcination weighed 2·30 gr.; and the alumine, after

after exposure to a red heat, 0.21 gr. As the ammoniacal solution of copper and zinc had a very slight blue colour, it did not appear necessary, to separate the few atoms of copper: so the whole was precipitated together by carbonate of potash, assisted by a boiling heat. The carbonate, being calcined, gave 0·17 gr. of zinc mixed with a very small quantity of copper. The quantity of copper and zinc was ascertained likewise by separating them from the iron by sulphuretted hidrogen, and it did not differ perceptibly from this.

Copper and zinc by carbonate of potash.

One hundred parts of these scoriæ gave therefore silex 35, barytes, not sulphurated, 12, alumine 4, lime 3, magnesia 2, red oxide of iron 46, oxide of zinc 3, beside some traces of copper and of sulphur.

Component parts of scoriæ No. 3.

This specimen contained much less iron than No. 1, which was formed under the same circumstances: but the difference will not appear surprising to those, who know how much the proportions of the gangue, and foreign matter added to the ore, vary in different smeltings. The following are the most important consequences deducible from these analyses.

Less iron than No. 1.

1. The predominant parts of the scoriæ of the pyritous copper of Chessy are oxide of iron and silex, the rest being variable, and in small proportion.

General inferences.

2. The combination of the silex with the oxide of iron is effected in the furnaces in which the ore is smelted.

3. The presence of barytes, not sulphurated, announces, that the sulphate of barytes, which made part of the gangue, is decomposed. No doubt it had been converted into a sulphuret, the sulphur of which was taken up by the iron or copper, and the free barytes remained in the scoriæ.

4. The scoriæ arising from the smelting of the copper pyrites and matts may be considered as iron ores, since they yield a considerable quantity of iron when assayed.

The application of chemistry to the art of treating ores, observes Mr. Gueniveau, is considered by all metallurgists as one of the most certain means of elucidating its operations, and consequently improving its processes: and accordingly he relates in a few words the principal parts of the metallurgic treatment of the pyritous copper of Chessy, from

Chemistry the means of explaining metallurgical processes.

from the account given by Messrs. Lemaire and Bouësnel, in order to render more perspicuous the comparison between the chemical and metallurgical results.

Treatment of the ore of Chessy, which is very poor.

The greater part of the ore wrought at Chessy is very poor in copper. It is a mixture of sulphuretted iron, sulphuretted zinc, pyritous copper, and a small portion of earthy substances, as quartz and sulphate of barytes. The ore after pounding is roasted by a very economical process, which saves the greater part of the sulphur contained in it. It is then smelted, with the addition of scoriæ from preceding smeltings, and quartz broken to pieces, the proportion of which varies from seven to ten hundredths of the weight of the ore.

Pounded, roasted, and smelted with the addition of scoriæ.

This smelting affords two products; the scoriæ, of which analyses have been given, Nos. 1 and 3; and matts, or sulphurets of iron and copper. These matts are roasted several times, and resmelted with scoriæ and quartz, but in less proportions than at first. By these operations are obtained black copper, matts rich in copper, and the scoriæ analysed No. 2. In all these fusions no oxide of iron is reduced*.

Matts roasted repeatedly, and resmelted with scoriæ and quartz.

The

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* Most of the ores and products of the smelting have been subjected to analysis, and assayed in the dry way. The following are some of the results.

Analysis of the poor ore.

The poor ore, called at Chessy meagre ore, is separated from what they call the yellow ore, which is much more rich, and is roasted and smelted separately. The poor alone is considered here. The crude ore roasted by itself, or in the great, most frequently yields no copper in assaying. Once with four parts of black flux Mr. Gueniveau obtained from it 1½ per cent of tolerably pure copper. By chemical analysis it never afforded him more than 4 or 5 per cent of copper; but it contained from 26 to 55 of iron, according as it was mixed with the gangue, or more or less roasted. The mean of the iron may be estimated at 40 per cent.

Matts.

The matts obtained from the first smelting of the ore amount to seven hundredths of its weight. They contain about 25 parts of sulphur, 40 of metallic iron, and from 25 to 27 of copper. It is of importance to observe, that of 45 parts of iron introduced into the furnace not above 3 or 4 remain in the matt after the first fusion. The whole of the zinc is volatilised in this operation.

Black copper and rich matts.

The matts are roasted and resmelted, and the products are black copper, containing 1 or 2 per cent of metallic iron; and rich matts, containing

The use of quartz essential.

The mixture of quartz with the ore deserves particular notice for its importance. Many trials have been made to use other earthy substances in its stead, but they were without success; and if a vein of quartz had not been discovered in the neighbourhood, the working of this poor ore must have been given up.

The sulphur separated by roasting, and the zinc sublimed.

The iron most troublesome.

Its oxide combines with the silex.

Of the component parts of the ore, the sulphur is separated in great part by roasting, and the zinc sublimes at the first smelting: but the iron, the proportion of which in the metallic state amounts to at least 40 per cent of the mass to be fused, is unquestionably the most embarrassing for its quantity, and because by being reduced it choaks up the furnace, and stops the operation. The intention of adding quartz is to carry the iron out of the furnace. In fact the analyses above given show, that a combination takes place between the silex and oxide of iron; and that this homogeneous compound becomes sufficiently fluid in the heat of the furnace, to allow the copper and the matts to separate from the mere difference of specific gravity; so that itself can be taken off the basin of the fore hearth during the fusion.

Their affinity pretty strong.

Iron here yields sulphur to copper from a double affinity.

The affinity between the parts of this compound must be pretty strong, to prevent the reduction of the oxide of iron, and to enable it to yield its sulphur to the copper, which does not happen but through some particular circumstance, as in this case the presence of silex. It is an established fact in metallurgy, that iron takes sulphur from copper: here on the contrary the iron yields the sulphur to the copper, while itself unites with a compound in which it is already abundant to form the scoriæ*, and does not appear in the

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taining about 21 parts sulphur, 66 copper, and 8 metallic iron. It appears from various calculations, that not above a hundredth of the iron in the roasted ore remains in these two products, while they contain nearly half of the copper, that was in such small proportion.

Iron carried off in the scoriæ.

The iron therefore has been separated and carried off in the scoriæ, since no other product retains any considerable quantity.

Zinc.

The sublimed matter, or cadmia of the furnaces, is nothing but oxide of zinc with some slight traces of copper, and free from iron.

* Beside the quartz and scoriæ presumed to retain copper, scoriæ from the smelting of the ore, containing about 0·66 of oxide of iron only (analysis No.

the rich matts, except in quantity sufficient as it were to complete the saturation of the sulphur.

A small quantity of copper extracted from a large portion of iron.

The process adopted at Chessy is as remarkable for the manner in which it accomplishes its object, as for the simplicity of the agents it employs. From a ferruginous mass containing only 4 or 5 per cent of copper, more than half this is obtained; a result, that theory could scarcely have predicted, since it can hardly render it conceivable.

Oxide of iron combined directly with silex.

To ascertain how far oxide of iron is capable of direct combination with silex, Mr. Gueniveau intimately mixed some red oxide with powdered quartz; put the mixture into a crucible lined with a thin stratum of levigated quartz; and exposed it to a forge fire equal to the heat of the furnace.

Forms a glass,

The first trials produced only glasses more or less opake and coloured; but at length he formed a compact compound of a metallic aspect, exhibiting laminæ indicative of crystallization, and in fact perfectly similar to the scoriæ. The coating of the crucible was attacked, but the crucible was not melted. When the silex was equal or superior in weight to the oxide, the product was vitreous; when it was only a third, fourth, or sixth of the mass, it was compact and metallic.

unless the iron be predominant.

The operation imitated in the small way.

He likewise attempted to produce in a crucible the same effects as took place in the furnaces of Chessy. Accordingly he mixed with great care some of the rich copper pyrites, as well roasted as possible, with 14 hundredths of its weight of pure silex, made it into a paste with olive oil, and put it into a crucible not lined. On heating it as in a common assay, a portion of red copper and some grains were obtained. Some matts were distinguishable, but the greater part of the substance covering the copper had precisely the appearance of the scoriæ of Chessy.

Why does not silex unite with oxide of iron in iron furnaces?

Perhaps it may be asked why in iron furnaces, where an ore mixed with quartz is frequently smelted, the same combination between the silex and iron is not formed? For this several causes may be assigned, acting either separately or

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No. 1), are added to the roasted matts; and the scoriæ issuing from the furnace during this operation contain three fourths of their weight (analysis No. 2).

conjointly. In the first place no doubt the high temperature of these furnaces, and the long time the ore is kept in them, must be reckoned*; neither must we omit the presence of earths and metallic oxides, the action of which on the silex may counterbalance that of the oxide of iron. To confirm this opinion, Mr. Gueniveau took an artificial compound of silex and iron, added ¼ its weight of very pure white marble, made the mixture into a paste with olive oil, and placed it in a crucible lined with charcoal powder. A button of iron and a few grains were obtained; and the scoriæ were an opake glass, of a light green colour, that did not contain a fifth of the iron. An analysis of the scoriæ of the high furnace shows, that the separation of the iron is much more complete, when lime, alumine, and oxide of manganese are found in due proportion with the silex.

Owing to the high temperature, length of the operation, and presence of other substances.

Lime, alumine, and manganese, promote the separation of the silex.

The presence of other earths beside silex, which is advantageous in iron furnaces, because they facilitate the reduction of this metal, is for the same reason prejudicial in furnaces for smelting cupreous pyrites. In these furnaces opposite effects are endeavoured to be produced on the iron: the object being to reduce the oxide of the metal in the former; while in the latter it is to prevent this reduction, and at the same time to promote the reduction of the copper. The means employed by the metallurgist to effect these opposite purposes merit the attention of the man of science.

These earths, advantageous in one case, prejudicial in the other.

The scoriæ produced in the smelting works of copper pyrites resemble iron ores, not only in their aspect and magnetic properties, but in the facility with which they yield a very considerable quantity of iron when assayed. It may be reckoned, that the mean produce of the assay would be at least 40 per cent; a quantity superior to that of many bog iron ores, which are notwithstanding wrought with advantage.

The scoriæ more rich in iron than many iron ores.

To ascertain how far these scoriæ are capable of being wrought for iron in the high furnace, they were treated with 25 per cent of carbonate of lime, and a well united button was obtained, equivalent to two thirds of the iron it contained.

Examined with a view to working them.

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* This is an opinion long held by Mr. Hassenfratz.

May be smelted with profit.

Mr. Gueniveau thinks therefore, that they may be smelted like the bog iron ores with an argillaceous carbonate of lime; and though the iron produced might not be of prime quality, it would answer for several purposes. This must appear an object of considerable importance to those, who have seen the vast quantities of scoriæ near the works. The only operations required are pounding and smelting; and the smelting would not be very expensive, if coke could be employed, as is done at Chessy for the copper, where the whole process is conducted in a very intelligent and economical manner.

Silex acts chemically in this case:

which elucidates many things that occur in metallurgic works.

The observations and experiments here given not only lead us to consider silex, or quartz, as a metallurgic agent, capable of separating iron in a state of combination from copper, which it allows to melt alone or mixed with sulphur, when assisted by the high temperature of the furnace; but serves to explain many passages in treatises on metallurgy. These works offer nothing precise respecting the manner, in which substances mixed or combined in ores act on one another, and allow the separation of the metal, which we seek to obtain. The same facts teach us why various kinds of scoriæ are mixed with the ores or matts under different circumstances: some being employed as fluxes of the gangues; others only to give fluidity to the whole mass, and produce a kind of solution, which facilitates their reciprocal action; and others act as a medium to separate the iron, when they are not already saturated with it. At Chessy the scoriæ of the ore perform this office in the smelting of the matts, as has been said. The general opinion of metallurgists therefore, who consider the addition of earthy substances to ores as serving merely to form fusible compounds with their gangues, without paying any attention to the action they exert on metallic oxides, requires modification in many cases.

VIII.

VIII.

Description of the Valve Siphon of the late Mr. Ami Argand, *Inventor of the Lamps with a Double Current of Air**.

Description of a valve siphon.

THIS improvement, though simple, is ingenious, and particularly adapted to large siphons, that require to be removed from one vessel to another. A valve, as E, or H, pl. II, fig. 1, is applied to the foot of the shorter or ascending leg of a siphon A B, B C, at the other foot of which a stop cock F is placed. The cock being open, and the foot E immersed in any liquid in a vessel I K, by moving the leg E perpendicularly downward and upward, the liquid will gradually ascend through the valve E, till it runs out at the point L. The pressure of the air on the surface I will then be sufficient, to force the liquid through the valve E, as long as this remains beneath it; and thus it will continue to act as a common siphon, and the vessel will be emptied, unless supplied from some reservoir, as N.

Conveniently removable.

As soon as the siphon is filled, and begins to discharge the liquid at L; or at any period while it continues full; if the cock F be turned so as to stop it, it may be very safely and conveniently removed to any other vessel; as the cock will prevent the liquid from running out at one end, and the valve at the other: and the moment the extremity E is immersed in the liquid in another vessel, and the stop cock F turned, it will act again as before.

Mode of filling without disturbing the sediment of liquids.

The siphon may be filled in this way in a clear liquid, and then removed into a vessel of the same kind of liquid, that has a sediment at bottom, which would be disturbed by moving it up and down. This however may not always be convenient: Mr. Argand therefore makes an aperture with a short perpendicular tube O in the horizontal branch B B, through which, by means of a funnel, D, the siphon may be filled, while the cock F is shut; so that it may be inserted into the liquid, and made to act without disturbing it. When

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* Sonnini's Bibliothèque Physico-économique, Nov. 1806, p. 117.

the siphon is thus filled, or when the funnel D is not required, the aperture at O is closed by the stopple G.

Made to take to pieces.

For the convenience of carrying the siphon, as well as for packing it up, or cleaning it, the horizontal and perpendicular branches are made to take asunder at the joints M M. The nozzle L is likewise made to take off, as it is frequently more convenient for the fluid to be drawn off perpendicularly.

IX.

Description of a new Instrument for proving the Strength of Gunpowder, by Mr. Regnier, *Keeper of the central Dépôt of Artillery*[*].

Defects of the old spring instrument.

THE old toothed wheel instrument for trying powder is obviously defective, as the friction of the spring blunts and rounds the edges of the teeth; the spring, being necessarily strong, and always in a state of tension, loses in time its strength and elasticity; and the wheel, which is too much confined in its movements, is exposed to irregular degrees of friction, which likewise vary according as the instrument is kept clean, or suffered to grow rusty. To obviate these defects Mr. Reguier has recourse to a weight; and the following is a description of the mode in which he applies it.

A weight here employed.

Decription of the instrument.

A, plate II, fig. 2, is a stock of walnut wood, about 8 inches long, with a plate of copper let into it, to support the mechanism.

B, a small copper mortar, to hold the powder to be tried, with a pan for firing it.

C, a brass wheel, grooved like the wheel of a pulley, with thirty ratchet teeth on its circumference. To this wheel is fixed a projecting piece, or obturator, accurately covering the mouth of the small mortar, B.

D, a forked support, in which the wheel C plays freely on its axis.

E, a spring pallet, acting as a click, to stop the wheel at the point to which the strength of the powder carries it.

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[*] Sonnini's Bibliothèque, March, 1807, p. 415.

F, a

F, a small copper weight suspended by a string, which is fastened by a knot at the hole G in the thickness of the wheel. The string passes through an aperture in the wooden stock and the plate of copper, sufficiently large to occasion no friction.

H, a loop at the end of the handle, to hang up the instrument when not in use.

Method of using it.

To use it the small mortar B is filled with powder, and the end of the finger passed over its mouth, that no grains may remain between the mortar and its obturator. A little priming is to be put into the pan, and the instrument is to be held in a horizontal position in the left hand, while it is fired. A red hot iron skewer is the most convenient for setting fire to the priming.

Its mode of action.

The elastic fluids extricated on firing the gunpowder impel the obturator upward, thus turning round the wheel, and raising the weight, that resists its action. The strength of this action is indicated by the extremity of the pallet E, that marks the degree to which the weight is raised.

The degree marked *good* denotes a good powder for shooting; but the farther it goes beyond this, the better the powder.

Adapted to the purposes of the sportsman only.

The same powder will not always produce an equal effect on the same instrument; to prove it properly therefore several trials should be made, and the mean of them taken. To prove the large grained powder used for military purposes other modes must be adopted, this instrument being suited only to the sportsman.

X.

Mode of making Phosphoric Ether by means of a peculiar Apparatus; by Mr. P. F. G. BOULLAY, *Apothecary, at Paris. Read before the First Class of the Institute, March the 23d,* 1807*.

Unsuccessful attempts to make phosphoric ether.

BOTH Scheele and Lavoisier had attempted to transform alcohol into ether by means of the phosphoric acid, without

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* Annales de Chimie, vol. lxii, p. 192, May, 1807.

without success, when Boudet the younger published a paper on this subject in the XLth vol. of the Annales de Chimie. The phenomena he described announce a real action between the acid and alcohol, and display various circumstances, that usually accompany the formation of ether. However this chemist confessed, that the product he obtained possessed little volatility; that it was entirely soluble in water; and that, though it had a peculiar smell, it did not exhibit the characters of real ether.

Owing to the difficulty of bringing the acid & alcohol into intimate contact.

Convinced by various trials, that the want of action of the phosphoric acid, when concentrated or even glacial, on alcohol depended particularly on the difficulty of uniting these two substances, and multiplying and prolonging the contact of their mutual particles, I resumed the attempt; and the hope I conceived of obtaining a more satisfactory result was realized by the following process.

Apparatus for this purpose.

To a tubulated retort, placed on a sand bath, I fitted a receiver, likewise tubulated, which communicated by a Welter's tube with a bottle full of limewater. From this bottle a second tube proceeded to a pneumatic trough, and there opened under an inverted jar.

Into the retort I introduced 500 gram. [1603 grs. troy] of pure phosphoric acid, arising from the combustion of phosphorus by nitric acid, vitrified, redissolved, and evaporated to the consistence of honey.

On the tubulure of the retort I placed a glass vessel, that may be called a *reservoir*, of an oblong shape, and open at both extremities, each of which might be shut close by means of a cork. From the lower end a tube descended to the bottom of the retort, being thus immersed in the phosphoric acid. The upper end, in which was a funnel, that might be made to communicate with the reservoir, or not, at pleasure, had a small aperture with a ground glass stopple intended to give vent to the air, when displaced by pouring in a liquid. See plate II, fig. 3; and for its horizontal section fig. 4.

Process.

The apparatus being thus disposed and carefully luted, and the first receiver being cooled by a mixture of pounded ice and common salt, a fire was kindled under the retort, and the heat gradually increased, so as to heat the acid to

95° of Reaumur [246° F.]. An equal weight of alcohol at 40° was then introduced into the reservoir, and by means of the lower cock allowed to fall drop by drop into the hot and fluid phosphoric acid. The mixture took place with violence and ebullition; it assumed a black colour, and copious streaks immediately appeared on the upper part and neck of the retort.

Products.

The fire being kept up, and the distillation continued to dryness, there passed into the receiver,

Alcohol slightly etherised.

1, A hundred and twenty gram. [3oz. 6dr. 53gr.] of alcohol weakly etherised.

A more ethereal fluid.

2, Two hundred and sixty gram. [8oz. 2dr. 55gr.] of a white, light fluid, of a brisk smell, and much more ethereal than the former.

Water saturated with ether, and oil of wine.

3, Sixty gram. [1oz. 7dr. 26gr.] of water saturated with ether, on which swam about 4 gram. [62grs.] of a lemon-coloured fluid, with an empyreumatic smell, very similar to that which comes after sulphuric ether, and which is commonly known by the name of sweet oil of wine.

A fetid liquor with acetate of potash.

4, Another fluid of an insupportably fetid smell, reddening tincture of litmus, and combining with carbonate of potash with effervescence. This combination, being evaporated to dryness, was a deliquescent salt, foliated, and perfectly similar to acetate of potash.

A little carbonic acid at the end.

The limewater was rendered turbid, but not till toward the end of the distillation.

Ether in the state of gas.

Beside the air in the vessels, a gas was collected of a sweet and penetrating smell, burning with a white flame, and when burned depositing on the sides of the vessel a very plentiful coat of carbonaceous matter. It was a little ether that escaped condensation, that passed at the same time as the most ethereal liquid product, and a little before the white vapours, that announced the presence of the oil.

Residuum.

What remained in the retort was a blackish, glassy matter, consisting of phosphoric acid, and a little charcoal.

Rectified.

The first two products mixed together, and rectified over dried muriate of lime, at a heat of about 50° [144°], afforded near 60 gram. [1oz. 7dr. 27gr.] of a liquid, which in smell and taste had the greatest resemblance to the purest sulphuric ether. It marked like it 60° on Baumé's areometer, the thermo-

Pure ether.

thermometer being at 10° [$54\frac{1}{2}$°]; dissolved in eight or ten parts of cold water; evaporated quickly; boiled at 30° [$99\frac{1}{2}$°]; dissolved resins and phosphorus; burned with a whitish flame, leaving a carbonaceous residuum, and without any trace of acid appearing from its combustion on the suface of water.

Alcohol slightly etherised.

The other product of the rectification was alcohol, slightly etherised, This alcohol, passed again in the same manner through the phosphoric acid employed in the experiment, gave rise to the formation of a fresh quantity of ether in every respect resembling the first.

More ether.

From these facts, and on examination of the products submitted to the inspection of the class, it appears to me,

Phosphoric ether

1st, That phosphoric acid is capable of transforming alcohol into a perfect ether, by means of the apparatus I employed, and the precautions I have mentioned:

most resembling sulphuric.

2dly, That, of all the different ethers known, the ether resulting from the action of phosphoric acid on alcohol has the greatest analogy to sulphuric ether, with respect to its properties, and the phenomena observed in preparing it.

XI.

Remarks on the Decomposition of Acetate of Barytes by Means of Soda; by Mr. D'ARCET*.

Acetate of soda said to be decomposed by barytes.

IN No. 180 of the Annales de Chimie, p. 286, Mr. Perpérès, speaking of the formation of acetous acid in cases of indigestion, says, that, to ascertain the presence of this acid, he saturated it with pure soda; afterward decomposed the acetate of soda by means of barytes; and having thus set the soda free, dissolved it in alcohol, which, uniting with the water of the solution, precipitated the acetate of barytes, that had been formed. The result of this experiment is certainly inaccurate, as the following details will show.

Solution of barytes added to acetate of soda, crystals fall down.

Take a hot saturated solution of barytes, pour it into acetate of soda, and immediately an infinite number of little, shining, and iridescent laminæ will fall down. If these be

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* Annales de Chimie, Vol. LXI, p. 248, March 1807.

be separated from the liquid after it is completely cooled, washed in the smallest possible quantity of water, and dried quickly by pressing them between several folds of blotting paper, they will be found to be crystals of pure barytes, without any mixture of acetate. Of this I satisfied myself in the following way.

These pure barytes.

1. I exposed part of these crystals to the air. After some days I washed the carbonate thus obtained with pure water, which then took up nothing, that sulphuric acid, or alkaline carbonates or sulphates would throw down. The whole of the crystals therefore had been converted into carbonate; which would not have been the case, had they contained any acetate of barytes.

Converted into carbonate by exposure to the air.

2. I dissolved two or three grammes of the same crystals in distilled water. The solution restored the blue colour of reddened litmus paper: consequently it contained an excess of alkali.

Were alkaline.

I added a few drops of sulphuric acid to this solution, and a precipitate of sulphate of barytes was formed. I tested the liquid again with litmus paper, and I still found an excess of alkali. I then gradually added more sulphuric acid, till there was a slight excess of acid in the liquor; filtered it, and found it no longer contained any barytes, but a little free sulphuric acid. This would not have been the case, had the crystals contained any acetate of barytes; for, on this supposition, the moment when the excess of acid began to be sensible by the test paper, only a small portion of the acetate would have been decomposed, and acetous acid would have been set free. The filtered liquor therefore ought to have contained a slight excess of acetous acid, and the undecomposed acetate of barytes: but this was contradicted by the experiment.

Sulphuric acid formed with them sulphate of barytes,

and no acetous acid appeared.

3. The mother water of the crystals employed in the preceding experiments ought to contain only that small quantity of pure barytes, which it could retain in solution when cold, in addition to the whole of the acetate of soda employed. This too the analysis of the mother water demonstrates, if alcohol be poured into it, as Mr. Perpérès directs. The shining scales that fall down are nothing but crystals of barytes: and examined in the way I have mentioned above,

The mother water contained only pure barytes and acetate of soda.

above, they produced nothing but very pure carbonate of barytes, and not an atom of acetate. If the mother water be farther examined with sulphuric acid, or alkaline carbonates, it will immediately appear, that it contains but little barytes and a great deal of acetous acid; which becomes still more sensible, if it be evaporated to dryness, and the residuum be redissolved in distilled water: for this solution does not contain an atom of barytes, but merely acetate of soda, the little barytes that was present being reduced to the state of a carbonate during the evaporation.

Barytes does not decompose acetate of soda.

Hence it follows, that barytes does not decompose acetate of soda; and on the contrary, that, if we try the opposite experiment, it will succeed. In fact the whole of the acetate of barytes may be decomposed, by adding to it a sufficient quantity of pure soda to saturate all the acetous acid. The barytes contained in the phial accompanying my letter was prepared in this way.

Potash and soda have stronger affinities than barytes for all the acids except the sulphuric and carbonic.

My object is not to invalidate the conclusion of Mr. Perpérès, which appears to me just, and consistent with what was already known. I only criticise one of the proofs he has adduced, and avail myself of this opportunity, to remind the public, that in the year 12 a paper on the affinities of barytes, by Mr. Anfrye and myself, was inserted in the Annales de Chimie; where we proved, that in the classification of alkalis barytes ought to be placed before potash and soda only with respect to the sulphuric and carbonic acids, the affinities of potash and soda being superior in every other case. How is it, that, notwithstanding the facts so positively announced in that paper, different authors have retained the ancient order of affinity assigned to barytes? It appears to me, that, with regard to experiments, either the results of such as are made public should be adopted; or they should be refuted, by repeating them, and proving their erroneousness.

I shall conclude this note by citing in confirmation one of the processes, the goodness of which has been proved by our labours on barytes in the large way. It follows naturally from the facts mentioned above.

This theory applied to the process of ob-

The decomposition of muriate, nitrate, and acetate of barytes by potash or soda is so complete and easy, that it is taining pure barytes in the large way with success.

is unquestionably the most simple method of procuring in a laboratory the barytes that may be wanted. For this purpose a hundred parts of sulphate of barytes accurately mixed with twenty parts of charcoal powder are to be calcined by a strong heat in close vessels. After being exposed to a high heat for an hour, the crucible is to be suffered to cool; the residuum separated and diluted in water; and a sufficient quantity of nitric, muriatic, or acetous acid added. The mixture is to be heated gently, when it will give out a large quantity of sulphuretted hidrogen and carbonic acid, which must be guarded against with care. When the effervescence ceases, and test paper indicates a slight excess of acid in the liquor, it is to be filtered and evaporated, to decompose the sulphuretted hidrogen, and precipitate the sulphur, that was retained in solution*. The residuum is to be redissolved in the least water possible, and a saturated solution of caustic potash is to be added. At the instant of mixture a large quantity of crystals of barytes falls down. The whole being left at rest in as low a temperature as possible for an hour or two, the mother water is to be poured off; the crystals are to be washed with a little distilled water, and then dried by pressing them between folds of blotting paper; and lastly they are to be dissolved in as much boiling water as is necessary. This solution, being filtered, will let fall when cold the barytes, which is much more pure, and costs less, than when obtained from the decomposition of nitrate of barytes by heat alone.

Muriatic or acetous acid to be preferred.

It is to be observed, that the muriatic or acetous acid is preferable to the nitric, because each forms a more soluble salt than the nitric, and the washing is more easy; and because in making the solution the nitric acid is partly decomposed, and oxigenizes a portion of the sulphuret of barytes, so that some of the acid is lost, and some of the barytes absorbed by the sulphuric acid formed.

The potash must be free from sulphate.

The caustic potash used in this process must be prepared from carbonate perfectly free from sulphate.

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* The same object may be attained more readily by pouring into the liquor a few drops of solution of nitrate of copper or lead, letting the metallic sulphuret subside, filtering afresh, &c.

Observation on the preceding article by one of the authors of the Annales de Chemie.

The result examined by Guyton.

Agreeably to the request of Mr. d'Arcet, in addressing to me this note, I have examined the liquid in the phial accompanying it. It was more than half full of small, white, crystalline scales. The liquor acted powerfully on paper tinged by mallow flowers, changing it green. On dropping into it sulphuric acid a little in excess, a copious precipitate of sulphate of barytes was formed, without the least smell of acetic acid. After having filtered the liquor from this precipitate, I evaporated it by a gentle heat, in a platina capsule; but it left no trace of any neutral salt. No doubt can remain therefore, but the acetate of barytes is radically decomposed by soda.

L. B. GUYTON.

SCIENTIFIC NEWS.

Adjudication of Prizes, with a proposed new Question, by the Imperial Academy of Sciences at St. Petersburgh.

Question proposed by the academy on light.

THE Imperial Academy of Sciences had proposed in their last public notice the prize of five hundred rubles [£112 10*s.*], to be given to any professor of natural philosophy, who would establish, and communicate to the academy, a series of "new and instructive experiments on light "considered as matter; also, on the properties, which may "in part be attributed to it; on the affinities, which it may "appear to have either on organized or unorganized bodies; "and upon the modifications and phenomena of these substances by their combinations with the matter of light."—The academy had declared at the same time, in order not to confine the learned who might have been pursuing similar inquiries, that they contented themselves with stating the subject generally, leaving them at liberty to consider the question in any point of view, that might appear best calculated to elucidate a question so difficult.

The academy has received within the prescribed time six tracts on the question, each having a note sealed and motto.

—viz.

—viz. No. 1. In the Russian language with the motto: "*A* "*philosopher who has learned to doubt, knows more than all* "*the learned, &c.*" No. 2. In the Russian language: "*Time is the earliest thing in nature, &c.*" No. 3. In Latin: "*Est-ne color proprius verum, lucisne repulsus eludunt* "*aciem?*" No. 4. In French: "*Nox abiit, nec tamen orta* "*dies!*" No. 5. In German: "*Ut noscas splendore novo* "*res semper egere, et primum factum, &c.*" No. 6. In German: "*La physique ne sera véritablement une science,* "*que lorsque tous les effets naturels se deduiront clairement* "*d'un seul et meme principe evidemment démontré.*"

The first three tracts, beside the common fault of wanting new experiments, a complete and instructive series of which was required by the academical notice, contained hypotheses and propositions, either well known, erroneous, or ill expressed, and advanced without demonstration. For these reasons the academy did not think they had any claim to the prize.

The tract No. 4 is not without merit. The author enters upon several interesting questions concerning the nature of light, in a manner that readily convinces us he is no stranger to the subject. But the deficiency of connexion and systematic arrangement, which is perceived in it, and above all, the absolute want of new experiments which might lead to new results, or serve as a support to a number of hypotheses advanced by the author, and destitute of every species of demonstration, would not permit the academy to adjudge the prize to this memoir, even had there been none of greater merit.

As to the last pieces, No. 5 and No. 6, the academy has found them worthy of their particular attention, from the report of the committee appointed to decide on the different performances. These essays are agreeable to the principal condition stated in the notice, inasmuch as they contain a great number of new experiments on the effects and properties of light, and a judicious application of those, which, though already known, were repeated, whenever they appeared doubtful to the authors. Both pieces are executed upon a plan wisely conceived, expressed with clearness, and arranged in sufficiently systematic order. On the other hand,

hand, in each were found some incoherent and contradictory conclusions; as also propositions advanced without sufficient proof; beside some errours, and obscure passages. But as these imperfections were overbalanced by researches of great merit, the academy, without acceding to every assertion of the authors, have nevertheless thought it their duty, to divide the prize between them, thinking them worthy of encouragement and honourable reward.

On opening the two sealed notes, Doctor Henry Frederick Link, professor of natural philosophy at the university of Rostock, was found to be the author of No. 5; and Mr. Placidus Heinrich, professor of natural philosophy and mathematics at the Abbey of St. Emereau, at Ratisbon, the author of No. 6. The notes of the remaining tracts were burned, without being opened.

Question on the resistance of fluids.

When the academy had made public the notice, in which the marine department proposed a prize on the question concerning the resistance of fluids, they had engaged to publish also the judgment, which that department, in conjunction with the academy, should make on the memoirs presented. Conformably to this engagement, the academy announce the receipt of three memoirs:—viz. No. 1. with the motto: "*Sit modus lasso maris et viarum militiaeque.*" No. 2. "*Praesta natura voce doceri, quam ingenio suo sa-*" *pere.* No. 3. "*England and France agree.*" The last of these arrived after the term.

Neither of them was found to satisfy all the conditions of the problem: but, as the tract No. 2 exhibits a new theory; which, though not established upon grounds sufficiently solid, or applied to naval architecture in the manner the notice required, is preferable, in some measure, to the theories of Romé and don George Juan, agrees better with experiments than the common theory, and deserves therefore to be noticed advantageously; the marine department, to recompense the author for his trouble and laudable efforts, have decreed to him the prize of 100 Dutch ducats [£46 5*s*.], and the academy have given their sanction to the decision. The opening of the sealed note discovered the author to be Mr. Zachary Nordmark, professor of mathematics in the university of Upsal.

In

In publishing these judgments and distribution of prizes for the year 1806, the academy proposes the following question for the present year 1807.

Question for 1807.

Chemistry teaches us the means of discovering the noxious quality of mineral bodies, whereas it is only empirically, that we have learned to distinguish poisonous plants from those that are not so. Even the characteristics, by which we think ourselves enabled to determine on the presence or absence of poison in vegetables, are not always sufficiently certain and incontestible. The livid colour, for example, which has rendered many plants suspected, is a fallacious sign. The burdock (arctium lappa) looks dull, and is of a pale colour, yet it is a wholesome plant; on the contrary, the laurel (daphne) is remarkable for the beauty of its flowers and leaves, yet this is poisonous. The families of ranunculus and anemone are as beautiful as they are numerous; they are however for the greater part noxious. The same may be said of the disagreeable smell of plants, which is taken for a diagnostic of the poisonous quality, and which sign is equally uncertain with the preceding. The smell of the laurel is very agreeable, while the stinking orach (chenopodium vulvaria), an innocent and even salutary plant, is of a very disagreeable smell. The smell of coriander is disagreeable to many persons, yet it is of a very salutary nature. The umbelliferous plants, that grow in wet and marshy situations, have the reputation of being poisonous; notwithstanding this, the water parsnep *(sium)* and all its species, the *sison inundatum et sulsum*, the *phellandrium aquaticum*, the *angelica sylvestris*, the *ægopodium podagraria*, plants which thrive in marshes, contain no poison. It is plain, therefore, that neither the livid colour, disagreeable smell, nor growth in marshy places, can furnish us with certain and indisputable signs of the presence of poison in plants.

The pretended repugnance of animals to pernicious plants is evidently as little infallible. The division of plants, made by botanists, into classes, orders, and families, according to their nature, is not more effectual in discriminating those that are venomous. To be convinced of this we have only to observe, that among the species of the nightshade, a genus so much suspected, are found the potato *(solanum*

(solanum tuberosum), and the winter cherry *(s. pseudocapsicum)*, which possesses the virtues of a stimulant, and of destroying the pernicious principle in narcotic plants.

In consequence of this want of an exterior and natural certain sign, by which poisonous plants might be immediately detected, it would be desirable, to find out some easy method of examining them, such for instance as a kind of *eudiometer*, or any thing that might produce changes in them, which, like the black colour assumed by mushrooms when they are boiling, might indicate their noxious qualities; though it must be confessed, the criterion of poisonous mushrooms is not yet sufficiently established.

"*An easy method is therefore required, by which any individual, not having the least knowledge of botany, may detect poisonous plants, in a short time, at a small expense, and in a manner perfectly decisive.*"

The prize is one hundred Dutch ducats [£46 5*s.*] and the precise time, after which no memoir can be admitted to the competition, is the 1st of July, 1808.

The academy invites the learned of all nations, without excluding its honorary members and correspondents, to investigate this subject.

The learned who compete for the prize are not to put their names to their works, but merely a sentence, or motto; and send with them sealed notes, which must have the same motto on the outside, and the author's name, quality, and place of residence, within. The note of the piece to which the prize is adjudged will be opened, and the rest shall be burned unopened.

The tracts should be written in legible characters, either in Russian, French, English, German, or Latin, and must be addressed to the permanent secretary of the academy, who will deliver to the person appointed by the author a receipt, marked with the same motto and number as shall be inscribed on the piece. The successful memoir is to be the property of the academy, without whose formal permission the author shall not print it.

The rest of the tracts may be received back from the secretary, who will deliver them at St. Petersburgh to any person commissioned by the author to apply for them.

Discovery of a new Planet, by Mr. OLBERS.

New planet discovered by Olbers.

Mr. OLBERS has written to Mr. le Français Lalande, that he has recently discovered a new planet. The following are such elements of its orbit, as he has been able to determine.

Elements of its orbit.

The 29th of March, at 21 minutes after 8 mean time, its ascension was 184° 8′; its north declination 11° 47′.

On the 30th, at 12 h. 33′, mean time, its ascension was 183° 52′; its north declination 11° 54′.

Seen at Paris by the naked eye.

It has been seen at Paris, and was visible to the naked eye. Its size appears nearly that of a star of the fifth magnitude. Apparently it is about the same distance as the three lately discovered planets, Ceres, Pallas, and Juno.

Fluoric Acid in Teeth and Bones.

Fluoric acid in teeth & bones.

In our Journal, Vol. XIII, p. 214, is a letter with which we were favoured by Mr. Brande, to show, that the enamel of the teeth does not contain any fluoric acid, an Italian chemist having asserted, that they did. A letter from Mr. Gehlen to the editor of the Journal de Physique, dated March 16, informs us, however, that the fluoric acid exists both in the enamel and bony part of teeth, and in other bones. His words are:

" The very extensive and accurate experiments of Mr. Berzelius of Stockholm have proved, that the enamel and the bony part of the teeth of man and of the ox, as well as their bones themselves, contain fluoric acid. The following are the results of the analyses.

Component parts of the enamel of teeth,

" *Enamel of human teeth.*		*Enamel of the teeth of the ox.*	
Phosphate of lime	85·3		81
Fluate of lime	3·2		4
Carbonate of lime	8·0		7·1
Phosphate of magnesia	1·5		3
Soda, combustible animal matter, water	2	Natron	1·34
		Animal matter	3.56
	100		100

Osseous

of their bony part,

	" *Osseous part of human teeth.*	*Osseous part of the teeth of the ox.*
Phosphate of lime	61·95	57.46
Fluate of lime	2·10	5·69
Carbonate of lime	5·50	1·38
Phosphate of magnesia	1·05	2·07
Soda, with a little muriate of soda	1·40	2·40
Gelatine, veins, water	28·00	31·00
	100	100

and of bones.

	" *Dry fresh human bones.*	*Dry fresh ox bones.*
Gelatine	32·17	33·30
Veins belonging to their organization	1·13	
Phosphate of lime	51·04	55·45
Fluate of lime	2·00	2·90
Carbonate of lime	11·30	3·85
Phosphate of magnesia	1·16	2·05
Soda, with a small quantity of muriate	1·20	2·45
	100	100 "

We regret, that we have not the particulars of the analyses before us, that we might see the proofs of the fact, or be enabled to trace the causes of the fallacy.

Fluoric acid in urine.

Mr. Berzelius says too, in a letter to Mr. Vauquelin, that the precipitate obtained from urine by limewater, when washed and dried, being treated with sulphuric acid, gives out fluoric acid, which corrodes glass. But it requires a pretty considerable quantity of this precipitate, to give any very perceptible signs of it.

Muriatic acid and soda obtained by galvanism only when salt is present.

He adds, that the Swedish chemists have never been able to obtain muriatic acid and soda by means of the galvanic pile from water perfectly pure. That they find pure water a very bad conductor; but if the least particle of salt be present, the decomposition is more rapid, and its acid and alkali are set free.

Sulphur inflamed by oxide of lead.

Brown oxide of lead inflames sulphur by trituration with it.

DR. THOMSON'S paper on the oxides of lead, Journal, Vol. VIII, p. 280, having been translated into French, and inserted in the Annales de Chimie, the passage in which he says he did not succeed, in triturating sulphur with the brown oxide, p. 288, is thus commented upon.

" Nothing however is more certain, than that Mr. Vauquelin has inflamed sulphur by triturating it with brown oxide of lead, as he formerly mentioned. He lately repeated this experiment, in one of his lectures, before upwards of fifty persons, among whom was prof. Proust of Madrid. The only precautions the experiment requires are, to boil the nitric acid a long time on the brown oxide, that no minium may remain among it; to wash it afterward with a great deal of boiling water, so as to take up all the nitrate of lead; and lastly, to dry it well, and to triturate it with flowers of sulphur equally well dried.

Necessary precautions.

" On observing these essential conditions, there can be no doubt, but Dr. Thomson will succeed in inflaming the sulphur. The supposition he makes, to account for the phenomenon, is inadmissible, for Mr. Vauquelin never employed oxigenized muriatic acid, to prepare the brown oxide of lead.

Yttria and Cerium.

Yttria oxigenizes muriatic acid.

THE chemists at Upsal at first imagined, that cerium was nothing but a mixture of barytes, yttria, and magnesia. Mr. Eckeberg, desirous of comparing them, has found that yttria, after having been a long time exposed to the action of fire, gives out oxigenized muriatic acid, when dissolved in the common muriatic acid. Is yttria, asks Mr. Berzelius, one of the new metals, uranium, titanium, or cerium, with its nature as it were changed ?

Alloy of cerium and iron.

Mr. Gahn has formed an alloy of cerium with iron, partly in a grayish powder.

New Process proposed by Mr. ALLAIRE, *Administrator General of Forests, for scowering Wool.*

Wool cleansed by chalk.

THIS process consists in repeatedly immersing the wool in a warm chalk bath. The calcareous earth forms an animal soap with the greasy matter of the wool. By this method the wool is cleansed quickly, and without affecting its quality.

Argand's Lamps.

Argand's lamps, with blue glass chimneys.

MR. ARGAND has made different improvements in his lamps. The first consists in using blue glass chimneys, which render the light mild, like that of day: for the light traversing a medium similar to that of the atmospheric air, is modified in a similar manner. This is an important advantage to many artists, who find it necessary to work by artificial light, as it is well known, that several colours do not appear by it of their natural tints*.

The light enclosed in alabaster or Derbyshire spar.

Another mean of obtaining a very mild, pleasant, and as it were mysterious light, is to enclose the beak of the lamp in a vase of alabaster or Derbyshire spar, the bottom and neck of which are pierced to admit this beak, while the body of the lamp, that contains the oil, is concealed behind it. A room lighted in this manner has a very curious and agreeable appearance†.

Means of preventing the oil from overflowing.

Mr. Argand has likewise found the means of remedying the inconvenience to which suspended lamps are liable, that of suffering the oil to run over, either by agitation, or from rarefaction of the air in the reservoir by heat. Thus consists in leaving an opening in the lamp at the top, so that het temperature cannot influence it; and it will always remain at the same height.

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* Chimneys made of the common blue glass of our glass houses, and of the ordinary thickness, will not answer; as I found by experience many years ago. At least they diminish the light in so great a degree, that the consumption of oil to produce a given effect with them must be so much more than with common glass chimneys, as to render them too expensive for general use. W. N.

† This is evidently analogous, but I should suppose inferior, to Count Rumford's method of using ground glass. See Journal, Vol. XIV, p. 22.

He

He has likewise contrived vessels for keeping oil, so that when the oil is drawn off, its place is supplied by water at the bottom. Thus, the vessel being always full, the oil is not thickened by the action of the air.

Vessel for keeping oil without thickening.

A Course of Lectures on Natural Philosophy and the Mechanical Arts. By THOMAS YOUNG, *M. D., For. Sec. to the R. S., F. L. S., late Prof. of Nat. Phil. in the Royal Institution of Great Britain, &c.* 2 *vols.* 4*to.* 1570 *pages,* 58 *plates.*

Young's Natural Philosophy.

THIS valuable work has been for some time eagerly expected; but it has suffered no longer delay, than the copiousness of the subjects it embraces, the great variety of figures to be engraved, the large body of references, and the accuracy required in every part of it, rendered indispensably necessary. To give any adequate view of the multifarious objects it embraces, would much exceed our limits; we must be content therefore with noticing them briefly. When Dr. Y. undertook the office of professor of natural philosophy he very properly conceived, that the plan of the Institution required something more, than a mere compilation from elementary works; and therefore set himself to collect from original authors, to examine with attention, and to digest into one system every thing relating to the principles of the mechanical sciences, that could tend to the improvement of the arts subservient to the conveniences of life. In pursuing this course he has referred the fundamental doctrines of motion to simply mathematical axioms, more immediately than has been usual, and facilitated their application to practical purposes: very fully investigated the passive strength of materials of all kinds, and formed many new conclusions respecting it, of considerable importance to the engineer and architect: simplified, extended, and illustrated the theory and motions of waves, circulation of the blood, and propagation of sound: investigated the curvature of the images formed by lenses and mirrors: minutely examined the functions of the eye: copiously described and accurately represented the phenomena of coloured light, and pointed out some new cases of the production of colours: reduced

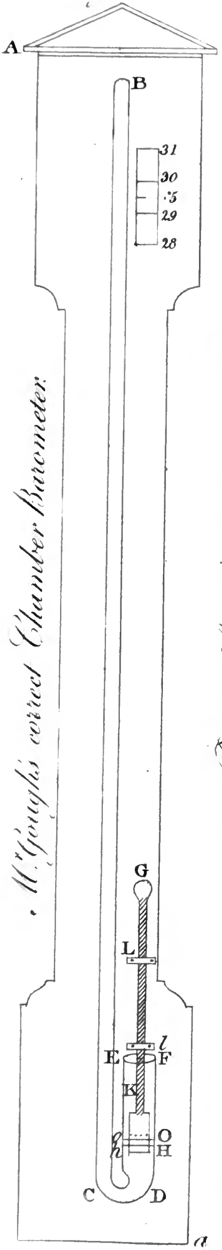
reduced the theory of the tides to a simple form: investigated the cohesion and capillary action of fluids, in which he has anticipated Laplace: made various comparative experiments on the elasticity of steam, evaporation, and the indications of hygrometers: and interspersed much practical information of various kinds, with new inventions and contrivances, that would take up too much room to enumerate.

The 2d vol. commences with the mathematical elements of natural philosophy, comprehending every proposition required for forming a complete series of demonstrations, leading to every case of importance that occurs in the science, except some of the more intricate calculations of astronomy. But the greater part of it is occupied by what many will consider as not the least important of the whole, a catalogue of works relating to natural philosophy and the arts, methodically subdivided as far as could be done with convenience and accuracy. In this catalogue, works of superior merit and accuracy are distinguished by asterisks, and those the author considered as erroneous or unimportant by obelisks: beside which he has pointed out those he has quoted; and, for the convenience of those who have access to the libraries of the Royal Institution, Royal Society, Sir J. Banks, and the British Museum, the books to be found in them. Extracts to, and remarks, for the most part brief, are frequently given, pointing out the leading objects, or affording hints for farther investigation. In fact, such a body of information, and such a copious list of references to original sources, condensed into a compass comparatively so small, will not easily be found among the modern productions of our press.

Lectures on Chemistry.

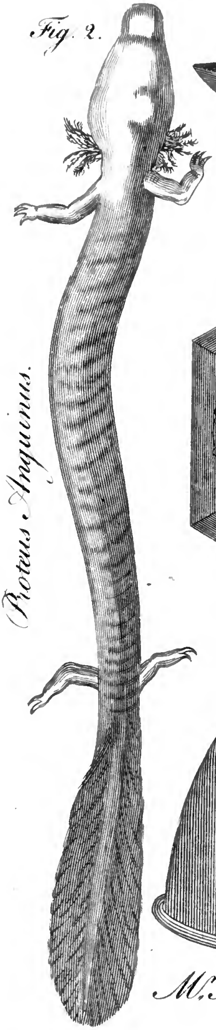
Mr. ACCUM'S lectures on operative chemistry and mineralogy, exhibiting a summary exposition of the processes of experimental chemistry, and general practical rules to be observed in the performance of chemical experiments; together with a summary view of analytical mineralogy, exemplifying the practical analysis of minerals; will commence October 1st.

Fig. 1.



Mr. Gough's correct Chamber Barometer.

Fig. 2.



Proteus Anguinus.

Fig. 5.

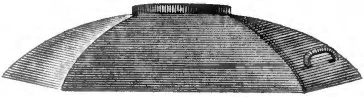


Fig. 4.

Sacro Catino.

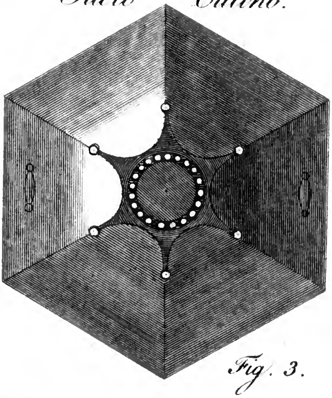
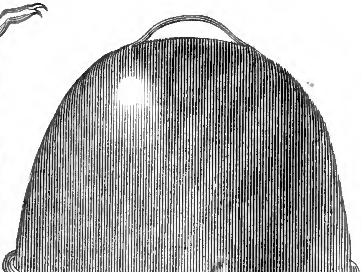


Fig. 3.



Mr. Maher's Blanching Pot.

Fig. 6.

Scale of Inches.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1807.

ARTICLE I.

Description of a correct Chamber Barometer. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR, *Middleshaw, Aug.* 17, 1807.

Correct barometers not easily procured in the country.

MANY intelligent persons in the country have been prevented from entering upon a course of meteorological observations, by the want of good barometers; which must be procured from London, not without considerable risk. I flatter myself, the present letter will remove this objection to the study in a great measure; for it describes an easy, and perhaps original method of correcting the imperfections of the instrument, and renders it fit for the purpose of every meteorologist, whose pursuits and observations are confined to his parlour or his study. When the correction in question first occurred to me, some time ago, I communicated it to Mr. Morris, of Kendal; who made a barometer on the principle explained to him, which has been some time in my possession, and has fully answered my expectations. The above gentleman also informs me, that any artist of moderate abilities may be instructed how to construct an instrument of the same kind, by a simple diagram properly explained:

the necessary instructions will accordingly be found in the annexed figure, and description accompanying it; which explains the cause, that renders common barometers imperfect measures of the changes in the weight of the atmosphere, and at the same time points out an easy and satisfactory apparatus for counteracting this source of errour.

A correct chamber barometer described.

Suppose A a (Pl. III, fig. 1) to be the frame or outline of the barometer, the ornamental part of which may be left to the discretion of the workman: moreover let 28,31 represent a scale of 3 or more inches properly divided and furnished with a nonius: let B C D E F be the compound tube or inverted siphon containing the mercury; it is hermetically sealed at B, and open at E F; the bore of the longer leg B C is $\frac{5}{16}$ of an inch in diameter, and that of the shorter D E F $\frac{12}{16}$, in the instrument from which the description is taken. When the siphon has been filled, it is to be fixed to the frame A a, in such a manner, that the two legs B C, and D E F may have a vertical position. This being done, a circle parallel to the horizon is to be cut on the external surface of the leg D E F at the distance of 31 inches from the top of the scale, or $29\frac{1}{2}$ inches from its middle, the place of which is denoted in the figure by the line O o. It will be perceived immediately that my barometer, as far as it has been described, differs in nothing from the common weather glass: the imperfection of which ought to be explained to the less scientific reader, before the method of correcting the instrument is described. In order to do this with the greater perspicuity, suppose, that when the tube is first filled, the surface of the mercury in the leg D E F coincides, as it ought to do, with the circle O o, and let the surface on the other leg B C fall exactly on some division of the scale, for instance 29·5: the weather glass will in this case give the true weight of the atmosphere; because the length of the column of mercury in B C is exactly 29·5 inches. But an instrument thus constructed will give the weight of the atmosphere falsely in all other instances: for let the length of the column in B C increase in consequence of an increasing pressure in the atmosphere, it is evident, that the surface of the mercury in the leg D E F will descend below the circle O o; because the augmentation in

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The cause of inaccuracy in the common weather glass.

the opposite column must be supplied by the branch D E F; let it then descend to the station H h. Now from the construction of the weather glass here described, O o is the only base from which we can measure the length of the mercury contained at any time in B C accurately by the scale; consequently the part of the column situate in H h and O o will be neglected in the observation; and the height of the barometer will appear to be less than it really is by the space H O; that is, the top of the column will not ascend so far as it ought to do above the middle point of the scale. On the contrary, when the mercury descends in the leg B C it will rise in the leg D E F, in consequence of a quantity of this fluid coming into it from the opposite branch. As oft then as the weather glass denotes a height less than 29·5 inches, the surface of the mercury in the leg D E F will rise above O o, the true base of the scale; consequently when the column comes to be measured by the index, it will appear longer than it really is by the height of the mercury's surface in the leg D E F above the base O o; that is, the upper extremity of the column in the branch B C will fall below the point 29·5, but not so far as it ought to do. Thus it has been made evident, that the motion of the mercury in the shorter leg diminishes the range of the barometer when it comes to be estimated by the scale; which circumstance points out the necessity of a correction, and perhaps the easiest method of doing it consists in bringing the surface of the mercury in the leg D E F to a coincidence with the base O o, before setting the index attached to the scale.

The method of correcting the common weather glass by a piston.

My apparatus for this purpose is nothing more than an ivory piston K of a cylindrical figure, about 1$\frac{2}{8}$ inch in height and $\frac{1}{2}$ an inch in diameter. Its lower extremity is a little convex and immersed in the mercury; it is also easily moved vertically by means of the handle or stem G L, which passes through a cap covering the mouth of the tube E F, and having a hole, l, in its centre, wide enough to receive the handle. This part of the piston is also attached to the frame A a by a loop or socket of brass, L, in which it would move vertically with perfect freedom, were it not for the pressure of a bent spring which is situate between L and l, and acts upon the handle G l, with a force, that keeps the piston in

any

Use of the piston.

any position assigned to it by the operator. The use of this apparatus is almost too obvious to stand in need of an explanation; its office is to place the surface of the mercury in the leg D E F on a level with O o, when an observation is to be taken with the instrument. This is done by placing the finger upon G, and pressing the piston downwards, when the barometer is rising, which operation must be continued until the quicksilver coincides with O o. On the contrary, when the mercury falls in the leg B C, the piston must be drawn up by means of the handle G L, so as to produce a coincidence in the opposite branch of the tube, similar to that just now mentioned. One circumstance must be attended to in the construction of the instrument; for it is necessary that the middle of the piston K, which is marked with a dotted line in the figure, should coincide with O o, as oft as the barometer stands at 29·5 inches. The method of providing for this coincidence will be easily pointed out by an example: suppose, when the tube is filled, that the middle of the piston and the surface of the mercury in the leg D E F coincide with O o, while the height of the barometer is 29·00 or ½ an inch less than 29·5; in this case, raise the piston until the mercury in B C stands at 28·5, or as much below 29·0 as 29·0 is below 29·5: this being done, pour mercury into the leg D E F, so as to make the opposite column rise again to 29·0, and the required coincidence will be provided for. On the other hand, suppose the instrument to be adjusted as above described, and the height of the barometer to be 30·0, or ½ an inch above 29·5: in this case push the piston downwards, until the column in B C stands at 30·5, or as much above 30·0 as 30·0 is above 29·5: this being done, take a quantity of mercury out of the leg D E F just sufficient to make the opposite column fall again to 30·0, and you will have secured the coincidence required.

A necessary precaution in the construction of the instrument.

Remarks respecting the dimensions of the piston.

I have observed in a preceding part of this letter, that the piston of my barometer is 1⅜ inch in length, and ½ an inch in diameter; but the reader is not to imagine, that these dimensions are fixed by necessity, for they may be varied at pleasure; and the following rule will determine the one, when the other is given. 1st. When the diameter of the piston

piston is given, multiply the length of the scale by the square of the internal diameter of the leg B C, and divide the product by the square of the given diameter, and the quotient will be the length of the piston: 2d. When the length is given, divide the product found above, by the given length, and the square root of the quotient will be the diameter of the piston. In adjusting these dimensions, we have two circumstances of some importance to attend to; the quantity of mercury requisite to charge the tube increases with the length of the piston, which suggests a consideration of an economical nature; but if the diameter of the piston be too much augmented, to avoid expense, the free motion of the barometer will be considerably impaired; the artist must therefore use his own judgment in giving those dimensions to the piston, which will be the most convenient for the tube he is going to fit up.

I remain, &c.

JOHN GOUGH.

II.

Observations on the Phytolacca, or American Pokeweed; by Mr. H. BRACONNOT, *Member of the Academy of Sciences, &c. at Nancy*[*].

The properties of many plants remain to be examined.

MAN, who lays all nature under contribution, to increase his enjoyments, has availed himself of a great number of vegetable productions; but, notwithstanding his extensive researches, he is yet far from being acquainted with the properties that characterise the majority of plants. The phytolacca, which has been greatly neglected, may furnish an instance of this.

SECT. I.

Incineration of the phytolacca.

Phytolacca.

THIS plant, which is acrid, has a very thick, fleshy root, as big as a man's leg. Its stalks are as big as a large walking stick, six or seven feet high, and purple.

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[*] Abridged from the Annales de Chimie, Vol. LXXII, p. 71, April 1807.

Stalk yields potash.

If a piece of the stalk be exposed to the flame of a candle, it is reduced to a reticular texture, exhibiting, when viewed by a lens, a series of longitudinal filaments connected by cross meshes. If this be again exposed to the flame, it swells up, melts, and the result is potash.

14 oz. troy incinerated.

Four woody stalks of this plant weighed when dry 440 gram. [14 oz. troy]. These I burned in an iron crucible; and when it began to grow red hot, the matter assumed a pasty consistence, and ended by fusing, accompanied with a swelling up occasioned by the evolution of hidrogen gas, which burnt with detonation as it burst from the melted matter. When the crucible was cold, it contained a hard brown substance, that had a caustic taste.

Coal lixiviated.

432 grs. of salt.

A little silex.

As it was impossible to get this saline residuum completely out of the crucible, I boiled water in it, and thus easily separated it, great part of it being dissolved. The liquor filtered and evaporated to dryness left 28 gram. [432 gr.] of a saline substance, which I saturated with pure nitric acid. The liquor deposited a blue precipitate, which weighed 4 decig. [6 gr.]. This precipitate was not altered by muriatic acid, and appeared to me to be prussian blue with a little silex.

8·5 grs. of muriatic acid.

In the solution saturated with nitric acid pure nitrate of strontian occasioned no precipitate: but nitrate of silver threw down some muriate of silver, which weighed when dry 22 dec. [34 gr.[. answering to 25 cent. [$8\frac{1}{2}$ grs.] of muriatic acid.

270 grs. of potash.

After having separated a little silver from the liquor by means of sulphuretted hidrogen, I filtered, and evaporated to dryness. Thus I obtained 33 gram. [510 grs.] of nitrate of potash; containing 17·5 gram. [270 grs.] of pure potash according to the analysis of Thenard. These 33 gram. of potash contained no foreign matter, for they crystallized to the last particle.

6 grs. of silex.

110 grs. of lime.

A little magnesia and lime.

The part insoluble in water being treated with nitric acid, 4 decig. [6 gr.] of silex were left. Carbonate of potash threw down from the nitric solution 13 gram. [200 grs.] of carbonate of lime; and the filtered liquor, being boiled, let fall a few decigr. of carbonate of magnesia and lime. It is probable, however, that the lime, which constitutes the greater

greater part of the insoluble portion of the ashes, is not in the state of carbonate in the plant, but saturated, as well as the potash, with another acid, which will be mentioned presently.

From these observations we may infer, that a hundred pounds of the ashes produced by incinerating the phytolacca will yield 66 lbs. 10 oz. 5 dr. of dried alkaline carbonate nearly pure, and containing about 42 lbs. of pure and caustic potash*.

100 lbs. of ashes give 66 lbs, 10⅝ oz. of carbonate of potash.

SECT. II.

Examination of the acid, that neutralizes the potash in the phytolacca.

I BOILED 4½ hectogr. [14½ oz. troy] of the fresh woody stalks of this plant in a quantity of water. The decoction did not change infusion of litmus. On evaporating to a sirupy consistence, 1 gram. [15½ grs.] of a salt confusedly crystallized was deposited by standing some time. Of this the greater part was soluble in water, 2·5 decig. [3¾ grs.] of a white powder remaining, which were dissolved in nitric acid. With this solution nitrate of lead gave a white precipitate, which, dried and put on a red hot iron, gave a little smoke, and left a yellow oxide of lead, soluble in a weak acid.

14½ oz. of the stalks boiled yielded 15½ grs of neutral salt.

The soluble part of the saline deposit did not afford any very distinct crystals on being evaporated, but a saline crust of a very pungent taste was formed.

Not distinctly crystallizable.

This salt swells up and is carbonized when exposed to the fire, and leaves as a residuum carbonate of potash. The same salt, when heated with concentrated sulphuric acid, is blackened, and produces sulphurous acid.

Swells up, burns, and leaves carbonate of potash.

Lime water, and the nitrates of lime, strontian, and lead, form white precipitates in its solution, which are insoluble in distilled vinegar.

With lime, strontian, and lead, insoluble in vinegar.

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* As potash diminishes considerably in vegetables in proportion as they approach the woody state, it is probable, that the phytolacca would afford a much greater proportion of potash in an earlier stage of its growth.

The decoction diluted and precipitated by nitrate of lead.

These experiments announced a deliquescent and difficultly crystallizable salt in the phytolacca: and to separate its elements, I diluted the decoction, which had been evaporated to a sirupy consistence, with a sufficient quantity of water. To this I added a solution of nitrate of lead, which formed a very copious precipitate; and the filtered liquor afforded an abundant crystallization of nitrate of potash.

With sulphuric acid gave the smell of uree.

The precipitate, after being well washed and dried, weighed 5 gram. [77 grs.]. Being treated with a fourth of its weight of sulphuric acid diluted with six parts of water, a tolerably decided smell of uree exhaled from the mixture, which had been exposed to a gentle heat on a sand bath; and the liquor was filtered. What passed through was acid; did not crystallize by evaporation or standing; and on continuing the evaporation a glutinous matter remained, yellowish, attracting moisture from the air, and carbonized by a stronger heat.

The acid liquor.

Farther examined.

In this acid the nitrate of lead produced a very copious flocculent precipitate; and the precipitate, exposed to the blowpipe, was immediately reduced to a globule of metallic lead.

The nitrates of lime and strontian produced no perceptible alteration in it, but ammonia occasioned a precipitate.

It precipitated lime water; and the precipitate put on a hot iron began by carbonizing, and left some ashes, which dissolved with effervescence in nitric acid.

What remained of the acid I saturated with soda, but no crystals were produced by evaporation. The result of this combination, when heated in a crucible, burned, leaving a light, alkaline coal, that effervesced with nitric acid.

Analogous to the malic.

From the properties here mentioned, it appears, that the acid of phytolacca has considerable affinity to the malic, but with a few shades of difference. With lime and lead malic acid forms flocculent precipitates very easily soluble in distilled vinegar; but those with the phytolaccic acid are insoluble. This acid may probably be a mean between the malic and oxalic, or an oxigenized malic acid; but as it is very abundant in the phytolacca, it will be easy to ascertain this by farther examination of its saline combinations.

Points in which they differ.

Perhaps intermediate between the malic and oxalic.

On

On the contrary, should it prove to be malic acid, it would be at least the first example of malate of potash found abundantly as a natural production

SECT. III.

Examinatian of the colouring matter contained in the berries of the phytolacca.

The colouring matter.

The berries being bruised in a glass mortar with a certain quantity of water, the filtered liquor was of a fine bright purple colour. I at first attempted to fix it on cloths, but soon found its extreme fugaciousness.

The juice of the berries.

The juice of the berries has a sweetish taste, leaving behind a sensation of acrimony, Paper tinged blue with litmus, and wetted with water, was reddened when dipped into it; but the blue was restored without the least alteration by washing with a little warm water.

Soon ferments. Taste bad, but yields spirit.

At a moderate temperature it soon underwent the vinous fermentation. The wine produced was unpleasant to the taste, but brandy may be obtained from it by distillation.

Lime water turns it yellow. Any acid,

If a few drops of lime water be added to the juice, it soon assumes a fine yellow colour; but the smallest quantity of acid soon restores its purple hue. If the yellow liquor have sufficient colour to write with it, breathing on the paper will change the yellow letters to purple; and so will even exposure to the air, though less speedily.

sulphuretted hidrogen, or urine, restores the purple.

Sulphuretted hidrogen, or urine added in small quantity to the yellow liquor, changes it immediately purple.

Water dilutes the yellow, and in large quantity restores the purple.

The deep yellow liquor produced by the combination of the purple of phytolacca with lime grows lighter by the addition of water, and assumes the tint of chromate of potash. But if the quantity of water be still increased, the original purple reappears. At first I ascribed this effect to the carbonic acid, that might be contained in the water; but water long boiled exhibits the same phenomenon. Hence I infer, that water acts by weakening the effect of the lime in the yellow liquor, which occasions it to return to purple.

A delicate test of acids:

From what has been said it appears, that the yellow liquid affords a very delicate test of the presence of acids; and

and a comparative experiment with litmus will corroborate this. Into two glasses I put equal quantities of the juice of phytolacca, which I had turned yellow by a few drops of lime water: and into the two other glasses I put an equal weight of infusion of litmus, of an equal depth of colour. More than 60 drops of a very weak acid were required to redden the infusion of litmus, but less than 15 restored the purple colour of the yellow liquor. Hence it follows, that the yellow liquor of phytolacca is at least four times as sensible as the infusion of litmus; but the yellow liquor being extremely fugacious, it cannot be kept, or even used but just after it is prepared. A few hours are sufficient to change it. First a fallow precipitate falls down, which, looked at in the sun, exhibits very small scales with the various hues of the opal. After a few days brown flocks separate from it. The properties of the reagent are then entirely destroyed, and at length the liquor is almost wholly deprived of colour.

but does not keep.

Its spontaneous changes.

The following are the results produced in the purple liquor by other reagents.

Caustic alkalis give it a yellow colour. Alkaline subcarbonates, a fine violet, that fades, and becomes yellow, by standing. Weak acids, nothing perceptible. Dilute oxigenized muriatic acid, a complete deprivation of colour, with white flocks. Alum, nothing apparent on mixing, but after some days a very light red precipitate. Muriate of lime, no change. Muriate of tin, a red sediment inclining to lilac, and leaving the supernatant fluid colourless. Nitrate of lead, a precipitate of the colour of wine-lees. Superoxided sulphate of iron, a dirty violet: and on adding an alkali a very deep green precipitate, changing yellow by exposure to the air.

Effects of reagents on the purple liquor.

The purple colour that tinges the epidermis of the stalks of the phytolacca is precisely of the same nature as that contained in the berries, and afforded the same results.

The colouring matter of the stalks the same.

I have convinced myself, that this purple does not arise from the alteration of any colour by an acid: for having changed it yellow by ammonia perfectly freed from carbonic acid, impregnated linen with this, and exposed the linen to a moderate temperature excluding the air, the purple reappeared in all its lustre as the ammonia was volatilized.

Not a colour altered by an acid.

This

This colour is different therefore from that of some other fruits, as the plum and cherry, which becomes green on the addition of alkalis; and from that of litmus, which alkalis turn blue: but it appears to have some analogy with that of the grape, as lime water turns red wine of a dirty yellow, which acids change again to red.

Differs from the red juices of some other fruits.

Analogous to that of the grape.

Sect. IV.

Other properties, and cultivation of the phytolacca.

In North America the leaves are boiled, and eaten as spinach, and I have found them very good*. The juice of the root is a purgative, and may be taken in the dose of a tablespoonful or two; but must not be used when there is any inflammation of the viscera. The narcotic virtues, that have been ascribed to it are illusory, as Lemery observed.

Its leaves esculent.

Juice of the root purgative.

It may be propagated by seed, sown in the spring in light ground, and transplanted to a dry soil, which should be dug very deep. When the plants have taken root, they require no care, but to be kept free from weeds. The stalks die with the first frosts, but the roots are perennial, and throw out shoots in the spring for several years.

Culture.

III.

A Memoir on the Proteus Anguinus; by Baron von Zoïs†.

THE proteus anguinus is found in Carniola, between Sittich, an ancient monastery about eight leagues from Laybach, on the road to Neustadt, and a small village called Vir in the Sclavonian language, and Weyer in German.

Where found.

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* The young shoots are said to be as good as asparagus. Tr.

† Translated from the Italian manuscript of Baron Zoïs by Mr. Siauve, commissary at war; and inserted in the Magazin Encyclopédique for January, 1807, p. 69; whence this article is taken.

The

Geology of the place.

The rock that composes the hills of Sittich is a compact, stratified, calcareous mass, rising in the centre of our Alps to the height of 1500 toises above the level of the Adriatic; and the geological character of which is to be interspersed with funnel-shaped hollows on the surface, and grottoes and caverns internally.

At the foot of a part of this calcareous rock, at the bottom of the valley of Vir, are two openings or mouths of grottoes, 15 or 18 inches in diameter, 3 or 4 feet above the surface of the ground, and about 12 feet distant from each other. From each of these a stream of cold and limpid water flows into a small basin beneath, which is afterward lost in the ground about 750 paces beyond the village. The amphibious animal in question has never yet been found in Carniola, except in these two basins: and as it is never seen in them, except on the melting of the snow, or after heavy rains, it is supposed, that the overflowing of the subterranean reservoirs, to which they belong, drives them out. The peasants of Vir, who know them by tradition as well as experience, call them *bela riba*, white fish, or *zhloveshka riba*, fish that has something human. The latter name alludes to the joints of their toes, or fingers, and the colour of their skin.

Seen only in two basins, supplied from subterraneous caverns, and after inundations.

Popular names.

By whom noticed.

The animal was first made known to the public by Dr. Laurenti, in his *Synopsis Reptilium*, in 1768. He gave it the name of proteus anguinus. Scopoli, who saw it alive, gave a fuller description of it in 1772, in his *Annus Quintus Historico-Naturalis*. He says, that Linneus, to whom he sent it, considered it as the larva of a lizard; but he thought it a distinct genus. Linneus however expressed himself doubtingly both on this and the proteus tritonius of Schranck, found in lakes in the interior parts of Austria; leaving it to accurate and repeated observation to decide, whether or not they underwent a transformation at a late period.

Linneus suspected it to be a larva.

Dissected.

To forward the solution of the problem, Baron Zoïs sent several specimens of the proteus anguinus preserved in spirit to Dr. Schreiber, professor of natural history at Vienna, that he might dissect them. The anatomical description is given at length in the Philosophical Transactions for 1801.

Its analogy to larvæ of some

The principal analogy between the proteus and the larvæ of some amphibia, which has occasioned them to be confounded

founded*, consists in the gills common to both. Mr. Schreiber however observes, that the gills of the proteus differ essentially from those of larvæ and fishes by their red colour, owing to the blood which it causes to circulate through them more or less abundantly at pleasure.

amphibia in the gills.

Difference.

As to the organs of respiration, Mr. Schreiber asserts, that, having dissected a great number of larvæ of aquatic lizards, he never found the least analogy between them and the proteus. He considers it as more allied to the siren lacertina, both having gills and lungs; though Camper indeed denies the existence of lungs in the siren. It is true Mr. Schreiber observes, the siren differs in having but two feet; a short head without any beak; a small, pointed mouth; eyes very apparent, and eyebrows; and the lungs, though equally formed of one single membrane, without any cellular divisions, running along both sides of the body, and exhibiting neither the various turns nor the very remarkable bladders found in those of the proteus.

More analogous to the siren lacertina.

Differences.

Lungs.

That Mr. Schreiber found it difficult to discover and ascertain all the parts of the organization of specimens, that had been kept a long time in spirits, is not at all surprising. He observed ovaries however, and even something that had the form of a uterus: but he lays no great stress on these slight appearances.

Difficult to ascertain its parts.

Apparently ovaries and a uterus.

I hope however he will be able to decide the question, by means of the dissection of some individuals, which I have found means to send him alive. The basins at Vir had furnished only three, four, or five in a year since 1798: but on the 26th of December, 1804, on the thawing of a deep snow, fourteen were brought me at once. These are at present at Vienna, some of them under the eye of Mr. Schreiber, and others in a subterraneous canal, under circumstances the most

Only from 3 to 5 found in a year.

Once 14.

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* The question appears at present decided, and the protei are considered as a distinct genus Beside the anguinus, which had already been figured and described, but of the manners and habits of which Baron Zoïs gives here new and interesting particulars, and the tritonius of Schranck, Humboldt and Bonpland observed another species in Peru, the skin and limbs of which perfectly resemble those of the salamander.

most favourable for their nourishment, and for their breeding, if they be capable of it*.

Manners.

As to the manner of living of the proteus, whether it be a perfect animal or not, its principal character is a very decided antipathy to daylight. Exposed to the sun it is agitated in an extraordinary manner, and makes continual efforts to escape. Yet it has no eyes externally, or, as Scopoli asserts, two tubercles in the place of eyes. Mr. Schreiber first discovered its small, black, subcutaneous eyes, which are perceptible sometimes, though but rarely; and this only in individuals that have grown lean by forced abstinence, and the epidermis of which is become very thin.

Strong antipathy to light.

No external eyes:

but two underneath the skin.

In its movements under water the proteus sometimes employs its paws, or feet; at other times its tail, in different manners. Its progress is slow and circumspect: but when it is irritated it flees with swiftness, and with a sinuous motion like an eel. In this case it makes no use of its feet; and as those behind might impede its velocity, it keeps them close to its body. During the day it likes to keep itself concealed, and seldom changes its place: by night on the contrary it is always seen moving about at the bottom of the water, and frequently attempts to get out.

Moves by means of its paws, or tail, slowly.

When offended quickly and like an eel.

Conceals itself in the day: at night in motion.

Those that are thus in captivity would never touch any of the food offered them, such as fresh eggs of fishes, fibres of fishes or frogs, aquatic worms, polypi, conferva, &c.; not even the helix thermalis, though it is certain in a state of liberty they swallow a great number of these testaceous animals; for I have found as many as eighty-four of their shells in the excrements of a well fed proteus, which he discharged at three times the second day of his captivity. When living ones were given him, he took up one with his mouth

Takes no food in confinement.

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* The archduke John has had a subterranean canal constructed of tufa. A stream of spring water runs through the sinuosities formed in it, and fills the different basins, in which the protei are. A spring was selected, the waters of which contain insects adapted for their food; and means have been contrived for inspecting the little colony, when it may be thought time. It is to be wished by the epicure, that the protei may breed, for their flesh is white, delicate, and of an exquisite flavour, as I have been assured by Baron Zoïs, who has eaten it. *Note of Mr. Siauve.*

Receptacle formed for them by the archduke.

mouth, but immediately threw it out to the distance of two inches. Afterward he chose rather to leave them to breed, than to taste them.

Lives a long while in water

Notwithstanding this obstinacy in refusing all kind of food, these animals live a long time in pure water, if they be kept in the shade in wooden tubs 15 or 18 inches high, and as much in diameter; and if the temperature of the water, which ought to be changed every four or five days, do not exceed 8° of Reaumur [50° F.]. Put into cold water in which snow was floating, I have seen them experience painful sensations, manifested by their agitation, and die within four and twenty hours.

about 50° F.

Know one another.

Utter a cry of satisfaction, and a hiss of displeasure.

When put together they appear to know one another, for the new comers soon grow familiar with the old ones, and sometimes even utter a gentle cry, that seems to indicate their satisfaction*: on the contrary, when they are offended by the curiosity of the spectators, or the appearance of the sun, they hiss with a harsh tone, very different from the preceding, and at the same time emit bubbles of air from the mouth and gills. The degree of their agitation may be distinguished by the greater or less tumefaction of the branchy plumes that rise from the gills, as well as from their colour, which in this state assumes a fine crimson. If these plumes be then viewed with a microscope, they resemble branches of coral; but when the animal is tranquil they fall, become flaccid, and are of a livid white.

Express offence by their gills.

Circulation of red blood seen in the gills.

With a lens of moderate power the systole and diastole of the pulse may be perceived in these plumes, the branches of

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* The proteus appears to possess the sense of feeling in an eminent degree. I have often been pleased to see it pass its little hands over others of its own species.

However this may be, the resemblance of its fore paws to the human hand, the fine carnation tint of this pretty animal, the transparent epidermis that may be compared with that which covers the person of a handsome woman, the sort of amorous cry it utters when it has a companion given it, have thrown me more than once into a revery. I know not whether the proteus possess more than ordinary intelligence: but every thing, even to its obstinacy in refusing all kind of nourishment, interest me in favour of this *fish-man*, as the Carniolians call it. *Note of Mr. Siauve.*

of which are so many tubes; and the red globules of blood may be seen to ascend and descend at each pulsation. I have counted from forty-five to forty-eight of these pulsations in a minute.

Out of the water soon sticks to the ground and dies.

Out of the water the proteus cannot proceed more than a few feet, for the glutinous substance, with which it is enveloped, soon dries, and glues it to the ground by the part on which it drags itself along. Several of mine died in this state. Having found one thus, that had still some signs of life, I separated it with my hand; but it died two days after, having a red streak, indicating inflammation, along the side which had adhered to the ground. Others after it, which were separated by means of warm water, continued to live.

Before death their slimy coat separates.

In all that died under my inspection I observed an infallible sign of approaching death: the gluten, with which they are covered, begins to separate from the body, and becomes visible in the water; it floats about them in flocks, attaching itself particularly to the paws and tail; at length they turn on their backs, and soon come to die on the surface of the water.

Size from 7 in. to 15, by 6 or 18 lines.

The annexed figure of a proteus anguinus drawn from nature, Pl. III, fig. 2, gives an accurate idea of its external form. With respect to size, they have been found from seven or eight to twelve or thirteen inches long, and even fifteen or sixteen; and in diameter from six to eighteen lines. These dimensions do not agree with those of the aquatic salamander, and there is no other reptile in Carniola, that can be compared with it.

Three have been kept 28 months.

I have at present three, that have lived in pure water two years and four months. They have grown thin, and diminished in size one half; and the fin that forms the tail even appears to be shortened: yet they are in perfect health. I shall not fail to observe the duration of their lives; though I begin to persuade myself that the vague term, which Linneus expresses by the words transformation at a late period, is already past.

IV.

Account of the Antique Vessel, that was preserved at Genoa under the name of SACRO CATINO, *and reputed to be an Emerald; with the Report made of it to the French Institute, August* 4, 1806, *by Mr.* GUYTON *.

Sacro Catino.

A Dish of an hexagonal shape had long been in possession of the city of Genoa, which was supposed to be an emerald, and consequently of inestimable price. Farther to enhance its value, according to the legend it was the very dish, on which the paschal lamb was served up, when Christ celebrated the passover with his apostles, afterward miraculously converted into an emerald; though some will have it to have been originally an emerald, and among the presents of the queen of Sheba to Solomon. Neither is it agreed in what manner it came into the possession of the Genoese; as some say it was their part of the booty found at the taking of Cæsarea in the first croisade; others, that it was presented to them by Baldwin king of Constantinople. It was so highly valued, however, that at the siege of Genoa in 1319, it was mortgaged for 1200 marks of gold, and redeemed eleven years after.

Legend respecting it.

Brought from the East.

Mortgaged for 1200 marks of gold.

An act passed in 1476 to prohibit its being touched with gold, silver, gems, coral, or any other substance, under very heavy penalties, and even in some cases pain of death, shows, that suspicions of its genuineness existed. William of Tyre is the first we know, that expresses such a suspicion. Barthelemy observed in it blebs, which induced him to think it glass. Condamine too, examining it by torchlight, and at some distance, could not perceive in it any of those clouds or defects of transparency, that are common in emeralds as well as in all gems of a certain size; yet he evidently discerned several small vacuities, resembling air blebs, both round and oval.

Suspicions of its being glass.

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* Abstracted from Mag. Encyclopédique, January, 1807, p. 137; and Annales de Chimie, Vol. LXI, p 250, March, 1807.

Removed to Paris, and examined.

At length this vessel having been transferred to the imperial library, the Institute was requested by the minister to ascertain its quality; and Haüy, Vauquelin, and Guyton, were accordingly appointed to examine it. What follows is the substance of Mr. Guyton's report.

Its figure.

Its diameter, from one angle to another, is 39·143 cent. [15 in. 2 l. Eng.], its depth within 12·357 [4 in. 8 l.], its height, including the foot, which is of the same piece, 16·476 [6 in. 4 l.]. It has two handles, likewise formed in the same piece, without any appearance of having being joined to it after it was made. One of these is broken. The bottom appears to have been wrought on the wheel, so as to form a circle of small cavities, whence issue six rays, corresponding to the angles. Pl. III, fig. 3, is an exact representation of the bottom; fig. 4 shows it standing on its foot, and fig. 5 inverted, with the position of its handles, which are placed so as to be concealed, yet in a manner to be taken hold of easily.

Colour.

The colour of the dish is an olive green, duller than that of the peridot, with something of a greasy cast, that brings it nearer to the plasma of the German mineralogists, than to the green emerald of Peru, or the bluish emerald, or aqua marina, of Siberia. Its transparency participates of this tint.

Has blebs.

On holding it up against the light, a bleb about 2 millim. [·78 of a line] is distinguishable near the centre, and farther off some very small blebs.

These marks doubtful.

It would be difficult from this description to infer the nature of its substance, since it is now well known, that the colour of gems of the same species varies considerably, and that rock crystals exhibit blebs interiorly, which we cannot always distinguish with certainty from those in glass.

Spec. grav. not examined.

We could not conveniently examine its specific gravity, on account of its size: and besides it would have been to little purpose, as the specific gravity of the emerald does not much exceed that of common glass, and is inferior to that of a glass loaded with metallic oxides.

Hardness.

We ascertained its hardness, however, which is a less equivocal character of gems; and found it very evidently scratched not only by the emeralds of Peru and Siberia, but

but even by rock crystal. This would be sufficient to decide, that it could not be a real emerald, if the enormous block, from which such a vessel must have been cut, would not be a phenomenon, that nothing hitherto found in nature renders probable.

Not an emerald.

It is a manifest errour in Dutens, to admit among the varieties of the Peruvian emerald a stone that can be touched by the file. Those of the environs of Limoges, which are scarcely transparent, scratch flint.

Mistake of Dutens.

The largest emeralds known, before the discovery of the colourless emeralds in the department of Haute-Vienne, and mentioned in the inventory of the Garde-Meuble, the catalogues of Davila, Daugny, &c., and those that Condamine saw at Rome, which he considered as enormous, did not exceed 10 or 12 cent. [3 in. 9 l. or 4 in. 7 l.] long, by 3 or 4 [1 in. 2 l. or 1 in. 6 l.] broad. Even those of the commune of Bessines have yet afforded only masses of 30 or 40 cent. [11 in. 7 l. or 15 in. 6 l.] high, by 15 or 16 [5 in. 8 l. or 6 in. 2 l.] thick.

Largest emeralds known.

Those of France.

We may judge what would be the value of the dish, if it were a real emerald, by taking the rule of its being worth one fourth the price of a diamond of equal weight.

What has been said is sufficient to authorise us to conclude, that the substance of the dish cannot be considered as an emerald, or any other gem, but is coloured glass. We leave to others to determine its value, either as a work of art, or as an antique; and whether it may be identified or compared with that which Herodotus says he saw at Tyre, in the temple of Hercules. We shall only observe, that the art of imitating gems by coloured glass dates from remote antiquity. Pliny speaks in several places of these false stones, and points out the method of distinguishing them, by trying their hardness against others.

As it is coloured glass it is valuable only as an antique.

V.

On the Cultivation of the Crambe Maritima of Linneus, or Sea Kale. By Mr. JOHN MAHER, *F. H. S.**

Culture of sea kale

improved by Mr. Curtis.

Very fine produced at Edmonton.

IF the man who makes two blades of grass grow where only one grew before, is to be esteemed an important benefactor to his country; he who teaches us how to improve a palatable and nutritious vegetable, hitherto often neglected, upon the barren cliffs of our sea-girt isle, has surely no small claims to our gratitude: as such, I must ever regard those of the late Mr. Curtis, from whose pamphlet upon the *Crambe Maritima*, or *Sea Kale*, I first learnt how to grow this early esculent; but as his useful directions are yet in the hands of comparatively few of my brother gardeners, and as the young shoots have been obtained at Edmonton of a size and delicacy greatly superior to what generally appears at the table, I venture to offer a particular account of the method of cultivating it there to the Horticultural Society.

Places where found wild.

The particular places on record where this plant grows wild, are below *Maryport;* also between *Ravenglass* and *Bootle*, in Cumberland; at *Roosebeck*, in Low Furness, Lancashire; near *Conway*, plentifully, but in the most inaccessible rocks; promontory of *Llyn*, and near *Cruccaeth*, in Caernarvonshire; between *Rhuddgaer* and *Llandwyn*, in the isle of Anglesea; about *Port Inon*, in Glamorganshire; near *Megavissey*, in Cornwall; marly cliffs, near *Teignmouth*, and *Sidmouth*, in Devonshire; on *Chesil Bank*, chalk cliffs at *Weymouth*, *Lulworth Cove*, and about *Poole*, in Dorsetshire; at *Western Court*, in Hampshire; near *Worthing* and *Shoreham*, cliffs at *Beachy Head*, and near *Hastings*, in Sussex; between *Folkstone* and *Dover*, at *St. Margaret's* and *Langdon Bays*, between *Whitstable* and the *Isle of Thanet*, at Lidde, in Kent; near *Harwich*, in Essex; on the north coast of *Norfolk*, abundantly; near *Fast-castle*,

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* From the Transactions of the Horticultural Society of London, Vol. I, Part I, p. 13. For an account of the objects of this society see Journal, Vol. XIV, p. 150.

Berwickshire.

Berwickshire. According to Dr. Smith, sandy shores are its natural soil, but by what I can learn from others, as well as my own personal observation, it prefers loamy cliffs, mixed with gravel. I found it near *Dover*, also in *Sussex*, in stiff loam: to the extensive beach of pure sand, both above and below *Scarborough*, in Yorkshire, it is, I believe, quite a stranger.

Natural soil.

Description.

The whole plant is smooth, of a beautiful glaucous hue, covered with a very fine meal; occasionally, however, it varies like the wallflower-leaved ten weeks stock, with quite green leaves. Root dark brown, perennial, running deep into the ground, divided into numerous wide spreading branches, but not creeping* Radical leaves very large, and spreading wide upon the ground, waved, more or less sinuated, and indented, containing a bud, or rudiment of the next year's stem at the bottom of the leafstalk, dying away in the autumn †. Stems several, from one foot and a half to two feet high, erect, branching alternately, and terminating in large panicles of spiked flowers, which smell somewhat like honey. Peduncles, as the fruit swells, considerably elongated. Calyx often tinged with purple, its leaflets nearly equal. Petals cream coloured, with purple claws, larger than in many genera of this natural order. Filaments purple. Anthers pale yellow. Glands of the receptacle between the longer filaments yellowish green. Stigma pale yellow. Pouch, as the accurate Mr. Woodward describes it in Withering's work, at first egg-shaped, afterwards nearly globular, fleshy, falling off when ripe, about August, with the seed in it, which is large, and of a pale brown colour.

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* Root not creeping, in the proper sense of that word, as Parkinson, Miller, and Bryant have described it; but if the branches be divided into a number of pieces, each piece will grow if committed to the earth; and as it is impossible to dig among the widely extended roots of these plants without cutting many of them, and leaving a number of fragments, plants arise from such around the original, and give to it the appearance of having creeping roots. CURT.

† Parkinson perhaps never committed a more egregious blunder, than in the account he has given of this part of the plant's economy; "*The root is somewhat great, keeping the green leaves all the winter.*" Bryant, in his *Fl. Diæt.* misled, perhaps, by this account, says, "*The radical leaves being green all the winter*, are cut by the inhabitants where the plant grows, and boiled as cabbage. CURT.

Exported 200 years ago.

The *Crambe Maritima* was known, and sent from this kingdom to the continent more than two hundred years ago, by l'Obel, and Turner*; but our immortal countryman, Philip Miller, has the honour of being the first who wrote upon it professionally, as an esculent, telling us, in the first edition of his Gardener's Dictionary, published in 1731, that the inhabitants of Sussex gather the wild plant to eat in spring, soon after the heads are thrust out of the ground, otherwise it will be tough and rank. Professor Martyn, next, in the last edition of the same work, has printed some valuable additional instructions, how to cultivate this plant, from the MS. of the Rev. Mr. Laurent. Lastly, the late celebrated Mr. Curtis has done more to recommend it, and diffuse the knowledge of it, in the dissertation above quoted, than any of his predecessors.

Miller noticed it.

Mode of culture.

To grow this vegetable in the highest perfection, prepare the ground in December or January, by trenching it two feet and a half deep; if not that depth naturally, and light, it must be made so artificially, by adding a due proportion of fine white sand, and very rotten vegetable mould. If your ground is wet in winter, it must be effectually drained, so that no water may stand within a foot at least of the bottom: for the strength of your plants depends on the dryness of the bottom, and richness of your soil. Then divide the ground into beds, four feet wide, with alleys of eighteen inches, after which, at the distance of every two feet each way, sow five or six seeds two inches deep, in a circle of about four inches diameter; this operation must be performed with strict care and regularity, as the plants are afterwards to be covered with the blanching pots†, of which a drawing

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* It would be difficult to ascertain the precise period of its being first used with us as a culinary plant; on many parts of the seacoast, the inhabitants for time immemorial have been in the practice of seeking for the plant in the spring, where it grows spontaneously; and, removing the sand or pebbles, they cut off the young shoots as yet blanched, close to the root. Mr William Jones, of Chelsea, saw bundles of it in a cultivatod state, exposed for sale, in Chichester market, in the year 1753. CURT.

† It appears to me, that for forcing, it would be a great improvement to make the blanching pots in two pieces, the uppermost of which should

a drawing is annexed, Pl. III, fig. 6, and both the health and beauty of the crop depends upon their standing at equal distances. In the months of May and June, if the seeds are sound, the young plants will appear. When they have made three or four leaves, take away all but three of the best plants from each circle, planting out those you pull up (which by a careful hand may be drawn with all their tap root) in a spare bed for extraforcing, or to repair accidents. The turnip fly and wire worm are great enemies to the whole class of tetradynamia plants. I know no remedy for the latter, but picking them out of the ground by hand; the former may be prevented from doing much damage, by a circle of quick lime strewed round the young plants. If the months of June and July prove dry, water the whole beds plentifully. In the following November, as soon as the leaves are decayed, clear them away, and cover the beds an inch thick with fresh light earth and sand, that has laid in a heap and been turned over at least three times the preceding summer; this, and indeed all composts, should be kept scrupulously free from weeds, many of which nourish insects, and the compost is too often filled with their eggs and grubs. Upon this dressing of sandy loam, throw about six inches in depth of light stable litter, which finishes every thing to be done the first year.

Wire worm and turnip fly.

In the spring of the second year, when the plants are beginning to push, rake off the stable litter, digging a little of the most rotten into the alleys, and add another inch in depth of fresh loam and sand. Abstain from cutting this year, though some of the plants will probably rise very strong, treating the beds the succeeding winter exactly as before.

2d year.

The third season, a little before the plants begin to stir, rake off the winter covering, laying on now an inch in depth of pure dry sand, or fine gravel. Then cover each parcel with one of the blanching pots, pressing it very firmly into the ground, so as to exclude all light and air; for the colour and flavour of the *Sea Kale* is greatly injured by being exposed

3d season.

should fit like a cap upon the lower; the crop might then be examined without disturbing the hot dung. SECR.

posed to either. If the beds are twenty-six feet long, and four wide, they will hold twenty-four blanching pots, with three plants under each, making seventy-two plants in a bed. Examine them from time to time, cutting the young stems, when about three inches above ground, carefully, so as not to injure any of the remaining buds below, some of which will immediately begin to swell; in this method, a succession of gatherings may be continued for the space of six weeks, after which period the plants should be uncovered, and their leaves suffered to grow, that they may acquire and return nutriment to the root for the next year's buds. The flowers, when seeds are not wanted, ought to be nipped off with the finger and thumb, as long as they appear. If a gentleman does not choose to be at the expense of the blanching pots, the beds must be covered with a larger potion of loose gravel, and mats; but the time and trouble of taking away the gravel from about the plants to cut the crop, and replacing it, is so great, that there is no real economy in it. In this way *Sea Kale* has been cut in Mr. Beale's garden, which measured ten, eleven, and even twelve inches in circumference, and upon an average each blanching pot affords a dish twice in a season.

10 to 12 inches circumference.

Forced with little expense or trouble.

No vegetable can be so easily forced as this, or with so little expense and trouble; for the dung is in the finest possible order for spring hot-beds, after the *Sea Kale* is gathered. The only thing necessary, is to be very particular in guarding against too much heat, keeping the temperature under the blanching pots as near to fifty-five degrees of Fahrenheit's thermometer as may be, but never higher than sixty. For this purpose, in November and December, according as you want your *Sea Kale*, prepare a sufficient quantity of fresh stable dung, to cover both the beds and alleys, from two to three feet high; for in the quantity to be laid on, a great deal must always be left to the good sense of the gardener, and the mildness or severity of the season. It should be closely pressed down between the blanching pots, placing heat-sticks at proper intervals, which by being examined occasionally will indicate the heat below. After the dung has remained four or five days, examine the pots. Worms often spring above the surface, and spoil the delicacy of the young

Worms.

young shoots: the best remedy against which is to cover with dry sea-coal ashes, sifted neither very small nor very large; salt also effectually destroys them, and will not injure the *Sea Kale*. The crop will be ready to gather in three weeks or a month from first applying the heat, but so much mischief ensues when this is violent, that I would advise every one to begin time enough, and force slowly, rather than quickly. It is also necessary to cut the leaves off a fortnight or three weeks before they decay, from such plants as you intend to force very early.

VI.

On Grease, and some Medicinal Compounds, of which it is the Basis: by H. A. VOGEL, *Chemical Operator in the Pharmaceutic School at Paris. Abridged by* BOUILLON-LAGRANGE*.

MR. Vogel, not having an opportunity of instituting a comparative examination of the soft fat of various animals, confined himself to hog's lard, the most common, and of most extensive use. Lard.

Lard, exposed for two months to the rays of the sun, without access of air, acquires a very pungent rancid smell, an acrid taste that affects the throat a long time, and a yellow colour, but no acidity. By the joint action of light and air the same phenomena take place, and in addition it becomes acid. Effects of light on it. Of air.

It melts at 104° or 108° F., and remains in fusion at this temperature without being decomposed; but above 212° decomposition commences. If it have been well washed, it affords no traces of ammonia on distillation. Of caloric.

Mixed with half its weight of washed flowers of sulphur, forming what is commonly called sulphur pomatum, and examined four days after, as well as when kept much longer, no trace of sulphuric acid was discoverable. By gentle fusion Dissolves sulphur.

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* Abridged from the Annales de Chimie, Vol. LVIII, p. 154, May, 1806.

sion on a water bath a portion of the lard was separated, and poured off; and by straining the rest through fine linen the greater part was obtained. It was of a gray colour, and a very strong, acrid, bitter taste; stood more readily on cooling; and blackened silver. If sulphuretted lard be boiled, decanted, and cooled quickly, part of the sulphur precipitates: but if it be cooled slowly it crystallizes in fine needles.

This compound distilled yields sulphuretted and carburetted hidrogen & carbonic acid:

If this mixture be distilled in a coated glass retort, to which a receiver is adapted communicating with a mercurial trough, a large quantity of gas is obtained, which appears to be a mixture of a great deal of sulphuretted hidrogen, some carburetted hidrogen, and a little carbonic acid. No sulphurous acid gas was found, as many chemists assert. As soon as the gasses cease to come over, thick, white vapours rise, that condense with difficulty, and a yellow matter sublimes into the neck of the retort, which is lard mixed with a little sulphur. The liquor in the receiver is milky, and on cooling affords small crystals in white scales, which are sulphur in a state of extreme division. A very bulky, shining, iridescent coal remains in the retort.

lard mixed with sulphur sublimes;

a bulky coal remains

Sulphuretted hidrogen not dissolved in it.

Sulphuretted hidrogen gas, passed through melted lard, produces no change, and does not dissolve in it.

Dissolves a little phosphorus.

Half an ounce of lard being melted on a water bath, two grains of well purified and very transparent phosphorus were added, and kept a quarter of an hour at the same temperature; care being taken not to shake it too much, that the air might not acidify the phosphorus. When cold, some of the phosphorus was found undissolved. The lard had acquired a slight smell of garlic, and a disagreeable taste: it reddened infusion of litmus: it formed a very copious black precipitate with nitrate of silver, and a less abundant black precipitate with neutral nitrate of mercury at a minimum. An ounce of lard, brought to boil gently, was found to dissolve five grains of phosphorus; but part was precipitated by cooling. The lard was repeatedly washed with boiling water, which it rendered acrid; but it still retained some of the phosphorus in actual solution, without its being acidified.

Faintly luminous when heated.

The phosphuretted lard prepared with a boiling or a gentle heat, and washed or unwashed, did not shine in the dark at a temperature of 55° or 65°, even when rubbed by the hand. At 167° it was faintly luminous.

Lard distilled with phosphorus.

Twelve grains of phosphorus being distilled with two ounces of lard, the mixture assumed a coally aspect much sooner than lard alone. At the commencement phosphuretted hidrogen gas was evolved, which took fire in the receiver: and afterward both phosphuretted and carburetted hidrogen were obtained in a jar over mercury. The receiver contained lard, which had carried over with it phosphorus and phosphuretted hidrogen gas. After cooling, on the admission of air, it burned the lard rapidly.

Phosphorous acid always formed.

Whatever temperature be employed therefore, to dissolve phosphorus in lard, more or less phosphorous acid is always formed; whence I am induced to think, that the same thing happens in many other phosphuretted compounds.

If air were excluded, it soon becomes acid on exposure to it.

All these experiments were made in contact with air. When air was excluded, the lard dissolved a portion of phosphorus without its being acidified; but it became acid in a few minutes, on pouring out the melted lard, or shaking it in the open air.

Takes phosphorus from phosphuretted hidrogen.

A cylinder 10 inches long and 8 lines in diameter was filled with melted lard, and immersed in mercury. Half of it being expelled in this situation by phosphuretted hidrogen gas, the cylinder was corked, removed into hot water to keep the lard in fusion, and shaken till cold. On examination it was found to have dissolved all the phosphorus contained in the gas.

Action of acids.

As the muriatic acid does not act on lard, and there is nothing interesting in the action of the sulphuric, Mr. Vogel confined himself to the nitric. He treated lard with it as directed by Fourcroy and Alyon for making the oxigenized pomatum. Alyon observes, that this pomatum has no need of being washed, as it is not acid. Vogel repeated his process with an ounce of acid at 32° to a pound of lard; and afterward with acid at 30°, 28°, and as weak as 24°: but he always found the oxigenized lard acid.

Nitric always leaves it acid.

Nitrogen, nitrous, and car-

Making the experiment in a retort communicating with a pneumato-chemical apparatus, he obtained nitrogen gas, not

pure,

bonic acid gasses evolved.

pure, as Mr. Alyon says, but mixed with nitrous gas and carbonic acid gas, as Van Mons found.

Lard oxigenized by nitric acid.

The lard thus oxigenized is as hard as suet, and requires a heat of 113° or 117½° to melt it. Water boiled with it, and partly evaporated, acquired a lemon colour, and a rough, bitter taste; reddened litmus; and constantly precipitated acetate of lead and nitrate of mercury. Distilled in a retort almost to dryness it yields a colourless fluid containing a quantity of acetic acid, and not precipitating the metallic solutions above mentioned.

Yields an acid to water.

The water in which it is washed, being evaporated to the consistence of a thick liquid, lets fall on cooling a brown, tenacious matter, attracting moisture from the air. The supernatant liquor being decanted and evaporated, an infinite number of small, white, very brilliant needles form in it. These Mr. Vogel took at first for oxalic acid, but lime-water was not rendered turbid by it, and it had none of the properties of oxalic acid. Its nature will be seen below.

Neither the colour nor acidity removed by washing.

Neither the yellow colour nor acidity of oxigenized lard can be removed by repeated ablutions; for after the twelfth boiling it continues yellow, and the water poured off from it reddens litmus.

Alcohol dissolves a large portion;

Alcohol comports itself differently. If it be boiled with oxigenized lard, it dissolves a very large quantity; and on cooling a great deal separates in flocks, which, collected and dried, afford an oxigenized lard strikingly whitened. The remaining lard is rendered whiter: the alcohol acquires a yellow colour, and becomes acid; and it retains matter enough in solution to form a copious precipitate with water. The alcohol being evaporated, a great deal of yellow acid fat remained, which was partly soluble in water.

which is afterward partly soluble in water.

Does not removes its acidity.

Boiling alcohol however, employed repeatedly to wash oxigenized lard, does not deprive it completely of acidity: it rather dissolves the greater part of it, and the last liquor is still acid.

The acid separated by lime:

As the acid adheres so intimately to the lard, I attempted to separate it by salifiable bases. For this purpose I boiled it with lime water, which was thus deprived of its alkalinity, and acquired a lemon colour. This neutral liquor, which I considered as a compound of lime with an acid and lard, was

was copiously precipitated by acetate of lead. Evaporated to the consistence of a sirup, it was divested of colour by the nitric and muriatic acids, which formed in it a whitish precipitate; and on pouring in the acid a very rancid smell was perceived.

Barytes water acts on the oxigenized lard more effectually. The orange yellow colour it acquires from it is equally destroyed by acids. I poured in a quantity of sulphuric acid sufficient to take up the barytes, boiled the whole, and filtered it at a boiling heat. The filtered liquor, which contained no barytes, was evaporated in great part on a sand heat. Small slender needles crystallized from it, interspersed with silky tufts. These were insoluble in alcohol, did not precipitate lime water, and were not sublimable in close vessels.

and more effectually by barytes.

If lard be boiled in concentrated nitric acid, and the ebullition be continued, adding water occasionally, a crystalline white powder forms in it on cooling. This powder is rough to the touch, insoluble in alcohol, and much more soluble in hot water than in cold. By its combinations with different bases, and other characters, I satisfied myself, that it was mucous acid*.

Boiling nitric acid forms pulverulent crystals,

which are saccholactic acid.

Lard thus oxigenized at a maximum is soft, of a brown colour, perceptibly soluble in water, and very soluble in alcohol. The water in which it was washed being saturated by potash, the result was a foliated salt, attracting humidity from the air, and giving out acetic acid on treating it with sulphuric†. The precipitate formed by acetate of lead in this water is nothing but the lard itself, which combines with the oxide of lead, and carries down with it a little mucous acid. The former swims on the surface, when the precipitate is decomposed by sulphuric acid.

Lard oxigenized at a maximum.

Affords acetic acid.

Oxigenized lard being very soluble in alcohol, a large quantity may be precipitated from it by water. By the powerful action of concentrated nitric acid on lard a certain quantity of nitrate of ammonia is formed, as may be seen by

Nitrate of ammonia formed.

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* Beef suet, though it decomposes nitric acid less powerfully, likewise affords mucous acid.

† Rancid fat and very old suet likewise afforded Mr. Vogel acetic acid, when treated in the same manner.

by boiling down the water in which it is washed, and adding to it potash or quick lime.

Action of oxigenized muriatic acid.

The action of oxigenized muriatic acid not having yet been described, Mr. Vogel conceived it might be useful to enter into it somewhat at large. He passed a large quantity of the acid in the state of gas through lard kept in fusion on a water bath. The gas previously traversed a phial containing water, and the process was continued, till the bubbles were no longer detained in the lard, which absorbed a very large quantity. When cold, the lard was considerably increased in weight, of a dirty white colour, and much altered in its consistence; as it was soft, resembling a thick oily fluid, so as to be capable of being poured easily from one bottle into another, at a temperature of 55°. When first exposed to the air it emitted white acid vapours.

After standing exposed to the air for two months, it acquired a little more solidity, but never that of common lard. Its taste was rancid, not perceptibly acid, and left behind it a slight bitterness irritating the throat. The simple muriatic acid was so combined with the lard, that only a very small portion could be taken up by washing with boiling water. Nitric acid however expelled it abundantly in the form of white vapours with effervescence: yet, what is singular, the nitric acid is not decomposed, in whatever quantity it be employed, and the lard acquires neither colour nor solidity from it.

Very little of the acid taken up by water, but expelled by the nitric, which does not act on it as before.

Actions on metals.

It is known, that fat acts more or less on several metals. Copper for instance gives it a green colour, when air can act jointly with it.

Mercury.

As the combination of mercury with it is of most importance to the art of pharmacy, Mr. Vogel attended to this more particularly. Many apothecaries have endeavoured to improve the processes for medicines of this kind, and particularly for the strong mercurial ointment. Mr. Veau Delaunay proposes rancid oil, and Fourcroy has shown, that fat when oxigenized is better adapted for the extinction of mercury. Many chemists have suspected, that the mercury in this ointment is in the metallic state, and not oxided. Mr. Vogel, knowing no experiment to support this opinion, made the following.

He

Gains no oxigen from the air in trituration with lard.

He triturated equal parts of lard and mercury in a mortar, which he had accurately weighed. When the mercury was completely extinguished, he weighed the mortar with the ointment in it, and found it had gained nothing. Hence he inferred, that the mercury, if oxided, must have been so at the expense of the lard, and not by the oxigen of the air.

It remains in the metallic state.

To discover the state of the mercury, he introduced this ointment recently prepared into a cylinder of glass hermetically sealed at one end; and kept it three hours in boiling water. After it was cold two very distinct strata appeared, the uppermost of which was white like lard. From this he separated the lower by cutting the cylinder with a file. On braying this gently with hot water, 3 drachms 3 grs. of running mercury were collected. The remainder, which obstinately retained a little lard, was treated with a lie of caustic potash. The soap formed was dissolved in alcohol, and thus the whole of the mercury was recovered.

Farther proofs of this.

He likewise separated the lard from the mercury by boiling the ointment in water. The lard swam on the top, slightly coloured by a little mercury, that adhered strongly to it: and the mercury remained at the bottom of the vessel, mixed with a little lard, but the slightest agitation united its globules.

The ointment being treated with muriatic acid in close vessels, no oxigenized muriatic gas was evolved.

The mercury acquires a little oxigen by long keeping.

Ointments that had been prepared three months, eight months, and several years, being examined, a little oxided mercury was found, but the greater part was still in the metallic state.

Mercury not altered by turpentine.

Mr. Vogel likewise triturated mercury with Venice turpentine, which extinguished it with facility. The turpentine being then dissolved in alcohol, the mercury was left behind in little globules; and the alcohol being evaporated, the turpentine was recovered without any alteration in its properties.

Merely divided in these and similar compounds.

In these ointments, therefore, the mercury is not in the state of oxide, as has generally been supposed, but merely divided very minutely. Mr. Vogel is likewise inclined to think, that it is in a similar state in many mercurial compounds more or less in use, as the mercurial plaster of Vigo, ethiops saccharatus, ethiops alkalisatus, Plenck's gummy mercurial, and a number of similar mixtures. If the colour be objected, it may be observed, that antimony, however brilliant, bismuth, or any other metal capable of being powdered, becomes of a blackish gray when minutely divided.

Action on salts.

Mr. Vogel next examined the action of fat on metallic salts.

Ointment of nitrated quicksilver.

He prepared the unguentum citrinum by dissolving three ounces of mercury in four of nitric acid, and mixing them with two pounds of lard. As the surface of this ointment always grows white after a time, for which some account by ascribing it gratuitously to the absorption of oxigen by the air, he poured this ointment while still fluid into squares of paper. Some of these he placed under a jar filled with air over mercury. In twenty-four hours no absorption had taken place, yet the surface was strikingly whitened. Others he placed under the receiver of an airpump, in which he speedily made a vacuum; and this he kept up for some hours, giving occasionally a stroke with the piston, which at first occasioned an ebullition of air-bubbles. The ointment when removed from the vacuum was perfectly yellow, and remained in this state without the least change.

Whiteness of the surface owing to air detained there.

From these experiments he conceives, that the white crust is owing to the extrication of gas, either nitrogen or nitrous, which arrives from all the internal parts at the surface, and increases its volume. As it gradually cools, it does not leave the gas time to escape entirely, so that part of it remains, and forms an infinite number of small white bubbles at the surface.

May be made so as to remain yellow.

In confirmation of this may be added, that, when the ointment is suffered to cool in the vessel in which it was kept in fusion, and particularly when it is still heated a little, the quantity of caloric is sufficient to expel all the gas, and the ointment remains constantly yellow, without undergoing any farther alteration.

Boiled in water.

To examine this compound, and form a judgment of the chemical changes, that might have taken place, Mr. Vogel boiled in water for half an hour some ointment, that had been made about two years. It became very clotty, and the water

water was so interposed, that it was difficult to separate the whole of it. The water acquired a yellow colour, and a slightly bitter taste; was scarcely at all acid; and did not contain an atom of mercury.

By way of comparison some ointment only a day old was treated with hot water as long as it would take up any thing. This had nearly the same characters as the water with which the old ointment was washed, and scarcely exhibited any traces of mercury on the addition of a hidrosulphuret.

The acid converts the mercury to yellow nitrate.

Hence it was natural to conclude, that the acid nitrate of mercury had undergone a change; and it might be presumed, that it had passed to the state of yellow nitrate, or nitrous turbith, which is little soluble in water. On keeping the ointment a long time in fusion, however, no turbith separated from it, so that it must be intimately united or dissolved in the lard. To satisfy himself of the possibility of this solution, Mr. Vogel heated turbith with oxigenized lard; and having decanted the clear fluid part, it perfectly resembled the unguentum citrinum, and contained a large quantity of mercury.

Oxigenized lard and yellow nitrate will form unguentum citrinum.

Some consider the mercury as useless.

With respect to the virtues of this ointment, which some physicians assert are the same with those of lard simply oxigenized by nitric acid, I do not pretend to decide: but it is probable, that a substance containing mercury in actual combination must produce different effects from one that does not.

Neutral nitrate changed from white to yellow by lard;

Instead of the acid nitrate employed above, Mr. Vogel next took neutral nitrate at a minimum, reduced it to a fine powder, and projected it into heated lard. Bubbles were immediately produced, and the white powder of the nitrate was soon changed to a yellow. The lard acquired a solid consistence, and contained mercury in solution.

which takes up part of its acid.

The neutral nitrate then is decomposed by lard: not that the mercury parts with oxigen to it, for it is already at a minimum; but the nitric acid quits in part the oxide of mercury, and attacks the lard, by which it is decomposed; the result of which is yellow nitrate of mercury, which in fact contains but little nitric acid.

Other metallic salts.

With the nitrates of silver and lead, and the oximuriates of platina and mercury, very little decomposition takes place,

and

and they do not produce the same effects on lard as the nitrates of mercury.

General conclusions.

From the facts here adduced we may infer,

1. That light, without air, turns lard yellow, and gives it an acrid, rancid taste, without acidifying it.

2. That lard yields no ammonia by distillation, and contains no nitrogen, so that it may be considered as a purely vegetable substance.

3. That in the sulphur pomatum a portion of sulphur is dissolved, without being acidified.

4. That phosphorus dissolves in it, but is quickly changed into phosphorous acid, and its acidification is increased by the contact of air.

5. That lard oxigenized by long exposure to air constantly becomes acid. That the water with which it is washed precipitates some metallic solutions, and, if distilled, gives out at last acetic acid.

6. That nitric acid forms with lard a yellow bitter substance, acetic acid and mucous acid. That the latter cannot be completely separated from it by washing; and that it is equally obtainable from suet by means of nitric acid.

7. That the oxigenized muriatic acid is decomposed by lard, which it leaves whitish, very soft, and incapable of furnishing the yellow bitter matter, when subsequently treated with nitric acid.

8. That mercury is in the metallic state, but very minutely divided, in fresh prepared mercurial ointment.

9. That in the unguentum citrinum the mercury is a nitrate with oxide at a minimum: that the white appearance of the surface is owing to bubbles of gas: and that the neutral nitrate of mercury at a minimum is decomposed in lard.

VII.

Extract of a Memoir of Mr. HAQUET, *on the Formation of Flint**.

Flint of recent formation.

AMONG the varions proofs, which this gentleman has adduced of the recent formation of flint, we shall cite the following.

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* Journal des Mines, No. 119, p. 405. November, 1806.

In

Petrified wood found in it.

In the chalks of Volhynia there are a great number of flints, in the form of nodules more or less large. In one place were found two as big as a man's fist, enclosing petrified roots of wood. The author possesses one of these, the wood in which has not altered in colour, and appears to be beech. Another nodule found in the same place contains a number of splinters of wood.

Analysis of the chalk bed.

The chalk, from which these flints were taken, analysed by Mr. Haquet, gave in 100 parts, lime 47, magnesia 8, carbonic acid 33, silex 7, alumine 2, oxide of iron 0·5.

He has likewise analysed several flints from different places, and found them containing:

Analyses of flints.

Silex	92·75	92·50	92·75	97	89
Alumine	1·10		1·50	1	2
Lime	1·25	3	2·75	0·25	4·15
Magnesia			0·51		
Oxide of iron	2	1·25	1	1	1·75
Oxide of manganese		0·75			

Always near the surface.

Mr. Haquet observes, that flints are never found at any considerable depth; and the deeper we go, the smaller and more distant from each other are the nodules.

VIII.

Of the Oxidation of the Solder of Leaden Vessels used in Wash-houses; by J. C. Delametherie *.

Lead solder corroded in vessels used in washing.

An enlightened amateur of the arts, Mr. Fougeray de Launai, who lives near Soissons, informed me, that laundresses, who wash great quantities of linen, use for their bucking tubs large vessels made of several sheets of lead, soldered together with the common solder consisting of lead and tin. This solder suffers no damage, as long as the vessel is kept in use: but if it be left empty for a few months, the solder is so much oxided, that the lead must be soldered afresh, before it will hold lie again. I requested prof. Vauquelin to examine this solder, and the following is the result.

The lead con-

He found, that it consisted almost wholly of carbonated

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* Journal de Physique, Vol. LXIII, p. 252. September, 1806.

verted into a carbonate,

oxide of lead, with a few particles of iron and tin. From this, and the circumstances under which it appeared to have been formed, he concluded, that the metals forming the solder were oxided by the air through the influence of the alkali, and that at the same time the carbonate of potash had been decomposed; that is to say, the alkali had united with the tin, and the carbonic acid with the oxide of lead.

and the tin oxided and dissolved.

Oxides of tin and lead together decompose carbonate of potash.

This conjecture he verified by a direct experiment, taking nearly equal parts of oxide of tin and oxide of lead, and heating them slightly with a solution of carbonate of potash. The tin was dissolved, and the lead carbonated.

VII.

Example of a Calculation in the Doctrine of Chances; a Tide Table: and Remarks on the breaking of Waves. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

A Friend of mine, who has been spending the summer at a watering place, has proposed to me some questions, which arose out of the amusements of the season, but which it required some little consideration to answer in a satisfactory manner. If you think the results of my reflections likely to be interesting to your readers, they are much at your service.

What is the chance of winning a raffle by a given number thrown?

1. The first question was how to determine the chance of winning a raffle, when you have thrown a given number. It is usual to throw three times, with three dice; the highest, or sometimes the lowest, wins; and if two or more persons throw the same number, they must raffle again among themselves. We must first calculate the chance, that none of the persons who are to throw will throw higher; and then the chance, that, if they do not, any one of them will throw the same number, or any two, three, or more; but as this calculation cannot easily be made during the time of raffling, I have made a table which is sufficiently accurate for the purpose.

A Table

A Table of the Chances of winning a Raffle.

Number thrown.

Persons to throw	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48
1	1	2	3	5	7	10	15	19	25	32	39	46	54	61	68	75	81	86	90	93	95	97	98	99	99	..	..	..	..
2		(In 100)			1	1	2	4	6	10	15	21	29	38	47	56	65	73	80	86	90	94	96	93	..	..	..	..	..
3							..	1	2	3	6	10	16	23	32	42	52	63	72	80	86	91	94	97	98	99	..	..	..
4							..	..	..	1	2	5	8	14	28	32	42	53	65	74	82	88	92	95	97	..	..	..	..
5							..	..	..	..	1	2	5	9	16	24	34	46	58	68	78	85	90	94	..	..	..	..	..
6							..	..	..	..	..	1	2	5	11	18	27	39	52	64	74	82	89	93	96	98	99	..	..
7											..	..	1	3	7	13	22	33	47	59	70	80	87	92	95	..	..	..	..
8											..	..	..	2	5	10	18	29	42	55	67	77	85	91	..	..	..	..	..
9														1	3	7	14	24	38	50	63	75	84	90	94	97	..	..	..
10														..	2	6	12	21	34	47	60	72	82	89	..	..	..	..	..
11														..	..	4	9	18	30	44	57	70	80	88	93	..	98	..	..
12														..	1	3	7	15	27	40	54	68	79	87	92	96	..	99	..
13														..	..	2	6	13	24	38	52	66	77	86	..	..	..	..	..
14														..	..	..	5	11	22	35	49	63	76	85	91	..	..	..	..
15														..	..	1	4	10	20	32	47	61	74	84	..	95	..	..	..
16														..	..	..	3	8	18	30	44	59	73	83	90	..	..	..	..
17														..	..	..	2	7	16	28	42	58	71	82	89	..	..	..	..
18														..	..	..	..	6	14	26	40	56	70	81	..	94	..	..	..
19														..	..	..	..	5	13	24	38	54	68	80	88	..	..	..	..
20														..	..	..	1	4	12	22	36	53	67	70	..	..	..	..	..

Number thrown.

	37	38	39	40	41	42	43	44	45	46	47	48
21	4	10	21	34	51	66	78	87	93	97	99	..
22	3	9	19	33	49	64	77	86	..	..	..	..
23	..	8	18	31	47	63	76	..	..	96	..	..
24	2	..	16	30	46	62	75	85	91	..	..	..
25	..	7	15	28	44	61	74	..	..	..	..	..
26	..	6	14	27	43	59	73	84	..	..	..	..
27	1	5	13	25	42	58	..	..	90	..	..	..
28	..	..	12	24	40	57	72	83	..	..	..	..
29	..	4	11	23	39	56	71	..	..	95	..	99
30	..	..	10	22	38	55	70	82	89	..	..	..

To find the chance of losing, subtract the chance of winning from 100. Thus, having thrown 42, if there are five persons to throw, the chance of winning is 90, and that of losing 10; the odds being 9 to 1.

If the raffle is to be won by the lowest number, the chance may be found by subtracting the number thrown from 63, and consulting the table; thus if 21 are thrown, and there are 5 persons to throw, the chances are 90 in 100.

Universal tide table.

2. In the second place I was asked for the easiest mode of finding the time of high water, with sufficient accuracy for common purposes. I have made a table, which, I believe, is tolerably correct; but not being so conversant in the subject as I could wish, I should be much obliged to any of your correspondents skilled in navigation, if they would compare it with the best observations, and inform me whether they approve of the principle.

Waves break into surf from the upper part moving faster than the lower.

3. The third inquiry related to the cause of the breaking of a wave into surf. Waves seldom break at sea, unless the wind is very high: but when they approach the shore, they always break sooner or later. The general reason of their breaking appears to be the excess of the velocity with which the upper part of the wave advances above that of the lower part: and this may be derived either from the effect of the wind on the upper part, or from the resistance of the bottom to the motion of the lower part, or from a third cause, which is more general, where the magnitude of the wave is at all considerable in comparison with the depth of the fluid; for in this case the upper part of the wave must have a natural tendency to advance more rapidly than the lower, on account of the greater depth which determines its velocity. Beside this, the form of the wave itself, where the water is shallow, may be such as to render it incapable of advancing without a change of the direction of its anterior surface into a situation more nearly vertical.

Defect in the calculation of the velocity of waves.

In the calculations by which the velocity of waves has been determined, it has been usual to neglect not only the difference of the whole depth of the fluid at different parts of the wave's surface, but also the immediate effect of the horizontal motion of the particles, so far as it is not concerned in producing an elevation or depression by its variations. The theory, abstracted from these considerations, is perfectly correct; and may be combined with their results so as to be rendered applicable to some cases, which are not otherwise comprehended by it. Thus if we suppose a wave, terminated by two planes, equally inclined, to be placed in a surface on which it can move without resistance, it may be shown, that the highest point will begin to be flattened with the velocity deducible from the depth at that point, the new angular

angular point advancing on each side upon the inclined surface with a velocity which is at first equal to that which is due to half the depth, and is afterwards uniformly retarded; so that the angle is twice as long in travelling over the whole surface of the wave, as it would otherwise have been. The centre descends at first more rapidly than the part nearer the margin, so that the wave becomes concave in the middle, instead of being flat, as it would be if the depth of the fluid were very great. In the mean time the margins of the wave advance with a velocity, which continues to be uniformly accelerated, until the angle reaches it; and this velocity is as much smaller than that of a body falling by its weight, as the height of the wave is smaller than half the breadth: for the whole horizontal pressure acting on any vertical section of the wave is every where proportional to the quantity of the fluid beyond it, and as long as the deeper parts retain their form, they will urge forward the shallower with a constant force. But if any part of the surface of the wave be concave, the velocity thus produced in its upper parts will cause them to advance more rapidly than the lower, and the surface will become more and more inclined to the horizon: if on the contrary it be convex, the lower parts will be protruded, and the convexity will be diminished. Beside the case of a wave advancing in consequence of its gravitation on a flat shore, these considerations are also applicable to that of a drop of oil, spreading, by the force of cohesion, on the surface of a vessel of water.

These reasonings applicable to a drop of oil spreading on water.

I am, Sir,
Your very obedient servant,
19 *Sept.* 1807. HYDROPHILUS.

ANNOTATION.

MY correspondent not having gone at large into the use and application of the tide table annexed, Plate IV, a little more particular detail on the subject will probably be acceptable to many of my readers. The small shaded circle in fig. 1 being cut out, a damp wafer is to be put in its place, and over it fig. 2; which is likewise to be cut out, and

Manner of fitting up and using the tide table.

and so adjusted, that its dotted circle shall coincide with that of the same size in fig. 1. Underneath the wafer a circular piece of paper, about the same diameter as fig. 2, is to be placed, so that fig. 2, may move freely round on its centre. The oblong shaded space in fig. 2 is to be cut out previously, so that the name of the place, for which we want to find the time of highwater, may appear through it. The table being thus put together, we have only to turn round the smaller circle, fig. 2, till the name of the place, for which we would know the time of highwater, is seen through the aperture; when the time for any given day will be shown by the part of the hour-circle, fig. 2, that stands against the line of the moon's age on that day. Thus for example, if I want to adjust the table to Worthing, having turned the circle till Worthing appears through it, I shall find, that, when the moon is two days old, it will be high water there about five minutes before 12; when the moon is nine days old, about a quarter after 6; and so for any other day.

The difference of the length of the lines in the larger circle points out the progressive increase and decrease of the rise of the tide; showing its comparative height, from the spring tide, when it is the greatest, being about 36 hours after new or full moon, at every place, to the neap, when it is the least. Hence, if we know what is the general rise of the spring tides at any place, we may calculate how high the tide may be expected to flow at any given time of the moon's age.

Table to show the moon's age.

As it is obviously essential for finding the time of high water to know the moon's age, a general table of lunations is annexed. The use of it being sufficiently explained in the table itself, all that is necessary for me to say is, that the small shaded circle of fig. 4 is to be cut out, and fig. 3 to be cut out and put over it in the same manner as fig. 2 over fig. 1.

For the gratification of such of my readers as might wish to have these tables in a form for use, without taking the trouble of copying them, or destroying the plate, I have given a duplicate of it, which may be cut out and pasted on a card.

Hydrophilus will no doubt perceive, that I have taken the liberty

Universal Tide Table.

Fig. 1.

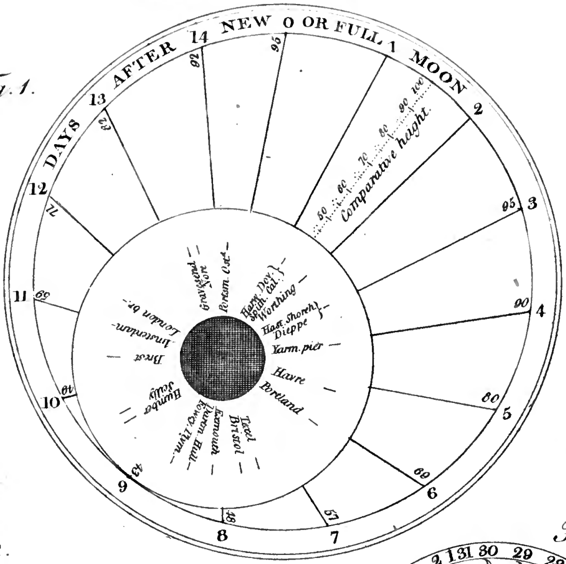


Fig. 2.

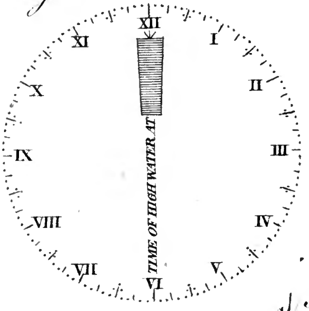
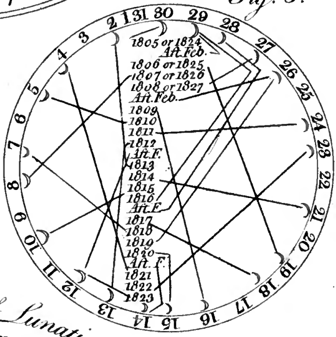


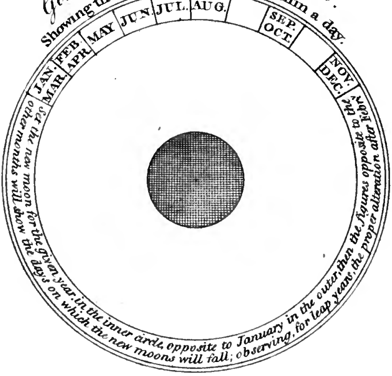
Fig. 3.



General Table of Lunations.

Showing the time of new moon within a day.

Fig. 4.



Set the new moon for the given year in the inner circle, opposite to January in the outer, then the figures opposite to the other months will shew the days on which the new moons will fall; observing, for leap years, the proper alteration after Feby.

liberty of making a slight alteration in the title of one of his tables. If the reason of this should not readily suggest itself to him, it will be explained in a private letter, on his favouring me with his address.

X.

Nondescript Encrinus, in Mr. DONOVAN'S *Museum.*

SIR,

Mr. Donovan's Museum.

PERMIT me through your excellent publication, to acknowledge my obligations to Mr. Donovan, for the advantages I have derived in my inquiries respecting the mineralized remains of the animals of the former world, from the examination of the inestimable fossils, contained in his matchless museum.

Several species of encrinites found in England.

By the investigations which I had previously made, and from specimens in my own collection, I had ascertained, that England alone yielded several species of *encrinites;* as I trust I shall show in the second volume of Organic Remains of the former World, now in the press. But by an examination of the series of fossils in this department of the London Museum as above mentioned, I have gained the knowledge, that our own country can boast of yielding at least one additional curious species of this animal, hitherto I believe unknown; and forming by the length of the arms an intermediate species between the lily and plumose encrinus. The specimen of which I speak is numbered 924 in the brief catalogue which is delivered at the museum.

New species.

Tortoise encrinus.

From another specimen in the same collection, marked 950, I also acknowledge having derived very considerable information respecting the structure of that wonderful lost animal, the *tortoise encrinus.*

Having no reason for concealing any of the motives which induce me to trouble you with this request, I do not hesitate to avow, that one of these is a wish to call the attention of the curious, as well as scientific, to the most complete collection of British Natural History, which has ever yet been

been formed; a museum not confined to any one particular branch, but comprehending alike the three great departments of nature, the zoological, botanical, and mineral productions of the island, upon the grandest scale possible. It will not be too much to say, that this museum, from the science evinced in its arrangement, independent of its importance as a collection of choice and valuable specimens, must, to those desirous of such knowledge, prove a most instructive school; and afford an inexhaustible fund of information to all those, who think the Natural History of their own country worth attending to.

I am, SIR,
Your most obedient servant,
JAMES PARKINSON,
Hoxton Square,
Author of "Organic Remains,"
"Memoranda Chemica," &c.

Mr. NICHOLSON.
Sept. 6th, 1807.

XI.

Inquiry respecting a Fact not hitherto noticed in the way of discussion. In a Letter from R. B.

To. Mr. NICHOLSON.

SIR,

Sediment of water thrown into undulation arranges itself in parallel ridges.

WHEN turbid water has become clear by subsidence, the light stratum of earthy matter, which covers the bottom of the vessel, is often, as might be expected, distributed without any particular appearance of symmetry: but if the vessel be slightly moved horizontally, so as to produce an undulation of the fluid, without much disturbing the deposit, this matter is found to arrange itself in a number of parallel ridges or embankments; or at least to indicate a manifest tendency to form such ridges. I find a great difficulty in accounting for this fact. It seems to suppose alternate differences in the velocity of the water, as it runs over the bottom. These, however, seem incompatible with the almost total want of elasticity in water.

Why?

Perhaps

Perhaps the hypothesis of a number of small eddies, or rolling cylinders of water, may account for it, as the ridges are at right angles to the course of the undulation. In this supposition, however, there appears too much of gratuitous demand.—Have the goodness to propose the matter to the consideration of your correspondents, if you should think as I do, that no fact can be too trifling for philosophical discussion.

I am, SIR,
Your obliged reader,
R. B.

XII.

Questions on some Appearances of the Electric Spark: by a Correspondent.

SIR,

Question respecting various appearances of the electric spark.

I Wish to be informed if it was ever remarked, that, when an electrical spark is taken from the conductor of a machine, the line of white light is interrupted, and the spark becomes red. Sometimes it assumes the form of two cones, one proceding from the conductor, and the other from the body which is applied to it: at other times the interruption is next the conductor; and again it will be perceived nearest the body receiving the spark. When the spark is taken at the least distance possible, the light is sometimes red, sometimes white; and when the spark is some inches in length, the interruption is perceived in two or three places of the line of white light. Any explanation of these phenomena will much oblige,

SIR,
Your humble servant,
TYRO.

XIII.

Extract of a Letter from Mr. BIOT *to Mr. Berthollet*[*].

Tarragona, 20th December, 1806.

I Have had an opportunity of conversing with that excellent observer, Mr. de Marty, on several subjects of experiments

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[*] Annales de Chimie, Vol. LXI, p. 271, March, 1807.

ments in which he has long been engaged; and I have requested his permission to communicate to you the results, persuaded that you will consider them extremely interesting.

Influence of time in diminishing the elasticity of fluids.

The experiments of which I speak have for their object in the first place the influence of time on the exercise of chemical actions, when these actions tend to deprive an elastic fluid of its elasticity.

Oxigen gas absorbed by water.

1. Into a flint glass phial, the stopple of which was ground with emery, and fitted perfectly tight, Mr. de Marty introduced a certain quantity of oxigen gas, and a certain quantity of rain water, boiled or unboiled. Supposing there is but a small quantity of water, on shaking the bottle for a few minutes a certain portion of the gas will be absorbed, as may be found by opening it under water. After it has been thus shaken and opened several times, the water in the phial will be saturated, and absorb no more.

Left standing on it in a close vessel more will be absorbed.

When things are in this state stop the bottle close, and put it away in a place shaded from the sun, observing at the same time the state of the barometer and thermometer. After it has stood thus two or three days, shake the bottle again, open it under water, and you will find the water rise into it a little. Stop the bottle, put it into its place, and shake it in the same manner from time to time. You will always find a fresh quantity of gas absorbed, and the effect will be the more perceptible, the longer you leave the phial before you make the experiment.

In 18 months half the bulk of the water absorbed.

I myself was witness to these effects. Mr. de Marty had the civility, to open under water before me a phial, that had been kept stopped for more than a year and half, and which contained oxigen gas with a small quantity of water. The water rose in it very perceptibly, and the absorption appeared to me equal at least to half the quantity of the water, that the bottle contained before it was opened. The barometer and thermometer were both nearly at the same height as when the bottle had been set by, and the water in the trough was at the same temperature.

At first the gas combines feebly;

From this experiment it appears, that the same volume of water, which at first was able to absorb only a certain portion of oxigen, absorbed by the assistance of time a more considerable

considerable quantity. Hence it would seem, that in the first case the air was but feebly combined, and in some sort interposed between the particles of water: but the continued action of the liquid, diminishing the elasticity of the gas more and more, and contracting its dimensions as it were by degrees, occasioned it to enter farther within the sphere of attraction of its particles, which rendered the water capable of absorbing a fresh quantity of gas.

but its elasticity diminishing, the union is more intimate.

2. The same thing takes place with regard to hidrogen gas, and Mr. de Marty afforded me the pleasure of witnessing this likewise. The absorption was equally great. He finds by his experiments, that this gas is absorbed in larger quantity and with greater promptitude than oxigen gas. He finds also, that the bulk of the gas absorbed is not equal to that of the water in two years.

Hidrogen gas affected in the same way, but water absorbs more, and in less time.

3. Water already loaded with oxigen is better adapted to absorb hidrogen, and the contrary. This is analogous to what von Humboldt and Gay-Lussac have observed, but the experiment of Mr. de Marty has the advantage, like the preceding, of having been made in close vessels.

Water loaded with the one absorbs the other more readily.

4. The absorption is so much the more sensible as the water is more considerable, and is proportional to it.

Proportional to the quantity of water.

5. These effects do not take place with nitrogen gas. After the water has been once shaken for some time with this gas, it will not absorb an atom more, however long it be left in contact with it.

Only a limited portion of azote absorbed.

6. If water loaded with nitrogen be placed in contact with hidrogen or oxigen gas, it will absorb it, without parting with its hidrogen. If it have been supposed, that an exchange takes place, it is because in fact a little nitrogen escapes at the commencement of the absorption of the hidrogen or oxigen: but on shaking the water and the gasses together, all the nitrogen, that was before interposed between the particles of the water, will enter into it again as before, independent of the hidrogen or oxigen absorbed.

Water saturated with azote still takes up hidrogen or oxigen; without ceding its place.

7. The preceding result is so true, that an accurate analysis of atmospheric air may thus be made by the absorbent action of water alone. To effect this it is sufficient, that the water be previously impregnated with nitrogen; when it will absorb exactly 21 hundredths of the volume of the atmospheric

Thus atmospheric air may be analysed by water.

mospheric air in contact with it, precisely as a sulphuret would do. Mr. de Marty asserts, that water thus employed in large quantity, to prevent the process from being too tedious, is an excellent eudiometer, and he has had recourse to it repeatedly. If you have no nitrogen at hand, water may be impregnated with this gas by shaking it in contact with atmospheric air, and leaving it some time in contact with it. By these means it absorbs all the nitrogen it can contain, and the oxigen it takes up with it does not prevent it from absorbing in time, according to the first experiment, that of the air to be analysed. Mr. de Marty avails himself of this absorbing property of water to ascertain whether oxigen gas contain any nitrogen; for, if it do, water saturated with nitrogen will not absorb the whole.

8 It is long since Mr. de Marty was acquainted with many of these facts. Some of them, particularly 6 and 7, were known to him, when he composed his Memoir on Eudiometry; but he contented himself with simply mentioning the property he had observed in nitrogen.

Does the oxigen form an acid?

Does this oxigen, continuing to be absorbed, form at length an acid? and if so, what acid is it? The solution of this problem Mr. de Marty awaits from time and experience.

The experiments were made with great care.

With respect to the preceding experiments I shall add, that they were all made with the greatest care, in vessels well closed; that Mr. de Marty has repeated and varied them in a thousand manners; and that he appears to have observed the most scrupulous accuracy in all.

Mr. de Marty's memoir on eudiometry.

I shall conclude this letter with some remarks respecting the Memoir on Eudiometry formerly published by Mr. de Marty, of which I have a copy before me in the Spanish language, in the *Memoriale Literario* for 1795; and of which there is an abstract in the Journal de Physique, year 9. In this abstract, however, many experiments have been omitted; the connexion and detail of which were indispensably necessary to understand the course of the author, and the conclusions at which he arrived: so that in consequence of this omission opinions have been ascribed to Mr. de Marty contrary to those he held, and results the reverse of those he sought to establish.

For

Mistaken by Berthollet, and others.

For instance, in your Chemical Statics you seem to insinuate, that Mr. de Marty ascribes to sulphurets the property of absorbing nitrogen from their nature; and von Humboldt and Gay-Lussac in their work on Eudiometry express this opinion still more affirmatively. This errour arose from the abstract. Mr. de Marty says expressly in his paper, that a hot sulphuret acts as any other liquid would do, that had been deprived of the quantity of nitrogen it is naturally capable of absorbing; and thus he accounted for the variations he experienced on operating with greater or smaller quantities of sulphuret.

Sulphurets have no affinity for nitrogen.

Proportion of oxigen in the atmosphere.

Von Humboldt and Gay-Lussac say too, that Mr. de Marty fixes the proportion of oxigen in the atmosphere to be between ·21 and ·23; and hence they draw an argument against the method of operating with sulphurets, which in consequence appears to them much less accurate than the proof by hidrogen gas. But this uncertainty between ·21 and ·23 Mr. de Marty experienced only in his first experiments: and it was from this very variation, and a wish to diminish its extent, that he was led to discover the errour occasioned by the absorption of nitrogen by the sulphuret, when it is deprived of this gas by heat; so that when he had completed the improvement of his method, the results were constantly restricted between ·21 and ·22.

As a test of it, hidrogen not superior to a liquid sulphuret in requiring less time, being more simple, or giving a more accurate result.

This being once thoroughly proved, I do not see what advantage the analysis by hidrogen gas has over that by the sulphuret, when it is performed with due precaution, which should always be a matter of course. It certainly has not the advantage of requiring less time; for by operating as Mr. de Marty does, any one who has acquired a little habit of making the experiment can perform this in five minutes. It has not that of greater simplicity: for the sulphuret requires only a graduated tube, and a ground stopple bottle; while for the hidrogen gas at least a small eudiometer and an electrophorus are necessary; and what is very inconvenient, the latter must be kept in a state to give sparks, which is not very easy on mountains and when travelling, particularly if the air be loaded with moisture. Lastly I will add, that it has not the advantage of superior accuracy: for when it is once proved, that the sulphuret will absorb only

only a given quantity of nitrogen as a liquid, and that, if it be taken thus saturated, it constantly gives the precise proportion of ·21 in close vessels, no objection can be brought against its use; while a very strong one may be urged against the use of the electric spark, since either boiled water is employed, and then it will absorb a small portion of the oxigen very greedily, or the water will be saturated with air, and then the pressure occasioned by the detonation will always force out some bubbles. One or other of these inconveniences appears to me unavoidable; and the latter in particular often teased me, in the numerous experiments I had occasion to make on the analysis of gasses by the electric spark, either with Thenard, or alone, on the Alps. It is true the differences hence arising, when we operate with care, amount only to some thousandth parts: but it is of thousandth parts we are speaking; and if there be another process, which gives at least equal if not superior accuracy, with less trouble, it appears to me to deserve the preference.

But objectionable, as boiled water absorbs some of the oxigen,

and unboiled gives out air.

Finally, to return to the memoir of Mr. de Marty, I shall add, that he has equally tried the air contained in theatres and in churches, when a great concourse of persons were assembled in them, and that he constantly found the same quantity of oxigen; an experiment which von Humboldt and Gay-Lussac made likewise at Paris.

Air in crowded churches and theatres not deficient in oxigen.

XIV.

Summary Considerations on the Prismatic Colours of Bodies reduced to thin Pellicles; with an Explanation of the Colours of Annealed Steel, and those of the Peacock's Feathers. A Fragment of a Work on Colours: by C. A. Prieur*.

CERTAIN extremely thin substances, the thickness of which varies progressively from one part of them to another, exhibit, as is well known, a series of colours of different tints,

Thin substances exhibit prismatic colours.

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* Annales de Chimie, Vol. LXI, p. 154, Feb. 1807.

tints, sometimes very brilliant. It is not my intention here to describe these, still less to dispute the particulars so admirably described by Newton. I shall only attempt to draw some conclusions respecting the origin of these colours, to establish a comparison with those arising from absorption, and to assign the true cause of some phenomena hitherto differently explained.

The principal effects, to which it is of importance for these purposes to call the attention, are the following.

When light falls on these

When the light falls on very thin bodies, that exhibit the prismatic colours:

it is partly reflected, partly transmitted.

1. At the places where these colours arise on the thin substance, each pencil of rays, or if you please the white light, is separated into two portions in a variable manner, and one of these portions is reflected, while the other can issue from the substance only by transmission.

This depends on its thinness, density, and inclination to the ray.

2. This division of the pencil varies according to a certain law, which depends on the thickness of the body, its density, and the inclination of the luminous rays.

Each ray has alternate fits of easy reflection and transmission.

3. Each ray in particular comports itself, as if it possessed the singular property of having fits of easy reflection at periodical intervals, and fits of easy transmission at other intervals alternating with the former. These various results are equally indisputable.

Newton supposes the cause of this inherent in the ray itself, in one place:

But whence can this disposition of the rays arise? Newton has considered it as inherent in the rays themselves, not only in that part of their passage comprised between the two extreme surfaces of a body that they traverse, but throughout the whole course of these rays, from the moment they begin to issue from a luminous body*. This is a kind of occult cause, of which it is difficult to form a clear idea; and accordingly some distinguished philosophers have shown great hesitation to admit it.

in another refers it to the same cause as its reflection.

But Newton himself, at the end of his work, puts us into the right road in a more happy manner, when he asks, whether it be not by virtue of the same principle, that the rays are reflected and refracted by bodies, and inflected in their vicinity †.

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* Opt. lib 2, part 3, prop. 13.

† Ib. lib. 3, quæst. 4.

It

It is much to be regretted, that this great man did not treat the subject of inflexion as extensively as he did that of coloured rings; or even that he did not attend to the deviation of light in the vicinity of bodies, before he examined its changes of direction by the action of their surfaces: as unquestionably he would have deduced new and very valuable consequences from it.

A great analogy between these phenomena.

In fact the greatest analogy subsists between the phenomena of inflexion round a minute body, and those of reflexion or transmission by thin substances: for the coloured fringes in one case appear to follow the same law as the coloured rings in the other. And if this be not very sensible with respect to the fringes adjacent to the shadow of a body of small diameter received into a dark chamber, it is more evident in the fringes produced by the light that passes between two bodies very near together; it is still more in the series of coloured images formed between the plumes of a feather, when looking at a candle through them; and it is very manifest likewise in the bars seen by the eye, when a piece of linen, or a series of wires very near together, is placed between it and the light, as in the experiments of Mr. Rittenhouse.

Coloured fringes and coloured rings follow the same law.

Method of rendering this more obvious.

I have found a method of rendering this resemblance still more conspicuous. For this purpose I employ black crape. If the eye be covered thus, and from a dark place you look at a light a little distant, you will perceive the light surrounded by a series of very apparent rings, the colours of which are very vivid, and of the same tints as those of the coloured rings of thin plates.

Candle seen through vapour.

If the flame of a candle be placed in the midst of a pretty abundant aqueous smoke, or so that it can be seen only through this smoke, the flame will appear surrounded by perfectly analogous rings. I can imitate them likewise very conveniently by tarnishing a glass with breathing on it, and immediately looking at the image of a luminous body either through it or reflected from it. Those rings, which are sometimes seen surrounding the sun or moon very closely, are probably phenomena of the same kind.

Newton speaks of serpentine undulations.

On the other hand Newton speaks of undulations like those of an eel, which he suspects are produced in the rays, when

when they pass very near bodies*. The formation of these, and the necessity of their existence, I think I can render sensible.

How these are produced.

With this view I would call the attention to the very interesting results of the experiments of Newton and s'Gravesande relative to inflexion: results so certain, that no one certainly will attempt to question them, but which it is notwithstanding satisfactory to be able to verify ourselves, and to observe with all their peculiarities, as I had the advantage of doing in experiments on the same subject made at Mr. Tremery's, in concert with Messrs. Berthollet, and the particulars of which I have given in a preceding part of my work.

Each particle of matter surrounded by an attractive sphere interiorly, and a repulsive sphere exteriorly.

From the action which a point, or the edge of any body, exerts on the luminous rays, it seems to me we are authorized to consider each molecule, or distinct parcel of matter, as enveloped with a double sphere of activity in respect to light: one more interior, in which the rays are attracted by the body; the other more exterior, in which the rays are repelled. Now it will happen, that, in several positions, a ray, coming to traverse the repellent sphere, will describe there a curve convex toward the body; that, if it afterward penetrate the attractive sphere, the curve of deviation will be concave toward the body; and that it will a second time become convex toward it, when the ray repasses into the sphere of repulsion, to continue its course. Here we have the commencement of an undulatory motion, the curves of which may be multiplied by a series of molecules.

Is this the cause of the fits of easy reflection & transmission?

Would this cause be sufficient, to effect the fits of easy reflection and transmission of the rays directed to the surface of a body?

Probably it is.

The phenomena of colouration here considered appear to me, to be very naturally explained by this simple mean: yet I merely announce it as a probability. To leave nothing to be desired in such a proposition, no doubt more profound investigation is necessary; as well as in particular to put it to the test of calculation, in order to see, whether it be possible by the attractive and repulsive powers ascribed

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* Opt. lib. 3, quæst. 3.

to each molecule of a body, in a case given to deduce the motion of the luminous rays repelled or impelled now in one direction, now in another, conformably to the reflections or transmissions produced by pellicles.

The same colours produced between bodies in a vacuum.

It is of more importance to my object however to remark, that the colours arising from fits of easy reflection and transmission are equally produced, as Mazéas very justly observed, between the surfaces of two bodies brought near together, without the interposition of any matter, as in two lenses, or two pieces of glass, applied to each other in the vacuum of an airpump.

And in thick plates.

On the other hand these colours do not always require a very small distance of the surfaces, since Newton himself obtained coloured rings by the action of two surfaces of a concave glass mirror three lines thick; and found, that in thick plates these rings depend on the ratio of the thickness, according to the same law as he had determined with respect to thin plates; which he confirmed by the observation of the rings of a mirror only one line thick.

Colours of pellicles independant of the substance itself;

and of its thinness.

Analogous to the rings in vapour, and between opake substances.

And may be traced to the action of particles of matter on light,

without any relation to their nature.

We see then by comparing the various phenomena I have mentioned, that the prismatic colours of a pellicle, or a thin plate of glass, are as fugitive and independant of the proper colour of the substance, as those of a thick piece of glass: that those colours even may not depend on the thickness in any respect, as when they arise in the interval between two glasses brought together, or in the fissures of certain minerals: that they have the greatest analogy with the rings produced in a mist, in smoke, or in the intervals of threads impermeable themselves to light: and finally, that, if we trace it up to the action of a point, or a single particle of matter, on the luminous fluid, we shall there find a very probable origin of the modifications of the direction of the rays, that are deflected by the particles of bodies in the different instances quoted, and which, being differently influenced each according to its nature, ultimately escape in a different direction. Hence results a variety of colours on these bodies, determined solely by the number or distance of their particles, without any relation to their nature.

Colours of bo-

Let us now proceed to establish a parallel between these sorts

sorts of colours, and those of the particles of bodies subjected to the laws of absorption.

dies that absorb light.

In the first place with respect to the latter the luminous pencil is not divided as in the former. The rays that do not reappear in a given direction are not thrown into another direction; they remain absorbed in the substance, even when the mass is perfectly transparent.

The pencil not divided here.

In the next place the colours resulting from absorption are sometimes owing to groups of rays very different from those that thin pellicles can furnish. For instance, these never produce a compound colour like that of bodies tinged violet by oxide of manganese, or like the blue of cobalt or of indigo. Besides in these two kinds of phenomena there is no relation between the progress of colour depending on the degree of thickness.

The colours differ from those of pellicles.

Thirdly the colours of the thinnest pellicles are very vivid. Those of the most intensely coloured solutions on the contrary are imperceptible when so thin. It is for this reason the colour of extremely thin leaves of mica has no relation to the yellow or other colour of the mass from which they are separated; they resembling pieces of the most colourless glass of similar thinness, so that mixed together they would not be distinguishable.

Are imperceptible when the body is thin.

Thus glass, mica, or any other substance, which when very thin is invested with the most brilliant colours, passes to a colourless state by increasing its thickness, or to a colour independant of that displayed by it when thin.

Substances coloured when thin, colourless when thicker.

But it may be said, to compare a coloured mass to an assemblage of parcels of a determinate thickness, these parcels must be kept at a suitable distance from each other.

Molecules of a given thickness, kept at a given distance,

In this case, I should answer, you will have a certain colour reflected, and another transmitted, which is precisely complementary to the former. Now this double colouration never takes place in perfectly diaphanous substances.

would reflect one colour, and transmit its opposite.

The examples of the infusion of nephritic wood and precipitates of gold are not more applicable to this case, since, as I have shown, the reflected colours are owing to particles impermeable to light, and disseminated in a transparent fluid; and we may alter the nature of these particles, or even have

Infusion of nephritic wood & precipitates of gold, opake particles in a transparent fluid.

have others, so as to change the reflected colour, without any alteration in the transmitted colour.

Hitherto there is no case known, that allows us any foundation to consider a body that is perfectly transparent, or even a little turbid, as composed of parcels of a given thickness, and kept at a necessary distance, in order to produce a colour dependant on the thickness of its elementary parts.

Colours of pellicles varied by the inclination of the rays, and by the adjacent medium.

Lastly the colours of pellicles are in certain cases variable by the inclination of the light and of the eye, and sometimes too by the influence of the mediums with which they are in contact. Nothing similar to this takes place in the colours proper to the particles of bodies; for these are fixed and permanent in whatever direction we look at them, and are equally unchangeable by immersion in a different fluid medium of less or greater density.

Permanent colours therefore have a different cause.

These characteristic differences I conceive are sufficient to authorize the opinion, that the colours of substances in masses have not the same origin as those of thin pellicles; a conclusion as important with respect to its object, as to the difference of opinion that still subsists on it among the learned *.

I shall conclude with some observations on two curious kinds of phenomena, analogous to the subject, which I think I have sufficient grounds to explain in a manner different from that generally admitted.

Colours of heated steel, referred to the same cause by Newton;

The first relates chiefly to the colours of annealed steel. Newton has ranked these among those that depend on coloured rings; not from a particular examination, but simply as a consequence of the system he had formed, supposing that the magnitude of the metallic particles must have been altered by the action of the fire. He did not consider whether there were any other causes, between which a choice was to be made.

to oxigenation by the moderns.

More modern philosophers on the contrary have ascribed these colours without any hesitation to a different degree of oxidation,

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* See among others in the 2d edition of Berthollet's Elements of Dyeing, and the 2d edition of Haüy's Treatise on Natural Philosophy, the discussions and opposite opinions of these celebrated authors on this question.

oxidation, because they have supposed they observed a great similarity between the appearances in question, and those of several metals placed in circumstances under which they are actually oxided. This subject however deserves at least a closer examination, and the following is the simple method I have pursued.

A steel watchspring heated by the flame of a candle acquired the prismatic colours apparently in concentric rings.

I held a steel watchspring across the flame of a candle a few seconds in a fixed position. After it was cold and cleaned, I found both on the right and left of the central point, where the flame had been, a series of colours more and more faint [*degradées*], with periodical recurrences, such as would have been exhibited by a small band cut precisely from the middle of a circle formed of a series of concentric coloured rings. The nature of the phenomenon then is very distinctly shown here, particularly as the exterior ring was nearly 3 cent. [11·7 lines] in diameter, and the others decreased interiorly with intervals of a few millimetres. Nothing was wanting to have completed the circles, but to have operated on a broad plate of steel suspended horizontally over the point of the flame.

A sheet of tin shows this more plainly.

Not having such a plate at hand, I took a sheet of tin, which, with appearances analogous to the preceding, afforded very vivid colours in consequence of its natural whiteness. With a proper degree of inclination the colours are most lively; particularly the yellow, red, and blue, which form together a spot, in which the blue occupies the centre, surrounded by the red, and beyond this with the yellow, with the intermediate tints and gradations.

Not from oxidation.

There is nothing in the property tin has of being oxided and forming salts, that indicates colours corresponding with these: on the other hand the periodical recurrences on the steel spring evidently belongs to a series of rings: we must conclude therefore, that this phenomenon is simply of the class of coloured rings.

Gold the same.

Another trial made with a gold ring equally produced repeated traces of rings, and here the suspicion of oxidation will hold still less.

Copper and lead similar.

A copper wire gave me similar indications, though more faintly; but I have observed them very striking on copper chimneys of stoves. Lead that has just been melted exhibits

bits the same kind of colours, according to the circumstances of its cooling, even on its lower surface not exposed to the air.

Caloric has not altered the dimensions of the particles,

With regard to the manner in which caloric acts on the metal in these instances, I will not say, that it has altered the magnitude of the particles; for how can we conceive, that a substance can vary the disposition of the constituent elements of its molecules without changing its nature? But I can more readily conceive, that there has been a progressive separation of the particles, increasing from the part scarcely heated to that in immediate contact with the flame. This separation, from the principles I have laid down, must in fact have been sufficient to produce these rings.

but separated them progressively.

Similar effects from altering the arrangement of the particles of bodies.

Besides, we daily see many examples of this sort of colours, where the arrangement of the particles appears to be the sole determining cause. Such are the spots formed on knife blades by the acid on fruit; those on silver by sulphurous vapours, or the continued contact of certain substances; and the prismatic colours of pellicles formed on the surface of liquids containing some matter at first dissolved, and afterward precipitated slowly by the gradual evaporation of some volatile principle, seen in manufactories and laboratories. The waters of dunghills are sometimes covered with similar colours.

Proof of this.

Now all these effects, whatever be the nature of the primitive matter, or of that secondarily formed, depends only on the arrangement of the parts. Nothing is more easy than to ascertain this. Scratch the surfaces, break the pellicles, and all this multitude of colours will be annihilated, the fragments of each leaving only particles uniformly possessing the common properties of their kind.

These effects shown in a striking manner.

The following among others is a very convenient mode of observing these effects. Take a small quantity of Scheele's green, dissolve it in an acid, and, after having largely diluted the solution with water, precipitate by an alkali, and add ammonia merely to redissolve the precipitate. Let the whole stand in a vessel not closed, and in a few days the surface will be covered with a very evident coloured pellicle, in which periodical recurrences of rings will be distinguished, if it have remained undisturbed. This pellicle may be taken up up by sliding underneath it a piece of paper, or of glass, as the side of a funnel for instance, to allow the water to run off. The colours of the pellicle will continue visible after it is removed; and by letting the matter dry slowly we may preserve them for an indefinite time with all their vividness. But if we pass the finger lightly over it, we shall collect nothing but a green powder: the red, yellow, blue, and purple, that appeared so brilliant, will be gone in an instant.

Changeable colours of feathers.

My second phenomenon respects the changeable colour of several parts of the plumage of the peacock, and of some other birds, as the cock, pigeon, duck, and turkey. Here, after a very deliberate comparative examination, and the most circumspect reflection, I have ultimately relinquished the idea, that these colours are to be referred to rings. My conviction was produced as follows.

Not owing to tenuity.

In the first place I considered, that these colours were not the necessary result of a certain tenuity of parts; for on the one hand several animals incontestably display in the slender filaments of their hair, feathers, or down, various degrees of tenuity, from the most imperceptible, without its producing colour. Is not the white peacock itself a striking instance of this?

All opake except the wings of flies.

On the other hand have not many birds and insects colours unchangeable in situation and in their reflections in every point of view? Those of the wings of some butterflies are perfectly fixed, though dependant on a down so fine as to be scarcely visible. It is likewise proper to remark, that all these colours indicate opacity, as those of the peacock's feathers; the wings of flies excepted, in which tints analogous to coloured rings are observed: but these membranes have a sensible transparency, like scales of mica or blown glass.

Feathers of the peacock's tail.

Red by perpendicular, green by oblique reflection.

I afterward observed the change of colour of several feathers of the birds mentioned. In those of the peacock's tail, we see the lateral filaments, on changing their position, pass suddenly from red to green. The red is produced by a nearly perpendicular reflection of light, the green by a very oblique reflection; and there is no alternation of reflection

flection and transmission, the opacity of which I have spoken not admitting it.

Eye of the feather.

Near the eye of the feather an external ring exhibits yellowish tints by perpendicular reflection, and greenish by oblique reflection; while more interiorly, with the same change of obliqueness, a space of the most vivid green takes the new tint of violet. These are the principal mutations of the colours, consisting only in two tints for each place.

Pigeon's neck the reverse of the peacock's tail.

In a feather from the neck of a pigeon, the disposition is the reverse of that of the lateral filaments of the peacock's tail feathers: that is to say, under similar circumstances, one of these feathers appears red, the other green, and *vice versa.*

The variation confined to two colours.

This alternation of colours, confined to two principal kinds, is very difficult to reconcile with that variety of tints, which coloured rings apparently should exhibit in a substance of so little density as feathers. And if it were attempted to be supported by the more sensible changeability of tints in the pigeon's feather, this would arise from a deception; for this changeability is owing to the naturally curved state of the feather, since it ceases when the feather is straightened against a flat surface.

Feather of a duck's wing.

But the feather of a duck's wing exhibits an appearance totally dissimilar. Here the transition is from green to blackish; and this green is not perceptible except in particular positions, in which the incidence and reflection of light take place under very unequal angles, as for instance, when the feather is seen with a certain degree of obliquity, the spectator's back being turned to the light. Do we meet with any thing like this in the succession of coloured rings?

Eye of the peacock's feather wetted, showed new colours,

Lastly I bethought myself of wetting with caution different parts of the eye of my peacock's feather. I then saw, not a weakening of the former tints, but new colours brought out with great vividness. Desirous of knowing

which were the same with various fluids,

whether I could not produce permanent alterations by some solvent, I wetted it successively with saliva, vinegar, muriatic acid both dilute and concentrated, ammonia, ether, alcohol, and deliquesced muriate of lime: and I found, that they produced no effect except as fluids, and all nearly equal,

equal, except the concentrated muriatic acid, which showed some difference; and all their effects ceased nearly alike on drying.

and disappeared on drying.

When the exterior orbit of the eye was wetted, its yellowish colour became of a lively blood red; and the reflection, which was before green when seen obliquely, was almost destroyed. If the green space within were wetted, it was the violet reflection that disappeared. Lastly with smoking muriatic acid this green space viewed perpendicularly exhibited a yellow inclining strongly to red, and the oblique reflection changed at first to green, and then it passed on to violet: none of these changes however continued permanent.

The colours thus produced.

By wetting in this manner the extremity of the feathers of a turkey's tail, I brought out very vivid new colours, which could not be perceived before in the same direction; but the existence of which was indicated by certain reflections when viewed with the back to the light, analogous to those I mentioned in speaking of the duck's feathers.

Feathers of a turkey's tail.

It was impossible for me, after all these particularities, to persist in classing together the changing colours of feathers, and those of the coloured rings of pellicles. An often repeated examination of them at length suggested the idea, that they might arise from the superposition of different coloured substances, sometimes of two only, or of three, or of a greater number; nearly as if, wishing to paint a body with several colours, we were to cover it in succession with a stratum of each of the proposed ingredients.

Perhaps from the superposition of different coloured substances.

This supposition, suitably adapted to each part of the feathers, very well accounts for all the appearances observed. In fact, if over a coat of green paint for instance we spread a thin stratum of a violet powder, it is obvious, that on looking directly at it it will appear almost wholly green; while on lowering the eye, so as to view it obliquely, the violet will gradually predominate, till at length it alone is seen. The intermediate tints will be different shades of green, to which various shades of blue will succeed, before the violet will appear.

This illustrated.

If in addition to this the green itself be laid on a red ground, this may remain invisible in the intervals of the coloured matter of the superior strata: but if these strata be rendered

rendered transparent by imbibing a fluid, the influence of the lower stratums will be perceived, and will naturally show itself here by a yellow or reddish colour, when seen perpendicularly, while its oblique reflections will exhibit greenish or violet tints; and the drying of the substance will restore every thing to its former state.

Such in reality are the variations of the tints of certain parts of a peacock's feathers: and such in my opinion is the probable cause of their formation, which is equally applicable to those of the cock, the pigeon, and several other birds, as well as to insects, and in particular that splendid butterfly, the large wings of which exhibit a fine green, when viewed perpendicularly, and when obliquely a beautiful violet.

XV.

Account of a Fulminating Compound of Silver, of a white Colour and Crystalline Appearance: by Mr. DESCOTILS*.

Detonating powder lately sold at Paris.

A Detonating powder has been sold lately at Paris as an object of amusement. It is enclosed between the folds of a card, cut in two lengthwise; the powder being placed at one end, and the other being notched, that it may be distinguished. If it be taken by the notched end, and the other be held over the flame of a candle, it soon detonates, with a sharp sound, and violet flames. The card is torn, and changed brown; and the part in contact with the composition is covered with a light metallic coating, of a grayish white colour.

A compound of oxide of silver, ammonia, and vegetable matter.

Having been consulted on the nature of this substance, which is sent to Paris ready prepared, I convinced myself by various trials, which it is unnecessary to relate, that it is a compound of oxide of silver, ammonia, and some vegetable matter; so that it is analogous to the fulminating mercury of Mr. Howard.

Methods of making it.

This compound, which I call detonating silver, to distinguish it from the fulminating silver of Mr. Berthollet, may

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* Annales de Chimie, vol. LXII, p. 198, May, 1807.

be

be made by dissolving silver in pure nitric acid, and pouring into the solution, while it is going on, a sufficient quantity of rectified alcohol: or by adding alcohol to a nitric solution of silver with considerable excess of acid.

Pour nitric acid on silver, heat it gently,

In the first case the nitric acid, into which the silver is put, must be heated gently, till the solution commences, that is till the first bubbles begin to appear. It is then to be removed from the fire, and a sufficient quantity of alcohol is to be added immediately, to prevent the evolution of any nitrous vapours. The mixture of the two liquors occasions an extrication of heat; the effervescence quickly recommences, without any nitrous gas being disengaged; and it gradually increases, emitting at the same time a strong smell of nitric ether. In a short time the liquor becomes turbid, and a very heavy, white, crystalline powder falls down; which must be separated, when it ceases to increase, and washed several times with small quantities of water.

& add alcohol.

Decant, and wash the precipitate.

Or to a heated solution of silver add alcohol.

If a very acid solution of silver previously made be employed, it must be heated gently, and the alcohol then added. The heat excited by the mixture, which is to be made gradually, soon occasions a considerable ebullition, and the powder immediately precipitates*.

This powder has the following properties.

Its properties.

It is white and crystalline; but the size and lustre of the crystals are variable. Light alters it a little. Heat, a blow, or long continued friction, causes it to inflame with a brisk detonation. Pressure alone, if it be not very powerful, has no effect on it. It likewise detonates by the electric spark. It is slightly soluble in water. It has a very strong metallic taste.

Action of sulphuric acid on it.

Concentrated sulphuric acid occasions it to take fire, and is thrown by it to a considerable distance. Dilute sulphuric acid appears to decompose it slowly.

Of muriatic.

Muriatic acid, whether concentrated or weak, decomposes it immediately, and forms with it muriate of silver. The quantity of muriate it produces indicates, that it contains about 0·71 of metallic silver. A pretty evident smell of prussic

Smell of prussic acid.

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* It would be superfluous to remind the chemist, that the mixture of alcohol with hot nitric acid is liable to occasion accidents, and that it is consequently prudent, to operate on small quantities.

prussic acid is perceived the first moment of the mixture, but I never could discover any traces of it.

Decomposed by nitric acid;

Nitric acid decomposes it by the help of a boiling heat; and the products are nitrate of silver, and nitrate of ammonia, if it be continued long enough.

sulphuretted hidrogen;

It is decomposed by sulphuretted hidrogen; the ammonia and vegetable matter remaining in the liquor.

and potash.

Caustic potash decomposes it; black oxide of silver being separated, and ammonia disengaged.

Ammonia dissolves it without alteration.

It is soluble in ammonia; but by a slow evaporation it may be separated from it, retaining its original colour and other properties, particularly that of detonating by heat, and not by simple contact.

A virulent poison.

Its most important property to be considered, with respect to the use made of it, is its action on the animal economy. Mr. Pajot-la-Forêt, who has made a great many experiments on this subject, has found, that very small doses are sufficient to destroy pretty strong animals, as cats. They all expired in the most dreadful convulsions. It is unquestionably one of the most violent poisons to be found among metallic compounds.

XVI.

Memoir on the Means of forming a Judgment of the Quality of Glass, particularly Window Glass, and distinguishing such as is liable to alteration: by Mr. GUYTON. *Read at the General Meeting of the Society of Encouragement, March the 11th,* 1807*.

Prize proposed for a test of good glass.

ABOUT two years ago Mr. Guyton suggested to the society, to propose as the subject of a prize, a ready method of ascertaining the goodness of window glass. It appears, that, from the negligence or ignorance of the glass manufacturers, the windows in several large houses had become disfigured in a few months by a spontaneous alteration of the glass, which destroys its transparency: accordingly it was of some importance, to be able to guard against this inconvenience. The prize was proposed, and its term prolonged; but

French window glass liable to spoil.

The question

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* Abridged from Annales de Chimie, vol. lxii, p. 5, April, 1807

but no paper was sent on the subject. This led Mr. Guyton to investigate it himself. unanswered.

Specific gravity.

With respect to the general characters of glass he observes, the specific gravity of different kinds of glass, all of which may be good, is very various; beside which, to ascertain it requires nice instruments, and an expertness in their use not commonly to be met with.

Fracture.

The inspection of the fracture affords but a loose conjecture to the most experienced eye: though Mr. Ducloseau asserts, that the fracture of good glass is always wavy, and its angles more or less acute.

Hardness.

The degree of hardness varies not only in different kinds of glass, but in glass of the same kind. Workmen used to handle the diamond readily distinguish glass that cuts soft, from that which cuts hard; so that this might seem a sufficient indication of its good or bad quality. It is very difficult however to discriminate degrees of difference in this respect, neither is it always to be depended upon.

Bad glass not a perfect non-conductor of electricity;

Glasses that are called greasy [*gras*] are bad insulators of electricity, and little capable of being rendered electric by friction. Probably this, as well as their being liable to alter, depends on an excess of saline flux, or an imperfect refining, that has left sandiver in them.

and tarnished by heat.

Bad glass placed on burning coals becomes dull and more or less meally on its surface. The same effect is more speedily and evidently produced before the blowpipe. But in all such trials we have to guard against the too rapid or too unequal action of the heat.

Neutral salts do not act on glass.

The experiments Mr. Guyton made with neutral salts convinced him, that this mode of proof, which would have been very convenient, was altogether inefficacious. Among the specimens of the commonest window glass however, which is in general unalterable, he found one, that underwent a perceptible change on its surface by merely boiling in a solution of alum and muriate of soda.

Acids act on powdered glass

There are few glasses, which, when reduced to an impalpable powder, are not slightly acted upon by concentrated acids: though previously all well made glass resists sulphuric acid, the most powerful of all, even assisted by heat. On the contrary it very readily attacks glass of bad quality, at the

Bad glass corroded by sulphuric acid.

the common temperature. I have seen, says Mr. Guyton, black glass bottles, into which concentrated sulphuric acid had been put, perforated with several holes in the course of a few days; which, being largest interiorly, exhibited there white, silky excrescences, formed of the acid united with the soluble earths of the glass. But all glass susceptible of alteration in the air is not of so defective a composition; so that to form a judgment of it, the acid must be digested upon it, and the heat carried so far as to raise the acid in vapour. When this is done, it will leave untouched only well made and well refined glass, whatever be its nature, transparency, or colour.

This a good test.

But may be neglected from fear.

Sulphuric acid then fulfils all the conditions required for a test. I foresee however, that this chemical instrument will frequently be neglected, from fear of the accidents, to which they may be exposed, who are not in the habit of using it with caution. This has led me to substitute an agent equally powerful, that may be procured more easily, and employed without any danger.

Easy mode of ascertaining its goodness by sulphate of iron.

This agent is sulphate of iron, the green copperas of the shops. After having put into a small Hessian crucible, or any other of stoneware, slips of the glass to be examined, either alone, or comparitively with others, let the crucible be nearly filled with this sulphate coarsely powdered. It may then be set on the fire, and kept there till the metallic salt has acquired a red colour. When the glass is grown cold, it only requires to be immersed in water, which will show whether it will be altered, and in what degree.

This process is not expensive, requires no apparatus, and is in every one's power: and the results I have obtained appear to me to leave no doubt of its advantage.

XVII.

Report on a Paper on Nitrous Ether, read to the Institute the 4th of August, 1806, *by Mr.* THENARD, *Professor in the College of France. By Messrs.* GUYTON, VAUQUELIN, *and* BERTHOLLET*.

Various ethers.

VARIOUS kinds of ether have been formed by the action of some of the acids on alcohol. Volatility, inflamma-bility,

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* Annales de Chimie, vol. lxi, p. 282, March, 1807.

bility, and a peculiar smell, give to them all a common character, which does not allow us to confound them with other substances: yet we have only an imperfect knowledge of the differences that distinguish them, and our theory of their production is perhaps still more imperfect. It is true Messrs. Fourcroy and Vauquelin have thrown much light on the production of sulphuric ether; but their explanation does not admit of being extended to some others. It was of importance therefore to resume this subject, and treat it in a general manner. This Mr. Thenard has undertaken. In the first paper he has presented to the Institute he treats on nitric ether; from this he intends to proceed to others; and he will examine why some acids have the property of producing ether, and others have not.

Have a common character.

Yet differ.

The processes recommended by different chemists for the production of nitric ether, which Mr. Thenard reviews, are very discordant, and have no other object than the ethereal liquor to be obtained, without any analysis of the gaseous products, or any consideration of the circumstances of the process; if a memoir by the Dutch chemists be excepted, which Mr. Thenard examines particularly at the conclusion of his own. Accordingly he found himself obliged to treat his subject independantly of the labours of others.

Various processes of making it.

Mr. Thenard began with distilling a mixture of equal weights of alcohol and nitric acid, both of given strengths, in an apparatus adapted for the separation of the fluid from the gaseous products. A gentle heat is sufficient; and indeed the action soon becomes so brisk, that it is necessary to suppress even that. He afterward examined the residuum in the retort, the fluid product, and the gasses.

Equal weights of nitric acid and alcohol distilled.

The residuum was composed of nitrous acid, acetic acid, alcohol, water, and a small quantity of matter, the nature of which is uncertain, but which is easily carbonized. The proportions of these he ascertained by ingenious and accurate methods. If the distillation be carried on to dryness, the viscous residuum contains oxalic acid, and probably malic.

Residuum.

The distilled liquor, which is considered in the shops as nitric ether, is composed of water, nitrous acid, acetic acid, ether, and probably alcohol.

The distilled liquor, or impure ether.

The gaseous product in particular required much patience and

Gasses produced.

and ability, to separate it into its different elements, to assign to each of these the properties dependant on it, and to explain the differences that result from the circumstances in which they are placed. It was composed of nitrous gas, azote, oxide of azote, nitrous acid gas, carbonic acid, and ethereous gas, which it was particularly necessary to separate from the rest, in order to examine its properties. By these experiments, which may be termed preliminary, the author was led to the following process for separating the pure ether, and examining it, either in the liquid or gaseous state.

Mr. Thenard's process.

Into a retort he put five hectogrammes [16oz. 48grs. troy] of alcohol, and as much nitric acid, and adapted to it in succession by means of glass tubes five tall bottles half filled with a saturate solution of muriate of soda. In the last was a tube opening under a jar to collect the gaseous part. All the bottles were surrounded with a mixture of pounded ice and salt, which was stirred occasionally. To commence the operation a little fire was applied, but it was soon necessary to extinguish it, and even to cool the retort.

The fluid left in the retort was similar to that of the first experiment already mentioned.

The ether produced.

On the surface of the solution in all the bottles was found a yellowish liquid; which weighed, when the whole was collected together, 255 gr. [8 oz. 98 grs]. That in the first bottle was a mixture of alcohol, ether, acetic acid, and nitrous acid: that in the others was nitric ether free from alcohol.

Its properties.

The nitric ether in this state has a strong smell: it is specifically lighter than water, and heavier than alcohol: dissolves in any proportion in the latter, but requires near 48 parts of water to dissolve it, and at the same time is partly decomposed by it, as will appear below. It possesses the property of combustibility in a high degree; yet it strongly reddens infusion of litmus, owing to a little nitrous and acetic acid that it retains, and which may be separated by means of lime.

Much more volatile than sulphuric.

The volatility of ether thus prepared is so great, that it indicates a tension of 0·73 of a metre, while that of the best sulphuric ether, under the same circumstanes, is but 0·46 of a metre, at 21° of the centigrade thermometer [70° F], and at 0·76 of a metre [29·7 in.] of atmospheric pressure. At this

this temperature, and this pressure, therefore, it is at the limit of its existence in the liquid state.

But if nitric ether be deprived of its acidity by means of lime, it soon becomes acid again, whether it be redistilled, left in contact with the air, or kept in full and closed bottles. This formation of acid takes place, when ether is treated with water, particularly at a temperature from 25° to 38° [77° to 86°]. The author explains the formation of this acid by the reciprocal action of the principles that constitute ether, and which are found to be feebly retained in it by combination.

Soon becomes acid.

Mr. Thenard next proceeds to the decomposition of nitric ether by heat, and analyses the gasses arising from it. Founding his calculations on the most accurate data we possess, the result is, that 100 parts of nitric ether, rejecting fractions, are composed of azote 16, carbon 39, oxigen 34, hidrogen 9.

Analysed by heat.

Its component parts.

Hence he infers what passes in the reciprocal action of alcohol and nitric acid. The oxigen of this acid combines with a great part of the hidrogen of the alcohol, and with a very small quantity of its carbon. Hence result, 1st, a great deal of water, a great deal of gaseous oxide of azote, a little carbonic acid, a little nitrous acid and nitrous gas: 2dly, the separation of a small quantity of azote, and the formation of a great deal of nitric ether, by the combination of a pretty large quantity of the two principles of nitric acid with the alcohol dishidrogenized and slightly decarbonized: 3dly, the formation of a little acetic acid, and a small quantity of a matter easily carbonized, by the combination of a part of the hidrogen with carbon and oxigen.

Action of nitric acid on alcohol explained.

Supported by these deductions, Mr. Thenard discusses the processes published before him for obtaining nitric ether, and he shows, that some are dangerous to attempt; that none of them furnish the whole of the ether, that might be obtained from the same ingredients; and that all of them yield only more or less compounded liquors, in which the nitric ether, though they bear its name, constitutes but a part.

Former processes, some dangerous,

all wasteful,

and the ether impure.

The Dutch chemists have published some interesting researches on nitric ether, or rather on the gasses obtained by the action of nitric acid on alcohol. But to explain the curious

The hypothesis of the Dutch chemists erroneous.

curious facts they have made known, they have employed an insufficient hypothesis. 1st, They have considered the gas in question as a compound of nitrous gas and ether; while it is composed of gaseous ether, nitrous gas, nitrous acid, azote, oxide of azote, carbonic acid, and acetic acid, in short of all the substances capable of assuming the gaseous state, in the various circumstances under which they are found. 2dly, They have supposed ether to be a substance always identically the same, so that they neglected to analyse nitric ether, and establish its peculiar characters. 3dly, In consequence of this opinion they have been led to ascribe to a preexisting nitrous gas phenomena, that are owing to a decomposition of nitric ether.

If the process were varied, the effects would differ;

After having discussed the opinion and experiments of the Dutch chemists, Mr. Thenard concludes his paper by observing, that he has considered only the products and phenomena obtained by given proportions, and under certain circumstances. The effects must be different, when these are varied; and he intends to subject them to experiment: but he has already satisfied himself, that those he has employed are most favourable to the production of nitric ether.

but that above given the best.

The memoirs printed by the Institute.

The committee concluded, that the memoir at large merited insertion in the Collection of foreign Papers, and the class adapted their conclusion.

XVIII.

Observations on subterranean Heat, made in the Mines of Poullaouen and Huelgoat in Brittany: by J. F. DAUBUISSON*.

Facts respecting the interior heat of the earth wanting

THERE are few questions in natural philosophy, respecting which we are more in want of positive and well established facts, to deduce consequences from, than that respecting the temperature of the interior part of our globe, taken at depths we are able to reach. I have already made known some facts I observed on this subject in the Saxon mines, and

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* Journal des Mines, February, 1807, p. 119.

and I shall now give some others noticed last summer in Brittany. The habit of making similar experiments, and the knowledge I had of the places, enabled me to choose with some discrimination the points of which I ascertained the temperature; so that I trust the facts I have recorded will not be uninteresting to those, who make our Earth an object of their study.

Thermometer used.

3′ or 4′ to alter 21° F. in water;

11′ or 12′ in the air.

The thermometer I employed was of mercury, and divided into eighty degrees from the freezing to the boiling point of water. It was enclosed in a tube. I found by trial, that when it indicated a given temperature, and was made to deviate from this about a dozen degrees, it required three or four minutes to bring it back to the former point by immersing it in water of that temperature, and eleven or twelve minutes if kept in the open air. Hence, whenever I was desirous of ascertaining the temperature of a body of water in the mines, I immersed the thermometer in it entirely, and left it there five minutes; and when I took the temperature in the air, I let it remain a quarter of an hour. All the observations were afterward reduced to degrees of the centigrade thermometer. Notwithstanding all the care and patience I employed however, I cannot answer for their exactness to less than a quarter of a degree.

Observations made at Poullaouen.

Situation and topography of Poullaouen.

THE mine of Poullaouen is in latitude 48° 17′ 49″ N., and longitude 5° 55′ 57″ West of Paris. Its mouth, that of St. George's pit, is 106 met. [347½ feet] above the level of the sea. It is 4 myriam. [25 miles] from the seacoast of Brittany on the north, and 6 [37½ miles] from that on the south and that on the west. It is in that tongue of land, which advances into the ocean under the form of a roof, raised in its centre about 260 met. [853 feet] above the level of the sea, and constitutes Brittanny. The country round the mine, to the distance of near 6 miles, is about 150 met. [490 feet] above the level of the sea; and is intersected in every direction by valleys, one of which is an almost circular basin about a millimetre [1093 yards] in diameter, that forms the roof of the mine.

According

Mean temperature by calculation.

According to the law of the temperature from the equator to the pole, the mean temperature of the surface at Poullaquen should be 12·4° [52·5° F.][*]. The elevation of the soil requires near 1° [1·8° of diminution, so that the mean temperature may be estimated at 11·5° [50·9° F.]

Observations made 5th of Sept. Day fine.

My observations were made the 5th of September, 1806. During the whole day the weather was fine, and but few clouds were seen. The temperature in the shade, in the middle of the day, was 19° [64·4° F.]. In reporting my observations I shall mention the situation of the places where they were made, as well as whatever appeared to me capable of influencing the temperature. Opposite each expression of the temperature I shall note the depth of the place below the surface of the ground.

Tabulated temperatures and depths, with remarks.

	Temperature by Fahrenh thermo	Depth English Ft. In.
1. In the first gallery, called *fifty foot level,* near the shaft, in a place where there was but a slight current of air, a little water that lay on the ground indicated	53·8°	52··3
2. In St. George's gallery, under the intersection of three branches of the vein, in a kind of cul de sac, very remote from the places where the miners were at work, in which there was no current of air, but a large quantity of water filtered from the roof: this water was ··	51·6°	127··4
3. The water that thus filtered into the gallery, when it reached the well from which it was raised, was·························	52°	127··4
4. Thirty six metres lower, at the level of Boullaye, toward the end of a long gallery, where there was no current of air, and no person at work, under very strong percolations and in the water I had ···················	51·6°	244.10

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[*] Theory and observation have led me to a very simple expression of the thermometrical temperature of a place, the latitude of which is known. This expression is 80·7° [85 4° F.]. $\cos.^{2\cdot 25}$ latitude; or with sufficient exactness in the temperate zone 28° [80 6° F]. $\cos.^{2}$ lat.

[†] In the atmosphere the temperature diminishes 1° [1·8° F] for every 175 met. [191 yards] in height.

5. At

Tabulated temperatures and depths, with remarks.

	Temperature.	Depth.
5. At the bottom of St. George's shaft, in the well in which the waters that penetrate into the lower parts of the mine around it are collected	55·7°	463··6
6. The air over this water	57·2°	460··3
7. In the well at the bottom of St. Barbe's shaft, at the other extremity of the mine	54·5°	489··6
8. In the air above this water	56·1°	489··6
9. The waters of the old excavations, that run into this well	54·1°	
N. B. These waters arising from filtrations that take place chiefly in the upper parts of the old workings are cold; and as they form the greater part of those that enter into St. Barbe's well, they are the occasion of the lowness of the temperature shown by the water in it.		
10. In an excavation but little distant from the bottom of St. Barbe's shaft, called the oven gallery, the sides of which are almost every where interspersed with radiated pyrites partly effloresced, the thermometer left a quarter of an hour in a small hollow made in the midst of the pyrites, and which contained a great deal of white sulphate, indicated	56·5°	457
11. Afterward put into a small hole, from which a pretty strong spring issued, it equally marked	56·5°	457

General deductions.

Superficial temperature agreeable to theory.

The observations 2, 3, and 4, incontestably prove, that the heat of the rock in the upper parts of the mine is 51·8°. The waters indicating this certainly acquired the temperature of the rock in filtering through it; and this temperature does not differ in any sensible degree from that indicated by theory. If the first observation showed a little higher temperature, it is because it was made in a place, where air from without, and consequently warm, as the experiments were made at the end of summer, was continually passing.

Temperature

Observations 5 and 6 show, that the temperature of the

lower

increases with the depth.

lower parts of the mine is more considerable than that of the upper parts. If in the depths the air appear hotter than the water, it is probably because it has retained a part of the heat it possessed when it entered the mine. I have already pointed out the reason why in 7, 8, and 9, the heat was less than the depth requires.

Pyrites do not always occasion heat.

Experiments 10 and 11 show, that there are circumstances in which the presence of pyrites does not occasion heat. That which they indicated was not occasioned by them, for in St. George's shaft, where there was none, the temperature was the same.

Heat increases 4° or 5° in 100 yds. of depth.

Thus, setting aside every extraordinary cause, the observations I have reported appear to me to indicate, that at the depth of 150 met. [163 yards] the temperature at Poullaouen is 2° or 3° [3·6° or 5·4° F.] more than at the surface.

Observations made at Huelgoat.

Huelgoat mine.

THE mine of Huel is in latitude 48° 18′ 17″ N., long. 6° 1′ 46″. The opening of its shaft is 173 met. [188 yards] above the level of the sea; and is on a large hill, that separates two valleys, the depth of which is 80 or 90 met. [87 or 97 yards].

Mean temperature by calculation.

From this latitude and elevation we may infer the mean temperature to be 11° [50° F.]

In schist.

The rock, like that of Poullaouen, is an argillaceous schist, but it likewise contains some strata of aluminous schist.

Observations the same day.

The following are the observations I made on the 5th of September, the same day as those at Poullaouen.

Tabulated temperatures and depths, with remarks.

	Temperature by Fahrenh. thermo.	Depth English Ft. In.
1. In a gallery about 16 yards below that by which the workmen commonly enter the mine, into which no person had been for several years, which has no other outlet, and in which there was no current of air, a thermometer at its northern end after twenty minutes indicated ……………………………	50°	
After having descended the shaft called the miners, and gone a few dozen yards into the		

gallery

Temperature. Depth. Tabulated temperatures and depths, with remarks.

gallery at its foot, I entered another shaft, which terminates in a gallery that has no communication with the rest of the mine, and in which consequently there is no current of air.

2. Here the thermometer immersed in a little stagnant water on the ground rose to .... 52·2° 228·6

3. I then ascended to the former gallery, and under a strong infiltration, in the water, and in a place traversed by a current of air, the thermometer indicated .... 54·9° 195.10

I then directed my course to the south, where the present workings are.

4. At the second gallery, at a little distance from the main shaft, in a place where people are continually passing, and where there was a pretty strong current of air, a little stagnant water indicated .... 57·2° 261

5. At the fifth gallery, the thermometer immersed in a reservoir of water, which was near the main shaft, rose to .... 60·8° 457

6. At the extremity of the gallery, No. 9½, the part where the works are farthest advanced in a southerly direction, a large quantity of water, slightly vitriolic, spouts from the rock. The thermometer, kept a quarter of an hour in the midst of this stream, constantly marked .... 65·7° 750

7. Held on one side of it, in the open air, it equally gave the same temperature .... 65·7° 750

8. It was the same about sixty paces toward the shaft, when immersed in the middle of the rivulet formed by the spring just mentioned .... 65·7° 750

The bottom of the mine was inundated, the water lying on it about 16 met. [52 feet] deep: and by a small shaft, at a little distance from the main shaft, I descended to the level of this subterranean lake.

9. The thermometer, kept a quarter of an

Tabulated temperatures and depths, with remarks.

	Temperature.	Depth.
hour on a board floating on the water, indicated	64°	776
10. Immersed in the water it equally indicated	64°	776
All the water that enters into this southern part of the mine runs into the lake, from which it is pumped up.		
11. The temperature of the water issuing from the pumps into the gallery No. 7 was	65·1°	587·6
Following the course of this gallery the water runs to another shaft in the north part of the mine.		
12. There it mixes with a small quantity of water, the temperature of which is	57·2°	391·8
13. And when the water thus mixed is conveyed by the pumps to the discharging gallery it raises the thermometer to	63·3°	

General deductions.

We have here two classes of observations to distinguish; those made in the north part of the mine, 1, 2, 3, and 12; and those made in the south part.

Natural temperature agrees with the theory. Increases with the depth.

The first appear to me to indicate the natural temperature of the soil. No. 1, made 20 or 30 yards below the surface of the ground over that part of the mine, must be considered as giving the true degree of heat of the surface of the earth in that country. I perceive no cause, that can have altered the natural temperature of the place, which is very far from all the workings: certainly it continues the same throughout all seasons: and its expression is precisely the same as theory indicates. Observations 2 and 3 show, that this temperature increases, as we penetrate deeper. The current of air in the first gallery, that through which the ore is conveyed in wheel barrows, accounts for the small excess of heat observed in it proportionally to the depth.

Increased in one part by thermal waters. Cause of their heat.

As to the temperature of the observations made in the south part of the mine, it is visibly influenced by a foreign cause, the arrival of vitriolic waters coming from the south. On sinking a new shaft a hundred yards from the southern part of the present workings, strata of aluminous schist were traversed, which had a very strong styptic taste as soon

as

as extracted. With a lens a number of pyritous points may be observed in it, which by their decomposition and action on the schist probably produce an evolution of caloric, that heats the water traversing these strata. These being at no great depth may have a communication with the atmosphere through some fissures, and thus the decomposition may have been effected.

However this may be, it appears to me certain, that the water must have acquired the heat of 20° [66·2 F.], which is much more than naturally belongs to their depth, by traversing these strata.

Pyrites in conjunction with schist.

I shall here observe too, that, if the heat is to be ascribed to the pyrites, they produce it by their action on the schist. In the observations made at Poullaouen, we found a considerable quantity of pyrites, without any particular heat being produced: and I must repeat what I have said elsewhere, that I have seen pyrites extracted, and found the heat not perceptibly greater than in other mines. Accordingly I am inclined to believe, that pyrites in the mass, or at least not radiated, do not produce subterranean heat: but such as are disseminated in extremely small parts through bodies on which sulphuric acid is capable of acting are in a different case, when the atmospheric air can have access to them. In another paper I remarked, that the inflammable air called fire-damp in coal-mines does not proceed from the coal that contains most pyrites, but from that in which scarcely any is discernible, and where the sulphuret of iron is probably in imperceptible particles.

Pyrites do not always occasion heat.

Firedamp not produced by the most pyritous coal.

XIX.

Letter from Dr. VEAU-DE-LAUNAY *to J. C. Delametherie, on the Production of oxigenized muriatic Acid by the galvanic Pile**.

Acid obtained by galvanism

THE experiments with respect to the production of the oxigenized muriatic acid, obtained in distilled water by the galvanic

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* Journal de Physique, Vol. LXIII, p. 472, Dec. 1806.

In small quantities hitherto.

1½oz. of water converted into oxigenized muriatic acid, dissolved 2·7 in. of gold wire.

Precipitated by nitrate of silver.

galvanic action, have hitherto exhibited but a small quantity of acid; which has led persons to question, and even to deny, the result of these experiments. What has lately been done by the experimental class of the Galvanic Society will leave no doubt with regard to the product. Fifty grammes, or nearly an ounce and half of distilled water, have passed into the state of oxigenized muriatic acid, and dissolved a gold wire of the length of about 0·07 of a metre, or 2½ inches [2 in. 7 lines Eng.], immersed in the cylinder, containing the distilled water, which had acquired a very sensible and even strong smell of that acid, as well as a yellow colour, such as a solution of gold exhibits. To these different characters, easy to distinguish, were added those of the action of different reagents. The tincture of litmus was reddened powerfully; the solution of nitrate of silver was speedily changed, and a precipitate of muriate of silver thrown down.

I can assure you, there is not the least doubt of the certainty of the results displayed by this conversion of distilled water into oxigenized muriatic acid.

If the acid were extricated from the salt merely, still interesting.

Even if the result of the experiment were to be considered as an extrication of the muriatic acid from the solution of salt contained in the cylinder of the copper pole, a very interesting phenomenon would remain to be considered in this product. This experiment therefore deserves particular attention; and it is to be wished, that it was varied and repeated in different ways. I doubt not but the result will offer facts of considerable importance to the science of natural philosophy.

SCIENTIFIC NEWS.

New Bavarian Academy of Sciences.

THE Bavarian Academy of Sciences at Munich, according to its new constitution, is to have a more extensive field for its labours than any other in Europe. Under the direction of the ministry, it is to have the immediate superintendance of all the public seminaries of education in Bavaria, from the universities down to the primary schools. It will be composed of learned natives, and foreigners of celebrity invited by the government from other parts of Germany.

many. Privy counsellor Jacobi is still talked of as its president. Among the other academicians, whose names have been mentioned, are Mr. Seyffer, whom the emperor Napoleon appointed engineer geographer in the war against Russia and Austria, and who has been director of the observatory at Gottingen; Eichhorn, the historian and orientalist, whom the king has likewise called from Gottingen; Wiebeking, of Vienna, whose skill in hydraulics has already been of service to the kingdom; and Wolf, known by a very good history of the Jesuits, to whom all the archives of Bavaria are opened, for the purpose of his compiling a national history.

Royal library at Munich.

The royal library, which is already a very good one, will be increased by a committee appointed to select for it every thing of value in the libraries that are suppressed.

Collection of paintings.

The collection of paintings at Munich has long been celebrated; but, since the galleries of Manheim and Dusseldorf have been added to it, it is unquestionably the finest in Europe, next to the Napoleon Museum.

Royal Academy of History & Antiquities at Naples.

A decree has been issued at Naples for forming a society consisting of forty men of letters, to be called the Royal Academy of History and Antiquities. The first twenty members are to be named by the king; and when these have assembled, they are to nominate three persons for each of the remaining vacancies, out of whom the king will choose one to fill it. It is to have a perpetual secretary appointed by the king, and to choose its own president for three months. The directors of the museum, excavations, and royal printing office, are always to be chosen from its members. The academicians are to be admitted at court.

Mechanical imitation of various wind instruments and others.

Mr. John Maeltz, of Vienna, has exhibited at Paris a musical machine of his invention, to which he has given the name of *panharmonicon*. This machine, moved entirely by springs, gives the sounds of various wind instruments with a clearness and perfection never before attained. The instruments that compose it are the german flute, flagelet, clarinet, hautboy, bassoon, horn, trombone, serpent, and trumpet; beside kettle drums, a great drum, cymbals, a triangle, &c. Pieces are performed by it with great precision, and the forte and piano distinctly marked. The execution

cution of the trumpet is particularly astonishing. For each instrument the inventor has contrived a mouthpiece adapted to its nature, which answers with the greatest perfection to the capacity of the human organs.

Intended tour in the East.

The chev. von Hoegemuller, superintendant of the Austrian military studs, is to set off in November on a tour in the East, with the necessary instruments and attendants, under the patronage of prince Charles. His principal object is the natural history of the horse; but he will make a point of endeavouring to answer any questions, that shall be addressed to him by the learned who cultivate geography, philology, archæology, numismatics, &c. He intends to traverse Hungary, Transylvania, Buchowina, the Ukraine, embark at Odessa for Constantinople, and thence proceed to Aleppo.

Ancient busts made by American Indians.

Mr. Jefferson, the president of the United States of America, has in possession several busts made by Indians. They are nearly of the natural size, and reach to the middle of the body. The features are well marked, and characteristic of the copper-coloured or American race. In one, representing an aged savage, the wrinkles and the expression of the countenance are very well marked. These busts were found in digging at Palmyra, on the river Tennessee. The substance of which they are formed, and which is extremely hard, is not known: some suppose them to have been cut by the chissel out of solid stone; others that they are a composition, first moulded, and then burned. Whether they were idols, or busts of distinguished persons, is equally questioned. Who were the progenitors of the present race of Indians, that were capable of thus executing a tolerably good resemblance of the human head, face, neck, and shoulders?

Extensive ridges of shells in America.

General David Merriweather writes to Dr. Mitchill of New York, that the vast banks of shells commencing on the southern bank of the Savannah, near White Bluff, extend in a right line through a space of about a hundred miles from the borders of the sea toward the south-west. The ridges are not entire, but the ground is more elevated to the breadth of six or eight miles than it is above or below. Not only oyster-shells, but those of cockles, and others,

are

are found. Some are entire, and very large: others are agglutinated as by a cement. Some are large enough to contain a man's foot. In different parts of the eminences for forty miles some of these shells occur. They are used for making lime; but a little higher up to the south-west there is a heap of shells forming a kind of rock, that is preferred for this purpose. At some distance still higher, and in the same direction, there are several quarries of a kind of siliceous stone, in which a great number of shells of all kinds are interspersed here and there. These are petrified, and as hard as the flint itself. Millstones are made of it, in quality nearly the same as those of France.

Fossil shells in silex, hard as flint. Millstones made of it.

At Hudson's Bay some experiments have been made with frozen mercury. It was reduced to a plate as thin as paper, by beating it on an anvil with a hammer brought to the same temperature as the mercury. A piece of it being thrown into a glass of hot water, the water froze instantly, the glass flew to pieces, and the mercury became fluid.

Quicksilver beaten into a thin plate.

Dr. Baconio of Milan has lately composed a galvanic pile entirely of vegetable substances. He forms it of disks of red beet root, two inches in diameter; and disks of walnut tree, of the same size, divested of their resinous principle by digestion in a solution of cream of tartar in vinegar. With this pile he produces galvanic effects on a frog, taking a leaf of scurvy-grass for an exciter.

THE seventh number of the new series of the Mathematical Repository, by Mr. Thomas Leybourn, contains: 1. Solutions to thirty curious mathematical questions proposed in a former number; 2. Solutions to some mechanical problems by Mr. John Dawson; 3. Solution of a curious diophantine problem by Mr. Cunliffe; 4. An essay on the theory of amicable numbers by Mr. John Gough; 5. An investigation of some theorems for finding the sums of certain infinite series by Mr. Cunliffe: 6. Le Gendre on elliptic transcendentals: and 7. Thirty new questions to be answered in a subsequent number.

Mathematical Repository.

New mineralogical work.

Mr. ACCUM, to whom the public is indebted for a System of practical Chemistry and several other works, has put to the press a System of Mineralogy and Mineralogical Chemistry, and its application to the arts. This work, which is formed chiefly on the plans of Haüy and Brongniart, will be in 2 vols. 8vo. with eight copperplates.

Lectures on Surgery, and on Physiology.

Surgical and physiological lectures.

Mr. A. CARLISLE, F.R.S. F.L.S. and surgeon to the Westminster Hospital; will begin his course of lectures on the art and practice of surgery, in all its branches, on Tuesday, October 6th, at eight o'clock, P. M. at his house in Soho Square. The subject will be continued on Tuesdays, Thursdays, and Saturdays, at the same hour. The diseases and accidents allotted to the province of surgery will be fully treated of, and illustrated by cases from the lecturer's experience. The different operations will be demonstrated, and the anatomy of the parts explained. On the same evenings, a course of lectures will be delivered on the natural history, physiology, and pathology of the human body, calculated to illustrate the several processes of healing, and to afford a compendious view of the animal economy. The introductory discourse will be open to all students.

Medical and Chemical Lectures, St. George's Hospital, and George Street, Hanover Square.

Medical and chemical lectures.

ON Monday, October 5th, a course of lectures on physic and chemistry will recommence at No. 9, George Street, Hanover Square, at the usual morning hours: viz. the medical lectures at 8, and the chemical at 9 o'Clock. By George Pearson, M. D. F. R. S. senior physician of St. George's Hospital, of the College of Physicians, &c.

Note, a register is kept of the cases of Dr. Pearson's patients in St. George's Hospital, and an account is given of them at a chemical lecture every Saturday morning at 9 o'clock. Proposals may be had at the Hospital, and at No. 9, George Street.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1807.

ARTICLE I.

Facts toward a History of Pitcoal: by Professor Proust*.

Coal left from ·59 to ·77 of coak.

A Coal of Decise, distilled by Sage, left ·59 or ·60 of carbonaceous residuum, or coak. A coal of Cévènes yielded Berthollet ·76 or ·77; and that of a pit in the forest of Gensane ·75. I obtained from a coal from England ·64: from Lieres ·60, and from Fondon, both in Asturia, ·64: from Belmez, in Estremadura, ·65: from Villa Nueva near Seville ·68: from Quiros ·70, las Camaras ·70, Langreo ·75, la Rionda ·76, and la Riosa ·77, all in Asturia.

No coal near Madrid, where it is much wanted.

The environs of Madrid exhibit a few vestiges of earth impregnated with bitumen, but no coal, as was hoped; for there is not a city in Europe at present so much in want of it. I have visited no coal pits in Spain, but some of those of the fertile and picturesque province of Asturia; a country that exhibits in miniature whatever of grand and sublime the traveller admires in the Alps. The coal there in general is in veins of little thickness; scarcely any so much as 18 inches.

The following were the products of a hundred pounds of some of the coals I examined.

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* Abridged from Journal de Physique, vol. LXIII, p. 320, Oct. 1806.

Products of different coals.

	Coak.			*Oil.*			*Water.*			*Gasses lost.*		
	lbs.	oz.	dr.	lbs.	oz.	dr.	lbs.	oz	dr.	lbs.	oz.	dr.
Villanueva	68	8	4	7	9	0	4	8	6	19	5	6
Belmez	70	0	2	10	4	0	7	7	2	12	7	4
Langreo	75	0	0	11	11	4	4	7	2	8	3	2
English	64	1	0	7	0	4	12	14	2	16	0	2

General conclusions.

From these observations it follows, that the oily product is more abundant than the aqueous from three of these coals.

2. That the weight of the gasses is as variable as that of the liquid.

3. That the real quantity of coal furnished by these bituminous substances is in general above ·60, and less than ·80.

4. That it is three times as much as wood affords: and we know nothing but indigo, that can be compared with pitcoal in this respect.

5. That the matter of pitcoal varies as much in its carbonaceous, oily, gaseous, and other products, as the organized bodies, that are formed at present before our eyes.

6. That its oily produce is in general much greater, than our resinous woods, as the oak, elm, ash, &c., can furnish.

7. That pitcoal is thrice as serviceable, in furnaces that admit its use, as any wood known, since it leaves thrice as much coally matter.

8. That its coak, in consequence of its azotization, derives from our atmosphere much more fire than charred wood, since it cannot burn but by decomposing a much larger quantity of oxigen,

9. That as the oily and gaseous products are formed as well in the open air as in close vessels, some coals produce more flame than others, and are consequently better adapted to furnaces where a considerable current of flame is required; as those of bronze, porcelain, earthenware, &c.

10. That those which leave most coak after distillation will consequently last longer in iron works, reverberatory furnaces, &c.

Those which yield most tar not easiest to distil.

Of these four coals perhaps the richest in oil would not be the most easy to distil, at least by lord Dundonald's method, since they run, swell up, and agglutinate, so as toward

ward the end to become a uniform mass, not to be divided without labour.

All pitcoals in general give out more or less sulphurous acid toward the end of their combustion, which is owing to pyrites. At first I supposed, that they contained a peculiar compound of sulphur and carbon; but all that I passed through nitric acid, to free them from pyrites, burned to the end without emitting the slightest smell of sulphurous acid.

Yield sulphurous acid.

The oily product of these coals varies greatly in consistence: it is more or less fluid, and a direct experiment is always necessary, to ascertain how much thick oil, or tar, it will afford. Is this tar really more preservative, and better adapted to retard the spontaneous oxidation of cordage, and the rigging of ships, than that of resinous wood? This has been asserted, but should it not be farther examined?

Oily product differs in consistence.

Should be farther examined.

The light oil separated from it is succinated, and not unpleasant. It is readily turned brown by the air. No particular use for it is known.

Light oil.

The aqueous product contains carbonate of ammonia, but in small quantity. I did not find in it any vinegar, but I could wish to examine this again; particularly as I find in my notes, that I separated a little succinic acid from it by treating it with the muriatic.

Water contains carbonate of ammonia, and succinic acid.

The gas is an oily hidrogen, that burns with a white flame, which limewater diminishes very little, because the ammonia retains almost all the carbonic acid.

Gas, oily hidrogen, with very little carbonic acid.

Naturalists, reflecting on the similitudes that analysis perceives between the composition of vegetables and that of pitcoal, have pretty generally supposed, that coal may have been produced by heaps of fern, polypody, reeds, and aquatic plants destroyed, the impressions of which are retained by the adjacent strata; or even by trees, and their fragments, such as those which certain rivers by their inundations sweep away from the earth; and that the sea has broken them by its agitation, and accumulated them in the basins, from which we at present extract them. But the mechanism, and even possibility of these operations, simple as they are in appearance, are exposed to innumerable difficulties, when we examine them in detail.

Coal supposed to be produced from vegetables.

For

Objections to this hypothesis.

For instance, the most elevated parts of the Globe on which depositions of coal have been found, as the Cordillera in Peru, where Leblond met with them, being more than four thousand yards above the level of the sea, do not easily bend to the explanation attempted, when it is alleged, that they are forests or plants swept away and comminuted by the waters. Had the sea at that period no lower place, on which to deposit its mud of broken plants? or was the Cordillera itself very favourable to the production of vegetables? These objections, which were made by Patrin, are not easy to remove. If we consider farther, that these immense tracts of carbonaceous mud, which resemble torrents of melted resin that a volcano has vomited out at once into valleys ten, fifteen, or twenty leagues long, and to the height of thirty, forty, or sixty feet, exhibit not the slightest interruption, not the least vestige of fishes, shells, bones, or stones, in their beds; no foreign body in their mass; to indicate those convulsions, or that disorder, which the imagination cannot easily separate from such great devastations of forests, mountains, and continents: we must confess, that such productions are not explicable by some of those accidents, of which nature at present gives us occasionally examples.

Alternation of their strata with sandstone.

Beside that the recurrence of fifty or sixty strata of coal, with as many of sandstone, do not allow the mind to conceive, how these two kinds of sediment can have been accumulated exclusively; as if previous to these periods the earth could produce nothing, but what furnished the sea with trees to pulverize, and silex to precipitate, alternately; and neither beast, nor bird, nor rock, nor flint, nor gravel, to disturb their continuity: we must likewise observe, that the coal, as we now find it, has certain characters, that will presently be mentioned, which perhaps place it at a greater distance from vegetable than from animal substances.

Impressions of plants in the surrounding schist only prove, that such then existed:

What data then have we after all for ascribing to plants the origin of bitumens? a few traces of mosses or ferns, scattered through the leaves of slate, that serve as their envelope? Such vestiges prove at most, that nature, during the period of these great operations, likewise made plants grow, and nourished animals in the seas, since we find shells in the strata that separate the coal in some countries; but not

not that it reaped from those plants sufficient materials to fill those inexhaustible lakes of bitumen, that intersperse our globe from one pole to the other, and which the generations yet to be born will perhaps never exhaust. They would prove too, if this could be doubted, that it did not fabricate this composition, as it does that of minerals, in the interior of the earth, but on its surface only; that is, in the region it has chosen for the existence of organized beings.

and that coal origiuated on the surface of the Earth.

It is true, that trunks of trees are found in veins of coal: consequently trees existed at that period. But are these trunks themselves coal? Have they been analysed with a view to compare their products, and examine whether similar changes have taken place in these trees, and in those that are supposed to have been converted into coal? The importance of analysing fossil wood in this respect appears obvious. If it were once demonstrated, that a fossil trunk of a tree contains charcoal in the same proportion as the bitumens surrounding it; and that this charcoal, beside its degree of concentration, is combined with a fresh dose of nitrogen, so as to have lost that prompt and easy combustibility, which characterizes the charcoal of our woods; we might flatter ourselves, that we had an argument of great weight in favour of the opinion here attempted to be shaken: and we should be less surprised to find in this astonishing result of their metamorphosis, pitcoal, seventy or eighty per cent of charcoal; that is to say a proportion, which, if it had been that of the vegetables existing before those periods, would appear difficult to reconcile with that elastic and robust organization, which our forest trees require, to raise firm and secure trunk, and resists the storms of an atmosphere agitated like ours. This weak part of the grand problem may soon be elucidated, if our cabinets do not delay the eagerness of chemistry to decipher the medals of this kind they contain: and if natural history, assisted by the light of analysis, do not discover something more satisfactory, than any thing that has yet been advanced respecting the origin of pitcoal, we ought no longer to waste our time in reasoning on this prodigious event in geology, but banish all the learned hypotheses that have been started on the subject,

Trunks of trees found in it, are they really coal?

Analysis of fossil wood important to the question.

Quantity of charcoal in pitcoal inconsistent with such vegetables as ours.

ject, in consort with those romances concerning the origin of metallic veins, to which our age has given birth.

Coal as much resembles an animal as a vegetable product.

I have said, that analysis discovers in these bitumens characters, that do not show a greater affinity to vegetables than to animals. The following are the facts: and I leave it to the learned to decide which way the balance inclines.

Its smell not like that of wood, but friendly to the lungs.

1. The smell that coals exhale when heated is aromatic, succinated, and decidedly resinous: it irritates neither the eyes nor the lungs, like that of wood or vegetables when burned: and this resinous smell has even been considered as friendly to the diseases of the chest.

Softens and agglutinates by heat.

2. All coals soften, lose their shape, run, mould themselves to the shape of the retort, and fill it with a spongy or puffy coal, like that common to mucous substances, resins, indigo, gluten, and animal matters; but not like that of any known wood or plant.

Yields more oil and ammonia, but no vinegar.

3. Distillation obtains from it a lighter, more aromatic, and more abundant oil, than the nonresinous woods used for fuel; and a great deal of water and ammonia, but none of that vinegar, which abounds in the distillation of our vegetables; that empyreumatic acid, which renders their smoke so troublesome and suffocating; vinegar which is formed, whenever oxigen is an integrant part of an organic oxide, and the absence of which in the products of coal would authorise us in a certain degree to doubt, that oxigen is one of its products*.

Coak does not burn like charred wood;

4. The combustion of coak does not at all resemble that of our vegetable charcoals. It is slow and difficult like that of mineral coals, because it likewise contains condensed nitrogen; accordingly it requires a condensed atmosphere to burn it.

affords prussic lixivium;

5. Coak passed through potash always affords a prussic lixivium, which vegetable charcoals in general do not.

cannot be set on fire by nitric acid;

6. Animal charcoal cannot be set on fire by nitric acid, even after it is disazotized by potash: neither can coak, even passed through this alkali.

Pyroligneous acid forms acetate of copper.

* I for some time doubted the fact, that the pyroligneous acid was really vinegar; but I am now convinced of it, as the salt I formed with oxide of copper and the acid of elm displayed its characters after three purifications. It gave large rhombs, differing in no respect from acetate of copper.

A mixture

and mixed with nitre burns difficultly

A mixture of nitre and coak burns with the same difficulty as mixtures of nitre and charcoal of blood, white of egg, indigo, &c.

Coal leaves less ashes than wood: therefore not clay mixed with bitumen.

7. There are few kinds of wood, that do not leave more ashes than pitcoal. Coal therefore is not clay impregnated with bitumen, as some naturalists have thought.

No oxide of manganese.

I have not yet found any oxide of manganese in animal charcoal; and I have sought it equally in vain in the five kinds of coal mentioned above.

Ashes of vegetables differ from those of coal.

8. Vegetable ashes contain a great deal of carbonate of lime, beside magnesia, alumine, and silex. Those of the five coals I examined afforded me only a great deal of silex, a little magnesia, alumine, and sulphate of lime, but very little carbonate; and in particular not an atom of those salts, which are habitually contained in our vegetable ashes; no phosphate, no muriate of soda, though the mud of these bitumens is supposed to have been formed with sea-water.

Contains sulphur.

10. All the soft or liquid parts of animals contain sulphur. They cannot be dissolved in potash, without having the solution loaded with it. Wool cannot be dipped in a bath of litharge and lime without being blackened, in consequence of the sulphuret of lead that adheres to it.

None free from pyrites.

If no pitcoal be absolutely free from pyrites, is it not because the sulphur and iron, those two habitual elements of animal matters, have withdrawn from the organic substance converted into bitumen, to form a separate combination?

Farther differences.

I content myself with bringing together these facts, and refrain from deducing any consequences from them, as they require to be compared with a greater number of coals, that it may be known whether they be as general as I suppose. But we now come to other properties, which separate still more the analogies supposed to exist between pitcoal and vegetables.

Charcoal feebly combined in coal.

If, for example, the carbonaceous principle be an element of their composition, in the same manner, and in the same sense, as it is in our vegetables and animals, we shall show, that it is very feebly combined however, very weakly enchained at least by the hidrogen, nitrogen, and oxigen: I could almost venture to say independant of them, since it may be extracted from pitcoal by means, that certainly would never

never succeed with any vegetable or animal production we know.

Coal treated with nitric acid.

Let powdered coal, such as that of Villanueva, which affords 68 per cent of coak, be heated in nitric acid of 18° or 20°; and thus in a few moments it will be deprived of the property it had before of forming oil, oily gas, and ammonia. The coal thus prepared, washed, and dried, and then exposed to a moderate heat, affords products of a new order, but with indications of those just mentioned, and is reduced to ·66 or ·67 of coak.

reduced to coak.

This not the case even with indigo.

All pitcoals afford this extraordinary result, which is obtained from no organic production known, not even from indigo, though it contains ·72 or ·73 of charcoal, or much more than many kinds of pitcoal.

The same as produced in distillation.

The carbonaceous principle, freed from the other elements of the compound by this method, has all the properties of what would be obtained by the simple distillation of pitcoal; for, if it be treated with potash, it gives a prussic lixivium like coak itself. I proceed to the consequences.

Hence the blackness and brittleness of coal.

If in these bitumens the carbonaceous part be thus feebly enchained by the other elements, and consequently approach in its properties charcoal uncombined, we need not be surprised either at the blackness or fragility of pitcoal,

The charcoal in coal retains some hidrogen and nitrogen,

The carbonaceous principle of organized bodies, freed from the fetters of combination by any means, and consequently brought nearly to its natural density, may not separate totally from the hidrogen and nitrogen, in consequence of the reciprocal affinity of these three combustibles; as is shown by the habitual state of the coal in our fires, which always contains more or less of them: but the same cannot be said of the oxigen. The condensation of the charcoal, carried to the degree that makes it appear black, is a state decidedly opposing the capacity of this principle to adhere to it as easily as the others. This admitted, if the charcoal in these bitumens be near its habitual density, we must not be surprised, that, remaining still united with the nitrogen and hidrogen, it cannot be equally so with the oxigen. On this principle neither is it strange to find, that pitcoal does not contain oxigen in a state to concur, during its distillation,

but no oxigen.

tion, in the formation of vinegar; as it does, when it is a constituent part of our vegetables.

Coak exists in coal not chemically combined.

But lastly, if nitric acid separate from these bitumens a charcoal united with nitrogen, such as distillation would furnish, it must be acknowledged, that coak exists free and condensed in their constitution; but not in that state of perfect combination, or mutual interlacement, which never fails to efface the characters of the elements of organized substances, as they reciprocally mask each other.

Coal retains nothing of the vegetable character.

I have said enough I believe to show, that pitcoal, if it consist of vegetables, has retained nothing of the characters, that would approximate it rather to vegetable than animal substances. Vegetables, animals, bitumens, all have the same elements; that is nitrogen, hidrogen, oxigen, charcoal, sulphur, &c.: but the combination of these elements in pitcoal certainly does not in any respect resemble those, that vitality now forms in the beings arising before our eyes. Where in fact are the vegetables or animals, that contain charcoal simply deposited in their texture, as an oil or resin is in a plant? Besides, what organization could admit without inconvenience such a considerable excess of charcoal as that we see unemployed in these bitumens? Such a profusion certainly could only be an oversight of nature.

Either there must have been organized beings different from the present:

We must therefore stop at one or other of the following consequences. Either nature was once capable of producing beings, the organization of which could admit so large a proportion of charcoal; and then the life, object, end, and means of existence of such beings, could in no respect be compared with those that now share the surface of the earth with us; and on this supposition pitcoal could only be the remains of animals or vegetables, which like so many others have disappeared from it for ever:

or their elements must have been separated, and combined in a different way, to form coal.

Or, if coal have originated from organic productions similar to ours, its interment has not only destroyed all marks of organization, but has displaced their elements, to reframe them, and fabricate with them those fossil masses, which have indeed retained all the combustibility of their nature, but in which we discern no trace of vegetation or animalization, no indication of the part they had to perform on the surface of the earth.

Oxide of Charcoal.

Charcoal unites with oxigen to form an oxide.

Charcoal and oxigen are susceptible of a kind of union, which does not appear to me to have been noticed. It is totally different from carbonic acid, and from gaseous oxide of carbon. These always take place between *carbon* and oxigen; but the other readily admits charcoal, whether azotized or not; such indeed as we burn. The following are the facts.

Coal treated with nitric acid,

gains an increase.

A hundred parts of coal of Villanueva, the coak of which amounts to 68, are increased by the application of nitric acid at 18° or 20° to 120 or 121 parts. Hence it follows, that, if this acid destroy by oxidation all the principles contained in this bitumen between 68 and 80, it leaves in their place first 32, and next 20, of some other principle, the nature of which we shall soon perceive.

The coal of Belmez likewise yields 120 or 121; that of Langreo the same; and the English, which leaves 64 of coak, produces 116, which amount to nearly the same.

This owing to a real combination.

That this is not owing to any thing merely imbibed, or to defective washing of the product, is decisively proved by the following experiment. Five drachms of Villanueva coal became 6 by the application of nitric acid. These 6 drachms were thrown into boiling water; but, after they had been collected and dried, they returned precisely to the weight of 5 drachms. Such a result clearly shows a surcharge, which, being of 50 or 52, amounts to half the coal employed. It is now time to make known its nature.

Oxide of charcoal, heated, in a retort,

detonates obscurely.

Hold in the hand a small retort containing one or two hundred grains of oxided charcoal, its belly being at some distance above a chafingdish, so as to receive a gentle heat, and its beak being kept under water, to give vent to the atmospheric air, which the aqueous vapour begins to expel; and the instant the powder is agitated by a rapid movement of ebullition, move the beak under an inverted jar filled with water. This movement, which raises up the charcoal briskly, is a strong but obscure detonation, which terminates quickly, and without the least danger. At the same time a copious dew is produced, which, by its precipitation at its exit,

exit, always carries some powdered charcoal out of the retort.

Evolves carbonic acid and gaseous oxide.

The gasses in this experiment are such as might easily be foreseen: a mixture of carbonic acid and gaseous oxide of carbon, burning with a blue flame, and not detonating. If it be yellow, it is owing to the charcoal having retained some nitric acid. This is perceived too by its reddening with oxigen gas.

Much water formed.

From the great quantity of water formed during this distillation, I was led at first to believe, that the oxigen of the nitric acid, hidrogen, and charcoal, might form together a sort of union comparable to that of a vegetable oxide, which a higher temperature would occasion to be converted into water, carbonic acid, and gaseous oxide of carbon. And perhaps it is so. Yet the experiment I made immediately with powder of fir charcoal induced me, to consider the phenomena as belonging exclusively to charcoal and oxigen.

Wood charcoal treated with nitric acid.

Let nitric acid of 20° or 25° be boiled on calcined charcoal of fir, elm, or other wood, reduced to powder; the charcoal, after being washed and dried, will commonly have a surcharge of twelve or thirteen per cent. If it be heated with the precautions already given, it will detonate with agitation; and afford the two gasses abovementioned, without any mixture of nitrous gas. Other experiments will be seen presently to prove, that this charcoal is in a state of peculiar combination, and not contaminated by remains of nitric acid.

This more variable.

I cannot fix with precision the method of thus oxiding vegetable charcoal, because I have found it variable. But it is not the same with that of pitcoal, the state of extreme division in which it is when deprived of hidrogen by the acid facilitating that union, so as not to leave us to grope our way.

Oxide of charcoal soluble by potash:

Liquid potash, even at a boiling heat, has no action on pitcoal; but if heated for a moment, in a very dilute state, on its oxide, or that of fir charcoal, a coffee coloured solution holding a considerable quantity is obtained, which is not altered by standing, or by the addition of water.

and by ammonia.

Ammonia acts with equal efficacy on them. A hundred grains of oxide of charcoal from Villaneuva coal dissolved in in it all except 15 grains, perhaps either because they were not oxided, or because the oxigen had accumulated in the rest, to facilitate its solution. From this solution an acid threw down a precipitate, which was black and shining when dry. It did not melt, and exhaled no smell on burning coals.

Oxigenized muriatic acid precipitated it, without acting on the precipitate.

The solution, precipitated, yielded silex, alumine, and iron.

This same charcoal, oxided, dissolved, and precipitated, left when burnt gray ashes, a little ferruginous, in which I found, silex, alumine, and a little oxide. What sort of union is there then between charcoals and their ashes? We should suppose potash could not dissolve the latter. I applied nitric acid to different sorts of charcoal, to divest them of ashes, and try whether they would burn away entirely; but I could not succeed.

What sort of combination is this?

I had intended to pursue the inquiry farther, but have been prevented. Meantime it will naturally be asked, what combination of oxigen and charcoal is this, that excludes neither nitrogen, nor any of the foreign substances it usually contains? and again what kind of union can charcoal form with the same principle, without losing any thing of its density or colour, which appears so little conformable to the laws of combination? I can only say, I know nothing on the subject.

We know less of charcoal than of carbon.

I would likewise ask, what is the nature of the combination that charcoal enters into with hidrogen and nitrogen, and other gasses which it absorbs and condenses in such large quantity? In fact we are far from being as well acquainted with charcoal, which we see daily, as with carbon, which never comes under the cognizance of our senses.

Dishidrogenized coal.

Coal divested of hidrogen.

When we consider how much the proportion of coak varies in pitcoals, we shall find it difficult not to believe, that, if these bitumens be really derived from plants like ours, the concentration of the carbonaceous principle, which is nothing but the loss of hidrogen, must have varied considerably in different countries. A coal that yields ·77 of coak cannot contain

contain as much hidrogen as another, that yields but ·57 or ·60.

This diminution of hidrogen has been carried so far in some coals, that from a total loss of hidrogen they are pure charcoal.

Nitrogen adheres to it more strongly.

But if hidrogen have been capable of withdrawing entirely from certain coally masses; which will not appear surprising: when we recollect, that the carbonaceous principle in them is very near that degree of condensation which does not admit combination; it is not the same with nitrogen. This appears to be the last of the movable principles to quit it.

Native coak.

Near the monastery of Harbas, in the mountainous defile that separates the Asturias from the province of Leon, there is a vein of coal, perfectly resembling that of Quiros in its foliated texture and shining blackness. In this the carbonaceous principle is ·93, while the hidrogen is absolutely 0. In fact it is a true native coak, that burns without any bituminous or sulphurous smell, and leaves ·07 of white ashes.

Still contains nitrogen.

If, notwithstanding all its characters combined, it can be supposed, that this was never the base of a bitumen, I would urge the following experiment. Let it be heated with potash, and prussic lixivium will be obtained from it. The nitrogen, though the sole remain of the principles that have disappeared, here assists the chemist to trace its origin. It incontestably indicates, that it has belonged to that organic matter, which the hand of time has decomposed, to reduce to pitcoal.

Jet.

Trunks of trees half jet, half wood!

It is said that trunks of trees are found in collections, one extremity of which is converted into jet, while the other is still ligneous. If it be so, the analysis of such specimens would be exremely interesting.

Jets differ.

There is a great deal of difference in jets. A jet from Almagra in Murcia gave ·46 of charcoal without even softening: another, wrought into buttons, melted like a resin, and yielded ·52.

The following results to a certain point assimilate jet with our vegetables.

Heat

Its resemblance to vegetables.

Heat it with an acid at 20°, immediately a bulky effervescence takes place. As the solution advances, a deep yellow or annotta coloured concretion forms. The ebullition should be continued a little while, to collect all that can be formed. This substance, which is soft while the liquor is hot, is easily softened and separated. On washing it in boiling water, this acquires a yellow colour, but does not dissolve it. When dry it is bulky, bitter, and soluble in alcohol. But what is remarkable is its property of detonating obscurely with a very moderate heat like oxide of charcoal, either in a retort, or held over the flame of a candle on paper.

Products.

Its products are water, a little oil, ammonia, carbonic acid gas, gaseous oxide of carbon burning with a blue flame, and ·40 of a coal more bulky than the original substance.

The liquor that remains after its separation is of a deep yellow, and very bitter. It yields crystallized oxalic acid, and benzoic acid.

Compared with coal.

Pitcoal heated with an acid of 40° dissolves slowly, does not afford any charcoal, and yields the detonating substance, but with more difficulty, and in less quantity, than jet. Cannel coal comports itself like common coal, and not like jet. Some jet however does not yield the detonating substance without an acid of 40°, or with as much difficulty as pitcoal.

Hidrogen not necessary to the oxide of charcoal.

If we consider, that the first action of nitric acid on pitcoal consists in destroying the hidrogen, we shall easily conceive, that this hidrogen is not necessary to the formation of the detonating substance. The following experiment evidently shows this. Oxided Villanueva coal certainly contains no hidrogen: yet, treated with an acid of 40°, it affords the detonating substance; and since this yields water, ammonia, oil, &c., it is evident, that the nitrogen, the oxigen of the nitric acid, and the charcoal which the water is capable of furnishing by the concurrence of affinities that bring on its decomposition, establish themselves in a fixed proportion, to give rise to this singular product. A vegeto-animal compound, an artificial oxide of this nature, removes the difficulties I at first found in the formation of tannin from the simple presence of nitric acid and charcoal.

Illustrates the formation of tannin.

Acid

Acid of 35° or 40°, heated with fir or elm charcoal, at first gives out the gasses, that might be expected: but at a certain point the gasses cease to be formed, and the charcoal enters into solution. I have very old solutions of this kind, that are not altered. Alkalies precipitate nothing from them, because charcoal, either oxided or in a state of simple division, is as soluble in alkalis as in acids. I had proceeded thus far, when I learned from the Chemical Annals, that tannin was formed by the solution of carbonaceous substances. I believe Crell is the first, who announced the solution of charcoal in nitric acid.

Nitric acid dissolves charcoal.

A hundred grains of Villanueva coal, treated with concentrated sulphuric acid, and perfectly washed and dried, left 104. Was this coal oxided? I find nothing mentioned in my notes, but that it burned without any bituminous smell, that of sulphur only being noticeable.

Coal treated with sulphuric acid.

Turf.

A Piece of turf from Dax afforded me the following results.

Turf

A hundred parts left 40 of charcoal without any change of bulk. Its products were water, and vinegar mingled with ammonia, the taste of which did not differ from that of wood: but a yellow oily vapour came over, that became hard like suet, which does not commonly occur in the distillation of wood. This suet was from 6 to 6½ per cent. I did not examine the gasses.

yields ·40 of charcoal, water, vinegar, ammonia, and ·6 of a sebaceous oil.

Potash, which has not the least action on pitcoal, to my astonishment perfectly dissolved the turf. The solution is coffee-coloured. Acids decompose it, throwing down a brown flocculent precipitate, which distillation renders black, shining, and fragile. When distilled it is converted into charcoal, without softening or diminishing in bulk; yields the butyraceous product; and is reduced to ·50 of charcoal.

Potash dissolves turf.

Nitric acid at 30° does not decompose turf, as it does pitcoal. Washed and dried it affords water, and the butyraceous matter, but no acid. It might be supposed, that this suet exists ready formed in turf. This deserves to be farther

Nitric acid does not decompose it.

ther examined, and particularly to be compared with those plants, the bituminization of which is not far advanced, to see what changes its progress effects in their constitution.

By boiling dissolves it.

Nitric acid of 40°, with a boiling heat, dissolves turf. Crystallizable oxalic acid is obtained from it, and the yellow bitter matter; but no detonating product, like that of jet. I know not whether Hatchett's tannim be found in it.

Incinerated.

Its incineration is tolerably slow. Its coal does not emit the ammoniacal smell of azotized charcoals. Its ashes are gray, without the least indication of saline matter or lime. Their lixivium does not alter the juice of the blue-bottle. Acids do not occasion the slightest effervescence with them. They contain a great deal of silex, sulphate of lime, and a little magnesia.

II.

Abstract of a Memoir on Muriatic Ether, read at the Institute February the 17th, 1807, *by Mr.* THENARD*.

Muriatic ether hitherto unknown.

AFTER having examined why muriatic ether has remained hitherto unknown to chemists, though it has been repeatedly an object of their research, the author gives the process for obtaining it. As it is habitually in the state of gas, the following apparatus must be employed.

A gas at the common temperature.

Arrangement of the apparatus for obtaining it.

Into a retort, capable of containing no more than the mixture in its belly, equal parts by measure of highly concentrated muriatic acid and alcohol at 36° are to be put, and well shaken, to bring all the particles of each into contact. This done 7 or 8 grains of sand at most are to be thrown into the retort, to prevent the sudden ebullitions that might otherwise take place in the course of the process: after which it is to be supported on the naked fire of a common furnace by a grate of iron wire, and a Welter's tube adapted to it, terminating in a threenecked bottle, the capacity of which is double that of the retort, and which must be half

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* Annales de Chimie, vol. LXI, p. 291, March, 1807: and Journal de Physique, vol. LXIV, p. 260.

filled

filled with water at 20° or 25° [66° or 72° F.]. The tube must be immersed in the water to the depth of 7 or 8 cent. [$2\frac{1}{2}$ or 3 in.]; a straight tube of safety must be introduced into the central tubulure; and from the third a curved tube must proceed, opening under inverted phials filled with water at the same temperature in an earthen bowl.

Process.

Proportion of gas obtained.

Probably still more by particular management.

Great attention to the fire requisite.

If the acid, or both, be in the state of gas, little or no ether will be formed.

The water dissolves a certain quantity.

The apparatus being thus arranged, the retort is to be heated gradually; and twenty or five and twenty minutes after the fire is kindled bubbles will be seen to rise from the lower part of the fluid, particularly from the surface of the grains of sand. These bubbles presently become more numerous, and abundance of ethereous gas is soon obtained. Acid, alcohol, and water, at the same time pass over, but remain in the first phial. From 500 gr. [$7722\frac{1}{2}$ grs.] of acid, and an equal bulk of alcohol, upward of 20 litres [wine quarts], or even as far as 30, of ethereous gas, perfectly pure, may be obtained. Much more will be obtained, if, as soon as the extrication of gas begins to slacken, fresh alcohol be added to the residuum; that is, to the strongly acid residuum, which remains in the retort, and will then be about two fifths of the bulk of the original mixture. I even think, that, if hot alcohol were occasionally poured into the retort through a tube 6 or 7 decim. [24 or 27 in.] long reaching to its bottom, the formation of etherized gas would be still more abundant; for it is obvious, that more alcohol than muriatic acid rises every instant, thus therefore we should reestablish their original proportions, which are best adapted to the success of the process. In all cases the management of the fire is of the highest importance: if it be too weak, it will produce no etherized gas; if it be too strong, it will produce but little. Neither will the alcohol be etherified in any sensible degree by loading it with muriatic acid gas, or by causing the alcohol and acid both to meet in vapour in a tube about 80° [$174\frac{1}{2}$° F.]. It is only therefore by maintaining a due medium in the application of the fire, that we can succeed completely. The cause of this is, that too great or too little elasticity in the alcohol and in the muriatic acid is injurious to their mutual action on each other. Another precaution to be taken is, to use the same water for collecting

lecting the gas, and to employ as little as possible, because it dissolves a certain quantity of it.

Characters of the gas.

The gas is perfectly colourless; it has a strong smell of ether; and its taste is perceptibly saccharine. It has no action whatever on infusion of litmus, sirup of violets, or limewater. Its specific gravity, compared with that of the air, is 2·219 at 18° [63° F.], and 75 cent. [29·4 in.] of atmospheric pressure. At this pressure water dissolves its own bulk. At the same pressure, but at the temperature of 11° [50° F.], the gas assumes the liquid state.

Method of obtaining the ether in a liquid state.

A large quantity may be procured in this state by employing an apparatus similar to that already described; only instead of causing the last tube to open under a bottle filled with water it must be made to reach to the bottom of a long, narrow vessel, well dried, and surrounded with ice, which must be renewed as it melts. The ethereous gas will arrive alone in this, and be completely condensed; for as soon as all the common air is expelled from the vessels, their communication with the atmosphere may be cut off without danger.

Characters of the liquid ether.

This ether in the liquid state is remarkably limpid. Like the gas it is colourless, and has no action on litmus or sirup of violets: is very soluble in alcohol, from which it may in great part be separated by water: has a very decided smell, and a very distinct taste resembling that of sugar, which is particularly observable in water saturated with it. Probably it may thus be employed with success as a medicine. Poured on the hand it suddenly enters into ebullition, and produces considerable cold, leaving a small whitish sediment. At the temperature of 5° [39·2° F.] its weight is to that of water as 874 to 1000. Thus, though it is much more volatile than sulphuric ether, and of course than alcohol, it is not only heavier than the first, but even than the second of these. Finally it does not congeal at a temperature of – 29° [22·2° below 0 F.].

Thus far we see nothing in this ether but agrees with the phenomena exhibited by other bodies. It is an object of curiosity only on account of its novelty, and the facility with which it is converted into a gas or a liquid. But if we study it farther, it will appear one of the most singular and extraordinary compounds we can form. It does not redden the

A singular compound.

the most dilute infusion of litmus; the strongest alkalis have no action on it; the solution of silver is not rendered in the least turbid by it: and all this whether it be used in the gaseous or liquid state, or dissolved in water. But set it on fire, and immediately such a large quantity of muriatic acid is developed, that it precipitates a concentrated solution of nitrate of silver in a solid mass, suffocates those who inhale it, and is even visible in the form of vapour in the surrounding air.

No test indicates the presence of muriatic acid in it, yet it gives out a great quantity when burned.

Is the muriatic acid formed in this combustion, as we might be tempted to suppose? or is it only set at liberty, which is possible? These questions Mr. Thenard afterward endeavours to solve.

Is the acid then formed, or only set free?

If the muriatic acid be formed in the combustion of the ethereous gas, the radical of this acid must exist in the gas; and it must necessarily proceed from the alcohol, or from the muriatic acid decomposed by the alcohol, or, which is improbable, though not impossible, from both together. In the first case, on distilling a mixture of muriatic acid and alcohol, we should find after the distillation all the muriatic acid employed, beside what arises from the combustion of the gas formed. In the second case, on the contrary, a large quantity of acid ought to disappear in the distillation: but the whole of this quantity, and no more, ought to reappear on the combustion of the acid formed. In the third case, a loss of acid should be occasioned by the distillation; but this loss should be more than compensated by the quantity of acid, which is produced from the combustion of the gas formed.

If the former, its radical must come from the alcohol, the acid, or both.

Tests of these three suppositions.

Now if the process be performed with 450·937 gram. [6962·722 grs.] of muriatic acid, of the specific gravity of 11·349, at temperature 5° [39·2° F.], with an equal bulk of highly rectified alcohol, 23 lit. [quarts] of ethereous gas will be formed at the temperature of 21° [68° F.]. and pressure ·745 met. [29·2 in.], and 122·288 gram. [1888·738 grs.] of acid disappear. Consequently the first supposition is false, since it is demonstrated, that, if the radical of the muriatic acid exist in the ethereous gas, it must proceed, not from the alcohol solely, but either from the muriatic acid alone, or from the muriatic acid and alcohol.

Proof that the radical does not come from the alcohol alone.

Let

It might proceed from the acid alone in two ways.

Let us see whether it can proceed from the muriatic acid alone, agreeably to the second supposition. There are two ways in which this may be conceived to take place: either the muriatic acid may have been decomposed by the alcohol so that its radical is found in the ethereous gas separated from its other principle: or the decomposition may have taken place in such a way, that both the principles of the muriatic acid exist in the ethereous gas, not united together, not forming muriatic acid, but combined with the principles of the alcohol, in the same state in which hidrogen, oxigen, carbon, and azote, are found in vegetable and animal substances.

If the radical alone enter into the gas, it cannot reproduce the acid when burned without access of air.

Now if the radical of the muriatic acid exist alone, or without some part of the other principle, in the ethereous gas, we ought to obtain no acid, when we decompose this gas in a redhot tube with exclusion of air, or less than disappeared in the experiment that produced it. But if the gas contain not only the radical of the muriatic acid, but all its constituent principles; as the principles of this acid, whatever they are, have a great tendency to combine together, we may presume, that, on destroying the ethereous gas by fire without the contact of air, we shall probably obtain the whole quantity of muriatic acid, that disappeared in the experiment in which the gas was formed.

If all its principles enter into it, the whole may thus be reproduced.

The gas decomposed in close vessels.

It was of the highest importance therefore, to effect this decomposition in close vessels. This was accordingly done with 900 gram. [29 oz. troy] of concentrated muriatic acid, and an equal bulk of well rectified alcohol. Between the redhot tube of glass, in which the gas was decomposed, and the retort where it was produced, a large tubulated bottle was placed containing water at 15° or 16° [57° or 59° F.], to retain the acid, alcohol, and water, that might rise with the gas. The glass tube had a communication also with two other bottles, one containing water, the other potash, to absorb all the acid that might reappear in the operation. Lastly by means of another tube the gasses were collected. That this operation may be attended with success, the glass tube must be well coated, and the fire cautiously managed, to prevent it from melting. Though near

near 50 litres of ethereous gas must have been produced in this experiment, and near 250 gram. of acid have disappeared in the first instance, yet the whole of the acid, except 4 gram. [62 grs.] reappeared in the redhot tube, and were dissolved in the last two bottles of the apparatus.

Nearly the whole of the acid reproduced.

Thus of all the suppositions above made, which are the only ones that can reasonably be formed considering the muriatic acid as a compound, there is only one admissible; which is, that the elements of the muriatic acid exist in the ethereous gas combined with those of the alcohol, in the same manner as the elements of water, carbonic acid, ammonia, &c., exist in vegetable and animal substances.

The elements of the acid therefore exist in the gas, if it be a compound.

But if we suppose the muriatic acid to be a simple substance, we must necessarily consider the ethereous gas as formed of muriatic acid and alcohol, or as a substance derived from the decomposition of the alcohol: for perhaps the alcohol is decomposed when we distill it with muriatic acid, at least this will easily be seen by and by. The question therefore being reduced to a choice between these two hypotheses, let us endeavour as far as possible to discuss their weight.

Supposition that the acid is a simple substance.

The latter presents us with phenomena very difficult to explain. In fact we must suppose that the alcohol, or the principle it contains, acts on the muriatic acid with much more energy than the strongest alkali; since this alkali cannot take the acid from it, and muriate of potash, as I shall hereafter show, contains less acid than the ethereous gas. How too can we conceive, that nitrate of silver, which takes the whole of the muriatic acid from muriate of potash, cannot take any from the ethereous gas, which contains still more?

Difficulties that may be objected to it.

On the other supposition, on the contrary, every thing is naturally explained. We see why the ethereous gas does not redden the infusion of litmus; why alkalis do not affect it; why nitrate of silver does not produce a precipitate with it; and why on burning it so large a quantity of muriatic acid is generated, that it appears in the surrounding air in the form of vapour: in short every thing is reconcilable with the phenomena exhibited by other substances.

The other naturally accounts for every thing.

Mr. Thenard however is far from absolutely adopting one hypothesis and rejecting the other. Both deserve to be investigated

Yet it cannot be considered as absolutely decided.

tigated, and on this he is at present eagerly engaged, since, however it may turn out, the results cannot fail to be very important.

Note on the Discovery of the Muriatic Ether; by Mr. THENARD.

The muriatic ether was unknown in France,

When on the 18th of February last I read to the Institute my paper on muriatic ether, all the members, among whom were Messrs. Berthollet, Chaptal, Deyeux, Fourcroy, Guyton, Vauquelin, and Gay-Lussac, considered the results it contained as perfectly novel, and were struck with the consequences, that might be deduced from them. Mr. Proust, who is at present in Paris, and before whom I was eager to repeat, at his desire, the experiments I had made with the ethereous gas, was not less surprised than the French chemists. But last friday, that is twenty-five days after I had read my paper, Mr. Gay-Lussac, turning over Gehlen's Journal, accidentally found in a note, that Gehlen himself had made experiments on muriatic ether, and recorded them in one of the volumes of his Journal published in 1804.

and in Spain; but not in Germany.

Gehlen made it in different ways in 1804.

It appears, that Mr. Gehlen made muriatic ether from equal weights of the fuming muriate of tin and alcohol. He likewise made it in Basse's method, by a mixture of seasalt, concentrated sulphuric acid, and alcohol; from which till Basse's time, and even Gehlen's, sulphuric ether only was supposed to be obtained. He did not obtain any with muriatic acid alone. Mr. Gehlen however observed most of the properties in muriatic ether, that I have mentioned. The chief differences between us are, that he has not attempted to investigate the source of the muriatic acid produced by burning the gas, the quantity it is capable of affording, or the theory of the formation of the ether. My process too not only affords ether in larger quantity probably than any other, but much purer; for the specific gravity of mine was ·874, of his only ·845, and in this case the greater gravity is a proof of greater purity. He likewise marks the point of its condensation as about 10° of Reaumur [54·5° F].

His was less in quantity, and less pure.

As from the account of Mr. Gehlen I could no longer doubt but muriatic ether had been made in Germany, and its property

property of yielding a large quantity of muriatic acid in burning observed; and at the same time convinced, that a fact so important was altogether unknown both in France and Spain; I was desirous of satisfying myself, whether the English chemists were any farther advanced in this respect. For this purpose I addressed myself to Mr. Riffault, who is translating the third edition of Thomson's Chemistry, a work of great erudition, begun long after Gehlen's paper was published. Mr. Riffault read to me every thing in it concerning muriatic ether. No mention is made there of Gehlen, or of the singular properties of the muriatic ether. Hence I consider myself authorized to conclude, that the muriatic ether was unknown in England, as well as in France and Spain; and that, without having information of Gehlen's labours, I may claim at least the merit of having made it known here. How often has it already happened, that a discovery has been made in one country some years after it had been made in another; and this because unfortunately all learned men do not speak the same language, and all the works published in one are far from being translated into the rest.

Not known in England.

Discoveries often made a second time, the first remaining unknown to foreigners.

In an additional note Mr. Thenard says, that, ten days after his paper was read, Mr. Boullay, an apothecary of Paris, acquainted him, that he had likewise formed muriatic ether directly from muriatic acid and alcohol, but had not made it public, because he had not yet completed his labours on the subject.

Mr. Boullay too had made it.

III.

Abstract of a Memoir on the Products that result from the Action of Metallie Muriates, oxigenized Muriatic Acid, and Acetic Acid, on Alcohol: by Mr. THENARD*.

IN this paper Mr. Thenard shows, that the metallic muriates form but a very small quantity of ether with alcohol: that this ether, which at first is found dissolved in a large quantity of alcohol, may be separated from it in the state of gas

Metallic muriates form but little ether with alcohol.

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* Annales de Chimie, vol. LXI. p. 308, March, 1807.

gas by a gentle heat; particularly by means of hot water, which seizes on the alcohol, and to a certain point sets the ether at liberty: that this ethereous gas has the greatest analogy with that which is obtained from muriatic acid and alcohol: that both have the same smell, taste, and solubility in water, and burn in the same manner with a green flame, and diffusing vapours of muriatic acid, though previous to the combustion no test can detect the presence of any in the gas: and that they differ only in this, that the muriatic ethereous gas is not condensible at a temperature above 12·5° [52·7° F.], while the other is at 16·5° [60° F.]

Not so volatile as that from the acid.

Formed wholly from the excess of acid in the muriate.

This difference being but slight, Mr. Thenard conceives, that the nature and mode of formation of both are the same; and that in the metallic muriates it is only the excess of acid that acts on the alcohol. For this reason a large quantity of metallic muriate is necessary to convert alcohol into ether; and it is the more easily effected, in proportion as the muriate contains a greater excess of acid, and is more soluble in alcohol. Hence the process succeeds better with the muriate of tin than with any other. In every case the muriate is not disoxigenized, and a portion of the oxide only is found to be precipitated.

Oxigenized muriatic acid decomposed by alcohol.

Considering afterward the action of oxigenized muriatic acid on alcohol, he shows, that in the mutual action of these two substances on each other, which is very powerful, almost all the oxigenized muriatic acid is decomposed: and the result is a great deal of water; a great deal of muriatic acid; some alcohol not decomposed; a tolerable quantity of an oily matter heavier than water, having a cool taste analogous to that of mint, a peculiar smell differing from that of ether; and beside these a small quantity of carbonic acid, of a substance easily carbonized, and probably of acetous acid, but no ether. Farther, that the oxigenized muriatic ether of Scheele is nothing but muriatic ether properly so called, when made with a mixture of alcohol, muriatic acid, and black oxide of manganese; or a mixture of muriatic and sulphuric ether, when made from black oxide of manganese, common salt, alcohol, and sulphuric acid: that Pelletier's is of the same nature, since he made it of a similar mixture: and that what

But no ehter produced.

Mistake of Scheele in this respect.

Solution of a peculiar oil in alcohol mistaken for ether.

what is said to be abtained by passing oxigenized muriatic acid gas through alcohol is nothing but a solution of more or less of the oily matter in alcohol. The oil may even be separated from the alcohol by the addition of water, and the same compound formed again by dissolving this oil in a given quantity of alcohol.

What is new in this part of Mr. Thenard's labours is not the formation of the oily matter, water, acetous acid, &c., by the decomposition of oxigenized muriatic acid: for Scheele, in his Chemical Essays, speaks of the oily matter; and Berthollet, in the Memoirs of the Academy for 1785, speaks not only of this, but of the water, acetous acid, &c., formed in the process: but it is the having proved*, that oxigenized muriatic acid cannot form ether with alcohol, and having explained why Scheele and so many other chemists did obtain some.

Acetic ether.

Finally, desirous of examining the formation of acetic ether, Mr. Thenard mixed together 120 gram. [1853 grs.] of alcohol as highly rectified as possible, and as much acetic acid, of an acidity determined by the quantity of potash the acid requires to saturate it. He distilled the mixture, cohobated it twelve times, and thus evidently decomposed all the alcohol employed, beside 66·16 gram. [1022 grs.] of acetic acid, answering to 32 gram. [509 grs.] of dry acid, or such as it exists in acetate of potash well fused. Yet about 120 gr, [1853 grs.] only of acetic ether were formed; though no gas was evolved, and, when the process was finished, a loss of 7 gram. [108 grs.] only appeared. Hence Mr. Thenard is led to suppose, that part of the oxigen of the acetic acid combines with part of the hidrogen of the alcohol, while the other principles of the acid, and those of the alcohol, unite to form the ether. Otherwise, if no water were formed, we must admit, in order to account for the appearances, that the best rectified alcohol contains nearly a fifth of its weight of water, which is scarcely probable.

Water formed in this process.

This

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* Mr. Berthollet even mentioned, that the oxigenized muriatic acid and alcohol produced but very little ether; and we perceive, that he inclined to consider this small quantity of ether as foreign to the mutual action of these two substances.

Characters of acetic ether.

This ether has an agreeable smell of ether and acetic acid; yet it reddens neither the infusion nor paper of litmus: and it has a peculiar taste, not very different from that of alcohol. Neither its specific gravity nor degree of elasticity was accurately ascertained: but it is lighter than water, and swims on it, and heavier than alcohol. Water appears to dissolve much more than it does of sulphuric ether. It burns with a yellowish white flame, and produces an acid, which is probably the acetic. It does not appear to undergo any alteration by keeping; at least it did not in the course of six months.

IV.

Some Hints respecting the proper Mode of inuring Tender Plants to our Climate.

By the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P. R. S. &c*[*].

Interesting and advantageous to harden tender plants.

RESPECTABLE and useful as every branch of the horticultural art certainly is, no one is more interesting to the public, or more likely to prove advantageous to those who may be so fortunate as to succeed in it, than that of inuring plants, natives of warmer climates, to bear without covering the ungenial springs, the chilly summers, and the rigorous winters, by which, especially for some years past, we have been perpetually visited.

Attempts in this way.

Many attempts have been made in this line, and several valuable shrubs, that used to be kept in our stoves, are now to be seen in the open garden; there is, however, some reason to believe, that every one of these was orginally the native of a cold climate, though introduced to us through the medium of a warm one; as the gold tree, aucuba japonica, the moutan, pæonia frutescens, and several others have been in our times.

Annuals.

In the case of annuals, however, it is probable that much has been done by our ancestors, and something by the present generation; but it must be remembered, that all that is required in the case of an annual is, to enable it to ripen

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[*] From the Transactions of the Horticultural Society, Part I, p, 21.

its fruit in a comparatively cold summer, after which we know that the hardest frost has no power to injure the seed, though exposed in the open air to its severest influence; but a perennial has to encounter frosts with its buds and annual shoots, that have sometimes been so severe with us, as to rend asunder the trunks of our indigenous forest trees.

Wheat.

It is probable that wheat, our principal food at present, did not bring its seed to perfection in this climate, till hardened to it by repeated sowings; a few years ago some spring wheat from Guzerat was sown with barley, in a well cultivated field: it rose, eared, and blossomed, with a healthy appearance, but many ears were when ripe wholly without corn, and few brought more than three or four grains to perfection.

Water oats

In the year 1791, some seeds of zizania aquatica were procured from Canada, and sown in a pond at Spring Grove, near Hounslow; it grew, and produced strong plants, which ripened their seeds; those seeds vegetated in the succeeding spring, but the plants they produced were weak, slender, not half so tall as those of the first generation, and grew in the shallowest water only; the seeds of these plants produced others the next year sensibly stronger than their parents of the second year.

Gradually acclimated,

In this manner the plants proceeded, springing up every year from the seeds of the preceding one, every one becoming visibly stronger and larger, and rising from deeper parts of the pond, till the last year, 1804, when several of the plants were six feet in height, and the whole pond was in every part covered with them as thick as wheat grows on a well managed field.

till perfectly vigorous in 14 years.

Here we have an experiment which proves, that an annual plant, scarce able to endure the ungenial summer of England, has become, in fourteen generations, as strong and as vigorous as our indigenous plants are, and as perfect in all its parts as in its native climate.

Bay tree.

Some of our most common flowering shrubs have been long introduced into the gardens; the bay tree has been cultivated more than two centuries; it is mentioned by Tusser, in the

list

list of garden plants inserted in his book, called 500 points of good Husbandry, printed in 1573.

Laurel.

The laurel was introduced by Master Cole, a merchant living at Hampstead, some years before 1629, when Parkinson published his Paradisus Terrestris, and at that time we had in our gardens, oranges, myrtles of three sorts, laurustinus, cypress, phillyrea, alaternus, arbutus; a cactus brought from Bermudas, and the passion flower, which last had flowered here, and showed a remarkable particularity, by rising from the ground near a month sooner if a seedling plant, than if it grew from roots brought from Virginia.

Orange, myrtle, &c.

Passion flower remarkable particularity.

All still tender.

All these were at that time rather tender plants; Master Cole cast a blanket over the top of his laurel, in frosty weather, to protect it, but though nearly two centuries have since elapsed not one of them will yet bear with certainty our winter frosts.

Have not been propagated by English seed.

Though some of these shrubs ripen their seeds in this climate, it never has been, I believe, the custom of gardeners to sow them; some are propagated by suckers and cuttings, and others by imported seeds; consequently the very identical laurel introduced by Master Cole, and some others of the plants enumerated by Parkinson, are now actually growing in our gardens; no wonder then, that these original shrubs have not become hardier, though probably they would have done so, had they passed through several generations by being raised from British seeds.

Plants propagated by cuttings or suckers do not grow hardy. Seed therefore should be tried.

Is it not then worthy a trial, as we find that plants raised from suckers or cuttings do not grow hardier by time, and as the experiment on zizania points out the road, to sow the seeds of these and such tender shrubs as occasionally ripen them in this climate. Fourteen generations, in the case of the zizania, produced a complete habit of succeeding in this climate, but a considerable improvement in hardiness was evident much earlier.

In plants that require some years to arrive at puberty, fourteen generations is more than any man can hope to survive; but a much less number will in many cases be sufficient, and in all, though a complete habit of hardihood is not attained, a great progress may be made towards it in a much less

less time; even one generation may work a change of no small importance, if we could make the myrtle bear the climate of Middlesex, as well as it does that of Devonshire, or exempt our laurel hedges from the danger of being cut down by severe frosts, it would be an acquisition of no small consequence to the pleasure of the gentleman, as well as to the profit of the gardener.

Myrtle and laurel begun to be tried.

Old as I am, I certainly intend this year to commence experiments on the myrtle and the laurel: I trust, therefore, it will not be thought presumptuous in me to invite those of my brethren of this most useful Society, who are younger than I am, and who of course will see the effect of more generations than I shall do, to take measures for bringing to the test of experiment the theory I have ventured to bring forward, I hope not without some prospect of success.

New Holland promises many plants.

The settlement lately made at New Holland gives a large scope to these experiments; many plants have been brought thence which endure our climate with very little protection, and some of these arrive at puberty at an early period; we have already three from the south point of Van Diemen's Island, where the climate cannot be wholly without frost; mimosa verticillata, eucalyptus hirsuta and obliqua. The first of these appears to have produced flowers within eight years of its first introduction, but as a settlement is now made very near the spot where the seeds of these shrubs were collected, we may reasonably hope to receive farther supplies, and, among them, the winterana aromatica, an inhabitant of the inhospitable shore of Terra del Fuego, which Mr. Brown has discovered on the south part of Van Diemen's Island also.

V.

Observations on the Method of producing new and early Fruits. *By* Thomas Andrew Knight, *Esq: F. R. S. &c.*[*]

Gifts of nature to be improved by man.

NATURE has given to man the means of acquiring those things which constitute the comforts and luxuries of civilized life

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[*] Transactions of the Horticultural Society, Part I, p. 30.

life, though not the things themselves; it has placed the raw material within his reach; but has left the preparation and improvement of it to his own skill and industry. Every plant and animal, adapted to his service, is made susceptible of endless changes, and, as far as relates to his use, of almost endless improvement. Variation is the constant attendant on cultivation, both in the animal and vegetable world; and in each the offspring are constantly seen, in a greater or less degree, to inherit the character of the parents from which they spring.

Fruits best in peculiar soils, situation, and management.

No experienced gardener can be ignorant, that every species of fruit acquires its greatest state of perfection in some peculiar soils and situations, and under some similar mode of culture: the selection of a proper soil and situation must therefore be the first object of the improver's pursuit; and nothing should be neglected which can add to the size, or improve the flavour of the fruit from which it is intended to propagate. Due attention to these points will in almost all cases be found to comprehend all that is necessary to insure the introduction of new varieties of fruit, of equal merit with those from which they spring; but the improver, who has to adapt his productions to the cold and unsteady climate of Britain, has still many difficulties to contend with; he has to combine hardiness, energy of character, and early maturity, with the improvements of high cultivation. Nature has, however, in some measure, pointed out the path he is to pursue; and, if it be followed with patience and industry, no obstacles will be found, which may not be either removed, or passed over.

Hence new varieties.

Hardiness and early ripening desirable.

Plants carried to a hot or cold climate and brought back.

If two plants of the vine, or other tree, of similar habits, or even if obtained from cuttings of the same tree, were placed to vegetate, during several successive seasons, in very different climates; if the one were planted on the banks of the Rhine, and the other on those of the Nile, each would adapt its habits to the climate in which it was placed; and if both were subsequently brought, in early spring, into a climate similar to that of Italy, the plant which had adapted its habits to a cold climate would instantly vegetate, whilst the other would remain perfectly torpid. Precisely the same thing occurs in the hothouses of this country, where a plant accustomed

Hothouses.

accustomed to the temperature of the open air will vegetate strongly in December, whilst another plant of the same species, and sprung from a cutting of the same original stock, but habituated to the temperature of a stove, remains apparently lifeless. It appears, therefore, that the powers of vegetable life, in plants habituated to cold climates, are more easily brought into action than in those of hot climates; or, in other words, that the plants of cold climates are most excitable: and as every quality in plants becomes hereditary, when the causes which first gave existence to those qualities continue to operate; it follows that their seedling offspring have a constant tendency to adapt their habits to any climate in which art or accident places them.

Plants from cold climates earliest.

But the influence of climate on the habits of plants, will depend less on the aggregate quantity of heat in each climate, than on the distribution of it in the different seasons of the year. The aggregate temperature of England, and of those parts of the Russian empire, that are under the same parallels of latitude, probably does not differ very considerably; but, in the latter, the summers are extremely hot, and the winters intensely cold; and the changes of temperature between the different seasons are sudden and violent. In the spring, great degrees of heat suddenly operate on plants which have been long exposed to intense cold, and in which excitability has accumulated during a long period of almost total inaction; and the progress of vegetation is in consequence extremely rapid. In the climate of England, the spring, on the contrary, advances with slow and irregular steps, and only very moderate and slowly-increasing degrees of heat act on plants in which the powers of life have scarcely in any period of the preceding winter been totally inactive. The crab is a native of both countries, and has adapted alike its habits to both; the Siberian variety introduced into the climate of England retains its habits, expands its leaves, and blossoms on the first approach of spring, and vegetates strongly in the same temperature in which the native crab scarcely shows signs of life; and its fruit acquires a degree of maturity, even in the early part of an unfavourable season, which our native crab is rarely or never seen to attain.

Not the aggregate quantity of heat, but its distribution through the season, the chief point. England and Russia.

Siberian crab.

Annuals.

Similar causes are productive of similar effects on the habits of cultivated annual plants; but these appear most readily to acquire habits of maturity in warm climates; for it is in the power of the cultivator to commit his seeds to the earth at any season; and the progress of the plants towards maturity will be most rapid, where the climate and soil are most warm. Thus the barley grown on sandy soils, in the warmest parts of England, is always found by the Scotch farmer, when introduced into his country, to ripen on his cold hills earlier than his crops of the same kind do, when he uses the seeds of plants, which have passed through several successive generations in his colder climate; and in my own experience, I have found that the crops of wheat on some very high and cold ground, which I cultivate, ripen much earlier when I obtain my seed-corn from a very warm district and gravelly soil, which lies a few miles distant, than when I employ the seeds of the vicinity.

Seeds from a warm climate ripen earliest.

Esculent plants.

The value, to the gardener, of an early crop, has attracted his attention to the propagation and culture of the earliest varieties of many species of our esculent plants; but in the improvement of these he is more often indebted to accident than to any plan of systematic culture; and contents himself with merely selecting and propagating from the plant of the earliest habits, which accident throws in his way; without inquiring from what causes those habits have arisen: and few efforts have been made to bring into existence better varieties of those fruits which are not generally propagated from seeds, and which, when so propagated, of necessity exercise, during many years, the patience of the cultivator, before he can hope to see the fruits of his labour.

Attempts to produce early varieties of fruit.

The attempts which I have made to produce early varieties of fruit are, I believe, all that have yet been made; and though the result of them is by no means sufficiently decisive to prove the truth of the hypothesis I am endeavouring to establish, or the eligibility of the practice I have adopted, it is amply sufficient to encourage future experiment.

Apples.

The first species of fruit, which was subjected to experiment by me, was the apple; some young trees of those varieties

rieties

rieties of this fruit, from which I wished to propagate, were trained to a south wall, till they produced buds which contained blossoms. Their branches were then, in the succeeding winter, detached from the wall, and removed to as great a distance from it as the pliability of their stems would permit; and in this situation they remained till their blossoms were so far advanced in the succeeding spring, as to be in some danger of injury from frost. The branches were then trained to the wall, where every blossom I suffered to remain, soon expanded, and produced fruit. This attained in a few months the most perfect state of maturity; and the seeds afforded plants, which have ripened the fruit very considerably earlier than other trees, which I raised at the same time, from seeds of the same fruit, which had grown in the orchard. In this experiment the fecundation of the blossoms, of each variety, was produced by the farina of another kind; from which process, I think, I obtained, in this, and many similar experiments, an increased vigour and luxuriance of growth; but I have no reasons whatever to think that plants thus generated ripen their fruit earlier than others which are obtained by the common methods of culture. I must therefore attribute the early maturity of those I have described to the other peculiar circumstances under which the fruit and seeds ripened, from which they sprang.

Fruit fecundated by farina from another tree.

I obtained, by the same mode of culture, many new varieties, which are the offspring of the Siberian crab and the richest of our apples, with the intention of affording fruits for the press, which might ripen well in cold and exposed situations. The plants, thus produced, seem perfectly well calculated, in every respect, to answer the object of the experiment, and possess an extraordinary hardness and luxuriance of growth. The annual shoots of some of them, from newly grafted trees in my nursery, the soil of which is by no means rich, exceeded six feet and a half in height, in the last season; and their blossoms seem capable of bearing extremely unfavourable weather without injury. In all the preceding experiments some of the new varieties inherited the character of the male, and others of the female, parent in the greatest degree; and of some varieties of fruit (particularly the golden pippin) I obtained a better copy, by in-

Siberian crab and a rich apple combined to produce cider fruit.

Variety of the golden pippin.

troducing the farina into the blossom of another apple, than by sowing their own seeds; I sent a new variety (the Downton pippin) which was thus obtained from the farina of the golden pippin, to the Horticultural Society, last year, but those specimens afforded but a very unfavourable sample of it; for the season, and the situation in which the fruit ripened, were very cold, and almost every leaf of the trees had been eaten off by insects. In a favourable season and situation it will, I believe, be found little, if at all, inferior to the golden pippin, when first taken from the tree; but it is a good deal earlier, and probably cannot be preserved so long.

Grape.

I proceed to experiments on the grape; which, though less successful than those on the apple, in the production of good varieties, are not less favourable to the preceding conclusions. A vinery in which no fires are made during the winter, affords to the vine a climate similar to that which the southern parts of Siberia afford to the apple, or crab tree: in it a similarly extensive variation of temperature takes place, and the sudden transition from great comparative cold to excessive heat is productive of the same rapid progress in the growth of the plants, and advancement of the fruit to maturity. My first attempt was to combine the hardiness of the blossom of the black cluster, or Burgundy grape, with the large berry and early maturity of the true sweetwater*. The seedling plants produced fruit in my vinery at three or four months old, and the fruit of some of them was very early; but the bunches were short, and ill formed, and the berries much smaller than those of the sweetwater, and the blossoms did not set by any means so well as I had hoped.

Vinery without fires.

Black cluster united with the sweetwater,

and with the chasselas.

Substituting the white chasselas for the sweetwater, I obtained several varieties, whose blossoms appear perfectly hardy, and capable of setting well in the open air; and the fruit of some of them is ripening a good deal earlier in the present year than that of either of the parent plants. The berries, however, are smaller than those of the chasselas, and with less tender and delicate skins; and, though not

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* This grape is often confounded by gardeners, both with the white chasselas and white muscadine.

without

without considerable merits for the desert, they are generally best calculated for the press: for the latter purpose, in a cold climate, I am confident that one or two of them possess very great excellence. I sent a bunch of one of those varieties to the Horticultural Society, in the last autumn, and I propose to send two or three others in the present year.

Good for wine in a cold climate.

I have subsequently obtained plants from the white chasselas and sweetwater, the appearance of which is much more promising; and the earliest variety of the grape I have ever yet seen, sprung from a seed of the sweetwater, and the farina of the red frontignac. This is also a very fine grape, resembling the frontignac in colour and form of the bunch; but I fear its blossoms will prove too tender to succeed in the open air in this country; a single bunch, consisting of a few berries, is, however, all that has yet existed of this kind. The present season also affords me two new varieties of the vine, with striped fruit, and variegated autumnal leaves, produced by the white chasselas and the farina of the Aleppo vine: one of these has ripened extremely early, and is, I think, a good grape. When perfectly ripe, I propose sending a bunch of it for the inspection of the Horticultural Society.

Very early variety.

Vine with striped fruit and variegated leaves.

In all attempts to obtain new varieties of fruit, the propagator is at a loss to know what kinds are best calculated to answer his purpose; and therefore, I have mentioned those varieties of the grape, from which I have propagated with the best prospect of success. My experiments are, however, still in their infancy; and I do not possess the means of making them on so large a scale, or in so perfect a manner as I wish: nevertheless, the facts of which I am in possession, leave no grounds of doubt in my mind, that varieties of the grape, capable of ripening perfectly in our climate, when trained to a south wall, and of other fruits, better calculated for our climate than those we now cultivate, may readily be obtained; but whether the mode of culture I have adopted and recommended be the most eligible, must be decided by future and more extensive practice.

Experiments yet in their infancy.

Better varieties may no doubt be produced.

I have made experiments similar to the preceding, on the peach; but I can say no more of the result of them, than

Peach.

that

that the plants possess the most perfect degree of health and luxuriance of growth, and that their leaves afford satisfactory evidence of the good quality of the future fruit. I am ignorant of the age at which plants of this species become capable of producing blossoms; but the rapid changes in the character of the leaves and growth of my plants, which are now in their third year, induce me to believe, that they will be capable of producing fruit at three or four years old.

May be brought to bear at 3 or 4 years old.

I shall finish my paper with stating a few conclusions, which I have been able to draw in the course of many years close attention to the subject on which I write.

Best mode of obtaining new varieties.

New varieties of every species of fruit will generally be better obtained by introducing the farina of one variety of fruit into the blossom of another, than by propagating from any single kind. When an experiment of this kind is made, between varieties of different size and character, the farina of the smaller kind should be introduced into the blossoms of the larger; for, under these circumstances, I have generally (but with some exceptions) observed a prevalence in fruit of the character of the female parent; probably owing to the following causes. The seedcoats are generated wholly by the female parent, and these regulate the bulk of the lobes and plantula; and I have observed, in raising new varieties of the peach, that when one stone contained two seeds, the plants these afforded were inferior to others. The largest seeds, obtained from the finest fruit, and from that which ripens most perfectly and most early, should always be selected. It is scarcely necessary to inform the experienced gardener, that it will be necessary to extract the stamina of the blossoms from which he proposes to propagate, some days before the farina begins to shed, when he proposes to generate new varieties in the manner I have recommended. When young trees have sprung from the seed, a certain period must elapse before they become capable of bearing fruit, and this period, I believe, cannot be shortened by any means. Pruning and transplanting are both injurious; and no change in the character or merits of the future fruit can be effected, during this period, either by manure or culture. The young plants should be suffered to extend

Peach.

Choice of seeds.

Seedling trees.

Should not be pruned or transplanted.

extend their branches in every direction, in which they do not injuriously interfere with each other; and the soil should just be sufficiently rich to promote a moderate degree of growth, without stimulating the plant to preternatural exertion, which always induces disease*. The periods which different kinds of fruit trees require to attain the age of puberty, admits of much variation. The pear requires from twelve to eighteen years; the apple, from five to twelve, or thirteen; the plumb and cherry, four or five years; the vine, three or four; and the raspberry, two years. The strawberry, if its seeds be sown early, affords an abundant crop in the succeeding year. My garden at present contains several new and excellent varieties of this fruit, some of which I should be happy to send to the Horticultural Society, but the distance renders it impracticable†.

Soil not too rich.

Age of bearing.

VI.

Memoir on the Desulphuration of Metals: by Mr. GUENIVEAU, *Engineer of Mines*‡.

AMONG the great number of metallic sulphurets, with which Nature presents us, the decomposition of many is of much importance in the arts. The sulphurets of iron, copper, lead, and mercury, and some others, give rise to metallurgical processes, that particularly claim the attention of those, who are addicted to the study of chemistry. The nature and properties of these have been well known, since chemistry has made them an object of her labours: but as the facts collected in laboratories have never been carefully compared

Decomposition of native sulphurets important.

The facts of the laboratory have not been compared with those of the smelting house.

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* The soil of an old garden is peculiarly destructive.

† The hautboy strawberry does not appear to propagate readily with the other varieties, and may possibly belong to an originally distinct species. I have, however, obtained several offspring from its farina; but they have all produced a feeble and abortive blossom. If nature, in any instance, permits the existence of vegetable mules (but this I am not inclined to believe) these plants seem to be beings of that kind.

Hautboy.

‡ Journal des Mines, No. 121, p. 5.

compared with those that extensive works furnish, though we are well aware, that this would be the best way of attaining useful results, the theory of the various operations to which sulphurets are subjected has not yet been improved by the progress of that science. My object is to supply this defect; and to accomplish it I have collected a great many experiments and observations, that have been long known; I have added some researches of my own; and from their examination I have deduced consequences, that must make some alteration in the ideas generally entertained respecting the treatment of metallic sulphurets.

SECT. I.

Of the action of heat on metallic sulphurets.

Heat always employ to decompose them;

THE action of heat on metallic sulphurets requires first to be examined, because it occurs in all the processes employed for their decomposition. To appreciate this with accuracy, I have chosen experiments and observations in which this action is completely distinct, which it is of importance to remark; for it is owing to not having analysed the effects produced by various causes, that metallurgists have ascribed to caloric alone a desulphurating power, which it does not appear to me to possess in any very high degree.

but it has not much effect alone.

Sulphurets of mercury and arsenic sublimed by it.

The sulphurets of mercury and arsenic are volatilized in close vessels, when exposed to a temperature a little elevated. The sublimed sulphuret has frequently a different colour from that which has not been sublimed; and the experiments of Proust and Thenard demonstrate, that this change is the consequence of an alteration in the proportions of the elements of the compound.

That of iron not freed from half its sulphur by it.

The native sulphuret of iron experiences but a partial decomposition by means of caloric. By distillation in a retort we cannot extract half the sulphur it contains. In Saxony the distillation of pyrites in the large way never yields more than 13 or 14 per cent of their weight of sulphur.

As these facts were not sufficient to determine my opinion respecting the effects of heat, because all the experiments that

that had come to my knowledge were made at no very high temperature, I put into a crucible lined with charcoal some powdered pyrites, covered it with charcoal powder, and exposed it for an hour to the heat of a forge. The result was a mass still retaining all the characters of pyrites. It appeared to have been completely melted, and retains two thirds of the sulphur originally belonging to it. This experiment, having been repeated, left me no doubt of the effects of heat alone on sulphuret of iron; and I think I may conclude from it, that, whatever be the temperature, only a partial decomposition can be produced by it.

Powdered pyrites, heated for an hour in a forge fire, was fused, and retained two 3ds of its sulphur.

On sulphuretted copper and pyritous copper heat produces effects analogous to those observed with iron. The distillation of pyritous copper afforded me but very little sulphur. These two ores however may be considered as mixtures of the sulphurets of copper and of iron, and the sulphur separated by heat comes from that of iron almost wholly.

Sulphuretted and pyritous copper similar.

The sulphuret of lead, or galena, is one of those minerals, the treatment of which is most varied. All chemists agree in considering it as a compound of sulphur and lead only, in the proportion of 15 parts sulphur to 85 of lead. I was the more careful in observing the effects of caloric on galena, as in separating the sulphur by its means I might hope to obtain metallic lead, the weight and fusibility of which would render its union very easy. I could likewise without difficulty exclude the air in the process.

Galena.

Into a retort I put 30 gram. [463 grs.] of powdered galena, which I heated for two hours so gently as not to agglutinate it. Only a little sulphurous acid produced by the action of the air of the vessels was evolved, and I perceived no sulphur sublime in the neck of the retort. I then increased the fire, and kept it thus two hours more, till both the galena and the retort experienced a commencement of fusion. The sulphur volatilized in this second stage of the operation was so little, that I could not detach it from the vessel and weigh it. The residuum had the metallic brilliancy, was agglutinated, and did not contain an atom of ductile lead.

Heated gently gave out a little sulphurous acid, but no sulphur.

Heated nearly to fusion

very little sulphur sublimed.

As the heat in this experiment was not very great, I subjected

Fused in a

forge fire scarcely 2-5ths of the sulphur expelled.

jected to a forge fire some powdered galena in a crucible lined and covered with charcoal powder. The result was a mass, that had been well fused, and resembled what metallurgists call lead matt. There was in it no lead united together, but some parts of the button were merely a little ductile. By analysis I found, that about three fifths of the sulphur still remained. Part of the loss it had experienced by the action of the fire, which was 27 per cent, I ascribed to the volatization of a portion of the sulphuret; for that owing to the separation of the sulphur could not have exceeded 6 per cent at most.

Loss ·27 from partial volatilization.

Galena then is but very imperfectly decomposed by heat.

I shall not speak particularly of the sulphurets of zinc, antimony, &c., because I am not acquainted with any experiments sufficient to determine with certainty the effects, that heat produces on them: but I am led to believe from analogy, that it does not decompose them completely.

Heat alone then expels but little of the sulphur.

All the facts I have adduced appear to me to evince, that the action of caloric alone on metallic sulphurets, and particularly on those of iron, copper, and lead, is limited to the taking from them a small portion of the sulphur contained, and afterward fusing and even volatilizing them.

SECT. II.

Of the simultaneous action of heat and air on metallic sulphurets.

Roasting by the joint action of heat and air.

That metallurgical process, the object of which is the desulphuration of metals, is known by the name of roasting. Most authors, who have treated of it, seem to consider caloric as the sole agent in the decomposition; and even those who have remarked the influence of the air, since the establishment of the new chemical theory, have not considered it as essential. The experiments I have collected having shown the insufficiency of heat alone to decompose a metallic sulphuret, the oxigen of the air must be considered as having a greater share in the desulphuration of metals by roasting. The affinities both of sulphur and metallic substances for this principle render it very probable; and it is likewise proved by the chemical examination of the products of

Oxigen has a great share in it.

of all roastings, as well as by the manner in which the process is conducted. In the roasting of sulphurets, instead of seeing the volatilization of the sulphur effected by a moderate and long continued heat, we find a sulphuret decomposed by the simultaneous action of caloric and air: and the acknowledged necessity of not fusing the ore, instead of arising from the fear of communicating to it by liquefaction a cohesive force capable of resisting the separation of the sulphur, will be ascribed more simply to this circumstance, that such a state will confine the action of the air to a surface that cannot be renewed, and will soon be covered with a metallic oxide. The combination of oxigen with the elements of sulphurets gives rise to oxides and acids, the affinities of which have great influence on the separation of the sulphur, and the results of roasting; which are commonly a mixture of an oxide, a sulphate, and undecomposed sulphuret. I shall now examine more particularly and separately the roasting of several kinds of sulphurets, because the nature of the metal greatly modifies the results; and I shall afterward point out how the sulphur is separated, and in what form.

The ore must not be heated to fusion.

Roasting of copper pyrites.

Pieces of pyritous copper are laid on billets of wood in the most convenient manner for the combustion to continue a long time. The first heat separates part of the sulphur, which is in some degree sublimed, and may be collected; but afterward it becomes the combustible, that serves by burning to continue the operation; and sulphurous acid is disengaged, the elasticity of which, being augmented by the increase of temperature, prevents its combination with the metallic oxides. The sulphuric acid, that is formed notwithstanding the care taken to moderate the combustion, unites with the oxides of iron and copper, but the sulphate of iron is in part decomposed by the superoxidation of the metal.

Copper pyrites.

Sulphur rises,

burns,

sulphurous acid flies off,

sulphate of copper formed, and of iron which is again in part decomposed.

Iron pyrites subjected to the same operation will undergo similar decompositions in the same order.

Iron pyrites.

The roasting of cupreous pyrites in the reverberatory furnace gives rise to the same phenomena, and might be supposed

Copper pyrites in the reverberatory furnace.

posed to allow a much more complete separation of the sulphur than that conducted in the open air. If it do not, this no doubt is owing to the difficulty of preventing the agglutination of the sulphuret produced by the elevation of temperature arising from the rapid and unavoidable combustion of a large quantity of sulphur.

Furnace at Fahlun.

It remains for me to speak of a furnace, in which both the smelting and roasting of the pyritous copper, to a certain point, are effected at the same time. It is used at Fahlun in Sweden. This has an interior crucible, which receives the product of a smelting of 24 or 48 hours, and in which a separation, or rather combustion, of the sulphur is effected. A stream of air from the bellows is made to blow on the melted mass with such force, as to drive off the scoriæ, and burn a part of the sulphur that is found on the surface. The iron is thus oxided, and quartz is added to vitrify it in proportion as the roasting goes on. This process is perhaps the only one, in which sulphur and iron are separated in so large a quantity at the same time.

Blast of air directed on the metal.

Progress of desulphuration.

The desulphuration of pyritous copper by roasting appears to me to be effected, 1st, by the sublimation of a small portion of sulphur, which may either be collected, or burned in the air: 2dly, by the disengagement of sulphurous acid, which is the more abundant in proportion as the process is well managed: 3dly, by the vaporization of a little sulphuric acid, the greater part of which however remains united with the copper.

Roasting of galena.

Sulphuret of lead.

Galena is very difficult to desulphurate completely by roasting. The affinity of its component parts for oxigen, it is true, renders their separation sufficiently speedy; but that of the new compounds, sulphuric acid and oxide of lead, gives rise to a new combination, which retains the sulphur, and thus forms an obstacle to the desulphuration. To this affinity of the oxide of lead for sulphuric acid must be ascribed the facility, with which this acid is formed in the roasting of galena.

I shall analyse in detail the various processes, to which this important decomposition has given birth, because I conceive

ceive I can account for the numerous and complicated phenomena they exhibit.

Whatever care be taken to roast galena in a roasting test, it is impossible to convert the whole of the sulphur into sulphurous acid, and avoid the formation of sulphuric. The result always exhibits a mixture of oxide and sulphate of lead.

In the small way sulphate always formed.

In roastings in the large way, on hearths prepared for the purpose, the proportion of sulphate of lead is still more considerable, being in the ratio of the temperature, and the facility with which the air pervades the ore. Numerous analyses made in the School of Mines lead me to believe, that the roasted ore of the mines at Pezey contains from a third to half its weight of sulphate of lead; whence it follows, that, even supposing the whole of the galena to have been decomposed, the roasting has not separated half the sulphur it contained.

In the large way still more sulphate.

The reverberatory furnace is employed with great success to roast ores of sulphuretted lead. In some works indeed, as at Poullaouen, such a complete separation of the sulphur is accomplished in this furnace, that, when the roasting is judged to be finished, nothing more than the addition of charcoal is requisite, to obtain directly a large quantity of metallic lead. It cannot be doubted however, but a great deal of sulphate of lead is formed, which, as we have seen, is a necessary result of the action of air on galena subjected to a high temperature: besides, the chimneys of the furnaces are filled with it. The decomposition of this sulphate by the charcoal produces a sulphuret, or lead matt, and though sulphurous acid may be evolved, it is very difficult to explain how the addition of charcoal causes the lead to flow immediately in considerable quantity. I have imagined that the sulphate of lead was decomposed during the roasting; and that after this operation nothing remained but an oxide very little mixed: and I think I have found the cause of this decomposition in the action of the galena still undecomposed on the sulphate formed. The following experiments will make known the nature and result of this action.

In the reverberatory furnace a complete separation of the sulphur may be effected.

Yet sulphate is formed.

Into a retort I put a mixture of one part of powdered sulphuret of lead and three of sulphate; which at first I heated slowly,

Sulphuret 1 part & sulphate of lead 3 heated together in a retort.

slowly. When the retort was redhot, a pretty considerable quantity of sulphurous acid gas was evolved; and this continued for an hour, at the expiration of which the retort began to melt. The residuum had been fused, and was found to be a mixture of oxide and sulphate of lead. I satisfied myself, that the sulphurous acid, which had been received into water, was not mixed with any sulphuric.

The result a mixture of oxide & sulphate.

The sulphate decomposed by the sulphuret.

This experiment proves the possibility of the decomposition of the sulphate of lead by the sulphuret; or rather that of the sulphuric acid it contains by the sulphur and lead of the galena. The sulphurous acid arises no doubt equally from the oxigenation of the sulphur, and the semidecomposition of the acid; for I convinced myself, that the residuum contains no sulphate. I repeated the process with equal parts of galena in sulphate, when the evolution of sulphurous acid was still more abundant, and what remained in the retort was a mixture of oxide and sulphuret. Hence I concluded, that, if the proportion of sulphuret of lead were too small in the former experiment, it was too large in this. I made also an attempt to ascertain more nearly the proportions, that would exactly effect the mutual decomposition; and at the same time I endeavoured to satisfy myself of the oxidation of the lead contained in the galena in the metallic state.

Equal parts left oxide and sulphuret.

14 parts to 8.

With this view I put 14 gram. of sulphate and 8 of sulphuret, well mixed together, into a crucible, not lined, which I suffered to grow redhot undisturbed. I observed, that a considerable ebullition took place, occasioned by the evolution of sulphurous acid; and I did not withdraw the crucible, till the matter was in quiet fusion. When cold I found two distinct substances; one, which was at the bottom, consisted entirely of sulphuret of lead, that had been fused, without any mixture of ductile lead; the other exhibited all the characters of the oxide called glass of lead, and was a compound of oxide and silex from the crucible, without any indication of sulphate of lead.

Sulphurous acid evolved.

Result, sulphuret and glass of lead.

This experiment convinced me, that the lead of the galena had been oxided at the expense of the sulphuric acid: but it did not show the quantity of galena necessary for the complete decomposition of the sulphate. I believe, however,

Sulphuret 1

ever, that the proportion of one part of galena to two of sulphate will be very near the mark; and besides it differs little from what a calculation of their component parts would indicate.

sulphate 2 would produce a decomposition.

Conclusions.

The following are the natural consequences of these facts: 1, Galena and sulphate of lead mutually decompose each other at a high heat. 2, This decomposition gives rise to the formation and evolution of a large quantity of sulphurous acid, and consequently to the separation of a considerable portion of the sulphur contained in the ore. 3, The result is oxide of lead, when the proportions are suitable; and when otherwise a mixture of oxide and sulphate, or oxide and galena.

Theory of roasting galena in the reverberatory furnace.

The application of these consequences to the roasting of sulphuret of lead in the reverberatory furnace is easy. I shall explain the theory of this process in the manner in which I conceive it. The powdered galena, or washed ore of lead, spread on the bottom of the furnace to the thickness of a few inches, the upper part of which is exposed to the action of the air, gives rise to the phenomena we have observed in ordinary roastings. The heat vaporizes a little sulphur: the air converts part of that on which it acts into sulphurous acid, which is evolved, and another more considerable into sulphuric acid, which combines with the lead oxided at the same time. The ore is stirred: the sulphate of lead mixes with that which is not decomposed, and their mutual decomposition produces sulphurous acid: the fresh surface reproduces sulphate, which serves to occasion a fresh extrication of gas, and thus to continue the desulphuration, which is limited only by the complete decomposition of the galena. If the process have been well managed, and too much sulphate of lead has not been formed, the result of the roasting will be almost pure oxide of lead: if the contrary, some sulphate may remain, which the charcoal will reduce to the state of sulphuret, and the decomposition of which will be effected in the same way as that of so much galena. Hence we may learn the importance of not fusing the sulphuret of lead subjected to the process of roasting; for the action of the air on the fused ore would soon be rendered null by the formation of oxide of lead which would cover it, and

and as the sulphate of lead could no longer mix with the galena, there would be no way of desulphurating it.

This the most complete desulphuration.

The roasting of galena in the reverberatory furnace then is reduced to the conversion of the sulphur it contains into sulphurous acid; and as this is in great measure effected by the intervention of the sulphate of lead, which is continually forming, it admits a more complete desulphuration than other processes.

Decomposition in the Scotch furnace similar.

A similar decomposition of the sulphuret of lead by the sulphate appears to me also to take place in the treatment of lead ores in the Scotch furnace. In Scotland galena is roasted and smelted in an uninterrupted process by means of coal and turf.

Used with success at Pezey.

The same furnace is employed with success at Pezey for fusing roasted galena containing at least one third of its weight of sulphate of lead. Its final result gives no matts, which proves, that it permits the decomposition of the sulphate, and the separation of the sulphur it contains. I conceive, that the action of the part reduced to the state of sulphuret, by the contact of the charcoal, on the undecomposed sulphate, is one of the principal causes of the desulphuration effected.

In some furnaces scarcely any effect from roasting.

Some furnaces have been mentioned, as that of Fahlun and the Scotch, in which metallic sulphurets undergo a real roasting; but there are others, in which this effect is scarcely sensible. Some reflections on their differences in this respect will probably not be out of place here; and they will be the more interesting, as they are intimately connected with our subject, and account for phenomena, which are inexplicable according to the idea generally entertained of roasting.

The higher the furnace the less sulphur carried off.

It is a fact well known in smelting houses, that the highest furnaces are least favourable to desulphuration, or in the language of metallurgists produce the most matts. If an indisputable proof of this were required, I need only say, that at Pezey I have seen roasted lead ores containing a great deal of sulphate of lead, which smelted in the Scotch furnace yielded not matts as the ultimate result, but produced a large quantity in the *fourneau à manche* [a kind of high furnace].

Heat alone ineffectual.

If heat alone could easily and completely decompose metallic

tallic sulphurets, the upper part of high furnaces would be well adapted to the roasting of ores; for, beside that the temperature there is not too great, the air that comes thither, being deprived of part of its oxigen, scarcely forms any of those sulphates, that oppose the separation of the sulphur. But the fact is the reverse of this, which is to me an additional proof of the little effect of the action of caloric alone on these substances. The sulphur is separated from the sulphurets, as has been seen, in the state of sulphurous acid, and oxigen is indispensable to its formation. In furnaces of no great height the air that comes into contact with the fresh charge of ore still contains a great deal of oxigen, and the sulphurous acid formed is soon withdrawn from the disoxiding action of the charcoal: but if a small portion be decomposed, a fresh sulphuret is formed, which is afterward roasted in the same manner as the ore. In the Scotch furnace for instance, when any matts flow from it, they are immediately thrown into the furnace again, and what escaped decomposition in the first process is decomposed in a second. In high furnaces on the contrary the ore placed in the upper part undergoes a very incomplete desulphuration, because the air coming into contact with it contains but very little free oxigen; the sulphurous acid formed in the interior is far the greater part decomposed in traversing all the height of the furnace filled with coals, and a sulphuret is recomposed; this by its gravity tends to descend into the basin, which it does not reach till after a succession of decompositions; and the consequence must be a considerable loss of metal, as in fact is observed.

Advantages of a lower furnace.

Disadvantage of the higher.

All these facts together seem to me to place it beyond doubt, that the decomposition of metallic sulphurets in roasting is produced by the oxigenation of their component parts, and the sulphur is separated more or less completely in the state of sulphurous acid.

These proofs of the utility of oxigen.

Sect. III.

Desulphuration of metals independently of the action of the air.

The various affinities of sulphur for different mineral substances afford means of decomposing certain sulphurets, and metallurgists have already availed themselves of several with success.

Desulphuration by elective attraction.

Requisite conditions.

success. In order that the decomposition of a metallic sulphuret by any mineral may constitute the basis of a metallurgical process, it is not sufficient, that the affinity of this mineral for sulphur be greater than that of the metal: it is farther necessary, beside the conditions economy requires, that several others indispensable to the success of the process be satisfied, which greatly diminishes the number of agents indicated by chemistry. For example, if the sulphuret resulting from the decomposition be infusible, or nearly so; or if it have the property of combining with the metal to be separated, or with the sulphuret yet undecomposed; it is obvious, that the object sought, which is the separation of the metal, will not be obtained. Hitherto scarcely any thing but lime and iron has been employed.

Desulphuration of mercury.

Sulphuret of mercury by lime or iron.

The sulphuret of mercury is easily decomposed. It is sufficient to present to the sulphur a substance capable of retaining it, and the mercury may be volatilized alone. Thus iron and lime are employed singly or conjointly in the treatment of cinnabarine ores.

Desulphuration of copper.

Copper pyrites by lime.

Copper pyrites are smelted in some works with lime, either in the *fourneau à manche*, or the reverberatory furnace; but this process is not sufficiently known in detail, to enable us to judge of the efficacy of this agent.

Iron does not answer.

I had thought with some metallurgists, that the acknowledged greater affinity of iron than of copper for sulphur might occasion the decomposition of sulphuret of copper by this metal, at least in some cases: but the experiments I am about to give induced me to relinquish this opinion.

Experiment in proof.

1st *Exp.* I mixed 10 gram. [155 grs.] of pyritous copper, the composition of which I knew, with 4·3 gram. [66 grs.] of iron filings; put the mixture into a crucible; covered it with charcoal powder; and heated it in a forge fire three quarters of an hour. The proportion of iron was calculated so as to be sufficient for taking up all the sulphur combined with the copper in the ore employed. In the crucible I found a perfectly homogeneous mass, weighing 13·1 gram. [202 grs], which did not contain the least globule of metallic copper,

nor

or any sign of separation between the sulphuret of iron and that of copper*.

2d experiment.

2d *Exp.* Another trial was made with 10 gram. [155 grs] of pyritous copper and 5 gram. [77 grs] of the same mineral roasted, which is nearly the state of the product when the ore or matts have not been completely desulphurated. The proportion of iron was still insufficient to separate any copper, of which there was abundance in the mixture. I heated it three quarters of an hour, and found, as in the preceding experiment, a homogeneous mass, without any sign of metallic copper, or pure sulphuret of copper: it was a true copper matt.

3d experiment.

3d *Exp.* Equal parts of crude and roasted copper pyrites were mixed, moistened with olive oil, and heated strongly for half an hour in a crucible lined with charcoal. The product was nothing but a powder, that had not undergone any fusion, no doubt owing to the superabundance of iron.

Iron, sulphur, and copper, from a triple compound in any proportion.

These few trials I conceive are sufficient to prove, that the desulphuration of copper by means of iron will always be very difficult to effect, because a triple compound of sulphur, iron, and copper, is formed, or a combination takes place between the sulphurets of copper and iron, which obstructs the separation of the copper.

Desulphuration of galena.

Sulphuret of lead

by lime and iron.

Lime little used.

Iron preferable.

Galena is one of those sulphurets, in which this decomposition is most readily effected. The fusibility of lead, which facilitates the union of its particles, as well as the little affinity it has for sulphur, are the causes of the success of the attempts of this kind. Lime and iron are employed in different circumstances for the desulphuration of galena. The use of lime is not very general, and it is impossible to judge of its effects from what is known of the properties of sulphuret of lime. The treatment of galena by malleable or cast iron in small pieces is more in use, and appears very advantageous.

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* In the decomposition of galena by iron, when the latter is in too small quantity, three distanct substances may be observed: lead, sulphuret of lead, and lastly sulphuret of iron at the upper part.

Experiments at the School n .

At the school of mines of Montblanc a great many experiments have been made on the desulphuration of galena by iron, the results of which were of sufficient importance, to render the publication of them desirable.

Hints may be drawn from the facts here given.

The present paper contains several facts applicable to the art of metallurgy, and capable of suggesting different experiments to those who cultivate it. I have not pointed out any, because they will readily suggest themselves to those, who are capable of conducting them.

Mr. Descotils superintended the experiments.

All the experimental researches here given were made in the laboratory of the Council of Mines, and under the eye of Mr. Descotils, whose advice was of great advantage to me, in giving them that accuracy, which he is accustomed to observe even in the least operations.

VII.

Heights of various Places determined by the Barometer, in the Course of several Tours through France, Switzerland, and Italy: by F. BERGER, *M. D., of Geneva**.

Ascertaining heights of places tends to the improvement of geology.

La Place.

AMONG the means best calculated to advance the physical department of geography in the present state of our knowledge we may reckon the ascertainment of the elevation of a great number of points on the Earth's surface. The learned author of *la Mécanique céleste* has proposed to employ with this view observations with the barometer conjointly with the longitude and latitude, to obtain a more complete and extensive levelling than trigonometrical measurement will admit, and at the same time to acquire a knowledge of the direction of mountainous chains, the slope of rivulets, and the forms of countries. To promote these useful objects I shall add to the researches of these natural philosophers, who have attended to this branch of physics, the observations I have collected in different journies. They will form the subject of a few papers, which I shall publish

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* Abridged from the Journal de Physique, vol. LXIV, p. 220, March, 1807.

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in succession, and in which I shall point out generally the nature of the countries mentioned, that my labours may be more immediately useful to geology.

Heights calculated according to de Luc and Trembley.

All the heights were calculated according to the formulæ of Messrs. de Luc and Trembley: not that I mean to speak of them as the only ones fit for practice; I know there is a method founded on different principles, which was proposed some years ago by a learned philosopher, and has a just claim to our best attention: but my labours were in great measure completed, before I was acquainted with it, so that it would have been too laborious a task, to begin them anew.

Modes of ascertaining heights by the barometer should be compared with trigonometrical calculations;

and tried at various seasons and different times of the day.

They who have at heart the improvement of measuring heights by the barometer however would do well, to calculate their observations according to different formulæ, for on this point the result of experience is chiefly to be considered, and we have not yet a sufficient number of decisive facts, to induce us to employ one formula or method exclusively of every other. It is desirable that some philosopher, residing in the neighbourhood of a solitary mountain, should measure it trigonometrically with great exactness, and afterward repeatedly ascertain its height by the barometer, calculating his observations according to several formulæ, and comparing their results with his geometrical measurement. It is particularly important, that these observations should be made at all seasons of the year, and at different times of the day, so as to take in every variation of temperature: but in reality when mountains of moderate height are visited, we generally find on them the same degree of temperature, and most frequently that at which the various formulæ give nearly the same results. We ought not to suppose therefore, that the science has already attained such a degree of perfection, as to enable us to dispense with farther trials: the only course to be followed appears to me to be that of experience.

When calculations are made in a loose way, this should be mentioned.

The necessity of employing good instruments is too obvious to mention. It is likewise to be wished that the traveller, into whose views it may not enter to give heights with any great precision, as the geologist in general for instance, would apprise us of this. It would farther be of advantage, when accurate barometrical measurements are published, to mention the hour of the day when they were made; and at the same time the measurements of other travellers, however different they may be from our own, with the formulæ they employed if possible. Thus useful materials would be stored up for the promotion of science.

The following observations are classed according to the journies in which they were made; and in them I have conformed as much as possible to the rules I have laid down.

SECT. I.

Tour through Picardy and Normandy.

Heights ascertained during a tour made in the ci-devant *provinces of Picardy and Normandy.*

Its leading objects.

This tour was made toward the end of the summer of 1803, in company with my friends and colleagues Messrs. Jurine and de la Roche. I set off from Paris with intention to follow the seacoast as much as possible, to examine the structure of the shores, and the different elevations of the cliffs. I used a siphon barometer made by Dumotier, which however I did not think sufficiently accurate to allow me to consider my results in any other light than simply geological.

Chalk soil interspersed with flints.

It is well known, that part of the soil of France, proceeding in a north-west direction from Champagne to the borders of the sea, is composed of chalk, which continues as far as England, and includes flints of irregular form, separate from each other, but arranged in parallel beds, more or less distant, which alternate with the chalk. Beauvais, Amiens, &c., are in this line. On the left bank of the Somme, below Amiens, are little hills of no great height formed of this chalk, which is burned for lime. At Picquigny there is very good peat.

Stratum of flints, which when washed out form the pebbles on the shore.

At St. Valery on the Somme the cliff is not above 60 or 80 feet high. The chalk is in horizontal strata, a foot and half thick, between which is a very thin bank of flints. These flints, separated and rounded by the waves of the sea, appear to compose the pebbles that are found at the mouth of the river. At Crotoy, a town formerly fortified, and built on the right bank of the Somme at its mouth, the cliff no no longer exists, and we find only a white quartzose sand, forming downs of little height following the direction of the coast toward St. Quentin. The same sand is seen all along the coast from St. Valery to Cayeux, and even to the environs of the town of Ault. This alluvion therefore, if it have been produced by the Somme, occupies a space of at least 7 or 8 leagues [18 or 20 miles]. Is it not rather owing to the retreat of the sea?

White quartz sand,

perhaps left by the retreat of the sea.

Near the town of Ault the pebbles are so accumulated on the borders of the sea, that they extend above a mile inland. I found there a flint passing to the state of calcedony. At this town the cliffs reappear, still exhibiting the same structure. At the city of Eu there is an interruption of the cliffs, which appear again at Treport with the same character.

Extensive bed of pebbles inland.

From Criel to Dieppe the soil is essentially sandy. This town is in a bottom, through which the river Arques flows: and it is these valleys, watered by so many different rivers, the course of which in general approaches more or less a west-north-westerly direction, that the continuity of the cliffs is interrupted. From Fécamp to Hâvre, the country being less intersected by them, the cliffs are more continuous.

Sandy soil.

Cliffs interrupted by rivers.

At cape de la Hève, about a mile and a quarter north-north-west of Havre, the cliff is not so abrupt as at Ault and Treport; in other respects its structure is nearly the same. At the bottom, toward the village of St. Adresse, a bank of marle is found, of which bricks are made: and the chalk rock includes different kinds of petrifactions, as well as flints, and nodules of pyrites, which are decomposed by oxidation when exposed to the air. The cliff is continued up the course of the Seine: and at Orcher, a pleasant village 3 leagues [7½ miles] east of Havre, it is about 200 feet high. Here it is more abrupt than at cape de la Hève, and about a fifteenth part at the bottom is composed of a sandstone with small siliceous pebbles.

Marle.

Petrifactions and pyrites in the chalk.

Sandstone under it.

At Honfleur the cliff ceases to contain strata of flint, and diminishes in height as it approaches the mouth of the Toucques, 4 leagues [10 miles] farther. From Tronville sur mer to within 4 miles of the mouth of the Dives it almost

Flints cease.

Reddish, shelly, limestone.

Blue marle including fossil shells, and traces of bituminized wood.

most wholly disappears. A reddish, shelly calcareous stone is very abundant on the shore. Imperceptibly the cliff rises again, and opposite the rocks called the Black Cows it is about 150 feet high, about two thirds of the lower part, being a blueish marle, include a large species of fossil oyster, called the crested oyster, other petrifactions, and signs of bituminized wood. The upper part is chalk.

Sand.

Caen river building stone.

From Dives to Savenelles, or Sallenelles, at the mouth of the Orne, nothing appears but sand, forming in some places downs. On proceeding up the Orne, near a mile and half beyond Savenelles, are quarries of a large grained calcareous stone, soiled with yellow earth, lying in horizontal strata, and used for building in the country round.

Cliff of blueish freestone.

Cornua Ammonis.

Fuci.

From Gray, at the mouth of the Seule, no cliff is seen till we come to Tracy, a village 8 or 10 miles to the west-south-west. There it is about 200 feet high; and is formed of a very fine grained blueish freestone, tolerably hard, interspersed with scales of mica, lying in horizontal strata, and including a prodigious quantity of cornua Ammonis, some of which are very large. The whole of this coast abounds in fuci and other marine plants.

Coalpit.

The same calcareous freestone forms the substratum of the soil from Bayeux to Littry, a village 5 miles to the south-west. At Littry is a coalpit, that deserves the attention of the naturalist. It was opened in 1741, and has two shafts, one of which, called St. George's, is 345 feet [368 Eng.] deep, and has several extensive galleries issuing from it. The thickness of the coal varies from 4½ to 9 feet [4 feet 10 in. to 9 feet 7 in.]: it lies on a brownish calcareous freestone, but little effervescent; and this on a clay, very soft to the touch, and not attackable by acids. At 250 feet [267 Eng.] from the surface occurs a stratum of a primitive conglutinated stone, composed of siliceous pebbles generally an inch or two in diameter, nodules of steatite, and thin laminæ of coal, cemented by a finer freestone, which does not effervesce with acids. No petrifaction has yet been found in this coal mine, except one branch of a tree, in which traces of woody fibres are perceptible. The coal is in general very sulphurous; that of the best quality is sold on the spot for

26s,

26s. [13d.] the bushel, weighing about 130 lbs; and the worst fetches 15s. [7½d.]. The water of a well 18 feet deep, at the bottom of St. George's shaft, freezes on its surface in winter. On the 27th of September Deluc's thermometer stood there at 12·8 [60·8 F.], while in the open air it was at 17·6 [71·6 F.]. The water of this well is extremely acid.

Water at the bottom freezes in winter.

Cold greater than in the open air.

The Vire, which falls into the sea not far from Isigny, forms a considerable bank of sand at its mouth. There is no appearance of cliff here; but at Vierville, a small town on its left bank, and not far from the sea, there are some traces, which soon give way to the sands and downs, that extend to Ravenoville 7½ miles N. N. W. Throughout this space an immense quantity of shells are found, which are collected and sold to the farmers for dressing their grass land. The zostera marina, which covers the shore, is collected for the same purpose. The rudiments of cliff seen at Vierville consist of horizontal strata of free stone, alternating with clay; both including many petrifactions, particularly gryphites and ammites.

Sand bank.

Shells and zostera marina used as manure.

At Ravenoville, which lies opposite the isles of St. Marcouf, we enter at once upon the primitive class of stones. A beautiful kind of reddish scaly petrosilex forms the transition from the primitive substratum of the peninsula of Cherbourg to the shelly calcareous stone of the surrounding country. The houses of the neighbouring villages, as well as the forts on the coast of la Hougue, are built with this petrosilex.

Petrosilex.

The islands of St. Marcouf are probably of a similar rock, since the corresponding coasts of England are; so that, as Mr. Delametherie observes, we can scarcely doubt, that the granite extends far into the sea on both sides of the channel: and if it were ever to be laid dry, we should probably find the continuation of the granitic chain from one country to the other; or at least they would be separated only by a few plains of secondary formation, as are the granites of Britanny and what was formerly Limousin.

Granite probably extends under the sea across the channel.

Ravenoville is perhaps the most northerly place in France, where salt is made by imitating to a certain point the process

Salt pans.

cess of salt marshes; the tide flowing into basins formed in the sand, where the water stands some time to evaporate before it is boiled down.

The cliffs do not reappear as far as fort de la Hougue, where I was obliged to give up my design of doubling cape Barfleur, viewing the real granite in its native situation. A disagreeable event, which it would be useless to mention, obliged me to proceed directly to Valogne. On this road I continually met with argillaceous schist, which as it proceeds in land forms a series of woody hills, rising in height as they recede from the coast. Near Valogne the *ulex Europæus*, furze, is seen in abundance. It is sown there, to be burned on the land as a manure.

Schist.

Furze sown to be burned for manure.

From Valogne to Cherbourg the country is woody, and the soil reddish. Cherbourg is built on a substratum of light green steatite, very greasy to the feel, in laminæ more or less curved. In some places they are in separate pieces, coarse grained, and easily broken. The new basins for the harbour are cutting out of this rock. Large nodules of true granite, and veins of quartz, are included in the steatite.

Woody country.

Steatite.

Harbour of Cherbourg.

The mountain of Roule, a little to the south-south-east of Cherbourg, may be considered as constituting the cliff. It terminates abruptly toward the town in a precipice about 40 toises high. It consists of a kind of dull petrosilex, with a shelly fracture, in some places reddish, in others whitish, much like that of Ravenoville, but evidently in strata several feet thick, all running S S. E. and N. N. W. Quartz crystals are occasionally found in it. This rock is wrought for the works of the harbour, being blasted in large masses.

Petrosilex.

Quarried for the harbour of Cherbourg.

Between Cherbourg and St. Lo the soil is variable. Thence to Aulnay and on to Falaise it is hilly, and woody, but the trees are in general low. The usual direction of these hills is north and south, and they diminish in height as they approach the coast. They consist of a micaceous argillaceous schist, which does not effervesce with acids, and includes no organic bodies. In some places it passes into true slate. Near Villers however, in the district of Caen, is found a granulous limestone containing a prodigious quantity of belemnites. The furze on the downs is employed for feeding cattle. The

Hilly country.

Schist.

Limestone

Cattle fed on furze.

Limestone strata

divided from the schist by the Orne.

The country on the right bank of the Orne begins to differ perceptibly from that on the left. At Ussy 10 or 11 miles N. N. W. of Falaise, limestone occurs in strata. The course of the Orne indeed appears the boundary of two different kinds of country: on the left bank we find micaceous argillaceous schist, and on the right limestone. The argillaceous schist of the woody part of Normandy may be considered as forming the transition to the primitive rock, that constitutes the most advanced part of the peninsula of Cherbourg to the N. N. W. At Verneuil nodules of flint reappear in the chalk, and we begin to perceive vineyards. The line traced by Mr. Arthur Young on this point appears to me very accurate.

Flinty chalk, and vineyards.

Extent of this soil.

Thus we see, 1st, that the part of France where we find a chalky soil interspersed with flints stretches S. E. and N. W., and is pretty accurately included between the mouths of the Seine and the Lys, occupying a breadth of about 50 leagues [125 miles] and a length of 70 [175 miles]: 2dly, that in this the highest cliffs occur, at least among those that are seen between St. Valery on the Somme and Cherbourg. The following table will show this more conspicuously.

Table of heights above the level of the sea.

Places.	Heights above the sea in toises & thousandth parts. According to Deluc	According to Trembley.
Beaumont-sur-Oise	39·352	39·514
Amiens	38·801	38·977
Frixecourt	30·009	30·321
Ault	25·437	26·100
Tréport	59·458	60·999
Etretat	52·944	54·243
Cape de la Hève	46·545	47·729
Honfleur	41·798	42·887
Caumont	141·326	143·707
Cahagnes	88·206	89·135
Aulnay	58·617	58·913
Harcourt	22·672	23·229

VIII.

A new Method of Classing the Hymenopterous and Dipterous Insects: by L. JURINE, *Correspondent of the Institute, Professor of Anatomy, &c**.

Hymenoptera a natural order.

Genera distinguished arbitrarily,

or by the parts of the mouth, which is difficult.

THE distinction of the order hymenoptera, pointed out by Aristotle, is so natural, that it has been retained in every system of entomology to the present day. Linneus, Geoffroy, and Degeer, divided it into a few genera, more or less arbitrary, from various particularities of confirmation: while Fabricius and Lareille have attended in this point to the parts of the mouth. The difficulty of dissecting this organ however in the smaller species is a great inconvenience; to avoid which, and at the same time adhere more closely to the system of classification by the wings, Mr. Jurine has recourse to the disposition of the principal ribs of the wing for the generic characters.

New method by the ribs of the wings.

Having observed, that these ribs, by intersecting or terminating in each other, form various reticulations, which are constantly uniform in insects of the same kind, he has studied these systematically, and given accurate representations of those of the hymenoptera in 14 coloured plates, included in a quarto volume, in which he details his method. On the outer edge of the upper or larger wing of the hymenoptera are two large parallel ribs, appearing to issue from the corselet, and strongly united by an expansion of the membrane. The outermost of these he terms the *radial* rib, the innermost the *cubital*. The place where they terminate toward the end of the wing, which is commonly distinguished by a spot or mark more or less deep, he calls the *point*, or *carpus*. The rib that proceeds from this point to the extremity of the wing has a membranous space between it and the outer edge of the wing, forming one or more areas, which he names *radial cells*. From the extremity of the cubital rib, and near the carpus, another prominent line proceeds towards the extremity of the wing, and the interval between this and the

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* Abridged from the Magazin Encyclopédique for April, 1807, p. 434.

preceding is the *cubital cell,* which is commonly divided into two, three, or four.

All these cells exhibit a great many differences: thus they are incomplete, appendiculate, petiolate, &c. These differences constitute the characters.

The whole of the hymenoptera with which Mr. Jurine is acquainted, and his own collection contains 2200 species, he includes in 86 genera, which he arranges in three suborders, distinguished by the manner in which the abdomen is attached. The diptera, arranged according to the same method, will shortly appear.

Divided into 3 suborders, and 86 genera.

The diptera promised.

IX.

Description and Manner of Using Mr. ROBERT SALMON'S *Geometrical Plotting Quadrant, Level, and Calculator, for the Use of Navigation and Land-Surveying; ascertaining inaccessible Distances, and Demonstrating and Determining various Problems in Geometry and Trigonometry*[*].

ON the instrument and parts thereof are engraved the names given by the inventor, and made use of in these explanations; the *base* line being that at right angles with the 90 degrees on the *arch,* as it is also to the *perpendicular,* which perpendicular always moves parallel to the 90 degrees. For the use of land-surveying, where the instrument can be made stationary, the sight (marked *a*, *Fig.* 1, *Pl.* V.) with the small hole in it, must be applied; but for sea-service, the one *b*, *Fig.* 2, with the mirror, must be substituted in its place.

Terms used.

Every person who has had occasion to describe or calculate the parts of the right-lined figures used in geometry, perspective, surveying, navigation, dialling, architecture, &c., must have perceived, that all of them are resolvable into the most simple of figures, a *triangle,* or some number of them.

All right-lined figures resolvable into triangles.

Hence the great importance of geometry and trigonometry,

Use of trigonometry.

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[*] The Society of Arts voted Mr. Salmon their silver medal and ten guineas for this invention. See their Trans. vol XXIII, p. 290, whence this article is extracted.

metry, in teaching, either by construction or calculation, the knowledge of all the properties or relations between the three sides and three angles, of which every plane triangle is composed. Euclid having demonstrated, in the fourth proposition of the sixth book of his Elements, that in any two *similar triangles* (by which he means their having the same angles, without regard to the actual lengths of their sides, for one triangle may be very small and the other ever so large) every pair of the corresponding sides in the two triangles are proportional; it is the business of trigonometry to solve such problems, with the help of the tables of sines and tangents, or of sectors, sliding or other rules, and scales, by which you can find, on inspection, a *right-angled* triangle, exactly similar to any given right-angled triangle, (or having one of its angles equal to 90°) which can be proposed, or can occur in practice; and by the Rule of Three we say, *as* any side of the tabular triangle *is to* the similar side, supposed to be known, of the triangle under consideration, *so is* any other side of the same tabular triangle, *to* the corresponding side supposed to be sought, of the triangle in question. It is evident, that by means of the *base line, perpendicular,* and either the *upper,* or *lower limb* of my instrument, by the two motions of which the *perpendicular* is capable, and the angular motion of which the *limbs* are capable; any right-angled triangle whatsoever, as C B E, or C D E, in the diagram *Fig.* 6, *Pl.* V, may be instantly formed, (by bringing the top corner of the *perpendicular* to touch the *limb*) with the same or greater facility, than it could be taken out of a trigonometrical table, measured by the compasses on the sector, or set on any instrument now in use for that purpose. But no instrument that I have seen or read of is capable of forming immediately *any obtuse-angled triangle,* as on my *geometrical plotting quadrant* can be done; nor can the trigonometrical tables be applied, to produce the sides and angles of such a triangle, without some trouble, in any case; and in some of the most useful cases in practice, the labour is very considerable. I shall therefore give the solution of five problems. First, supposing, that *Figure* 6, *Pl.* V, represents my instrument, set to answer this and the following problems; A, B, C, being the triangle under consideration;

Similar triangles proportional in all their parts.

Practically applied to rectangles.

Instrument applicable to these;

and equally to obtuse angles.

Problems to be solved by it.

ation; then since the ∠ A C E, is by Euclid (I, 20) equal to the ∠ B A C, it is evident that this angle will be shown, or may be set, by means of the divisions on the arc F G; also, that since C B E, and I C B, are also equal, the *arc* H I, with the addition of 90° (for the angle E B A), will show the ∠ C B A, of the triangle; it is equally evident, that the *arc* F H will show the sum of the two ∠s B C A, and A C F, at the same time that the lengths of all the sides may be read off, on the divisions or scales, on C A, C B, and B A. Therefore:

First.—*To construct or set a triangle, having two of its angles and the side between them given.*

To set a triangle of which 2 angles and the side between them are given.

Set the limb C G, to the division at G upon the *arc* answering to one of the angles, say A, and make it fast, then to this ∠A add the other given angle, (which we will call C) and set *the other limb* C H, and make it fast at the division H, on the arc answering to the sum of their degrees; then on the *limb* C G seek the length of the given side C A; next, push the *perpendicular* up or down, till the *parallel* cuts the point A, (always observing, the divided edges are those you work to), and by the help of the mill-headed *nut*, move the *perpendicular*, till its top corner just touches the *limb* C H, say in the point B; when it is evident that the degrees on the arc H I, added to 90°, is equal to the angle B, and that the other sides C B, and B A, may be read off thereon. Or supposing C B D to be the triangle, whose angles B and C and side B C are given, we have only to move the *limbs* so as to make I H equal to B, and H G equal to C, and then to bring the top of the *perpendicular* to touch C H, at the division B, answering to the side C B, when the other ∠ D will be shown by the division on the *arc* G F, adding 90° thereto; and the remaining sides C D and B D may be read off on their respective scales.

Second.—*To set a triangle, having two sides and the angle included between them given.*

Two sides and the angle between them given.

Let A B C be the triangle, A B and A C the given sides, and A the given angle; first set the *limb* C G to the division answering to A, then bring the *parallel* up to the point A, answering

swering to the side C A, and by the *nut* move the *perpendicular*, till B A answers to the given side B A; next bring down the *limb* C H to touch B, and on C B may be read the other side, while H G will show the angle C, and I H + 90° the ∠ B, whence all the six parts are known.

Third.—*To set a triangle, having two sides and an angle opposite to one of them given.*

Two sides and an angle opposite to one of them given.

Let A B C be the triangle, A C and C B the given sides, and A the given angle; first read the angle A on F G, and set the *limb* C G thereto; then push up the *parallel* to the division at A, answering to C A, and with one hand work the *nut* and with the other move the *limb* C H, till they touch at B, the division answering to the side C B; then B A is the side sought, and the arc G H will show the ∠ C, and I H + 90° the ∠ B.

Fourth.—*To set a triangle, having two angles, and a side opposite to one of them given.*

Two angles and a side opposite to one of them given.

Let A B C be the triangle, A and C the given angles, and B A the given side; first, set F G to the ∠ A, and G H to the ∠ C, then push the *perpendicular* up or down with one hand, while the other works the *nut*, till the given side B A, on the *parallel*, is applied exactly between the *limbs* C H, and C G, then I H + 90° will show the remaining angle B, and on C B, and C A, may be read the lengths of those sides.

Fifth.—*To set a triangle whose three sides are given.*

Three sides given.

Let A B C be the triangle; on the *limb* C H seek the point B, answering to the side C B; then, using one hand to move the *perpendicular*, and the other to turn the *nut*, let an assistant at the same time, with his right hand, gently move the *limb* C H, while you cause the top corner of the *perpendicular* always to touch the point B; at the same time let the assistant move the *limb* C G with his left hand, till the lengths of C A, and B A, on their respective scales, are found to intersect each other, when F G will show the ∠ A, G H the ∠ C, and H I + 90° the ∠ B.

The instrument here less convenient

My solution to the last problem is inferior to the common method of plotting the triangle on paper, and measuring the

the angles with a protractor; but I have introduced it here, to show that my instrument is capable of solving this, as well as all other cases of obtuse-angled triangles, and might, by extending the *arc* to a semicircle, as shown by the dotted lines on the figure, solve any triangle. In the practical problems in surveying, which follow, the triangles can always be taken right or obtuse angled, and the instrument as at present constructed is fully competent. I might here add, that a given line can readily by my instrument be divided into any number of equal parts; drawings might be enlarged or diminished, as readily as with the proportional compasses, and many other equally useful purposes may be effected thereby.

than the common method.

Line divided by it into any number of parts.

First.—*To measure an inaccessible distance, by a perpendicular line set off towards the right hand, from the line or base between the observer and objector.*

Set the *base line* of the instrument in a line pointing to the object, at the same time place a staff at any distance at pleasure, as a perpendicular (being 90 degrees from the base). On this perpendicular measure any distance (say 50 yards or other measures) as a second station; move the instrument to this distance, and place it with its *perpendicular* in the same line as before; the instrument being so placed, set the *lower-limb* pointing to the object, and with the screw make the same fast; this done, the distance of the object will be thus readily known. Raise the moving *perpendicular* of the instrument to the division 50 (as before suggested), then with this height move the same by means of the *nut*, till the extremity intersects the *lower limb* before set, at which intersection, the distance from the second station will be shown; and on the *base line* will also at the same time be seen the distance from the first station: this is a case of right-angled triangles.

Method of measuring an inaccessible distance.

Note.—As the divisions on the *perpendicular* are denominated (either feet, yards, poles, or other measures), so will the distances be indicated on the other *limbs*, and on the *base* of the instrument.

Secondly.—*To determine the distances of any two inaccessible objects, both objects lying in a right line from the observer.*

As before directed, place the instrument with its *base* in

To measure the the distance of

two inaccessible objects in a right line.

the line of the objects; then by means of the *upper limb*, set at 90 degrees, place a staff as a perpendicular at any distanct at pleasure (say 50 as before). This done, remove the instrument to this second station, and place it so that the *upper limb* (still at 90°) may be in the same line as when at the first station; this done, move the *upper limb* into the direction of the nearest, and the *lower limb* into the direction of the most distant object; which *limbs* being so set, and made fast, the distance of both objects from the second station will be seen on the two *limbs*, and the distance from the first station at the same time seen on the *base line*, by setting and moving the *perpendicular* as directed in the last case. This is also a case of right-angled triangles.

Thirdly.—*To measure an inaccessible distance in an oblique angle, where a right angle cannot be obtained, by reason of some impediment on the ground.*

To measure an inaccessible distance in an oblique angle.

At the first station, from which the distance is required, place the instrument; then set up a staff in any attainable direction, to any distance at pleasure (the more distant the better). The instrument being set with its *base* in direction to the staff, with one of the moving *limbs* take the angle of the object, and with the screw fix it thereto. This done, move the instrument in the direction of its base (being between the first station and staff set up) to any certain distance, (say 50 yards or measures) as a second station. From this second station again take the angle of the object, and thereto fix the other moving limb; this done, the distance both from first and second station, as also the bases and perpendiculars thereto will thus readily be seen. Set the *perpendicular* at random to any height, move the same till the upper point intersect the *upper limb*, or that most distant from the *base*, then read off on the *parallel*, the divisions parallel to the base subtended between the two *hypothenuses* or *limbs*; if this distance or division be equal to the distance measured on the base line, (*i. e.* 50) then the distance of the object from both stations will be shown on the two *limbs*, as will also the base and perpendicular on the respective lines. If the divisions on the *parallel* do not agree with the distance measured, the *perpendicular* must be altered till that division is shown, when

when the required distance will be given. This is a case of our first problem.

Fourthly.—*To level, or measure the altitude of any object.*

To measure the altitude of an object.

It is only necessary to set the plane of the instrument vertical, instead of horizontal, by means of the joint under the instrument, whence it is evident every case may be known as on the horizon; and to level, it is only requisite to set the *spirit level* at the back of the instrument, the *base line* and every object cut by the same will be level thereto.

Fifthly.—*To take angles or altitudes at sea, where the instrument cannot be made stationary.*

To take angles or altitudes at sea.

For this purpose, it is first requisite to change the *sight* a, *Fig.* 1, and substitute the one b, *Fig.* 2; which being firmly fixed and adjusted at right angles with the upper limb, it is evident that when by reflection any object is brought to coincide on the *mirror*, at the extremity of the *base line*, with another object seen in the direction of such *base*, the angle will then be known, being double what the upper limb denotes on the *arch*, to which true angle, or its double, the *lower limb* may be fixed, leaving the one with the *mirror* again at liberty to take another observation and angle, at any distant place, or time; which being so taken, this *limb* may be also moved and fixed to double its apparent angle, and the altitude or distance be then determined, by setting the *perpendicular* and *parallel* as in other common cases on land.

Supersedes the use of calculations and tables of sines and tangents.

From this mode of determining distances, as the use of calculations and of tables of sines and tangents is superseded, it is presumed that much convenience will arise to the unlettered who may have occasion to use it, and thereby the errours of calculations will be avoided.

Various other uses to which it is applicable.

As well as the before-mentioned purposes to which the instrument applies, it is presumed there will be found other things which it will perform, some it is hoped useful, and some amusing, amongst which may be enumerated, Multiplication, Division, Rule of Three, Double Rule of Three, &c.; determining the area or sides of any sort of triangle from any proper data; determining the inscribing or inscribed circle of any triangle, square, or polygon, showing a mean proportional between two numbers, &c.

It is presumed, that an instrument, if perfectly made, on a large scale, would be found very useful and accurate in various practical calculations, as well for making them, as for proving them after made in figures.

The following are specimens of the manner of calculating by this instrument.

First Question.—*If* £100 *in* 12 *months produce* 80 *shillings interest, what will* £200 *produce in* 18 *months, and also what will it produce in* 12 *months?*

Question in interest worked by it.

On the *base line* of the instrument set £100. On the *perpendicular* set 80 for shillings interest. Then bring the *lower limb* to intersect, which angle will then be, as per question, equal to 12 months at all places on the *base;* having so fixed the *lower limb,* move forward the *perpendicular* till it intersects the *lower limb* at the height 12 on the *perpendicular,* then raise the *perpendicular* to 18, and to the extremity thereof fix the *upper limb* to intersect, which angle will then be in proportion as 18 to 12 to the *lower limb,* being equal to the different times. The limbs being so fixed, it is only requisite to move the *perpendicular* to 200 on the *base;* and, raising the *perpendicular* till it intersects the upper limb, you will have thereon the answer 240 shillings, and at the same time, at the intersection on the lower limb, 160, being the interest for 12 months only.

Question second.—*To determine the inscribed or inscribing circle of any polygon, the side being given; for example, a hexagon the side of which is* 100 *feet.*

To determine the inscribed or inscribing circle of a polygon.

Set one of the limbs to half the angle included in the required side of the hexagon (*i. e.* 30 degrees), then set the *perpendicular* to the height of half the side given, being as per question 50. Then move the *perpendicular* till the extremity intersects the *limb* before set, on which, at such intersection, will be denoted the radius of the inscribing circle, and at the same time may be seen on the *base* the radius of the inscribed circle.

Question

Question third.—*To find a mean proportional as between* 600 *and* 200.

This depends on the well known property of a right angled triangle.

To find a mean proportional.

Set the *perpendicular* on the *base* line, at the distance of half of the difference of the two numbers (*i. e.* $\frac{600-200}{2}$); this done, raise the *perpendicular*, and move either of the *limbs* till the extremity of the *perpendicular* intersects thereon at half the sum of the numbers, being 400. This done, the height of the *perpendicular* will show the proportional required, being 347.

Note.—On the plate on which the perpendicular slides will be found Noniuses for subdividing the divisions on the base or perpendicular into 10 divisions.

Reference to the Engraving of Mr. Salmon's Geometrical Quadrant and Staff, Plate V.

Explanation of the plate.

Fig. I represents the face of the quadrant, on which A is the fixed base line; B the moveable perpendicular; C the upper limb; D the lower limb; E the arc; F the nut, which moves the perpendicular by means of a rack and pinion.

G, a spring to keep the perpendicular steady; H, a screw for fixing the joint of the staff; *a* the eye piece, or sight, with a small hole in its centre; I, I, I, the sights for direct vision, consisting of only a small slit in each. When objects are to be viewed by reflection, as with a Hadley's quadrant; the sight *a* at the centre is taken off, and the sight *b* with a mirror, shown at *Fig.* II on rather a larger scale than the former, must be substituted.

Fig. III.—K is the staff, the mode of applying which to support the instrument when in use is shown by the same letters in the other figures; L is the screw, by which the staff is fixed firmly in the ground.

Fig. IV represents the back of the quadrant; M M are the screws, by which the upper and lower limbs are fastened after taking an observation.

O, a spirit level; H the tightening screw for the joint before noticed; P the socket attached by its joint and tightening

ing screw to the back of the quadrant; the staff K is screwed into the above socket.

Fig. V shows the practical method of using the instrument for determining the distances of the objects Q and R from the two stations S and T, at which the instrument is to be successively placed, and used as before described.

Fig. VI is the diagram referred to at page 220.

Fig. VII represents the mode of applying the tightening screw H, in *Fig.* I and IV, by means of the semicircular spring, enclosing the cylindrical stem, or neck of the joint.

X.

Description of a Larum applicable to any Pocket Watch, by Mr. JOHN PRIOR, *of Nessfield, near Skipton, in Craven**.

Applicable to any watch without injuring it.

IN constructing this machine, I have endeavoured to make it as simple as I could; so that by the assistance of a pocket watch, of any size, or any number of turns of the fusee, which ever way it is wound up, it will cause the larum to let go at any time required, without hurting the going of the watch.

New method of winding up the main-spring,

In winding up the main-spring of this machine, I have used a method different from that of any other person, and which admits of its acting with considerably more power than where thicker pivots are used.

and stopping it without a fusee.

The discovery of stopping the main-spring when wound up without a fusee, I must beg leave to say, gives me a great deal of satisfaction; and will, I hope, be useful to my brother workmen.

To show with what ease a watch will turn the larum screw, I made the following experiment.

Ease with which it acts.

When the machine was wound up and the lever put upon the screw, I turned the axis of the screw, while one of the pins, which has a communication with the key when the watch

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* The Society of Arts voted Mr. Prior their silver medal and twenty guineas for this invention. Trans. Vol. XXIII, p. 393.

watch is put to the larum, was parallel to the horizon; its distance from the centre of motion was one eighth and three fourths of an inch. I hung a slender thread upon the pin, with ten grains troy weight tied to it, which moved the screw.

May be set as long before hand as the watch will go.

This larum may be set a week before-hand, if the watch would go as long, by increasing the number of threads of the screw.

Left hand watch.

When a watch is made to wind up the contrary way, it is necessary only to take out the pin in the axis of the detent, and turn the lever the other side up, and then it will drop off at the other end of the screw to disengage the larum.

Number of hours in a turn of the fusee.

By taking notice at what hour you wind up your watch, and by winding it up again at twenty-four hours, you will ascertain how many hours are contained in one turn of the fusee, the most common are four, five, six, hours for each turn. I have divided the common index into 120, which does for three concentric circles. That next the centre is for a watch fusee of four hours a turn; the next five, and the last six. While the hand of the larum passes over one of the divisions, it will be equal to two minutes to the four hours circle, two and a half to the five hours, and three minutes to the six hours circle, all shown by the same hand.

A particular key necessary.

Before a watch is put to the larum, it must be fitted with a key that will not drop off when the watch is turned with the key-hole downward, something like that which I have sent; then hang up the watch by the pendant to the holder, which may be turned while the key end on the watch is opposite to the axis of the screw, and the face of the watch parallel to the plate. Then turn the sliding pieces any way, as may suit the watch in that situation, and screw it fast.

Method of setting it.

Supposing the watch is four hours in making a turn of the fusee, turn back the hand of the larum while one of the pins touches the pin in the watch key, and if the hand is not at top, turn it back till it is. If the larum is required to go off in four hours, lift the lever into the first turn of the screw; if in five hours, turn back the larum hand one hour; if in six hours, two back; if in seven hours, three back; and if in eight

eight hours, do not turn it back, but put the lever into the second turn of the screw; and so for the rest.

I am, Sir,

Your very humble servant,

JOHN PRIOR.

Reference to the Engraving of Mr. Prior's Larum. Pl. VI.

Explanation of the plate.

Fig. 1 shows a bird's-eye view of the machine.

A, shows the position of the watch on the larum.

B. The spiral cylinder, fixed on the axis, and moved by a pin across a key placed on the fusee square of the watch.

C. The acting lever, one end of which lies upon the spiral, the other end is movable upon an arbor D.

E, shows a notch cut in the arbor D. This notch is cut more than half through the arbor, in a situation opposite to a pin at F, in the middle of the rim of the larum contrate wheel G. When the lever C falls off the cylindrical spire B, the notch E is moved to a situation so as to allow the pin F to pass through the notch which discharges the larum hammer H, which works by pallets in the contrate wheel G in the usual manner.

O. The barrel which contains the spring, the inner hand of which is connected with the same axis as the contrate wheel.

K. The finger piece which winds up the spring.

b, b, b. The three sliding pieces which hold the watch.

c, c. Two projecting pins, which are carried round by the pin *d*, which is fixed across the key fitted to the fusee.

Fig. 2 shows that side of the machine on which the watch is fixed.

a. The sliding piece, on which the pendant of the watch is hung.

b, b, b. The three sliding pieces which serve to adjust the watch, and hold it in such a position that the fusee square may be in a line with the axis of the spiral cylinder B, shown in *Fig.* 1.

I is a ratchet wheel, on the centre of which is a button or finger-piece K, to wind up the larum spring.

L. The

Mr. John Prior's Larum for Pocket Watches.

Fig. 5.

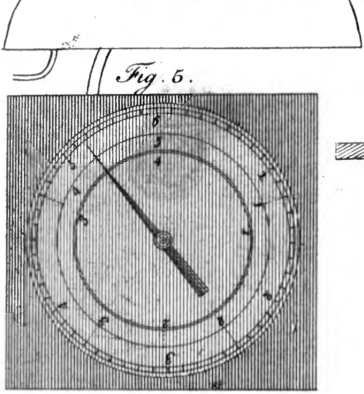


Fig. 3.

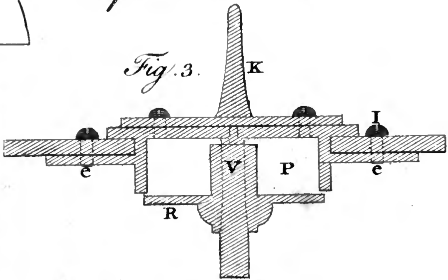


Fig. 4.

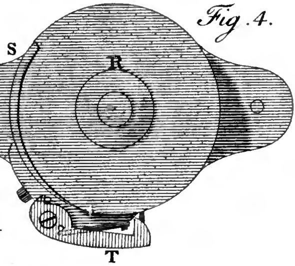


Fig. 2.

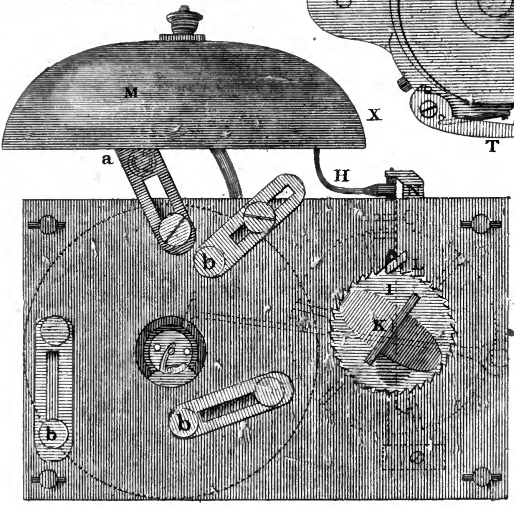
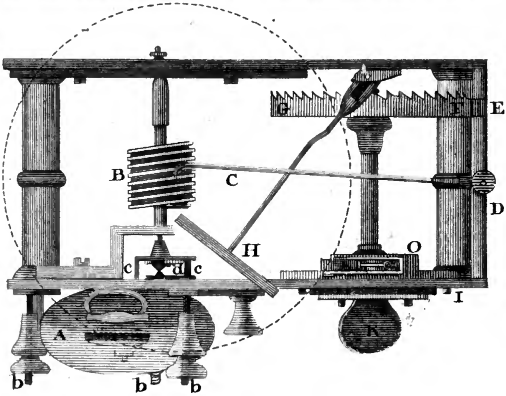


Fig. 1.



L. The click which works in the teeth of the ratchet wheel.

H. The lower part of the arm of the hammer.

M. The bell, within which the hammer strikes.

N. The cock in which the pivot of the axis of the pallets acts, and to which the hammer is connected.

Fig. 3, shows a section of the ratchet wheel and spring barrel, which are screwed together, and move at the same time, but are kept in their place by two pieces *e*, *e*, which fit a groove in the barrel.

K. The button or finger piece.

I. The ratchet wheel.

P. The box for the spring.

R. The cap which covers the spring box.

V. The axis on which the main spring is wound.

Fig. 4. R. The cap, under which is the spring. On the edge of this cap, at S, is an indent to retain the spring when wound up.

T. A lever, with a hook at its end.

At the end of this lever, on a line with the hook, is a small piece of steel, which goes through a hole in the box, and presses upon the main spring; so that, when the main spring is wound up on the axis V, the hook is at liberty to fall into the way of the indent S, and is there stopped.

Under the lever T is a small spring, which presses it against the main spring within the barrel.

Fig. 5, shows the index of the larum; the outside circle of which is divided into 120 parts, which index serves for three concentric circles; that next the centre is for a watch fusee of four hours a turn, the next five, and the last six, as explained in the letter.

XI.

Observations on the Combination of fixed Oils with the Oxides of Lead, and with Alkalis: by Mr. F. FREMY, *Apothecary, of Versailles**.

SCHEELE was the first who observed, that the water, which serves as an intermedium when fat oils are combined with

Water used in making litharge plaster is sweet.

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* Annales de Chimie, Vol. LXII, p. 25, April, 1807.

Sweet principle of oils.

Is it not owing to the lead?

Questions to be answered.

with litharge, hold in solution a substance, that he called the sweet principle of oils, because it has a very decided saccharine taste. But as, according to the observations of that eminent chemist, the water likewise holds in solution a certain quantity of oxide of lead, may it not be inferred, that this taste is owing to the property that metal has of imparting sweetness to most of its compounds? If experience prove the contrary, would it not be interesting, to inquire how this principle is formed? what are its properties? in what state the oil is left, after having lost the principles that give birth to it? whether this abstraction be indispensable to the combination of oil with oxide of lead? and on the experiments necessary in this research to establish the theory of one of the most important operations of pharmacy and the analogy of its results to alkaline soaps?

Such were the propositions that led to the experiments I am about to describe.

Oil, litharge, and water boiled together.

The oxide changed yellow, and then white,

and carbonic acid evolved.

Into a tubulated glass body I put equal parts of olive oil, litharge, and water. To its tubulure I adapted a tube, terminating in a vessel of lime water; and to its orifice a bladder, to prevent the contact of air. This bladder was so contrived, as to allow me to stir the matter with a spatula, so as to prevent it sticking to the bottom. Having brought the mixture to boil, I observed the oxide of lead change in succession from red to yellow, and from yellow to white: and during the experiment carbonic acid was almost always flying off. Having suffered the apparatus to cool, I examined the results of the experiment in succession.

The water had a metallic taste, & would not ferment.

Lead precipitated from it.

The water, that had served as an intermedium, had a strong metallic taste. With the addition of yeast, and at a proper temperature, I could never bring it to ferment*. It formed an evident precipitate with sulphuric acid, and with hidroguretted sulphurets†. I passed sulphuretted hidrogen through it, till nothing more was thrown down, and then filtered to separate the sulphuret of lead.

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* I was for a moment led into an errour, by employing yeast, which, not having been washed, contained some alcohol.

Oil dissolves lead without being rancid.

† I satisfied myself by various experiments, that it is of no consequence to the solution of oxide of lead, that the oil or fat should be rancid, as Scheele supposed.

The filtered liquor still retained a strong saccharine taste. It was evaporated to the consistence of a sirup, and the acetate of lead then no longer indicated the presence of sulphuretted hidrogen. My attempts to ferment it were as unsuccessful, as before the oxide of lead was separated. Exposed to the air, it strongly attracted moisture: thrown on burning coals, it flamed like an oil: on boiling it with the red, yellow, and white oxides of lead, it dissolved only the yellow: on distilling it repeatedly with nitric acid, oxalic acid was formed: distilled in a retort on an open fire, part of it rose, as Scheele observed; and by increasing the heat the products were an empyreumatic oil, acetic acid, carbonic acid, carburetted hidrogen gas, and a light, spongy coal, containing no oxide of lead.

The filtered liquor sweet. Evaporated.

Would not ferment.

Attracted moisture; burned with flame: dissolved yellow oxide of lead: formed oxalic acid.

Its products.

From what I have mentioned it might be presumed, that the oil, when it had combined with the white oxide of lead, was not in the same state as before the combination.

To free it from this oxide I employed acetic acid, because the solubility of acetate of lead would afford ready means of separating it from the oil, the properties of which I wished to examine.

The oil separated from the oxide.

This oil has the consistence of soft fat, and the taste of this animal substance when rancid. It is insoluble in water, but soluble in alcohol; from which it is precipitated by water as volatile oils are, and like them partly rises in distillation*.

Its characters.

The slightest boiling is sufficient to combine it perfectly with white oxide of lead, and give it the consistence of a strong plaster, which does not take place with litharge, or with massicot.

Readily unites with white oxide of lead.

The yellow and white oxides of lead cannot combine with common oils. I satisfied myself of this fact by boiling them together much longer, than would have been necessary if I had used litharge.

Neither white nor yellow oxide unites with common oil.

From these experiments it follows, that, when fat oils are treated with litharge, the oxigen of the latter takes from them carbon, and previously hidrogen, to form with them water and carbonic acid.

Oxigen of litharge takes carbon and hidrogen from oils,

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* All fat oils are soluble in alcohol; but they are far from possessing this property in so striking a manner, as after they have been boiled with litharge.

That

and thus produces the sweet principle.

That this abstraction, rendering the oxigen more abundant in the oil, gives birth to that saccharine substance, which Scheele calls the sweet volatile principle of oils.

Differs from mucilage and sugar.

That this sweet principle differs from the mucoso-saccharine by its property of dissolving the yellow oxide of lead: that its presence is independant of the presence of oxide; and that it differs from sugar by its volatility, and by the impossibility of bringing it to ferment.

The oil acquires some properties of volatile oils.

That the oil, deprived of the elements that give birth to the sweet principle, and the quantity of hidrogen and carbon that constituted it fixed oil, acquires several of the properties of volatile oil.

The only state in which it combines with lead.

And finally, that this last state of oil is the only one, in which it can combine with white oxide of lead.

Are plasters metallic soaps?

From the knowledge I had thus acquired of the theory of this combination of oils, I thought I should not neglect to examine how far the opinion of several chemists, who consider plasters as real metallic soaps, is well founded. The analogy between plasters and soaps can be confirmed only by observing in their respective combinations a similarity of phenomena, or at least of results.

Strong lie mixed with oil.

I mixed very pure soapboilers lie with olive oil, and exposed the mixture to the air under a glass jar. A week after there was but a very slight absorption; the soap had still a strong alkaline taste; and the oil of this soap did not dissolve entirely in alcohol. But at the end of six weeks the absorption of oxigen was complete; the soap was very white, and of a good consistence; the taste of alkali in it was faintly perceptible; dilute sulphuric acid extricated from it carbonic acid; and the oil proceeding from this decomposition had the same consistence as that from plasters, dissolved very readily in alcohol without the assistance of heat, and was precipitated from it by water.

No sweet principle in water after making soap.

I made some soap in the same way as soapboilers, and very attentively examined the liquor, that remained after the soap was completely formed; but I could not discover in it any trace of the sweet principle.

The effect different in degree, rather than in kind.

As the absence of this principle in making an alkaline soap probably depends only on a greater or less abstraction of carbon or of hidrogen; and in other respects the action of of oxigen on the oil, and the state of the oil, are absolutely the same in the fabrication of plasters and of soaps; I conceive, that plasters ought to be considered with respect to soaps the same as insoluble metallic salts are to alkaline salts.

Soaps with potash, soft from the nature of the alkali.

I satisfied myself, that the want of consistency in soaps made with potash is not at all owing to the state of the oil, but to the nature of the compound; for by treating with potash the oil obtained from a very hard soap of soda, I could form nothing but a soft soap.

XII.

*Account of a pretended pure native Magnesia**.

Fossil sold as pure magnesia by the German dealers.

THE German dealers in minerals sell under the name of pure indurated magnesia, and as coming from Moravia, a mineral substance found in amorphous masses of the size of the fist, covered with an earthy crust of a yellowish white colour, that slightly adheres to the tongue, and is a little greasy to the touch. Its fracture is dull, entirely compact, imperfectly conchoidal, and approaching to a plane: its colour is yellowish white: it has small cavities, lined with little mamillary projections, that appear to be composed of crystalline points when viewed in a strong light: it is sufficiently hard to scratch glass, and steel leaves a coloured mark on it, but it does not strike fire with steel: its specific gravity is 2·83: at its edges it is very transparent: it does not absorb water, or adhere to the tongue.

Its characters.

Before the blowpipe it cracks, and does not melt: it is not at all phosphorescent: it dissolves with effervescence in the nitric and muriatic acids; with the sulphuric it occasions a copious precipitate.

Difference of native magnesia.

It cannot be confounded with the native magnesia of Werner, which is easily scratched by steel, strongly adheres to the tongue, and is of the specific gravity of 2·88.

Its component

This mineral, analysed by Mr. Bucholz, yielded in 100 parts

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* Journal des Mines, January, 1807, p. 75.

parts. parts 28 of pure lime, 20·5 of pure magnesia, 1·5 of manganese with a little iron, and 48 of carbonic acid.

A muricalcite. Hence it appears, that it is a muricalcite, or a variety of the bitterspath of the Germans.

XIII.

Some remarkable Occurrences in Natural History. From the Rev. James Hall's Travels in Scotland.

WHILE Mr. Hall was at Elchies, on the north-west banks of the Spey, about fifteen miles from Elgin, he saw a remarkable migration of eels. The following are his own words.

Migration of eels. When I first observed them, it was about one in the afternoon of a Sunday. How long the eels had been transmigrating before I know not. They continued making their way up the river all that day till about eight in the evening, when it grew dark. They began again early next morning, but how long before five I cannot say. They continued to migrate for three whole days after I observed them, with only an interval of a few hours in the night. They kept as near the north-west edge of the river as they could: and, when there were bays in the edge of it, they went regularly round these, whether great or small. They were about ten abreast, and each eel about three inches and half long: they marched at regular distances, which might be about four feet; or rather three feet and half. There were stronger eels as a guard, and generally about five or six inches long. I observed the smallest and weakest ones always kept nearest the edge, where the current was least. From an accurate calculation a hundred passed every minute, making six thousand per hour.

In lines abreast at regular distances.

Smallest next the shore.

They proceeded at this rate for three days, from about half an hour before the sun rose till about half after he set, making about sixteen hours each day, in all about forty-eight hours, which, multiplied by the six thousand that passed every hour, make two hundred and eighty-eight thousand, most of which I saw pass; but whence they came, or what they were in quest of, I know not. They did not stay for one another, but each made the best of its way, wriggling

From a little before sunrise to a little after sunset.

wriggling with the utmost celerity; and when I pushed any of them farther into the river, they always came to the edge as fast as they could. Not one but had its head up the water. They seemed to be in great haste, and breathing hard, as small bubbles of air often rose up to the surface; and when, having caught any of them, I turned its head downward, so as to swim with the current, it would not, but with all the expedition in its power joined its new companions, and wriggled on along with them. As I could not be always there, I appointed others to watch their motions, and I found, though I could not see exactly how they acted, that, during the time it was beginning to grow dark, by a kind of signal, they all at once hid themselves in the sand or mud for miles at the same instant, and seemed not only under the command, but the protection of the larger ones, that, like officers, commanded them. Indeed, I saw sometimes large eels from twelve to fifteen inches long, making up the water now and then, about three or four yards farther towards the middle of the river, and about five and twenty yards behind one another; but whether they were connected with the general emigration I know not, though I rather suppose they were, as they were never above twelve or thirteen feet from the small eels, and often seemed to turn an anxious look towards their young friends. The young ones, as they were near the edge, were seldom an inch below the surface. Those about five or six inches long might be between one and two inches below the surface, being in deeper water, and the large eels went at a much greater velocity than the small ones. But, if they had any connection, or care of the small fry, they must sometimes have stopped short, or slackened their pace. I have seen the horse and foot guards reviewed by his majesty in Hyde Park, and ten thousand men performing the same action at the same instant of time; but the eels in the river Spey kept their ranks as regularly, and seemed to be as subservient to the greater ones, as any of the corps at a review are to the command of their officers.

Travelled hastily.

All at once hid themselves in the mud owards night.

Apparently under the command of larger ones.

Another fact he observed may be mentioned as an instance of the resources of animals, when prevented by circumstances from following the usual practices of their species.

Necessity

Magpie's nest in a gooseberry bush.

Necessity often leads both men and other animals to do what they otherwise would not. As I was one day amusing myself with the objects around me, on the road between Huntley and Portsoy, I observed two magpies hopping round a gooseberry bush in a small garden, near a poor-like house, in a peculiar manner, and flying out and in to the bush. I stepped aside to see what they were doing, and found, from the poor man and his wife, that, as there are no trees all around for some miles, these magpies, for several years, built their nest, and brought forth their young in this bush.

Strongly barricadoed.

And, that foxes, cats, hawks, &c. might not interrupt them, they had barricadoed, not only their nest, but the bush all around, with briars and thorns, in a formidable manner; nay, so much so, that it would cost even a fox, cunning as he is, some days labour to get into the nest. The materials in the inside of the nest were soft, warm, and comfortable to the touch, but, all around on the outside, so rough, strong, and firmly entwined with the bush, that, without a hedge knife, hatch bill, or something of the kind, even a man could not, without much pain and trouble, get at their young; for, from the out to the inside of the nest, it was longer than my arm. Frogs, mice, worms, or any thing

Assisted by the young in killing a rat.

living, was what they brought their young. One day, one of the magpies having lighted on a rat, but not being able to kill it, one of the young ones came out of the nest to its mother and the rat, while they were fighting about the bush, and assisted in killing it; which they did not accomplish, till the father, arriving with a dead mouse, also lent his aid. The poor woman told me, that, of the two magpies,

A chicken rescued by its mother.

the mother was the most active and thievish. She was also very ungrateful: for, although the children about the house had often frightened cats, hawks, &c. from the nest, yet she one day seized a chicken, and carried it to the top of the house to eat it. But the chicken's mother flew up after the magpie, and, having rescued the chicken, took it in her nib; and, as it was not able to fly, brought it down in the same way as a cat carries her kittens in her mouth, taking its neck in her nib; and the poor chicken, though it made a great noise while the magpie was carrying it up, was extremely quiet, and seemed to feel no pain while its mother

was

was bringing it down. These magpies had been faithful to one another for several summers, and drove off their young, as well as every one else that attempted to take possession of their nest. This they carefully repaired, and barricadoed in the spring with rough, strong, prickly sticks, that they sometimes brought to it jointly, one at each end, pulling it along, when they were not able to lift it from the ground.

The same magpies resorted to it for several years.

He adds: The same poor people, having one year lost the mother of some chickens, the cock became their protector, took them under his wings in the night time, and whenever it was cold; and continued this paternal care, notwithstanding that his wives often tried to seduce him from the chickens, to attend to themselves. Here too I was informed, a pigeon took care and fed the young himself; his wife, the mother of the young ones, having been seized and carried off by an insidious cat.

Chicken nursed by the cock, and young pigeons by the male.

XIV.

Facts respecting Indian Corn; *by* PROFESSOR PROUST*.

A Hundred parts of the grains of maize, subjected to distillation, left twenty four of charcoal. I converted into charcoal as much as when very dry weighed 3200 grains. Washed in distilled water, this afforded indications of phosphoric acid: it rendered lime water turbid, and precipitated nitrate of lead.

Charred maize contains phosphoric acid.

This charcoal was very difficult of incineration. Its ashes, to obtain which I was obliged to repeat its combustion five different times, weighed only 60 grains. They were carbonaceous, fitty, and without any particular taste. Water extracted from them but two grains.

Very difficult to incinerate.

Ten drachms of charcoal of maize, after being calcined for two hours, were reduced to seven. There was no appearance of ashes. These seven drachms, calcined for two hours and half, were reduced to somewhat less than five, without any ashes appearing. On calcining them three hours longer, they were reduced to two drachms thirty

Ten drachms by repeated calcination reduced

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* Journal de Physique, Vol LXIII, p. 449, Dec. 1806.

grains. The residuum was still very black, pasty, and adhered to the iron with which it was stirred. On washing it was reduced to two drachms 8 grains.

to about one.

Gave 18 grs. of saline extract.

These two drachms 8 grains were calcined another hour, which reduced the weight to about 60 grains; and were then lixiviated. The lixiviums mixed together yielded 18 grains of a saline extract, the taste of which was not perceptibly alkaline.

Phosphate of potash.

These 18 grains, redissolved and dried, would not crystallize, and were reduced to 14. Suspecting that the potash was saturated with phosphoric acid, I dissolved it in distilled vinegar, and afterward treated it with alcohol. This operation reduced it to 11 grains of that acidulous phosphate, which crystallizes in tetraedral prisms terminated by similar pyramids. I forget whether the faces of the pyramids answered to the faces or edges of the prism.

If 10 drachms of charcoal, the produce of 41 of maize, gave 14 grains of phosphate, 100 drachms of maize would yield but 34 or 35 grains, which is far from 40 per cent, as mentioned in Delamétherie's Journal. So great a difference could not have escaped such a man as de Saussure: it must have been an errour of the press therefore, or of the manuscript.

The obstinacy with which the charcoal of maize resists burning is astonishing: animal charcoal could not exhibit more.

Charcoal from the stalk burns in about half the time.

The same plant affords a charcoal of very different kind. The charcoal of the stalk, triturated with five sixths its weight of saltpetre, is consumed in a tube of a given diameter in 28 seconds. A similar mixture made with the charcoal of the grains requires 52 seconds for its being consumed in the same tube.

To Correspondents.

The communication with which N. R. D. promises to favour me will be very acceptable. I have likewise to thank him for his concluding hint, and shall certainly avail myself of the source it points out.

Mr. Cayley's communication is received, but on account of the engraving it could not be inserted in the present month.

I hope to be able to give some correct Observations and Results in our next respecting the Comet, which is at present visible.

Structure of covered ways.

Fig. 1.

Fig. 2.

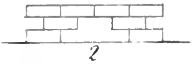


Fig. 3.

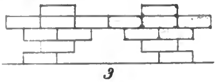


Fig. 4.

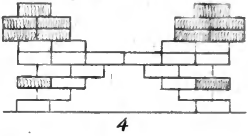


Fig. 5.

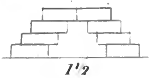


Fig. 6.

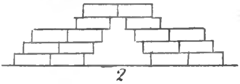


Fig. 7.

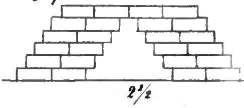


Fig. 8.

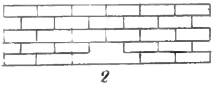


Fig. 9.

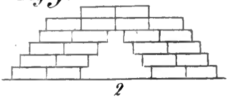


Fig. 10.

Fig. 11.

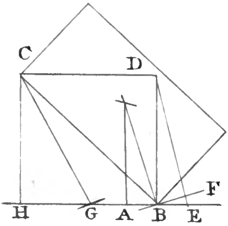


Fig. 12.

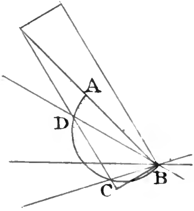


Fig. 13.

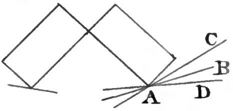


Fig. 14.

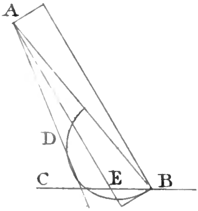


Fig. 15.

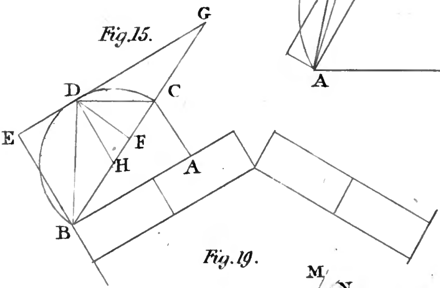


Fig. 16.

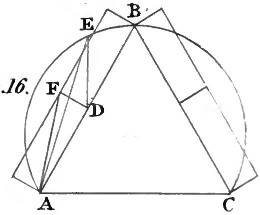


Fig. 17.

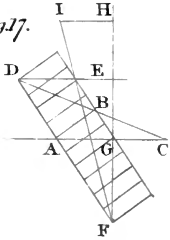


Fig. 18.

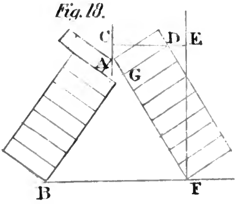


Fig. 19.

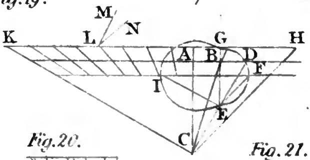


Fig. 20.

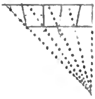
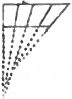


Fig. 21.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1807.

ARTICLE I.

Remarks on the Structure of covered Ways, independent of the Principle of the Arch in Equilibrium, and on the best Forms for Arches in Buildings. From a Correspondent (Apsophus).

To Mr. NICHOLSON.

SIR,

Ancient substitute for the arch.

THE subterraneous passages or tunnels of the Babylonians, and perhaps the cloacae of the Romans, were constructed, according to the opinion of the best informed antiquaries, by simply causing the bricks or stones of each of the side walls to project more and more as they rose higher, till they finally met in the summit. The most ancient remains of the Grecian buildings, for example, the treasury of Atreus at Mycenae, and other ruins in the Peloponnesus, exhibit in general over their doors, according to the reports of modern travellers, a triangular aperture, formed by large stones; the base of the triangle coinciding with the lintel of the door; and the pointed arches of the Gothic buildings are by no means universally so arranged, as to derive their stability from the proportion of their curvature in every part, to the pressure which would be produced, according to the commonly received theory, by the height of the superincumbent wall. As far as I know, this subject has not been mathematically investigated in all its parts, and I shall therefore submit to the consideration of your readers some propositions relating to the stability of overhanging walls and of triangular covered ways.

Bricks overhanging till they meet in a point.

I shall examine those cases only, in which the materials employed are equal rectangular parallelopipeds, whether bricks or wrought stones, and in the first place I shall suppose them destitute of all friction or adhesion, and placed horizontally. With such materials, it may be shown from the principles of the lever only, that a covered way may easily be made, not exceeding in breadth the length of three or four bricks or stones, and that the combinations, represented in Pl. VII, fig. 1··7, will stand in equilibrium without external support: and that if the breadth of the way be equal only to the length of two bricks, it may have any height of wall added over it without destroying the equilibrium (Fig. 8). These combinations are however incapable of resisting the pressure of any considerable force, and the method of building in this manner cannot be generally advisable; but the weight of two bricks is supported at the vertex in Fig. 9, and by extending the basis, and heightening the wall at the sides, a much greater strength might be obtained. It is however obvious, that a wall terminated in this manner would by no means necessarily exert such a pressure on any stones, forming a facing of the oblique surface, as is commonly supposed in the theory of the arch; on the contrary it is plain, that an arch might be turned under it, which would be sufficiently strong for every purpose, if capable of supporting little more than its own weight: and the same reasoning is applicable to the wall in contact with the lower parts of every common arch. Hence it becomes often eligible to construct the arch in such a manner as to be more capable of resisting a pressure near its vertex; and thus its form will approach in some degree to that of a pointed arch. The arches of bridges, on the contrary, have to support the pressure of materials of a very different description; and for this reason their greatest curvature should be near the abutments.

To support much weight, the width must be small.

A stronger form.

Arch turned under it.

Pointed arch.

Arches of a bridge.

Requisites to

In the next place I shall inquire into the conditions requisite

site for the stability of an oblique facing, composed of rectangular bricks or stones only, both with and without the consideration of the effects of friction. The simplest case that can be proposed is that of two bricks meeting each other, and standing on a perfectly smooth and horizontal plane, the centre of gravity of each being vertically above the lowest angle (Fig. 10). But if the base be widened, the surfaces supporting the bricks must be rendered oblique. The weight of the brick acts on a lever of which the length is A B (Fig. 11), in turning it round the point B; and this is resisted by the horizontal thrust at C acting on the lever B D, hence the horizontal thrust must be to the weight as A B to B D, and making B E = A B, the horizontal thrust at B combined with the weight will act in the direction D E, and the brick will be supported by a surface B F perpendicular to D E. Supposing the thickness of the brick inconsiderable, the centre of gravity being in the line B C, taking B G half B H, the line C G will be perpendicular to the surface on which it will rest in equilibrium; and this theorem may be of considerable use in carpentry, for finding the best possible direction for the abutment of a rafter. If the abutment is in the direction of the end of the block Fig. 12, describe on half the diagonal, A B, the semicircle B C D A; and C B, or D B, will show the position of a line, which being made horizontal, the block will be supported in equilibrium. If the horizontal line cross the circle between C and D the end B will slide downwards, but if between A and D, or B and C, it would be urged upwards, but the bearing will be transferred to the lower corner, and the whole will remain at rest: and this will be the case in all positions, when the circle falls wholly within the side of the block, that is, when its thickness is not much less than half its length. Thus two common bricks would remain firm in all elevations if placed with the narrow sides of their ends lowermost; even without any friction: but with the wider sides lowermost, they would slide down the abutments if the distance of their ends were more than about two, and less than fourteen inches.

the stability of an oblique facing.

The same principles applicable to the abutment of a rafter.

Bricks in an inclined position firm when the narrow side is downward.

The last additional circumstance which requires to be examined, with regard to the stability of bricks or stones in oblique

Effects of friction.

oblique situations, is the effect of friction or adhesion. This force may be considered, in all practical investigations, as proportional to the mutual pressure of the surfaces concerned; and the most convenient way of estimating its magnitude is to incline the surfaces to the horizon, until they begin to slide on each other. The angle at which this happens will be always very nearly if not exactly the same for surfaces of the same kind, and it may with propriety be called the angle of repose: and it is obvious, that any other force acting on the surface in the same angle as that in which the force of gravity acts in this instance, will be completely obviated by the resistance of the surface: and the friction will be to the pressure as the tangent of the angle of repose to the radius. If therefore the surface A B (Fig. 13) is calculated to resist the pressure of the block A without friction, by making the angles B A C and B A D each equal to the angle of repose, we may determine the greatest and least inclination which will be sufficient for retaining the block by the assistance of the friction or adhesion; the stability being greatest of all in the original situation A B. In the same manner the rectangular block A B, (Fig. 14) will be supported by its abutment as long as the horizontal line B C crosses the semicircle within the line A D, D A E being equal to the angle of repose.

Convenient mode of measuring it.

Angle of repose.

When two blocks of equal dimensions form an overhanging facing on each side of a triangular aperture, (Fig. 15) the upper one is in the same predicament as if it rested simply on a fixed abutment; the lower one is retained in its situation by the force of friction only. If A B C be the angle of repose, the direction of the force supporting each of the upper blocks will be B C; and if the vertical line B D represent the weight of the block A, B C will be the resisting force, and A C the friction, which counteracts the tendency of the block B to descend along the abutment, and this force is represented by E B. In order therefore to find the postion in which the block B will most readily slide away, we must make the proportion of E B to A C a maximum; and this will happen when the mean of the angles D B A and D B C is equal to half a right angle. For the sine

Case of a facing of two blocks on each side.

Worst possible position.

sine of the angle D B C being $\frac{DC}{BC}$, and its cosine $\frac{BD}{BC}$, and the sine and cosine of A B C being $\frac{AC}{BC}$, and $\frac{AB}{BC}$, the sine of D B A is $\frac{DC.AB}{BCq} + \frac{BD.AC}{BCq}$, and consequently E B $= BD.\frac{DC.AB + BD.AC}{BCq}$, which, divided by A C, is $BD.\frac{DC.AB}{AC.BCq} + \frac{BDq}{BCq}$, and this must be a maximum, consequently, B C being supposed constant, $\frac{AB}{AC}.\frac{BDC}{BC} + \frac{BDq}{BC}$ must also be a maximum. Then if we make D F perpendicular to B C, and the angle F D G = A C B, D F will be $\frac{BDC}{BC}$, F G $= \frac{AB}{AC}.\frac{BDC}{BC}$, and B F $= \frac{BDq}{BC}$, so that B G must be a maximum, which will evidently happen when D G is a tangent to the semicircle B D G, and the angle D B C half D H C, which is the difference between A B C and a right angle. If we wish to determine the proportion of the friction to the pressure when the friction is barely capable of retaining the block in its situation in the most unfavourable position, let x be the sine, and y the cosine of half the angle A B C, then the sine and cosine of half a right angle being $\surd\frac{1}{2}$, the sine of A B D or B D E, as well as that of B C D, will be $\surd\frac{1}{2}x + \surd\frac{1}{2}y$. Now, if the weight be B D, B C $= \frac{BD}{\surd\frac{1}{2}(x+y)}$, and the sine of A B C being $2xy$, A C is $\frac{2xyBD}{\surd\frac{1}{2}(x+y)}$: but the weight which produces the friction is three times the weight of a single block, the friction on the upper surface being derived from the pressure of the highest block, and that on the lower from the pressure of both blocks; while the tendency to descend belongs to the lower block only, and is therefore expressed by B D. $\surd\frac{1}{2}(x+y)$; hence we have the equation $\surd\frac{1}{2}(x+y) = \frac{6xy}{\surd\frac{1}{2}(x+y)}$; therefore $\frac{1}{2}(x+y)^2 = 6xy$, $(x+y)^2 = 12xy$, $x^2+y^2 = 10xy = 1$, $2xy = \frac{1}{5}$, which is the sine of A B C, and the friction is in this case to the oblique resistance as 1 to 5, and to the pressure nearly as 10 to 47: so that whenever the friction is greater than this, which

Case of the friction being barely sufficient to retain the block.

The friction sufficient to retain two pairs of blocks in most positions.

When more.

Friction of common bricks half the pressure.

18 bricks might stand at an angle of 60°.

which is almost always the case with the materials commonly employed, two pairs of equal blocks meeting each other in this manner will be secure from sliding in every possible position. If there are more than two blocks on each side, or if the lower blocks are larger than the upper one, the force tending to support the lower ones, which is derived from the pressure of the upper one, is twice the immediate friction occasioned by its weight, since the same pressure acts in two different places, and as long as this exceeds the difference between the friction and the relative weight of the lower block or blocks, they will be secure from sliding along the abutments. For example, in the case of common bricks or stone, the friction is at least half of the pressure; for if a brick be placed with the short side of its end downwards on another which is gradually raised, it will fall over before it slides; we may therefore safely estimate the friction as equal to half the pressure, the tangent of the angle A B C being ·5, its sine ·446, and its cosine ·892. Now if the whole aperture be supposed equilateral, the sine of D B A will be ·5, and its cosine ·866; and the sine of D B C nearly ·06; and the friction A C will be to the weight B D as ·45 to 1, and to E B as 9 to 10, so that 18 bricks on each side might be secured from sliding by the double effect of the upper pair.

Two modes in which they might give way.

There are however two other ways in which such a structure might give way: the lower portion revolving on its lowest point, and the higher either moving with it towards the opposite side, or sliding upwards in a contrary direction: and in order that the pile may stand, it is obvious that it must possess sufficient stability in both these respects. When there are only two equal blocks on each side, it is easy to determine whether or no their breadth is sufficient to prevent their both falling inwards, by describing round the triangle A B C (Fig. 16), a segment of a circle, making D E vertical, and joining A E, which must either coincide with the diagonal A F, or be below it. If there are more than two pieces on each side, in order to determine the stability of any joint A B (Fig. 17), let A C and D E be horizontal, and F E vertical, draw D B C, make E H = E G, and H I horizontal and equal to half A C; then if F I fall below

below B, the structure will not give way at the joint A B. The demonstration may easily be deduced from the principle of the equality of the horizontal thrusts in the case of an equilibrium: and it may be shown, that, if the aperture be equilateral, 15 common bricks on each side will stand, but 16 will give way at the sixth joint from the summit. The stability is however less considerable with respect to the second mode of failure, in which the upper brick slides outwards, while all below it fall inwards (Fig. 18). In this case the angular motion of the two portions is initially equal, the points A and B remaining fixed. The velocities of the centres of gravity reduced to a vertical direction are as the distances C D, D E; in order therefore that there may be an equilibrium without friction, the weight of the upper portion must be to that of the lower as D E to C D; and in all cases the force of A D, tending to support D F, is to the weight of D F, acting at its centre of gravity, as $A G . C D$ to $F G . D E$, or as $A G . \frac{C D}{D E}$ to $F G$. The friction of the upper block, of which the magnitude may be determined in the manner already shown, will act upon the whole length of the arm F G, while the weight of D F acts only on the length of half D E, consequently its effect must be considered as increased in the ratio of D E to twice F G. Thus if we take the example of an equilateral aperture, constructed with 8 common bricks on each side, and without cement of any kind, C D will be 9·3 inches, D E 2·7, and F G 21; hence the brick A will produce immediately a force equivalent to the weight of 3·4 bricks, and by its friction, which is $\frac{9}{20}$ of its weight, another force equivalent to the weight of 7 more; consequently the sum of both will be fully adequate to the support of the 7 bricks which form the lower portion of the structure. But if we make the same calculation for 9 bricks, we shall find that they will not stand without some external support.

Two modes in which they might give way.

7 bricks would stand by their own weight: 9 would not.

This compared with an arch.

It is obvious that in all these cases the addition of any load at the summit of the structure would very materially increase its stability, and that even a block, of sufficient magnitude to fill up the angle only, would enable us considerably to extend the base. It is also plain, that an inclined facing

facing of this kind is not distinguished from an arch by the want of a key stone, since the two middle blocks act nearly in the same manner as if they were united, except when they are forced outwards by the pressure of the lower parts; and a centre is as necessary for rasing a facing of this kind, as if it were an arch of any other form.

I am, Sir,
Your very obedient servant,
17 *Oct.* 1807. APSOPHUS.

Flattened arches over windows.

Postscript.—The equilibrium of the flattened arches, commonly placed over windows, may be determined in a similar manner, the principles being the same as those which are employed in the construction of Fig. 11 and Fig. 13. Supposing the blocks without friction and of equal height, if their divisions converge to one point, the lateral thrust will be equal throughout, and the whole will remain in equilibrium, provided that the ends do not slide outwards. In order to find the breadth which is within this limit, let the horizontal line A B (Fig. 19) pass through the centre of gravity of the blocks, draw any line C B from the centre of divergence C, make B D = A B, join C D, and let the vertical line B E meet it in E; then E F, drawn to the intersection of the semicircle E F G with the lower termination of the blocks, will show the direction of the abutment *d*, which will afford an equilibrium: and C H parallel to it will determine the greatest breadth that will stand. But since the blocks thus disposed, and supporting a wall, cannot slide away without displacing the superincumbent weight, the whole wall may be considered as adding to the height of the blocks, and the stability in every case that can occur in practice, must be complete: it is only necessary to reduce the horizontal thrust as much as possible, and this must be done by making the point C as near the blocks as convenient: the thrust being equal to the weight of the portion A H, supposing A C H half a right angle. If we wish to estimate also the effects of friction, let the segment E I G contain a right angle diminished by the angle of repose, then C K, parallel to E I, will be the direction of the abutment

abutment which will secure the blocks from sliding outwards, with the assistance of the force of friction. Generally however the obliquity must be much less than this; and the resistance of the abutment becomes capable of being exerted in the most favourable direction that its friction will allow, that is, in a direction more nearly vertical than the perpendicular to its surface, for example L M, M L N being the angle of repose; and if we wish to have the thrust equal throughout, we must employ blocks of such a form that their divisions may make, with the lines converging to C, angles equal to M L N; this however would lead us to make the middle blocks of the form of inverted wedges (Fig. 20), or at least to make their divisions parallel: but it will be sufficient in practice to cause the parts next the abutments to converge to points somewhat nearer than the point of convergence of the middle parts (Fig. 21); nor, indeed, has this arrangement any material advantage over the simpler form of lines converging to a single centre.

Observations on arches in general.

From a consideration of these principles, we may derive some useful inferences with respect to arches in general, especially such as are employed in buildings. The objects to be attained in the construction of an arch are to diminish as much as possible the horizontal thrust, and to secure the stability by such an arrangement as requires the least size in the blocks and firmness in the joints. The size of the blocks must be such, that the curve of equilibrium, under the pressure actually produced by the walls, may be every where included within their substance, and even without coming very near their termination; and the horizontal thrust will be less in proportion as the curvature at the vertex is greater, that is, other things being equal, as the arch is higher. Supposing the height of the wall supported by the arch to be very considerable in proportion to that of the arch itself, the curve of equilibrium must be very nearly a parabola: if the wall is raised but little above the arch, it will approach to a segment of a circle. In order therefore to find whether the size of the blocks is sufficient, describe a parabola through the summit and the abutments; and if it pass wholly within the blocks, they will stand; provided however that their joints are either perpendicular to the curve,

Size of the blocks.

Horizontal thrust.

Curvature.

curve, or are within the limits of the angle of repose on either side of the perpendicular. But if the wall is very low, and the arch flat, a segment of a circle will be more correct than a parabola. Hence it is obvious, first, that a segment of a circle is a better form for an arch than an ellipsis of equal height and span, although less pleasing to the eye, the horizontal thrust being less: secondly, that for the same reason, a Gothic or pointed arch is preferable to a Saxon or semicircular arch, when its height is greater; and even when the height is equal, an arch composed of two parabolic segments meeting in the vertex is stronger than a semicircular arch: for, supposing the wall very high, the depth of the arch stones of a semicircular arch must be at least $\frac{1}{13}$ of the span, in order that the arch may stand, but that of the stones of a Gothic arch, composed of two parabolic segments, may be less by one twentieth; the parabola of equilibrium touching in this case the internal limit of the arch at $\frac{23}{100}$ of its whole height above the abutments. If, however, the arch is flatter, a segment of a circle will be somewhat stronger than a pointed arch composed of parabolic or elliptical segments. When the arch is higher, it is obvious that a single circular curve is no longer applicable: and in this case, it is of little consequence whether the segments be circular or parabolic, either of these forms approaching sufficiently near to the curve of equilibrium, and both producing equally a much smaller horizontal thrust than a semicircular arch.

Circle preferable to the ellipsis.

Pointed arch best in certain cases.

In others the circular.

II.

Additional Remarks on the capillary Actions of Fluids. By ALETES.

To Mr. NICHOLSON.

SIR,

Capillary action of fluids.

IT has been observed, with apparent justice, by Mr. Laplace, that the force of capillary action, other things being equal, must be proportional to the square of the density of a liquid; and it is easy to deduce this result from the

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the demonstrations which you did me the honour to insert in your 76th number. The area of the triangle A E C (Vol. XVIII, Pl. I, Fig. 8), which shows the magnitude of the cohesive force at C, is proportional to the square of the line A C, representing the distance to which the force of cohesion extends; and if the same number of particles be condensed into any smaller space, the force will remain the same, and it will still be proportional to the square of the number of particles concerned; or, in other words, to the square of the density of the substance. The same remark is also applicable to the tension of the common surface of two liquids, or of a liquid and a solid; and this determination of the force ought perhaps to have been employed in the investigation of "the angle of contact of a solid with a fluid"; but it is very singular that the result of this investigation will be precisely the same, whether we proceed on the supposition of a tension proportional simply to the difference, or to the square of the difference of the densities. Thus if the density of the fluid C B E (Fig. 6) be called a, that of the solid B, b, and that of a second fluid, supposed to occupy the space C B A, c; if the tension be simply proportional to the difference of density, we may call the force acting in the direction B A, $b—c$, in the direction B E, $a—b$, and the difference of these, $2\,b—c—a$, must be equal to the force $a—c$ in B C, reduced to the direction B H, and must be represented by the line B H, if $a—c$ be represented by B C or A B, A H being $2\,b—2\,c$; or if A E be called $a—c$, A H will be $b—c$. Now if, instead of $b—c$, $a—b$, and $a—c$, we take their squares, the difference of the first two will be $c\,c—a\,a—2\,b\,c+2\,a\,b=2\,b\,(a—c)—(a\,a—c\,c)$ $=(2\,b—(a+c)).(a—c)$, which is to $(a—c)^2$ as $2\,b—a—c$ to $a—c$, and B H will be to B C in the same proportion as before. It is obvious that when there is only one fluid, and $c=0$, A H must be to A E as b to a, upon either supposition.

The two suppositions are however not indifferent with respect to many other cases of the actions of capillary forces. Thus if two liquids be capable of perfectly wetting a tube, supposing both of them to be contained in it at the same time, the whole weight supported by the force of capillary

action

Capillary action of fluids.

action would be the same as when the densest only is contained in it, if the tension of the common surface were simply as the difference of the densities; but it would be always less, if the tension were as the square of the difference.

It is very easy to determine this point by a simple experiment. Take a clean capillary tube, about one tenth of an inch in diameter, and immerse it in water, so that about half an inch of its extremity may remain empty: introduce slowly, with a bristle, a small quantity of oil in successive drops, so as to form a thin coating on the water. It will then be observed that the height of the fluid is very conspicuously diminished, and reduced to about three fourths, or two thirds. Your readers are not required to place implicit faith in the anonymous statement of an experiment: they may easily repeat it for themselves; and it is only adduced in support of a chain of reasoning founded on mathematical analogy.

In this respect therefore it cannot be allowed, that Mr. Laplace's second method of considering the effects of capillary action is so perfectly satisfactory as it appears upon a cursory examination; for both demonstration and experiment are in direct contradiction to his assertion, that "If the indefinite vessel, in which the parallelopipedon is immersed, include any number of fluids placed horizontally one above another; the excess of the weights of these fluids contained in the tube, over the weight of the fluids which it would have contained without capillary action, is the same as the weight of the fluid that would rise above the level, if the vessel contained only that fluid in which the inferior extremity of the parallelopipedon is immersed." Journal, Vol. XVII, p. 291.

Upon these grounds we may infer, that if a great number of liquids, becoming insensibly less and less dense, and the density of the last being inconsiderable, were contained in the same tube, their capillary action would be wholly destroyed: and it is not impossible that the capillary forces of various substances, of equal density, may be somewhat modified by the more or less abrupt change of density at their surface; since there is reason to think, as we have already

Capillary action of fluids.

ready seen, that the density of all bodies is a little diminished in the neighbourhood of an exposed surface; and when these bodies come into contact with others, new varieties may again arise, from the effect of the mutual cohesion on the superficial density.

We may also obtain, from this important law, a very distinct idea of the manner in which a drop of a lighter liquid spreads itself on a heavier: the sum of the tensions of the two surfaces of the drop being always smaller than the tension of the surrounding surface. When, however, one drop has thus diffused itself, it leaves an invisible film on the surface, which reduces the tension to an equality with that of the drop: and in this state of the surface, a new drop will remain undisturbed. The form of the lower surface of the drop will be of the same nature with that of the upper, its curvature being proportional to the height above a certain line, at which the internal and external pressure would become equal. It cannot therefore happen, except by accident, that the surface of the fluid should remain perfectly horizontal, although it may require a very nice examination to detect its curvature. For example, in the case of oil floating on water, the tension of the surface of the oil is only about half as great as that of the water, consequently the tension of the common surface must be about one fourth, or half as great as that of the oil: but the inclination of this surface to the horizon, when the drop is thin, is more than ten times as great as that of the upper surface; consequently the direction of the joint effect of the tension must be inclined to the horizon in an angle about one fourth as great as that which is contained by the surfaces of the drop; and the contiguous surface of the water must assume a similar direction in order to counteract this force; this will require a slight depression of the drop, and consequently a slight alteration of its curvature, but the general result will be the same. On the supposition of a force simply proportional to the difference of density or of capillary force, a still greater derangement of the general surface would, in this case, be required: so that the phenomena of drops, floating in this manner, have not, in fact, any tendency to establish such a law, although they may appear at first sight to favour it.

The

Capillary action of fluids.

The diminution of the effect of the tension of the surfaces of the drop, in consequence of their obliquity, appears to be exactly counteracted by the force derived from the curvature of the horizontal action; and the film left on the surface seems to occasion a resistance to all motion, which renders it difficult to observe the slight mutual attractions of the drops which must arise from the minute depressions that surround them.

I am, SIR,
Your obedient humble servant,
16 *November*. ALETES.

III.

On a Kind of Death, that may be presumed to be only apparent: by Mr. Du Pont de Nemours. *Read at the first Class of the Institute, Oct.* 28, 1806*.

Effects of heat vary according to the body to which it is applied.

IT has long been known, that the effects of heat and cold vary according to the nature of the bodies that are exposed to them. Extreme heat is necessary to liquefy steel, platina, or good porcelain: lead requires far less; and a portion much smaller still is sufficient for frozen water. On the other hand, the degree of cold requisite to render mercury solid is very great; while that which forms ice is very moderate.

Some vegetables not killed by the severest frosts

Leaves destroyed by it;

and stalks:

but some roots not, even though frozen.

Among vegetables there are many, the living principle of which resists the strongest frosts, these only occasioning disease in them, or, if I may use the term, setting them asleep. Our native trees lose their leaves in winter, without their stems being injured. Many of our herbaceous plants lose their stalks, though their roots retains their functions. There are plants still more robust, which, after their roots have been frozen in the ground, into which the frost has penetrated several feet beneath their ramifications, revive notwithstanding at the return of spring.

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* *Archives littéraires*, Vol. XIII, p. 8.

If

If we proceed to animals, we see the ant fall asleep in a very slight degree of cold; and the common fly does the same with every appearance of being dead. Nor are these the only insects subject to this lethargic sleep.

Ant, fly, and other insects, sleep in cold weather.

In the class mammalia we have dormice, marmots, and other sleepers, in which life appears to be suspended, when cold weather comes on. This suspension of life is so complete in some of the species, that their heart ceases to beat for whole months.

In some of the mammalia life suspended.

The snail and the toad undergo the same stupefaction. Several serpents exhibit a phenomenon still more surprising: they can be frozen so as to become brittle, and die if they be broken in this state; but if they be left in their holes, into which the warmth of spring penetrates by very slow degrees, they revive, and give proof they were not dead.

Snail and toad.

Serpents may be frozen without being killed.

It is in the season when their food begins to fail, when the fruits and herbs on which they fed disappear, after having fattened them by their temporary abundance, and in this fat supplied them with a narcotic to induce sleep as well as food to support them while it lasts, that most of these little devourers conceal themselves to sleep, and cease to afford prey to the larger devourers their enemies, which in their turn lose thought and motion.

Little fructivorous animals sleep first:

then the larger & carnivorous.

Those that would be deprived of food by the snow covering it, sleep till the snow melts, and a little longer. Perhaps for a similar reason the white bear, which lives by fishing on the seashore during the summer, and on the islands of ice in autumn, does not fall asleep till the ice united, thickened, and raised too high above the water, is no longer the resort of the seal. His means of subsistence continuing longer, a much severer cold is requisite to deaden in him the call of seeking it, than in the black bear in the first place, a great devourer of honey and vegetables, and next in the brown bear, which lives on animals that winter drives into their retreats before him.

They sleep while their food is wanting.

Hence the white bear retires later than the black bear;

or even than the brown.

That hunger should cease in these animals at the period when famine would take place, and in consequence of the same degree of temperature, is certainly a great benefit conferred on them by that *Intelligence*, which regulates every part

Providence has ordered it thus, to prevent them from starving.

part of the universe. If they retained their energy, they would perish from inanition. They are unacquainted with want; they feel not its pains; they incur not its danger. Nature saves them from it by that axiom, which has been considered as a jest: he who sleeps, dines. The state of stupefaction, in which their vital principle takes refuge as long as the cold and its companion want continue, occasions them no uneasiness: it commences even with a sensation of tranquil enjoyment, a sensation not unknown to ourselves.

Sleep is a succedaneum for food.

This stupor not painful, but pleasurable.

Not a disease,

It is certain, that being thus benumbed is not even a disease: that the drowsiness, which brings it on, is pleasurable: that sleep is an asylum, in which life fortifies itself, expending less, and husbanding its resources: that it is even a protection from the injuries as well as from the pains of cold: and that it renders living bodies more capable of retaining heat, by diminishing their conducting power.

but friendly to life,

and renders the body a worse conductor of heat.

When the cold increases with too much violence, man becomes insensible to it. If one of his limbs freeze, he does not perceive it, till he is informed of it by others*. On the contrary, he fancies himself at length growing warmer; and if no one of his limbs be more affected than another, his state seems pleasurable: he feels a seducing and delightsome propensity to sleep: he is angry with his friends, who urge him to walk on, and prevent him from indulging his inclination: he intreats them, to let him close his eyes for a few moments; and if they yield, he falls asleep, and appears dead like a dormouse.

When our limbs freeze, we are insensible of it.

Effects of general cold.

Let us venture to suppose, that he is not more dead in reality. There is no doubt in this case, but he would sleep like the dormouse, deprived of thought and of apparent vital action, at least as long as the same temperature continued. We may even presume, that he would really and completely lose his life at the expiration of a certain time, if he received no succour: for instance, after his fat was consumed, if he were not frozen as well as asleep; or after the habit of the vital functions had been entirely extinguished by a frost too severe, or of too long continuance, so as to stop the alimen-

His death in this case only apparent, like that of other animals:

though it may end in extinction of life.

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* This is very common in Poland and Russia.

tary transfusion of the fat, or stiffen the organs to such a degree, as to cause an absolute cessation of their secret movements, which appears to be an accident that the mammalia are liable to, though some of the serpents and gelatinous animals are not.

We may infer this by reasoning from a part to the whole.

This important point, that the general vitality of man is merely suspended in this case, is indicated by the repeated experience of northern countries and lofty mountains with respect to the particular vitality of frozen limbs.

Effect of cold on a single part.

It is by no means uncommon for the nose, ears, hands, or feet, to freeze in cold climates. If this be not quickly remedied, the contrast between the living state of the rest of the body and the incipient death of the limb attacked occasions this limb to sphacelate: Nature cuts it off by the strenuous resistance of the contiguous and threatened limb. If hasty means be adopted to remedy it, the too rapid distension of the capillary vessels by the fluids contained in them ruptures their sides, and extravasations take place; the communication, instead of being restored, is thus completely interrupted, and a gangrene is induced from the same cause. The same thing happens to those buds of plants, that are exposed to the rays of the sun, before the frost that covered them has been dispersed; while those that are thawed gradually in the shade receive no injury. "Nature," says the great Newton, "is consistent with herself." In all things, and at all times, she follows the same laws. The more she is observed with a philosophic eye, the more we perceive, that these laws are few, and combined with admirable, with astonishing benevolence.

Effects on vegetables similar.

They must be treated by diminishing the cold by slow gradations.

It is the same then with the limbs of animals, as with those of vegetables. If they be thawed with cautious slowness; if the part frozen be removed from the extreme cold it has experienced to a less degree of cold; if it be rubbed with snow, then immersed in the water of melting ice, and this be suffered to warm gradually, at first by the mere contact of the contiguous unfrozen portion of the limb, the part frozen will recover its local vitality. The cure may then be completed by very small successive additions of water a little warmer

warmer than that by which the part was thawed, and thus the limb be saved.

Similar treatment would probably succeed when the affection is general.

It is agreeable to all analogy to believe, or at least to suspect, that what thus happens with complete certainty to every limb, under such circumstances, must also take place with respect to the whole of the limbs, if the same or still greater precautions were observed, if the delay of succour be not too long, and if its application be not too rapid.

There are many facts in support of this.

Instances of effectual assistance being given to men, either entirely frozen, which however is perhaps doubtful, or at least profoundly benumbed, have occurred frequently in our glaciers, and are mentioned by our colleague Ramond. Haller regrets, that no means were tried with a man, who was thrown up by a torrent of melted ice long after he had been buried under it, at least as far as could be judged by his dress, though his skin was not in the least discoloured.

Portal's method good,

but too hasty.

Should be more cautious than in a mere local affection.

Our learned colleague Portal, in his excellent work on asphixies, points out for that which results from cold a method of treatment founded on very good principles; but I am apprehensive it would be found too hasty in its progress, as it is more so than that employed in the case of a single limb, and it may be presumed than a general affection, being more serious and formidable than an affection merely local, must require still more circumspection in the progressive graduation of the means employed: the rupture or even aneurism of a few vessels might have much more fatal consequences in this case.

The path is opened:

but not sufficiently explored.

Be it as it may, it is evident, that the first steps are already taken toward a constant and complete theory of the effects of cold on plants and animals, in respect to the degree of temperature, that suits each species. But if this new branch of the beautiful stream of knowledge be opened, and begin to excite our attention with advantage, we must confess, that it has not hitherto been sufficiently explored; that of the important phenomenon of the life or death of men frozen or simply laid asleep by cold we know nothing certainly, either with respect to the periods or the physiology of the transition from one of these to the other, the succession of means to be employed for the preservation of those whose lives

lives are thus endangered, or the path to be pursued for the advancement of our knowledge of them.

Desiderata.

Experiments have not been repeated sufficiently or with due regularity: they have neither been as scientifically directed, nor as scrupulously described, as the case requires. It appears to me therefore an object worthy the attention of the class, to point out these experiments; to indicate the proper path of inquiry with a view to improvement; to ascertain, whether man be in fact, like the bear and the marmot, an animal that cold benumbs and lays asleep without killing; whether it be true, that he can endure being completely frozen like the serpent; and whether in this extreme case a gently graduated warmth, applied in time, and slowly developed, would restore life. This might throw a great light on the question, which Drs. Herholdt and Rafn have yet treated but in part, and the term of which you have prolonged.

Difference in hibernating animals.

We see that several animals destitute of vertebræ, among those with vertebræ serpents, and among the mammalia a great number of the smaller species with cutting teeth, sleep three or four months, or even more, in a very moderate degree of cold: that a greater degree, and this in different proportions, is required for the several species of bears, the sleep of which too, in this state of imperfect death, does not appear to be so profound, or the suspension of life so complete: that man falls asleep in a still greater degree of cold: that it is more than probable, nay almost certain, that judicious means, prudently administered, would be capable of awaking him from this dangerous sleep, so near akin to death: and that it might be the same with other animals, or, indeed, with all animals, that are rendered torpid only by a degree of cold still greater than that which deprives man of the appearance of life.

Man.

Perhaps all animals may be rendered torpid by cold, and recovered.

The minutest particulars should be collected.

Every particular, even to the minutest, that the respectable monks of St. Bernard, and the guides to the glaciers of the Alps and Pyrenees, can give, should be collected. I even think that the class would do well, to call the attention of all the learned to this subject, and particularly to invite the four academies of Europe, that are best situate to pursue it with success, those of Petersburg, Wilna, Copenhagen, and Stockholm, to investigate this point of natural history.

Wars have nothing to do with the sciences.

To this political circumstances can be no obstacle. There is never war between the learned, between academies. Neither our emperor nor the king of England has disapproved your communicating with the royal society of London for the interests of the sciences. Scientific voyages have been respected by both parties. In the electorate of Hanover the university of Gottingen was protected by our army. The republic of letters, that great and noble benefactress of all polished states, ever preserves its honourable and friendly neutrality.

Suspended animation of drowned and suffocated persons already restored.

Already many of those who happen to be drowned, or suffocated by carbonic acid, are restored to life. These are two provinces, which Humanity has conquered from the empire of Death at the two extremities of his domain: for these two diseases, so long considered as deaths, are totally different in their nature, and require opposite means of cure.

Those princes, who dispute with each other the territories of the living, see with regret by how many murders they must be purchased: they would be the first to encourage the peaceful labours and fraternal correspondence, that might conduce to recover from the yet doubtful bonds of death a third class of its victims, men benumbed by frost.

IV.

Description of an Engine for affording Mechanical Power from Air expanded by Heat; by Sir GEORGE CAYLEY, *Bart.*

To Mr. NICHOLSON.

SIR, *Brompton, Sept.* 25, 1807.

Expansion of air by heat considered as a first mover.

I Observed in your last vol. p. 368, that some experiments have been lately made in France upon air, expanded by heat, applied as a first mover for mechanical purposes. This idea,

Sir Geo. Cayley's Expansion Air Engine.

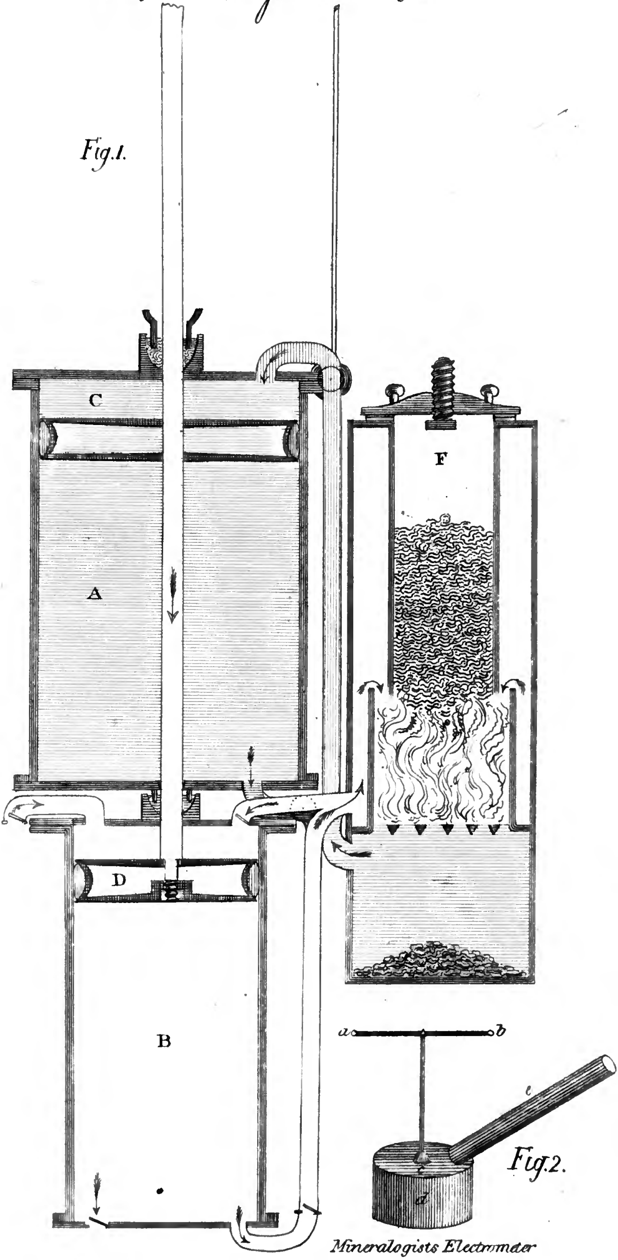

Fig. 1.

Fig. 2.

Mineralogists Electrometer

idea, as you justly remark, is by no means new in this country; yet I have not heard that any successful experiments have been made, exclusively upon this principle, in England, though you hint that something promising has been accomplished relative to it.

Its advantages.

The subject is of much importance, as the steam engine has hitherto proved too weighty and cumbrous for most purposes of locomotion; whereas the expansion of air seems calculated to supply a mover free from these defects. Under this impression I send you a sketch of an engine I projected upon this principle several years ago; it was made on a considerable scale at Newcastle, though I must confess without success in the result, which I attributed to the imperfect manner in which it was executed, the cylinders being made of sheet copper, and so irregular, as not to be rendered tolerably air-tight by any packing of the piston. I think there can be no doubt that the scheme is practicable in some way or other; and I conceive that the form of the engine here sketched will be the basis of whatever experience may prove to be additional requisite to perfection in the apparatus of the air engine.

Description of an engine; consisting of a blowing cylinder & a working cylinder.

A and B, Pl. VIII, fig. 1, are two cylinders, placed one above another; C and D, their respective pistons connected by one rod. F is a cylinder, containing a fire in a vessel within it in such a manner, that any air passing between the upper and lower portions of it must go through the fire. This vessel also contains a long cylinder, open at the bottom, and directly over the centre of the fire, for the purpose of holding coke or other fuel. This cylinder is covered at the top, and packed air-tight when it has served the purpose of permitting the fire to be kindled through it: and has been filled with fuel.

The cylinder B is fitted up to answer the purpose of a double stroke forcing pump, or bellows, to drive the air into the upper portion of the vessel F, from whence it passes *downwards* through the fire for the purpose of consuming the smoke (the fresh fuel being supplied from the reservoir above) in its passage through the more completely ignited cinders below. In this act the air is expanded; and, by means of pipes from the lower portion of F, it is conveyed alternately

alternately above and below the piston of the cylinder A. In each pipe is fixed a stop cock or valve, so constructed as to open a passage to the external air, when it shuts the connexion with the fire vessel. These cocks are worked by a plug frame.

From this construction it will appear evident, that whatever expansion the air receives, its pressure will operate alike upon the piston of the bellows and of the receiver; and that always in opposition to each other: Hence the power of the stroke will be in proportion to the excess of the area of the receiving piston, over that of the feeding one, multiplied by the expansive force of the contained air, and by the length of the movement.

The engine may be used for gas or decomposed water.

If, when the engine is well constructed, the expansion of the air in keeping up the fire be not found sufficiently sensible, still the form of the engine is such as to admit of either inflammable gas, oil of tar, or other inflammable matters, being injected, each stroke, upon the fire; so that all the heat generated by the united combustion may operate without waste; perhaps even a slight sprinkling of water, either upon, or round the sides of the fire, might answer the purpose. It scarcely need be observed, that a tube connected with a small forcing pump are the only things required for producing these effects.

I remain, Sir,
Your obedient Servant,
GEORGE CAYLEY.

V.

A Letter from Mr. Robert Harrup *to the Editor, on the Diseases of Wheat.*

To Mr. NICHOLSON.

SIR,

Reference to the author's former paper.

IN a former communication on smut in wheat, inserted in your Journal last year, I gave an account of some experiments which proved, that the *principal* cause of the disease is is smut mixed with the seed, and that although the diseased grains do not vegetate, they produce smut ears in the crop. It was also shown, that the seed corn prepared with lime prevented the disease from proving injurious in any considerable degree. At that time my observations led me to conjecture, that animalcula might probably be the primary cause. By reasoning from analogy, it still, however, remained doubtful, whether these minute creatures might not be the effect rather than the cause of the disease: I therefore resolved to attempt an analysis of the smut itself.

On the cause of smut in wheat.

From various unexpected circumstances I have been prevented completing the inquiry, and am at *present* only warranted in announcing, that *one* of the component parts of smut is the sole cause of that destructive malady, and that wherever it exists, whether in the seed or in the *soil*, the crop will be tainted. My chief motive in forwarding you at *this time* what may be deemed a premature communication is, to earnestly recommend to all practical agriculturists the following receipt for the preservation of seed wheat.

Receipt for preparing seed wheat.—Soak it well in lime-water: then keep it for some hours in lime & water. Dry it with lime powder, and sow.

Put the wheat gradually into limewater*, at the same time carefully taking off the light grains which float on the surface. After standing covered with the water to the depth of five or six inches, and the vessel closely shut during twelve or fifteen hours, stirring it twice or thrice in that time, the liquor is to be drawn off, and the wheat put on a floor. The following mixture is then to be poured regularly over it, viz. Lime, five pounds; boiling water, three gallons: stir them together till the lime is reduced to a powder, which will happen in a minute or two. It is then to be intimately mixed with the wheat, which after lying covered with cloths for some hours may be dried with lime in powder, and immediately sown. The above quantities are sufficient for five bushels of grain, and more ought not to be prepared together.

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* Limewater is made by mixing (boiling) water and quicklime together, about one pound of lime (more or less) will be sufficient for three gallons of water; and, after standing an hour or two in a covered vessel, pouring off the liquor, which, if not immediately used, must be kept in a vessel closely shut.

There

An alkali would probably have the effect of lime.

There can be little doubt but either of the fixed alkalis would have the same effect as the lime; but as they are now under trial, I do not venture to recommend them till I am certain of the result.

Other diseases of wheat.

Although smut is the most destructive, yet there are other diseases incident to wheat, which are sufficiently injurious to deserve attention. I shall therefore take the liberty of making a few observations on each of them.

In the second edition of Adams's Essays on the Microscope by Kanmacher a description of animalcular eels is given, in what is there called *blighted wheat.* The grains are said to be blackish, and contain a white soft substance, which separates into numerous filaments when put into water.

Account of the animalcular eels in diseased corn.

Needham was the first who discovered, that each of these filaments was a living creature. He sent some of the grains to M. Folkes, Esq., at that time president of the Royal Society, with an account of his discovery. They were delivered to Mr. Baker for examination, who after repeated trials could discover no other motion than a separation of the fibres or threads, which he imputed wholly to the elasticity in them; and perceiving no token of life, after watching them with due care and repeating the experiments, an account thereof was sent to Needham, who from his own trials found out the cause of failure, and advised him to steep the grains before he attempted to open them. This method proved successful; and at different times after this Baker made experiments with grains of the same parcel, without being once disappointed. He soaked a couple of them in water for the space of thirty-six hours, when, believing them sufficiently moistened, he cut one open, and applying some of the fibrous substance to the microscope in a drop of water, it separated immediately, and presented multitudes of the anguillulæ without the least motion or sign of life; but being taught by experience, that they might notwithstanding possibly revive, he left them about four hours, and then examining them again, found much the greatest number moving their extremities pretty briskly, and in an hour or two more they appeared as lively as these creatures usually

are.

are. These grains were four years old at the time the experiments were made.

M. Roffredi sowed some of the grains, which sprung up; but the ear was either wholly or in a great measure spoiled, being filled with these eels. He also found them in other parts of the plant. In order to disengage which, it must be soaked in water, and then compressed a little. At first sight they seemed to resemble the foregoing; but a more accurate inspection showed, that they were different in structure, and much more lively than those which were procured from the dried grain. They also increased in size in a certain proportion to the plant, so that at last they were observed with great ease by the naked eye, being two tenths of an inch long, and nearly one tenth in diameter.

The diseased grain propagated, and had the same kind of animalcula.

This disease, which I am informed is known by the name of *ear-cockle*, or, in some parts of the country is called *burnt-wheat* from the appearance of the grains, claims particular attention, as it is by no means uncommon, and never fails to prove highly injurious. I saw a field of wheat the present season, in which ears of this sort were so abundant, that it could not be worth above one third of the value had it been clean: and I have heard of several more.

The disease is called the ear-cockle, or burnt wheat.

The cause seems to be either in the seed or in the soil, or perhaps both contribute; but it seems by no means to be influenced by the weather. On a superficial view, the ears have much the appearance of those of smut, but on closer examination are found to be very different. The grains, before they are ripe, are of a dark olive green colour, not exactly the shape of sound grains, and contain a white soft matter, which does not fill them exactly. When at maturity they are black, and by keeping become considerably harder than common wheat, and have much the appearance as if they had been scorched. When they are opened in this state, the black hard crust is found to be about one third of their thickness, surrounding the white substance. The white matter is the anguillulæ, as mentioned before. By viewing them with a magnifier before they are removed, they have somewhat the appearance of very fine cotton fibres coiled together, and no motion is perceptible amongst them. But if a recent grain is opened, and a few of them taken out

Inquiry respecting its cause.

out on the point of a quill, and in that situation examined by a hand magnifier, a slow yet very perceptible motion in every direction may be seen in the extremities of those that do not adhere closely to the main body. Indeed they seem to be incapable of motion in any great degree till put into water, being glued together by a viscid matter. When taken from the recent grain and put into water, they are seen to use every exertion to disentangle themselves, which is generally a work of time.

Life of the animalcula in the grain, &c.

It would appear from these facts, that they enjoy existence while enclosed in their dark cells; and probably the small space which they do not occupy in the grain, and which appears empty, is designed to contain air, which I believe is absolutely necessary to the existence of every creature however minute. It also appears, that when the grains become dry animation is suspended, and continues so till moisture is again added.

Suspension of animal life.

That animal life can be suspended for four years at least, as appears from the experiments of Baker, already noticed, and how much longer we know not, and be then restored by a drop of water, must ever excite wonder and admiration. The reason why Baker failed in restoring life by immediately putting the anguillulæ from the old grains into water seems to be, that the *sudden* application of that fluid was too much for their delicate frames, for when the grains were previously soaked, so that the water gradually penetrated the containing crust, he succeeded.

Grains eight or nine months old do not require to be soaked, as the anguillulæ will revive in a few hours, when put immediately into water.

The author never succeeded in raising any plant from this diseased grain.

I repeatedly sowed some of this diseased grain, but never succeeded in raising a single plant, nor even in producing the least appearance of vegetation. Indeed I had little expectation of success, for the whole of the substance contained in the seed consisted of animalcula, excepting a very minute portion of flocculent matter, which could only be discovered when in water. I took up some of the grains after lying in the ground upwards of nine weeks. Several of them were empty, and others contained a few of the anguillulæ, which moved briskly the instant they were put into water.

water. One of the grains was filled with a brownish substance full of perforations, from which issued small worms discernible by the naked eye. They continued to live in water, and upon examination corresponded in all respects with those seen by Roffredi in the plants which he raised. Since that time I have opened several recent grains, which contained a few of this sort, as well as the anguillulæ. They were more than twice the diameter, and about the same length. Perhaps the grains which vegetated with Roffredi might not have been so much diseased as those I sowed, for that some are only partially infected I accidentally discovered. The first parcel I procured were only blackish on one side, and contained very few animalcula. The rest of their contents appeared similar to that of sound wheat. There can therefore be little doubt, that had they been sown, they would have come up. However that may be, it is certain they do vegetate, for I have since that examined several roots of this diseased wheat, and frequently found the grain from which they sprung to be similar to that in the ear.

but he thinks it possible and probable.

Examination of the anguillulæ in the microscope.

None of the anguillulæ I examined exceeded the two hundredth part of an inch in diameter, and they were in general about twenty-six times that length. When viewed with a hand magnifier they appear of a silvery white, but when placed in the microscope under a highly magnifying power, they are of a bright chesnut colour. A row of transparent globules, placed at regular distances, run down each side of the body till within a third of the extremities. Last winter I preserved an immense number of them three months in a watch glass, by frequently adding fresh-water to them. They did not increase in size, neither were they changed in any respect. If they are not endowed with sight, they are at least very much affected by light. When placed in the rays of the sun, or in the light of a candle, in a minute or two they run together into one or several bunches or knots, and continue so for some time. This effect is most distinctly viewed by means of the solar microscope. When placed in that instrument, they are seen floating from every part of the fluid to form themselves into an apparently inextricable bunch,

They are long lived;

and affected by light.

bunch, and those which arrive last use every exertion to bury themselves amongst their companions.

Many other particulars relating to them might be stated; but as the subject seems not to lead to any purpose of utility, I shall close this account of them by mentioning two facts, which may be of advantage to be known.—*They are instantly killed by adding a few drops of limewater to the water containing them. And if the entire grains are steeped in limewater from twelve to twenty-four hours, the anguillulæ they contain are incapable of being revived, either by placing them in fresh water, or by any other means.* The evident inference from these facts is, that the same preparation which has been recommended for the prevention of smut will also prove effectual for ear-cockle.

The great value and importance of inquiries into the diseases of grain.

An experimental inquiry into the nature, causes, and cure of the diseases of corn would prove a valuable acquisition; indeed it is surprising, considering the importance of the subject, that nothing of this kind has been attempted. Sir Joseph Banks, in his *Short Account of the Cause of the Disease in Corn called by Farmers the Blight*, &c., published in the beginning of 1805, presumes, that the want of actual observation will be abundantly supplied by those, "whose leisure and residence in the country enable them to examine, not only the progress of the crops, but the origin and advances also of all those obstacles which nature has opposed to the success of agricultural labours." We have still to regret, that the expectations of that justly celebrated philosopher have been hitherto disappointed; for I believe little or nothing has been given to the public on the subject since that time.

Two species of fungi in the blight—

From the few observations I have been able to make on the blight, it appears, that the parasitic fungi which occasion it are of several different species, and that none of them are particularly injurious, except the dark coloured, such as that which destroyed the crops in 1804. I was never able to discover, that the orange coloured, which appears early in the summer, was the dark coloured in an immature state.

the yellow and the dark.

On the contrary, I have always found them different; the yellow changing to a dirty ash colour as it approached to decay, and the dark coloured continuing unchanged from its first appearance. In the summer of last year I carefully examined

examined a number of wheat fields at different times, and found the straw untouched by the blight, and in every respect in a healthy state. Every thing continued to have a promising aspect till the heavy rains which fell a few weeks before the commencement of harvest; the dark coloured blight then began to show itself, and continued to spread considerably; and, if we may judge from its progress, had the cutting down the crops been delayed a fortnight or three weeks longer, it would have proved equally destructive with that which took place in 1804. A sufficient number of facts is yet wanting to warrant any conjecture on the manner in which the fungi were produced by the heavy rains; however, we may be pretty well assured, that all diseases which depend so much on the state of the atmosphere, must ever baffle human ingenuity to prevent. The earlier the crops are ripe, the less liable will they be to be injured, and the only remedy at present known is, to cut down the crop whenever the blight begins to make any progress.

Appearance of white and apparently perished plants in a crop of wheat.

Although the above are all the principal diseases of wheat, at least as far as my observation goes, I cannot take leave of the subject without noticing a very common appearance in wheat crops, which is more or less frequent every season, and varies considerably in different fields. Some time after the corn is come out in ear, but yet in a green, unripe state, we frequently observe several plants entirely white, with every appearance of having perished. As harvest approaches, and the corn changes to a bright yellow, these plants, particularly after rain or heavy dews, put on a blackish appearance, as if sprinkled with a black powder. Upon examination by the microscope, this appearance is found to be occasioned by innumerable tuffets of a parasitic plant growing out of the pores of every part of the plant which is exposed to the action of the air, very much resembling some species of the plantulæ of mould; even the sap vessels of the straw are frequently filled in different parts with a black substance, easily discernible by placing the straw between the eye and a strong light. The grains, as might be expected, are small in size, and of a reddish brown colour. Most probably this affection arises from some decay at the root, but whatever that may be I have not hitherto discovered. I have frequently

quently examined the roots with attention, both in an entire state and when dissected, but could never perceive the smallest difference between them, and those of healthy plants.

With the hope that the subject will be further investigated by those whose leisure affords them opportunity, and whose abilities are adequate to the inquiry,

I remain, SIR,

Your obedient humble servant,

Chobham, Oct. 1, 1807. ROBERT HARRUP.

VI.

Description of a simple and convenient portable Electrometer for Mineralogists. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Portable electrometer.

LOOKING over Brongniart's Treatise on Mineralogy, lately published at Paris, it appeared to me, that the electrometer he has figured and described for the purpose of detecting electricity in minerals deserves to be better known than I imagine it is; its simplicity rendering it very portable, and always ready to use, without being liable to be out of order. If you entertain the same opinion of its convenience to the mineralogical traveller, perhaps you may find a corner for it in some plate or other of your valuable and useful publication.

I am, SIR,

Your obliged reader,

O. N.

Method of detecting electricity by it.

To discover the production of electricity in a stone by heat or friction, it is brought near to either end of the slender brass needle, *a b*, Pl. VIII, fig. 2, and whatever kind of electricity the stone has acquired, it will make the needle move, if this be done with proper precaution.

But

Mode of distinguishing whether it be positive or negative.

But to distinguish the kind of electricity developed in the stone, the electrometer must be insulated, by placing it on a cake of resin, and positive or negative electricity may be communicated to it in the following manner. Place a finger on the metallic base *c* of the electrometer; and bring within a proper distance of it a rod of glass, or resin, *e*, electrified by friction. When the instrument may be presumed to be charged with the kind of electricity desired, withdraw first the finger from the base, and then the rod of glass or resin. The stone being then presented to one of the knobs of the electrometer, *a* or *b*; if the stone repel it, the electricity it possesses is of the same kind as that imparted to the electrometer; if it attract it, it is of the opposite kind.

Some stones communicate positive electricity to resin by friction.

Some stones communicate positive electricity to the resin on which they are rubbed. To discover this property, a piece of sealing wax may be flattened on a smooth substance, and the stone rubbed gently on this plane surface. The kind of electricity the resinous matter has received may then be found by means of the insulated electrometer.

VII.

A Method of Sowing Clover, and a new Plan for a Rotation of Crops; by Mr. DE VINCENS, *of Thède, near Clermont*[*].

Clover sowed on rye

AT the end of winter, after the hard frosts are over, and when the weather is dry, I sow twenty pounds of clover seed on a *septerée* of land, about 1200 toises [2500 yards] in circumference, sowed with rye the preceding autumn. This seed is harrowed in with a common wooden harrow, which is drawn all over the field by a pair of oxen. Instead of injuring the rye, this harrowing accelerates its growth, and it actually affords a finer crop than rye that has not been harrowed.

and harrowed in.

When the rye is ripe, I cut it in the usual mode; and

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[*] Sonnini's Biblioth. Physico-économique, Oct. 1807, p. 14.

when

when it is carried, the clover forms a green sward intermixed with yellow stubble.

Clover mown in September, and cattle turned on it.

The clover may be mown in September the same year; and cattle may afterward be fed on it till the frosts come on, without inconvenience.

2d year cut three or four times, and then pastured

The second year the clover will be in its most productive state: it may be mowed at least three times for hay, or four for green fodder; beside which it will afford an abundant pasture till the frosts come*.

3d year cut twice, and then ploughed in.

The third year I mow it but twice, and when it has shot up a little after the second cutting, I plow it in with the simple plow of the country. I afterward plow and harrow it repeatedly, till the land is brought to a proper tilth for rye or wheat, which I sow without any manure.

Clover a natural manure for wheat,

particularly with plaster of Paris.

It is to be observed, that clover is a natural manure for wheat; that, as is well known, a good crop of wheat may be had any where after a good crop of clover; that the wheat will be so much the better, if the clover have been dressed the preceding spring with 2 cwt. of plaster of Paris to every *quartelée* of 300 toises scattered over the surface.

Wheat after clover requires weeding.

Wheat succeeds very well after clover without plaster or or any other dressing, but it is indispensable to weed it, and for want of hands I shall in future prefer rye, which when once sown requires no farther care.

Advantages of this practice.

To prove the advantage of my practice in every respect, observe, that on rye † well manured I sow my clover in the spring. This clover costs me nothing either for ploughing or manuring; but only the price of the seed] and the labour of sowing and harrowing it].

A septerée of clover furnishes me, beside the feed at the end of the first and second years, at least five crops of hay during the two years that it wholly occupies the land.

Rotation for corn every other year.

* If the farmer would adhere to the order of cropping where corn is sown every other year, he must plow in the clover after the second mowing of this year.

Rye better than oats with clover.

† I prefer sowing clover on rye to sowing it on oats, in the first place because the crop of rye is more valurble, and in the next because experience has convinced me, that the clover is more forward; no doubt because the rye being already at some height, it germinates and grows more advantageously under its shelter, particularly in dry seasons.

Calculating each crop to give me a ton of hay, at £2 10s. a ton, its medium value since the revolution, this would produce me £12 10s. for the two years, at the expense only of the seed, mowing, making, and carrying.

Average price of clover hay in France £2 10s. a ton.

To this considerable return may be added the saving of dung for the rye or wheat sown immediately after, and a fine crop of which is certain; if no unforeseen and irremediable accident, as hail or frost, disappoint our expectations.

A fine crop of rye or wheat without manure.

After this first crop of corn without dung, I immediately sow rye, manuring it well.

Rye.

As soon as this crop is carried off the ground, I sow winter pease immediately on the stubble, covering them in with one single plowing and harrowing. This crop has never failed me: it is earlier than that of corn, and nearly at the same time with winter barley.

Winter pease.

When the pease are carried I plow and dung the ground, and sow it with rye.

Rye.

After this rye I crop the ground in the spring partly with potatoes, partly with other roots, and the rest with vetches mixed with oats, to be cut as soon as the seed has formed, and employed as pasture. All these crops are previously well manured.

Mixed crop.

When the ground is cleared of these, I manure it well, and begin my rotation again with rye, on which I sow clover in the spring, as I mentioned above.

Rye and clover again.

If I intended only to sow rye, I should not manure the land, after its having been well manured for the roots and pasturage, and my rye would be the fuller eared. But as the rotation of clover will leave the land three years without dung, I consider this dressing as necessary.

The manure necessary on account of the clover.

I shall recapitulate my rotation of crops in the following table.

Rotation of crops.

1st *year*. Rye manured: clover sown on it in spring, during dry weather, and harrowed in, without fear of injuring the corn.

2d *year*. Clover in its most productive state. If you would have a crop of clover seed, the second growth this year must be left to ripen. With this view it should be mowed the first time in May or June, when in full flower, and then left to stand for seed.

3d *year*. Clover to be mowed only twice. The third growth to be plowed in for manure.

4th *year*. Rye, or wheat, without manure. The wheat must be hoed.

5th *year*. Rye manured.

6th *year*. Winter pease, sowed on the stubble, and covered by one plowing and one harrowing.

7th *year*. Rye manured.

8th *year*. Vetches, oats, and turnips to be fed off, and potatoes: the whole well manured.

9th *year*. Rye manured, on which clover is to be sown in spring as before.

Observations.

Observations on this rotation of crops.

Corn once in two years.

In the first place it will be seen, that I take care to have a crop of corn once in two years. If my clover interrupt this course by occupying the land two years following, this is balanced by two successive crops of corn after the clover; the first without manure, the second with.

Crops changed to rest the land.

As the land is rested by changing its produce, I do not sow clover on the same land till after an interval of five years.

Crop every year, with manure once in 2 years.

My land produces a crop of some kind or other every year, and in nine [eight] years I manure it but four times. Consequently I have twice as many crops as if it were fallowed every other year, without more expense of manure, and I might almost say without more labour.

Pease do not exhaust the land.

Fallowing may be rejected without clover.

It is particularly to be observed, that, except after the clover, my land has never two successive crops of grain, unless the winter pease be reckoned so, which do not exhaust the land; and that it is so ordered, as to be cropped with corn one year, and with green feed or roots the next. This last rotation interposed between the crops of corn before the clover comes round again, appears to do away the necessity of fallowing, without the assistance of clover, which however I am far from wishing to exclude by this observation.

ANNOTATION.

Remarks on constant crop-

A continual succession of crops without fallowing has a specious appearance of being profitable at first sight; but as an intelligent friend of mine, an excellent practical farmer, observes, a fair estimate of all the advantages of fallowing is seldom taken into the comparative calculation. That land may be brought to bear a crop of some sort or other every year, there can be no doubt; though it is obvious, that precisely the same management cannot suit every species of soil. But when we compute the true value of this practice, we should not reckon from the produce of a few years at first, which will probably be higher than the average at the long run: at the same time we must consider, where a proper rotation of fallows is observed, the saving of seed, of labour in sowing, cutting, inning, threshing, and carrying to market; and the advantage of having the land clean, and reduced to a proper tilth by repeatedly plowing and stirring the soil at times when the cattle and servants of the farm are not required for more necessary labour. Thus when we take into account the certain additional expense on the one hand, to be deducted from the produce of two moderate or perhaps indifferent crops; and on the other the savings in one year, and the produce of a good crop in the next, beside the certainty of keeping the land in heart; we may perhaps be inclined at least to doubt on which side the balance preponderates, in cases where the too sanguine speak decidedly without hesitation. At present it may be presumed no country in Europe can be put in competition with our own for agricultural skill; certainly France cannot: as however it stands foremost among the useful arts, whatever seems likely to suggest any hint toward its promotion is not undeserving of notice, from whatever quarter it may come.

ping compared with occasional fallowing.

VIII.

A Memoir on Roman Alum, compared with different Kinds manufactured in France; by Messrs. THENARD *and* ROARD. *Abridged by Mr. Bouillon-Lagrange*[*].

THE art of manufacturing alum originated in the East, and remained for a number of years the exclusive property

History of alum works.

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[*] Annales de Chimie, vol. LIX, p. 58, July, 1806.

of some cities in Syria. In the 15th century it was brought into Europe, and soon became common in Italy, where that of Tolfa required great reputation by the constant uniformity of its product, as well as its purity. But this art, still in its infancy, was very slowly improved; and it was not till three hundred years after, when chemistry was sufficiently advanced to discover the intimate nature of substances, that it made some progress. Margraff, Monnet, Erxleben, and Bergman, then analysed all the kinds of alum most generally known. Bergman in particular was so well aware of the importance of the question, that he wrote a dissertation of considerable length on the history, preparation, analysis and purification of alum; in which he lays particular stress on the necessity of carefully separating the iron from it by repeated crystallizations, by means of which he says he manufactured alum even purer than that of Rome. He had some erroneous ideas however, which modern chemists have corrected.

Mistake of Bergman.

Potash necessary.

Mr. Chaptal first perceived Bergman's mistake in proposing to saturate the acidulous solutions with clay; and the simultaneous discoveries of Decroissilles, Chaptal, and Vauquelin, on the action of potash in the formation of alum, and on the various combinations of the sulphuric acid with alumine, left us nothing more to wish on these heads.

Vauquelin.

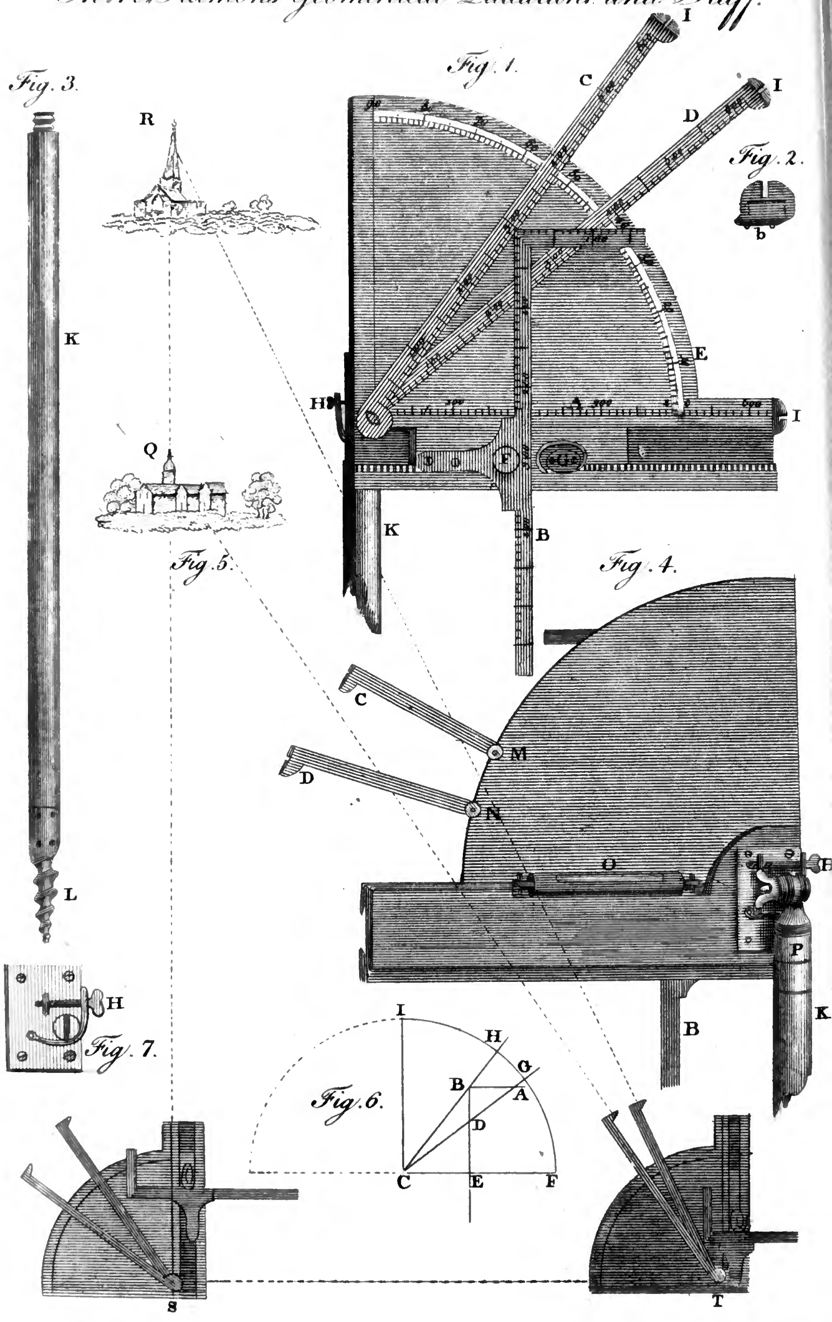
Alum a constant salt,

but frequently contaminated with ammonia and iron.

Preference given to Roman alum by the dyers supposed to be without cause.

The knowledge thus acquired gave rise to several alum works, the produce of which, though approaching that of Tolfa, was not able to diminish the preference given it by all manufacturers, or to lower the price it bore. The learned awaited with impatience the solution of this important problem, when Mr. Vauquelin made known the result of his analyses of Roman alum compared with that of some other kinds most generally known. He showed, that the proportion of the constituent principles of alum is always the same, and that they differ only in consequence of a few particles of sulphate of ammonia and of iron, which he could not find in any appreciable quantity in Roman alum. He concluded his interesting analysis by saying, that, if there were so much difference in alum as the dyers say, chemistry in its present state was not able to detect the cause; but that it appeared to him more natural to suspect them of exaggeration:

Mr. R. Salmon's Geometrical Quadrant and Staff.



ration; and he concluded, that any alum, free from iron, would be as good for use as the Roman. To place this beyond question however, it would be proper to make comparative experiments with them in dyeing.

Encouraged by this some skilful manufacturers farther improved the produce of their works, and supplied the shops with alum, that wanted only a different name and appearance to rival the Roman.

Factitious alums made to pass for Roman.

But the predilection for Roman alum was soon abused; and considerable quantities of the alums of Liege and Javelle, to which all the outward appearance of that of Tolfa had been given, were sold. Most of the dyers and manufacturers however, who at first had been imposed on by this appearance, were induced afterward to be only so much the more eager for the true Roman alum: for it was much more easy to deceive than to convince them.

Prize offered by the Society of Encouragement.

Directed a comparative examination of French and Roman alum.

Such was the state of our knowledge repecting alum, when the Society of Encouragement, ever animated with a desire of giving our own manufactures a great preponderance over those of foreign countries, thought fit to offer a prize for the means of giving our alums all the properties of that of Rome. The society having employed Messrs. Thenard and Roard, to compare the Roman alum with that of French manufacture*, in order to ascertain the difference both of their nature and effects; these gentlemen, after having acquainted the society with the results of their inquiry, submitted them to the Institute, before whom they laid the numerous experiments they had made to solve the question.

Their precautions.

They were very careful to obtain the French alums in the state in which they are commonly sold, and accordingly procured them themselves either from the manufacturers or from the warehouses, taking at a venture a great variety of crystals from among considerable heaps.

It was of particular importance likewise, that they should procure unmixed Roman alum. Accordingly they applied to Mr. Schlumberger, their colleague, who has the care of the warehouse at Paris on account of the proprietors, and to

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* Messrs. Thenard and Roard say nothing of the English alum, though it appears from Vauquelin's paper, that the French consumers give it a decided preference over any made in France. T.

whom all the Roman alum is directly sent. Accordingly he had a great number of casks opened, that they might examine the external appearance, figure, and colour of the crystals; and from each they took what they judged proper, to make up in the whole the weight of 30 kilogrammes [about 67lbs.] The superiority of the Roman alum over all other kinds met with in the shops being the object of the dispute between the chemists and manufacturers, Messrs. Thenard and Roard conceived, that to decide it an analysis on a large scale alone would be insufficient; and that it was particularly necessary, to make numerous and very accurate experiments with the best known colouring drugs on the fabrics most in use: and they conceived, that if, from the whole of the facts, they could discover any necessary and direct connexion between the results of the analysis and the practical experiments, between the principles found by the one and the effects obtained by the other, all the difficulties would be elucidated, all doubts removed, and theory conjoined with experience would lead them to a complete solution of the question.

Plan of their proceedings.

Alums compared.

The French alums subjected to their researches comparatively with the Roman were those of Bouvier, Liege, Javelle, and Curaudau.

Before they compared the effects of these various alums in dyeing, their first care was to subject them to all the analytical trials already made by the chemists we have mentioned: thus at the same time they determined the proportions of acid, alumine, potash, and water, and observed, as Bergman, Vauquelin, and Chaptal had done before them, the dangerous influence of iron. The experiments they made on this subject constitute the first part of their memoir.

Part I.

Analysis of Alums.

Analysis.

For the sulphuric acid.

Exp. 1. To determine the proportions of sulphuric acid, they dissolved in 16 litres [or wine quarts] of water 489 gr. [15 oz. 6 dr. troy] of each of the preceding alums entirely freed from the dust that covers the surface of some of them*.

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* The rosy dust on the Roman alum yielded on analysis saturated sulphate of alumine and potash, silex, and oxide of iron.

In

Into the limpid solution of each, when completely dissolved, they poured muriate of barytes to saturation, and even added a very slight excess, that they might be certain all the sulphuric acid was thrown down. Each of the solutions required precisely the same quantity of muriate of barytes. The precipitates were washed in 90 quarts of water; and when that of the last washing was rendered but very slightly turbid by nitrate of silver, as the water used for the purpose itself was, they were collected with the greatest care.

After being dried, and calcined at a red heat for an hour, the weight of the sulphate of barytes produced was:

No.		grammes.
1,	Roman alum	489·42
2,	Alum of Bouvier	490·70
3,	——— Liege	490·27
4,	——— Javelle	490·27
5,	——— Curaudau	488·23
	Mean of the whole	489·63

Messrs. Thenard and Roard adopted the proportion of 26 per cent of sulphuric acid in sulphate of barytes, because it is the mean between the results of the analysis of this sulphate obtained by one of them, and those found by Mr. Berthollet after experiments made with the greatest care.

The determination of the proportion of sulphuric acid being the most important experiment, they attempted it a second time with as much precision as before, and found no difference between the quantities of sulphate of barytes obtained by the two analyses.

For the alumine.

Exp. 2. The equal quantities of sulphate of barytes obtained by Messrs. Thenard and Roard in the preceding trials leaving them no doubt with respect to the proportions of sulphuric acid in the alums they had examined, they did not think it necessary to analyse any but those of Rome, Bouvier, and Liege, for the purpose of acertaining the proportions of the other principles. These give us one artificial alum, and two native alums, of which one is the most common, the other the most esteemed. Of each of these 489 grammes well powdered were dissolved by heat in 16 quarts of water, and decomposed by equal quantities of ammonia, which was added in very great excess. The alumine precipitated was washed with 60 quarts of water; and when that of the last washing ceased to precipitate muriate of barytes, it was collected, and dried in a large silver basin. After being dried, and kept at a red heat for an hour, it weighed:

No.		grammes.
1,	Roman alum ···· ·····	60·92
2,	Alum of Bouvier········	61·82
3,	———— Liege··········	61·02

Thus Messrs. Thenard and Roard found in these alums exactly the same quantity of alumine; for the trifling differences observed between them do not amount to a gramme [15 ½ grains], and are such as could not be avoided in such a long series of operations.

The authors took so much care in washing the alumine, and not pouring off the water till the sediment was completely formed, and had left it perfectly clear, that they cannot fear having assigned the quantity too small. Neither can it be too great, since, when it was dissolved in nitric acid, the solution did not render muriate of barytes turbid: it was completely freed from any sulphate therefore, that might have increased its weight.

For the potash.

Exp. 3. The 60 quarts of lixivium produced by washing each of these alums were evaporated to dryness in a silver bowl, and the products obtained were boiled several hours with an equal weight of quicklime. The residuum was treated four times successively with boiling water, to take up completely every thing soluble; and these waters were evaporated to dryness, the residuum dissolved in a very small quantity of distilled water, and this repeated alternately several times, in order to separate completely the last portions of sulphate of lime. The solution of each of the sulphates of potash was evaporated for the third time, and at length heated red hot in a platina capsule.

The

The weights of the sulphate of potash thus obtained were:

No.		grammes.
1,	Roman alum	77·05
2,	Alum of Bouvier	76·80
3,	——— Liege	77·33

These sulphates no longer gave any sensible precipitate with oxalate of ammonia, and rendered nitrate of silver but very slightly turbid. They contained a little excess of alkali, but in so small a quantity, that a few grains of sulphuric acid were sufficient to saturate it.

Messrs. Thenard and Roard preferred treating the sulphates with lime to employing calcination, for they had satisfied themselves that by calcination acidulous sulphate of potash can only be obtained, part of the alkali always flying off.

Constituent principles of sulphate of potash.

The analysis of the sulphate of potash, repeated several times following, constantly afforded them the same results, and showed, that a hundred parts of this salt consist of

Sulphuric acid	36·4
Potash	63·6
	100

Analysed for ammonia, but none found.

Exp. 4. Desirous of knowing whether the alums they had analysed contained ammonia, they treated them with caustic potash, and with lime; and as they obtained none by this method, they heated them strongly in a retort with an equal weight of powdered quicklime; but they could not thus discover the slightest trace of it. In fact, say they, we should have been surprised, if we had found any, for we knew to a certainty, that it could not be one of the constituent parts of the artificial alums we examined; and as to the native alums of Liege and Rome, as no urine is added in their preparation, the ammonia must have existed in the ore, and from this it must have been expelled by the roasting.

May exist in some alum.

We must not however conceal, that it is possible to find alums with an ammoniacal base, though they must be very rare, for the practice of saturating the excess of acid in the aluminous lixivia by means of urine has been very confined, as it has generally been supposed, that this alkali would injure the beauty of dyes.

Analysed for iron.

Exp. 5. The presence of iron in alums had been demonstrated in a positive manner by several eminent chemists, who considered them all, including the Roman, as one and the same salt, except so far as its properties were altered by foreign matters, and particularly by sulphate of iron.

Mode of estimating its quantity.

To ascertain its influence, it was necessary to know the quantity contained in the alum: but analysis not affording any means of determining it with sufficient accuracy, Messrs. Thenard and Roard had recouse to the synthetical plan. Accordingly they took some alum perfectly free from iron, to the solution of which they added from $\frac{1}{100}$ to $\frac{1}{2000}$ part of sulphate of iron; and then they compared the effect of prussiate of potash on each of these solutions, more or less ferruginous, with that it produced in solutions of the five kinds of alum.

Proportions.

By this method they found, that the alum of Liege contained at most $\frac{1}{1000}$ of sulphate of iron, that of Javelle a little less, that of Bouvier and of Curaudau $\frac{1}{1000}$ or $\frac{1}{1700}$, and the Roman scarcely $\frac{1}{2000}$.

Component parts of alum.

From all these experiments it follows, that the alums of Rome, Bouvier, Liege, Javelle, and Curaudau contain precisely the same quantities of sulphuric acid, alumine, potash, and water, and differ only by a few thousandth parts of sulphate of iron: and that a hundred parts consist of

Sulphuric acid	26·04
Alumine	12·53
Potash	10·02
Water	51·41
	100

PART II.

Experiments with Dyes.

Experiments with dyes.

After having given the results of their analyses in the first patr, Messrs. Thenard and Roard proceed to the second, which includes all their experiments with dyes. As this does not

not appear to us capable of being abridged, we shall give it entire.

All the alums identical, except from the sulphate of iron.

Convinced, say they, by the preceding experiments, that the alums of which we have spoken are formed of the same quantities of sulphuric acid, alumine, potash, and water; and that they may be considered as identical, differing only by a thousandth part of sulphate of iron, we began with examining, whether their action in dyes were as different as is commonly asserted. Desirous that this part of our labours should not be inferior in precision to the former, we endeavoured to remove every cause of uncertainty that might occur either from the mixture of the colouring matters or the substance dyed, the variations produced by the time or vessels employed in the application of the mordant, the unequal body of liquor, or the difference of temperature in the baths of dye. As we were anxious to observe with the greatest care all the effects, that might present themselves in the course of our experiments, we performed the greater part of them ourselves, and all the rest were executed under our inspection in our own dyehouse.

We have not laid before the Institute the results of more than five hundred experiments with dyes that we have made, the greater part of which served only to point out our course, or confirm facts we had already observed: all those we have suppressed would have added nothing to the various proofs we set before them.

All our researches were made at the Gobelins: we could not choose a dyehouse more convenient, or offering us more advantages; for the processes there constantly carrying on, to supply the demands of three imperial manufactories, enabled us to make without interruption very numerous and varied experiments, which could not have been executed elsewhere without considerable expense. There we found every thing we wanted, whether of vessels, dyes, or matters to be dyed; and no where else could we have been assisted by a more able dyer than our foreman, Mr. Blondeau, who to great skill in colours adds very extensive practical knowledge.

ART. I. *Comparison of the effects obtained in dyeing with the alums of Rome, Bouvier, Liege, Javelle, and Curaudau.*

Comparative trials with wool, silk, linen, and cotton.

With printed cottons.

The materials we employed in operating with these five alums were wool, silk, thread, and cotton. Each of these we subjected to the preliminary preparations adopted in the most celebrated dyehouses. Aware of the extensive use of cotton for printed goods, and that Roman alum is employed exclusively for all their delicate colours, we were desirous of making some trials, that would enable us to decide upon this subject. We then had recourse to Mr. Davilliers, who readily, and with the greatest politeness, made a trial of our five alums in his manufactory. The patterns he was so obliging as to send us agreed very well with our results; but as the unequal application of the mordant might with some plausibility have been objected to us, we endeavoured to obviate this by adopting another method, that used in dyeing piece goods.

Mr. Berthollet, jun., who has already distinguished himself in the science and in its application to the arts, particularly with respect to printed calicoes, which he has studied with great care in the fine manufactory of Jouy, had the civility to come and direct us in this important part of our labour, and assist us in all the researches we made on this subject.

List of experiments.

Each of the experiments that compose this article was made with all the five alums.

Woollens.

Exp. 1.........Weld yellow.
2.........Cochineal.
3 & repeated. Madder.
4.........Kermes.
5.........Archil.

Thread.

6.........Weld yellow.

Cotton Thread.

7.........Weld yellow.
8.........Madder.

Exp. 9.........Sumach.

Calicoes.

10.........Weld yellow.
11.........Madder.
12.........Sumach.

Silks.

13. ....Weld.
14.........Crimson.

Silks with the acetates produced from the five alums.

15.........Weld.

By

No difference on wool, little on cotton, great on silk.

By these experiments we find, that the five alums act generally in the same manner on woollens, that they produce some difference in cotton, and that their effects differ greatly on silk. But these alums contain precisely the same proportions of the same principles, and differ only by $\frac{1}{1000}$ of sulphate of iron: we are therefore obliged to conclude, that the differences mentioned must be ascribed to this sulphate.

The following are the experiments we made to establish this fact.

ART. II. *Alums of Rome, Bouvier, Liege, Javelle, and Curaudau, in their common state, compared with the same alums purified.*

The differences owing to the sulphate of iron.

After having freed these five alums from all the iron that existed in them, we made comparative trials with them thus purified, and with Roman alum and the alums of French manufacture.

We first employed prussiate of potash to precipitate their iron; but as this method was slow and expensive, we substituted the more simple and well-known process of dissolving the alum in boiling water, and washing the pulveriform crystals in cold water. In this way we obtained the complete separation of all the sulphate of iron from our most impure alum, which then was no longer perceptibly affected by prussiate of potash, even after several days exposure to the air. So complete a purification however is altogether unnecessary for the purposes of the arts.

Table of experiments.

Wool.

Exp. 16. . . . . . . Weld yellow.
17. . . . . . . Cochineal.
18. . . . . . . Madder.
19. . . . . . . Kermes.

Thread.

20. . . . . . . Weld.

Cotton thread.

21. . . . . . . Weld.
22. . . . . . . Madder.
Exp. 23. . . . . . . Sumach.

Calicoes.

24. . . . . . . Weld.
25. . . . . . . Madder.
26. . . . . . . Sumach.

Silks.

27. . . . . . . Weld.
28. . . . . . . Cochineal.
29. . . . . . . Fustic.

With

General effects of the iron.

With weld and cochineal, which are colouring matters the most sensible to the action of sulphate of iron, the purified alums gave us colours more brilliant, fresh, and in a slight degree lighter; while those with our common alums were all duller, and evidently of a deeper hue. This slight increase of the intensity of the colour arises solely from the small quantity of sulphate of iron found in our common alums. To satisfy ourselves of this, we added to our purified Liege alum scarcely appreciable quantities of sulphate of iron, and gradually more and more; till, by thus restoring all it had lost in its purification, we caused it to assume the different states of Roman alum, that of Bouvier, Curaudau, and Javelle, and lastly its original state of Liege alum.

ART. III. *Comparison of the alums of Rome, Bouvier, Liege, Javelle, and Curaudau, in their ordinary state, with the same alums, to which we had added increasing proportions of sulphate of iron.*

Comparison of the alums with additions of iron.

We were convinced to demonstration, that the slight differences produced by these several alums in dyeing were owing to the different and scarcely calculable quantities of sulphate of iron they contained: but to remove completely every doubt, that might still be entertained in this respect, we confirmed by synthesis all the facts, that we had collected from analysis.

The substances to be died, woollen, linen, and cotton, were prepared with solutions of the alums purified, of the same alums with the addition of $\frac{1}{100}$, $\frac{1}{50}$, $\frac{1}{25}$, $\frac{1}{12}$, $\frac{1}{6}$, and $\frac{1}{3}$ of sulphate of iron, and of sulphate of iron alone.

The silks were alumed in the same proportions with the five different alums, and with pure alum to which from $\frac{1}{2000}$ to $\frac{1}{100}$ of sulphate of iron was added.

Table of experiments.

Wool.

Exp.30.	......Weld.	Exp.36.	......Madder.
31.		37.	
32.		38.	
33.	......Cochineal.	39.	......Kermes.
34.		40.	
35.		41.	
		42.	..Prussiate of potash.

Thread.

Exp. 43. …… Weld.

Cotton.

44. …… Weld.
45. …… Madder.
46. …… Sumach.

Calicoes.

47. …… Weld.
48. …… Madder.
49. …… Sumach.

Silks.

Exp. 50. …… Weld.
51. …… Cochineal.
52. …… Fustic.

Silks.

53. …… Weld.
54. …… Cochineal.
55. …… Fustic.
56. …… Weld.
57. …… Cochineal.

General effects.

From these experiments it appears, that weld yellows are greened and deadened by sulphate of iron. That cochineal is turned violet by it, without being altered so quickly as kermes, or even as madder: and that, without being made dull its colour is sufficiently heightened for persons not much used in comparing colours to prefer generally on wool those produced by Roman alum with $\frac{1}{25}$ of sulphate of iron to those of the pure Roman alum.

On cotton.

In the colours on cotton, whether sumach or weld yellow, or madder red, notwithstanding the slight differences from the drying of the mordant, experiments 44, 45, and 46, with purified Liege alum and $\frac{1}{100}$ of sulphate of iron, never afforded us deeper or duller colours than the same experiments with Roman alum and $\frac{1}{100}$ of sulphate of iron.

On silk.

Sulphate of iron acts in a more striking manner on silks, for the weld yellows and cochineal crimsons on them were more affected by $\frac{1}{100}$ of sulphate of iron, than on woollens by $\frac{1}{25}$.

The quantity of iron tested by silk.

Knowing the great sensibility of silk in manifesting the smallest quantities of iron, we employed it to satisfy ourselves whether our alums did not contain above $\frac{1}{1000}$ part, as we had found synthetically by pouring prussiate of potash into solutions of pure alum, afterward altered by greater or smaller quantities of sulphate of iron.

We alumed silks with alum freed from iron, Roman alum, the alums of Bouvier, Liege, Javelle, and Curaudau, and similar quantities of pure alum, to which we had added from $\frac{1}{1000}$ to $\frac{1}{100}$ of sulphate of iron.

After

After they were dyed, we found in the series of tints experiments 56 and 57, produced by our pure alum rendered more or less ferruginous, colours perfectly similar to those of our ordinary alums. Thus $\frac{1}{2000}$ part of sulphate of iron added to this pure alum, afforded us with weld and cochineal the same colours as Roman alum; $\frac{1}{1000}$ the same as the alums of Bouvier and Curaudau; $\frac{1}{1200}$ the same as that of Javelle; and $\frac{1}{1000}$ the same as that of Liege.

The different effects of alum owing to iron.

We can no longer therefore ascribe the differences we obtained in dyeing with different alums to any other cause, than these infinitely small quantities of sulphate of iron; since, by adding this substance, we converted purified and Roman alum into alums, which gave us the same results with reagents as the most impure kinds of the shops; and, on the contrary, by abstracting the sulphate of iron, we could make at pleasure, from the most impure kinds, alums producing as fine or finer colours than those obtained with Roman alum.

ART. IV. *Experiments on the influence of sulphate of ammonia, and of alum with an ammoniacal base.*

Experiments with sulphate of ammonia.

Many distinguished chemists have asserted on the authority of Bergman, that alums with an ammoniacal base are injurious in dyeing. To ascertain whether this opinion were well founded, we treated wool and silk with several proportions of sulphate of ammonia, which we added to Roman alum, and with alum without potash, having its base entirely of alumine and ammonia.

It produced no effect,

$\frac{1}{100}$ and $\frac{1}{50}$ of sulphate of ammonia produced no perceptible change in silk or wool with weld or cochineal colours.

unless in considerable quantity.

$\frac{1}{25}$, $\frac{1}{12}$, $\frac{1}{6}$, and $\frac{1}{3}$ of this salt produced a regular degradation, in which the colour with Roman alum and $\frac{1}{3}$ its weight of sulphate of ammonia was two or three shades weaker than that with Roman alum alone.

Common alums not injured by this.

Hence we had reason to expect evident changes from alum with an ammoniacal base, but we found no difference in its effect and that of Roman alum.

Wool

Wool with sulphate of ammonia.	*Silk with alum having an ammoniacal base.*
Exp. 58······Weld.	
59······Cochineal.	Exp. 60·········Weld.

General deductions.

Such are the facts we were desirous of laying before the Institute, relative to the long undecided question of the superiority of Roman over all other alums. They afford us an exact and complete coincidence between the results of our analyses and of our experiments with dyes: they show us, that much too extensive an action has been ascribed to the sulphate of iron, the whole of the influence of which we have pointed out, at the same time marking its limits: and in particular they prove, that the opinion of the exclusive advantages of Roman alum, formerly perhaps sufficiently just, is now to be considered as an errour successfully combated by theory, and demonstrated by experiment. These facts lead us directly to the following consequences.

Alum itself identical.

1. All alums contain precisely the same proportions of sulphuric acid, alumine, potash, and water; though they produce sensible differences with reagents, and in their application to the art of dyeing.

Differs from being contaminated with iron.

2. These differences arise solely from the unequal quantities of sulphate of iron found in them, amounting merely to a few thousandth parts, for they disappear completely on the purification of the alums, and are reproduced with the same intensity, if we restore to them as much sulphate of iron as had been abstracted.

Roman alum freest from it.

3. The Roman alum contains the least sulphate of iron: the alums of Bouvier and Curaudau afford us a little more, but the quantity they contain is appreciable only by reagents, and on silk in weld and cochineal colours. In the alums of Javelle and Liege prussiate of potash immediately indicates the presence of sulphate of iron.

Other alum may be made equal to it.

4. Roman alum does not merit the exclusive preference given it over other alums, for we have obtained on wool, cotton, and silk, with Liege alum purified by means of water, and even with the alums of Bouvier and Curaudau, as fine and brilliant colours as those produced by Roman alum: and if the latter appeared to us to have the advantage over

the alums of Bouvier and Curaudau, we can affirm, that the differences were very trifling, and only to be perceived by an experienced eye.

$\frac{1}{1000}$ part of sulphate of iron injurious.

5. The alum of Javelle, and that of Liege in particular, though not containing above a thousandth part of sulphate of iron, almost always produce duller and less bright colours than those with purified or pure alums.

Its effect greatest on silk,

least on wool.

6. The effect of sulphate of iron is not the same on all substances, and with all colouring matters: it is very evident on silk with weld and cochineal colours: it is a little less so on cotton, and it is much less on wool, with the same substances. Wool appears to fix a less quantity of sulphate of iron than cotton, and particularly than silk; for the colours on wool are less altered by $\frac{1}{25}$ of this sulphate than on silk by $\frac{1}{100}$; and in all madder, archil, and kermes colours, very large proportions of this substance are necessary to alter the shade, or even to diminish its liveliness.

Little with madder, archil, or kermes.

Every manufacturer may make alum of the best quality.

7. Every manufacturer of alum therefore, if he will, may change his most impure alum, by simple and not expensive means, into an alum, that in its application in all the arts, to the most lively colours, and to substances the most sensible to the influence of sulphate of iron, shall possess all the properties of the long boasted Roman alum.

Remarks on the French trade in it.

Let us hope, that the importation of foreign alum into France, which amounted a few years ago to several millions of livres, and which has already decreased in a considerable degree, will soon cease entirely: that our alum manufacturers, better acquainted with their real interest, will no longer endeavour to distinguish their goods by that coloured coating, which has most frequently been the resource of fraud: that their endeavours to furnish the shops with an alum constantly pure will soon lead all our manufacturers to think no more of Roman alum: and that ultimately our alum works, obtaining deserved celebrity, will be greatly increased, extend their sale to foreign countries, and enrich France with a considerable branch of trade.

IX.

IX.

Essays on the Improvement of Pottery in general, or the Art of making, at the least Expense, Vessels for every Use, more handsome, strong, and wholesome, without employing Lead or Tin, in the Composition of the Coating, Enamel, or Glaze: by Mr. C. R. JOUSSELIN, *Manufacturer at Nevers. An Abstract by Mr.* GUYTON *.

History of the art.

IN the pamphlet that has just appeared under this title, the author, after noticing the importance of this branch of industry, takes a rapid view of the periods when the porcelains of Japan and China arrived in Europe, the introduction of Delft ware into France, which dates no farther back than the 15th century, the extensions of the art which have long rendered it an advantageous branch of foreign trade, the importation of the white English ware, and manufactories established to imitate it.

No good pottery but stoneware and porcelain.

He next lays it down as an established principle, at least among men of science or those acquainted with the art, that there is no truly good pottery but stoneware and porcelain: as they are the only kinds, in which strength, neatness, and wholesomeness are combined.

Importance of the art to France.

Before proceeding to the proofs of this, Mr. Jousselin justly observes, that this principle is not only interesting to the progress of the art; but that it merits the greatest attention in a political and commercial view, particularly as the neglecting it must render the French tributary to foreign nations, and this to no small amount, were it merely for the lead and tin employed in the composition of the coatings.

Common pottery for culinary uses.

Common pottery, intended to stand the fire, has a very porous biscuit, which is but slightly baked, that it may be capable of sustaining the transition from heat to cold, and because it is not refractory enough to support a greater heat. For the same reason it can only have a very fusible covering.

Its glaze.

This is commonly sulphuret of lead, and oxides of copper, iron, and manganese.

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* Annales de Chimie, Vol. LXII, p. 213, Mai, 1807.

Delft.

Delft ware, which was a grand invention in its time, on account of the beauty of its coating, has likewise the defect of being baked only so far as to vitrify the enamel, as a degree of heat beyond this would spoil it. This renders it necessary, to employ a sufficient quantity of lime in it, to give it a little consistency by a commencement of fusion.

Its defects.

Its coating, composed of glass of lead and silex, rendered white and opake by oxide of tin, cannot support changes from heat to cold, and its biscuit is liable to imbibe grease.

Queen's ware.

The white or *pipe* ware, after the English fashion, is lighter; its biscuit has more solidity, being composed of purer clay, and prepared flints; and it is previously baked: but the coating given it is much more fusible than that of delft; it is a glass, incapable of enduring an equal heat; is subject to crack; is very easily scratched, when any oily matter will penetrate the biscuit and leave spots; and if the glass of lead be in excess, which is unfortunately a too common case, oils and vegetable acids attack it, and render its use dangerous.

Its glaze defective and dangerous.

The memoir published by an able chemist, Mr. Proust, to remove any apprehension of injury from its use, induced Mr. Gay-Lussac and myself, to pay great attention to this subject, at the time of the last exhibition of the products of French Industry. We found very little, that was capable of completely resisting the edge of a knife; and after this it could not stand the test even of boiling acetic acid, or the yolk of egg boiled hard. We cannot therefore avoid adopting the opinion of Mr. Jousselin, that, whatever attempts be made to improve this manufacture, it can never form good pottery.

Is stone-ware capable of being made a substitute for every thing but porcelain?

Hence then it may be admitted as a general principle, that only two kinds are admissible, stone-ware and porcelain. But is it possible, to answer every purpose of strength, elegance, wholesomeness, and economy, in a word, to render stone-ware capable of supplying the place of common earthen-ware, or such as is required to stand the fire, of delft, and of *pipe-ware?* Mr. Jousselin affirms, that he is convinced it is by numerous experiments. As he is establishing a manufactory, it is natural for him to keep secret

Yes.

the processes he has discovered, though what he says appears sufficient, to give him a claim to our confidence.

Without attempting to divine his secret, I shall add in support of its possibility, that it might reasonably have been questioned, when the art was but a traditional practice, and when all our stone-ware was of a close texture, incapable of supporting the fire without cracking: it might have been questioned before the experiments of Lauraguais in 1762, whence Mr. Jousselin dates the first conception of a common porcelain, and whose successes did not meet the encouragement they deserved: before the property of magnesia to put a stop to the fusion without imparting any colouring principle, and that of barytes to supply the place of saline fluxes, were known: before the analyses of feldtspar had taught us to compose it artificially with very common materials: before the property of pumice stone to afford a covering not attacked by any menstruum was discovered; and before the inventor of this process, Fourmi, crowned by the Institute in the year 12, had fabricated his *hygiocérames*, a species of common porcelain capable of standing the fire: before the effects of heat prolonged to devitrification had been observed: and before the productions of the manufactories of Utschneider, Lambert, and Mittenhof, had been seen, which the jury of the exhibition of 1806 recognized as a true stone-ware capable of standing the fire, that is to say common porcelain*.

Stone-ware may be made to stand the fire.

Lauraguais.

Use of magnesia and barytes.

Artificial feldtspar.

Pumice stone as a glaze.

Hygiocérames.

Devitrification by heat.

Thus

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* I have here pointed out only the principal facts. I might quote many others, that tend not less powerfully to confirm the opinion For instance, the spuma maris, the keffekil of Kirwan, to which the name of magnesite has been given, and of which the Turks make their pipes, contains according to Klaproth but 0·50 silex, and 0·17 magnesia I have found, that it loses 0·23 of its weight in the fire. It has the property of stopping both the vitrification and the contraction of the compositions into which it enters.

Keffekil, its use.

Mr. Giobert has shown, in the Memoirs of the Academy of Turin for 1802, that the earth of Baudissero, long considered as almost pure alumine, and used with success in the porcelain manufactory of Vinovo, is a magnesian earth, containing about 0·14 of silex

Magnesian earth.

Among the results of the synthetical essays made in my laboratory at the Imperial Polytechnic School, I obtained a glass perfectly similar to that

Glass from ar-

Every necessary kind of pottery may be made without any metal.

Thus in the present state of our knowledge it is far from impossible, that an artist perfectly acquainted with it, and improved by practice, should succeed in fabricating, as Mr. Jousselin professes, three kinds of pottery, to supply the place, 1st, of close grained stone-ware, for containing liquids and other matters, with or without glazing: 2dly, of less close grained stone-ware, with a brown glaze externally, and a white enamel internally, for culinary utensils: and 3dly, of delfts and white earthen-wares, retaining both elegance of form and lustre of glazing, without employing any metal.

Very cheap glaze of earths alone.

The enamel of which Mr. Jousselin announces the discovery is entirely earthy, and composed of materials so cheap, that the enamel, which now costs the manufacturer at the rate of 320 franks for a certain quantity of ware, will come to no more than 15 or 20.

X.

Process for proving the Quality of a Glaze of Earthen-Ware.*

Defects of glaze.

THE glaze of earthen-ware may have several defects: it may be scratched more or less readily by a hard body; weak acids, such as vinegar, lemon-juice, verjuice, &c., may attack and dissolve the lead it contains; or oily substances standing long on it may produce the same effect, stain it, and render it dull.

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tificial feldtspar.

that afforded me by the feldtspar of Baveno, by urging to fusion in a platina crucible a mixture of 62 parts silex, 16 alumine, 10 lime, and 12 potash.

Porcelain without kaolin.

I have likewise made, without kaolin, a biscuit having the hardness, semitransparency, and grain of porcelain, by giving the proper degree of baking to a paste composed of 50 parts silex, 20 alumine, 24 magnesia, and 6 lime I need not say, that it would be very easy to employ the same proportions of silex and alumine, by choosing a good clay, without being obliged to have recourse to the decomposition of alum for the earth.

* Sonnini's Bibliothèque Physico-économique, July, 1807, p. 43.

To

To determine its power of resisting friction, it may be rubbed with sand; and if this scratch it more readily than it does a glaze known to be good, we may be assured it is soft. If soft, sand will scratch it.

If vinegar be boiled for some hours in a vessel coated with a soft glaze, it will attack the glaze, and dissolve a portion of its lead, which will be precipitated from the vinegar on the addition of a few drops of sulphuric acid, commonly called oil of vitriol. Vinegar will dissolve its lead.

But a method more within every one's reach, and therefore deserving to be known, is, to let fall a drop of strong ink on a piece of earthen-ware, dry it before the fire, and then wash it. If the glaze be too soft, the ink will leave on it a slight spot. Ink a ready test.

XI.

Heights of various Places in France, &c.; by Dr. Berger.

Continued from p. 217.

Sect. II.

Heights ascertained during a tour in the ci-devant Province of Auvergne.

THE following observations were collected in a tour made in the spring of 1802, in company with Mr. Leopold von Buch, a celebrated Prussian mineralogist, and Mr. A. Jurine. With these gentlemen I set out from Geneva to visit the chain of the mountains Dôme and d'Or, traversing the ci-devant provinces of Bugey, Bresse, Lyonnois, and Forez, and returning through Dauphiny. As all this country, particularly the most interesting, which is for the greater part included in the circle forming the department of Puy-de-Dôme, has been carefully examined by several able mineralogists, I shall say little respecting its physical constitution. Most of the heights were calculated by Mr. von Buch from the simple formula of the difference of the logarithms of the numbers expressing the heights of the barometers at the Tour in Auvergne.

the two stations*: neither were they made with such strict attention to accuracy, as to be considered as absolutely determined.

It would be difficult to add any thing to what Mr. von Saussure says of the road from Geneva to Lyons in his Tours to the Alps: I shall only enlarge a little more than he has done on some places where pebbles or blocks of primitive rocks occur. Following more or less closely the course of the Rhone from Geneva, we meet with some at the villages of Confignon and petite Grave, where they rest on a bed of soft gritstone; at Chancy, where we found on the banks of the Rhone a granite with reddish feldtspar; and in the bed of the London, a small river that comes from mount Jura, and falls into the Rhone, where there are several pebbles of serpentine, including tolerably large garnets. But in a marshy bottom situate below the village of Pougny a large quantity of primitive compound rocks are seen, some with a base of diallage [smaragdite of Saussure] and jade, others of almost pure jade, or compact petrosilex. Not far from the loss of the Rhone, near the village of Vanchy, primitive pebbles are still perceptible; afterward toward Chatillon they become more rare; yet I have seen blocks of gneiss on this road, about a mile from the little lake of Syant or Sylant, which no doubt formerly made but one with that of Nantua, about 120 yards below it. Every thing leads us to believe, as Mr. Saussure remarks, that the latter extended much farther to the south-west, covering the large flat meadows observed in that quarter, the soil of which is composed of rounded pebbles for the most part calcareous.

From Geneva along the course of the Rhone.

Lakes Syant and Nantua.

Extremity of the Jura.

From this place to the extremity of the Jura, between Poncin and Pont-d'Ain, scarcely any primitive pebbles occur. There we begin to meet with pebbles of quartz in considerable quantity, and some blocks of gneiss in the en-

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* "Though the correction for temperature, with respect to the dilation of the air, is indispensable in measuring differences of level in the same country and at the same time; it is not quite certain, that it ought to be employed when we compare countries very distant from each other, and take the mean of a great number of observations." J. B. Biot's Physical Astronomy, vol. 1, p. 145.

virons

virons of Priay; whence the road to Lyons is over plains covered with pebbles, frequently in such quantity as to prevent the land from being cultivated. The prevailing species are quartz, and hard quartzose gritstone. The pebbles of the Alps indeed frequently occur, as micaceous schist, schistose hornblende, and serpentines: yet when we traverse the bed of any torrent or river coming from the adjacent mountains, the calcareous stones always predominate.

Road to Lyons.

Between lake Sylant and Chatillon, about two or three miles from this town, on the left bank of the little river is a tolerably fine spring, called Entrebilliet, the temperature of which on the 1st of April was 7·5° of Deluc [49° F.], while that of the open air was 6·5° [46·7°]. The height of the place above the sea, as found by the barometer, was 241 toises. At Varambon, near Pont-d'Ain, a spring rose out of the ground, the temperature of which was 9° [52·25°], and that of the open air 12° [59°]. The height of this place was about 140 toises. These two observations agree sufficiently with the law established empirically by Mr. Saussure, that the heat of the air decreases about 1° of Deluc for every hundred toises in height.

Heat of springs,

agreeing with Saussure's law.

From Chatillon to Nantua we found a prodigious quantity of box. All the country, except the summits of the mountains, which are crowned with firs, is covered with this shrub; and from the warmth of its local aspect, it grows to a considerable size, as in Campania and the East.

Box.

From the environs of the loss of the Rhone we do not meet with any vines in the road, till we reach Cerdon*. At this place is a plantation, reaching from the top of the mountain, which is 403 feet higher than the lake of Geneva, to the bottom of the declivity on the high road, which is 192 feet lower than the lake.

Vines.

If in proceeding from Lyons into Auvergne we travel directly westward, traversing the Lyonnois and Forez, we are constantly on the primitive soil. The chief base of the country of Limagne, that fine part of France, is well known

Granitic country.

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* The extent to which Mr. Arthur Young has availed himself of his accurate observations on the locality of certain cultivated plants, among which is the vine, is well known. I do not hesitate to pursue his views of vegetable physics, when opportunity offers.

The Cevennes.

to be granitic: the same kind of soil too, that we meet with in going down the Rhone from Lyons by the way of Vienne and Tournon, occurs when we proceed to Beaujolois by way of Tarare, Thizi, and la Claire: thus from the considerable extent occupied by this kind of soil in these countries the centre of all these chains cannot be far distant, and it is not without reason, that Mr. Delametherie places it in the Cevennes, which he considers as one of the principal centres of the primitive mountains of France.

Mountain of St. Bonnet.

Coal.

Chesnuts.

The rock that composes the mountain of St. Bonnet-le-Froid is an undulated reddish gneiss, evidently stratified, and intersected by strata or veins of other rocks, particularly white quartz, and schistose hornblende, which assumes a porphyritic appearance on the back of the mountain to the west, particularly between the villages of Coursieux and Ste Foy-l'Argentière. Throughout the whole district of the latter, situate in a valley watered by the little river Brévenne, pitcoal is found. Among the fruit-trees surrounding the houses a few chesnuts are to be seen, but all of them poor and low: probably this tree will not thrive without shelter.

Vines and walnuts congenial.

On the back of the mountain of St. Bonnet, toward Coursieux, there are some fine plantations of vines; and in the bottom of the valley very fine walnut trees. This is not the only place where I have observed the vine and the walnut thriving together; probably therefore they require nearly a similar temperature.

Holly

tall

and smooth leaved at top.

The holly is common among the firs in the mountains of the Lyonnois, and sometimes appears as a tree ten or eleven feet high. In these cases the upper leaves are smooth, while the lower ones are prickly as usual.

Lake diminished to ponds.

Basaltic mountain.

Wood sorrel.

The plain of Forez is covered with a multitude of ponds, and no doubt formerly was one extensive lake. In it there is a basaltic mountain. The level of this plain is 185 feet below that of Limagne. Its soil is evidently formed by the decomposition of the primitive rocks; but it appears perfectly adapted to wheat. One weed only infests all the fields, the wood sorrel; but this is in great abundance, particularly on the fallows: however it is beneficial to the sheep folded on them, for they prefer it to any other plant, whence Tabernæmontanus called it oxalis ovina.

Sheep fond of it.

In the neighbourhood of Feurs I saw furze for the first time on my road, a plant not to be met with in any part of Switzerland.

Furze.

Table of heights above the sea in toises and thousandth parts.

Table of heights above the level of the sea.

Places.	Without cor. for temperature.	According to Deluc.	According to Trembley.	As given by Deluc.
Chataillon de Michaille		286·068	287·945	264
Lake Syant ··········		302·582	304·843	
Cerdon ··············		191·820	193·874	158
Meximeux············		131·080	132·634	118
Lyons················	89·680			88
			By Saussure	84 or 85
Petit-St.-Jean ········	121·500			
The highest part of the road over St. Bonnet-le-Froid ············	390·500			
Coursieux ············	188·000			
Ste Foy-l'Argentière ··	229·500			
St. Martin de l'Estra ··	297·000			
Feurs················	173·000			
St. Germain-le-Val ····	210·000			
St. Just-en-Chevalet ··	352·636			
The highest point of the chain of Thiers, taken on the road, near Arconsat··············		481·601	488·096	
Boen ················	200·000			
Noire-Table ··········	344·500			
Thiers, at the lower part of the town ········	192·000			
Clermont ············	200·000			
Summit of the Pradelle, an ancient basaltic promontory, resting on granite, but separated from it by a thin stratum of bolar earth····	352·500			

Table of heights above the level of the sea.

	Without cor. for temperature.
Orgines*	393·000
The summit of Puy-de-Dôme†	751·657
Puy-de-Parion, on the edge of the crater	610·833
———— at the bottom of the crater‡	574·166
Puy-de-Barme	561·166
Orcival	448·000
Mount Jughat, on the edge of the crater	574·166
———— at the bottom of the crater	552 500
Issue of the current of lava from Puy-de-la-Vache	509·000
Lake Aidat	419·000
Mountains of Croix-Morand	693·166
Village of Mont-d'Or-les-Bains	523·333
Cascade of la Dogne, at bottom	654·000
———— at the top§	694·333
Rock of the Cousins‖	885·333
Mountain of Cacadogne‖	902·666
	Summit

Lava.

* Here is found the grand stream of lava from Puy de-Pariou. This lava is very fragile, and contains only small crystals of feldtspar, which retain their *native lustre.*

Porphyry.

† This mountain is formed of a sort of porphyry, the cement of which, not very hard, and of an earthy gray colour, includes mica and a great many large crystals of feldtspar, which are cracked and have a vitreous appearance; while those found in the granite, that constitutes the base of these mountains, have a pearly gloss.

According to Perrier the height of this mountain above the sea is 700 toises; according to Cassini and le Monnier, 757.

Crater.

‡ This crater, which is perfectly circular, measures 200 paces round: it is covered with grass, and cattle feed in it.

Volcanic productions.

§ In this excursion fine feldtspars are found, in large double crystals, in a porphyry, which according to Mr. von Buch cannot have a very different origin from that of Puy-de-Dôme. He adds, that it is a volcanic production, but not lava.

‖ If we proceed along the ridge of the mountain from the top of the cascade of la Dogne, says Mr. von Buch, we arrive at the rock of the Cousins, where we see substances that have much more appearance of basaltes, and in which the feldtspar becomes more rare. Continuing along the ridge toward Cacadogne, we go round a frightful, semicircular precipice, the sides of which are covered with scoriæ. This is the only place in the neighbourhood, that can be termed a crater. From Cacadogne the ascent to the summit of Mont-d'Or is easy. It is an immense circus,

Basaltes.

Crater.

Table of heights above the level of the sea.

	Without cor. for temperature.
Summit of Mont d'Or*	958·500
Circus at the foot of Mont d'Or, before the junction of the waters of the d'Or and the Dogne	611·000
Summit of Mount Capucin†	709·500
La-Tour-d'Auvergne‡	472·833
Murat-le-Quayre, at the château	539·000
Rocks on the Dordogne, about half a mile lower§	417·666
St. Laurent-des-Mûres	117·000
Bourgoin	173·200
La-Tour-du-Pin	158·200
Pont-de-Beauvoisin	118·000
The lake of Epin or Aiguibellette	193·334
Mount de l'Epin	463·666

Sect. III.

Brief description of several mountains in the department of the Leman lake.

Mountains near the lake of Geneva.

Salève.

Mount *Salève*, four miles east of Geneva, is narrow, but of considerable length from N. N. E. to S. S. W. On the W. N. W. it exhibits naked and steep rocks, in nearly hori-

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circus, truly alpine, terminated on one hand by the sides of the valley of la Cour, and on the other by the rock of the Cousins. These answer to each other, and formerly closed the circus on the side of the valley of Bains. But the whole of this vast circus cannot be a crater. I conceive, continues Mr. von Buch, that there are two, the valley of la Cour, and the funnelshaped hollow between Cacadogne and the rock of the Cousins; the remainder of the cavity was formed by the falling in of the parts between these two craters, as is shown by the bare and salient angles below the summit of Mont d'Or, and the valley of Enfer.

Formerly two.

* According to Cassini the height of this mountain above the sea is 1048 toises: and he afterward calculated by the barometer its height above the village of les Bains to be 512 toises, while according to our observations it is only 435.

† Cassini makes this 224 toises higher than les Bains, and 760 above the level of the sea.

‡ A causeway of basaltes in prisms of six feet diameter, with decisive appearances of having been originally a stream of lava, is seen here. *Von Buch.*

Basaltes evidently from a volcano.

§ In this place is a grand colonnade of basaltes resembling the pipes of an organ.

Colonnade of it.

zontal

zontal strata. On the E. N. E., toward the valley of Bornes, or the Alps, which lie beyond it, the strata decline with a gentle and almost uniform slope. On this side we find strata of soft sandstone imposed on calcareous strata inclining under an angle of 45°. Similar strata are found on the little Salève, sloping in the same manner to the east. The sandstone strata extend to some distance from the foot of Salève, joining underground those of the hill of Essery, and still retaining the same direction. The brook Viézou has hollowed itself out a very deep bed in this soft standstone. The Arve too has made its way across it: and in the little Salève we see beneath it strata of calcareous breccia, covering those of compact limestone that form the body of the mountain. The sides of the mountain toward the village of Croisette are woody, and on the top the vegetable mould covers a considerable bed of white sand. In the interior of the calcareous strata are petrified marine bodies in great variety, some indications of coal, several nuclei of silex or petrosilex of a naturally round form, iron in the state of earthy oxide, &c.

Voirons.

Mount *Voirons*, three miles farther from the Leman lake than Salève, would be in some measure parallel with it, if it did not incline more to the south. Its summit forms a long ridge. On the side next the lake its slope is gentle for about two thirds of its height, where there is a small plain; and thence it becomes very steep, and is covered with firs. It is composed chiefly of a calcareous gritstone, the nature of which however varies greatly; for the southern part of its ridge is a kind of primitive puddingstone, in which I have found nuclei of a fine granite with red feldtspar and black mica, the most rare, as Mr. Deluc observes, in that part of the Alps which approaches Geneva. Its declivity toward the Alps is much more steep than that of Salève. Near the southern extremity, about half way up the mountain, is a large limestone quarry, the strata of which are nearly perpendicular to the horizon, lie east and west, and include several species of marine petrifactions. Among others Mr. Deluc found two large bufonites, which he considers as new.

The

Vale of Abundance.

The valley *d'Abondance*, rich in fine pastures, ascends with an imperceptible slope to a defile, in which is placed a cross, marking the limits between France and the Valais. Thence the descent to the village of Monteiche, not far from the Rhone, is pretty quick. All the mountains on this road are calcareous, generally steep toward the lake, and in several places their strata are nearly vertical. Above the cottages of Bize, on the chain skirting this valley, good coal has been found, and is wrought with advantage. The mountains here are higher and steeper than those nearer Geneva, because they are nearer the centre of the Alps, and probably too, as Mr. Saussure observes, because some of the lower steps of the grand amphitheatre of the Alps are wanting here.

Coal.

Môle.

The *Môle* is a long mountain, lying W. N. W. and E. S. E. It is composed of compact limestone, which in some places begins to assume the appearance of a schist by its disposition to split in leaves. There is some irregularity in the situation of these rocks, yet they follow the law common to all the exterior mountains of the chain of the Alps, their slopes being on the inner side, and their precipices on the outer. Here Mr. Saussure observes for the first time, that the secondary mountains are so much the more irregular and inclining in proportion as they approach the primary. It is from the Môle too we perceive very distinctly, that the Alps, to which all the surrounding mountains are attached, are composed of a great number of chains, nearly parallel, and separated by valleys following the same direction, which in general, with some slight exceptions, is north-east and south-west. On the Môle we meet with coarse calcareous brecciæ, imperfect vestiges of petrifactions, and frequently nuclei or even veins of petrosilex included in limestone.

Secondary mountains most irregular near the primary.

General view of the Alps.

The valley of Taninge.

The valley of *Taninge*, which opens into the great vale of the Arve near Bonneville, runs very evidently east and west. It is watered by the Gifre, a pretty considerable river or torrent produced by the melting of the ice on Buet and the mountains near that Glacier. Several of the calcareous mountains in the course of this river include argentiferous lead-ores. The extremity of the valley is closed by mountains covered with eternal ice. The strata of the mountains that

Lead ores containing silver.

Curved strata under horizontal.

that surround the valley from Samoin are singularly distorted in several places. Among others I observed at the bottom of the mountain called Pointe de Sale strata bent concentrically on one another, so as to form a large ellipsis, and over these horizontal strata. This is one instance of many confirming Mr. Saussure's remark of the irregularity of the strata of secondary mountains as they approach the primary. I did not find a single block of primitive stone in this valley. At Samoin, the aspect of which is certainly the hottest in the valley, I observed some *cretins*. On the left of the road from Taninge, about an hour's journey before we reach Samoin, is a chalybeate spring.

Goitres.

Chalybeate spring.

Brison or Brézon.

Mount *Brison*, called *Brézon* by naturalists and geographers, forms with the Môle the entrance of the vale of Arve. It is of compact limestone, and W. S. W. from the Môle. Its summit toward this mountain is perpendicular to a great depth, while its strata slope toward the Alps, though very steep at the top. Its foot is covered with large strata, nearly perpendicular, resting against the body of the mountain. In it there is a natural icehouse, containing ice all the year round, though it is of little depth. On the 25th of July, 1800, the temperature of this cavern was 0° [32° F.] while that of the open air was 9·5° [54·5°].

Natural icehouse.

Vergi.

The name of *Vergi* is given to a chain of loftier calcareous mountains behind this. It declines to the north-east, and rises to the south-west, without any remarkable and distinct summit. On its back are two lakes; that of Saxonnex, or Béni, to the north; and Lessy, which is larger but not so deep, to the south. This has no apparent outlet; but between the little town of Entremont and that of petit Bornand there is a pretty considerable cascade, spouting forcibly from a circular aperture in the middle of a rock fifty or sixty yards below the summit of the mountain, and fed by the lake. The limestone that forms this chain is of a dirty gray, of a foliated texture like the schists, and in general rising against the Alps at an angle of 31° or 33°. This is not uniform however, for to the north-east above Scionzier the strata are arched, or bent double, and of a compact instead of a slaty texture. All this chain, which some geographers still call *les montagnes maudites*, is very rich in plants,

Lakes Saxonnex and Lessy.

Arched strata.

Alpine poppy.

plants, some of them rare. On the ground accumulated at the foot of the Encrenaz, above lake Saxonnex, grows the alpine poppy, remarkable for its beautiful milk white petals, and its agreeable smell of vanello, particularly when the flower first opens.

Vale of le Reposoir.

Mount Meiri.

On the east of this chain is the valley of *le Reposoir*, which on the opposite side is bounded by another calcareous chain of very lofty mountains. A convent of Carthusians here is an excellent station for a naturalist, who would wish to explore the neighbouring mountains, which are in many respects interesting. Having ascended mount Meiri to Point-de-Château, one of the loftiest summits, the eye takes in the whole chain of the Alps, and looks down upon the vale of the Arve beneath. The highest point however is a little farther west, and is called the mountain du Four, or de Pierre percée, because its ridge is perforated. This mountain is seen from every part of the Alps, and at a distance appears inaccessible, though it is not so. Had it been later in the year I would have ascended it, but I was there in July.

Lake Chède.

Fragments of a fallen mountain.

Cretins.

The road from St. Martin to Servoz passes at the foot of a chain of mountains, the base of which is composed of slate, or a brown calcareous stone in thin leaves, intersected by veins of calcareous spar or quartz. You then ascend to the pleasing lake Chède; soon reach the fragments of a mountain, the summit of which fell down in 1751; cross le Nant-Noir; and, having passed a forest, the soil of which is a yellowish tufa, reach Servoz, where we meet with a few persons afflicted with the bronchocele, owing probably to its southern aspect, and being sheltered from the north winds.

Peak of Varens.

Petrifactions.

Ascending toward the point called l'Aiguille de Varens, we meet with petrifactions in a transition limestone about 1200 toises above the level of the sea. It has no impressions of vegetables, but a large species of turbinite [*vis*], and a bivalve of which frequently nothing but the edges remain. The real summit of the mountain is higher than this peak, but cannot be seen from the bridge of Sallenches, being behind the peak, which, as well as many others, is separated from the mountain by a kind of circus of considerable depth. On the summit of Varens is an extensive bed of numismal stones, and some hundred toises below, among the matters that have fallen from the mountain are many remarkably large cornua ammonis.

Buet. Different roads up it.

Vale of Villy.

Grit or puddingstone generally between the primary and secondary strata.

Mount Buet may be ascended not only from the valley of Sixt, taken by its first visitors, Messrs. Deluc; or that of Bérard, the way chosen by Mr. Bourrit, and described by Mr. Saussure; but by crossing mount Bréven from the priory of Chamouni, or from Servoz, by going up the valley of Villy. This valley is watered by a rivulet, which forms the precise boundary between the primary and secondary rocks. Another observation to be made in this road is, that the mountain above Servoz, which terminates the chain of Aiguilles rouges to the south-west is in great part formed of a primitive micaceous gritstone; there being only the summit called Aiguilette, that is composed of a foliated rock like that of mount Bréven. In like manner at the north-east of the chain the pass of Charlenton is formed of a primitive gritstone, remarkable for its well defined small quartz crystals; and is the intermediate point between the primary rocks of the chain of Aiguilles rouges and the transition limestone of the summit of Buet. This is another instance in support of Mr. Saussure's remark, that beds of grit or puddingstone are almost always found between the last secondary and first primary strata.

Valley of Chamouni.

Contains some secondary strata.

A calcareous hill.

The valley of Chamouni runs south-west and north-east, parallel to the chain of the Alps, and is bordered by mountains of the primary class. The col de Balme however, that bounds it on the north-east, and mount Lacha, its termination at the south-west, are composed of slate or calcareous stones. This valley contains other secondary rocks likewise, as some fine white gypsum, about three miles south-west of the priory, on the borders of the *nant* or torrent of Taconay; some limestone opposite the priory, at a place called Biolay; and the isolated hill of Piget, in the bottom of the valley, and stretching in the same direction, which is entirely formed of limestone. These excepted, every thing in the valley of Chamouni is of the primitive class.

Bréven.

Exactly above the priory, on the north-west of the valley, is mount Bréven, the base of which is connected with the Aiguilles rouges. Its summit is isolated, and its strata are cut off perpendicularly on this side, but slope gently on the other

other toward the valley of Villy, which is parallel with this. Its ascent from the priory, though steep, is practicable to about two thirds of its height, through fragments of lamellar rocks, mixed with quartz, feldtspar, and mica, in every possible proportion, and of different degrees of hardness, from the hardest lamellar granite to the softest micaceous rock. These fragments come either from the summit or from the flanks of the mountain. The summit in particular is formed of a rock, that appears to belong to the class of true granites, notwithstanding the parallel situation affected by the scales of mica, that form part of it. The back of the mountain is composed in great measure of a veined granite, with lenticular crystals of quartz of various sizes, but ranging in the direction of the laminæ.

Needles.

The Aiguilles, or needles, on the south-east of the valley of Chamouni, are five lofty pyramids, decreasing in height from the southernmost to the northernmost. The base that supports them, which rises seven or eight hundred toises above the valley, is composed of lamellar rocks of different kinds, but chiefly quartzose or micaceous, arranged in very regular strata, and running in the direction of the valley. Their inclination toward the bottom of the mountains is very little, but they gradually rise against the valley to the top, where they are exactly vertical. The higher up the mountain they are, the more they approach the nature of granite. The pyramids that rise above this are of granite in mass. They are composed externally of pyramidal laminæ, subdivided into strata parallel to the planes of the laminæ themselves. These laminæ are nearly vertical, and do not lean against the valley, like the lower strata of the mountain, but against the body of the pyramid. The heart of the pyramids, or their interior part, appears in some places to have no regular structure, and to be divided only by accidental clefts. These pyramids however must not be imagined to stand upon the mass below, as a pillar on its base: on the contrary they must have a base of their own beneath, on which the strata of this mass in part rest.

Aiguille du Dru.

The Aiguille du Dru is of a nearly similar structure, but among the fragments at its foot are blocks of primitive petrosilex, with a great deal of feldstpar, and a little mica.

Mount Jura.

The Jura is a chain of calcareous mountains 150 or 200 miles long, by 35 or 40 broad; running S. S. W. and N N. E. from the neighbourhood of Ponçin in Bresse to Basil. Its course therefore is nearly parallel with that of the Alps, of the external chains of which it must be considered as a dependance. It is not very easy to mark the situation and form of the strata of the Jura. Mr. Saussure thinks, that the strata of the eastern chain, which is the loftiest and the nearest the Alps, rise leaning against the centre of the chain, and decline on the opposite side; while the strata of the following chains to the west have the form of arches, or semi-arches, and terminate in plains, the base of which consists of horizontal beds of limestone.

Dôle not so high as Reculet.

The Dôle, twelve miles north of Geneva, has been generally deemed the highest summit of the Jura. According to Mr. Deluc it is 658 toises above the lake, and consequently 847 above the sea. I consider the Reculet, ten miles north-west of Geneva, as rather higher. It is one of the number of mountains in the chain, that appear to contradict the general form of the strata above given; for, instead of rising against the centre of the chain, they present their cliffs to the Alps.

Reculet.

Temperature of springs.

In an excursion I made on the Reculet, the 7th of August, 1802, I ascertained the temperature of two springs at the same time with their height above the sea. The temperature of one, situate above the *chalet* of Arderan, and 730 toises above the sea, was 4·8° [43° F.], while that of the open air was 21·5° [80·3 F.]. That of the other, called Converse, 815 toises above the sea, was 4° [41° F.], the thermometer in the open air being at 20·5° [78° F.]. These two observations agree sufficiently with the law of the decrease of heat laid down by Mr. Saussure; the following is not quite so consistent with it. On the 29th of August, 1802, the thermometer in a fine spring near the village of Veiri, at the foot of mount Salève, 200 toises above the sea, stood at 8·5° [51·2° F.], and in the open air at 13·7° [62·8° F.].

Agrees with Saussure's law.

Another does not.

XII.

XII.

Transformation of Mr. Dubuat's Hydraulic Theorem. By THOMAS YOUNG, *M.D. F.R.S.*

To Mr. NICHOLSON.

SIR,

Dubuat's hydraulic theorem transformed.

IN the course of some investigations respecting the motion of the blood, and the cause of fever, I have had occasion to employ the rules derived from the hydraulic experiments of Dubuat; and having reduced them into a more correct form than those which are mentioned in the second volume of my Lectures on Natural Philosophy, p. 225, I beg leave to make public, through the medium of your Journal, the formula which I have obtained.

Dubuat's rule, reduced to English inches, is $\frac{l + 45d}{h} = b$, and $v = 153\ (\sqrt{d} - \cdot 2)\,.\,\left(\frac{1}{\sqrt{b} - \text{h.l.}\ \sqrt{(b + 1\cdot 6)}} - \cdot 001\right)$ l being the length of a pipe, d its diameter, h the height of the reservoir, and v the velocity in a second. Now $\text{h.l.}\,x = n\,x^{\frac{1}{n}} - n$, when n is infinite, for the fluxion $\frac{x^{\frac{1}{n}}\dot{x}}{x}$ becomes ultimately $\frac{\dot{x}}{x}$; and the same is very nearly true when n is any large number: we may therefore express the hyperbolical logarithm, taking $n = 16$, in the terms $8\,b^{\frac{1}{16}} - 8 + \frac{\cdot 8}{b}$; and the whole formula may be changed into another, which will often be more convenient, in this manner, making $c = b^{\frac{1}{16}} - 1$, $v = 153\ (\sqrt{d} - \cdot 2)\,.\,\left(\frac{10\sqrt{b} + 75c + 8}{10b} - \cdot 0012.\right)$ This expression will be found as near the truth, in all cases, as can be supposed to have been correctly ascertained by Dubuat's experiments.

I am, SIR,
Your very obedient servant,
THOMAS YOUNG.

Welbeck Street, 22 Nov.

XIII.

XIII.

Observations on the Theory of Ear Trumpets, with a View to their Improvement; by JOHN GOUGH, *Esq.*

SIR, *Middleshaw, 20th Nov.* 1807.

An improved ear trumpet requested.

ONE of your correspondents, who appears at page 51 of your XIIIth volume, under the signature of A. B. has lately addressed me in the same anonymous character on the subject of ear trumpets. He requests to learn my sentiments respecting these instruments, and hints leading to their improvement through the medium of your Journal.

The subject hitherto neglected by philosophers.

I believe but little attention has been hitherto bestowed on this part of acoustics, though the inquiry is intimately connected with the ease and happiness of the partially deaf of all ranks and ages. This negligence in experimental philosophers, who have done so much for the improvement of optics, obliges me to begin with a fundamental and essential point of my subject; for we are in uncertainty at present in what manner an ear trumpet acts on the auditory organs. We may conceive the sonoriferous pulses to be collected in the cavity of the vessel, and to pass thence into the meatus auditorius in a state of increased condensation.

Two theories of the ear trumpet proposed.

On the other hand, we may suppose, that the same pulses strike the sides of the trumpet, and excite similar vibrations in this metallic shell, which imparts them to the orifice of the auditory duct. The latter supposition is rendered plausible by a simple experiment; if the porches of the ears be securely stuffed with wet paper, you may convey the clicking of a watch along a rod of wood or metal to the seat of hearing by simply touching the watch with one end of the rod, and pressing the other against the forehead, your teeth, or the wet paper in your ears. The preceding fact, in conjunction with other circumstances and arguments, induced me at first to prefer the second theory of ear trumpets, and to conclude, that the vibrations of the metal constitute the real cause of augmented sound rather than the condensation of sonoriferous pulses.

The latter theory plausible.

After

Mr. Nicholson's idea, vol. XIII, page 52, examined.

After taking this view of the subject, I was inclined to adopt your opinion in vol. XIII, page 52, that an instrument consisting of a broad thin surface, furnished with a tail or stem, promises to relieve partial deafness as effectually and more conveniently than a trumpet. A number of experiments, however, made for the purpose, have convinced me, that the vibration excited in thin plates of metal, wood and pasteboard, by sonoriferous pulses of air, cannot be condensed in a stem attached to these substances, so as to be conveyed with effect to the seat of hearing. My first trial was conducted in the following manner: a hole was made through a partition of lath and plaster, which was just large enough to receive a rod of deal 2 feet long, and ½ an inch in diameter. Some circular plates of metal were provided at the same time, as well as thin boards and pieces of pasteboard of the same figure, which were fixed at pleasure on the ends of the rod, by means of holes in their centres. This contrivance gives the observer an opportunity of placing himself in one room and the sonorous body in another; and this precaution prevents all the pulses from reaching his ear, except those that are conveyed by the rod, provided the force of the sound be too weak to make its way through the partition itself. The effects produced by this apparatus were the following: when one of the circular plates properly mounted on the end, was slightly scratched with a pin, or even with a piece of twisted paper, the sound passed very distinctly along the deal into the room where the observer was situated, and was thrown off into the air from a circle of wood or metal fixed on the rod in that apartment. The same circumstance took place when a watch was brought into contact with one of the circles, and the observer stood near the other; but absolute contact was found to be necessary, for the sound ceased as oft as the watch was removed the shortest distance from the circle.

Not continued by experiment.

Other experiments to the same purpose.

The discovery of this fact damped the expectation which I had hitherto entertained of affording relief to partial deafness by solid conductors of sound, but not to dismiss the inquiry apparently in a negligent manner, I procured two or three plates of different elastic substances, furnished with slender tails tales of wood, with which I made an unsuccessful experiment on the ears of a lady who labours under a considerable degree of nervous deafness. An attempt was also made to convey weak sounds by the same instrument to the auditory organs through the medium of the teeth, when the ears were stopped; but all these trials failed, unless the sonorous body happened to touch some part of the apparatus.

This succession of disappointments convinced me, that solid conductors can be of no advantage to the partially deaf.

Probable use of solid conductors.

Instances of great insensibility may occur indeed, in which we may arrive at the seat of hearing by their assistance, through the channel of the mouth, after every trial to approach it by the natural ducts and passages have proved fruitless. In this manner, perhaps, some persons apparently in a state of absolute deafness, might acquire some idea of the musical scale by attaching one end of a stick to a harpsichord, and holding the other in the mouth. At the same time, I am apprehensive your correspondent A. B. will be unfortunately disappointed in his expectations of relief from conductors, which are to be held in the teeth in the manner of a tobacco pipe.

An experiment on the ear trumpet.

The next experiment relates more directly to ear trumpets, and discovers the mode in which they operate on the auditory organs. I took a hollow copper cone, the mouth of which was 6 inches in diameter, and having closed one of my ears with wet paper, I introduced the small end of the tube into the other, taking care to cover that side of my face with a folded handkerchief, with a view to intercept as much as possible such vagrant pulses as were not received by the trumpet. Upon directing the wide end of my clumsy instrument towards a watch on a table, I found that it magnified the strokes of the balance beyond my expectation. But this was the case only while the tube remained open, for the watch ceased to be audible after a plug of wet paper had been forced into the narrow part of the tube, at the distance of 2 or 3 inches from the smaller extremity.

Trumpets condense soniferous pulses.

This experiment points out the office of an ear trumpet in a satisfactory manner. Its business is to condense the pulses which happen to fall into its cavity, and thereby to discharge them with greater

greater effect into the auditory ducts; our attention therefore, must be turned in future to the most probable means of increasing the condensing power of the instrument, if we wish to mitigate the inconvenience of nervous deafness.

Parabolic trumpets considered.

A vessel of a parabolical figure, and well polished, promises to be of service to the infirmity of partial deafness, on a superficial consideration of the subject, because such an instrument would concentrate in its focus all the pulses which happened to enter its mouth at the same instant, in a direction parallel to its axis. A trumpet of this description is liable to two serious objections, for the construction of it is very difficult, if not impossible; and if such a thing could be made, it would be attended with inconveniences, arising from its shape and dimensions, that would render the application of it very troublesome. This may be easily proved by a few simple calculations derived from the properties of the parabola; in reality it may be feared, that the partially deaf have little to expect from any kind of ear trumpets, but those of a conical figure; and apparently strong reasons lead me to suspect the best of them to be very imperfect augmenters of sound. Perhaps I may take a future opportunity to consider their defects mathematically; but it will be sufficient for the present purpose to observe, that very few of the pulses received at the mouth of a conical tube are transmitted to the ear through the opposite extremity. This might be demonstrated on the well known laws of incidence and reflection, and the truth of it is experimentally proved by the office of the funnel in the invisible lady, which does not transmit the whisper committed to it so much as it reflects it.

The improvements of conical trumpets difficult.

An instrument on the principle of a drum recommended.

It may be here naturally asked, if the partially deaf are to lay aside all future hopes of additional relief from the improvement of acoustic instruments? I will not venture to give a decisive answer to this question; perhaps future experiments may discover a more convenient and efficacious form of the ear trumpet than any in use at present; but I would recommend your correspondent A. B. or some one of his fellow sufferers, to exchange his trumpet for a drum, by way of trial. Perhaps this hint will appear obscure in its present form,

form, and an explanation will be expected. By a drum I mean a circular box, or funnel, furnished with an ear pipe, and having its mouth or widest aperture covered with a thin elastic membrane, which must be stretched with an uniform force in every direction, like the vellum of a military drum. The pulses which fall from the atmosphere upon this membrane will be immediately transmitted by it to the air confined in the box, and their escape from this cavity must evidently be through the ear tube, because the covering of the mouth will not permit them to return into the atmosphere by that aperture.

An experiment made with such an instrument.

The instrument here recommended is not a mere project of theory, for I have made some experiments, which induce me to hope for beneficial consequences from a contrivance of the kind. I took a metal funnel of 2 inches radius, and a circular wood box of the same diameter, which was a segment of a sphere of 8 inches radius. The mouth of each vessel was covered with a piece of hog's bladder, moistened in water, and securely fixed by a thread to the outside of the instrument. The bladder contracted in drying, by which it acquired a considerable degree of tension; and I do not hesitate to say, that both these drums, when properly applied to my ear, augmented the beats of a watch and other weak sounds, in a manner which was very perceptible. Should the experiment, however, appear worthy of further attention, it must be left in future to those who expect advantage from it, because such inquiries are attended with the greatest difficulties, when undertaken by persons who possess the sense of hearing in perfection. If your correspondent A. B. should conclude to pursue the subject, he will recollect, that a fine membrane is prepared from the intestines of cattle, which is called gold beater's skin, at least in the north of England; and it is unnecessary to inform him, that this substance is preferable in every point of view to the bladder which I used in my experiment.

The further prosecution of the experiment left to the partially deaf.

JOHN GOUGH.

P. S. I have long expected, that some of your correspondents would call an opinion in question, which I advanced nearly

nearly two years ago in the Philosophical Journal; but their silence obliges me to become my own accuser. Some experiments of mine will be found in your number for March, 1806, from which I inferred, that water cannot retain its fluidity when cooled below 32°. Such was my opinion at the time, but I was soon induced to suspect its accuracy from a conversation on the subject with Mr. Dalton, of Manchester. The glass vessels used in my experiments were externally covered with the freezing mixture nearly to their brims; but in consequence of this gentleman's information, I repeated the same experiment last winter, care being taken to raise the upper half of the vessel, containing the water, above the salt and snow which surrounded its bottom. With this precaution I found water might easily be cooled many degrees below the freezing point; in consequence of which discovery, I was obliged to abandon the theory referred to above, and in pronouncing it to be an errour I am only doing justice to the truth and your excellent miscellany.

Water may be cooled below 32°

XIV.

Report made to the Mathematical and Physical Class of the Institute, on a Memoir of Mr. DESCOTILS, *relative to Iron Spar: by Messrs.* BERTHOLLET, LELIEVRE, *and* VAUQUELIN*.

IN January 1806 Mr. Descotils read to the class a memoir in which he proved by experiments, that the iron spar, which was the subject of it, varied in the proportions of its constituent principles; and hence he explained the differences that the ores require in their metallurgic treatment. The difficulty of fusing some of them constituted at that time the principal object of his research; and the comparative analysis he made led him to the conclusion, that the magnesia, which is frequently found in them in large quantity, was the cause of their refractoriness.

Iron spars vary in their principles, & should be differently treated.

Their refractoriness owing to magnesia.

Reflecting on the processes adopted to deprive these ores of

Processes by

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* Annales de Chimie, vol. LXII, p. 135, May, 1807.

which they are deprived of it.

of the principle of their infusibility, which consist chiefly in exposure to the air and rain, either before or after roasting, Mr. Descotils conjectured, that these processes had no other effect than that of separating the magnesia.

Their mode of operation.

In the first case, that is to say, when these ores were exposed to the air before roasting, he supposed, that this earth was dissolved in the state of carbonate by the rain. In the second, on the contrary, he ascribed this effect to the sulphuric acid developed by the efflorescence of the pyrites, with which the iron spar is almost always accompanied.

Hassenfratz started some objections, but appears to have given them up.

Since that period Mr. Descotils has communicated to this assembly a second memoir, in which he furnishes substantial proofs of the explanations he had offered in the former paper as merely conjectural; at the same time avails himself of them to answer some objections, that had been advanced by Mr. Hassenfratz. The latter gentleman however, after having made some fresh experiments and observations, has withdrawn his memoir, which the class had referred to the same committee: we shall not therefore enter into any discussion of the points, on which these two learned chemists differed, but shall consider the facts related by Mr. Descotils, and the conclusion he has deduced from them, as if they had never been disputed.

The author has repeated his experiments, and made fresh.

On this second occasion Mr. Descotils has repeated his former experiments, which gave him the same results. He has likewise made new ones; and all, mutually supporting each other, have only confirmed him in his opinion. But let us relate some of these experiments.

Experiment with ore of Elba.

He exposed to the fire a mixture of fifteen parts of magnesia, and a hundred parts of iron ore from the isle of Elba, finely powdered; and the result he obtained was perfectly similar to what every magnesian iron spar has furnished him.

Effect not owing to its being powdered.

To ascertain whether the division of the particles of the substance had any influence on its fusibility, he made a trial with part of the same specimen of iron ore of Elba, without wasting or powdering it, and he obtained a perfectly compact button, at a degree of heat similar to what would have been requisite for an assay of earthy iron ore with the addition of borax.

Refractoriness

This fact shews, says the author, that cohesion does not diminish

diminish the fusibility of iron ores; at least if this cohesion can be estimated by the hardness of the ore, and the resistance it offers to the action of acids, for none possess these two qualities in a more striking degree than the iron crystals of the isle of Elba. The committee are of a similar opinion, only the fusion must require so much longer time in proportion as the ore is in fragments of a larger bulk.

therefore not owing to cohesion.

Mr. Descotils could have wished to analyse specimens of refractory iron spar comparatively with specimens of the same ore become fusible by exposure to the air: but not having been able to procure any, he thought he might supply their place by two pieces from the same vein, one of which was not altered, the other had passed to the state of a free ore.

But to magnesia.

Without describing the method he employed for this purpose, which we consider as very accurate, we shall only say, that he found the decomposed ore no longer contained any magnesia or carbonic acid, while the other contained four per cent of carbonic acid and magnesia.

Farther proofs.

The analysis of five other specimens of free ores, from different places, gave him the same results, whence he concludes, that the separation of the magnesia is complete when the decomposition of the ores is complete.

Mode in which the magnesia is removed.

In some cases he suspects, that it is to the efflorescence of the pyrites, from which scarcely any sparry iron ore is free, that the solution and abstraction of the magnesia of the raw ore is owing; since sulphate of magnesia is sometimes to be observed on heaps of ore of an analogous nature exposed to the air, as well as in the waters with which these ores are washed; and he has obtained similar results in a small way, by putting magnesian iron spar in powder, into a solution of sulphate of iron.

Action of carbonic acid.

He believes however, that it is most frequently the carbonic acid, which, disengaged from the iron in proportion as this absorbs oxigen, dissolves and carries off the magnesia by means of water.

Effect of roasting.

As to the change effected in the roasted ore by exposure to air and rain, the conjectures of Mr. Descotils are confirmed by analysing the waters, with which a heap of roasted ore long exposed to the air had been washed. These waters contained

contained nothing but sulphate of magnesia, and a little sulphate of lime; which salts could have been produced only by the action of the sulphuric acid, arising from the pyrites, on the earthy substances contained in the ore.

Old ores more fusible than new.

Mr. Descotils quotes letters of several well informed persons, and worthy of credit, who, in agreeing on the point that sparry iron ores recently extracted and roasted are more difficult of fusion, and less productive, than those that have remained three or four years in the open air, give ſtill more force to his theory.

Magnesia most injurious to rich ores.

Though it is certain, that the presence of magnesia in iron ores diminishes their fusibility more or less, the author of the memoir observes however, that, if it be accompanied with a sufficient quantity of lime, silex, and alumine, or of oxide of manganese, it is not so injurious, because it becomes fusible by combining with these substances.

No external marks of its presence.

Conceiving the advantage iron masters would find in having an easy method of knowing by simple inspection a free from a refractory ore, Mr. Descotils has examined, whether among the external characteristics of these substances there might not be some, by which these properties could be distinguished: but the strictest scrutiny in this respect was without success. He has been obliged therefore, to have recourse to chemical means, and what he found most to the purpose was fusing the ore without the addition of any flux.

Application of heat alone the best test.

Marks of a refractory ore.

If after this operation the matter present itself in a grayish, earthy, friable mass, interspersed with small globules of cast iron, it is a proof, that the ore is magnesian, and consequently more or less refractory.

Marks of a fusible ore.

But on the contrary, if a well fused button be obtained, with brown and not very abundant scoriæ, the ore is fusible, and contains but little magnesia.

Indication of manganese.

When the scoriæ are green, they indicate the presence of oxide of manganese, part of which is reduced, and mixes with the cast iron, by a high and long continued heat.

Loss in roasting.

The least altered kinds of sparry ores, that Mr. Descotils assayed, lost in roasting from 31 to 37 per cent. The altered or free ores lost at most but 14 per cent, and this loss was merely water.

Quantities of

The quantities of magnesia and manganese vary greatly: sometimes

sometimes there may be 12 per cent of either in the raw ore, and at others there is scarcely any.

manganese and magnesia.

Never a maximum of both.

From the results of his analyses Mr. Descotils concludes, that a high proportion of one excludes a high proportion of the other, without the absence of the one necessarily indicating the presence of the other; so that the iron, when brought to the state of red oxide, always amounts to 50 per cent at least.

Catalonian works.

Hence Mr. Descotils explains what takes place in the Catalonian forges, where the different species of ore are treated according to the nature, number, and quantity of the principles they contain. He points out the method, that each requires, and the product they afford, according as the operation is conducted. Sometimes it is cast steel, at others malleable iron, or some mixture of the two. On this occasion he expresses his surprise, that no one has yet thought of establishing a manufactory of cast steel in the Pyrenees.

Steel from the the Pyrenean ore.

Rich ores.

He thinks justly, that all rich iron ores, which contain but few earthy parts, such as those of the island of Elba, might be fused with advantage in the Catalonian method.

General deductions.

It follows evidently from the experiments of Mr. Descotils, that certain kinds of sparry ores owe their infusibility to the presence of a large quantity of magnesia: and that the principal object of the exposure of these ores to the air and rain, either before or after roasting, is to separate the magnesia, and render them fusible. The various experiments we have witnessed, and the results of which we have seen, leave us no doubt on this head: since on the one hand the ores in which there is no magnesia are easy of fusion, and those which contain a certain proportion are wholly infusible; while on the other the addition of magnesia to fusible ores divests them of this property, and infusible ores, when their magnesia is abstracted from them, become fusible.

From the observations of Mr. Descotils it farther follows, that there is no external character, by which we can distinguish whether a sparry iron ore be fusible or not: but he has pointed out chemical means of determining their nature, which are easy to put in practice.

Hence we are of opinion, that Mr. Descotils has thrown much light on the working of sparry iron ores; and that, as his

his memoir may in consequence be a very advantageous guide to the iron master, both from the well conducted experiments it exhibits, and the reflections and ideas he has added to them, the class should direct it to be printed in the volumes of its foreign contributions.

SCIENTIFIC NEWS.

Analysis of lycopodium.

MR. Bucholz has analysed the seed of lycopodium, which has afforded him the following results. A thousand parts of the seed contain 60 of a fat oil, analogous to castor oil, and very soluble in water, 30 of true sugar, and 15 of a mucilaginous extract. The remainder consists of a substance altogether insoluble in water, alcohol, ether, oil of turpentine, or caustic lixivium of potash. By long boiling with liquid potash however this substance is decomposed, gives out ammonia, and is converted into an extractive matter.

New substance in it.

By distillation it affords carburetted hidrogen gas, and carbonic acid gas; and afterward a watery liquor, impregnated with acetate of ammonia, and an empyreumatic oil. There remains a coal very analogous to anthracite, and difficult of incineration.

Nitric acid moderately concentrated being boiled on this substance converts it into a fat oil equally soluble in alcohol. The author concludes from his experiments, that this peculiar matter must be considered as distinct from all other vegetable or animal substances.

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No water formed in making acetic ether.

Mr. Thenard had supposed, see our last number, p. 185, that a certain quantity of water was formed in the mutual action of alcohol and acetic acid: but he now says he is convinced, that none is actually formed, of which he shall furnish proof in his memoir on ethers.

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To Correspondents.

Mr. C. Sylvester's paper was too late to be inserted in the present number, but will be given in the next.

A
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THE ARTS.

SUPPLEMENT TO VOL. XVIII.

ARTICLE I.

On some Chemical Agencies of Electricity. By HUMPHRY DAVY, *Esq. F. R. S. M. R. I. A. Read November* 20, 1806 *.

1. *Introduction.*

THE chemical effects produced by electricity have been for some time objects of philosophical attention; but the novelty of the phenomena, their want of analogy to known facts, and the apparent discordance of some of the results, have involved the inquiry in much obscurity.

Introductory remarks.

An attempt to elucidate the subject will not, I hope, be considered by the Society as unfitted to the design of the Bakerian Lecture. I shall have to detail some minute (and I fear tedious) experiments; but they were absolutely essential to the investigation. I shall likewise, however, be able to offer some illustrations of appearances, which hitherto have not been fully explained, and to point out some new properties of one of the most powerful and general of material agents.

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* From the Philosophical Transactions for 1807, Part I.

II. *On the Changes produced by Electricity in Water.*

Early observations of acid and alkali in Voltaic experiments.

The appearance of acid and alkaline matter in water acted on by a current of electricity, at the opposite electrified metallic surfaces, was observed in the first chemical experiments made with the column of Volta *.

Mr. Cruickshank † supposed, that the acid was the nitrous acid, and the alkali ammonia. M. Desormes ‡ soon after attempted to show by experiments, that muriatic acid and ammonia were the products, and M. Brugnatelli § asserted the formation of a new and peculiar substance, which he has thought proper to call the electric acid. The experiments said to be made in Italy, and in this country, on the production of muriate of soda are recent ||, and the discussions with regard to them still alive. As early as 1800, I had found that when separate portions of distilled water, filling two glass tubes connected by moist bladders, or any moist animal or vegetable substances, were submitted to the electrical action of the pile of Volta by means of gold wires, a nitro-muriatic solution of gold appeared in the tube containing the positive wire, or the wire transmitting the electricity, and a solution of soda in the opposite tube **; but I soon ascertained, that the muriatic acid owed its appearance to the animal or vegetable matters employed; for when the same fibres of cotton were made use of in successive experiments, and washed after every process in a weak solution of nitric acid, the water in the apparatus containing them, though acted on for a great length of time with a very strong power, at last produced no effect upon solution of nitrate of silver.

In cases when I had procured much soda, the glass at its

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* Nicholson's Journal, 4to. Vol. IV, p. 183.

† Ibid. Vol. IV, p. 261.

‡ *Annales de Chimie*, Tom. XXXVII, p. 233.

§ Phil. Mag. Vol. IX, p. 181.

|| By M. Pacchioni, and by Mr. Peele. Phil. Mag. Vol. XXI, p. 279.

** I showed the results of the experiment to Dr. Beddoes at this time; and mentioned the circumstance to Sir James Hall, Mr. Clayfield, and other friends in 1801.

point

Changes produced by electricity in water.

point of contact with the wire seemed considerably corroded; and I was confirmed in my idea of referring the production of the alkali principally to this source, by finding that no fixed saline matter could be obtained, by electrifying distilled water in a single agate cup from two points of platina connected with the Voltaic battery. Similar conclusions with regard to the appearance of the muriatic acid had been formed by the Galvanic Society of Paris, by Dr. Wollaston, who hit upon the happy expedient of connecting the tubes together by well washed asbestus; and by M. M. Biot and Thenard *.

Mr. Sylvester, however, in a paper published in Mr. Nicholson's Journal for last August, states, that though no fixed alkali or muriatic acid appears when a single vessel is employed; yet that they are both formed when two vessels are used. And to do away all objections with regard to vegetable substances or glass, he conducted his process in a vessel made of baked tobacco-pipe clay inserted in a crucible of platina. I have no doubt of the correctness of his results: but the conclusion appears objectionable. He conceives, that he obtained fixed alkali, because the fluid after being heated and evaporated left a matter that tinged turmeric brown, which would have happened had it been lime, a substance that exists in considerable quantities in all pipe-clay; and even allowing the presence of fixed alkali, the materials employed for the manufacture of tobacco-pipes are not at all such as to exclude the combinations of this substance.

I resumed the inquiry; I procured small cylindrical cups of agate, of the capacity of about ¼ of a cubic inch each. They were boiled for some hours in distilled water, and a piece of very white and transparent amianthus, that had been treated in the same way, was made to connect them together; they were filled with distilled water, and exposed by means of two platina wires to a current of electricity, from 150 pairs of plates of copper and zinc 4 inches square, made active by means of solution of alum. After 48 hours the process was examined: paper tinged with litmus plunged into the tube containing the transmitting or positive wire was immediately

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* No. XL *du Moniteur,* 1806.

strongly

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strongly reddened. Paper coloured by turmeric introduced into the other tube had its colour much deepened; the acid matter gave a very slight degree of turbidness to solution of nitrate of silver. The fluid that affected turmeric retained this property after being strongly boiled; and it appeared more vivid as the quantity became reduced by evaporation; carbonate of ammonia was mixed with it, and the whole dried and exposed to a strong heat: a minute quantity of white matter remained, which, as far as my examination could go, had the properties of carbonate of soda. I compared it with similar minute portions of the pure carbonates of potash and soda. It was not so deliquescent as the former of these bodies, and it formed a salt with nitric acid, which like nitrate of soda soon attracted moisture from a damp atmosphere, and became fluid.

This result was unexpected, but it was far from convincing me, that the substances which I had obtained were *generated.* In a similar process with glass tubes, carried on exactly under the same circumstances, and for the same time, I obtained a quantity of alkali which must have been more than twenty times greater, but no traces of muriatic acid. There was much probability, that the agate might contain some minute portion of saline matter, not easily detected by chemical analysis, either in combination, or intimate adhesion in its pores. To determine this, I repeated the experiment a second, a third, and a fourth time. In the second experiment turbidness was still produced by solution of nitrate of silver in the tube containing the acid, but it was less distinct; in the third process it was barely perceptible: and in the fourth the two fluids remained perfectly clear after the mixture. The quantity of alkaline matter diminished in every operation; and in the last process, though the battery had been kept in great activity for three days, the fluid possessed in a very slight degree only the power of acting on paper tinged with turmeric; but its alkaline property was very sensible to litmus paper slightly reddened, which is a much more delicate test: and after evaporation and the process by carbonate of ammonia, a barely perceptible quantity of fixed alkali was still left. The acid matter in the other tube was abundant; its taste was sour; it smelt like water over which large quan-

Changes produced by electricity in water.

tities of nitrous gas have been long kept; it did not affect solution of muriate of barytes; and a drop of it placed upon a polished plate of silver left after evaporation a black stain, precisely similar to that produced by extremely diluted nitrous acid.

After these results, I could no longer doubt that some saline matter existing in the agate tubes had been the source of the acid matter capable of precipitating nitrate of silver, and of much of the alkali. Four additional repetitions of the process, however, convinced me, that there was likewise some other cause for the presence of this last substance; for it continued to appear to the last, in quantities sufficiently distinguishable, and apparently equal in every case. I had used every precaution; I had included the tubes in glass vessels out of the reach of the circulating air; all the acting materials had been repeatedly washed with distilled water; and no part of them in contact with the fluid had been touched by the fingers.

The only substance which I could now conceive capable of furnishing the fixed alkali was the *water itself*. This water appeared pure by the tests of nitrate of silver and muriate of barytes; but potash and soda, as is well known, rise in small quantities in rapid distillations; and the New River water, which I made use of, contains animal and vegetable impurities, which it was easy to conceive might furnish neutral salts capable of being carried over in vivid ebullition.

To make the experiment in as refined a form as possible, I procured two hollow cones of pure gold containing about 25 grains of water each, they were filled with distilled water, connected together by a moistened piece of amianthus which had been used in the former experiments, and exposed to the action of a Voltaic battery of 100 pairs of plates of copper and zinc of six inches square, in which the fluid was a solution of alum and diluted sulphuric acid. In ten minutes the water in the negative tube had gained the power of giving a slight blue tint to litmus paper: and the water in the positive tube rendered it red. The process was continued for 14 hours; the acid increased in quantity during the whole time, and the water became at last very sour to the taste. The

Changes produced by electricity in water.

The alkaline properties of the fluid in the other tube, on the contrary, remained stationary, and at the end of the time, it did not act upon litmus or turmeric paper more than in the first trial: the effect was less vivid after it had been strongly heated for a minute; but evaporation and the usual process proved that *some* fixed alkali was present. The acid, as far as its properties were examined, agreed with pure nitrous acid, having an excess of nitrous gas.

I *repeated* the experiment, and carried on the process for three days; at the end of which time the water in the tube was decomposed and evaporated to more than one half of its original quantity; the acid was strong, but the alkali in as minute a portion as in the last experiment. It acted indeed rather more vividly on the tests, on account of the greater diminution of the fluid, but presented the same results after being heated.

It was now impossible to doubt, that the water contained some substance in very minute quantities, capable of causing the appearance of fixed alkali, but which was soon exhausted; and the question that immediately presented itself was, Is this substance saline matter carried over in distillation? or is it nitrogen gas, which exists in minute portions in all water that has been exposed to air, and which, if an element of the fixed alkali, would under the circumstance of the experiment have been soon exhausted, whilst its absorption from the atmosphere would be impeded by the saturation of the water with hydrogen?

I was much more inclined to the former than to the latter supposition. I evaporated a quart of the distilled water that I had used, very slowly at a heat below 140° Fahrenheit, in a silver still; a solid matter remained, equal to $\frac{7}{10}$ of a grain; this matter had a saline but metallic taste, and was deliquescent when exposed to air: I could not obtain from it regular crystals; it did not affect turmeric or litmus, but a part of it, after being heated red, in a silver crucible, exhibited strong alkaline properties. It was not possible to make a minute analysis of so small a quantity, but it appeared to me to be principally a mixture of nitrate of soda and nitrate of lead; and the metallic substance, it is most likely, was furnished by the condensing tube of the common still.

The

Changes produced by electricity in water.

The existence of saline matter in the distilled water being thus distinct, it was easy to determine its operation in the experiment. I filled the two gold cones with water in the usual manner; that negatively electrified, soon attained the maximum of its effect upon turmeric paper. I then introduced into it a very minute portion of the substance obtained by the process of evaporation that has been just described; in less than two minutes its effects were evident; and in five minutes the tint of the paper was changed to a bright brown.

I now conceived that by collecting the water obtained in the second process of slow distillation I should be able to carry on the experiment without any appearance of fixed alkali, and the trial proved that I was not mistaken.

Some of this water was introduced into the gold tubes, and the amianthus moistened by it.

After two hours the water in the negative tube produced no effect upon turmeric paper; it did produce an effect upon litmus, which it required great minuteness of observation to perceive; but it wholly lost the power by being heated strongly for two or three minutes, so there is every reason for supposing that it was owing to a small quantity of ammonia.

I made a similar experiment with a portion of the same water in the tubes of agate that had been so often used, and I had the pleasure of finding the results precisely the same.

To detail any more operations of this kind will be unnecessary; all the facts prove, that the fixed alkali is not *generated*, but *evolved*, either from the solid materials employed, or from saline matter in the water.

I have made many experiments in vessels composed of different substances, with the water procured by very slow distillation: and in almost every instance some fixed alkali appeared.

In tubes of wax the alkaline matter was a mixture of soda and potash; and the acid matter a mixture of sulphuric, muriatic, and nitric acids.

In a tube of resin, the alkaline matter seemed to be principally potash.

A cube of Carrara marble of about an inch, having an aperture

Changes produced by electricity in water.

aperture in its centre, was placed in a crucible of platina, which was filled as high as the upper surface of the cube with the purified water, the aperture was filled with the same fluid; the crucible was positively electrified by a strong Voltaic power, and a negatively electrified wire introduced into the aperture.

The water soon gained the property of affecting the tint of turmeric; and fixed alkali and lime were both obtained from it: and this effect took place in repeated experiments: the fixed alkali, however, diminished in quantity every time; and after eleven processes conducted from two to three hours each, disappeared altogether. The production of lime-water was uniform.

I made a solution of 500 grains of this marble in nitric acid; I decomposed the mixture by carbonate of ammonia, and I collected and evaporated the fluid part, and decomposed the nitrate of ammonia by heat. About $\frac{3}{4}$ of a grain of fixed saline matter remained, which had soda for its base.

It was possible that the Carrara marble might have been recently exposed to sea-water; I therefore tried, in the same way, a piece of granular marble, which I had myself broken from a rock on one of the highest of the primitive mountains of Donegal. It afforded fixed alkali by the agency of negative electricity.

A piece of argillaceous schist from Cornwall, treated in the same manner, gave the same result; and serpentine from the Lizard, and grauwacke from North Wales, both afforded soda. It is probable that there are few stones, that do not contain some minute portions of saline matter, which in many cases may be mechanically diffused through their substance: and it is not difficult to conceive the possibility of this, when we consider that all our common rocks and strata bear evident marks of having been anciently covered by the sea.

I was now able to determine distinctly, that the soda procured in glass tubes came principally from the glass, as I had always supposed.

I used the two cones of gold with the purified water and the amianthus; the process was conducted as usual. After a quarter

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a quarter of an hour, the negatively electrified tube did not change the colour of turmeric. I introduced into the top of it a bit of glass; in a few minutes the fluid at the surface rendered the tint of the paper of a deep bright brown.

I had never made any experiments, in which acid matter having the properties of nitrous acid was not produced, and the longer the operation the greater was the quantity that appeared.

Volatile alkali likewise seemed to be always formed in very minute portions, during the first few minutes in the purified water in the gold cones, but the limit to its quantity was soon attained.

It was natural to account for both these appearances, from the combination of nascent oxigen and hidrogen respectively; with the nitrogen of the common air dissolved in the water: and Dr. Priestley's experiments on the absorption of gasses by water (on this idea) would furnish an easy explanation of the causes of the constant production of the acid, and the limited production of the alkali: for hidrogen, during its solution in water, seems to expel nitrogen; whilst nitrogen and oxigen are capable of coexisting dissolved in that fluid *.

To render the investigation more complete, I introduced the two cones of gold with purified water under the receiver of an air pump; the receiver was exhausted till it contained only $\frac{1}{64}$ of the original quantity of air; and then, by means of a convenient apparatus, the tubes were connected with an active Voltaic pile of 50 pairs of plates of four inches square. The process was carried on for 18 hours, when the result was examined. The water in the negative tube produced no effect upon prepared litmus, but that in the positive tube gave it a barely perceptible tinge of red.

An incomparably greater quantity of acid would have been formed in a similar time in the atmosphere, and the small portion of nitrogen gas remaining in contact with the water seemed adequate to the effect.

I repeated the experiment under more conclusive circumstances. I arranged the apparatus as before; I exhausted

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* Priestley's Experiments and Observations, Vol. I, p. 59.

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the receiver, and filled it with hidrogen gas from a convenient airholder; I made a second exhaustion, and again introduced hidrogen that had been carefully prepared. The process was conducted for 24 hours, and at the end of this time neither of the portions of the water altered in the slightest degree the tint of litmus.

It seems evident then, that water chemically pure is decomposed by electricity into gaseous matter alone, into oxigen and hidrogen.

The cause of its decomposition, and of the other decompositions which have been mentioned, will be hereafter discussed.

III. *On the Agencies of Electricity in the Decomposition of various Compounds.*

Action of electricity in decomposing compounds.

The experiments that have been detailed on the production of alkali from glass, and on the decomposition of various saline compounds contained in animal and vegetable substances, offered some curious objects of inquiry.

It was evident, that in all changes in which acid and alkaline matter had been present, the acid matter collected in the water round the positively electrified metallic surface; and the alkaline matter round the negatively electrified metallic surface; and this principle of action appeared immediately related to one of the first phænomena observed in the Voltaic pile, the decomposition of the muriate of soda attached to the pasteboard; and to many facts which have been since observed on the separation of the constituent parts of neutrosaline and metallic solutions, particularly those detailed by M. M. Hisinger and Berzelius *.

The first experiments that I made immediately with respect to this subject were on the decomposition of solid bodies, insoluble, or difficultly soluble in water. From the effects of the electrical agency on glass, I expected that various earthy compounds would undergo change under similar circumstances; and the results of the trials were decided and satisfactory.

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* Annales de Chimie, Tom. LI, p. 167.

Two

Action of electricity in decomposing compounds.

Two cups made of compact sulphate of lime, containing about 14 grain-measures of water each, were connected together by fibrous sulphate of lime, which was moistened by pure water: the cups were filled with this fluid; platina wires from the Voltaic battery of 100 pairs of plates of six inches were introduced into them, so that the circuit of electricity was through the fibrous sulphate of lime. In five minutes the water in the cup connected with the positive wire became acid; that in the opposite cup strongly tinged turmeric. After an hour the fluids were accurately examined; when it was found that a pure and saturated solution of lime had been produced in the cup containing the negative wire, which was partially covered with a crust of lime; and that the other cup was filled with a moderately strong solution of sulphuric acid.

I procured two cubical pieces of crystallized sulphate of strontites, of about an inch; a hole was drilled in each capable of containing about eight grains of water: the cubes were plunged in pure water in a platina crucible; and the level of the fluid preserved a few lines below the surface of the cubes; two platina wires were introduced into the holes, which were filled with pure water. The disengagement of gas, when the wires were connected with the battery of 100, proved that the sulphate of strontites was sufficiently porous to form a proper conducting chain. The results were much longer in being obtained in this experiment than in the last: some time elapsed before a sensible effect could be perceived; but the termination was similar. In 30 hours the fluid in the cavity containing the negative wire had gained the property of precipitating solution of sulphate of potash; and the presence of sulphuric acid in the other cavity was evident from its effect upon solution of muriate of barytes.

I made an experiment upon fluate of lime under like circumstances; but the crystallized fluate not being equally permeable to moisture, the two cavities were connected by moist asbestus. This decomposition was likewise very slow; but in the course of two days a pretty strong solution of lime was obtained in one tube; and an acid fluid in the other, which precipitated acetite of lead, and left a spot upon the glass from which it had been evaporated.

Action of electricity in decomposing compounds.

Sulphate of barytes, as might be supposed, proved much more difficult of decomposition than either sulphate of strontites or fluate of lime. I had made four or five experiments upon it, with the same kind of apparatus that had been applied to the fluate of lime, before I was able to gain decided results. In the last process performed on this substance, two pieces of a large single crystal were hollowed by grinding, so as to contain about five grains of water each; they were connected by moist asbestus, and constantly subjected during four days to the strong action of a battery of 150 pairs of plates of 4 inches square. As the water diminished, its place was supplied by new quantities. At the conclusion of the experiment the fluid on the positive side of the apparatus instantly reddened litmus, tasted very sour, and gave a distinct precipitate with a solution of muriate of barytes; the water on the other side deepened the tincture of turmeric; but did not render solution of sulphate of potash turbid. There was a small quantity of white crust, however, on the sides and the bottom of the cavity, and I conceived that this might be the barytes, which, during the extremely slow decomposition, would have combined with the carbonic acid of the atmosphere. To ascertain if this had been the case, I introduced into the cavity a drop of diluted muriatic acid; a slight effervescence appeared, and the fluid obtained occasioned a distinct white cloudiness in solution of sulphate of soda.

In all these cases the constituent parts of the bodies newly arranged by the effects of electricity existed in considerable quantities, and exposed on a large surface to its action. I had great reason to believe, from the trials with distilled water in different vessels, that very minute portions of acid and alkaline matter might be disengaged by this agency from solid combinations, principally consisting of pure earths.

This part of the investigation was easily elucidated.

For a purpose of geological inquiry, which on a future occasion I shall have the honour of laying before the Society, I had made a careful analysis of a specimen of fine grained basalt from Port Rush in the county of Antrim,

Action of electricity in decomposing compounds.

by means of fusion with boracic acid: it afforded in 100 parts 3½ parts of soda, and nearly ½ a part of muriatic acid, with 15 parts of lime. This stone appeared to me very well fitted for the purpose of experiment: cavities were drilled in two pieces, properly shaped; they contained about 12 grains of water each; they were connected by moistened amianthus, and the process conducted as usual with a power of 50 pairs of plates. At the end of ten hours the result was examined with care. The fluid that had been positively electrified had the strong smell of oximuriatic acid, and copiously precipitated nitrate of silver; the other portion of fluid affected turmeric, and left by evaporation a substance which seemed to be a mixture of lime and soda.

A part of a specimen of compact zeolite, from the Giant's Causeway, which by analysis had given 7 parts in 100 of soda, had a small cavity made in it; it was immerged in pure water in a crucible of platina, and electrified in the same manner as the cube of Carrara marble, mentioned in page 328. In less than two minutes the water in the cavity had gained the property of changing the colour of turmeric; and in half an hour the solution was disagreeably alkaline to the taste. The matter dissolved proved to be soda and lime.

Lepidolite, treated in the same way, gave potash.

A piece of vitreous lava, from Etna, gave alkaline matter, which seemed to be a mixture of soda, potash, and lime.

As in these trials the object was merely to ascertain the general fact of decomposition, the process was never conducted for a sufficient time to develope a quantity of alkaline matter capable of being conveniently weighed, and of course any loss of weight of the substance could not be determined.

I thought it right, however, to make one experiment of this kind, for the sake of removing every possibility of doubt on the source of the different products; and I selected for this purpose glass, as a substance apparently insoluble in water, and not likely to afford in any way erroneous results.

Action of electricity in decomposing compounds.

The balance that I employed was made for the Royal Institution, by Mr. Fidler, after the model of that belonging to the Royal Society; it turns readily with $\frac{1}{500}$ of a grain when loaded with 100 grains on each side; a glass tube with a platina wire attached, weighing 84 grains $\frac{56}{128}$ was connected with an agate cup, by amianthus; they were filled with purified water, and electrified by a power from 150 pairs of plates, in such a way that the platina in the glass tube was negative. The process was continued for four days, when the water was found alkaline. It gave by evaporation and exposure to a heat of about 400° Fahrenheit, soda mixed with a white powder insoluble in acids, the whole weight of which was $\frac{36}{128}$ of a grain. The glass tube carefully cleaned and dried weighed 84 grains, $\frac{37}{128}$. The difference between the loss of weight of the tube and the weight of the products in the water may be easily explained: some minute detached particles of amianthus were present, and the soda must have contained water, a substance which it is probably perfectly free from in glass.

Having obtained such results with regard to the disengagement of the saline parts of bodies insoluble in water, I made a number of experiments on soluble compounds: their decomposition was always much more rapid, and the phenomena perfectly distinct.

In these processes I employed the agate cups with platina wires, connected by amianthus moistened in pure water; the solutions were introduced into the cups, and the electrifying power applied from batteries of 50 pairs of plates, in the usual way.

A diluted solution of the sulphate of potash treated in this manner, produced in four hours at the negative wire a weak lixivium of potash; and a solution of sulphuric acid at the positive wire.

The phenomena were similar when sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, phosphate of soda, succinate, oxalate, and benzoate of ammonia, and alum were used. The acids in a certain time collected in the tube containing the positive wire, and the alkalies and earths in that containing the negative wire.

Solutions of the muriatic salts, decomposed in the same way, uniformly gave oximuriatic acid on the positive side.

When

Action of electricity in decomposing compounds.

When compatible mixtures of neutrosaline solutions containing the common mineral acids were used, the different acids and the different bases seemed to separate together in a mixed state, without any respect to the orders of affinity.

When metallic solutions were employed, metallic crystals or depositions were formed, as in common galvanic experiments, on the negative wire, and oxide was likewise deposited round it; and a great excess of acid was soon found in the opposite cup. With solutions of iron, zinc, and tin, this effect took place, as well as with the more oxidable metals: when muriate of iron was used, the black substance deposited upon the wire was magnetic, and dissolved with effervescence in muriatic acid; and when sulphate of zinc was used, a gray powder possessed of the metallic lustre, and likewise soluble with effervescence, appeared; and in all cases acid in excess was exhibited on the positive side.

Strong or saturated saline solutions, as might have been expected, afforded indications of the progress of decomposition much more rapidly than weak ones; but the smallest proportion of neutrosaline matter seemed to be acted on with energy.

A very simple experiment demonstrates this last principle. If a piece of paper tinged with turmeric is plunged into pure water in a proper circuit, in contact with the negative point, the very minute quantity of saline compound contained in the paper affords alkaline matter sufficient to give it instantly a brown tint near its point of contact: and acid in the same manner is immediately developed from litmus paper, at the positive surface.

I made several experiments, with the view of ascertaining whether, in the decompositions by electricity, the separation of the constituent parts was complete from the last portions of the compound; and whenever the results were distinct, this evidently appeared to be the case.

I shall describe one of the most conclusive of the experiments: a very weak solution of sulphate of potash, containing 20 parts water and one part saturated solution, at 64°, was electrified in the two agate cups by the power of

50 pairs

Action of electricity in decomposing compounds.

50 pairs of plates for three days: the connecting amianthus, which had been moistened with pure water, was removed, washed with pure water, and again applied, twice every day; by this precaution the presence of any neutral salt that might adhere to it, and disturb the results, was prevented. The alkali obtained in this process in the solution had the properties of pure potash; and when it had been saturated with nitric acid it gave no turbidness by mixture with solution of muriate of barytes: the acid matter exposed to a strong heat evaporated without leaving any residuum.

IV. *On the Transfer of certain of the constituent Parts of Bodies by the Action of Electricity.*

Transference of certain constituent parts of bodies by electricity.

M. Gautherot has stated*, that in a single galvanic circle of zinc, silver, and water, in an active state, the oxide of zinc formed is attracted by the silver †; and M. M. Hisinger and Berzelius detail an account of an experiment, in which solution of muriate of lime being placed in the positive part of a siphon, electrified by wires from a Voltaic pile, and distilled water in the negative part, lime appeared in the distilled water.

These facts rendered it probable, that the saline elements evolved in decompositions by electricity were capable of being transferred from one electrified surface to another, according to their usual order of arrangement; but to demonstrate this clearly, new researches were wanting.

I connected one of the cups of sulphate of lime, mentioned page 331, with a cup of agate by abestus; and, filling them with purified water, made the platina wire in the cup of sulphate of lime transmit the electricity from a power of 100; a wire in the agate cup received it. In about four hours a strong solution of lime was found in the agate cup, and sulphuric acid in the cup of sulphate of lime. By reversing the order, and carrying on the process for a similar time, the sulphuric acid appeared in the agate cup, and the solution of lime on the opposite side.

Many trials were made with other saline substances, with

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* Annales de Chimie, Vol. XXXIX, page 203.

† Ibid. Vol. LI, page 171.

analogous

Transference of certain constituent parts of bodies by electricity.

analogous results. When the compounds of the strong mineral acids with alkaline or alkaline-earthy bases were introduced into one tube of glass, distilled water connected by amianthus being in another tube, both connected by wires of platina in the Voltaic arrangement, the base always passed into the distilled water when it was negative, and the acid when it was positive.

The metals and the metallic oxides passed towards the negative surface like the alkalies, and collected round it. In a case in which solution of nitrate of silver was used on the positive side, and distilled water on the negative, silver appeared on the whole of the transmitting amianthus, so as to cover it with a thin metallic film.

The time required for these transmissions (the quantity and intensity of the electricity, and other circumstances remaining the same) seemed to be in some proportion as the length of the intermediate volume of water. Thus when, with the power of 100, sulphate of potash was on the negative side, and distilled water on the positive side, the distance between the wires being only an inch, sulphuric acid, in sufficient quantity to be very manifest, was found in the water in less than five minutes: but when the tubes were connected by an intermediate vessel of pure water, so as to make the circuit eight inches, 14 hours were required to produce the same effect.

To ascertain whether the contact of the saline solution with a metallic surface was necessary for the decomposition and transfer, I introduced purified water into two glass tubes; a vessel containing solution of muriate of potash was connected with them respectively by amianthus; and the arrangement was made in such a way, that the level of both the portions of purified water was higher than the level of the saline solution.

In this case, the saline matter was distant from each of the wires at least $\frac{2}{3}$ of an inch; yet alkaline matter soon appeared in one tube, and acid matter in the other: and in 16 hours moderately strong solutions of potash, and of muriatic acid had been formed.

In this case of electrical transfer or attraction, the acid and alkaline matter seemed to be perfectly pure; and I am

inclined

Transference of certain constituent parts of bodies by electricity.

inclined to believe, that this is uniformly the case in all experiments carefully made. One of the instances in which I conceived acid most likely to be present, was in the transfer of magnesia from sulphate of magnesia in the positive tube, to distilled water in the negative tube. I examined the case, taking care that the distilled water was never upon a lower level than the saline solution: the process was continued for some hours, till a considerable quantity of magnesia had appeared. The connecting amianthus was removed, and muriatic acid poured into the tube: the saturated solution did not precipitate solution of muriate of barytes.

I endeavoured to ascertain the progress of the transfer, and the course of the acid or alkaline matter in these decompositions, by using solutions of litmus and turmeric, and papers coloured by these substances; and these trials led to the knowledge of some singular and unexpected circumstances.

Two tubes, one containing distilled water, the other solution of sulphate of potash, were each connected by amianthus with a small *oz.* measure filled with distilled water tinged by litmus: the saline solution was negatively electrified; and as it was natural to suppose, that the sulphuric acid in passing through the water to the positive side would redden the litmus in its course, some slips of moistened paper tinged with litmus were placed above and below the pieces of amianthus, directly in the circuit. The progress of the experiment was minutely observed; the first effect of reddening took place immediately above the positive surface, where I had least expected it; the red tint slowly diffused itself from the positive side to the middle of the vessel, but no redness appeared above the amianthus, or about it, on the negative side, and though it had been constantly transmitting sulphuric acid, it remained unaffected to the last.

The order of the experiment was changed, and the saline solution placed on the positive side; a solution and papers tinged with turmeric being substituted for those tinged with litmus. The effect was precisely analogous; the turmeric became brown first near the negative wire, and no change took

took place in the intermediate vessel near the positive wire.

In another process, the two glass tubes were filled with solution of muriate of soda, and the intermediate vessel with solution of sulphate of silver: paper tinged with turmeric was placed on the positive side, and paper tinged with litmus on the negative side; as soon as the electrical circuit was complete, soda began to appear in the negative tube, and oximuriatic acid in the positive tube, and the alternate products were exhibited passing into the solution of sulphate of silver, the muriatic acid occasioning a dense heavy precipitate, and the soda a more diffused and a lighter one; but neither the turmeric transmitting the alkali, nor the litmus transmitting the acid, had their tints in the slightest degree altered.

(To be continued.)

II.

Extract of a Letter from Mr. J. M. HAUSSMANN *to Mr.* BERTHOLLET *.

Superoxigenated sulphate of iron.

WHEN I published my memoir on Stahl's alkaline tincture of steel, I imagined that this superoxigenated sulphate of iron would not fail to be examined more minutely: but finding myself disappointed in this respect, I cannot refrain from again calling to mind some of its properties that appeared to me striking. These are its being wholly insipid to the taste, when completely deprived of its water of crystallization: and of acquiring astringent powers, surpassing those of any other astringent known, as soon as it has imbibed moisture from the air, or been dissolved in water. This superoxigenated sulphate of iron produces the most beautiful prussian blue possible; and it may be used with advantage in dyeing, particularly for blacks.

Its properties.

Nitrate of iron crystallized.

I read with pleasure Mr. Thenard's paper, but I cannot be of his opinion with respect to the nitrate of iron saturated with oxigen, crystals of which I can easily produce

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* Annales de Chimie, Vol. LVIII. p. 182, May, 1806.

without diluting the nitric acid of 40° of Messrs. Coustou and Co. at Paris. I use this same acid, to convert sugar into oxalic acid; and, whether I employ it of its full strength, or diluted with equal parts of water, I constantly obtain a little greasy matter, when I conduct the process in the large way on a vapour bath.

Fatty matter from sugar.

On treating the same sugar three times successively with equal portions of this acid, either concentrated or diluted, the first portion occasions a brown colour, and produces a smell of burnt sugar. And when the action of the nitric acid has ceased, we already perceive some of this grease swimming at the top; and it appears to be farther increased by the successive addition of the other two portions of acid, which cause the brown colour and smell of burnt sugar to disappear, forming a great abundance of oxalic acid, and a small quantity of the malic and citric acids. Perhaps, if the gasses were collected, we should find a little acetic acid also.

Sugar treated with nitric acid,

To satisfy myself whether the sugar gave rise to the formation of the grease, I examined one of the largest sized sugar-loaves, which I commonly use. I divided it into two equal portions, the first consisting of the outer part of the loaf, the second of the inner. Each of these portions I boiled for a few minutes in three times its weight of water. No grease swam on either of these solutions of sugar, after they were cold: but as they were not very clear, I began to suspect, that, the sirup for common sugar being clarified with bullock's blood by the sugar bakers, the gelatinous part of this animal substance unites in some measure with the particles of sugar by a forced and confused crystallization, and, when acted upon by nitric acid, may give rise to the separation of grease. I was not long before I satisfied myself, that my suspicion was just, for, on making oxalic acid with some fine white sugarcandy, and at the same time with the finest loaf sugar I could procure, neither of these showed any signs of grease.

No oil from it by boiling.

Probably from the fining.

None from fine sugar or candy.

Fat oils in their natural state have not the least action on asphaltum, jews' pitch, or copal: but if they be reduced to a soap, and afterward separated by any acid, they not only exert a strong solvent power on these substances,

Fat oils separated from soaps by an acid acquire a solvent power.

stances, but they further acquire the property of decomposing acetate of lead, as well as other metallic acetates, and of combining readily with their oxides, the acetic acid of which is given out. These oils thus separated would produce the same effect perhaps on other metallic salts. In general all fats, resins, and turpentines, combine better with other substances, after they have been reduced to soap and separated by an acid, than in their natural state. Wax comports itself in the same manner. A knowledge of this effect induced me to subject to the action of the process for forming oxalic acid a small portion of oil separated from Marseilles soap, which I mixed with sugar previously powdered. At the end of the operation I found, that the oil had acquired the consistence of suet, and that it had assumed a yellowish colour and a rancid smell, retaining the property of swimming on water. This grease, having been exposed to the same process a second time, had its rancidity increased, contracting at the same time a little of the smell of wax; and its specific gravity became so great, that, after it had been well washed and perfectly freed from acidity, it sank to the bottom of water, without having lost its property of being soluble in alcohol.

This common to fats and resins.

This oil exposed to the action of nitric acid.

Indigo.

My memoir on indigo shows, that I had long ago built great hopes on the action of nitric acid with respect to other substances, and it is with great pleasure I perceive, that Messrs. Fourcroy and Vauquelin have pursued my researches on indigo exposed to the action of nitric acid with more success than I obtained. I could only have wished, that Mr. A. Laugier had passed me over in silence in his abstract of the paper of those learned chemists, for my way of thinking in chemistry is totally different now from what it was eighteen years ago. When Mr. Laugier quoted me, he should not have forgotten that passage in my paper, which mentions the results of treating indigo with nitric acid, results that struck me so forcibly, as to induce me to recommend them to the attention of chemists. Neither had I omitted to mention the phenomena of the deflagration of the mixture, with the throwing of the glass rod out of the evaporating vessel. As a little time before I undertook these experiments I had extracted the benzoic acid

acid from its gum, I was too well acquainted with its smell, not to have distinguished it in purifying and drying the residuums of indigo treated with nitric acid, if my occupations as a manufacturer, which prevent me from gratifying my inclination for chemical experiments, had not proved an obstacle. Perhaps too I should not have missed the discovery of the detonating property of the bitter portion of the residuum: but it seems I was not born to make a figure in the career of discovery.

Arsenical alkaline solution of indigo.

With respect to the solution of indigo by means of an alkaline solution of red arsenic, which is used in calico printing, I no longer observe the proportions indicated in my memoir. I simply make a caustic alkaline solution of red arsenic, to which I add, while it is yet boiling, a sufficient quantity of brayed indigo, to obtain a very deep shade, which it is easy to render lighter afterward, according to the object proposed, by diluting the solution of indigo with a weak lixivium of caustic potash. This is preferable to pure water, because it retards in some measure the absorption of oxigen from the atmosphere, and consequently the regeneration of the indigo. The beauty of the blue in the calicoes requires, that this regeneration should be neither too slow nor too speedy. The too slow absorption arising from too great excess of caustic alkali ought to be avoided in pencilling blues, as well as in the blues in block-printing, which are procured by passing the goods, first printed with brayed indigo mixed with a gummy solution of sulphate of iron, alternately through vats of caustic potash, water, sulphate of iron at a minimum of oxidation, and lastly a vat acidulated by sulphuric or muriatic acid.

Cautions respecting its application.

Indigo and muriate of tin.

On exposing to a sand-heat a mixture of brayed indigo with a muriatic solution of tin oxided at a minimum, in which there is an excess of acid, the colouring substance is decomposed, occasioning the evolution of a gas of an insupportable and noxious smell, which deserves to be examined.

Sulphate of indigo and muriate of tin.

If indigo treated with the muriatic solution of tin oxided at a minimum, without the assistance of a caustic alkali, cannot be of any use in dyeing, it is not the same with

sulphate

sulphate of indigo, treated or mixed in different proportions with the same solution of tin, after having previously absorbed sulphuric acid. This is employed in the manufacture of printed goods for producing all sorts of blues and greens.

III.

Observations on the Distilled Water of common Borage; by PHILIP ANTONY STEINACHER, *Member of the Pharmaceutic Society of Paris* *.

Borage water very carefully distilled.

HITHERTO no particular property had been observed in borage water, except its depositing mucous filaments after being kept some time. Some that I distilled on the 7th of June, 1806, exhibited the following remarkable properties. The borage was very fresh, succulent, and immediately after being very finely shred was put into the body of a tinned copper alembic. Two parts of distilled water were poured on it, which moistened it sufficiently. The head of the still was put on, and a receiver adapted to it, both of which were previously rinsed clean with distilled water. The distillation was commenced immediately with a heat so gentle, that 20 or 30 seconds intervened between the fall of the successive drops. Only half a part of water was drawn off, which was limpid and colourless, and smelt and tasted strongly of borage, at the same time having another smell resembling that of a cucumber.

Reddened infusion of litmus.

This water neither reddened litmus paper, nor turned green paper tinged with an infusion of red roses; but it perceptibly reddened an aqueous infusion of litmus, which had been diluted with distilled water so as to appear of a pure blue.

Precipitated lime.

It rendered lime-water turbid instantly.

With sulphuric acid effervesced.

A few drops of pure rectified sulphuric acid, distilled almost to dryness, and diluted with distilled water, produced after the expiration of a few minutes a disengagement of some

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* Annales de Chimie, Vol. LX, p. 83, October, 1806.

some very small bubbles, without emitting any nitrous, muriatic, or acetous smell, and without disturbing its transparency.

Action of other tests.

It instantly whitened the aqueous solutions of oxalate of ammonia, muriate of barytes, nitrate of lead, and sulphate of silver *.

Oxigenized muriate of mercury converted by it into mild muriate.

The oxigenized muriate of mercury, purified by slow sublimation, produced in it a copious white precipitate. At the expiration of half an hour I added lime-water in excess, which increased the quantity of the precipitate, and did not turn it yellow even in twenty-four hours; which it would infallibly have done, if the oxigenized muriate of mercury had not been converted into muriate at a minimum.

With acid sulphate of mercury gave an opal precipitate.

Finally, having mixed with it some acid sulphate of mercury in a liquid state, made by dissolving the yellow sulphate in sulphuric acid, the addition of caustic potash purified by alcohol separated from it in a quarter of an hour flocks of an opal colour; while the same alkali, added to the same sulphate of mercury without borage water, immediately formed in it yellow flocks †.

Principles contained in it.

Hence it follows, that my borage water, distilled with so much care, and by a heat so gentle, contained carbonic acid, sulphate of lime, and a disoxiding principle.

Sulphate of silver decomposed by that of lime.

* Sulphate of silver is decomposed by the action of sulphate of lime. The following experiment is a direct proof of this. I took some very limpid lime-water, and added a few small drops of pure sulphuric acid. The solution remained clear, and had an excess of acid. To this I added a little of my acid sulphate of silver, which immediately occasioned a flocculent precipitate. After this had been washed, it was not soluble in muriatic acid. This fact proves, that the oxide of silver enjoys a very considerable power of cohesion, and renders me very circumspect in forming a judgment of the experiments for deciding the presence of muriatic acid in delicate fluids from the single phenomenon of precipitation by means of any solution of silver, and without examining the other circumstances, that might occasion the insolubility of the oxide of silver.

Forms an insoluble oxide, without muriatic acid.

Erratum in Fourcroy's Chemistry.

† According to Fourcroy's Chemistry, the sulphate of mercury with excess of oxide is precipitated gray by the alkalis; but this must be an errour of the press, our illustrious professor having before shown, that this property belonged to the neutral sulphate of mercury, which he had discovered.

I must

Some borage produced less.

Other distilled waters contain the same principle.

Wild lettuce water.

Hence their use in dyeing, and in some diseases.

I must observe, that several parcels of borage, gathered on different soils, and not so fresh, did not exhibit the same phenomena in an equal degree. As to this disoxiding principle, which has a sensible effect on mercurial solutions, I have found it in several other distilled waters, particularly in the water of silver weed, *potentilla anserina,* and strong scented lettuce, *lactuca virosa.* The water of the last-mentioned plant holds in solution besides a fetid volatile oil, which is rendered visible by adding rectified alcohol at 37°. It is no wonder therefore, that these distilled waters are capable of producing some effect in the art of dyeing*: they must be of use likewise in some sthenic diseases.

IV.

A Memoir on Acetic Acid; by Mr. J. B. TROMMSDORFF †.

Nitrogen said to be one of the principles of acetic acid by Proust.

THE object of Mr. Trommsdorff was, to know whether azote make a part of the acetic acid, as Proust asserted. Having considered what is at present known respecting the composition of ammonia, and of vegetable acids, he was justly surprised to find in Mr. Proust's paper, that he had found ammonia and prussic acid in decomposing acetates. Accordingly, notwithstanding the known accuracy and sagacity of the chemist of Madrid, he was desirous of satisfying himself of the existence of azote in concentrated acetic acid. The importance of the fact, and a love of truth, led this indefatigable chemist to make a similar research.

Before relating the processes Mr. Trommsdorff employed, it may be proper to give a succinct statement of the objections, that occurred to him.

Objections.

If, says he, in the distillation of acetates ammonia be formed, it is evident that they contain azote; but whence

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Distilled waters act on silk

* On consulting the anecdotes that Mr. Deyeux has published on distilled waters, in No. 168 of the Annales de Chimie, it appears, that he found the distilled water of silver-weed had a decided action on the silks that he used to make gauze.

† Annales de Chimie, Vol. LVIII, p. 190, May, 1806.—Abridged from the Berlin Journal by Mr. Bergman.

can this principle be derived? Is it from the base? But this cannot be, since ammonia was equally obtained from acetate of lead. In this case it could be furnished only by the acetic acid; or it must be allowed, that azote is only a modification of hidrogen.

Does nitrogen occur in vegetable acids?

He then inquires whether azote occur as frequently among vegetable acids, as among animal acids: because, if it be so, their classification should be altered.

Very pure substances employed.

In repeating the experiments of Mr. Proust, it appeared essential to Mr. Trommsdorff, to employ only very pure substances. Accordingly, in order to have acetic acid in the purest state possible, he decomposed the acetate of potash by sulphuric acid; he then saturated this acid with carbonate of soda well purified, and evaporated the saline solution in a silver basin. The salt obtained, which he put into a bottle with a ground stopper, was extremely white.

In preparing the acetates of potash and of lead he employed similar precautions.

These distilled.

He took eight ounces of each of these salts, and introduced them separately into three strong glass retorts. These retorts were placed on the open fire of a furnace, and to each was adapted a receiver, from which issued a glass tube, terminating under a jar for receiving the gasses that should come over.

The products.

The products were, as every body knows, an acidulous ethereous fluid mixed with oil. The alkali and carbone remained in the retort; and in the decomposition of the acetate of lead nothing of this salt remained but the lead oxided.

The alkalis promoted the decomposition of the acid.

Thus by the predisposing affinity of the alkalis for carbonic acid, these determined the decomposition of the acetic acid, to give rise to the formation of carbonic acid. The oxide of lead on the contrary, not having so great an affinity for carbonic acid, gave out the acetic acid in its greatest purity.

How does the metal act?

The author asks, whether the metallic base yielded up oxigen to burn the carbone; or whether the attraction between an oxide and an acid be less powerful than between an acid and an alkali. To answer this question, he would wish a great number of experiments to be made.

The

The gasses on examination emitted no ammoniacal smell, it being merely empyreumatic and penetrating. The liquids had the same smell, and none of the chemical tests could detect the presence of ammonia in them.

No ammonia in the gasses or liquids,

The residuums, which according to Mr. Proust contained prussiate, were nothing but pure alkaline carbonate; or pure oxide of lead.

or in the residuums.

Mr. Proust, on examining the residuum of acetate of potash, says, that he had a residuum consisting in part of prussiate, in part of carbonate of potash. Mr. Trommsdorff expected to find these two salts; but, after having broken the retort, he found only a homogeneous coal, which afforded him neither ammonia nor prussic acid, and which had no smell of either of these substances. Yet we know how easy it is to distinguish the smell of this acid wherever it exists in a free state.

From the acetate of potash Proust obtained prussiate and carbonate.

Mr. Proust adds, that the residuum of the acetate of potash was so saturated with prussic acid, that its bitterness was as striking as if the acid had been combined directly with the alkali; whence Mr. Trommsdorff infers, that he must have employed common vinegar in his experiments.

The prussic acid in the residuum very apparent.

To distinguish the products resulting from the decomposition of acetic acid, Mr. Trommsdorff passed the vapour of it through a red hot tube, which afforded him nothing but carbonic acid gas, carburetted hidrogen gas, and a small quantity of an empyreumatic liquor, without ammonia, and without prussic acid. These substances were equally absent in the residuums.

Vapour of acetic acid passed through a hot tube.

He afterwards examined attentively the ethereous acidulous fluids mingled with oil. These he distilled over carbonate of potash, and obtained an ether, which, from all its properties, appeared to be true acetic ether.

Acetic ether obtained by rectifying the liquors.

The results of his experiments are:

General conclusions.

1. That the presence of azote in acetic acid is not proved.

2. That pure acetates, when distilled, give out neither ammonia nor prussic acid.

3. That pure acetic acid has its nature very little altered by passing through red hot [glass] tubes.

4. That, in an iron tube, it is completely decomposed into carbonic acid gas, and carburetted hidrogen gas.

5. That

Constituent principles.

5. That the constituent parts of acetic acid are demonstrated to be oxigen, carbone, and hidrogen.

Acetic ether.

6. That the ethereous fluid is similar to others in its general properties. The author considers it as a medium between alcohol and ether. That as acetic acid is changed in part into ether, and this, when treated by nitric acid, is transformed into oxalic acid: the conversion of acetic acid into oxalic is demonstrated, though it is true indirectly. And lastly, that it is probable Mr. Proust did not employ pure acetates in his experiments; or else the ethereous and very penetrating smell led him to believe, that ammonia was present.

Acetic acid convertible into oxalic.

V.

Account of an Engine for splitting Sheep Skins: by Mr. BENJAMIN STOTT, *of Bermondsey Street**.

Advantages of an engine for splitting skins.

I HAVE invented an engine for the purpose of splitting sheep skins, that is, of making two good skins out of one. The former and common mode of dressing skins is, to shave one side off, reserving the shavings for glue pieces; whereas by my method, these shavings are all taken off in one piece, forming a good skin of leather; and thus, independently of the advantage arising to the proprietor, an additional revenue will be caused to the nation, in proportion to the increase of leather made.

Description of the engine.

Pl. IX, Fig. 1. A, the barrel of cast iron (having wooden ends) round which barrel the skin is wrapped, and kept close by means of pins run through the edges into the wood, as at *e*, *e*, Fig. 2. B, (Fig. 1) an iron running in a groove along the barrel, catching in a hole at *c*, and fastened down at the other end by a hook fixed in the end of the barrel, the bar having points in it (as shown at B, Fig. 3,) under which the edges of the skin are fastened (as seen at D, Fig. 2). F, F, (Fig. 1 and 2) bars fixed across each end of the strong wooden frame G, G, G, G, over which the barrel is supported on friction-rollers, as at *h*, *h*, (Fig. 2) which

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* Transactions of the Society of Arts, vol. xxiv, p. 133. The Society voted Mr. Stott twenty guineas for this invention.

Mr. Stott's Machine for Splitting Skins.

Fig. 3.

Fig. 1.

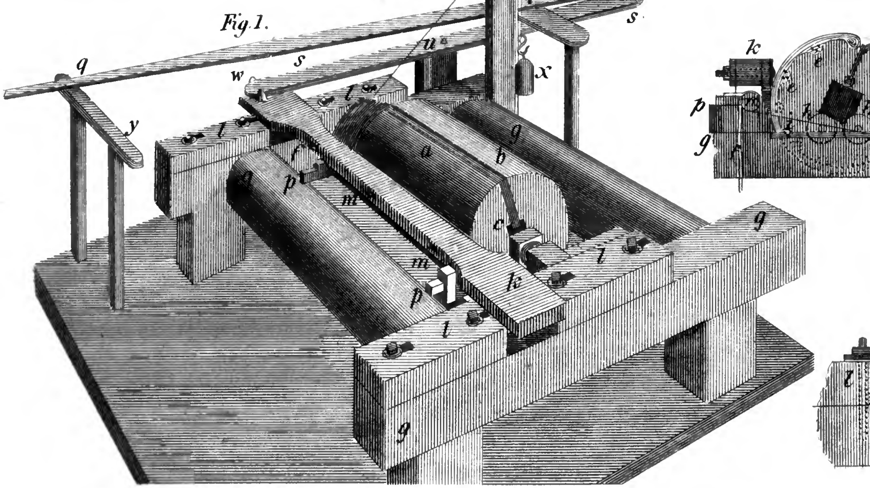


Fig. 2.

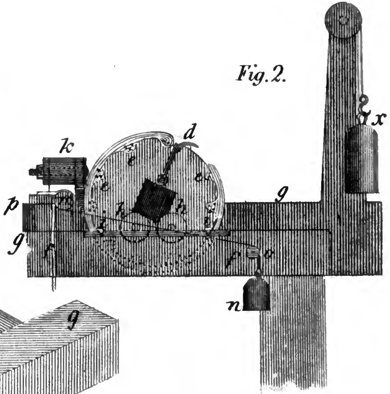
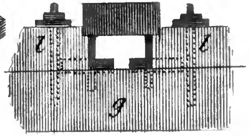


Fig. 4.



run on a slip of brass, moveable under the screws *i*, *i*, to adjust the barrel to the knife. K, K, (Fig. 1 and 2) a strong bar of cast iron, to which the knife is screwed, moving lengthwise on friction-rollers between the pieces of wood L, L, L, L, on the frame G, as at K, (Fig. 4). The pieces of wood L, L, L, L, are each moveable under two screws, by which they are adjusted to steady the motion of the knife-bar. M, M, (Fig. 1 and 2) is a roller at the back of the knife, to which it is kept close by a weight N, at each end, acting over pulleys, as at O, (Fig. 2) suspended from the slider *p*, between which the roller is placed; by drawing the spare skin over this roller, as it is cut off, it keeps both sides of the skin equally up to the knife, and makes it cut more uniform. Q, Q, is a lever acting on a pin *r*, and moving another lever S, S, by means of a pin and a notch *t*, which acts on another pin at *u*; and by means of the two pins at W it moves the knife lengthwise to and fro: as fast as the skin is cut the barrel is drawn round by the weight X. *y* is a guide to the lever, from which end it is worked.

VI.

A Memoir on Sulphuric Acid; by Mr. KLAPROTH: *read at the Philomathic Society of Berlin* *.

Proportions of the elements of sulphuric acid according to various authors.

THE object of the author was to ascertain the respective quantities of the elements of sulphuric acid and of sulphate of barytes, and he mentions the analyses that have been made of these substances by the chemists whose names are subjoined:

Sulphuric acid consists,	Sulphur.	Oxigen.
according to Lavoisier, of	69	31
Berthollet	72	28
Thenard	55·56	44·44
Chenevix	51·5	38·5
Trommsdorff	70	30
Richter	42·05	57·95
Bucholz	42·5	57·5

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[*] Abridged from the German by Mr. Bergman. *Annales de Chimie*, Vol. LVIII, p. 122, May, 1806.

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The last two, though calculated in different ways, come nearest to each other, and therefore deserve most confidence. But Mr. Klaproth conceived it necessary to satisfy himself by his own experiments of the respective quantities of the elements of sulphuric acid, that he might afterward apply the results with more certainty to the analysis of pyrites or metallic sulphurets. For this purpose he employed, as other chemists had done, nitric acid and carbonate of barytes; the elements of this salt having previously been ascertained by him to be barytes ·78, carbonic acid ·22.

Proportion of the elements of sulphate of barytes.

Mr. Bucholz, however having since asserted, that this salt consists of ·79 barytes, and ·21 carbonic acid, Mr. Klaproth repeated his analysis with all possible care, and still obtained the same proportions as before. We may here add, that Mr. Bucholz admits only ·25 of acid in the carbonate of strontian, while Mr. Klaproth has found ·30 in all the analyses he has made of this substance.

The results of the analysis of the sulphate of barytes made by various chemists do not differ less than the proportions they have assigned to the constituent principles of sulphuric acid. It is composed,

Given differently by different authors.

	Barytes.	Sulphuric acid.
According to Fourcroy, of -	66	34
Clément and Desormes	67·82	32·18
Thenard -	74·82	25·18
Chenevix -	76·5	23·5
Kirwan - -	67	33
Richter -	69	31
Bucholz - -	67	33

Kirwan's preferred by Klaproth.

If indeed we except the analyses of Chenevix and Thenard, the rest do not vary greatly from each other; and if we take a mean of these, Kirwan's comes nearest to it, which has induced Mr. Klaproth to adopt it as the most accurate.

Process for deciding the question.

Proceeding on these data, the following was the method adopted by Mr. Klaproth. He introduced 200 grains of pure sulphur, and eight ounces of pure nitric acid, of the specific gravity of 1·32, into a retort, and distilled till about three fourths had passed over into the receiver. The product of this distillation was returned into the retort, and distilled a second time. Eight ounces more of acid were then added, and the whole distilled again.

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The unburned sulphur was found to weigh 48 grains and half; consequently 151½ were converted into sulphuric acid. The product diluted in a certain quantity of water was mingled with muriate of barytes, till no more precipitate was produced. The sulphate of barytes, well washed and dried, weighed 1109 grains; but, calcined in a platina crucible, its weight was reduced to 1082 grains.

To find the proportions of the constituent principles of concrete sulphuric acid, Mr. Klaproth took a hundred grains of highly concentrated sulphuric acid, the specific gravity of which was 1·85: this he diluted with fifteen parts of water, and added to it muriate of barytes, till no more precipitate was formed. The sulphate of barytes, carefully washed and dried, weighed 225 grains. Hence it follows: 1st, that 100 parts of sulphuric acid of the specific gravity of 1·85 are composed of

Proportions of highly concentrated sulphuric acid:

Concrete sulphuric acid	74·4	or,	sulphur	31·5
Water - -	25·6		oxigen	42·9
			water	25·6
	100			

2dly, That 100 parts of concrete acid are formed of sulphur 42·3, oxigen 57·7.

of concrete acid:

3dly, That 100 parts of calcined sulphate of barytes contain barytes 67, sulphur 14, oxigen 19.

and of sulphate of barytes.

SCIENTIFIC NEWS, &c.

A Classification of Vegetables, and Plan of a new Method formed on that of Tournefort, according to which the Plants of the Garden of the private School of Pharmacy at Paris are arranged: by D. L. GUYART, *Assistant Professor of Botany at the School, &c.*

IF, among the different botanical methods, that of Tournefort has always been considered as the most easy, and best calculated to guide the first steps of those who would study plants; it must also be confessed, that it is insufficient, when we endeavour to obtain an accurate idea of vegetable organization. For this reason, no doubt, the methods of Linneus and Juspieu are at present preferred, and almost universally adopted by botanists.

Tournefort's classification of plants.

Improvement of his method.

It is of importance, however, that Tournefort's should not be lost, as well on account of the celebrity of its author, as for the utility of which it may still prove to young students. By these motives Mr. Guyart has been induced to compose a new classification of vegetables, founded on the method of Tournefort; but in which, availing himself of the progress subsequently made in the science of botany, he has formed his classes from more striking and constant characters than those adopted by Tournefort. Thus he has given fresh youth to the method of that botanist, and rendered it more natural.

M. Guyart's arrangement.

Tournefort's new method, as proposed by Mr. Guyart, consists of sixteen classes. The first eight are formed of plants with complete simple flowers. The first containing the monopetalous: the second, the personate: the third, the labiate: the fourth, the cruciform: the fifth, the rosaceous: the sixth, the umbelliferous: the seventh, the caryophyllaceous: the eighth, the leguminous. The next three include the plants with complete compound flowers, with united anthers: the semifloscular, the floscular, and the radiate. The four following are appropriated to the distinct incomplete flowers: the apetalous, the amentaceous, the glumaceous, and the liliaceous. The sixteenth and last is assigned to the anomalous plants, or those with indistinct incomplete flowers.

Useful to beginners and those who wish for a general knowledge merely.

This classification, as the author observes, is not free from defects; but, notwithstanding its imperfections, almost unavoidable, perhaps, in such an undertaking, in the opinion of some botanists of celebrity, whom he has consulted, it will much facilitate the study to beginners, and is still better adapted to those, who, not having time to cultivate the science to its full extent, require only an acquaintance with its elements.

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Winterl,

END OF THE EIGHTEENTH VOLUME.

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