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THE ARTS.

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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in this Volume, are J. A. De Luc, Esq. F. R. S.; Mr. Thomas Reid, Edinburgh; Luke Howard, Esq.; Alfio Ferrara, M. D.; William Hamilton, Esq.; Dr. Bostock; Dr. Marcet, F. R. S.; Mr. J. Singer; Messrs. Kerby and Merrick; Mrs. A. Ibbetson; R. B.; Mr. R. Porret, jun.; W. N.; Dr. Tuthill, F. R. S.; Dr. Pearson, F. R. S. &c.; John Trotter, Esq.; L. O. C.; Mr. F. Accum; Mr. Charles Sylvester; Lord Gray.

Of foreign Works, M. M. Hassenfratz; La Place; Tollard Lelieur; Le Bouvier des Mortiers; M. H. Klaproth; D'Arcet Dufaud; Vogel; Bouillon La Grange; Gay Lussac; F. Peron; Thouin; Malus; Goering; Larrey; Marcel de Serres.

And of British Memoirs abridged or extracted, Mr. J. Allan; John Davy, Esq.; H. B. Way, Esq.; Rt. Hon. Sir Joseph Banks, Bart. P. R. S. &c.; W. T. Brande, Esq.; W. H. Wollaston, M. D.; P. C. Boadie, Esq.; F. R. S.; T. A. Knight, Esq. F. R. S. Pres. Hort. Soc.; Don Joseph

Rodriguez; Dr. Wm. Roxburgh; Sir H. Davy, F. R. S. &c.; Hon. H. Grey Bennet, Esq.; Mr. Tollard, Sen.

The Engravings consist of: 1. Sections of Plants to shew the manner of the Buds in the Stalks, by Mrs. Agnes Ibbetson. 2. The secret and open Nectaries of Flowers, by the same. 3. An Apparatus by Messrs. Kerby and Merrick, for measuring the sonoriferous vibrations of the Gases. 4. A Compensation Pendulum, by R. B. 5. An economical Lamp. 6. New disposition of Musical Keys, by J. Trotter, Esq. 7. The Growth and Increase of Trees, by Mrs. Ibbetson. 8. An Apparatus for manufacturing Prussian blue. 9. Designs to illustrate the Structure of the Roots of Trees, by Mrs. Ibbetson.

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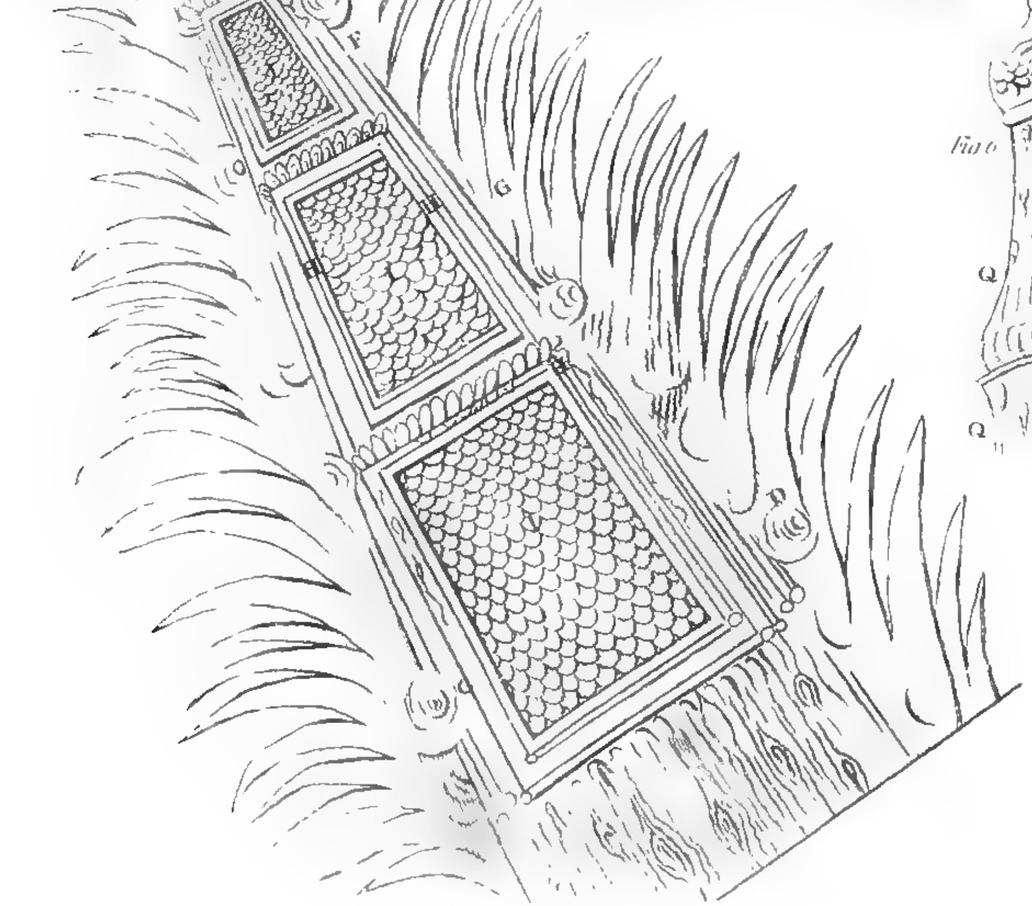
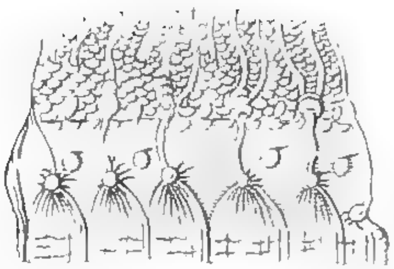


Fig 3



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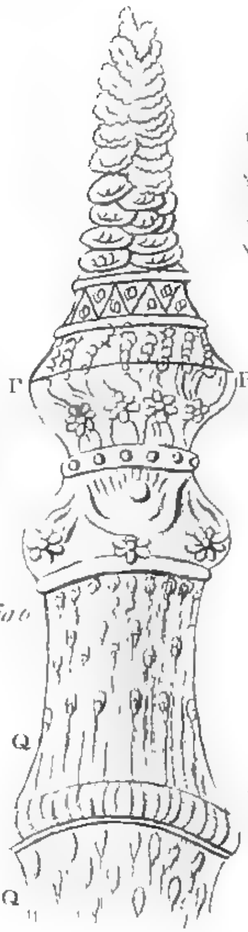
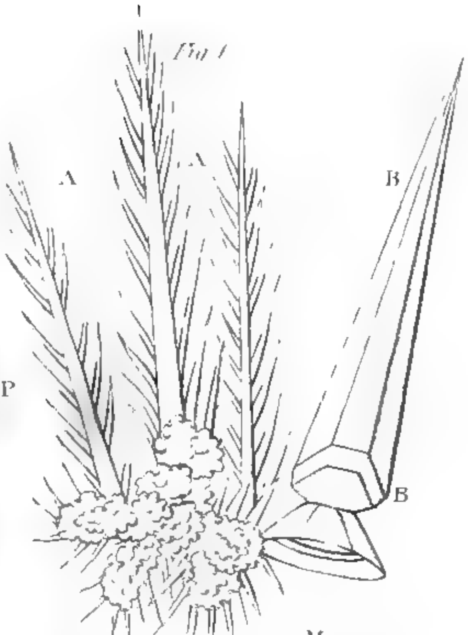


Fig 4



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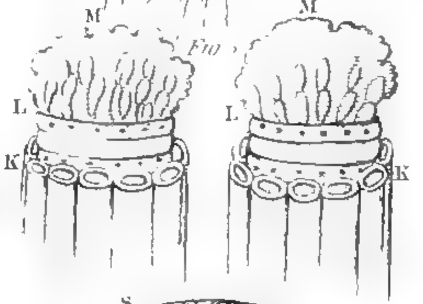

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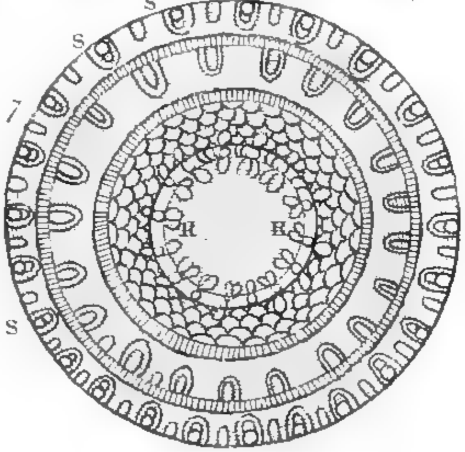

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SEPTEMBER, 1812.

ARTICLE I.

On the Interior Buds of all Plants. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON,

SIR,

HAVING shown in my last letter how the various parts of a stem finish in a flower, and I think proved, in the most absolute manner, that each part of a flower is formed by an appropriate part of the stem, peculiarly or separately dedicated to its formation, I shall now turn to the middle of a plant, and give as complete a picture as I have been able to discover of that part, with the various changes produced in it by the manner in which the buds run up the interior of the stem, in all plants whatever, that shoot their bud each year from the root; and the stalks of which grow yearly from the ground; whether perennial or biennial, whether dying down or fresh sown. The plants which are the subject of the present letter will embrace an amazing class; for after the strictest search, and most exact dissections, I can discover only five sorts or divisions in nature, comprising many classes and orders, and which, from their consequence and

Plants that rise from the earth yearly.

and paucity of numbers, may perhaps well deserve to form the foundation of a natural method, and open to us that which nature herself designed as the commencement of such a plan. I shall first give the five different manners of shooting of the bud, and then enter into farther details concerning this important subject.

The bud shoots in five different modes, forming so many natural divisions of plants.

1st. All those plants which shoot their bud from the nearest line of life, whether in branch or twig: as trees, shrubs, and semishrubs.

2d. All those plants that rise from the earth each year, having a new stem, let their real existence be long or short, and that shoot their bud from the root.

3d. The plants that have no flower stem, but that have in its stead a rallying point, which is immediately discovered by a band or knot; from which the flower buds proceed, and which is found only in grains and grasses.

4th. Those plants which have no regular flower stem, but which are divided from the last by shooting a few partial vessels, with the line of life, just before flowering, enclosing the flower buds: but which are all concealed together within the cuticle of the leaf: as in the palms, arums, and all plants having grass leaves, without bands or bulbs.

5th. All plants that shoot their buds from a bulb.

These five collections of plants are all I can gather from the most exact examination and dissection of British as well as exotic plants; and it appears to me to lay open that view to the discovery of the system of nature I have so long and so ardently sought. But this subject I shall enter into more fully when better prepared to give satisfaction to the public; at present I shall confine myself to the shooting of the bud in the stem of plants.

Manner of shooting the bud in trees, &c.

Of the 1st example, or manner in which buds shoot in trees, shrubs, and semishrubs, I have already given many descriptions: it is as beautiful a process as nature presents: that so soft, so tender a being, should pass through so hard a substance unhurt, that by the moisture of the pith (retained for the purpose) the wood should be separated into collections of vessels, and made to bend both ways, so as to form a covered way for the bud, that it may pass in the midst, unpressed and unconfined, is a conception that the view of the

specimen

specimen alone could prove the truth of; but it is so easily seen, that it requires only to strip the bark from a branch of any tree, and plenty of buds will be found just shooting from the interior, making their way through the hard substance. It can never be mistaken by a careful observer for that harsh and diminutive piece of wood, which, when the bark is taken off, appears as passing to each leaf; for this is hard, but the buds are always found at the end, very soft and succulent, and covered with albumen.

Shooting of the bud in plants that rise yearly from the earth.

To the 2d example I shall now turn, namely, that in which the buds shoot each year from the root, and the stem is but annual, let the root be older or not. This for a long time puzzled me beyond measure, and few will conceive the labour the discovery has cost me, and the quantity of herbaceous and annual plants I have dissected before I could perceive the whole truth. Perhaps it even exceeds in beauty and contrivance the shooting of the buds in trees. I had long been convinced, that the bud shot from the root, but except in those plants where it runs across the pith, and where I had traced it occasionally up the wood vessels, I could not discover what became of the buds, after they had disappeared at the beginning of the stem, till I found them again in the axilla of the leaves. I shall now take a pentandria digynia plant, and show the whole process of its growth.

I have already said, that the bud is formed in the interior of the root of annual plants, or such as die down to the ground every year; and shall now show how it continues its way in those plants that cross the pith, and then proceed to the buds that do not cross it. The best way of dissecting for both these purposes, is to take a long succession of plants, each a few days or a week older than the preceding. The alteration this little time produces in the interior is amazing. Taking a very young heracleum spondylium; the preparation for forming these immense leaves are all that appears at first in the plant; and this is all confined to the bark only, which it enlarges. The leaves differ in some measure in their manner of forming from the leaves of trees and shrubs, though they are equally woven: no part is more indebted to those occasional hairs (mentioned in a former letter) than the

Formation of the heracleum spondylium.

the leaves of this plant. As soon as the midrib of the leaf is completed, and the quantity of vessels for weaving the first row of the cross work of the leaf is finished; it is all rolled together into a spike, and this spike is surrounded by immense hairs, as shown at AA, fig. I, Pl. I. (B B represents one of the hairs much magnified). These soon draw plenty of moisture to mix with the juices to form the pabulum of the leaf. The hairs then disappear, the part unrolls, and the leaf begins to weave itself, as at C D, fig. 2. All this time the root is plain and simple, and the pith of the stem (though frequently crossed by the line of life) showing nothing beside, but its own original figure; and though but a small part of the stem is yet formed, it is employed with all its forces, and juices, in rolling and unrolling the leaf. But no sooner is the weaving of the leaf finished, than the shooting of the bud in the root begins; the knots are soon formed on the line of life, within the centre of the root; the ends break, and two buds shoot from each knot; they pass through part of the wood in the root, and then disappear; for each row of bud has its appropriate wood vessels, up which it then passes. It should seem, that all the pentandria digynia plants appear to have too much flower for the wood vessels to contain, nature therefore has recourse to an expedient of a very curious kind: the pith is divided into compartments by the line of life; and at each compartment the buds are pressed out of the vessel, and ranged across the plant. All the pentandria digynia tribe are umbelliferous, and shoot at different times (but at very short intervals) small collections of flowers. It will be seen therefore, that nature already prepares them for the purpose; dividing them in the stalk as they are to shoot. With what art, what exquisite beauty, nature has managed to keep the pith still in the middle of the stem, in order to retain that moisture necessary to the shooting such a quantity of buds; and yet contrived to secure plenty of room for those buds to spread, and come to perfection: dividing the stalk in umbels as they are afterward to shoot into flowers! See fig. 2, E, F, G, the different divisions; H the line of life: I the pith. When the buds were in the root, they were scattered in a careless manner, and moving in the same direction as they do in the

Buds pass through the wood vessels.

wood of trees, that is horizontally. The wood therefore made way for them. But no sooner do they begin to run up the stem, than the wood vessels instead of forming a covered way for them, opens and receives them within their apertures. A picture of one of the vessels extremely magnified will give an idea of their formation, see fig. 3: K K are the vessels through which passes the sap; L the interior vessels containing the buds; M the albumen, which always appears above and round the buds. All the buds are tied together as in seeds, see fig. 4. The sap vessels, though forming only a part of the new cylinder, are much larger than in trees. When the buds rise to the top of the stem or axilla of the leaf, where they are to flower, several of the squares join together, and form one sort of flower bud; which afterward divides into umbels.

Buds distinguished from the wood of the leaves.

But all the plants that shoot their buds from the root, and have flower stems, have not their buds crossing the stem in this manner. In the greatest number of herbaceous plants, the buds runs up the interior of the wood vessels to the place where they are to show themselves; that is, to the top of the plant, or the axilla of the leaf: and are not seen till they get there. Yet take a pretty thick cutting of any of these plants, and keep it a few hours, and you will see the buds growing out of the vessels exactly as the buds of silver or lead grow in the arbor dianæ, or saturni under your eye. It was by such a piece of the plant that I discovered how the buds crept up the interior of the wood vessel, and remained so long concealed.

All buds shooting from the root do not cross the stem in this manner.

I shall now turn to the third division, which embraces grain and grasses of every kind, that possess the peculiar band or knot, which is perfectly unlike every other contrivance in the vegetable world. It exceeds indeed any yet shown; but I fear that at every new proof of this exquisite performance, one must have neither soul nor feeling not to become an enthusiast on the subject, when contemplating such wonders; beholding such astonishing productions. As there is no stalk properly so called, the different parts of the plant are collected in the flat leaf of the grass. Thus the bark, the inner bark, the wood, and the line of life, are all possessed of stripes in the leaf. How then can the two sorts of buds

Bud, how formed in grain or grasses.

be protruded? by all the different parts meeting in a band, when a collection of each matter is selected to produce the circle of the leaf buds, and form a new leaf. Few operations can be more plain and easily understood than this part of the process; it is attended with an odd kind of contrivance, which shows that nature often makes use of the same means we do to effect the same purpose. When the part is to be selected to form the leaf bud, the rest is tied with a knot, lest it should tear down or unravel (see *o o* fig. 5). Then the vessel selected rises, and soon produces a bud, and when the whole row is completed they join together and form a new leaf under the other: this is repeated three times, but at the fourth knot, when the leaf is produced, it is formed round instead of flat, and a quantity of albumen is generated by the stopping of the sap. The line of life then strikes out of the edge of the leaf, and forms a broad circle in the interior of the band, which is always a forerunner of the bud; immediately knots appear on the line, they break, and the flower buds are seen shooting from the ends; their numbers soon fill the round leaf: the buds are all tied together by the line of life as in seeds, and remain in their enclosure till they are perfectiy ready to shoot out at the top of the plant in a spike of grassy flowers. Thus this band or rallying point not only serves to strenghthen the plant and support it, but gives a new way of forming the leaf buds, and of protruding the flower buds; and this is no work of imagination, as I shall now show. Fig. 5 is the part selected to form a new leaf bud. Fig. 6 is the first shooting of the flower bud; and though there is some little part of the mechanism I do not quite understand, still as far as I have described, what with watching and dissection, I am pretty certain of being right, and not misleading those who will venture to follow me. That the flower bud is merely the embryo of the plant, enclosed by a few seminal leaves, and is not covered by the meal till the flower rises as high as P, is a certain truth, since I have dissected them both before and after. That the flour of corn, or meal, is formed of the inner bark juices alone, I have the most positive proof; since it is only in the inner bark vessels it is to be found, even from the root: at each new band it grows

Formation of the bud in grain.

Use of the knot in grain and grass:

more

more like wheat, and when the fourth knot is perfected the meal is quite milky and sweet to the taste; and when the embryo is ready to be covered by it, as at P, it is the vessels P P that convey the meal to the embryo, when it is little more than very thick milk, which soon however hardens when spread on the embryo, and when the buds next appear they are covered by it, though before absolutely destitute of it. As in every other flower, each part is produced by its own appropriate matter: the male by the wood, the female by the line of life; the bark produces the scales, and the inner bark the meal. Thus all concur with other plants to show the truth of that fact. All grain as well as grasses are alike in their formation, they differ only in the quantity of meal with which their seeds are covered: there is however in the grasses some little difference in the mechanism, but not worth mentioning.

conveying the meal.

A little difference of mechanism in the grasses.

Though a digression I cannot help here giving a piece of information, which appears to me of no little consequence. We suppose that seed to be the finest for producing wheat-flour, which has on it the greatest quantity of meal. I have repeatedly tried the experiment, and two gentlemen have also essayed the same; to sow a part of a field with refuse wheat, provided the seed is perfect. The difference of product between this and the finest and largest seed that could be procured was not to be discovered. Provided the embryo is strong, the quantity of meal on it signifies little; for the best covered is certainly not the strongest producer. In dissecting wheat I have always found that seed with the largest star or hilum gave the greatest returns, and not the one most covered with meal. Much care should be taken to choose seed from a field where there has been no smut, no corn cryptogamia, and to prefer seed not taken from a thrashing machine, or lime and sand floor, for they all in some measure injure; the first two indeed to a great degree: but its being only thin of flour is far from being against it, on the contrary, the embryo is often the stouter for it. Nature keeping the embryo such a time without meal is surely a hint to us, and shows that it does not strengthen the plant.

Refuse wheat for sowing

would save us much flour.

I now turn to my fourth example evinced in the palms,

4th example.

arums, callæ, and dracontia. These have been arranged with grain and grasses; but they are extremely different in their formation. Though they have (like the grasses) no apparent stem; yet they send their buds from the root just before flowering; by enclosing them in wood vessels, the buds being tied together by the line of life; and this slender piece running up within the cuticle of the leaves unperceived till it strikes out of the axilla, or bosom of the leaf, in a bunch of flowers. But it is very easily detected by dissection, if sought; I have repeatedly taken it out of its place of concealment after watching its progress from the root. So careful has nature been to separate this collection from the leaf; that there is a double cuticle on the way up for the purpose; and I suppose to prevent the contamination of the juices.

The buds mounting in bulbous plants.

In the 5th example, that is in all bulbous roots, the flowers are known to be formed in the root, and to rise by degrees improving till they show themselves perfect flowers at the top of the plant, merely by the lengthening of the peduncle: but no person ever suspected, that this was a repetition of the case of all those plants which rise yearly from the earth; that they all equally draw their buds from the root, and pass it to the top, in the interior of one or many wood vessels, according to the sort or size of the flower. In the ranunculus tribe the flower is so complete, (even in the root) that I have taken it out, and dissected it, and proved that the seeds are already formed in it. The various parts of the flower of a bulb are all formed; it is only the proportions that are not observed in its earlier state. It is a curious truth, that certain parts, as the stamen, germe, and nectaries, are much larger when first forming, than they are when more thoroughly perfected. As to the offsets of a bulb they are merely a second sort of bud; with all the attendant parts the same; or another sort of seed; for the difference between these parts is very trifling, all equally proceeding from a knot in the line of life.

Decreasing of the stems in breadth.

I have thus given an account, as I promised, of the five divisions, which might well form the foundation of a natural method: as a more important point in physiology than the budding of plants could not well be found, or fixed on for the purpose. I shall now take notice of an observation, that was one of the first I made when examining herbaceous plants, and dissecting them; "The increase in breadth of "the stems of those plants at a certain time of their growth." It is now accounted for by the running up of the buds, as this must of course enlarge them: it however puzzled me not a little to comprehend, why all plants, that rose yearly from the earth, should decrease so much when coming out into flower; especially as they must at that time require more rather than less sap. But the discovery of the buds solves the whole mystery: the flower being within the plant takes nearly the same quantity for its support, and the increase of the stem falls wholly on the interior buds, which most naturally accounts for it. I shall now close this long letter with a few words on dissections in general.

On dissections in general.

Will it not appear by the specimen of the heracleum spondylium, fig. 2, how very difficult it is to dissect plants, and how necessary it is to take them so as to become acquainted with their continual variations, which are still greater within (if possible) than at the exterior? how impossible that a person, who only dissects a plant a few times, should understand it? I may say without the smallest exaggeration, that several persons might give a picture of the dissection of the same part of a plant; and each might accuse his neighbour of giving a false delineation, and yet each drawing might be just and true; but taken at various seasons, or different ages of the plant. Mirbel has given many excellent specimens much magnified; but in some not keeping to the right cylinder his pipes do not join, and appear therefore placed for nothing. Nature acts not in this manner: all is consistent and useful; and that use most apparent and easy to be understood, if the plant is properly cut. To dissect a plant rightly, so as to produce specimens that will truly explain the nature and habit of a plant, is no easy thing; for it requires the most perfect knowledge of its different parts; which is only to be acquired by long study and constant dissection. The 1st point requisite is to be attentive to keep to the right cylinder, particularly if you mean to halve it; as at fig. 2, for if you do not divide it properly, you will have the back of the bud in one square; the front in another;

another; and no bud in a third. In short, instead of that regularity of figure with the line of life leading up to each bud as in fig. 2, all will be confusion and disorder. I never divide a plant without first marking it with an instrument. I use almost as many different sorts as a surgeon: to separate the cylinders requires a very sharp and cutting instrument, and I am at last driven to the necessity of inventing and contriving my own.

On the instruments used for the purpose.

I noticed above the having frequently found instances, where nature makes use of the same means we should ourselves have had recourse to in the same predicament: and some have expressed surprise at it. But why should we be astonished? whence proceed the ideas of man, but from the suggestions of that Creator whose works we are studying? Is it strange then, that we should find them alike; when proceeding from the same source? The works of nature are certainly infinitely more perfect; and if we studied them with stricter attention, ours would borrow more of that beautiful simplicity, which so admirably distinguishes them. Still both proceed from God alone; though our notions and ideas are so contaminated with the feebleness of man's nature.

I am, Sir,
Your obliged servant,
AGNES IBBETSON.

Fig. 6, the first passing up of the flower buds, or rather the embryo, as Q, to form the flower at PP.

Fig. 7 a horizontal section of the wheat in the middle of the fourth knot or band; where the embryo is generated on the line of life, throwing out two buds at each end; see RR. SS are the bark vessels which form the meal.

II.

An Account of some Experiments on the Combinations of different Metals and Chlorine, &c. By John Davy, *Esq. Communicated by Sir* Humphry Davy, *Knt., LL. D.; Sec. R. S*[*].

Introduction.

Combinations of oximuriatic acid similar to oxides.

MY brother, Sir Humphry Davy, appears to me to have demonstrated, in his last Bakerian Lecture, the existence of a class

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[*] Philos. Trans. for 1812, p. 169.

a class of bodies similar to metallic oxides, and consisting of metals in union with chlorine or oximuriatic acid.

These combinations are the principal subject of the following pages. I shall do myself the honour of giving an account of the experiments I have made to ascertain the proportions of their constituent parts, and likewise of describing some that have not yet been noticed.

Doctrine of definite proportions.

I shall have to relate also the attempts I have made to ascertain the proportions of sulphur in several sulphurets, and the experiments I have performed to estimate the quantity of oxigen in some metallic oxides. The general analogy of definite proportions led me to both these undertakings. This analogy, it will be perceived, I have constantly kept in view, and have had recourse to, both for detecting inaccuracies in my own experiments, and in considering the results of the experiments of others.

New nomenclature.

As the nomenclature connected with the old hypothesis respecting oximuriatic acid is inconsistent with the new views of this substance, I shall venture to call the compounds of the metals and chlorine to be treated of, by the names which my brother has proposed for them.

1. *On the Combinations of Chlorine and Copper, &c.*

Oximuriatic gas combines with copper in two proportions.

There are two distinct combinations of chlorine and copper, both of which may be directly made by the combustion of this metal in chlorine gas. When the gas was admitted into an exhausted retort containing copper filings, the filings became ignited, a fixed fusible substance quickly formed, and the interior of the retort soon became lined with a fine yellowish brown sublimate. The former substance evidently contains least chlorine, for when it was heated alone in chlorine gas, it absorbed an additional portion, and was converted into the latter. Hence the fixed compound may, in conformity with the principles of Sir Humphry Davy's nomenclature, be called cuprane, and the yellow sublimate, cupranea.

1st compound: Boyle's resin of copper.

Cuprane may be procured in several other ways. It may be obtained by heating together copper filings and corrosive sublimate; and it was thus first discovered by Boyle, who called it resin of copper, from its similitude to common resin.

sin. Two parts of corrosive sublimate, and one part of copper filings, I have found the best proportions of the materials.

Another mode of obtaining it.

It may be obtained by boiling copper filings in muriatic acid, or by exposing slips of copper partially immersed in this acid to the atmosphere. In the last instance, I have found the changes connected with the formation of cuprane rather complicated; the copper exposed receives oxigen from the atmosphere, and acid from the ascending muriatic acid fumes, and is thus converted into a green insoluble salt, and this, absorbing more muriatic acid, slowly passes into the deliquescent muriate, which flowing into the muriatic acid is changed by the action of the immersed copper into cuprane.

Proust's white muriate of copper.

Mr. Proust, the first modern chemist who examined cuprane, and who is commonly considered as the first discoverer of this compound, found it produced by the action of muriate of tin on muriate of copper; he named it white muriate of copper, and ascertained that a similar substance results from the decomposition of the common deliquescent muriate by heat.

Properties of this compound.

Cuprane, by whatever means prepared, possesses the same properties. It is fusible at a heat just below that of redness; and in a close vessel, or a vessel with a very small orifice, it is not decomposed or sublimed by a strong red heat; but if air, on the contrary, is freely admitted, it is dissipated in dense white fumes. It is insoluble in water. It effervesces in nitric acid. It silently dissolves in muriatic acid, from which it may be separated by the addition of water, which precipitates it unaltered; and it is decomposed by a solution of potash; or by heating it with the fused hydrated alkali: when it affords the orange oxide of copper. Its colour, transparency, and texture appear alone to vary. It is generally opaque, of a dark brown colour, and of a confused backly texture; but I have obtained it by cooling it slowly after it has been strongly heated, of a light yellow colour, semitransparent, and crystallized, apparently in small plates.

2d compound. How procured.

Cupranea is only very slowly formed by heating cuprane in chlorine gas. The best mode, that I have found, of procuring it, is by slowly evaporating to dryness, at a temperature

ture not much above 400 of Fahrenheit, the deliquescent muriate of copper. Thus made, it has the same appearance and the same properties, as when directly formed. It is of a yellow colour, and pulverulent. Exposed to the atmosphere, it is converted, by the action and absorption of water, into the deliquescent muriate; and its colour, during this alteration, changes from yellow first to white, and lastly to green. It is decomposed by heat; and even in chlorine gas when the experiment is made on a pretty large quantity, part of the chlorine is expelled, and assumes the gaseous state, and cuprane remains.

Component parts of both ascertained.

I have employed the same methods for ascertaining the proportions of the constituent parts of both these combinations. I have separated the copper by iron, and the chlorine by means of nitrate of silver.

1st compound.

A solution of 80 grains of cuprane in nitro-muriatic acid, precipitated by iron, afforded 51·2 grains of copper, well washed, and perfectly dried.

A solution of the same quantity of cuprane in nitric acid, precipitated by nitrate of silver, afforded 117·5 grains of horn silver, dried till it ceased to suffer any loss of weight by exposure to a temperature above 500 Fahrenheit.

Since horn silver contains 24·5 per cent of chlorine*, 80 grains of cuprane appear to contain 51·2 grains of copper and 28·8 of chlorine. And 100 appear to consist of

Component parts.

36 chlorine
64 copper
———
100

2d compound.

A solution of 40 grains of cupranea in water, acidulated with muriatic acid, precipitated by iron afforded 18·8 grains of copper.

And a solution of 20 grains of cupranea in water, precipitated by nitrate of silver, afforded 43 grains of horn silver.

Component parts of horn silver.

* This I have ascertained by synthesis; 12 grains of pure silver dissolved in nitric acid, and precipitated with muriate of ammonia, yielded 15·9 grains of fused horn silver. I do not give the particulars of the experiment, which was very carefully made; because the result very nearly agrees with that of Klaproth, and of other chemists.

Hence 100 of cupranea, omitting the very slight loss, appear to consist of

Component parts.

53	chlorine
47	copper
100	

Two other muriates.

The deliquescent muriate, and the native muriate of copper of Peru, belong to a class of compounds apparently distinct from the preceding combinations of copper and chlorine.

The deliquescent:

The deliquescent salt is well understood; and its composition may be inferred, independent of its water, from that of cupranea.

The native muriate is less known, I shall therefore relate the experiments I have made on this interesting mineral.

the native muriate of Peru.

The specimen I have examined is part of a very fine one, presented to Sir Humphry Davy by William Jacob, Esq. M. P., and deposited in the Museum of the Royal Institution. It consists of muriate and carbonate of copper, of red oxide of iron, and of green coloured quartz. The muriate is partly crystallized; the crystals, from the trials I have made of them, appeared to be pure, and they were, on that account, made the subject of my experiments.

Its properties.

The crystallized muriate dissolves entirely and without effervescence, in all the acids in which I have tried it, and the deliquescent muriate of copper is in each instance formed, and a combination of brown oxide of copper with the acid employed.

Heated slowly in a bent luted glass tube, connected with mercury, the native muriate affords water and oxigen gas, and the residue is an agglutinated brownish mass, which dissolves in muriatic acid, and gives a greenish precipitate with potash, and is apparently a mixture of brown oxide of copper and cuprane. When the heat is raised rapidly to redness, the water expelled is impregnated with muriatic acid, and muriate of copper. I have obtained from 25 grains of the mineral, heated to redness till gas ceased to be produced, just two cubic inches of oxigen. This expulsion of oxigen seems to be owing to the action of chlorine on the brown oxide to form cuprane; and there is, I have ascertained, a similar

similar production of oxigen when heat is applied to a mixture of the deliquescent muriate and brown oxide of copper.

A submuriate of copper.

From these results, which perfectly agree with those obtained by eminent chemists on the Continent, who have examined different specimens of this mineral, it appears to be a submuriate of copper, differing in a chemical point of view from the deliquescent salt merely in containing a smaller proportion of acid.

Analysis of it.

The following experiments were made with the design of ascertaining the proportions of its constituent parts.

50 grains of the crystals in powder, boiled in a solution of 50 grains of potash, afforded 36·5 grains of brown oxide of copper heated to dull redness.

And 20 grains dissolved in nitric acid, and precipitated by means of nitrate of silver, afforded 12·9 grains of dry horn silver.

Hence, considering the deficiency of weight as indicating the quantity of combined water, 100 of the native submuriate of copper seem to consist of

Its component parts.

$$\begin{array}{l} 73{\cdot}0 \text{ brown oxide} \\ 16{\cdot}2 \text{ muriatic acid} = \\ 10{\cdot}8 \text{ water} \end{array} \left\{ \begin{array}{ll} 15{\cdot}8025 & \text{chlorine} \\ & \\ {\cdot}47 & \text{hidrogen} \end{array} \right.$$

This analysis, allowance being made for the difference of theory, nearly agrees with that of Klaproth.

Methods of forming a similar compound artificially.

Mr. Proust, I believe, first discovered an artificial compound similar to the native submuriate of copper. He obtained it in the preparation of the nitro-muriate of copper, and also by a partial abstraction of the acid of the deliquescent muriate by means of an alkali. I have found that it may be procured in several other ways. It may be made directly by adding the hydrated blue oxide of copper to a solution of muriate of copper; and it may be very readily and economically prepared, by exposing to the atmosphere slips of copper partially immersed in muriatic acid; and it is also produced by the exposure of cuprane to the atmosphere. Its production in the last instance is accompanied with that of deliquescent muriate; and the formation of both seems to be owing to the absorption of water and oxigen; for cuprane I have found, though apparently not in the least acted on by by dry oxigen gas, is quickly changed when moistened with water and confined in a jar of this gas, and there is a rapid absorption of the oxigene*.

The results agree.

I have not examined all the specimens obtained by these different methods minutely, though sufficiently, I conceive, to ascertain their identity, and their similarity to the native compound. The colour of all of them is greenish white, like that of the native, in a finely divided state. When heated, they all afford water, oxigen gas, and a mixture of cuprane and brown oxide of copper.

Analysis.

I have analysed only the submuriate precipitated from a solution of muriate of copper by a weak solution of potash.

Fifty grains of this, well washed and dried, boiled in a solution of potash, afforded 36·3 grains of dried brown oxide of copper.

And 20 grains dissolved in nitric acid, and precipitated by nitrate of silver, afforded 12·75 grains of dried horn silver. These results differ so little from those obtained with the native, as fairly to permit the conclusion, that the composition of the artificial and native submuriate of copper is the same.

2. *On the Combinations of Tin and Chlorine, &c.*

Two compounds of tin with oximuriatic acid.

Tin, like copper, is capable of combining with two different proportions of chlorine. The liquor of Libavius, one of the combinations, is directly formed by the combustion of the metal in chlorine gas; and the other, I find, may be produced by heating together an amalgam of tin and calomel. Thus obtained, it is similar to that which may be procured by evaporating to dryness the muriate containing the gray oxide of tin, and fusing the residue in a close vessel. Both are of a gray colour, and of a resinous lustre and fracture; and both inflame, like tin itself, when heated in chlorine gas, and are converted into the liquor of Libavius by the absorption of a fresh portion of chlorine. Hence, as the liquor of Libavius contains the largest proportion of chlorine, it may be called stannanea, and the other compound stannane.

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* I have been informed, that submuriate of copper is sometimes found in the neighbourhood of volcanoes, particularly in that of Vesuvius. By means of the above facts, it is evident that its production might be accounted for in such situations.

1st compound

Stannane is fusible at a heat below that of dull redness; it bears this temperature, if air be nearly excluded, without undergoing any change; but when subjected to a heat as strong as glass will bear without being fused, it appears to be, from the slight fume produced, partially decomposed.

It affords the liquor of Libavius when heated with corrosive sublimate, nitre, red oxide of mercury, or with the hyperoximuriate of potash. In the last three instances, oxide of tin is also formed; and with the hyperoximuriate, the action is so violent, that inflammation is actually produced.

The liquor of Libavius and aurum musivum are formed when stannane is heated with sulphur.

Stannane, by the action of water, appears to be converted into the insoluble submuriate of tin, and the acidulous muriate.

2d compound, or liquor of Libavius.

The stannanea, or liquor of Libavius that I have examined, was made by heating together an amalgam of tin and corrosive sublimate, in the proportions commonly recommended. I have obtained this compound in another way, by treating the concentrated solution of the peroxide of tin in muriatic acid, with strong sulphuric acid; a gentle heat applied to this mixture contained in a retort, expels the fuming liquor, which may be condensed, as usual, in a cold receiver.

Its action on oil of turpentine.

The only new and remarkable property, which I have observed the liquor of Libavius to possess, is its action on oil of turpentine. I was led to make trial of it from an idea of sir Humphry Davy, that the combinations of the metals and chlorine might be soluble in oils. In the first experiment, when I poured the fuming liquor into the oil, inflammation immediately took place, with violent ebullition and production of dense reddish fumes. I have used other specimens of oil of turpentine, expecting a similar inflammation, but without its occurrence, though there has been in every instance a considerable action. The mixture of the two being made in a retort connected with mercury, no gas was generated, oxide of tin appeared to be formed, and a viscid oil was produced, which, like the fat oils, left a permanent stain on paper, and had little smell or taste, and which, digested with alcohol, imparted something which occasioned a permanent cloudiness on the admixture of water, and an odour

odour to me not unlike that of artificial camphor. The action of the liquor of Libavius on the oil of turpentine is worthy of farther inquiry. The preceding account of it, I am aware, is very incomplete; but I trust it will serve to call the attention of chemists to a subject so curious.

Analysis of the 1st compound.

To discover the proportions of tin, and consequently of chlorine, in stannane and stannanea, I have taken advantage of the superior affinity of zinc for chlorine, by means of which the tin is separated in its metallic state.

69·5 grains of stannane, made by heating in a glass tube with a very small orifice an amalgam of tin with calomel, were, with the exception of two grains of metallic mercury, apparently a mere mechanical mixture, entirely dissolved in dilute muriatic acid. A slip of clean zinc, immersed in this solution decanted from the residual mercury, quickly precipitated the tin in a very beautiful plumose form; and this precipitate collected on a filter, and well washed, and dried, and fused into one globule under a cover of tallow in a small glass tube, weighed 42 grains.

As therefore 67·5 grains of stannane contain 42 grains of tin, 100 appear to consist of

62·22	tin
37·78	chlorine
100·	

Analysis of the 2d.

As stannanea is extremely volatile, it is difficult to weigh it with perfect accuracy. The mode I adopted was to pour it into a bottle half full of water, the weight of which was previously ascertained, and to infer the quantity added by the increase of weight.

81·75 grains of stannanea thus weighed in water, afforded when decomposed by zinc 34 grains of tin*.

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* A little muriatic acid was added before the zinc was introduced, to dissolve the oxide of zinc, which, in other similar experiments, I observed was rapidly formed, and which, from the large quantity of hidrogen evolved, appeared to be owing to the decomposition of water, chiefly in consequence of the galvanic effect of the contact of the two different metals, zinc and tin.

Hence

Hence 100 of stannanea appear to be composed of

42·1	tin
57·9	chlorine
100·	

I am not acquainted with any analytical method for directly ascertaining the proportion of chlorine in either of the two preceding combinations. Nitrate of silver, when immediately applied, will not answer the purpose, because the oxide of silver is partially reduced by the solution of stannane; and an oxide of tin is thrown down in mixture with the horn silver from the liquor of Libavius.

Submuriate of tin.

Mr. Proust, to whom we are indebted for very excellent investigations of the different combinations of copper and tin, first discovered a submuriate of tin. He found that a solution of potash precipitated from the solution of muriate of tin this compound, and not the pure gray oxide of tin.

Its properties.

I have obtained it by this method, and all its properties, which I have observed, are perfectly agreeable to its supposed composition.

It is decomposed by a red heat. Subjected to distillation in a small bent glass tube connected with mercury, no gas was produced, water containing muriatic acid and muriate of tin was expelled, and a sublimate like stannane was formed, and the fixed residue was gray oxide of tin.

It effervesces violently with nitric acid; and strong sulphuric acid expels from it muriatic acid fumes.

It dissolves without effervescence in the muriatic and acetic, and in the dilute nitric and sulphuric acids; and all these acid solutions, as they give a black precipitate with a solution of corrosive sublimate, appear to contain the tin in the state of gray oxide.

Analysis of it.

The complete analysis of this submuriate of tin is difficult. The oxide it contains cannot be accurately separated by potash, nor can nitrate of silver be employed to ascertain the proportion of muriatic acid.

I have found 50 grains of it, dissolved in muriatic acid, to afford, when precipitated by zinc, 31 grains of metallic tin. Now as this submuriate is similar to the submuriate of copper, the analogy being imperfect only in the latter containing the per-

peroxide, and the former the protoxide, it is natural to infer, that the proportion of muriatic acid is similar in both. But the proportion of muriatic acid in the submuriate of copper is apparently half of that which exists in the muriate; hence, supposing the composition of the submuriate of tin to be similar, 100 of it will consist of

70·4	gray oxide
19·0	muriatic acid
10·6	water
100·	

Probability alone can be attached to this estimate. I have not given the calculations by which it was made, as their data are liable to objection.

3. *On the Combinations of Iron and Chlorine.*

Two compounds of iron and oximuriatic acid.

As there are two oxides of iron, so there are also two distinct combinations of this metal and chlorine. One may be directly formed by the combustion of iron wire in chlorine gas; it is that volatile compound described by sir Humphry Davy in his last Bakerian Lecture, which condenses after sublimation in the form of small brilliant iridescent plates.* The other, I find, may be procured by heating to redness, in a glass tube with a very small orifice, the residue which is obtained by evaporating to dryness the green muriate of iron; it is a fixed substance requiring a red heat for its fusion; it is of a grayish but variegated colour, of a metallic splendour, and of a lamellar texture. As it absorbs chlorine when heated in this gas, and becomes entirely converted into the volatile compound; and as the volatile compound may likewise be obtained by heating in a glass tube, nearly closed, the residue from the evaporation of the red muriate, it is evident, that the fixed compound contains less chlorine than the volatile, and that the former, consequently, may be called ferrane, and the latter ferranea.

2d compound.

1st compound.

Its solution in water.

Ferrane dissolves in water and forms the green muriate of iron; but the solution of the whole substance is not complete

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* Journal, vol. XXIX, p 226.

plete. There is always left a small and variable quantity of black oxide, which may be considered, on account of its variability, in a state of mechanical mixture, rather than of chemical union with the ferrane.

Solution of the 2nd.

Ferranea is entirely soluble in water. The solution is identical with the red muriate of iron.

Analysis

The analysis of both these compounds is easily effected by means of nitrate of silver.

of the 1st compound:

50 grains of ferrane were put into water: the insoluble residue separated from the solution by decantation; washed, dried, and heated to redness for a minute, previously moistened with oil, weighed 3 grains, and was in the state of the black oxide, being attracted by the magnet. The solution entire, precipitated by nitrate of silver, afforded 102·5 grs of dried horn silver, which indicating 25·1125 grs of chlorine, the proportion of iron, omitting the 3 grains of oxide, appears to be 21·8875. And hence 100 of ferrane seem to consist of

53·43 chlorine
46·57 iron
100·

and of the 2d.

Ferranea is not easily obtained in considerable quantities, I have been obliged in consequence to operate upon small portions. The subject of analysis was procured by sublimation from the residue by evaporation of the red muriate. 20 grs of this, in brilliant scales, were weighed in water. The solution, precipitated by nitrate of silver, yielded 53 grs of dried horn silver. Hence 100 of ferranea appear to consist of

64·9 chlorine
55·1 iron
100·

(To be concluded in our next.)

X.

III.

METEOROLOGICAL JOURNAL.

		Pressure.			Temperature.					
1812.	Wind	Max.	Min.	Med.	Max	M	Med	Evap.	Rain	
7th Mo.										
July 1	S W	29·70	29·44	29·570	63	52	57·5	—	·29	☽
2	Var.	29·56	29·40	29·480	65	47	56·0	—	·53	
3	N	30·00	29·56	29·780	61	42	51 5	—		
4	S W	30·05	30·01	30·030	61	42	51·5	·45		
5	S W	30·02	29·96	29·990	63	51	57·0	—		
6	N W	30·27	30·02	30·145	67	50	58·5	—		
7	N	30·29	30·27	30·280	72	51	61·5	·43		
8	E	30·33	30·29	30·310	71	46	58.5			●
9	N E	30·33	30·29	30·310	73	50	61·5			
10	N	30·39	30·29	30·340	72	51	61·5			
11	N	30·29	30·16	30·225	69	54	61·5			
12	N W	30·17	30·16	30·165	66	41	53·5			
13	N W	30·19	30·16	30·175	64	52	58·0			
14	Var.	30·19	30·17	30·180	64	46	55·0		·04	
15	Var.	30·17	30·05	30·110	69	50	59·5		·01	
16	E	30·05	29·95	30·000	65	55	60·0	—		☾
17	Var.	30 14	29·95	30·045	67	56	61·5	—		
18	S E	30·10	30·00	30·050	75	56	65·5	·37		
19	S W	30·00	29·74	29·870	73	55	64·0	—	·17	
20	W	29·85	29·70	29·775	75	50	62·5	—	·34	
21	W	29.96	29·94	29·950	65	45	55·0	·35		
22	S W	30·09	29·96	29·025	63	42	52·5	—	·15	
23	S W	30·09	29·94	29·015	65	52	58·5	—		
24	S W	29 94	29·78	29·860	62	58	60·0	—	·26	○
25	S W	29·79	29·78	29·785	71	57	64·0	·55	·03	
26	N W	29·85	29·79	29·820	68	49	58·5	—		
27	Var.	29·66	29·60	29·630	61	48	54·5	—	1·00	
28	S W	29·66	29·65	29·655	64	50	57·0	—		
29	W	29·80	29·66	29.730	63	49	56·0	·55	·22	
		30·39	29·40	29·975	75	41	58·34	2·70	3·04	

The observations in each line of the Table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

NOTES.

Seventh Month. 1. Much wind: very cloudy: rain at intervals through the day and night. 2. Fair a. m. Thunder showers with hail, p. m. 3. Cloudy: a few drops of rain. 4. The wind veered gradually from N. by E. to S.W. 5. Wind moderate. 22. Thunder and hail***.

RESULTS.

Winds variable.

Barometer: highest observation 30·39 inches; lowest 29·40 inches;
Mean of the period 29·975 inches.

Thermometer: highest observation 75°; lowest 41°;
Mean of the period 58·34·°.

Evaporation (in 21 days, the rest being lost by an accident) 2·70 inches.

Rain 3·04 inches.

PLAISTOW. L. HOWARD.
Eighth Month, 17, 1812.

IV.

Chemical Researches on the Blood, and some other Animal Fluids. By WILLIAM THOMAS BRANDE, *Esq., F. R. S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society**.

I. *Introduction.*

Analysis of animal substances difficult.

IN the following pages I shall have the honour of laying before this Society an account of some experiments upon the blood, which were originally undertaken with a view to ascertain the nature of its colouring matter. The difficulties attendant on the analysis of animal substances have rendered

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* Philos. Trans. for 1812, p. 90.

some of the results less decisive than I could have wished, but I trust, that the general conclusions to which they lead will be deemed of sufficient importance, to occupy the time of this body.

Iron in the blood.

The existence of iron in the blood was first noticed by Menghini*, and its peculiar red colour has been more recently attributed to a combination of that metal with phosphoric acid by Messrs. Fourcroy and Vauquelin†. The very slight discoloration occasioned by the addition of infusion of galls to a solution of the colouring matter, under circumstances most favourable to the action of that delicate test of iron, first led me to doubt the inferences of those able chemists; and subsequent experiments upon the combinations, to which they allude, tended to confirm my suspicion, and induced me to give up no inconsiderable portion of the time which has elapsed since the last meeting of this Society, to the present investigation.

Examination of the chyle and lymph.

An examination of the chyle and of lymph, in order to compare their composition with that of the blood, formed an important part of this inquiry; especially as those fluids have not hitherto been submitted to any accurate analysis, on account of the difficulty of procuring them in sufficient quantities, and in a state of purity. While engaged in assisting Mr. Home in his physiological researches, several opportunities occurred of collecting the contents of the thoracic duct under various circumstances, and in different animals; on other occasions Mr. Brodie has kindly furnished me with the materials for experiment.

II. *On the Composition of Chyle.*

Contents of the thoracic duct variable.

The contents of the thoracic duct are subject to much variation. About four hours after an animal has taken food, provided digestion has not been interrupted, the fluid in the duct may be regarded as pure chyle: it is seen entering by the lacteals in considerable abundance, and is of a uniform whiteness throughout. At longer periods after a meal, the quantity

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* Vincentius Menghinus de Ferrearum Particularum Progressu in Sanguinem. Comment. Acad. Bonon. T. 2, P. 2, page 475.

† Système des Conn. Chym. Vol 8, p. [Vol. 9, p. 152. C.]

quantity of chyle begins to diminish, the appearance of the fluid in the duct is similar to that of milk and water; and lastly, where the animal has fasted for twenty-four hours or longer, the thoracic duct contains a transparent fluid, which is pure lymph.

Properties of the chyle.

A. The chyle has the following properties.

1. When collected without any admixture of blood, it is an opaque fluid of a perfectly white colour, without smell, and having a slightly salt taste, accompanied by a degree of sweetness.

2. The colour of litmus is not affected by it, or that of paper stained with turmeric; but it slowly changes the blue colour of infusion of violets to green.

3. Its specific gravity is somewhat greater than that of water, but less than that of the blood; this, however, is probably liable to much variation.

Separates into two parts.

4. In about ten minutes after it is removed from the duct, it assumes the appearance of a stiff jelly, which in the course of twenty-four hours gradually separates into two parts, producing a firm and contracted coagulum, surrounded by a transparent colourless fluid. These spontaneous changes, which I have observed in every instance where the chyle was examined at a proper period after taking food, are very similar to the coagulation of the blood and its subsequent separation into serum and crassamentum; they are also retarded and accelerated by similar means.

Properties of the coagulum.

B. 1. The coagulated portion bears a nearer resemblance to the caseous part of milk than to the fibrine of the blood.

Action of alkalis,

2. It is rapidly dissolved by the caustic and subcarbonated alkalis. With solutions of potash and soda it forms pale brown compounds, from which, when recent, a little ammonia is evolved. In liquid ammonia the solution is of a reddish hue.

acids,

3. The action of the acids upon these different compounds is attended with nearly similar phenomena, a substance being separated intermediate in its properties between fat and albumen. Nitric acid added in excess redissolves this precipitate in the cold, and sulphuric, muriatic, and acetic acids when boiled upon it for a short time.

alcohol and ether,

4. Neither alcohol nor ether exerts any action upon the

coagulum

coagulum of chyle; but of the precipitate from its alkaline solution they dissolve a small portion, which has the properties of spermaceti: the remainder is coagulated albumen.

and sulphuric acid.

5. Sulphuric acid very readily dissolves this coagulum, even when diluted with its weight of water; and with the assistance of heat, it is soluble in a mixture of one part by weight of acid, with four of water; but when the proportion of water is increased to six parts, the dilute acid exerts no action upon it. I was surprised to find, that the alkalis produced no precipitation in these sulphuric solutions when heat had been applied in their formation, and where a small proportion only of the coagulum had been dissolved; and was therefore led to examine more particularly the changes, which the coagulum had undergone by the action of the acid.

The solution in this acid examined.

On evaporating a solution of one drachm of the coagulum in two ounces of dilute sulphuric acid (consisting of one part by weight of acid with three of water) down to one ounce, a small quantity of carbonaceous matter separated, and the solution had the following properties.

It was transparent, and of a pale brown colour.

Neither the caustic nor carbonated alkalis produced in it any precipitation, when added to exact saturation of the acid, or in excess.

Infusion of galls, and other solutions containing tannin, rendered the acid solution turbid, and produced a more copious precipitation in that which had been neutralized by the addition of alkalis.

When evaporated to dryness, carbonaceous matter was deposited, and sulphurous acid evolved, with the other usual products of these decompositions.

Action of nitric acid on the coagulum.

6. On digesting the coagulum in dilute nitric acid, consisting of one part by weight of the acid to fifteen of water, it was speedily rendered of a deep brown colour, but no other apparent change was produced for some weeks; when, on removing it from the acid at the end of that period, it had acquired the properties of that modification of fat, which is described by Fourcroy under the name of adepocire*.

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* Mém. de l'Acad. des Sciences, 1789.

A mixture of one part of nitric acid with three of water acted more rapidly upon the coagulum of chyle; a portion of it was dissolved, and, when the acid was carefully decanted from the remainder, it was found to possess the properties of gelatine. But when heat was applied, or when a stronger acid was employed, the action became more violent, nitrogen and nitric oxide gas were evolved, and a portion of carbonic acid and of oxalic acid was produced.

Action of muriatic acid,

7. Muriatic acid in its undiluted state does not dissolve the coagulum of chyle; but when mixed with an equal quantity of water, or even more largely diluted, it dissolves it with facility, forming a straw-coloured solution, which is rendered turbid when the alkalis are added to exact saturation, but no precipitate falls, nor can any be collected by filtration. When either acid or alkali is in excess in this solution, it remains transparent.

of acetic,

8. Acetic acid dissolves a small portion of the coagulum of chyle, when boiled upon it for some hours. As the solution cools, it deposits white flakes, which have the properties of coagulated albumen.

oxalic, &c.

9. The action of oxalic acid is nearly similar to that of the acetic, but neither citric, nor tartaric acid, exerts any action upon this coagulum.

Destructive distillation.

10. The destructive distillation of this substance affords water slightly impregnated with carbonate of ammonia, a small quantity of thin fetid oil, and carbonic acid and carburetted hidrogen gas.

Coal.

The coal which remains in the retort is of difficult incineration; it contains a considerable portion of muriate of soda and phosphate of lime, and yields very slight traces of iron.

Properties of the serous part of chyle.

C. 1. The serous part of the chyle becomes slightly turbid when heated, and deposits flakes of albumen.

Crystals in it.

2. If after the separation of this substance the fluid be evaporated to half its original bulk, at a temperature not exceeding 200° Fahrenheit; small crystals separate on cooling, which, as far as I have been able to ascertain, bear a strong resemblance to sugar of milk; they require for solution about four parts of boiling water, and from sixteen to twenty parts of water of the temperature of 60°. They are sparingly soluble in boiling alcohol, but again deposited as the

the solution cools. At common temperatures alcohol exerts no action upon them. The taste of their aqueous solution is extremely sweet. By nitric acid they are converted into a white powder of very small solubility, and having the properties of saccholactic acid, as described by Mr. Scheele*.

The form of the crystals I could not accurately ascertain even with the help of considerable magnifiers. In one instance they appeared oblique six-sided prisms, but their terminations were indistinct.

Some of the crystals, heated upon a piece of platina in the flame of a spirit lamp, fused, exhaled an odour similar to that of sugar of milk, and burnt away without leaving the smallest perceptible residuum.

Destructive distillation.

3. The destructive distillation of the serous part of chyle afforded a minute quantity of charcoal, with traces of phosphate of lime, and of muriate of soda and carbonate of soda.

III. *Analysis of Lymph.*

Pure lymph.

The food found in the thoracic duct of animals that have been kept for twenty-four hours without food is perfectly transparent and colourless, and seems to differ in no respect from that which is contained in the lymphatic vessels. It may therefore be regarded as pure lymph.

Its properties.

It has the following properties†.

1. It is miscible in every proportion with water.

2. It produces no change in vegetable colours.

3. It is coagulated neither by heat, nor acids, nor alcohol: but is generally rendered slightly turbid by the last reagent.

4. When evaporated to dryness, the residuum is very small in quantity, and slightly affects the colour of violet paper, changing it to green.

5. By incineration in a platina crucible the residuum is found to contain a minute portion of muriate of soda; but I could not discover in it the slightest indications of iron.

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* Chemical Essays, No. XVII,

† The term lymph has been applied indiscriminately to the tears, to the matter of encysted dropsy, and to some other animal fluids.—Vide Aikin's Dictionary of Chemistry and Mineralogy, Art. Lymph.

6. In

6. In the examination of this fluid, I availed myself with some advantages of those modes of electrochemical analysis, which on a former occasion I have described to this Society*.

Analysis of it by electricity.

When the lymph was submitted to the electrical action of a battery, consisting of twenty pairs of four inch plates of copper and zinc, there was an evolution of alkaline matter at the negative surface, and portions of coagulated albumen were separated. As far as the small quantities on which I operated enabled me to ascertain, muriatic acid only was evolved at the positive surface.

IV. *Some Remarks on the Analysis of the Serum of Blood.*

This fluid has been so frequently and fully examined by chemists, that I shall not enter into a detailed account of its composition, but merely state such circumstances respecting it as relate particularly to the present inquiry, and have not hitherto been noticed by the experimentalists to whom I have alluded.

Serum of blood.

The fluid which oozes from serum that has been coagulated by heat, and which, by physiologists, is termed *serosity*, is usually regarded as consisting of gelatine, with some uncombined soda, and minute portions of saline substances, such as muriate of soda and of potash, and phosphate of lime and of ammonia. Dr. Bostock regards it as mucus†.

Serosity.

From some experiments which I made upon the serum of blood, on a former occasion, I was induced to regard the serosity as a compound of albumen with excess of alkali, and to consider the coagulation of the serum analogous to that of the white of egg, and of the other varieties of liquid albumen.

To ascertain this point, and to discover whether gelatine exists in the serum, I instituted the following experiments.

Examination of it.

Two fluid ounces of pure serum were heated in a water bath until perfectly coagulated: the coagulum, cut into pieces, was digested for some hours in four fluid ounces of distilled

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* Phil. Trans 1809, p. 373 [Journ. vol. XXVI, p. 14.]

† Transactions of the Medical and Chirurgical Society of London, vol. I, p. 73.

distilled water, which was afterward separated by means of a filter.

The clear liquor reddened turmeric paper, and afforded a copious precipitation on the addition of the infusion of galls, and when evaporated to half an ounce, it gelatinized on cooling. It was rendered very slightly turbid by the addition of dilute sulphuric and muriatic acid; but alcohol produced no effect.

From the result of these trials, it might have been concluded, that gelatine was taken up by the water, but as an alkaline solution of albumen forms an imperfect jelly when duly concentrated, and as albumen and gelatine are both precipitated by tannin, I was inclined to put little reliance on the appearances just described, until I had examined the solution by the more accurate method of electrical decomposition.

It contained albumen,

Upon placing it in the Voltaic circuit my suspicions were justified, by the rapid coagulation which took place in contact with the negative wire. I therefore made some other experiments in order to corroborate this result.

One fluid ounce of pure serum was dissolved in three of distilled water: the conductors from a battery of thirty pairs of four inch plates were immersed in this solution at a distance of two inches from each other; the electrization was continued during three hours and a half, the solid albumen being occasionally removed; at the end of that period, no farther coagulation took place, and a mere decomposition of the water was going on.

but no gelatine.

Having ascertained in previous researches, that gelatine is not altered during the electrical decomposition of its solution carried on as just described, my object in this experiment was, to ascertain whether any gelatine remained after the complete separation of the albumen had been effected. I accordingly examined the water from which the coagulated albumen had been removed, and found that it was not altered by infusion of galls, nor did it afford any gelatine when evaporated to dryness.

Action of muriatic acid on it.

Two fluid ounces of dilute muriatic acid were added to one of serum. The mixture immediately assumed a gelatinous appearance; it was heated, and a more perfect coagulation

lation of the albumen took place; the liquid part was separated by a filter. No effect was produced upon it by Voltaic electricity, nor did infusion of galls occasion any precipitation.

Farther test of its composition.

I repeated the first experiment with the addition of twenty drops of a solution of isinglass to the serum. The liquid which now separated, after the albumen had been entirely coagulated by the action of electricity, was copiously precipitated by infusion of galls.

It consists of albumen combined with much alkali.

It may be inferred from these experiments, that gelatine does not exist in the serum of the blood, and that the serosity consists of albumen in combination with a large proportion of alkali, which modifies the action of the reagents commonly employed, but which is readily separated by electrical decomposition.

Slight traces of iron in it.

To ascertain whether iron exists in the serum of the blood, one pint was evaporated to dryness in a crucible, and gradually reduced to a coal, which was incinerated and digested in muriatic acid, to which a few drops of nitric acid were added; some particles of charcoal remained undissolved; the solution was saturated with ammonia, which afforded a copious precipitation of phosphate of lime, accompanied with slight traces only of oxide of iron.

V. *Some Experiments upon the Coagulum of Blood.*

Coagulum of blood.

Mr. Hatchett's valuable researches on the chemical constitution of the varieties of coagulated albumen have shown, that this substance varies but little in its properties, whether obtained from the crassamentum of the blood, or from washed muscular fibre, or other sources; but that the proportion of earthy and saline matter is different in the different varieties*.

It will also be remarked, on referring to the dissertation which I have just quoted, that the ashes obtained by incinerating the coal left after the destructive distillation of albumen, did not contain any appreciable proportion of iron.

Inquiry whether the colour of blood be owing to iron.

Assuming the existence of iron in the colouring matter of the blood, I made the following experiments upon the crassamentum of that fluid.

Two pints of blood were collected in separate vessels.

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* Phil. Trans. 1800, p. 384.

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The one portion was allowed to coagulate spontaneously; the other was stirred for half an hour with a piece of wood, so as to collect the coagulum, but to diffuse the principal part of the colouring matter through the serum. These two portions of coagulum were now dried in a water-bath, and equal weights of each reduced in a platina crucible to the state of coal, which afterward was incinerated. The ashes were digested in dilute nitromuriatic acid, and the solution saturated with liquid ammonia, in order to precipitate the phosphate of lime, as well as any iron which might have been present.

The precipitates were collected, dried, and treated with dilute acetic acid, by which they were almost entirely dissolved; some very minute traces only of red oxide of iron remaining, the quantity of which was similar in both cases, and so small as nearly to have escaped observation.

It appeared not.

It is reasonable to infer, that, if the colouring matter of the blood were constituted by iron in any state of combination, a larger relative proportion of this metal would have been discoverable in the former than in the latter coagulum; but frequent repetitions of these experiments have shown, that this is not the case, and the following result appears to complete the evidence on this subject.

This farther confirmed.

The colouring matter of a pint of blood was diffused by agitation through the serum, from which it was allowed gradually to subside, the coagulum having been removed: after twenty-four hours, the clear serum was decanted off, and the remainder containing the colouring matter, after having been evaporated to dryness, was incinerated, and the ashes examined as in former experiments. But the traces of iron were here as indistinct as in the other instances above mentioned, although a considerable quantity of the colouring matter had been employed.

The minutiæ of analysis I have purposely excluded, as leading into details which would exceed the proper limits of this paper, and unnecessary in the present investigation; I shall now merely dwell on the principal results which have been obtained, and on the general conclusions which these afford.

(To be concluded in our next.)

V.

V.

On the Nature of falling Stars and the large Meteors, in Answer to Mr. JOHN FAREY, *Senior. In a Letter from Mr.* G. J. SINGER.

To W. NICHOLSON, Esq.

SIR,

Mr. Farey's hypothesis of falling stars.

THE appearance of a second communication from your correspondent, Mr. Farey, on the nature of falling stars*, leads me to offer a few observations on that subject, which would have followed his first paper, had not the obviously hypothetical nature of his suggestion appeared to render any remark unnecessary.

Mr. Farey notices the electrical nature of these appearances, and their frequent occurrence in "clear frosty nights," and the clear intervals of "showery weather†:" and observing that such "states of the air are best adapted, by its "clearness, for seeing the smaller stars and planets," supposes "that these phenomena are occasioned by an almost infinite number of *satellitulæ*, or very small moons, "constantly revolving round the Earth, in all possible directions, and appearing only during the very short time "that they dip into the upper part of the atmosphere each "time they are in *perigee*: and that no step seems wanting "in the degree of this dip into the atmosphere, and their "consequent brightness, length, and slowness of courses, "&c., between the smallest instantaneous *shooting stars*, "and the largest *meteors*, (such as that of August, "1783.")

His assumption of their being explicable without electricity.

This is the substance of Mr. Farey's first communication; and apparently amounts to an *assumption*, that the phenomena may be explained without electricity; *if it be admitted*, that *clear* weather and the *absence* of *twilight*, *moonlight*, &c., are *essential* for their observance: and that

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* Journal, vol. XXXII, p. 269.

† Ib. vol. XXX, p. 285.

planetary bodies may move with *immeasurable velocity*; and appear luminous *only when they dip into our atmosphere.*

Shooting stars and large meteors not the same.

If we were to accede to all these *suppositions*, still I presume it would be impossible without manifest absurdity, to refer two appearances so distinct and dissimilar as *instantaneous* shooting stars, and large *progressive* meteors, to the *same cause.*

Facts contradictory to the conditions assumed by Mr. Farey.

But the supposed conditions for the appearancee of luminous meteors are not necessary. Mr. Forster has noticed their occurrence not only in clear weather, but "when cirrocumulus and thunder clouds abound." Mr. Morgan described them darting from the vertex of a bright conical stream of the northern lights; and Beccaria relates minutely the occurrence of a very remarkable one, an hour after sunset.

Numerous observations to the same purpose by the author.

For ten years my attention has been much occupied by these phenomena; I have observed many thousands of them in various situations, and under almost every possible diversity of circumstances. Of the smaller meteors (the shooting stars) I have frequently counted 40 and 50 in an hour, in the brightest *moonlight* nights of summer. I have seen them when no cloud has been apparent, and when the atmosphere has teemed with clouds; and have occasionally observed them when the rays of the Sun had scarcely ceased to illumine the atmosphere.

Meteors seen by him. 1st.

Of the larger progressive meteors I have seen but three; one of these occurred in bright light, at 6 o'clock on a summer's evening. Its motion was apparently rectilinear, and in a horizontal direction from east to west. I had an opportunity of comparing the accounts of several other observers, they nearly coincided with my own observation. The meteor left no visible luminous track, but was followed by a luminous conical tail nearly three times the length of its apparent diameter. It left the field of view to which I was confined, without dispersion. 2d. The second meteor I observed at three o'clock in the morning, in the month of April, 1806. It descended in a curve from a considerable height in the north to within an apparently short distance from the Earth, when it dispersed in luminous particles; it left no impression of a luminous track; its form was spherical, and its

its size apparently equal to the full moon, which it surpassed considerably in brightness. I had two intelligent observers with me at the time, and an excellent opportunity was afforded for this comparison, as the moon was shining, and the atmosphere unobscured by clouds. The last progressive meteor I have had an opportunity of observing occurred on a cloudy, but moonlight night in august 1808, at one o'clock. It was smaller than either of the preceding, and of a bright red colour; it described a curve of short radius; its course was nearly from north to south, appearing and disappearing nearly at the extremities of the same horizontal line; it dispersed in luminous particles; no track or train of light was observed.

3d.

Smaller meteors very different, and apparently electrical.

The peculiarities of smaller meteors are very different. They move with inconceivable velocity; their light is less brilliant; their course usually rectilinear; their appearance frequent, and attendant on states of the atmosphere known to be most connected with its electrical changes. Like lightning they more frequently strike from one part of the atmosphere to another, than from the atmosphere to the Earth; and like it also, when they appear to strike the Earth, they leave no evidence of a moon, or planetary body, having done so.

Difference of large meteors.

Large meteors have rarely dispersed over any spot within reach of observation, but stony bodies have been found; but this has not to my knowledge been ever the case with falling stars, or meteors of a similar nature. From this circumstance, and from the different appearance of their light, the different velocity with which they move, the frequent appearance of the one, and the rare occurrence of the other, I think the two kinds of meteors are distinctly defined, and decisively separated.

Occasional streaks of light in the track of the smaller may be differently accounted for.

With regard to the streaks of light sometimes seen in the track of shooting stars, I am rather inclined to think with Mr. Farey it may be an optical illusion; but I confess this conclusion is in the highest degree doubtful; though its accuracy does not appear to me capable of being easily ascertained, as various equally probable explanations may be given of the phenomenon. No light was apparent in the track of either of the *progressive* meteors I have observed,

and

and the occurrence of such an appearance as an attendant on rapid motion may be consistently accounted for without reference to optical illusion.

Nature of these bodies not yet clear.

In the present state of our knowledge it is certain no positive conclusion relative to the nature of meteors is warrantable; but, in the absence of precise and accurate views, that explanation should be preferred, which is most extensively applicable to the known peculiarities of their appearance.

The large meteors.

The larger progressive meteors are, I think, at present perfectly mysterious. They are certainly not connected with any of the obvious phenomena of electricity; and the chemical character of the stones that have fallen when they have appeared has been adduced as a proof, that they are "travellers from another planet." The uniformity of their composition appears however to render it probable, that they are always derived from the same source, and their light is in all probability the light of combustion.

Arguments for the smaller being electrical.

Falling stars are evidently a more simple phenomenon. The arguments in favour of their electrical origin are numerous, as will be apparent from the following summary of the principal facts.

1st. Their light is similar to the light of the electric spark.

2d. Their motion, like that of electricity, is inconceivably rapid.

3d. They occur as frequently as other electric changes in the atmosphere.

4th. Their occurrence is most frequent after such changes of weather, as are known to influence the electrical state of the atmosphere.

5th. Their direction is never constant; they move vertically, horizontally, and at various degrees of inclination, in all parts of the atmosphere; such is also the case with lightning.

6th. The appearance of falling stars may be accurately imitated by electricity; and the circumstances on which the success of such experiments depend are such as are likely to occur in the production of the natural phenomenon.

Objections to the planetary hypothesis.

If more powerful evidence can be adduced in support of the planetary hypothesis, it may he intitled to consideration: in its present state it is mere conjecture; and, as opposed to facts

facts, and to analogy, must be considered inadmissable according to the strict principle of experimental inquiry. A few of the circumstances opposed to it may be thus stated:

1st. The *number* of falling stars, and their *frequent*, but not *constant* appearance.

2d. The rapidity of their motion.

3d. Their transient duration.

4th. Their occurrence in a cloudy state of the atmosphere.

5th. Their occurrence when the bright light of the moon renders many small stars and planets invisible.

6th. Their appearance in the lower as well as higher strata of the atmosphere.

From the extensive observations I have made many other particulars might be stated, but I trust what has been advanced will convince your correspondent of the fallacy of his hypothesis, and I cannot but lament he should have stated with such apparent satisfaction the discontinuance of a reference to these phenomena in Mr. Foster's valuable meteorological observations; agreeing, as I imagine every candid inquirer will, in the propriety of Mr. Foster's remark, " that " it is only by repeated and accurate observations of a " multitude of phenomena, the science of meteorology can " be brought to its required perfection."

I remain, sir,

With great regard, yours, &c.

Princes street, Cavendish square, G. J. SINGER.

August the 10*th*. 1812.

VI.

Sketch of the Geology of Madeira: by the Hon. HENRY GREY BENNET. *In a letter addressed to* G. B. GREENOUGH, *Esq., F. R. S. Pres. G. S**.

Geology of Madeira little known.

THE following notes were taken during a short stay I made last summer in the island of Madeira. As there appears to be but little known of the structure, or of the phenomena

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* Trans. of the Geolog. Soc. vol. I, p. 391.

Its vallies.

nomena which the strata in that island exhibit, the following observations may not perhaps be wholly unacceptable. They may be considered as furnishing directions to others, where to look for some of the most interesting objects; and may afford to future travellers a small portion of the information, which my guide, Dr. Shuter, so liberally communicated to me. That gentleman, having long resided in the island, had repeatedly traversed it, and was thereby able to point out to me some of the circumstances which were most worthy of examination, particularly the nature of the various strata that are exposed to view in the deep and abrupt vallies which intersect the island in all directions. These vallies are no less picturesque to the eye of the common traveller than they are deserving of the attention of the geologist. They are in general narrow and deep, the summits of the hills that form their boundaries are broken into peaks, rugged and bare, while their sides are covered with the cedar and other trees peculiar to southern latitudes, and with a profuse variety of shrubs and plants, among which the erica arborea is the most beautiful, and in the greatest quantity.

Size of the island.

The island of Madeira (though I believe it never has been surveyed) is said to be about 50 miles in length, and in its broadest part about 20, but the average breadth does not exceed 15 miles.

Hills.

Vallies.

Surface clay.

Extinct volcanoes.

It consists of a succession of lofty hills rising rapidly from the sea, particularly on the eastern and northern extremities. The summits of many of these ranges present the appearance of what has been called a table land; yet occasionally the forms are conical, and surmounted by a peak, which in some instances I found to be of columnar basalt. Deep ravines or vallies descend from the hills or serras to the sea, and in the hollow of most of them flows a small river, which in general is rapid and shallow. The soil of the island is clay on the surface, and large masses of it as hard as brick are found underneath. Though there are not at present any existing volcanoes in the island, yet the remains of two craters are to be seen, one on the eastern the other on the western side, the largest being about a Portuguese league, or four English miles in circumference. Every thing around wears marks of having suffered the action of fire, yet I was unable to discover any deposit of sulphur, and was told that none had hitherto been found in the island.

Varieties of lava in strata.

The varieties of strata, which I shall term generally lava, are not numerous. I myself saw but four, and I was informed there are no more to be met with. Three of them were invariably alternating in the same order. The first or lowest lava is of a compact species, containing few, if any, extraneous substances, is of a blue colour, and of a remarkably fine grain. Upon that, the second, which is a red earthy friable lava, rests; sometimes separated by beds of clay mixed with pumice, and layers of black ash and pumice. This red lava contains minute pieces of olivine; sometimes it assumes a prismatic form, and in one place was of a moderate degree of hardness: the principal springs of water in the island issue from this stratum. On the top is the third, a grayish lava, generally compact, though at times near the surface very cellular, and containing much olivine. This lava takes principally the prismatic form of basalt. I have seen it in the most perfect prisms from 30 to 40 feet or more in height, the surface being covered with scoria, ash, and pumice. These masses of lava contain more or less of what I consider to be olivine, occasionally carbonate of lime and zeolite, which last assumes either a crystallized or globular form, or is diffused in a thin coating between the different layers.

A distinct stratum.

The fourth species of lava is of a coarse grain, is used for the making of walls, and the commonest and poorest houses are built of it, the blue and gray lavas being used for the copings, &c. It works easier than the two other kinds abovementioned, is more friable and soft, and its colour is a mixture of brown and red. I observed it in a stratum by itself, and it did not seem to have any connection with the other three kinds.

Varieties not in strata.

These are the principal stratified lavas that the island affords, but in the beds and rivers, particularly in that which flows in the valley of the Corral, several varieties occur in isolated masses, containing olivine and zeolite in greater or less quantity, and exhibing detached portions of strata, similar to those that are found in the fossa grande on the side of Vesuvius.

In

In the deep and singular valley called the Corral, which I had an opportunity of examining for several miles, the red and gray lava alternated five or six times. The tops of some of its barrier hills are formed of columnar basalt; here and there rising to a peak, or broken into what might be termed a crystallised ridge, or tapering to a point like the granite needles in the Mer de Glace. The columnar strata are found here in all directions. They dip usually to the sea, but occasionally are dislocated in the most abrupt manner. Dykes of lava, rising perpendicularly to the horizon, intersect the strata at right angles. I saw one 200 or 300 feet in height, which cut through several of the alternations of the red and gray lava. This valley of the Corral well merits the most attentive examination; yet the journey there is one of some labour, and the walk down the river that flows in its bottom so difficult and toilsome, as almost to deter every one from the undertaking. We left the town of Funchal soon after day break, and did not return till between eight and nine at night, having been, during the whole of that period, in a state of incessant exertion on horseback or on foot. The bed of the valley itself cannot be descended on mules or on horseback. The walk is eight or nine miles in length, and you are compelled to clamber over rocks, as there is not even a track, or wade, in the bed of the river, which is rapid, and full of large and pointed stones. Some of the highest hills of the island border on this valley. Several of them rise from the bed of the river in a perpendicular height of 1000 or 1500 feet, judging only by the eye, and are what the French term taillé à pic. Others are broken into a succession of steep descents, and are covered with forests of wood and a profusion of plants. Down many there fall small cataracts of water, and some are hollowed into deep recesses, whence issue from the lava numerous little streams that contribute to swell the principal river in the valley.

Columnar basaltes.

Dykes.

Valley of the Corral.

As you arrive on the brink of the Corral, after a ride of about 10 miles from Funchal, you find yourself suddenly on the edge of a precipice, near to which a sort of traversing stair-case is cut, with a track winding to the bottom. On the right is a wall of lava nearly perpendicular from 400 to 500 feet in depth, composed of the two species of the red and

Wall of lava.

and gray, alternating five or six times, and assuming in its dislocation the form of a bow, both the lavas following in a regular bend the shape of the curve.

Lava in very small columns.

On the left of the stairs by which you are to descend, innumerable small columns of the gray lava project from the side; they dip N. W. and their form in general is quadrangular; but I found several of them in prisms of three, five, and six sides. They are remarkably small, and as they lie in this bed, appear almost all to break off from each other at five or six inches in length, and I never found them exceed this size. They seem to form a dyke that cuts through the horizontal beds of lava.

Basaltic columns.

Volcanic products.

At the edge of the descent there is a projection or range of basaltic columns, rising like a wall, tapering to the top, and separating into large quadrangular prisms. We found no black ashes in the valley of the *Corral,* though toward the bottom there are considerable strata of pumice, great masses of scoriæ, and cellular lava, and lava in a state of semi-vitrification, the whole presenting evident marks of an eruption, anterior to that which had formed these various strata of lava, which are visible from the summit of the hill to the bed of the river.

Dip of the strata.

Breccia.

The dip of the strata is in general toward the sea. Basaltic columns shoot from the side of the ordinary strata, which are intersected by various dykes; and one of these in particular swept across both sides of the valley. There are here also rocks of about 100 feet in height, composed of a species of breccia. We examined one near the church, at the extremity of the winding staircase, forming the descent into the valley, which was composed of large and small pieces of lava, some of them of many yards in length and depth, the angles being rounded, and the whole agglutinated together by a hard black earthy substance, that resisted all the force we could use to break off a piece of it. There are other rocks where the red lava forms the base, and these are soft.

Nodules of lava in concentric layers.

On our road from Funchal to the *Corral* we saw a stratum of large nodules or balls of lava, composed of concentric layers similar to the coat of an onion, and lying one above another

another; the stratum exposed was 30 or 40 feet in depth, and appeared to go down to the bottom of the hill.

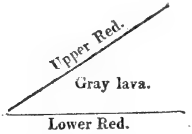
Coast to the west of Funchal.

We also examined the coast to the weetward of the town of Funchal. From the beach before the town to Illhoo Castle, and beyond it to the land called the *Punta de la Cruz*, the general character of the coast is as follows: the red stone is the apparent base upon which rests a bed of gray prismatic lava, the stratum being sometimes from 40 to 100 feet in depth. At times this gray lava rests upon a deep bed of ashes and pumice, agglutinated together like the *peperino* and *puzzolano* in the vicinity of Naples. The scoria at the surface is remarkably thick, and all the upper parts of the lava appear to be cellular. The general dip of the lava on the coast near Funchal is to the north, but near the fort of Illhoo, it forms with a mass of pumice, that is intersected with slight veins of carbonate of lime and zeolite, a rapid angle or curve of declination to the east. To the westward of the fort the lava is not found for a little distance, and there is nothing but deep beds of pumice and the agglutinated mass abovementioned. These beds of pumice are of various thickness, the deepest appearing to be about 4 feet, and alternating with that stratum which I have called *peperino*. In different cavities of the pumice bed, there are large deposits of black ashes. Toward the extremity of the strata the red stone appears on the surface in a more solid state, and lies in prismatic masses, the prisms being small, and not exceeding a few inches in diameter. Their substance is brittle and crumbles with ease. This stratum of red lava is of a short continuance. Passing a small brook, it dips rapidly to the westward, and in its place, the gray lava is found in a confused though sometimes prismatic form, and rises from the beach while the red lava still runs along the surface to the height of near 100 feet, the top being covered with a thick scoria.

Cascade to the east.

There is also in the vicinity of Funchal, to the eastward of the town, a fall of water, which, independent of the romantic beauty of the situation, merits being visited on account of the exposure of the two strata of lava in their relative position. The hills are composed wholly of lava, sometimes of a prismatic formation, the red and gray lavas being visible

visible on both sides of the valley. Near the head of it, a short distance from the cascade, the red stratum is at the bottom, and about 60 feet higher it reappears, and again, about 200 feet higher, alternating with the gray lava. The upper red lava dips rapidly to the south, and the strata are disposed in the following manner.



The rock, down which the cascade falls, is also intersected with a red stratum of about 3 feet wide, that traverses it, and dips to the westward, and is broken off by a broad dyke of gray lava. It appears about 30 feet higher, and dips again to the westward. The substance of the red rock in this place is hard, and it breaks into a columnar form, being by far the most compact of the red strata I met with in the island. I saw this red lava also in the island of Teneriffe, to the eastward of Santa Cruz, as well as in the neighbourhood of Orotava.

The island demands farther examination.

I have thus endeavoured to give you a slight sketch of that which appeared to me most deserving of attention in the island of Madeira. The short stay I was able to make there prevented a more accurate survey of the island; yet I saw enough to induce me to recommend a careful examination of the strata to those who may have more time than I had to spare, and more knowledge to estimate the value of that which was to be seen. To my mind the most interesting geological facts are: 1st. The intersection of the lava by dykes at right angles with the strata. 2dly, The rapid dips the strata make, particularly the overlaying of that of the *Brazen Head*, to the eastward of Funchal, where the blue, gray, and red lavas are rolled up in one mass, and lie in a position as if they had all slipped together from an upper stratum. 3dly, The columnar form of the lava itself reposing on, and being covered by, beds of scoriæ, ashes, and pumice, which affords a strong argument for the volcanic origin

Most interesting facts observed.

origin of the columns themselves; and 4thly, The veins of carbonate of lime and zeolite, which are not found here in solitary pieces as in the vicinity of Etna and Vesuvius, but are *amid* the lavas and *in* the strata of pumice and tufa, and are diffused on the lava itself, and occasionally crystallized in its cavities.

VII.

On the Decomposition of Sulphates by Heat: by Mr. GAY-LUSSAC, *Member of the Institute*[*].

Effects of heat on sulphates not such as commonly supposed.

THE object of Mr. Gay-Lussac, in the paper of which I am about to give an abstract, was to make known the effects of heat on sulphates; and the experiments he made for this purpose led him to results very different from those, that others had hastily promulgated on no better ground than probability. Beside extending our chemical knowledge, this paper has the advantage of presenting immediate applications to metallurgy, one of the sciences, to the promotion of which this Journal is devoted. It is in this point of view I shall present the labours of Mr. Gay-Lussac; and, that their application may be more obvious, I shall take the liberty of making a little change in the order of his experiments.

Roasting of sulphuretted ores.

He observed the phenomena that accompany the roasting of several metallic sulphurets, as well as the results of that operation: and his experiments farther confirm the opinion, that the formation of sulphates is unavoidable in the roasting of sulphuretted ores; and that the separation of the sulphur is not complete, till these sulphates have been decomposed. I shall first give his experiments on this subject, and then proceed to the decomposition of the sulphates.

Sulphate of copper made by roasting the sulphuret.

"I knew," says Mr. Gay-Lussac, "that in several manufactories sulphate of copper is made by roasting the sulphuret in reverberatory furnaces. At Goslar sulphate of zinc is prepared by a similar process. I attempted to imitate this process

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[*] Journal des Mines, vol. XXII, p. 325. Taken from the Mém. of the Society of Arcueil, vol. I.

process in the small way, and succeeded completely. Repeating it on sulphuret of iron, and on a mixture of sulphur and black oxide of manganese, it still afforded me sulphates. The temperature at which these sulphates were roasted was a red heat scarcely visible."

.... " The formation of sulphuric acid in the roasting of metallic sulphurets is not peculiar to them. It takes place also, and much more decidedly, in the roasting of alkaline sulphurets. I made some sulphuret of potash, which remained fluid at a low red heat as long as it was kept from the contact of air: but as soon as air was admitted freely, it began to thicken; and soon after it became solid, because a great deal of sulphate was already formed. I removed it from the fire to powder it, and exposed it anew to the action of heat. In less than an hour it had lost its sulphurous taste, and threw down only a white precipitate with acetate of lead. The sulphuric and muriatic acids extricated nothing from it. The sulphuret of barytes, treated in the same manner, likewise afforded me sulphate; but after three hours roasting in a red heat it was still sulphuretted. I examined these two alkaline sulphurets, and several metallic sulphurets, at various periods of roasting, without ever being able to extricate sulphurous acid from them. Consequently they must pass directly to the state of sulphates.

Sulphuric acid formed in roasting alkaline as well as metallic sulphurets.

They pass directly to the state of sulphates.

" It is easy to understand why the alkaline sulphurets pass immediately to the state of sulphates in roasting; for Mr. Berthollet has shown, in the Memoirs of the Academy, that the sulphite of potash is converted into sulphate at a red heat, exhibiting an excess of sulphur and of alkali. On treating sulphite of lead in the same way I obtained a great deal of sulphurous acid; which proves, that the oxide of lead has a much weaker action than potash on sulphuric acid. It is probable however, that some sulphate is formed with this oxide also; and if I cannot absolutely affirm this, it is because what I found in the residuum might proceed from sulphuric acid contained in my sulphurous acid.

Reason of this.

Oxide of lead acts more feebly than potash on sulphuric acid.

" All the metallic sulphurets however are not equally adapted to produce sulphates by roasting. A necessary condition for the formation of sulphuric acid is its having a base to combine with capable of condensing it sufficiently. I have taken sulphuret

All metallic sulphurets not alike in the formation of sulphuric acid.

Sulphuret of tin,

antimony, bismuth,

and silver.

sulphuret of tin, a metal that does not combine with sulphuric acid but very difficultly; and I have roasted it in a red heat for an hour, without any thing but sulphurous acid being produced. In like manner the sulphurets of antimony and bismuth, after having been roasted, presented me only with traces of sulphuric acid. It may be remembered too, that, if sulphates of these different metals be distilled, almost all the sulphuric acid passes over, as if it had been alone. The affinity of the metal for oxigen also has some influence. When sulphuret of silver is distilled in a stone retort with a strong fire, it is not decomposed: but if it be roasted, it decomposes with the greatest facility, sulphurous acid only is evolved, and the acid is not oxided.

Important circumstance, the condensation of the acid.

"Thus, then, an important circumstance, the condensation of the acid, modifies the phenomena presented by the metallic sulphurets in roasting. When the metals have the property of combining with sulphuric acid, and causing it to undergo a certain degree of condensation, sulphates are always formed. When, on the contrary, they combine but very difficultly with it, sulphurous acid only is formed, which flies off, as its great elasticity cannot be overcome by the affinity of the metallic oxides."

We will now proceed to the decomposition of the sulphates, which is the principal subject of the paper.

Common theory of the distillation of sulphates

erroneous.

...... "It was supposed," says Mr. Gay-Lussac, "that, on distilling a metallic sulphate, sulphuric acid was obtained, if the oxide were not susceptible of a farther degree of oxidation; or sulphurous acid, if its oxidation might be carried farther. It was thought too, that all the alkaline and earthy sulphates with excess of acid were brought back to the neutral state by the action of caloric, or entirely decomposed, yielding as the result only sulphuric acid. This theory is not the expression of facts accurately observed."......

Effects of heat on sulphate of copper.

......"The first sulphate subjected to the action of heat was that of copper. Water first passed over: but as soon as the retort began to grow red, white vapours of sulphuric acid arose, accompanied with a nebulous gas; smelling strongly of sulphurous acid, and in which a match kindled several times following, when it had been washed. This gas therefore

therefore was a mixture of sulphurous acid and oxigen. As the distillation proceeded, it appeared to me, that the quantity of sulphuric acid diminished in regard to the oxigen gas and sulphurous acid; and consequently that less acid escaped decomposition than at the commencement of the process. When nothing more was given out, I removed the retort from the fire. The oxide had not been fused, and it retained some acid; which proves, that at a higher temperature it would have been decomposed completely. The sulphurous acid and oxigen gas arose necessarily from the immediate decomposition of the sulphuric acid. The oxide of copper dissolved in fact in nitric acid without effervescence: and it is known, that it does not acquire a higher degree of oxidation in the distillation of its sulphate. The two gasses were to each other in bulk nearly as two to one."......

Decomposition of sulphate of iron by heat.

"Though sulphuric acid has long been prepared by distillation from sulphate of iron, and this has been an object of continual examination, attention has not been paid to several circumstances presented by its decomposition. It was known, it is true, that the sulphuric acid was always accompanied with sulphurous acid: but as the iron takes a higher degree of oxidation in this process, it was supposed to give rise to all the sulphurous acid by decomposing the sulphuric. Mr. Chaptal was, I believe, the first, who remarked, that a little oxigen also was obtained. In fact, the sulphate of iron undergoes the same decomposition by heat as the sulphate of copper; only the results are modified by this circumstance, as the metal is susceptible of a higher degree of oxidation, the proportion of sulphurous acid to oxigen gas evolved is greater."

Sulphates of manganese and zinc.

"The sulphates of manganese and zinc have exhibited to me precisely the same phenomena as the sulphate of copper; and therefore I shall not stop to describe them. I shall only observe, that the first of these salts may easily be prepared by calcining the black oxide of manganese in a red heat; for after this it dissolves readily in sulphuric acid."

Two sulphates of tin, antimony, & bismuth.

"When concentrated sulphuric acid is made to act on tin, antimony, or bismuth, two compounds are formed. One, which is very soluble, retains a great deal of acid, and very

very little oxide: the other, on the contrary, is formed of much more oxide than acid, and has little solubility."

Action of heat on them.

"If the first of these compounds be distilled, the sulphuric acid is volatilized as if it were alone: but if the second, in which the sulphuric acid is more strongly retained, be subjected to distillation, oxigen gas and sulphurous gas are evolved."

Products of sulphates retaining the acid in weaker degrees.

"The salts that have hitherto been examined have yielded different products, according to the strength with which the sulphuric acid is combined in them. When it is feebly retained, and has undergone no condensation, it is volatilized by heat as if it were alone, without being decomposed. If it be retained more forcibly, part only escapes decomposition, and the other part is converted into oxigen and sulphurous acid gasses. The insoluble sulphates, in which there is no sign of acidity, appearing to retain the acid with great force, it is essential to know what is the action of heat on them."

Those that retain it very forcibly examined.

Sulphate of silver.

"I put some sulphate of silver into an uncoated glass retort, furnished with a tube for collecting the gas. When it began to grow red, the salt melted, but was not decomposed. Having taken it out, I exposed it to a more violent fire in a stone retort; and then a great deal of oxigen gas was evolved, mixed with sulphurous acid, as Mr. Fourcroy announced. I did not perceive any dense white fumes as in the preceding experiments, because very little sulphuric acid was given out. When the process was finished, I found in the retort a button of silver completely reduced. Thus the sulphate of silver is decomposed by heat like the other sulphates, but it gives out more oxigen than they; on the one hand, in consequence of the reduction of the metal, on the other, because it yields very little sulphuric acid."

Sulphate of mercury.

"I afterward prepared some sulphate of mercury, by precipitating nitrate of mercury little oxided with sulphate of soda. The precipitate, washed and dried, was exposed to heat in an uncoated glass retort. Scarcely had this begun to grow red, when the salt entered into fusion, and it was soon decomposed. Very little sulphuric acid passed over; and mercury sublimed, with a little sulphate. The other products were sulphurous acid and oxigen gasses mixed in the

the proportion of 51·5 to 48·5. Though oxide of mercury requires a higher temperature for its reduction than the oxide of silver, the sulphate of silver is not so readily decomposed as that of mercury. This difference may arise in part, no doubt, from the difference in the affinities of the metals for sulphuric acid; but it must depend likewise on the great volatility of the mercury. In general it appears to me, that the affinity, the more or less easy reducibleness of the metals, and their volatility, must be considered as so many causes, capable of modifying the action of caloric on their sulphates."

Sulphate of lead.

"From my first experiment with sulphate of lead, not having employed a temperature sufficiently high, I concluded, that it was not decomposable by heat. But on having recourse to a reverberatory furnace surmounted with a chimney, I obtained its decomposition, and collected a great deal of oxigen gas and sulphurous acid. I did not perceive any lead reduced, or any very sensible quantity of sulphuric acid. It is very possible, that the separation of the acid was determined by the action of the stone retort, for it was coated internally with vitreous glaze*. Be this as it may, it is evident, that the sulphate of lead, which is insoluble and without excess of acid, and the decomposition of which cannot be promoted either by the easy reduction of the oxide, or by the volatility of the metal, is much more difficultly decomposed by fire than the acid and soluble sulphates. We may conclude therefore, that the insoluble sulphates resist the action of caloric more than those that are soluble, and that they give out much less sulphuric acid. But to render this conclusion still more general, we must take into consideration the more or less easy reduction of the metals, and their volatility."

Difference between the sulphates.

"It may have been observed, that the soluble sulphates yielded more sulphuric acid, than those that are insoluble. When the former have lost a part of their acid, their solublity is diminished, the acid remaining is held with more force

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* This suspicion of the author appears to me well founded. The great affinity of the oxide of lead for earths, and particularly for silex, must facilitate the decomposition of the sulphate, if it be not its sole cause.

Two portions of acid in metallic sulphates.

force, and then they must approach nearer the second. We can conceive two portions of acid in metallic sulphates: one, which is feebly retained, escapes without undergoing decomposition; the other, which is retained more strongly, supports a more elevated temperature, and is decomposed into sulphurous acid and oxigen gas. These two portions of acid, which we may conceive to exist in sulphates, are not the same in all: and it appears, all other circumstances being equal, that, the more soluble a salt is, and the greater its excess of acid, the more sulphuric acid is obtained in its distillation. Hence it is, that sulphuric acid may be prepared, as is done in Germany, by distilling sulphate of iron or of zinc. The insoluble sulphates would not be any way adapted to this purpose."

Alkaline and earthy sulphates.

Mr. Gay-Lussac has extended his researches to the alkaline and earthy sulphates: and he has found, that the salts with excess of acid comport themselves altogether as the metallic sulphates; that is to say, they give out sulphuric acid, sulphurous acid, and oxigen. Those that do not admit an excess of acid do not yield, even if sulphuric acid be added to them, either sulphurous acid or oxigen gas; and nothing is obtained in distilling them but the acid, that exceeded what was requisite for their neutralization. Such are the sulphates of barytes, lime, &c.

Roasting of sulphurets.

The author, applying all these facts to the roasting of sulphates, concludes.... " that, when the roasting is performed at a temperature equal to that at which the sulphates are decomposed, and still more when at a higher temperature, no sulphuric acid will be obtained, and all the sulphur will be given out in the state of sulphurous acid."

Another mode of decomposing sulphates by heat: the addition of sulphuret.

" Beside this mode of decomposing sulphates by heat, there is another, which is more convenient, as it does not require so high a temperature. It is that employed by Mr. Gueniveau to decompose the sulphate of lead, distilling it with the sulphuret of the same metal*. I satisfied myself, that by treating the sulphates of iron and copper in like manner with the sulphurets of the respective metals sulphurous acid only was obtained: which proves, 1st, that in this way we may separate the sulphur from metallic sulphurets

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* See Journal, vol. XVIII, p. 203, 204.

phurets and sulphates; 2d, that to effect this separation does not require so high a temperature as is necessary to decompose the sulphates."

Metallic oxide distilled with its sulphuret.

"Lastly in distilling a metallic oxide and its sulphuret, a great deal of sulphurous acid is obtained, and a little sulphate: but, if the temperature be sufficiently high, nothing remains but sulphuret, or merely oxide, according to the proportions employed."

Theory of the roasting of sulphurets.

"Now we are acquainted with the various circumstances, that may present themselves in the roasting of a sulphuret, it is easy to give the theory of it. To roast a sulphuret is, as the ultimate result, to separate the sulphur by the the simultaneous action of air and heat. The products obtained vary in general according to the temperature, and to the sulphuret roasted. At an ordinary red heat these sulphurets, the metal of which combines but difficultly with sulphuric acid, yield scarce any thing except sulphurous acid. Those, on the contrary, that condense it strongly, yield also, it is true, sulphurous acid; but at the same time sulphuric acid is produced, which remains combined with the oxides. At a very high temperature, superior to what would be necessary to decompose the sulphates, all sulphurets yield only sulphurous acid. When once sulphate is formed, it may be decomposed by a more powerful action of heat; or still better by those parts of the sulphuret, that have not yet undergone any change. In fine, when other portions have lost their sulphur, and are oxided, they are capable of taking the sulphur from those that still retain it, and converting it into sulphurous acid."

Examination of the composition of sulphuric and sulphurous acid.

The author has availed himself of the facility, with which, in the decomposition of sulphates, the sulphuric acid yields its two component parts, sulphurous acid and oxigen, to ascertain the composition of this acid. He has found, that 100 parts by measure of sulphurous acid gas take 47·79 of oxigen gas to form sulphuric acid; and, admitting the proportions given by Klaproth for the sulphate of barytes, he thence deduces the composition of sulphurous acid, which is 100 sulphur to 91·68 of oxigen.

Sulphuric acid decom-

Mr. Gay-Lussac afterward describes the ingenious experiment, in which he decomposed pure sulphuric acid simply posed by heat alone.

by heat, by passing it through an incandescent porcelain tube, thus obtaining oxigen and sulphurous acid gas. This experiment explains the decomposition of sulphates.

Different effects of heat on sulphates.

" All the neutral or acid sulphates, that lose their acid at a temperature below that required for decomposing sulphuric acid, will decompose without giving out oxigen or sulphurous acid. Those, on the contrary, that retain all their acid so strongly as to resist a heat equal or superior to that which decomposes sulphuric acid, will give out only oxigen gas and sulphurous acid. Lastly, as a compound does not equally retain every portion of its elements, there are sulphates, the decomposition of which will partake of the two preceding, and which will give out sulphuric acid, oxigen gas, and sulphurous acid."

Effects of the attraction of the base for the acid.

Thus then, setting aside the particular influence of a given base, we should consider the affinity, that unites the sulphuric acid to the base with which it forms a sulphate, as a force that enables it to support without being volatilized a heat sufficient to decompose it ; while, if it had been free, it would have withdrawn itself from the action of the heat, long before it experienced the degree necessary for this decomposition. But this force of affinity is likewise an additional obstacle to be overcome by the action of heat : and this obstacle is very considerable, when the base undergoes no alteration by heat, as the oxide in the sulphate of lead, and the fixed alkalis in their sulphates.

The paper contains many other very delicate researches of considerable importance to the chemical theory of several phenomena, but I shall here finish my abstract with the following

General deductions.

" *Conclusion.*

Decomposition of the metallic sulphates,

" 1st. All the metallic sulphates are decomposable by the action of heat, affording results that depend on the affinity of the metals for sulphuric acid. The sulphates in which the acid is but little condensed yield only sulphuric acid by distillation. Those that retain it more strongly, and are insoluble, give out sulphurous acid and oxigen gas. Lastly, those that have properties common to both the preceding

preceding, and which are acid and insoluble, give out sulphuric acid, oxigen gas, and sulphurous acid.

metallic sulphurets,

2d. In the roasting of metallic sulphurets, the products vary according to the temperature, and according to the sulphuret. At a very high temperature, sulphurous acid is given out in quantities so much the greater in proportion as the oxide is capable of condensing it more strongly; and when it has but a very weak affinity for it, no sulphurous acid is formed.

earthy sulphates,

"3d. All the earthy sulphates, which are naturally acid, are decomposable by fire, giving out sulphuric acid, oxigen gas, and sulphurous acid.

and alkaline sulphates.

"4th. The neutral alkaline sulphates are not decomposable by fire, that of ammonia excepted: but when they are capable of forming crystallizable salts with excess of acid, condensing it, and diminishing its volatility, part of this excess of acid is changed into oxigen gas and sulphurous acid.

Action of phosphoric and boracic acid.

"5th. Sulphates treated in the fire with phosphoric or boracic acid yield sulphuric acid, oxigen gas, and sulphurous acid.

Composition of sulphuric and sulphurous acid.

"6th. Sulphuric acid is composed of 100 parts sulphurous gas, and 47·79 oxigen gas, by measure.

"7th. 100 parts of sulphur by weight require 50·61 of oxigen, to convert them into sulphurous acid, and 85·70 to form sulphuric acid.

Sulphuric acid decomposed by heat.

"8th. Sulphuric acid is decomposable by heat alone into oxigen gas and sulphurous acid gas.

Manufacture of sulphuric acid.

"9th. A great elevation of temperature is not favourable to the production of sulphuric acid; on the contrary, it is detrimental to it. The instant of the combustion of sulphur, sulphurous gas only is obtained, whether it take place in the open air or in oxigen gas; and the sulphuric acid obtained in leaden chambers must be the result of the action of nitrous gas and of the air on the sulphurous acid, as well as of the action the last mentioned gas exerts on oxigen by means of water.

VIII.

Remarks on some useful Applications of Meteorological Observations to Nautical Prophylactics: by F. Peron, *Naturalist of the Voyage of Discovery to the Austral Lands, Correspondent of the Imperial Institute, &c.**

Meteorological observations

METEOROLOGICAL instruments, it is true, are but modern acquisitions to science; yet observations with them have been pursued so steadily, and in so many different climates, that we have reason to be equally astonished at the imperfection of their theory, and of the few useful applications they furnish. Perhaps the chief reason of this is to be found in the nature of the theatre on which these experiments have been made almost exclusively to the present day. In fact, how many joint causes concur, in the midst of our continents, to complicate results essentially so different and so delicate! The observer on the ocean, on the contrary, left to the exclusive influence of the air and water, may give greater precision and developement to his experiments, and deduce conclusions more exact, and more general in their application. It is not my intention here to enter into what I had myself an opportunity of doing in this way amid so many seas, repeating my observations daily at six o'clock morning and evening, at noon, and at midnight; but to confine myself to a few experiments, which appear to me more immediately connected with the health of mariners.

on land

and at sea.

Observations connected with the health of seamen.

In this class I conceive may be placed a series of tables of the variation of the barometer, hygrometer, and thermometer, and of the temperature of the sea at its surface, taken at every hundred leagues for 95 degrees of latitude; an undertaking that appears to me as new, as it is capable of becoming at a future period of importance in preserving the health of seamen. By multiplying tables of this kind, constructed with as much care as I employed in it, we should soon have a kind of *meteorological hydrography*, equally indispensable to the natural philosopher and the physician. The latitude and longitude of a part of the sea being given,

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* Journ. de Phys. vol. LXVII, p. 29.

we might find by these tables the general state of the atmosphere and water pertaining to it; and thus ascertain its influence on the mariners who traverse it, and the animals that people it.

Chief cause of scurvy.

In my meteorological labours, however, I had in view an object still more essential, and more immediately useful to seamen. Theory and experience, in fact, seem to unite to prove, that the chief, if not the exclusive cause of scurvy is moisture, whether combined with heat, or with cold. This opinion, which Mr. Kerauden has particularly unfolded in his excellent dissertation on this subject, and which my own disasters confirmed, led me to consider it as a duty, to direct my inquiries to this subject; and to pursue them with the more care, as I had the advantage of being the first to traverse the seas with an hygrometical instrument capable of being compared with others, that of de Saussure, executed by Richer. Besides, Mr. Hallé, to whose instructions and advice I am so much indebted, recommended observations of this kind to me at my departure; and the desire of testifying my gratitude to him, at least by my zeal, was a powerful motive with me to undertake them.

The author's observations.

Independant of my other meteorological researches, therefore, I imposed on myself the task of making particular experiments on the comparative state of different parts of our vessel. Every ten days, at noon and at midnight, I went from the poop beneath the quarter deck and forecastle*, thence to the gunroom, and lastly to the hold, where I caused myself to be shut up for half an hour, to obtain results more exact, and more accurately comparable. The captain, who had requested me to communicate to him the results, and transcribed them into his journal, always afforded me, as I must candidly confess, every possible convenience for this purpose; and in this respect, at least, he was pleased to second my endeavours.

Information derived from them.

My observations toward the close of october 1800 showed me, that the matter of the vomitings of several persons attacked with seasickness, and too much crowded in the gunroom, had altered the air in a dangerous manner by its decomposition

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* *Sous le gaillards*, in the original; though, from the table, I suppose the author means the between-decks. C.

decomposition. The tempestuous weather we had experienced for several days not having allowed us to open the ports, this occasioned other inconveniencies, not less serious than those from the cause just mentioned. The thermometer, which without was scarcely so high as 8° [46·4° F.], in the gunroom was at 15° [59° F.]; and the hygrometer rose from 78° to 96°. Lastly, a considerable portion of sulphuretted hidrogen gas evinced its presence, not merely by its peculiar smell, but by the yellow hue almost every article of silver in the place contracted. On the report I made to the captain, the taking down all the hammocks was strictly enforced, the decks were carefully swept, fumigations were repeatedly made, the ports were directed to be opened, a windsail was employed, and in a few days the former salubrity of the gunroom was restored.

Another instance.

In my report of the 21st of november I again apprised the captain, that the excessive heat I observed during the night in the gunroom indicated, that too many persons slept in it; and as this damp and hot temperature could not fail to be prejudicial to all, it was indispensably necessary to remove some of them. The captain reduced the number from twenty-four to fifteen or sixteen, and the results I obtained the following night confirmed the justice of my observations.

On the 11th of december, on going into the hold, I perceived a sour, nauseous, and extremely disagreeable smell; and my candle burned with difficulty. I soon learned, that a cask of wine had been leaking for some days; so that I had no difficulty in accounting for the smell, and for the great proportion of carbonic acid gas. I hastened to acquaint the captain with this; and recommended pumping the ship dry, throwing fresh water into the well, and pumping it out repeatedly. Orders were immediately given for this purpose, and the ship was once more rendered sweet by my advice.

A third.

My experiments at the close of december afforded me a triumph peculiarly flattering, as they served evidently to prove the importance of meteorological observations on board ships. The storeroom of the captain and principal officers was filled with all sorts of provision put on board in Europe; fruit dry and preserved, *adaubages* in large quantity, fats, oils, &c. On going into it with my instruments, I was equally

equally surprised and grieved at the results they offered me: and I gave an account of them to the captain in the following words.

Report to the captain.

"A noisome smell, and excessive heat and moisture, conspire to render the storeroom unwholesome. On attempting to make my usual experiments there, I found myself so ill and faint, that I could not finish them. My thermometer however had already risen to 27° [80·6° F.], and the hygrometer was beyond the point of saturation. The flame of the candle was feeble and pale, indicating the presence of a great quantity of gas unfit for respiration. It is true no one lives in the storeroom: but is there not reason to fear, that such of the men as are obliged to work there will soon feel its fatal effects? It appears to me, therefore, indispensably necessary, to empty this place for a few days, and to endeavour by fumigations, sprinkling with cold water, ventilating, and repeated sweeping, to renew the air, and remove its humidity. This precaution is as necessary for preserving the provision, as for the health of the men: for there can be no doubt, that many articles are already spoiling, and others will soon be so, from the high temperature and extreme moisture combined. At any rate, if the nature of the service will not allow these means to be employed, it is to be wished, 1, that the men were forbidden to go alone into the storeroom; not only to prevent suffocation, of which there are but too many instances in similar cases; but to obviate the more fatal effects that might follow, should such an accident take place, from the person's being left, or from the candle: 2, that the men should have their allowance of wine increased one fourth; for it is to be feared, that, coming out of the storeroom in a profuse perspiration, some accident might happen from their drinking a large quantity of water to quench the thirst produced; an effect I could not avoid myself, notwithstanding the short time I staid, and my remaining almost perfectly still."

Neglected from the assertion of an officer.

The captain, alarmed at this report, immediately sent for the officer, under whose care it was, and communicated it to him. He asserted, that it was altogether erroneous, that the observations were of no consequence, that the stores were in good condition, &c. Accordingly nothing was done: but

Consequence.

but in a few days after one of the strongest of the men, being employed in this storeroom, fainted away, and was with difficulty recovered. This accident, which I had so clearly foreseen, determined the captain. He ordered the storeroom to be cleared, and the stores to be examined. More than half the *adaubages* were rotten: all the dried fruits had fermented; the oils and fats had run from all the vessels, and some of them were obliged to be thrown into the sea. It was found necessary to clean the storeroom in the manner I at first proposed, and I set the greater value on my observations.

Putrefaction of potatoes,

On the 1st of january, 1801, I found in the gunroom a large chest of potatoes, belonging to the gunner, which, being stowed under the tiller, had rotted there, and diffused a noisome smell throughout that close place. With this I acquainted the captain, who ordered them to be thrown into the sea, and the gunroom to be cleaned and fumigated.

and of carrots.

On the 10th of the same month I found a cask of carrots, belonging to the midshipmen's mess, which had been stowed in the gunroom, and, having been forgotten, had rotted there.

Old cheese.

On the 20th I procured a large chest of old cheese, that had just been opened in the gunroom, to be removed to a place that was more roomy, and better ventilated.

Sulphuretted hidrogen gas in the hold,

The same day the extreme heat and moisture in the hold, and the suffocating smell of sulphuretted hidrogen prevailing there, rendered it incumbent on me to acquaint the captain with it; and to request him, to order the water to be pumped out, and fresh to be thrown in. This was immediately done.

and the gunroom.

It has been seen, that sulphuretted hidrogen gas was several times produced in abundance in the gunroom, and still more in the hold: perhaps it may be necessary, to point out its origin.

Its origin.

However nicely the seams of a ship may be caulked, it is impossible but more or less water will penetrate them, particularly in hard gales, when the seams open, as the sailors say, from the shock of the waves. Hence independent of accidents, there is a permanent cause of more or less water accumulating at the bottom of the ship. In the same place are stowed those pigs of iron, that are employed as ballast. From the simple action of water on this metal, an evolution of of hidrogen gas must take place in the hold; but this action is increased in consequence of the salts with which the water is impregnated, and the high temperature generally prevailing in the hold: and this hidrogen gas receives from various vegetable or animal substances in a state of decomposition, in the place where it is evolved, those noxious qualities, and that sulphuretted smell, of which I have several times spoken.

Remedy for this.

It is easy however, in a ship in good condition, to prevent, if not the formation of this gas, at least its injurious effects. This is to be accomplished chiefly by frequently pumping the ship out dry, and then throwing in a large quantity of water, both to wash out and carry off all the substances in a state of decomposition, and to cool the hold. But in ships where these little attentions are neglected, the black oxide of iron, which is formed in abundance by the decomposition of the ballast, mixing with the remains of vegetable and even animal substances in a state of fermentation, produces a kind of stinking black mud, the exhalations of which have frequently produced fatal diseases on board ships.

Necessity of attention to the hold.

Hence it is easy to perceive, how much this part of the ship should be an object of attention to the officers and surgeons. From it arise most of those noxious gasses, and offensive smells, that render living on shipboard so unpleasant. The thermometer and hygrometer constantly afforded me valuable data respecting the state of this place with regard to its salubrity; the evolution of gas, and consequently the decomposition of water and of the animal or vegetable substances, being pretty generally in the ratio of the temperature and moisture combined: their use therefore cannot be too sedulously recommended. The same may be said of the preventive means I have mentioned, to which must be added above all the apparatus for oximuriatic gas; as here in particular it may be employed with the greatest success, and without any inconvenience.

The author obliged to cease his observations,

The observations I have just mentioned were nearly the last of the kind I could make. Notwithstanding the request of the captain himself, I was obliged to sacrifice them to private considerations, which it would be useless to mention here. So true it is, that to have both the means and the de-

sire

though their advantages had been sufficiently proved.

sire of doing good is not always sufficient. I consoled myself however by reflecting, that I had acquired a certainty of the advantages of meteorological observations on board ships; and I am still firmly persuaded, that the continuation of similar labours, and the particular inspection to which they would have led, would have been of great service during the rest of the voyage; and though they would not have prevented the dreadful scurvy, that made such ravages among our crew, they would perhaps have checked its progress. The little good I was able to accomplish, while it proves the utility of such experiments, will no doubt stamp a due value on the counsels of our naval officers of health, and may thus contribute to the improvement of nautical medicine, too little acquainted hitherto with the assistance it may derive from that application of natural philosophy to the healing art, which Mr. Hallé has so successfully pointed out.

Advantages of having meteorological instruments on board all ships.

What for instance can be more easy, and at the same time more necessary, than to place in the care of the surgeon of every ship a good marine barometer, a few thermometers, and a couple of hygrometers? What series of valuable observations on the constitution of all the climates on the globe, and what important materials for nautical medicine and the science of natural philosophy, would thus be acquired at a trifling expense! How advantageous also would these instruments be to mariners themselves! I do not speak merely with respect to a more accurate judgment of changes in the atmosphere, which the barometer and hygrometer would frequently furnish, and which established the reputation of these instruments with the officers of our expedition; but with regard to their health, and its preservation. Beside what I have already said, how often, for instance, at an anchorage, or when tents were pitched ashore, the changes of the weather having been shown to be dangerous by our meteorological instruments, might the crew have been preserved from their effects at a trifling expense, and without inconvenience! Thus at the head of the Bay of Seals, where I observed variations of 20° [36° F.] of temperature and 33° of humidity within the 24 hours, those of the crew of the Naturaliste who slept on shore being almost all attacked with a violent diarrhœa, need we seek any other cause for it than the

Setting up tents ashore.

the daily and alarming vicissitudes of the weather? And when the results of our meteorological observations had pointed out the true etiology of this kind of epidemic, would they not have led a sagacious observer to the means, equally simple and efficacious, which the natives of this coast, afflicted no doubt by such fatal changes, have contrived to diffuse around them, to obviate their dangerous effects? means which were probably the fruit of too long experience, and too long misfortunes to these rude people*.

Ordering up hammocks.

Regulations of diet.

Washing decks.

By the assistance of the same instruments how often should we be led to act with more caution in regard to exposing the seamen to the weather, and the daily practice of ordering up all the hammocks! How often might we not introduce with equal advantage and facility some salutary variations either in the distribution of the provision, or in the succession of the various aliments, with which a ship is furnished! On seing daily the thermometer sink instantaneously several degrees, and the hygrometer indicate 8° or 10° of additional moisture, precisely when, by order of the captain, the deck, forecastle, quarterdeck, and great cabin, had just been sluiced with sea water for the purpose of cleaning them, what commander, less opinionative than ours, but would have been eager to put a stop to such a fatal practice? what officer but would have preferred simple dry scraping to those monstrous ablutions with salt water, which daily filled the interior of the ship with a damp and cold atmosphere, and in my opinion contributed not a little to the rise of that terrible epidemic scurvy, which destroyed our crew on the coasts of Napoleon Land, and Van Diemen's Land?

Attention to little circumstances important.

They who are unacquainted with the minutiæ of long voyages may think most of these precautions useless: but if they reflect on the importance attached to them by the most celebrated navigators, and particularly by the most successful, they will be convinced, that an attention to a multitude of little things, apparently indifferent, especially if considered singly, form the essential basis of that science of preserving the health of seamen, sanctioned by the valuable success of

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* For an explanation of this passage, see chap. XXX of the Narrative of our Voyage, where I have described the singular habitations of the people of Endracht's Land.

Bougainville, Cook, Vancouver, and Marchand. In the ship of the last of these in particular the art of preventive medicine displayed in a striking manner what may be expected from these little attentions. Mr. de Fleurieu, in his account of the voyage I have just mentioned, has given a just eulogium of the surgeon of the *Solide*, Mr. Roblet; and when I was at the Isle of France I had an opportunity of becoming acquainted with this gentleman, and receiving from him a confirmation of the useful hints here given for the improvement of nautical medicine, which is so greatly indebted to him. His happy employment of warm sand baths for the cure of the scurvy at sea, and the striking success with which it was attended, confirming that of Mr. Bellefin, surgeon of the *Naturaliste*, must render his name dear to every lover of the art and friend of mankind.

While paying to this gentleman, equally learned and modest, the tribute of praise due to him, I cannot avoid noticing a remarkable expression of Vancouver's, well adapted to show the importance of such services, too little known and too soon forgotten. After having spoken of the improvement of this branch of physic, which he ascribes particularly to the beneficient genius of Cook, he adds: "it is to this *inestimable* improvement, that Britain is *in great measure* indebted for the high rank she at present holds among nations."

If we must learn the principles of preserving the health of seamen from a nation, to which men are so valuable, because its population is so greatly disproportionate to its establishments; it belongs to the celebrated Society*, to which I have the honour of addressing myself, to make them known, and render them useful to our country.

Table of Experiments made to ascertain the relative proportions of humidity in different parts of the ship le Geographe.

Meteorological observations at sea.

Oct. the 22d, 1800, at noon, lat. 49° 36′ N., long. 6° 44′ W. [4° 24′ W. from London], after several days of tempestuous weather, that did not allow the ports to be opened in any part of the ship.

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* The Medical School of Paris, in whose memoirs this paper is inserted.

On

	Thermometer.		Hygr.
	Cent.	Fahr.	
On the poop	8·5°	47·3°	78°
In the gunroom, the ports shut	14·5	58·1	96

Meteorological observations at sea.

Oct. the 23, at noon, lat. 48° N., long. 8°43′ W. [6°23′] the cessation of the tempestuous weather having allowed the ports to be opened, and the different parts of the vessel to be cleaned.

On the poop	11·5	52·7	85
In the gunroom	13	55·4	89

Nov. the 1st, at 8 A. M., in sight of the island of Teneriffe.

On the poop	16·5	61·7	78
In the gunroom, the ports open	17·5	63·5	81
Under the between-decks	18·5	65·3	85
In the hold	18·9	66·02	90

Nov. the 19th, at 8 A. M., lat. 13° N., long. 22° W. [19° 40.]

On the poop	21	69·8	93
In the gunroom, the ports open	22	71·6	94
Under the between-decks	22	71·6	96
In the hold	24·5	76·1	98

Nov. the 22d, at noon, lat 8° N., long. 20° W. [17° 40′].

Poop	24·5	76·1	90
Between decks*	24·4	75·92	94
Gunroom	24·9	76·82	92
Hold	22·6	72·68	97

Nov. the 30th, at midnight, lat. 6° 38′ N., long. 19° W. [16°40′].

Poop	22·6	72·68	92
Between-decks	23	73·4	93
Gunroom	24	75·2	96
Hold	21·5	70·7	96

Dec. the 10th, at noon, lat. 2° N., long. 20 W. [17°40′].

Poop	21·8	71·24	93
Between-decks	22·5	72·5	98
Gunroom	22·3	72·14	96
Hold	23·7	74·66	101

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* Here and throughout the remainder of the table, the expression is changed by omitting the preposition: but, as it is omitted equally for the other parts, I suppose the author means the same here by *entrepont* simply as before by *sous l'entrepont*. C.

The

Meteorological observations at sea.

	Thermometer.		Hygr.
	Cent.	Fahr.	
The same day at midnight, lat. and long. the same.			
Poop	19·8°	67·74°	97°
Between-decks	23·6	74·48	100
Gunroom	22·6	72·68	97
Hold	24·5	76·1	103
Dec. the 21st, at noon, lat. 11° S., long. 31° W. [28°40′].			
Poop	21	69·8	91
Between-decks	21·4	70·52	95
Gunroom	21·8	71·24	92
Hold	23	73·4	100
The same day at midnight, lat. and long. the same.			
Poop	20·2	68·36	91
Between-decks	22	71·6	96
Gunroom	21	69·8	90
Hold	23·2	73·76	103
Dec. the 30th, at noon, lat. 23° S., long. 26° W. [23°40′].			
Poop	19·1	66·38	90
Between-decks	20	68	92
Gunroom	19·6	67·28	91
The same day at midnight, lat. and long. the same.			
Poop	18	64·4	92
Between-decks	19·3	66·74	95
Gunroom	19·7	67·46	94
Jan. the 10th, 1801, at noon, lat. 30° S., long. 21° W. [18°40′].			
Poop	18·5	65·3	81
Between-decks	19·8	67·64	86
Gunroom	18·9	66·02	84
The same day at midnight, lat. and long. the same.			
Poop	16·4	61·52	88
Between-decks	20·2	68·36	89
Gunroom	17·4	63·32	90
Jan. the 20th, at noon, lat. 33° S., long. 3° W. [0°40′].			
Poop	15·5	59·9	80
Between-decks	15·8	60·44	85
Gunroom	16	60·8	83
Hold	21·2	70·16	92
The same day at midnight, lat. and long. the same.			
Poop	13·8	56·84	79

Between-deck

	Thermometer.		Hygs.
	Cent.	Fahr.	
Between-decks	16·5°	61·7°	87°
Gunroom	15·4	59·72	78
Jan. the 30th, at noon, lat. 35° S., long. 7° E. [9°20′]			
Poop	16·6	61·88	92
Between-decks	16·7	62·06	91
Gunroom	15·9	60·62	89
The same day at midnight, lat. and long. the same.			
Poop	14·7	58·46	102
Between-decks	17·3	63·14	98
Gunroom	17·6	63·68	96

General results.

General conclusions.

On comparing these different observations we find, with respect to the temperature,

Temperature.

1, That the temperature of the air in the interior of the vessel is generally 3° or 4° [5·4° or 7·2° F.], higher than that of the exterior air.

2, That the difference of temperature between the gunroom and the between-decks was scarcely 1° [1·8° F.], when, by opening the parts and employing windsails, care was taken to keep up a salutary current of air in the gunroom.

3, That, circumstances being the same, the hold of the ship is the hottest part. The exceptions to this rule appeared to me to correspond with its being washed out, which was done by introducing into it repeatedly large quantities of water, the happy effects of which were both to clean and to cool this place.

Moisture.

With respect to *moisture* we find from the preceding experiments,

4, That there is habitually more dampness in the vessel, than in the open air. The few exceptions to this rule depended on slight variations in the atmosphere, by which the outer air was naturally affected sooner than that within the ship.

5, That the difference of moisture between the air within and without the ship is generally greater than that of the temperature, it frequently amounting to 10° or 12°.

6, That, circumstances being the same, the gunroom is less damp than the between-decks, and this singular result appeared to me to be owing entirely to those fatal inundations which were employed daily between decks, while the gunroom was cleaned dry, the vicinity of the powder preventing the introduction of water there.

7, Lastly it follows from the experiments, that, if the hold be the hottest part of the vessel, it is also the dampest, and that on both accounts it ought to be considered as the most unwholesome.

IX.

Account of the Vicuna: by Mr. LARREY, *Physician in Chief of the Imperial Guard, one of the Inspectors General of Military Hospitals, &c.**

Two young vicunas brought to Spain.

A Merchant of Cadiz, a lover of natural history, brought from Peru two young vicunas, a male and female. He first landed them at Cadiz at the beginning of the year 1808; and toward the end of April in the same year conveyed them to Madrid. They did not appear to be inconvenienced by the change of climate, or difference of food, till the weather began to get very hot. They were very badly lodged in a small, dark room, not well ventilated. In this hole I had an opportunity of seeing them, examining their figure and gait, and studying their manners and habits.

Death of the female,

The female, which was larger and older than the male, being about three feet high, died soon after, during a short tour I made in the neighbourhood of Madrid to inspect the hospitals. I could not learn the cause of her death; but, as the body quickly putrefied, it was thrown into the fields.

and soon after of the male.

On my return I hastened to visit the two strangers, but found only the male, sad, dejected, and uttering plaintive cries at the slightest touch. He ate but little, and remained constantly squatted on his four legs: but he appeared better and more lively in the cool of the evening and morning, which he seemed to seek; while in the heat of the day he

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* Sonnini's Bibl. Phys. Econ. Sept. 1809, p. 168.

was

was overcome, and breathed with difficulty. Thus melancholy and unwell he passed the first week of June; and about the 15th symptoms of inflammation appeared, a few days after which he died.

Dissection of the latter.

Foreseeing this event, I had obtained permission of the owner, to dissect the animal after his death, and dispose of his skin. My first care was to remove this with due caution, that I might be able to preserve the natural shape of the animal in stuffing it: after which I proceeded to examine the viscera, the articulations, and the general disposition of the muscles.

Linea alba.

On opening the abdomen I found the linea alba, or aponeurosis uniting the large muscles, was extremely strong, and much thicker than is usually observed in other quadrupeds.

Abdominal viscera.

The viscera of the abdomen exhibited marks of the inflammation I have mentioned. The stomachs were distended with gas, and the mucous membrane inflamed. The epidermis of the ruminating stomach had already peeled off, and the intestines were nearly in the same state. There was no urine in the bladder. The epiploons exhibited nothing but very thin membranous skins destitute of fat.

Stomachs.

The distribution and figure of the stomachs were the same as in the camel. The second [*le bonnet*] was full of vesicles, from which a serous or aqueous fluid issued abundantly. The paunch and the other two stomachs did not differ in the interior form of their cavities from those of the camel. The cellular stomach [*la poche à cellules*] was remarkable for the internal arrangement of the cells; they having apertures of communication furnished with membranous valves, which no doubt may still be discovered in the dried stomach of the animal. The last stomach is united to a portion of intestine, which may be considered as the duodenum. This was continued in another intestine of equal bulk, which, after forming an arch in the circumference of the abdomen, terminated in the left lumbar region in a *cul-de-sac.*; whence issued another intestinal tube, very slender and smooth, and forming ten or twelve concentric circles in the space made by the former. The circumvolutions were attached to a common mesentery. This slender

Intestines.

intestine afterward made a thousand circumvolutions in the abdomen, terminating at length in another cæcum, without an appendage like the former; whence issued a portion of intestine of considerable bulk, which, after forming two or three curves in the manner of a colon, terminated in the rectum.

Thus it appears, that the vicuna has three sets of intestines, the first and third large, and the middle slender.

No worms in them.

I met with no worms in the intestines, the infinite windings and intersections of which would appear favourable to their formation.

Liver.

The liver, which I did not at first perceive, was found deeply concealed behind the stomach, and attached by very close membranous ligaments to the crura of the diaphragm, and to the corresponding dorsal vertebra. It was of very small bulk, of an oval figure, flattenned transversely, and exhibiting two lobules at its anterior edge. It was destitute of a gall bladder; and the bile was taken directly from the liver by a duct, that conveyed it into a portion of the duodenum. This duct and the vena portæ crossed each other.

No gall bladder.

Spleen.

The spleen, which was likewise very small, and of a rounded form, was situate in the left lumbar region, contiguous to the kidney of the same side. These two organs were enclosed in one common duplicature of the peritoneum.

Lungs.

The lungs exhibited nothing remarkable. They partook of the general inflammation, and the bronchiæ were filled with a frothy sanguineous fluid. The trachea and larynx had the same figure and organization as those of the camel.

Heart.

The heart, which was of a size proportional to the animal, formed almost a perfect cone; only its point, which was very accute, curved upwards and to the left, and the cavity of the ventricle on that side reached to the point.

I did not see the brain, as I wished to preserve the skull entire.

After having examined the viscera of the animal, I proceeded with the dissection. The cartilaginous state of the extremities of the bones did not allow me to make an artificial skeleton of them.

Among the bony parts of the thorax the sternum merits some attention. It is in a horizontal plane, like that of the camel,

camel, thick, rounded on its outward surface, and covered in the natural state with a fatty substance of a close texture. The integuments on this part are much thicker than elsewhere. This bone was intended to serve as a point of support for the animal when lying down; and the almost constant use he made of it during his illness had pressed the extremities of the sternocostal cartilages inwards. The middle, spinal apophyses of the vertebræ formed a gibbosity, which, if it had been covered externally by a little fat, would have resembled the bunch of the camel. The remainder of the vertebral column inclined imperceptibly toward the pelvis, which was of itself inclined and of small capacity. The edges of the haunch bones were cartilaginous. The sacrum was lengthened by a series of caudal vertebræ, so as to form a tail in every respect similar to that of the camel.

Hump on the back.

Tail.

The scapulæ, very thin and without clavicles, were connected with the trunk only by means of scapular muscles, as in the camel. The cervical vertebræ formed a very long column, curving from below upward, so as to give the neck the same figure and length in proportion to the size of the animal as those of the camel. As in the latter these vertebræ had no spinal apophyses; but a very strong cervical ligament, extending from the occiput to the spine of the first dorsal vertebra, supplied their place for the attachment of the muscles, and kept the head and vertebræ in their proper position. The anterior face of these vertebræ had a longitudinal hollow, adapted for the reception of the trachea and œsophagus.

Shoulder blades.

Neck.

The head of the vicuna has the same shape and external characters as that of the camel. The jaws have the same number of grinding teeth. The lower has only four cutting teeth, the middlemost of which are the most prominent. The upper has none, as in other ruminating animals.

Head.

The fore and hind limbs in every respect resemble those of the camel.

Limbs.

The joints of the limbs form a perfect ginglymus, admitting a direct and complete flexure of one part against the next, so that this animal, like the camel, bends all his four legs underneath his breast when he lies down: and

Joints.

and this double flexure is the effect of the natural structure of the limbs, as in the camel, which I had an opportunity of studying in Egypt, and of examining from its birth to its adult age. It is not therefore the result of training.

Feet. The feet of the vicuna are terminated by two, long, narrow, soft soles; and have much resemblance to the feet of young camels.

Ears. Wool. The outward figure of the head perfectly resembles that of a young camel, except in the ears, which are erect and smooth like those of a kanguroo. The neck, body, and limbs are similarly disposed; and the body, like it, is covered with a fawncoloured, silky wool, but of extreme fineness. From it may be made stuffs as soft and fine as the shawls of Casimire. This tufted fleece keeps the animal so warm, that it seeks and prefers for its habitation the summits of mountains covered with snow. If the ears of this animal were uniformly cut, it would exactly resemble a camel two or three months old.

Manners. The vicuna has the same cries as the camel, the same gait, and nearly the same disposition. It is extremely shy and timid. It utters plaintive cries at the least unpleasant sensations; and when too much alarmed its eyes are filled with tears. The very active movement of its tail and ears indicate its different sensations. It is very gentle and caressing when tamed.

A little camel. The resemblance the vicuna bears to the camel in its external figure, internal stucture, and qualities, would lead me to call it *camelus parvus auribus rectus,* the little camel with erect ears.

The owner of the animal gave me the following account of the Peruvian mode of hunting it.

Mode of hunting it. The vicunas commonly inhabit the frozen summits of the high mountains of the Cordilleras. Several of the inhabitants assemble together to hunt them. They first surround the mountain where they are most numerous; and by means of mournful cries, or the discordant sound of large wind instruments, as hunting horns, they terrify the animals, who take flight to the summit of the mountain, where no doubt they suppose themselves inaccessible. Here the hunters form a line of circumvallation with stakes, on which are small red flags. These stakes are connected with each other by cords placed pretty close. Two or three hunters then attack the herd, which disperses. Frequently some of the vicunas are surprised, and the rest rush down the mountain, but as soon as they reach the fence, instead of leaping over it, which they might easily do, terrified at the colour of the flags, they crouch down in the snow, or in holes, where hunters posted for the purpose easily take them. After tying their legs, they carry them to a convenient place, to sheer their fleeces. If the animals be old, they let them loose: if young, they take them to their huts, keep them, and train them to carry burdens, loading them in the same manner as camels. They cannot live in the burning plains of America, and accordingly the inhabitants of the mountains alone can keep them. This no doubt is the reason why the animal has been hitherto so little known.

Domesticated.

When the animal is young, its flesh is good eating; but the wool is justly in high estimation. The merchant assured me, that it was seldom sent to Europe pure, being almost always mixed with other wool of less value.

Flesh and wool.

The latter commonly adulterated.

I think with him, that it might be naturalized and breed in the Pyrenees, on the summit of which the snow scarcely ever thaws; particularly as the pasture there is excellent.

The Pyrenees adapted to it.

X.

Observations on the Hydrosulphate of Soda, and improving the Preparation of the Soda of the Shops: by Mr. FIGUIER, *Prof. of Chemistry at Montpellier**.

THE 41st vol. of the Ann. de Chim. contains a note by Mr. Vauquelin on hidrosulphuret of soda accidentally found by him in the mother water of a solution, from which he had obtained crystals of carbonate of soda†. The soda he lixiviated was from the manufactory of Messrs. Payen and Bourlier; and he supposed, that they had not employed carbonate of lime enough to saturate all the sulphur arising from

Hidrosulpuret of soda found mixed with the carbonate.

Apparent source of it.

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* Ann. de Chim. vol. LXIV, p. 59.

† See Journal, vol. I, p. 303.

the decomposition of the sulphate of soda by charcoal; and that this was the cause of the formation of the hidrosulphuret. This was probably the case, and no doubt those manufacturers availed themselves of the discovery of Mr. Vauquelin. The following observations however show, that the hidrosulphuret exists also in the soda obtained in the combustion of the plants, that furnish this alkali.

But it exists in the crude soda.

Sulphuretted hidrogen evolved in preparing Rochelle salt.

On saturating a lixivium of Alicant soda with the tartarous oxidule, in order to prepare the tartrite of soda and potash, during the effervescence I perceived a very evident smell of sulphuretted hidrogen gas. Reflecting on this, I imagined the evolution of this gas must be owing to the decomposition of some hidrosulphuret, contained in the alkaline lixivium; and I determined to make some experiments for the purpose of satisfying myself on this head.

A lixivium of soda set to crystillize.

Taking a certain quantity of the lixivium, I evaporated it so as to separate the greater part of the carbonate of soda by crystallization. After it had stood at rest a few days, I decanted the liquor, and put about two quarts into a glass vessel, which I placed on a shelf in my laboratory. After a month I examined it, and found the bottom of the vessel strewed with crystals of a colourless transparent salt, in rectangular tetraedral prisms, terminated by quadrilateral pyramids. I likewise observed octaedral crystals with rhombic bases. The supernatant liquor, being decanted and evaporated, furnished a fresh quantity of the same salt, differing only in being coloured, and in the octaedral crystals being less abundant. The geometrical figures of these crystals led me to presume, that they were a mixture of hidrosulphuret and carbonate of soda, the latter being the smaller quantity. The prismatic crystals, being separated from the others, exhibited the chemical characters of hidrosulphuret of soda. They had an acid and caustic taste, followed by considerable bitterness. They diffused a slight smell of sulphuretted hidrogen gas; and acids poured into a solution of them expelled this gas in some quantity. Not fused, they gave a green colour to blotting paper. With the solutions of sulphate of iron and of copper they threw down a black precipitate, as well as with those of acetate of lead and nitrate of silver. On pouring an acid on the saline crystals in

Two sorts of crystals from the mother water.

One hidrosulphuret of soda.

in the state in which they had been taken out of the evaporating vessel, a brisk effervescence took place, arising from the extrication of carbonic acid and sulphuretted hidrogen mixed. Willing to satisfy myself whether the formation of hidrosulphuret of soda had not taken place during the boiling of this alkali to extract the carbonate, I took 3gr. [46grs] of powdered soda, put them into a phial, and poured on them muriatic acid. This produced a brisk effervescence, and a very strong smell of sulphuretted hidrogen gas. On passing this gas, by means of a tube, through several solutions of metallic salts; the oxides were precipitated of the same colour as they would have been by sulphuretted hidrogen gas obtained from the decomposition of sulphuret of iron by sulphuric acid.

This not formed during the boiling.

From a mixture of crystallized tartarous acid in powder and soda sulphuretted hidrogen gas was equally evolved. These experiments were made with the various sorts sold in the shops under the names of Carthagena soda, kelp, and barilla. They all presented the same results.

Sulphuretted hidrogen evolved from various specimens.

It cannot be doubted therefore, that the hidrosulphuret of soda is contained in all the kinds of soda; and that it may be obtained from the mother waters of the lixiviums, that have furnished carbonate of soda. The formation of this salt is easily understood. When the plant is burned for procuring the alkali, the fire is urged so far as to cause the ashes to undergo a semivitrification: the sulphates contained in them are decomposed by the action of the charcoal: the sulphur is liberated and forms sulphurets. At the same time there is an extrication of hidrogen gas, which may be furnished by the charcoal itself, or by the decomposition of the plant or of water; and no doubt by the three together. This hidrogen gas, uniting with the sulphur, constitutes the sulphuretted hidrogen gas, which in its turn combines with a part of the alkali, and forms hidrosulphuret of soda.

Crude soda always contaminated with hidrosulphuret.

Source of it.

The formation of the sulphurets and hidrosulphurets that barilla contains being occasioned by the strong calcination of the ashes furnishing this alkali, we may infer, that the method of preparing it is defective. It is evident, that part of the alkaline salt enters into the constitution of the sulphurets and hidrosulphurets contained in it. These remain in

The preparation therefore defective.

Inconveniences of it.

in solution in the mother waters, that have furnished carbonate of soda. When the barilla is employed for making soap, the same loss is experienced. The soapboiler's lie contains sulphurets and hidrosulphurets, which diminish its causticity, as they have a weaker affinity for lime than for soda. The consumption of this alkaline substance in pharmacy, and more especially in soap-making, is sufficiently extensive, to turn the attention of chemists and manufacturers to an improvement in preparing this alkali; which is also much employed in dyeing cotton, and in the washhouse. In the latter, the sulphurets and hidrosulphurets in barilla not only render it so much the dearer, but are injurious to the whiteness of the linen and cotton*.

Farther inconveniences.

These are not the only inconveniences arising from the high degree of heat, to which the soda is exposed in manufacturing it. A still greater is, that a part of the alkali enters into combination with the earthy substance contained in the ashes, and forms a kind of frit, indecomposable by the action either of water or of acids; and the quantity of alkali wasted in forming this semivitrified substance is greater than that taken up in the formation of the sulphurets and hidrosulphurets. Here no doubt is a great loss of alkali, occasioned by the semivitrification of the ashes of the plants that furnish soda; and which would be avoided by adopting the mode used in preparing potash; lixiviating the ashes, evaporating the lixivium to dryness, and selling the alkaline salt in this state of preparation. The consumer would find so much

Method of avoiding these.

Serious accident in a soap manufactory.

* The sulphurets contained in unprepared soda frequently occasion serious accidents in soap manufactories. In these, where it is customary to keep caustic lie in large covered stone vats, the sulphurets decompose the water. The hidrogen not absorbed by the hidroguretted sulphurets thus formed occupies the empty part of the vat. When the workman takes off the cover, to dip out the lie, and holds in his hand a lamp to light the inside of the vat, the hidrogen takes fire, and endangers the building. In a visit I just paid to Marseilles I saw one of these manufactories, that had been destroyed by a violent explosion of this kind. The hidrogen being mixed with atmospheric air such an explosion took place, that the house was near being thrown down. The owner, supposing the manufacturer had maliciously attempted to destroy his house, summoned him before the magistrate; and the cause is still pending before the first tribunal of the department.

the

the more advantage in this, as it would be easy for him to satisfy himself of the purity of the alkali; a knowledge so necessary to the success of various processes both in pharmacy and in the arts.

Reflections on the manufacture.

But what are the reasons, that have induced the manufacturers of barilla to give it this solid consistency? Is it because in this state it is more convenient for carriage? or because it was originally less used in the making of soap than of glass? It is true, that in the glasshouse there are fewer inconveniences from its use, than in the processes of chemistry and the other arts. When the crude alkali is employed in making glass, not only are the hydrosulphurets, sulphurets, and other salts it contains, decomposed by the high heat required in this process, and their alkali serves as a flux; but the frit itself enters into the state of vitrification, and thus adds to the bulk of the glass. This, however, cannot be considered as an advantage; for it is certain, that barilla contains nearly four fifths of its weight of heterogeneous substances, which of course increase the expense of carriage in this proportion. This is a consideration, that claims the attention of the consumer. The first source of profit in a manufactory is economy in the raw materials.

Hint to manufacturers.

XI.

An Essay on the Cultivation of the Red Beet, by Mr. GOERING, *a Saxon Agriculturist*[*].

Red beet very beneficial.

NEXT to the potato, the utility of which is well known, the red beet is one of the most beneficial plants, the cultivation of which is particularly to be recommended. Every one knows, that sugar has been obtained from it not inferior to that of India; and the manufacture of which would probably have been established in Germany, had not the consumption of wood necessary for it checked its most zealous partisans; for the resources of Germany in this respect are daily diminishing.

Yields much good sugar,

but this consumes too much wood.

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[*] Sonnini's Bib. Phys. Econ. May, 1810, p. 289.

Beside

Other properties of it.

Beside this essential point, which cannot be attained from local difficulties, and which may not exist in many other countries, the principal properties of the beet are those of being nourishing, emollient, cooling, laxative, &c.

Advantageous for feeding cattle.

White beet injures the milk:

Supposing it to be cultivated only for feeding stock, particularly cows, in winter and in summer, it deserves in every respect to be preferred to most plants both for the root and leaf. Though the white beet is of pretty extensive use, and much cultivated, it cannot in any respect be compared with the red. It is neither so firm nor so sweet; and we find by experience, that the milk of cows fed some time with it loses its sweetness, and becomes bitter. Besides, it can scarcely be kept through the winter, as it soon grows rotten.

the red beet improves it.

The red beet on the contrary is firm, sweet, and but in a moderate degree watery. It is at least as nutritious as the turnip cabbage, and imparts to the milk a pleasing sweetness, which continues as long as the cow is fed on it. It keeps very well through the winter, either in cellars or in pits, provided it be not put in wet; and is as fresh when taken out in the spring as it was when laid up. They who cultivate both sorts, therefore, should use the white in the fall, and keep the red for the spring.

The leaves good fodder.

The leaves of the red beet, which may be gathered in the middle of july, the time of sowing the white beet only, is excellent fodder, particularly for horned cattle and pigs. It is true however, that the leaves cannot be thus gathered but at the expense of the roots.

A crop to be depended on.

It is also indisputable, that the red beet is one of the roots that succeed almost always. It has few enemies, and a good crop may always be depended on, provided the ground has been well tilled and prepared, and the seed properly sown.

May be sown at any time,

and no insect injures it.

There is no season amiss for sowing the red beet. It may be sown as early as you please in spring, or even in autumn; for the first leaves which in most other plants are very tender, are able to stand the cold winds of spring. No insect can hurt them; and while the turnip, the turnip cabbage, the cabbage, &c., are destroyed by the leaflice, the red beet grows astonishingly: and when in autumn the leaves of those plants are devoured by caterpillars, none are seen on the red beet.

The

Fowls extremely fond of the leaves.

The only enemies it has, that I know of, are fowls; for these are so fond of its leaves, as entirely to lay waste the fields of it, to which they can have access. Their appetite for this plant, when they once have discovered it in a field or garden, is such, that it is almost impossible to keep them out. They should not be sown therefore in gardens or fields too near houses, as in this case the crop may be looked upon as lost.

The following is the method I have adopted of cultivating it.

Method of cultivating it.

I first select, if possible, a good black mould, rather rich. If it be mixed with a little sand, and provided it has not too much clay, it is good for the beet, which always requires a little moisture. It may be cultivated indeed on light ground, but not with equal success.

In autumn I lay on manure, in the proportion of six two horse cart loads of dung of horned cattle to a hundred and forty square perches. This dung I afterward bury at least six inches deep with the plough: and then I give the ground another ploughing in narrow furrows.

As soon in the spring as the land can be worked, I sow the eed where the plants are to remain; for experience has taught me, that transplanting them is injurious. They should not be sown too thick: there should be at least six inches distance between the plants; and it is often necessary to pull up some in the thickest places, for three or four plants frequently spring from a single seed.

It is usual to cover the seed by raking or harrowing: but as from their lightness they frequently lie on the surface and rot, it is better to use the hoe, or the plough, taking care not to bury them too deep. In this way we may be certain of their germinating quickly, if the soil be good.

As soon as the plants have their sixth leaf, they should be weeded, and thinned out where too close. A few weeks after they should be hoed, but so as rather to draw the earth from them than to heap it round them.

Gathering the leaves.

When the leaves begin to bend down to the ground, the largest, at the bottom of the plant, may be gathered for the cattle: but they must not be stripped too much, as this would injure the root. Nor should the leaves be plucked off before they separate as it were of themselves, inclining toward the ground.

If

Taking up and preparing the roots.

If weeds appear again, or the ground get hard and dry, they should be hoed a second time. Lastly, in the month of October the roots should be taken up, and laid in the places intended for keeping them, first cutting off the stalk close to the root, that they may not vegetate during the winter.

XII.

Account of a Composition commonly called Turkish Rose Pearls; by Mr. MARCEL DE SERRES, *Inspector of Arts, at Vienna*[*].

Elegant and fragrant black beads made of rose leaves.

TURKEY has a considerable trade in a composition known by the name of rose pearls; and as this composition is very simple, I imagine it may not be uninteresting to make it known, that it may be imitated in other countries. Nothing more is necessary than to take the petals of fresh gathered roses, and pound them carefully in a cast iron mortar well polished. They are to be pounded till they are thoroughly bruised and form a smooth paste. This paste is to be spread on a sheet of iron, and dried in the air. When it is nearly dry, it is to be pounded again with some rose water, and dried afresh. This is to be repeated, till the mass is reduced to a very fine paste, when it is fashioned into the proper shape with the fingers, or with an instrument similar to that used for cutting pills. The sort of beads thus formed are then perforated for stringing, and the paste is dried afresh, till it becomes very hard. When they are smooth and well polished, they are rubbed with oil of roses, to increase their fragrance and lustre. By this simple process the paste of rose leaves takes a very decided black colour, owing to a combination of the gallic acid in them with iron.

Similar beads of other colours.

With a similar paste beads of various colours are formed. The most common, next to the black, are red and blue. The colouring matter is added to the paste. It is possible however, that these red or blue beads, which are said to be nothing

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[*] Sonnini's Biblioth. Phys.—Econ. Feb. 1810, p. 105.

nothing but the paste of rose leaves so coloured, may be made of a particular paste; and if I must give my opinion, I should think this is the case, from the difficulty of giving a red or blue colour to a paste so black as that of roses*. The red necklaces in question must not be confounded with those made of pimento, or those of the fruit of the red bead vine, *abrus precatorius*.

Additions to their perfume.

Frequently to render the Turkish rose pearls more fragrant, oil of roses, storax, and musk, are mixed with the paste; but this addition makes no alteration in the mode of preparing it.

The black generally prefered.

The black beads are most prized, either because they set off the colour of the skin to more advantage, or because their perfume is more agreeable. These beads find their way over Europe through Austria, and are of some consequence as an article of trade.

XIII.

On the tall Oatgrass: by Mr. TOLLARD, *sen*†.

Utility of the tall oat-grass.

THE tall oatgrass, *avena elatior*, grows and produces an abundance of fodder, both in good and bad soils. It is of very early growth, and rises to the height of two or three feet. Its stalk is fine, slender, and makes very good hay. It is mowed twice a year. If it be eaten green, it may be cut oftener; but it is principally cut for hay.

It may be sowed in autumn, or in spring, after two ploughings; at the rate of 70 kil. [154 lbs] to the *are* [2·5 acres]. Frequently saintfoin is sown with it, in the proportion of a hectolitre [2 bush. 3 pecks] of saintfoin, and 60 kil. [132 lbs] of oatgrass seed, to the above quantity of ground.

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* It is obvious, that this difficulty would be removed by wholly avoiding the use of iron in making them. At the same time the petals of other fragrant flowers of different colours might probably be used with advantage. Thus the violet appears to be well adapted for the blue. The manufacturer too might avail himself of the well known property acids possess of heightening the red of roses, and of changing vegetable blues to red; as well as perhaps that of alkalis, in converting the blues into green. C.

† Abridged from Sonnini's Journal, Dec. 1810, p. 375.

Utility of the tall oatgrass.

It is particularly adapted to horses; but all animals, that are commonly fed with hay, eat it with pleasure.

Opinions have been so divided respecting this plant, that several writers have been eager to boast its advantages, while others have endeavoured to depreciate it. This difference of opinion respecting a plant of real utility has arisen from the authors who have mentioned it omitting its botanic name: hence some have confounded with it the ray grass, *lolium perenne*; others with the way bennet, *hordeum murale*, which has no relation to it, and is one of the very numerous plants injurious to meadows.

I repeat, that the *avena elatior* is the best basis of a natural meadow; and that, when cultivated alone, it makes an excellent pasture. It is one of the best of the family of grasses, as any one may readily be convinced by observation. It may be known any where by its slender stalk, rising above the other grasses, and terminating in panicles a little drooping.

SCIENTIFIC NEWS.

St. Thomas's and Guys' Hospitals.

Medical and surgical lectures:

The Winter Course of Lectures, at these adjoining Hospitals will commence as usual on the 1st of October, viz,

At St. Thomas's.

Anatomy and the operations of surgery, by Mr. A. Cooper, and Mr. Henry Cline. Principles and practice of surgery, by Mr. A. Cooper.

At Guy's.

Practice of medicine, by Dr. Babington and Dr. Curry. Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen. Experimental philosophy, by Mr. Allen. Theory of medicine, and materia medica, by Dr. Curry and Dr. Cholmeley. Midwifery and diseases of women and children, by Dr. Haighton. Physiology, or laws of the animal œconomy, by Dr. Haighton. Structure and diseases of the teeth, by Mr. Fox.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and, with the lectures on anatomy, and those on the principles and practice of surgery, given at the Theatre of St. Thomas's Hospital adjoining, the whole is calculated to form a complete course of medical and chirurgical instructions.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1812.

ARTICLE I.

On the Electric Column, and Aerial Electroscope. By J. A. De Luc, *Esq. F. R. S.*

To William Nichołson, Esq.

Sir,

Two points on which the author dissents from Dr. Maycock.

1. IN my last paper, published in your No. 149, after having shown, that Dr. Maycock had very ably refuted the natural philosophers, who thought that the *galvanic effects* depended on the *electrical energies* of the particles of *matter*; and proved, that it was produced by the action, on each other, of two proper *metals*; I was obliged to dissent from him on two points: 1. that the *electrical excitation* produced by the two metals did not exist during their *contact*, but only at the instant they were *separated*—2. that the *galvanic apparatus* can only be *excited* by a *decomposable fluid*, which is always *decomposed* when the apparatus acts. But I opposed to these two propositions my experiments related in your Journal for June and August, 1810, which demonstrate, that, in the *galvanic pile* the *chemical* effects are independent of the cause of the *electric* effects; the former being only produced when the *electric fluid* pervades a *pile*, in which a *liquid* acts on the *metals* to *corrode them*. I have explained, also, in the same paper, how these ex-

His reasons for this.

experiments led me to the discovery of an *electric apparatus*, which, without any *liquid*, and composed only of alternate *zinc plates*, and equal pieces of *Dutch gilt paper*, produces strong *electric*, but no *chemical* effects.

Spontaneous electric machine

2. My paper, Sir, in your Journal for October, 1810*, contains the description of a small apparatus of this kind, which, in order to distinguish it from the *galvanic pile*, I have named *electric column*. It is really a *spontaneous electric machine*, the power of which can be increased without limits, by increasing the number of the groups: it is lasting; for I have still the first *column*, (that represented in the *figure*†) which I constructed five years ago, preserving still its power: and indeed there is no reason why it should lose it, as there is no *liquid* to affect the *metals*; for it requires only that the *papers* should possess the small degree of *moisture* of the surrounding air.

of unlimited power and duration.

Column of 20000 pieces, by Mr. Allen.

3. As to the increase of power of this natural *electric machine* by increasing the *number* of the *groups*, a very ingenious and well-known experimental philosopher, Mr. W. Allen, has carried it to such a degree as to produce very remarkable effects. His apparatus consists of ten *columns*, each containing 1000 groups of *zinc* and *Dutch-gilt paper*, forming together a series of 10,000 *groups*: but they are of a *small* size, so as to be enclosed in glass tubes; a circumstance, the effect of which shall be seen. The following are the phænomena observed by Mr. Allen, which confirm some points which I have stated.

Jar charged by it.

1. Having tried to *charge* a *coated jar*, the *charge* arrived sometimes to such a degree, as to give a *shock* up to the elbows; but at other times it could never arrive at that point. This circumstance is owing to what I had observed of the influence of the *electrical state* of the *air*.

Did not decompose water.

2. Notwithstanding such *electric* power, Mr. Allen did not perceive any production of *gasses* in *glass tubes with water*, made to connect the extremities of these *columns*.

Effect of size and number of plates.

3. That apparatus shows also what I had found with respect to the *size* of the *plates*; that the *size* was indifferent to the *intensity* of the *ultimate effect*; but that the *larger* they were, the *sooner* that *effect* was produced. The *plates* of Mr. Allen being *small*, many minutes were required to *charge* his *jar*.

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* Vol. xxvii, p. 81.

† Ib. pl. iii.

Mr. De Luc's apparatus.

4. I come now, Sir, to my experiments contained in your Journal for October, 1810, with a *figure,* half the size of the original, of the apparatus to which I shall refer them, but only in their parts concerning Dr. Maycock's system. For these experiments, beside the *electroscopes* at the *ends* of the *column,* there is one in the *middle.* I shall call A, that connected with the *positive* end; B, that at the *negative*; and C, an electroscope placed at the *middle point*: but by taking off the wire 4, it may be made to communicate with any part of the *column,* the *electric* state of which is wanted to be known; and this by means of an insulated *wire,* so soft as to be easily bent, and thus made to connect, by one end, with the electroscope C, and by the other, with the part of the *column* the *electrical* state of which is wanted to be known.

5. These experiments, which begin at p. 88 of the same number of your Journal, referring to some TABLES given in the conclusion of my preceding paper*, I think better, for an immediate reference, to copy them here, with this previous explanation, that in all of them, A indicates the *positive* end, and B, the *negative.*

Different states of the pile.

TABLE I.	TABLE II.	TABLE III.
Insulated Column.	*B in communication with the ground.*	*A in communication with the ground.*
A	A	A
+ 10	+ 20	0
+ 8	+ 18	— 2
+ 6	+ 16	— 4
+ 4	+ 14	— 6
+ 2	+ 12	— 8
0	+ 10	— 10
— 2	+ 8	— 12
— 4	+ 6	— 14
— 6	+ 4	— 16
— 8	+ 2	— 18
— 10	0	— 20
B	B	B

Experiments showing the motion of the electric fluid in the column.

6. I come now to the experiments. Experiment 7 is thus related in p. 88 of your Journal above quoted. "At the time "when (on account of the *electrical state* of the *air*) there are "simple and equal *divergences* in the electroscopes at the extre-

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* Vol. xxvi, p. 265.

" mities of the *column*, then *positive* at A, and *negative* at B; " there is no *divergence* in the electroscope C; it is a *neutral* " point, and is expressed by 0 in TABLE I. If, at the same " time, any point of the *column*, at a distance from the point " C, on the *negative* or *positive* side, proportional to one of " the terms of TABLE I, be tried by the insulated wire, the *di-* " *vergence* produced is, as exactly as can be expected in such " experiments, correspondent to that expressed in the *table*, with its sign either — or +."

7. In *Exp.* 8 are seen the two different cases expressed in TABLES II and III, observed also by means of the *insulated wire.* The case of TABLE II is produced by placing the end B, or *negative,* in communication with the *ground.* In this case, the electrometer at the *middle* point C, which, in the preceding case, was *neutral,* has a *positive* divergence, equal to that before observed at the extremity A, where it is now *double.* There is no *neutral* point perceptible, but close to the end B, whence the *positive* state is increasing towards A, at the rate expressed by TABLE II. If the communication with the *ground* be inversely placed at A, or the *positive* extremity, which is the case of TABLE III; then the only point found *neutral* in the *column* is close to that end A; and thence the *negative* state is increasing up to B, where it is become nearly *double*; and at the middle point C, the divergence, now become *negative,* is equal to what it is at the extremity B, when the *column* is *insulated.*

The effects produced while the metals are in contact.

8. If Dr. Maycock had known these experiments, in which the electroscopes indicate, to the eye, the *motions* of the *electric fluid* in the *column,* he could not have persisted in his opinion, that the *metals* do not produce their *electrical* effects while *in contact,* and only at the instant they are *separated*; for they were not *separated* an instant during the course of these experiments. Besides, I am induced to think, that Dr. Maycock did not even know yet the existence of that new instrument, the *electric column*; or at least had not had the opportunity of seeing any; for each *column,* of whatever *number* of *groups* it is composed, which constantly remain *in contact,* produces *electrical* effects at its extremities, in proportion to the *number* of *groups.*

This equally takes place

9. Dr. Maycock, not having read my papers, might say, that he had applied his system only to the *galvanic pile*; but had he known

known the same paper of which I am speaking, he would have seen, in p. 91, that I have made the very same experiments directly on the *galvanic pile*; and that I found the same gradation of *plus* and of *minus*, from the *middle point*, as in the *column*. Only these operations cannot be so long continued on the *pile*, because its *electrical signs* are diminishing in proportion to the *erosion* of the surface of the *metals*. But Dr. Maycock could not be informed of these *electrical* phænomena of the *galvanic pile*; because, as it appears, he has not used it; and his galvanic observations have been only on the galvanic apparatus of *troughs*, which, as I shall show in a future paper, has deceived him.

in the galvanic pile.

Troughs act differently.

10. I now come to another class of experiments, which, it seems, are also unknown to Dr. Maycock; for, had he read them, he would have found evident proofs of a *irculation* of the *electric fluid*, when the extremities of the *column*, or of the *pile*, are *connected* together. These experiments consist in producing the *connexion* of the *extremities* by different *bodies*, and observing their effects on the *gold leaf* electroscopes. It has been seen, in the above experiments, that, when the extremities of the *column* are *unconnected*, there is an *accumulation* of the *electric fluid* at the extremity A, where the *gold leaves* diverge as *positive*, and a *deficiency* at the extremity B, where they diverge *negatively*. If a good *conductor* be applied to produce the communication between the extremities, the gold leaves *fall* on both sides: if it be a perfect *nonconductor*, their *divergence* is not altered: but if an *imperfect conductor* be applied, they *fall*, in proportion to the *conducting* faculty of the body.

Other proofs of the circulation of the electric fluid in the pile.

11. These experiments begin at p. 91 of the same number of your Journal. I made them for the purpose of ascertaining a very essential point in electricity, that of the best *insulation* of all our *electrical apparatuses*; having found, that the want of a complete *insulation* may lead to errour. *Glass* is the only body used, on account of its solidity, for pillars in all these apparatuses; and it has, in this respect, the essential property not to be *permeable* to the *electricuid*: but it is not a perfect *nonconductor*; the *electric fluid* moves, though slowly, along its surface, and to prevent it, it is necessary to cover it with some *insulating varnish*. These experiments, therefore, I made first, in order to find out which were the *best conductors*;

Glass requires varnish for a perfect insulation.

next,

next, what was the best *insulating varnish* to cover *glass*; and in their course I ascertained the different *conducting faculty* of various bodies.

Conductors and nonconductors.

12. The general results of these experiments were, that with respect to the *best conductors*, the *glass tubes* with *water* and proper wires, when no *chemical* effect is produced in them, are sensibly as good conductors as *metals*. As to the *insulating* faculty, I found, that *sealing wax*, in which no spirit of wine is added to make it softer, being laid on *glass rods* sufficiently heated to melt it, is equal to the best other *varnish*; for when placed on the extremities of the *column*, with the precautions I have indicated (the want of which produce very remarkable phænomena) these rods do not affect the *divergence* of the *gold leaves*. Lastly, I have given many details of my experiments on intermediate bodies, showing that, in proportion to their *conducting faculty*, each produces a determined degree of *diminution* in the *divergence* of the *gold leaves*.

The experiments prove a circulation of the fluid.

13. The whole of these experiments affords such proofs of a *circulation* of the *electric fluid* in the *column* when its extremities are connected together, and consequently of its *motion*, that, if Dr. Maycock had known them, he could not have had any doubt of these effects. The *circulation* is in consequence of an *accumulation* constantly tending to be produced on the *positive* extremity, at the expense of the other. This tendency continues, though the extremities are connected together; but the *electric fluid* cannot *accumulate* on the *positive*, while a *good conductor* can transmit it instantly to the *negative*; whence it also instantly returns to the *positive*, by the property of the column. But if the intermediate body be an *imperfect conductor*, the *circulation* is lessened, and some *electrical* signs remain at the extremities.

The electrical action not sudden.

14. There is another set of my experiments, which might have made Dr. Maycock doubt of the very ground of his system. He has imagined a certain *property* producing the *electrical* effects; such, I suppose, as that of the *magnet*; which, in consequence, ought to act *suddenly*. If this were the case, when a communication with the *ground* has changed the *divergences* of the *gold leaves*, that communication being removed, the same *divergences* ought to be *suddenly* restored; but it is far otherwise, as may be seen in Exp. 4, 5, 6, of the same paper.

But progressive.

There is some sort of impediment to the *motion* of the *electric fluid* along the *column*, probably caused by a *reluctance* in the *zinc plates* to part with the superior quantity of *electric fluid* they must possess when united with *copper*, to produce their *electrical equilibrium*. The consequence is: that, after having observed the *divergence* of the *gold leaves* in both electrometers, if one of the extremities of the *column* be made to communicate with the *ground*; by which the gold leaves *fall* on this side, and they *diverge* more on the other side; it requires a long time, in some cases many hours, for the same *divergences* to be restored.

Similarity between the action of the column, and that of the sensitive plant.

15. There is an entertaining experiment, which may lead to some discovery respecting the physiology of *vegetables*. Each of the *electrometers* of the *column* may be made to imitate the *sensitive plant (mimosa sensitiva :)* for, as the *contact* of one of the extremities of the *column* produces the *fall* of the *gold leaves* on this side, which rise *slowly*; the contact of the *sensitive plant* makes its leaves *fall*, and they also rise *slowly*. This analogy of *slow* effects, pointing out some general analogy between their causes, must render us cautious not to assign hastily to some vague *property* the effects that we may follow distinctly in their process, such as those of the *electric column*; for they may lead us, in time, to the discovery of causes, in those phænomena which now appear the most obscure.

The rapidity of elective circulation influenced by some external cause.

16. The IId part, Sir, of the same paper in your journal, concerns the *electric column* in its phænomena as an *aerial electroscope*, and contains the observations which I had already made with that instrument. This class of experiments relates to the opinion of Dr. Maycock, forasmuch as they prove, not only a constant *motion* of the *electric fluid* in the *column*, but that some external cause influences much the *rapidity* of its *motions*; an object the explanation of which I had postponed.

As shown by the aerial electroscope.

17. These changes are seen, when the *extremities* of the *column* are not made to communicate immediately with each other; but only by the alternate *strikings* of a body suspended between them, taking some *electric fluid* from the *positive* side, and bringing it back to the *negative*. Now, the more *rapid* is the *motion* of the *electric fluid* in the *column*, the more *numerous* are the *strikings* in a *given time*; and the difference is

is very considerable, in different days, and different parts of the same day.

This instrument made known imperfectly,

18. When I first observed that phænomenon, it pointed out to me a new and very interesting object of study: but, according to a plan of observations which I then formed, I was obliged to make many additions to my *column*, which required much time: but the first description which I had given of that apparatus in a paper to the Royal Society, and of its purpose, made it partly known.

and imitated by Mr. Forster.

19. This accidental communication to the public was a lucky circumstance; for before I could have time to do it myself through your Journal, a very ingenious experimental philosopher, Mr. *B. M. Forster*, not knowing it precisely, imitated it in a curious manner: he formed two *columns*, containing together 1500 groups of *zinc* and *silvered* paper, of the small size of my first *column*, and having placed them horizontally, he connected with each extremity a small *bell*, and suspended between them, and very near them, a small brass *ball*, held by a silk thread. When the apparatus was ready, he heard it *chime*, with a sort of buzzing noise on account of the rapidity of the motion of the ball.

Defect of his apparatus.

20. This apparatus had been mentioned in Mr. Tilloch's Phil. Magazine, and having seen there its description, I spoke of it in the same paper of your Journal, p. 103. But since that time, having had the pleasure of making personal acquaintance with Mr. Forster, and corresponding with him, he has communicated to me, from time to time, his observations of this kind of *aeroscope*, which, though in a different manner, indicates also changes in the *electrical state* of the *air*; for, after having chimed for some time, it stops totally, then begins again, and stops; sometimes it chimes for a moment, between long intervals of silence. This is a very curious phænomenon, but there is a want of intermediary *terms* between the *cessation* and *return* of motion. These inequalities are occasioned by the insulation of the little *ball*, it being suspended by a silk thread. Having tried what would be the effect of a greater distance between the bells, I found that it stopped the motion of the little ball, and I soon judged what was the cause of that cessation. When there is more distance, the little ball tending sensibly as much to the *positive* as to the *negative* bell, the difference

ference between these tendencies is not sufficient to surmount its weight, and it remains without motion; but when it is very near each bell, a very small difference of attraction on one side can make it move towards it, whence it is repulsed. The difference, however, between these attractions may be so small, that the little ball remains undetermined, even at that small distance, though the *column* has a sensible action.

This did not exist in Mr. de Luc's.

21. My plan had been different from the beginning, and thus free from that impediment: it was to obtain a separate *electrometer*, formed of a long brass rod with a large ball at the bottom, and to suspend at the top, by a conducting thread, a small metallic ball. This small apparatus being connected by its upper part with one side of the *column*, the little ball was to diverge; and I intended to have another large ball in communication either with the other side of the *column*, or with the ground, against which the little pendulum should strike, fall, and rise again. This apparatus is represented in the figure annexed to my paper in your Journal for October, 1810. In the same paper, I explained all the difficulties which I encountered, before I could prevent the little ball of the pendulum from sticking to the large ball. At last, however, I succeeded by the means expressed in the figure; and having determined the distance of the second large *ball*, at which the pendulum should never cease to *strike* it by the smallest power of the *column*, the purpose of the apparatus became to *count* the *number* of the *strikings* in a *given time*; which was the precise indication that I had desired to obtain of the smallest changes happening in the power of the *column*.

Observations with this apparatus.

22. This apparatus was ready for observation in the beginning of April, 1810, and in the remaining part of the same paper I related the phænomena, which it exhibited during this month and the following month of May. The tables of these observations are composed of five columns: the first indicates the *days* and *parts of the days* in which the observations were made. The second, the points at which the *barometer* stood. The third, the points of the *thermometer* in the room. The fourth that of my *hygrometer**. The fifth, the *number* of *strikings* of the *pendulum* in determined *times*.

23. By

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* This instrument has been taken up by a very ingenious Hanoverian gentleman

The phenomena depended on the electric state of the atmosphere only.

23. By comparing the last column with all the others, in the paper above-mentioned, it may be seen, that there is no connexion of the *number* of *strikings* with either the *barometer*, the *thermometer*, or the *hygrometer*, and only with the different *days* and *parts* of the *day*. Which circumstance confirmed me in the idea, that it was only the different *electrical* states of the surrounding *air*, that produced these changes in the power of the *column*; however obscure was still this connexion, for the reasons which I explained.

The inquiry pursued by Mr. T. Forster.

24. This is a new and very interesting subject of experimental and even natural philosophy, and in publishing it in this its infancy, I had the hope that it might lead some attentive observer to follow it up. This hope has been realized, when I have seen in your Journal, that Mr. T. Forster has undertaken that investigation; particularly as I know his talents, being, since that time, personally acquainted with him.

Improvement in the apparatus.

25. I shall only mention farther, that I have made a new step in this pursuit. Knowing by my former experiments, that, though the *size* of the *plates* is indifferent to the final simple *divergence* of the *gold leaf electrometers* at the extremities of the *column*, it is not the same when, one of them *striking* the side, they are reduced to the electrical state of the ground; for they *rise* faster and *strike* again, when the *plates* are *larger*. Applying, therefore, this result of my former experiments to the motion of a *pendulum*, I have constructed a *column*, which, in two connected parts, contains 1300 groups, formed of *zinc* plates 1½ inch square, and equal pieces of *Dutch gilt paper*. This *column* moves a *pendulum* consisting of a *gilt pith ball* the size of a pea, suspended like the other by a *conducting* thread, and placed in the same apparatus, which prevents its sticking when it strikes the large ball. This *pendulum*, guarded against the agitation of the air by a glass case, moves between the two same large *balls*, being near one inch distant from each other, and it has not ceased to strike

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gentleman, residing at present at Cumberland Lodge, near Windsor; Mr. Hausemann, he has succeeded in every point, and is resolved, from its utility, to construct it for the experimental philosophers who shall desire it.

during already two years that it has been constructed : but the *frequency* of its *strikings* is also very various ; for I have observed at times forty-five in a minute; but passing at other times by all the intermediate numbers down to hardly one.

Mr. B M. Forster constructing a similar apparatus.

26. In this state I must leave this pursuit, on account of my age; but I have learnt, with great pleasure, that Mr. B. M. Forster is employed in constructing also a *column* with *large plates* and a *pendulum* ; and that his nephew, Mr. T. Forster takes great notice of the connexions of this phænomena with various circumstances in the appearances of the air, and with diseases. This, in time, may lead to some useful discovery, both for science and for society.

Mr. Hausemann also observing with one.

27. This new *electrical* phænomenon, so connected with the state of the *air* which surrounds us, cannot but interest many natural philosophers, were it only with respect to meteorology : it is a new *thread* leading in the maze of *atmospheric* phænomena, provided it is not associated with gratuitous hypotheses. This, Sir, has been the object of the IIId part of my paper on the *electric column,* contained in your No. 124, for December, 1810; in which part I have given an abstract of some other *threads* obtained in the *atmospheric* phænomena, considered both in themselves, and in their relation with those exhibited by the spontaneous appearances and disappearances of the *electric fluid*; especially in the great phænomenon of *lightning* and *thunder.* Mr. Hausemann, of whom I have spoken above in a note, having had the opportunity of observing the different *motions* of my *pendulum,* and persuaded that they must have some connection with the *atmospheric* phænomena, has constructed the same apparatus with large plates, and begun regular observations.

28. I stop here on this interesting subject, having, I think, recalled it sufficiently to show, that Dr. Maycock had not embraced, or considered with attention, all the branches of experimental philosophy connected with the determination of the *nature* and *functions* of a *fluid* influencing almost all the atmospheric phænomena. But in a future paper I shall treat of another part of the same subject, by coming to the idea Dr. Maycock has conceived of the effect of *friction,* to produce

electrical effects; which will give me an opportunity of examining his system under a different point of view.

I have the honour to be,
Sir,
Your obedient, humble Servant,
J. A. DE LUC.

Windsor, August the 27th, 1812.

II.

Effect of the Attraction between the Weights and the Pendulum on the going of Clocks. In a Letter from Mr. THOMAS REID.

To W. Nicholson, Esq.

Edinburgh, 18*th Aug.* 1812.

SIR,

The going of a time-piece affected by attraction.

OF late, having been much engaged with astronomical clocks, which require a great deal of attention, to see that they are fit to perform, and to keep as near to time as it is possible from the nature of things to bring them; it will perhaps be thought strange to say, that attraction comes in for a share in those obstacles, which stand in the way of good timekeeping. This is what has never been even hinted at before; if it has, I confess it is new to me. We have heard of clock pendulums disturbing one another, where clocks were set agoing on the same board, and where the pendulums were not sufficiently fixed, but this arose from a very different cause.

A six-weeks astronomical clock.

Having fitted up a clock in every respect particularly good, and unexceptionable both in the plan and the execution of it, which, by express order, was made to go about one month or six weeks, the scapement of it made after the principle suggested by Mudge as far back as the year 1763, and which he afterwards introduced or used in his timekeepers. This clock, from the nature of the scapement, and from that of its pivots being so independent of oil, at almost all of the holes, was, from these circumstances, expected to keep the arc of the vibration of the pendulum as nearly constant as possible; but after keeping

keeping this arc perfectly for above two weeks, it surprised me, and even mortified me not a little, to find no constancy in the arc even here, notwithstanding very sanguine hopes had been entertained of it. One morning it was observed to have come in a little, which afterward it did gradually more and more for some time, from a maximum to a minimum, and vice versa, until it had regained its original extent of arc, which it kept on, till the same circumstances came again in the way to disturb it.

Began to diminish its arc of vibration in three weeks,

and after a time increased it again.

Although I saw this, and was in some degree convinced, that this must be owing to the attraction of the weight for the pendulum; yet I would not rest altogether satisfied without again examining the clock, lest something might be there, which tended to give rise to this inequality of the arc of the pendulum's vibration. But on examination, there was evidently no cause, that could in the smallest degree be suspected, for this.

This presumed to arise from the weight attracting the pendulum.

In constructing the clock, attraction was suggested by a very ingenious friend, on an idea taken from the experiments that had been made by Mr. Cavendish: and accordingly the barrel was so contrived, as to throw the weight, when it came as low down as the pendulum ball, the farthest then possible from it. The weight is about 27lb.; and the pendulum, which is a mercurial one, has about 10lb. of mercury in a glass jar or hollow cylinder. In order to be more convinced in this matter, the clock was again wound up, and during the time of the first ten days or a fortnight it kept the arc of vibration constantly the same, and its rate of time was somewhat less than +0·1″ per diem; when the weight had got down, and partly opposite to the cylinder of mercury, the arc of vibration began to come in, and the clock gained in the mean time from less to more, even to five seconds in one of those days; when the weight had got below the pendulum or cylinder of mercury, the former arc of vibration was regained, and its rate of time also. The full extent of the arc of vibration is about one degree eleven minutes on each side the point of rest, of this it lost about six minutes when at its least extent.

This had been foreseen, and guarded against, but not effectually.

There was another clock observed, which goes a month, has somewhat of the common scapement to it, and a compensation pendulum of Ward's form, which I think is a very excellent one. The arc of vibration is about three degrees six minutes on each side the point of rest, the weight of the pendulum ball about

Another clock affected in a similar manner.

about 12lb., and that of the going weight 28lb. From the forcible swing or motion of this pendulum, it was thought, that attraction would scarcely have any influence here; however, the weight, when opposite to the pendulum ball, brought the arc of vibration gradually in to three degrees; but on the weight leaving or getting below the pendulum ball, the former arc of three degrees six minutes was regained.

Month clocks frequently troublesome, even stopping, from this cause.

There are few clock-makers, who may not in the course of their experience have had a great deal of trouble with old month clocks, much vexation and running after them, on account of their stopping; and this apparently from causes inexplicable and undiscoverable; and with the greatest difficulty, after taking them into their hands, could they *sometimes* be made to go. Clock-makers, who may know the principles of their business tolerably well, and who would find no difficulty in making a common eight-day clock to go and perform its office easily, have often been much put to it by month clocks. They in general have very heavy weights, perhaps thirty pounds to the going part, and more than that to the striking part; the pendulum balls are very light, little more than one pound weight, if even this, and withal have very short arcs of vibration. These circumstances lead me to suspect, that attraction had had a great hand in the stopping of these clocks; and had *this* been known then, we should have found the stopping generally to have taken place about the time when the weights had got down, and nearly opposite to the pendulum ball; which is fully confirmed by my examining some old and well-experienced clock-makers on this part of the subject, who said, that this actually was the case, that they always suspected among other causes of stopping, that the weights might have touched the pendulum, and this they very frequently examined, to see if there was sufficient freedom for the weights to pass the pendulum ball without touching it, never dreaming of such a thing as attraction being there; but this of the weights being opposite to the pendulum would not be much noticed, from no suspicion being attached to them, but that of merely touching the pendulum ball. If the influence of attraction (which now must be admitted) takes place in those clocks which have heavy weights, it must take place, though in a less degree, in those clocks which go eight days; and, were it not for the trouble of daily

Thirty hour clocks have advantages over others.

daily winding up, a thirty-hour clock would, on this account of attraction, as well as for many other good reasons, be the most preferable of any.

I am, Sir,
Your most obedient Servant,
T. REID.

III.

An Essay on the apparent Figure of Stars and luminous Objects, seen at a very great Distance, and under a very small Diameter. *By Mr.* J. H. HASSENFRATZ*.

Phænomena of vision through small apertures.

IN the year 1806, I laid before the physical and mathematical class of the Institute several observations on the phænomena of vision through small apertures; and from them I have extracted the following, on the apparent figure of stars, which are here brought together in one paper.

Luminous objects seen at a distance.

If we look with the naked eye at a very remote luminous object; a star, a planet, a torch, a candle, or even a house on fire; we perceive, that these bodies are surrounded with rays of light, having particular directions; and that these rays prevent our distinguishing and ascertaining the figure of the object.

The rays from them differ to different eyes.

The number of these rays differs to different eyes: but two rays, AB, AC, pl. II, fig. 1, in the direction of the eyes, are pretty generally observed; as also a third, AD, perpendicular to them. Some distinguish a fourth ray, AE, which is a prolongation of AD: others see a fifth, AF, fig. 2: and in certain circumstances the spectator observes six or eight, fig. 3.

The commencement and progress of this phænomenon described.

When the luminous body is so near the spectator, that he can approach it, he may see this phænomenon commence and increase.

On receding from a candle to the distance of distinct vision, its figure is commonly that of a spear head, fig. 4. On receding farther,

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* Ann. de Chim. vol. lxxii, p. 5.

farther, its dimensions are changed, and the flame appears broader, as at fig. 5. Lastly, on retiring still farther, it assumes the figure of a lozenge, as at fig. 6; and at a greater distance it begins to exhibit cross rays.

The distance at which these cross rays begin to appear, is different with different people. I have always observed them, when I was twenty-five or thirty yards from a candle, and when consequently its flame was seen under an angle of one or two minutes. Some distinguish these rays at a shorter distance; others do not begin to perceive them till they are much farther off*.

The length of the rays issuing from the stars is so much the greater, in proportion to their brightness, and to the darkness of the night. The luminous rays of candles, torches, and bodies on fire, diminish in length in proportion to the intensity of their light; but we see those of the stars increase from the time of twilight, when they begin to be perceptible, to the time when the night is very dark.

Luminous objects seen through a transparent substance.

If we look through a body of clear water, or any transparent substance, at a star or bright light, that would appear to give out long luminous rays to the naked eye; we shall perceive, that the length of their rays diminishes, in proportion as the thickness of the body of water, or transparent substance, through which they are seen, is increased.

Star seen through a telescope.

A star seen through a telescope, the object glass of which, being of large diameter, increases considerably the intensity of the light at its focus, appears to be accompanied with four or more luminous rays. But if the intensity of the light be diminished, either by lessening the diameter of the object glass, intercepting a part of its light, enlarging the surface of the image, or any other method, we find the length of the rays gradually diminish, till, the intensity of the light not being sufficiently great, the rays cease to be perceptible; and then the

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* Is not this appearance of luminous objects seen at a distance, the origin of the figure under which stars are generally represented, though we have every reason to believe, that their form is spheroidal? Is it not their different appearance to different eyes, that has given rise to their being represented with a different number of rays? And is it not because most people perceive five rays, that they are usually delineated with this number?

star has the appearance of a circular disk, surrounded by a coloured aureola.

Coloured aureola round a star.

The coloured aureola, that accompanies the circular image of the stars, when their light is too weak for the rays to be perceived, is independent of the cause that produces this latter phænomenon. It appears to be occasioned by the object glass being more or less imperfectly achromatic; by the faintness of the image, which permits the aureola to be distinguished; by the inflection of the ray of light at the edges of the diaphragm, that lessens the object glass; and by several other causes, on which I shall not attempt to enlarge in this paper.

Disc of a planet magnified.

As the rays that accompany luminous bodies are not perceived, unless the object producing them be seen under a very small angle, it follows, that when the diameter of a planet is increased by the assistance of a telescope, so as to be seen under an angle of some minutes, the rays disappear, and the planet is clear and well defined.

Direction of the rays depends on the eye.

In these rays there is this particularity, that their direction always depends on that of the eyes looking at the object. Thus, if, when looking at a distant light, we incline the head, as at fig. 7, we shall immediately perceive the direction of the rays change. One of these directions is constantly parallel with the two eyes, moving with them; and the others preserve their relative situation with respect to this.

The rays produced by the visual organ.

The rays distinguished round luminous objects may be produced, either by the luminous body itself, or by the organ that discerns them. In the first case, the number and position of the rays should be the same to every spectator: but, as the number of these rays varies to different eyes, and follows the direction of the eyes when these are inclined in looking at the object, it follows, that the rays are produced by the organ perceiving them.

They are destroyed by looking through a small hole.

This truth is farther confirmed by looking at stellate lights through a small aperture; as this immediately destroys the rays, and the luminous bodies appear of smaller dimensions. In this case their figure is altered only by the inflection of the light at the edges of the small aperture; an inflection which generates aureola round the images of luminous objects.

How does the eye produce them;

Since the rays, that appear to emanate from luminous objects seen at a great distance, are produced by the eye that perceives

them;

this appearance?

them; it remains to ascertain, for the explanation of the phænomenon, how this appearance may be produced, and what part or parts of the organ produce it.

Rays caused by the lachrymal fluid are of a different kind.

The lachrymal fluid, by which the cornea is constantly covered, gives rise to the perception of several rays, when, in winking the eyes, the two lids approach the iris: but these rays are essentially different from those here considered, to which the name of *irradiation* has been given. The rays produced by the lachrymal fluid are all perpendicular to the direction of the eye-lids, and are produced only when these are brought very near together. They are seen sometimes at top only, sometimes at bottom, sometimes both at once, ; and this according to the position of the eye-lids with respect to the iris: and they are perceived at all distances from the luminous object. Irradiation, on the contrary, is discerned only when the light is at a very great distance, and seen under a very small angle. The separation of the eye-lids has no influence on this phænomenon: it is always perceived, however wide they are asunder: and lastly the rays are seen in four, five, six, or eight directions, one of which is always parallel to the eye-lids.

The cause not in the retina, or aqueous or vitreous humour.

As the production of this phænomenon cannot be ascribed to the retina that covers the bottom of the eye, and receives the image; or to the aqueous and vitreous humours, through which the pencil of light passes; we have every reason to believe, that it is owing to the action of the cornea, or of the crystalline, or to the action of both conjointly.

Effect of the cornea and crystalline.

The cornea and crystalline, by the nature of the curvature of their surface, refract the divergent rays that arrive at the eye, and cause them to converge to a particular focus. Now it is demonstrable by analysis, and may be verified by experiment, as I have ascertained, that if the surfaces separating mediums be segments of a sphere, the image produced by the rays emanating from a luminous object, and received on a plane perpendicular to the axis of the pencil is always a circle: but if the curved surface, convex toward the least refracting medium, be generated by two different osculatory radii, the image is formed of two ellipses, which intersect each other at an angle depending on the position of these two radii. If, therefore, the surfaces of the cornea, or of the crystalline, be not segments of a sphere, this is sufficient to cause the image, formed at the bottom

Effect of a compound curvature of a refracting surface.

tom of the eye by the light traversing this organ, to approach so much the more the form of a cross, in proportion as the luminous object is more remote from the eye, and as the two radii, generating those surfaces, differ more from each other.

Ellipsoidal lens.

The two ellipsoidal images decussating each other, fig. 8, are constantly seen, if a ray of solar light, the light of a candle at a distance, &c. be made to pass through an ellipsoidal lens. The same image, too, is observable, if they pass through irregular surfaces; such, for instance, as phials or decanters filled with water, &c.

Figure of the surface of the cornea difficult to ascertain.

It is extremely difficult to ascertain with precision the figure of the surface of the cornea in the living subject. After death the cornea becomes flaccid, and undergoes alterations that prevent us from distinguishing accurately the nature of the surface it had. Attempts were made to ascertain the figure of the eye by freezing it: but the increase of bulk of the fluid by congelation so altered it, that it was impossible to form a precise idea of the nature of its curved surface.

Apparent irregularity in it.

Petit's opinion.

On looking at a fresh human cornea, it appears to be of an irregular figure, but this irregularity is occasioned, in great measure, by the projection of the tunica conjunctiva over the upper part of the cornea. Dr. Petit, who paid great attention to the figure and dimensions of the eye, says, in a paper published among those of the Academy of Sciences, in 1726, that, when the portion of the conjunctive coat advancing upon the cornea is dissected off, the latter is commonly round: yet he met with the cornea of a negro, that measured 5·5 lines French from right to left, and only four from top to bottom. I have found it hitherto impossible, to obtain accurate data; all my observations, however, lead me to believe, that it is not spherical.

Probably not spherical.

The crystalline.

As the crystalline may easily be separated from the eye, physiologists could not fail to make observations on it. Accordingly, all who have treated on the organ of sight have been ready to describe the figure and composition of the crystalline.

Different accounts of its form.

Galen considers the crystalline as not being a perfect sphere, uniform throughout its whole extent; but approaching to a compressed globe, fig. 9.

Rufus of Ephesus thinks, that from its figure it should be called lenticular.

The-

Theophilus asserts, that the interior surface of the crystalline is less convex than the exterior: fig. 10. Fallopius, Zinn, and many other anatomists, are of the same opinion.

Vassalli says, he has observed the convexity equal on both surfaces: fig. 11.

Brisseau asserts, that the surface of the crystalline next the cornea is more convex, than that which is in contact with the vitreous humour.

Petit, who had seen and observed a great number of crystallines, says he observed some to have the anterior curvature greater than the posterior, fig. 12; but that most commonly the side toward the cornea had a radius of curvature greater than that in contact with the vitreous humour, fig. 10. He says even, that he found in several subjects the crystallines to have their greatest curvature, one at the anterior surface, the other at the posterior; and that he has met with crystallines both surfaces of which were equal, fig. 11.

Those anatomists agree in considering the two surfaces of the crystalline as two segments of a sphere applied to each other, fig. 13.

Dr. Thomas Young concludes from observations made on his own eyes, that the anterior surface of the crystalline must be a portion of an hyperboloid, and the posterior surface a portion of a paraboloid: but he admits, that his experiments would not succeed equally with every eye.

Dr. Petit says, he has met with crystallines, the posterior convexity of which was not spherical, but approached the paraboloid form.

Its figure varies with age. All persons, who have made observations on crystallines, know that their figure varies with age. Those of infants are small and thick. In some fœtuses the thickness is but little less than the breadth. Those of adults are about twice as broad as they are thick. Those of old men grow flatter and yellow.

Its dimensions. Petit has observed, that the dimensions of the crystalline are not always proportional to the age; and though in general the breadth is 9 mil. [3 54 lines],and the thickness 4·5 [1·77 lines], he has found it from 5 to 8 thick [1·97 to 3·15*l.*], and even 9 [3·54*l.*].

Thus,

Thus, there is not only a want of agreement between anatomists respecting the figure of the crystalline; but they who have observed it with the greatest care, as Petit, find very great differences in it; differences that must necessarily affect vision, and produce in great measure those variations, which have been noticed in the sight of different persons by physiologists and natural philosphers.

It differs greatly.

Whatever care has been taken to ascertain the figure of the crystalline, observers hitherto appear to have attempted only to determine the proportions that exist between the versed sines of the curvature of the segments, AB, AC, fig. 14, and the length of their chord, DE. No one that I know has endeavoured to ascertain, whether the plane of the posterior and anterior segments were circular; and whether there existed any difference between its diameter from right to left DE, and its height GF. This difference appeared to them not sufficiently perceptible to be measured.

Its anterior and posterior surfaces supposed to unite in a circular line.

However, as there are some crystallines, the horizontal and vertical diameters of which exhibit a pretty considerable difference, these could not escape an accurate observer. Thus Petit, in a paper read to the Royal Academy of Sciences in 1730, says: "the circumference of the crystalline is commonly round; yet I have found some in the human subject, that were not so, and the diameter of which was a quarter of a line longer one way than the other."

This not always the case according to Petit.

In this paper Petit describes a great number of observations made on the crystallines of various animals; those of man being introduced only as forming one of the links of the great chain.

He examined it in various animals.

As Petit is the only person, who has measured crystallines with sufficient care to perceive, that those of man are not round; and to observe, that one of the diameters exceeded the other by a quarter of a line; I thought it might not be amiss to repeat these experiments, in order to satisfy myself whether this particular observation of Petit was sufficiently general, to contribute to the production of the irradiation; and at the same time ascertain the directions in which the longer and shorter diameter are placed.

The examination repeated.

I immediately procured two sheep's eyes, which I opened cautiously. On taking out the crystallines, and laying them flat, I perceived, that the curve uniting the two segments was longer in one direction than in the other.

Sheep's eyes.

The author assisted by Dr. Chaussier,

Having no human eyes at my disposal, I requested Dr. Chaussier, professor at the School of Physic, to assist me with the means of ascertaining, whether the curve of the human crystalline, seen in front, was constantly a circle, as is commonly supposed. This gentleman complied with my wishes; and had the goodness, not only to procure me the eyes necessary for my researches, but to assist me with his skill and advice.

and Mr. Ribes.

Mr. Ribes, at that time assistant dissector to the school, brought us eyes of fœtuses, adults, and old men. To these we added eyes of sheep and oxen*, in order to compare their dimensions.

Mode of performing the experiments.

Mr. Ribes opened some of the eyes, cutting them transversely; and took out the crystallines with sufficient caution, to ascertain the place that each part occupied in the eye. Dr. Chaussier likewise opened some eyes by the operation for the cataract. He took out the crystallines, retaining precise marks of their situation. These crystallines laid flat had all an oval form. I measured with a pair of compasses the vertical diameter CD, and the horizontal AB, fig. 15, and constantly found the former greater than the latter.

The crystalline described.

The crystalline is a kind of oblate spheroid. I call the horizontal diameter, fig. 15, the length AB, which is in the direction of the eye-lids; the vertical diameter, the height CD, which is perpendicular to it; and the less diameter, the thickness EF of the two segments.

After each operation, Dr. Chaussier dipped the crystallines in sulphuric acid, diluted with water, in order to harden them, and free them from the membrane that envelopes them. After this we measured them anew; and constantly found, that the curve of intersection between the two segments was elongated in the direction of its height.

Measured again after the membrane was removed.

Dimensions of the crystallines examined.

In the two crystallines of a fœtus the vertical diameter CD was 8 mil. [3·147*l.*], and the horizontal AB 7·75m. [3·049*l.*]. The vertical diameter of one of the crystallines of an adult was 11 m. [4·328*l.*], and the horizontal 10·25 m. [4·033*l.*]. The second was not measured, because in taking it out the position of each part in the eye was forgotten to be marked. The two

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* It appears from what follows, that they were the eyes of one fœtus, two adults, one old man, one sheep, and one ox only.

crystallines of a man of forty had their vertical diameter 10m. [3·934*l.*], and their horizontal diameter 9·6m. [3·777*l.*]. The yellow crystalline of an old man had a vertical diameter of 9·25m [3·639*l.*], and a horizontal diameter of 8·75m. [3·442*l.*]. The second crystalline was not measured, because it appeared to have had its shape altered.

Those of the ox and sheep.

The two crystallines of an ox had a vertical diameter of 19·15m. [7·534l.], and a horizontal diameter of 18·75m. [7·377l.]. That of a sheep had a vertical diameter of 17·35 m. [6·826l.], and a horizontal diameter of 17 m. [6 688l.].

The surfaces formed of different curves,

These observations, made by Dr. Chaussier, Mr. Ribes, and myself, prove, that in the human crystalline the vertical axis is longer than the horizontal; and, consequently, that the two surfaces, the anterior and posterior, are generated by different curves, among which those that are vertical have a greater radius of curvature than those that are horizontal.

and apparently a little irregular.

The curve of several of these crystallines appeared to us a little irregular. The diameter measured in various directions seemed to be different from those an ellipsis should have had; but the differences were not considerable enough to be determined accurately with the compasses which we used for measuring the diameters.

The crystalline consequently may produce rays:

Since the curve formed by the planes of the anterior and posterior segments of the crystalline is not a circle, it follows, that their surfaces are not spherical; and hence, that the rays of light passing through them must have as many different foci, as we can conceive osculatory radii to have been employed in generating their surfaces. Thus the crystalline alone, from the irregularity of its surfaces, is capable of producing wholly, or in part, those irradiations, which are perceived on looking at very remote lights.

but the cornea contributes to the phænomenon.

The surface of the cornea too, appearing not to be an exact segment of a sphere, must contribute to the production of new foci, whence arise new irradiations. Thus every thing apparently concurs to refer the production of the irradiations perceived from remote luminous objects seen under a very small angle to the combined actions of the crystalline and cornea, that is, to the nature of their curved surfaces.

From the facts here recited it follows:

General conclusions.

1. That the figure of luminous objects within the sphere of distinct vision is perfectly distinguishable. 2. That

2. That these figures are altered, in proportion as we recede from this; and that at a great distance, when these objects are seen under an angle of one or two minutes, they appear surrounded with several irradiations, two of which are in the direction of the eye-lids.

3. That these irradiations are independent of the figure of the luminous object, and are produced by the organ perceiving them.

4. That these irradiations are occasioned chiefly by the irregular figure of the surfaces of the crystalline and cornea.

5. Lastly, that this irradiation is not well distinguished, except in the dark; because, the iris having then a greater opening, the irradiation occasioned by the irregularity of the surfaces of the crystalline and cornea becomes more perceptible.

IV.

On the double Refraction of Light in transparent Crystals: by Mr. LAPLACE.*

Refraction of light.

LIGHT, when it passes from the air into a transparent medium not crystallized, is refracted so that the lines of incidence and refraction are constantly in the same ratio: but in passing through most diaphanous crystals it exhibits a singular phænomenon, which was first observed in Iceland crystal, where it is very perceptible.

Double refraction.

A ray of light, falling perpendicularly on one of the natural faces of this crystal, divides into two parts; one traversing the crystal without altering its direction; the other deviating from it in a plane perpendicular to that face, and passing through the axis of the crystal, that is, through the line that unites the summits of its two obtuse solid angles. This division of the ray generally takes place with regard to any face, natural or artificial, and whatever be the angle of incidence; one portion following the law of common refraction, the other a law of extraordinary refraction first discovered by Huygens; and which, considered as the result of experiment, may be classed among the

Law of the extraordinary refraction discovered by Huygens.

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* Journal des Mines, vol. XXIV, p. 401.

the finest discoveries of that eminent genius. He was led to it by the idea he had formed of the propagation of light, which he supposed to be produced by the undulations of an ethereal fluid. According to him, the velocity of these undulations was less in common transparent mediums, than in a vacuum, and the same in all directions. But he supposed there were two kinds of undulations in Iceland crystal: and that the velocity of one was the same in all directions, as in ordinary mediums; but that the velocity of the other was variable, and represented by the radii of an oblate ellipsoid of revolution, the centre of which was at the point of incidence of the luminous ray on the face of the crystal, and the axis parallel to the axis of the crystal. Huygens had also found, that, to render the hypothesis answerable to experiment, the velocity of the undulations respecting the *ordinary* refraction must be represented by half the conjugate axis of the ellipsoid; which connects in a very remarkable manner the two refractions, the ordinary and extraordinary. This great geometrician did not assign the cause of this variety of the undulations; and the singular phænomenon exhibited by the light in passing from one crystal to another, which will be noticed at the end of this paper, is inexplicable on his hypothesis. This, added to the great difficulties offered by the theory of waves of light, occasioned Newton, and most of the philosophers who have followed him, to reject the law of refraction, that Huygens had attached to it. But Mr. Malus having proved the precision of this law by a number of very accurate experiments, we should separate it altogether from the hypothesis that led to its discovery*. It would be very interesting to connect it, as Newton has done ordinary refraction, with the attractive or repulsive forces, the action of which is sensible only at imperceptible distances. It is, in fact, very probable, that it depends on them; and I have satisfied myself of it by the following considerations.

His hypothesis of the propagation of light.

His law rejected:

but Malus has proved its accuracy.

The result of attraction.

The principle of a minimum of action generally takes place in the motion of a point subjected to forces of this kind. On applying this principle to light, we may set aside the imper-

Principle of least action applied to light.

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* Dr. Wollaston had before shown the hypothesis of Huygens to be agreeable to experience: See Phil. Trans. for 1802, or Journal, vol. IV, p. 148. C.

ceptible curve it describes in its passage from a vacuum into a transparent medium, and consider its velocity as constant, when it has entered into it by a perceptible quantity. The principle of a minimum of action then is reduced to this, that the light arrives from a point without the crystal to a point within it, in such a manner, that, if we add the product of the right line it describes without multiplied by its primitive velocity, to the product of the right line it describes within multiplied by its corresponding velocity, the sum will be a *minimum*. This principle always gives the velocity of light in a transparent medium, when the law of refraction is known: and reciprocally it gives this law, when the velocity is known. But a condition to be fulfilled in the case of extraordinary refraction is, that the velocity of the luminous ray in the crystal shall be independent of the manner in which it entered, and depend only on its position with respect to the axis of the crystal, that is, on the angle which it forms with a line parallel to the axis. In fact, if we imagine an artificial face perpendicular to the axis, all the interior extraordinary rays, that are equally inclined to this axis, will be so likewise to the face, and will evidently be subjected to the same laws at issuing from the crystal: all will resume their primitive velocity in the vacuum; the velocity in the interior therefore is the same for all. I have found, that the law of extraordinary refraction given by Huygens fulfils this condition, as well as it does that of the principle of a minimum of action; which leaves no room to doubt, that it is owing to attractive and repulsive forces, the action of which is sensible only at imperceptible distances. Hitherto it could only be considered as approaching it within limits less than the inevitable errours of experiment: now it may be taken as a precise law.

Case of extraordinary refraction.

Law of Huygens.

A valuable datum for the discovery of the nature of the forces that produce it is the expression of the velocity, to which analysis has conducted me; and which I find equal to a fraction, the numerator of which is unity, and the denominator of which is the radius of the preceding ellipsoid, according to which the light is directed, the velocity in vacuo being taken as unity. I show, that the velocity of the ordinary ray is unity divided by the semiaxis of revolution of the ellipsoid; and by these

Expression of the velocity.

these means the very remarkable connexion, that Huygens found by experiment, between the ordinary and extraordinary refractions of the crystal, is demonstrated *a priori*, as a necessary result of the law of extraordinary refraction. The velocity of the ordinary ray in the crystal therefore is always greater than that of the extraordinary ray, the difference of the squares of the two velocities being proportional to the square of the sine of the angle that the axis forms with the latter ray. According to Huygens, the velocity of the extraordinary ray in the crystal is expressed by the radius of the ellipsoid itself; his hypothesis therefore is conformable to the principle of least action: but it is remarkable, that it is also conformable to the principle of Fermat, which consists in this, that the light arrives from a given point without the crystal to a point within in the shortest time possible; for it is easy to see, that this principle is reduced to that of the least action, by reversing the expression of the velocity. Thus the law of refraction given by Huygens is deducible equally from both of these principles. For the rest, this identity of the laws of refraction, deduced from the mode in which Huygens viewed the refraction of light, with those given by the principle of least action, takes place generally, whatever be the spheroid, the radii of which, according to him, express the velocity of the light in the interior of the crystal. This I demonstrate very simply in the following manner.

Difference of the velocities of the two rays.

Principle of Fermat.

Construction of Huygens.

Huygens considers a ray R C, pl. II, fig. 16, falling on the natural or artificial face A F E K of an Iceland crystal. Drawing a plane, C O, perpendicular to this ray, and taking O K, parallel to C R, to represent the velocity of light in vacuo, he supposes, that all the points C *o* *o′* O of the luminous wave arrive in the same time, and in parallel directions, at the plane K *i′* *i* I; which he finds thus. A F E D is an ellipsoid of revolution, of which C is the centre, C D the semiaxis of revolution; and the radii of which represent, according to Huygens, the respective velocities of the light that follows their directions. Through the ray R C he draws a plane perpendicular to the face, and cutting it in the right line B C K; and through the point K he draws, in the plane of the face, K T, perpendicular to K C. Lastly, through K T he draws a plane K I, touching the ellipsoid in I. According to him C I is the direction

tion of the refracted ray. In fact it is easy to show, that in this construction any given point *o* of the luminous wave arrives at *i*, through the broken line *o c i*, in the same time as O arrives at K. C I representing the velocity of the refracted ray, the right line C I is traversed in the same time as the right line O K. Let us take this for the unit of time, and O K for the unit of space. The point *o* arrives at *c* in a time proportionate to *o c*, and consequently equal to $\frac{C\,c}{K\,C}$. It passes from *c* to *i* in the interior of the crystal, in a time equal to that which the light employs in passing from C to I multiplied by $\frac{K\,c}{C\,K}$, and consequently equal to $\frac{K\,c}{K\,C}$, *c i* being parallel to C I. By adding this time to $\frac{C\,c}{K\,C}$ we shall have unity for the time that the point *o* employs in arriving at *i*.

Let us take $o'\,c'$ infinitely near to *o c*, and parallel to it, the point o' will arrive at i' in the unit of time. Draw the right lines $c'\,o$ and $c'\,i$, and suppose, that the point *o* proceeds to *i* through the broken line $o\,c'\,i$. Now $c'\,o'$ being perpendicular to C O, the right line $c'\,o$ may be supposed equal to $c'\,o'$, and the times required to pass through them may be supposed equal. Moreover, the time required to pass through $c'\,i$ may be supposed equal to the time required to pass through $c'\,i'$, because, the plane K I touching in *i* the spheroid similar to the spheroid A F E D, the centre of which is in c', and the dimensions of which are diminished in the ratio of K c' to K C, the two points *i* and i' may be supposed in the surface of the spheroid. According to Huygens the velocities according to $c'\,i$ and $c'\,i'$ are proportional to these lines; the times employed in passing through them therefore are equal. Thus the time of the transmission of the light in the broken line $o\,c'\,i$ is equal to unity, as in the broken line *o c i*: the differential of these two times therefore is null, which is the principle of Fermat.

The reasoning applicable to any spheroid.

It is clear, that this reasoning is generally applicable, whatever be the nature of the spheroid, and the position of the points *c* and c' on the face of the crystal; even if they be not in the right line CK, provided they be infinitely near it.

The hypotheses of Huygens, though false, represent the fact.

Reversing the expression of the velocity, the principle of Fermat gives that of the least action. The laws of refraction arising from the hypotheses of Huygens, therefore, are

generally

generally conformable to this latter principle; and for this reason these hypotheses, though erroneous, represent the fact.

If we put $b=$ the semiaxis of revolution of the ellipsoid of Huygens, $a=$ its semitransverse axis, $o=$ the velocity of a ray of light in the interior of the crystal, and $V=$ the angle its direction makes with the axis, the radius of the elipsoid will be

$$\frac{a\,b}{\sqrt{a^2-(a^2-b^2).\ sin^2.\ V.}}$$

Thus the velocity v, from the principle of least action, being equal to unity divided by this radius, we shall have $v^2=\frac{1}{b^2}-\left(\frac{1}{b^2}-\frac{1}{a^2}\right).\ \sin^2.\ V.$

This velocity is least when the ray of light is perpendicular to the axis of the crystal, and then it becomes $\frac{1}{a}$: it is least, when it is parallel to this axis, and then it is equal to $\frac{1}{b}$.

Connexion between the two refractions.

Huygens found by experiment, that b is the ratio of the sine of refraction to the sine of incidence in the common refraction of the Iceland crystal. This very remarkable result, which connects the ordinary and extraordinary refractions, is a necessary consequence of the modifications that distinguish the ordinary from the extraordinary ray not being absolute, but solely relative to the position of the ray with respect to the axis of the crystal. To show this, let us refer to the singular phenomenon, that light exhibits after its passage through a crystal.

Phenomena of light after its passage through a crystal.

In passing through a crystal the light is divided into two pencils, one ordinary, the other extraordinary, and each of them issues out of the crystal undivided, If we conceive a second crystal placed beneath the first, in a situation perfectly similar, the ordinary ray will be refracted ordinarily on passing into the second crystal, and the extraordinary ray will be refracted extraordinarily. This will take place generally, if the principal sections of the two opposite faces be parallel. By the *principal section* of a face is meant a section of the crystal by a plane perpendicular to that face, and passing through the axis of the crystal. But, if the principal sections be perpendicular to each other, the ordinary ray will be refracted extraordinarily on passing into the second crystal, and the extraordinary ray will be refracted ordinarily. In the intermediate positions, each ray will be divided into two others at its entrance into the second crystal.

Now suppose a ray refracted ordinarily by one crystal to fall per-

perpendicularly on a second crystal cut by a plane perpendicular to its axis: it is clear, that an infinitely small inclination of the axis to the face of incidence will be sufficient to change this ray into an extraordinary ray. But this inclination can produce but an infinitely small change in the action of the crystal, and consequently in the velocity of the ray within it: this velocity, then, is that of the extraordinary ray, and consequently it is equal to $\frac{1}{b}$: which comes to the same as the result of Huygens: for it is known, that the velocity of light, in common transparent mediums, expresses the ratio of the sines of incidence and refraction, its velocity in vacuo being taken as unity.

Reflection of light.

The principle of least action may serve also to determine the laws of the reflection of light; for, though the nature of the force, that causes light to rebound from the surfaces of bodies, is unknown, it may be considered as a repulsive force, which restores, in a direction contrary to that of the light, the velocity it causes it to lose; as elasticity restores to bodies in a contrary direction, the velocity which it destroys. Now we know, that, in this case, the principle of least action always subsists. With respect to a luminous ray, whether ordinary or extraordinary, reflected by the exterior surface of a body, the principle is reduced to this, that the light passes from one point to another by the shortest path of all those that fall in with the surface. In fact, the velocity of reflected light is the same as that of direct light: and it may be laid down as a general principle, that, when a ray of light, after having experienced the action of as many forces as you please, returns into a vacuum, it resumes its original velocity. The condition of the shortest path gives the equality of the angles of reflection and incidence in a plane perpendicular to the surface, as Ptolemy had already remarked. It is the general law of reflection at the external surfaces of bodies.

Reflection in the case of double refraction.

But when light, on entering into a crystal, is divided into ordinary and extraordinary rays, one portion of these rays is reflected by the interior surface at their exit from the crystal. In being reflected, each ray, whether ordinary, or extraordinary, divides into two others; so that a solar ray, penetrating the crystal, forms by its partial reflection at the surface of emission four distinct pencils, the direction of which I shall proceed to determine.

Let

Case of parallel surfaces.

Let us suppose the surfaces of entrance and emission, which we will call the first and second faces, to be parallel to each other. Let the thickness of the crystal be imperceptible, yet greater than the sum of the radii of the spheres of activity of the two faces. In this case it will be demonstrated, by the preceding reasoning, that the four reflected pencils will form but one perceptibly, being in the plane of incidence of the generating ray, and forming with the first face an angle of reflection equal to the angle of incidence. Now let us restore the crystal to its proper thickness : it is clear, that, in this case, the reflected pencils, after issuing from the first face, will assume directions parallel to those they had taken in the former case : these pencils, therefore, will be parallel to each other, and to the plane of incidence of the generating ray; only, instead of being confounded to the senses, as in the former case, they will be separated by distances so much the greater, as the crystal is thicker.

Now, if we consider any given interior ray issuing out in part by the second face, and in part reflected by it into two pencils, the issuing ray will be parallel to the generating ray ; for the light, as it issues out of the crystal, must take a direction parallel to that it had on entering into it; since, the faces of entrance and exit being supposed parallel, it is acted on at its exit by the same forces as it was at its entrance, but in the opposite direction. In the direction of the issuing ray, let us conceive a plane perpendicular to the second face ; and in this plane let us imagine a right line, exterior to the crystal, passing through the point of exit, and forming with the perpendicular to the face, but on the side opposite to the direction of the issuing ray, the same angle as that direction ; lastly, let us conceive a ray of light entering the crystal according to this right line. This ray, at its entrance, will be divided into two others, which, at issuing out of the crystal by the first face, will take directions parallel to that of the ray before its entrance by the second face; they will be visibly parallel to the directions of the two reflected pencils ; which cannot take place but as far as the two rays, into which the ray of light is divided on entering by the second face, confound themselves respectively in the interior of the crystal with the directions of the two reflected pencils. But the law of Huygens gives the directions of the rays into

The law of

which

Huygens applicable here,

which the ray of light is divided; therefore it will give those likewise of the two pencils reflected in the interior of the crystal.

and in the case of surfaces not parallel.

If the two faces of the crystal be not parallel, we shall have by the same law the directions of the two rays, into which the generating ray is divided in entering by the first face: we shall have also, by this law, the direction of each of these rays at its exit by the second face: next, the preceding construction will give the directions, in the interior of the crystal, of the four pencils reflected by this face: and, lastly, by the law of Huygens we may deduce their directions at issuing out of the crystal by the first face. Thus we shall have all the phenomena of the reflection of light by the surfaces of transparent crystals. Mr. Malus first discovered these laws of the reflection of light, and he has confirmed them by a great number of experiments. Their agreement with the results of the principle of least action completes the demonstration of the position, that all these phænomena are owing to the action of attractive and repulsive forces*.

V.

Description of a Reflecting Circle, in which the Screens can be readily shifted in taking altitudes: by Mr. J. ALLAN, *Blewitt's Buildings, Fetter Lane.*†

SIR,

Improved reflecting circle.

I BEG leave to inform you, that on Thursday last I left at the Society's house a mathematical instrument, adapted for the use of mariners, which I wish to submit to the Society's attention. It is a reflecting circle, commonly called, La Borda's circle, for the purpose of taking altitudes and distances at sea; and which I have greatly improved lately, by fixing the shade glasses different to what had heretofore been done, with some other improvements as a reflecting circle. The late Dr. Mackay,

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* For the two papers of Mr. Malus on this subject, see Journal, vol. XXX pp. 95 and 161

† Trans. of the Soc. of Arts, &c. vol. XXIX p. 106. The silver medal and twenty [illegible] to Mr. Allan for this improvement.

in a publication of his, called Mackay's Longitude, has a plate of La Borda's original instrument, but the shade glasses are so fixed, as to render the instrument useless, and which he was convinced of, on my pointing out to him the fault. He said he would alter his plate to my method, and that he would state it as my improvement; but his death soon afterwards prevented it. I am aware the Society do not confer their rewards without advantageous qualities to merit their sanction. I respectfully say, that I consider my instrument to have merit, both in economy, and in the great improvement made on the plan of the reflecting circle first invented. I shall be happy to point out this to the Society, and have the honour to be,

Defect in La Borda's.

Sir, your humble servant,

JAMES ALLAN.

Blewitt's Buildings, Fetter Lane,
Dec. 24th, 1810.

SIR,

Agreeably to the intimations of the committee on Thursday evening last, I beg leave to explain to the Society the properties of my improved reflecting circle; and which, with a theodolite attached to it, would be useful both to the mariner and surveyor.

Useful both to mariners and surveyors.

The committee inquired what sort of centre or axis the instrument had. I beg leave to state, it is an improved one of mine. The former way of centring this instrument was only by a single pin, which both indexes acted upon; but the pin had so little bearing in the index, that it was not sufficient to keep the index-glass upright to the plane of the instrument in all its positions; I have therefore contrived to put what is called in our business a male and female centre or axis, upon a simple but accurate method.

Mode of centring.

Permit me to make a few observations on circular instruments in general. I believe it will be universally allowed, that it is easier to make a circle nearer to truth, with respect to its horizontal plane, than it is to make a separate part of a circle so.

Horizontal plane of a whole circle,

A sextant is only the sixth part of a circle, and is got flat by means of a plane, as near as the maker can get it, but is not turned on its own axis as a circle is: therefore I have no doubt, but that the best sextant usually made is very short of the

made truer than of a part.

the horizontal truth of a sixth part of a circle; and if we were to suppose a circle made of six of the usual sextants, it would be a very untrue circle with respect to its horizontal plane.

Reflecting circles made in various ways.

It has, therefore, been a general desideratum, that a circular instrument of reflection should be introduced, of simple construction, easy to adjust, and convenient for use. I have been induced to make several circular instruments of reflection in various ways, but none upon so simple a construction, or so cheap as the present, nor so well calculated to prove any untruth, as my improvement upon Borda's; and I believe it will now be generally adopted for use.

Borda's unsatisfactory.

There have been great numbers of Borda's circles made; I myself assisted about twenty-five years ago to make many, also since I have been in business for the last twelve years on my own account, but I never found any of them to give satisfaction till I invented the present improvement.

Captain M'Lennan, who traded to South America, had one of Borda's circles made, similar to that described in Dr. Mackay's Longitude, but could not use it till altered by me last April.

The improved circle well adapted for proving its correctness.

The glasses in my instrument are movable to any quarter that a person may wish to use it in; and by taking the same angle with each quarter, it affords an opportunity of proving the correctness of the instrument, which circumstance I hope justifies me in saying, that it is the only instrument of reflection that I know, so well calculated to prove itself. I beg pardon for being so tedious; I assure you that I can make the instrument better than I can write or talk about it.

I have the honour to be, Sir,
Your humble servant,
JAMES ALLAN.

Blewitt's Buildings, Jan. 16, 1811.

Testimonials in its favour.

A certificate was produced from Captain H. C. Coxen, R.N. dated February 5th, 1811, stating his opinion, that the dark screens which are fixed to Mr. Allan's reflecting circle, so as to act in the manner they do in a sextant, are improvements on the reflecting circle of Mr. Borda, which are not so fixed.

That it must be evident, even to the least experienced mariner, that there are frequent occasions, in taking the altitude of the sun, to change the screens *alternately*, in the shortest possible

possible time, which cannot be effected in near so short a time by screens which take off and on, as in Borda's reflecting circle.

Captain Mackay, who has commanded the Lord Forbes, in the Jamaica trade, for twenty years, stated, that the manner in which Mr. Allan's screens are fixed in his reflecting circle is a great improvement. That from not being obliged to take out the shades when the sun is clouded, the object is not lost; and that when an instrument is obliged to be taken from the eye, to fix the screen in the old mode, the object is lost.

That by this instrument being a reflecting circle, it makes sure of a horizontal plane well divided, which can hardly be the case in a portion only of a circle.

That Mr. Allan's is the most complete instrument he has ever seen, and that he shall always take one with him to sea.

Description of the Drawing of Mr. James Allan's Improvement on the Reflecting Circle of Borda. Pl. III.

Description of Mr. Allan's improved reflecting circle.

The reflecting circle, first invented by Tobias Mayer, of Gottingen, and afterwards improved by the chevalier La Borda, of Paris, is an instrument, which in its principle admits of such a degree of accuracy, as to be of the most important service to navigators; but it has hitherto been constructed in such a manner, that the inconveniences attending the use of it have prevented its general adoption among seamen: any contrivances, therefore, tending to diminish these inconveniences, were deserving of the Society's notice. The construction of Borda's circle, as it has hitherto been made, is minutely detailed in Dr.Rees's New Cyclopædia, article, Circle; and the mode of using it is there explained; it will be therefore unnecessary to describe any thing more of the circle delineated in pl. III, than is essential to the elucidation of the improvements made by Mr. Allan.

Application of the dark glasses.

The first of these is in the mode of applying the dark glasses, which are fixed on joints, so as to turn back out of the way, in the same manner as in the sextant. In the old instrument these glasses were fitted into sockets provided with tenons on the indexes, and fastened by a milled head screw, which took much time to change them. The second is the addition of

Double verniers.

double verniers to the index, carrying the telescope and horizon glass; these read upon opposite sides of the circle, and if a difference is observed between these readings, by taking the mean of them the errour arising from any eccentricity the index may have, will be corrected. And the third consists in fixing the index glass upon an axis, accurately fitted into the centre of the circle. By this means it is assured, that the index glass, in turning round, shall always be exactly perpendicular to the plane of the circle. In the old method, when the index-bar was merely fitted on a pin fixed in the centre of the circle, it was impossible to make the circle so perfectly flat, or keep the index so accurately in contact with it, as by having an axis. To explain these improvements more perfectly, the reader is referred to plate III, which contains a perspective view of the instrument; A, is the circle with six arms; B, is the index carrying the telescope C, and the horizon-glass D, with the two clusters of dark glasses E and F. At the opposite ends of this index are the two verniers *a* and *b*; the former has the clamp screw and slow movement attached to it; consisting of a screw *c*, which fixes the index to the circle; and *d* the tangent screw, which will move the index a small quantity when turned, to adjust it accurately. G is the index mirror screwed upon the index H, which has also a vernier, and a clamp and tangent screw *e e*, similar to the other. I is the handle by which the instrument is held when in use; it is fitted to a socket K, which is screwed to the centre of the circle, and is unscrewed from the circle when packed away. The handle is fitted to a springing socket, so as to turn round upon the socket K, that it may be turned to any side of the circle for the convenience of holding it; it may be fastened by a small milled nut, seen in the figure, which binds the ends of the spring socket together. L is a magnifying glass for the purpose of reading the divisions of the verniers; it is fitted upon a pin screwed into the indexes, and may be applied to either. The figure 2 in the corner of the plate is a section, showing the construction of the central part of the circle, where M is a section of the thickness of the circle, with a hole through the centre, and a recess turned out in the lower side to receive a centre piece N, which is fixed in with three small screws; a hole is turned in the centre of this piece, and an axis O is fitted into it with the utmost accuracy; this axis has

Centring of the index glass.

Explanation of the plate.

has a flanch on the upper end, by which it is screwed to the index H, and upon this, the under glass G, fig. 1, is fastened, by other screws passing through a piece projecting from the back of it. The axis is held in its place by a collet *r*, fitted on a square part of it, and held fast by a screw *s*; beneath this a piece is fixed on in the centre of the circle, the edge of its flanch being shown by *t* in fig. 1; it is part of the screw which holds on the spring socket K, for the handle I. The upper end of the centre piece N, which comes up above the circle, is turned extremely true, and upon this the index B is fitted, or rather a brass ring *v* screwed to it, so as to turn round upon it as a centre.

The telescope C is fixed to the index by two cocks and by two screws XX, in these it can be raised up or lowered, to adjust the different brightness of the two objects seen in the horizon glass D, the one reflected from the central mirror G, and the other seen directly through it. The dark glasses at E are intended to moderate the light of the sun, in passing from the index to the horizon glasses; the frames containing these glasses have holes E through them, to see through the telescope and horizon glass; the other dark glasses, F, are situate behind the horizon glass D, and may be turned up or down, as occasion requires.

The instrument is used in the same manner as the common reflecting circle; the angle being first taken on one side of the parallelism of the glasses, and then on the other; so that the angle is doubled; then it is repeated on a fresh part of the circle, as many times as the observer thinks proper, and the product divided by the number of observations taken. The mode of taking these observations is explained at full in Dr. Rees's Cyclopædia, and in Dr. Mackay's publication on the means of finding the longitude.

VI.

VI.

METEOROLOGICAL JOURNAL,

1812.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
7th Mo.										
JULY 30	N W	29·96	29·80	29·880	61	46	53·5			☽
31	S W	29·96	29·80	29·880	64	51	57·5	·20	·08	
8th Mo.										
AUG. 1	Var.	29·86	29·80	29·830	61	53	57·0	—	·02	
2	N E	29·86	29·80	29·830	64	54	59·0	—	·10	
3	Var.	29·85	29·80	29·825	63	52	57·5	—	·02	
4	N E	29·90	29·85	29·875	65	50	57·5	—	·32	
5	S W	29·95	29·90	29·925	57	50	53·5	·29	·23	●
6	Var.	30·00	29·95	29·975	63	47	55·0	—	·41	
7	N W	30·00	29·95	29·975	61	49	55·0	—		
8	N W	29·96	29·94	29·950	57	51	54·0	·17	·03	
9	N W	29·97	29·96	29·965	57	45	51·0	—		
10	N W	29·97	29·96	29·965	58	53	55·5	—	·02	
11	Var.	30·07	29·96	30·015	63	49	56·0	—	·05	
12	N E	30 14	30·07	30·105	57	44	50·5	·18		☾
13	N	30·15	30·14	30·145	64	43	53·5	—		
14	N E	30·15	30·12	30·135	67	49	58·0	—		
15	E	30·12	30·07	30 095	65	50	57·5	—		
16	E	30·06	30·05	30.055	68	54	61·0	·34		
17	S E	30·05	29·98	30·015	73	55	64·0	—		
18	S E	29·98	29·76	29·870	78	58	63·0	—		
19	S W	29·96	29·76	29·860	72	55	63·5	·56	·01	
20	W	30·00	29·98	29·990	71	55	63·0	—		○
21	S W	29 97	29·94	29·955	69	57	63·0	—		
22	W	30·05	29·97	30·010	68	53	60·5	—		
23	S W	30·04	29·86	29.950	70	60	65.0	·66		
24	S W	30·10	29·86	29·980	66	47	56·5	—	·05	
25	S W	30·10	30·04	30·070	69	52	60 5	—		
26	N W	30·04	29·96	30·000	67	53	60·0	·35		
		30·15	29·76	29·968	78	43	57·91	2·75	1·34	

The observations in each line of the table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

NOTES.

REMARKS.

Eighth Month. 4. Wet afternoon. 5. Wet morning. 6. "The day was gloomy : about 4. p. m. a very heavy shower commenced, which continued for about 20 minutes, then abated for a short time, but increased again, and continued all the evening, with thunder and lightning : the barometer was nearly stationary." Such were the phænomena at the laboratory, where there fell 1·39 inches of rain. At Plaistow, two miles distant, there appears to have fallen only 0·41 inches of rain, and I find only this note, "Thunder in the afternoon." 13. Foggy morning : a *stratus* at night. 14. The same. 17. The same : Lunar halo. 18. Some lightning during the night. 21. Thunder between one and two p. m. 24. Bright moonlight. 28. The wind this night very high. 30. Very showery.

RESULTS.

Prevailing winds westerly.

Barometer : highest observation 30·15 inches ; lowest 29·76 inches ; Mean of the period 29·968 inches.

Thermometer : highest observation 78° ; lowest 43. Mean of the period 57·91°.

Evaporation 2·75 inches. Rain 1·34 inches.

Plaistow. **L. HOWARD.**
***Ninth Month*, 15, 1812.**

P. S. The observations on the barometer, wind, and evaporation, with the remarks, for the last two months, are chiefly due to my friend, John Gibson. The account of temperature and rain was carefully kept (during my absence) at Plaistow.

VII.

An Account of some Experiments on the Combinations of different Metals and Chlorine, &c. By JOHN DAVY, *Esq. Communicated by Sir* HUMPHRY DAVY, *Kt. LL. D., Sec. R. S.*

(Concluded from p. 21.*)*

4. *On the Combinations of Chlorine with Manganese, Lead, Zinc, Arsenic, Antimony, and Bismuth.*

Only 1 compound with some metals.

I HAVE attempted, by several methods, to obtain more than one combination of these different metals and chlorine, but without success.

Compound of oximuriatic acid and manganese.

I have procured a compound of manganese and chlorine, by evaporating to dryness the white muriate of this metal, and heating to redness the residue in a glass tube, having only a very small orifice. Muriatic acid vapour was produced, and a fixed compound remained, which required a red heat for its fusion, and was not altered by the strongest heat that could be given to it in the glass tube; but was rapidly decomposed when heated in an open vessel, muriatic acid fumes being evolved, and oxide of manganese formed, which was black or red, according to the intensity of the heat applied. The compound of manganese and chlorine is a very beautiful substance, it is of great brilliancy, generally of a pure delicate light pink colour, and of a lamellar texture, consisting of broad thin plates.

Mode of freeing it from iron.

There is not much difficulty in obtaining this compound pure. Iron, with which manganese is commonly contaminated, may be separated by two or three repetitions of the solution of the compound in water, the evaporation to dryness of the clear filtered muriat, and fusion of the residue procured by evaporation. Indeed, I think this a good general method for purifying manganese from iron. One of the combinations of the latter metal and chlorine being volatile, heat must separate it from the compound of manganese. And I have thus obtained it so free from iron, that triple prussiate of potash, added to its solution in water, gave merely a white precipitate, without the slightest tint of blue.

Its properties.

This compound deliquesces when exposed to the atmosphere, and is converted into the white muriate. Like ferrane, it affords a trifling residue when heated with water. The residue

sidue is oxide of manganese, white at first, but soon becoming red, and even black; it varies in quantity, according to the exclusion of air in the formation of the combination.

Analysis.

50 grains of the compound dissolved in water, with the exception of 1 grain; this residue was separated by decantation of the fluid, washed, dried, and heated to redness, it was in the state of black oxide. The colourless solution was precipitated by nitrate of silver. The horn silver formed, when dried, was equal to 108 grains. Hence, omitting the 1 grain of mixed oxide, 100 of this compound appear to consist of

54 chlorine
46 manganese

100

Horn lead.

The horn lead, that I have analysed, was made by the decomposition of the nitrate of lead by muriatic acid, and it was well washed, dried, and fused in a glass tube with a small orifice. The strongest red heat that I could apply to it, under these circumstances, did not occasion its sublimation.

Analysis.

50 grains of it, that had been fused, were dissolved in water. This solution, heated with nitrate of silver, afforded 52·65 grains of dry horn silver. Hence 100 of horn lead appear to be composed of

25·78 chlorine
74·22 lead

100·00

As this compound, when decomposed by an alkali, affords the protoxide of lead, it may be called plumbane.

Butter of zinc.

The butter of zinc I have examined was obtained by evaporating to dryness the muriate of this metal, and by heating to redness the residue in a glass tube. This compound is not volatile at a strong red heat in a close vessel, it fuses before it acquires a dull red heat, and on cooling it goes through several degrees of consistence, being viscid before it becomes solid.

This compound, when heated with water, affords a small residue of oxide of zinc, which, as in the preceding instances, may be considered as in the state of mechanical mixture.

In consequence of its powerful attraction for water, it is a very

very deliquescent substance; on this account it is necessary to weigh it in water to avoid errour. 49·5 grains of it, thus weighed, dissolved entirely in water, with the exception of 1 grain of oxide of zinc, which was separated by decantation, and dried and ignited, and its quantity ascertained to be as stated. The solution precipitated by nitrate of silver afforded 99 grains of dried horn silver. Hence, excluding the 1 grain of oxide, 100 of butter of zinc seem to consist of

50 chlorine
50 zinc

100

This compound may be called zincane.

Fuming liquor of arsenic.

A compound of chlorine and arsenic has been long known, bearing the name of the fuming liquor of arsenic. It may be formed in several ways: by the combustion of arsenic in chlorine gas; by heating in a retort a mixture of arsenic and corrosive sublimate, or of arsenic and calomel; and by the distillation of muriate of arsenic with concentrated sulphuric acid. The old method by means of corrosive sublimate appears best adapted for procuring it in a pure state. About 6 parts of corrosive sublimate to 1 of arsenic are, I find, proper proportions. The mixture of the two substances should be intimate, and the heat applied to the retort for the distillation of the fuming liquor, gentle. When the liquor was not colourless at first, I have purified it by a second distillation.

Its properties.

The fuming liquor of arsenic, it is well known, is decomposed by water. The precipitate produced appears to be merely white oxide of arsenic, for, independent of other circumstances, it does not afford the fuming liquor when heated with strong sulphuric acid.

The fuming liquor, when gently heated, dissolves phosphorus, but it retains on cooling only a very small portion of this substance. The warm solution is not luminous in the dark.

The fuming liquor also, when warm, readily dissolves sulphur; indeed sulphur fused in the liquor seems capable of combining or of mixing with it in all proportions; but on cooling the greatest part of the sulphur is deposited, and assumes a fine crystalline appearance; the form of the crystals was apparently the octahedron. This deposition seems to be merely sulphur

sulphur with a little of the fuming liquor between the interstices of the crystals, for the crystals bear washing, and become tasteless superficially, but remain still acid internally, where the water has not penetrated.

It likewise dissolves resin. That which was called rosin was the subject of experiment. The solution was of a blueish green colour; but when gently heated it became brown, and remained so on cooling. The portion of resin the fuming liquor is capable of taking up is very considerable; when the resin was added in excess, a viscid mixture was formed. The resinous solution was decomposed by water, and the resin was separated, apparently unaltered, mixed with white arsenic.

The fuming liquor is capable of combining with oil of turpentine and with olive oil. When the mixture was made with either of these oils, there was a considerable elevation of temperature, and a homogeneous colourless fluid was in each instance obtained.

Analogous to the fuming compounds of sulphur and phosphorus.

In these and some other properties, the fuming liquor of arsenic is analogous to the fuming compounds of chlorine and sulphur, and chlorine and phosphorus; these two, having the power of dissolving sulphur, and phosphorus, and resin, and of entering into union with the fixed and volatile oils.

Its component parts.

It is difficult to ascertain the proportion of the constituent parts of this compound by the ordinary modes of analysis; I have chosen therefore a synthetical method in preference; and from repeated experiments I find, that 2 grains of arsenic require for complete conversion into the fuming liquor 4 cubic inches exactly of chlorine gas.

The experiments were thus conducted: the arsenic in one piece was put into a small glass retort having a stop-cock; the retort was exhausted, and a known volume of chlorine gas was admitted from a graduated receiver by means of other stopcocks; and the absorption of chlorine, after the entire conversion of the metal into the fuming liquor, was considered as the proportion condensed by the arsenic.

Now, since 100 cubic inches of chlorine gas weigh just 76·5 grains, 2 grains of arsenic combine with 3·06 grains of chlorine, the weight of 4 cubic inches of the gas. Hence 100 of the fuming liquor appear to consist of

60·48 chlorine
39·52 arsenic

100·00

As the fuming liquor gives the white oxide when decomposed by water, arsenicane may be substituted for its old name.

Butter of antimony.

The butter of antimony is a well known substance. That which I have examined was obtained by heating together corrosive sublimate and antimony, or antimony and calomel; and was always purified by a second distillation at a low temperature. The best proportion of corrosive sublimate and the metal for making the compound, I have found to be about 2½ parts of the former to 1 part of the latter.

Its properties.

The butter of antimony, like arsenicane, is capable, when rendered fluid by heat, of dissolving resin and sulphur, and of combining with the fixed and volatile oils. It affects the oil of turpentine very like the liquor of Libavius; the action is considerable, much heat is produced, and the oil is rendered brown.

When the butter of antimony is decomposed by a sufficiently large quantity of the hydrosulphuret of potash, that compound is formed, which is commonly called the golden sulphur of antimony; and which, when decomposed by heat, I have found to afford merely water and sulphuret of antimony*.

Its component parts.

To ascertain the proportion of antimony in the butter of antimony, 60·5 grains of this substance, colourless and crystallized, weighed in water, were heated in a solution of hydrosulphuret of potash. The whole of the antimony was dissolved, and the hydrosulphuret of potash being in excess, there was no precipitation on cooling. The solution was decomposed by

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Golden sulphur of antimony.

* These results appear to me to demonstrate the truth of Mr. Proust's opinion, that the golden sulphur is a hydrosulphuretted oxide of antimony. From my experiments the only difference of composition between kermes mineral and the preceding compound seems to consist in the former containing a smaller proportion of sulphuretted hidrodrogen than the latter; for I have obtained by the decomposition of kermes mineral, by heat, a compound of sulphuret of antimony and protoxide; and I have converted kermes into the golden sulphur, by means of water impregnated with sulphuretted hidrogen.

muriatic

muriatic acid, and the golden sulphur thus thrown down was collected on a filter, well washed, and dried; heated slowly to redness in a glass tube, steam in plenty was disengaged with very slight traces of sulphur, and sulphuret of antimony remained, which, fused into one mass, weighed 45 grains. According to the experiments of Proust, which I have repeated with the same result, sulphuret of antimony contains 74·1 per cent of metal. Hence 45 grains of sulphuret, or the 60·5 of butter of antimony, from which the sulphuret was procured, must contain 33·35 of metal; and considering the remainder 27·15 of the 60·5 as the proportion of chlorine, 100 of the butter of antimony seem to consist of

39·58 chlorine
60·42 antimony

100·00

This compound, as it yields, when decomposed by water, the submuriated protoxide, may be called antimoniane or stibiane.

Butter of bismuth.

A compound of bismuth and chlorine has been long known, bearing the name of the butter of bismuth. It is obtained both when bismuth is heated with corrosive sublimate and calomel. 2 parts of corrosive sublimate to one part of metal I have found good proportions for its preparation. There is some difficulty in procuring it pure, and entirely free from the mercury revived; this is most readily effected by keeping the butter of bismuth in fusion, at a temperature just below that at which mercury boils; the mercury slowly subsides and collects in the bottom of the vessel, and this operation, continued for an hour or two, affords a pure, or nearly pure, butter of bismuth. Thus prepared, it is of a grayish white colour, opaque, uncrystallized, and of a granular texture. In a glass tube, with a very small orifice, it bears a red heat without subliming.

As a hydrosulphuret of bismuth is produced when the butter of bismuth is heated with the hydrosulphuret of potash; and as this hydrosulphuret, like that of antimony, affords, when decomposed by heat, a sulphuret and water; I have applied the same mode of analysis to this compound as to the last.

Analysis of it.

55 grains of butter of bismuth were decomposed in a warm solution of hydrosulphuret of potash. The dark brown hydrosulphuret of bismuth thus formed, and not dissolved, was collected

collected on a filter; the hydrosulphuretted solution was decomposed by muriatic acid, the slight precipitate of hydrosulphuret produced was added to the first portion, and the whole was well washed, dried, and heated to redness in a glass tube; the sulphuret of bismuth thus obtained, fused into one mass, weighed 44·7 grains. I had previously ascertained the proportion of metal in this sulphuret, and found it to be 81·8 per cent. 44·7 grains of sulphuret, or 55 grains of the butter, must therefore contain 36·5 grains of bismuth; and hence, 100 of bismuth appear to consist of

33·6 chlorine
66·4 bismuth

100·0

The butter of bismuth may be called bismuthane.

Singularities of these compounds with regard to volatility and fusion.

Among the preceding combinations of the metals and chlorine, there is a surprising difference in respect to volatility and fusibility. Iron and manganese, two difficultly fusible metals, form with chlorine readily fusible compounds, and a combination of the former metal and chlorine is even volatile; the compounds of tin and chlorine, and of chlorine and antimony, are very volatile substances, though the metals themselves are fixed at very high temperatures; on the contrary, the combinations of chlorine with bismuth, zinc, and lead, do not exceed in fusibility; indeed are not quite so fusible as the metals themselves. I can offer no explanation of these phenomena.

Singularity of the fuming compounds.

Another singularity attending the liquid fuming compounds of chlorine, such as the liquor of Libavius, the fuming liquor of arsenic, and the oxymuriates of sulphur and phosphorus, is, that they do not become solid at low temperatures. I have reduced, by means of a mixture of snow and muriate of lime, the temperature of all these substances 20 degrees below the zero of Fahrenheit's thermometer, but without affecting their liquidity.

Influence of atmospheric air at high temperatures.

The influence of atmospheric air on the compounds of the metals and chlorine at high temperatures is curious, and worthy of particular attention. The combinations of chlorine with lead, zinc, copper, and bismuth, appear to be volatile in open vessels, and fixed in closed ones. How moist air operates in these

these instances, it is difficult to say. In other cases, where it evidently acts chemically, the changes explain themselves; thus, when the compounds of iron and chlorine, and of manganese and chlorine, are heated in the open air, hygrometrical water of the atmosphere seems to be decomposed, as muriatic acid fumes are produced, and oxides of the metals formed. Probably the volatility of the other compounds is connected with similar circumstances. This action of moist air has hitherto been much neglected; it is certainly worthy of being more fully inquired into, both in a theoretical and practical point of view. Its importance in practice is exemplified in the reduction of horn silver, and in the formation of several of the compounds of chlorine and the metals; if moist air be admitted in these operations, the silver will be lost, and the compounds not formed.

Action of moist air important.

Guided by analogy, I have been led to try whether the muriate of magnesia, which is readily decomposed by heat in the open air, would not, when the air was excluded, by introducing it into a glass tube with a very small orifice, afford a permanent compound. The result was agreeable to my expectations; I obtained, by strongly heating the muriate for a quarter of an hour, a substance like enamel in appearance, being semifused, and which appeared to be a mixture of magnesia and the true compound of magnesium and chlorine, for heated with water magnesia was separated, and a muriate of magnesia formed.

Action of heat without air on muriate of magnesia.

5. *On the Relation between the Proportion of Oxigen and Chlorine in Combination with several Metals.*

Errours being very common in chemical analyses, even in those conducted most skilfully and carefully, all possible means should be taken to discover them; and no means, I think, promise to be more effectual for this purpose, than the general analogy of definite proportions. From a great variety of facts, it appears that oxigen and chlorine combine with bodies in the ratio of 7·5 to 33·6. With 1 part by weight of hidrogen, for example, 7·5 of oxigen unite to form water; and 33·6 of chlorine unite with the same proportion to produce muriatic acid gas. To judge therefore of the accuracy of the analyses of the preceding combinations of the metals and chlorine,

Doctrine of definite proportions a test of the accuracy of analyses.

chlorine, it is only necessary to compare them with the analyses of the oxides of the same metals. If the two agree, there will be reason to consider them both correct, but should they disagree, there is equal reason for supposing one or both of them to be wrong.

Applied to the compounds of copper;

Thus as the orange oxide of copper is analogous to cuprane, and the brown oxide to cupranea, the oxigen and chlorine should be to each other in these compounds as 7·5 to 33·6. And from comparison of my analysis with those of Mr. Chenevix and Mr. Proust, it appears, that in the two first, copper being as 60, the oxigen is to the chlorine as 7·79, instead of 7·5 to 33·77, instead of 3·36; and in the two last as 7·5 to 33·6, or as 15 to 67·2. Coincidences as near as might be reasonably expected.

of tin;

There is not the same agreement between Mr. Proust's analyses of the oxides of tin and the preceding ones of the combinations of this metal and chlorine. This discordance induced me to repeat my analyses; and, obtaining the same result as at first, I directed my attention to the oxides of tin, and made the following experiments to ascertain the proportion of their constituent parts.

(Experiments on the oxides of tin.)

42·5 grains of tin, which had been precipitated from the muriate of this metal by zinc, were heated with nitric acid in a platina crucible, and slowly converted into peroxide; the acid and water were driven off by gentle evaporation at first, and afterward by a strong red heat, continued for a quarter of an hour. The peroxide thus produced was of a light yellow colour, and being very gradually dried, it was semitransparent, and hard enough to scratch glass; it weighed 54·25 grains. Hence, as 42·5 grains of tin acquire, on conversion into peroxide, 11·75 grains of oxigen, this oxide appears to contain 21·66 per cent of oxigen, just the quantity found in the native oxide by Klaproth, instead of 28, the proportion stated by Proust.

Mr. Berthollet, jun., has shown, that Mr. Proust's estimate of 20 per cent of oxigen in the protoxide is incorrect. To ascertain the true proportion, 20 grains of tin were dissolved in strong muriatic acid in a retort connected with a pneumatic apparatus, and without the assistance of heat; 16 cubic inches of hidrogen gas were produced. (Barom. 30, thermom. 60.)

As

As the production of this quantity of hidrogen indicates an absorption of oxigen by the tin equivalent to 8 cubic inches, or (as 100 cubic inches weigh 34·2 grains) to 2·736 grains, the protoxide of tin appears to contain 11·99 per cent of oxigen.

These analyses of the oxides, compared with those of the combinations of tin and chlorine, are found very nearly to agree. The ratio of oxigen to chlorine in the first two similar compounds, the tin being as 55, is as 7·5 to 33·4; and in the last two, viz. the peroxide and the liquor of Libavius, as 7·6 to 33·5, or as 15·2 to 67.

of iron;

As the black oxide of iron is formed by the decomposition of ferrane by a solution of potash, and the red oxide by that of ferranea, it is evident, that these oxides and combinations of iron and chlorine should coincide in the proportions of their constituent parts. This appears from the analyses* of Dr. Thompson to be nearly the case; for, iron being as 29·5, the oxigen is to the chlorine in the black oxide and ferrane as 8 instead of 7·5 to 33·6; and in the two others as 8 to 33·6, or as13·2 to 55·5. Here the agreement is less than in other instances; but this is not surprising, considering the different estimates of the proportions of oxigen in the oxides of iron, and the difficulty of ascertaining them correctly.

of other metals;

The yellow oxide of lead, and the white oxides of antimony, bismuth, zinc, and arsenic, are formed, when the combinations of these metals and chlorine are decomposed by a solution of potash. But on comparison with the best analyses of the oxides, there is not, excepting in the case of zinc and arsenic, that coincidence of proportions which might be expected.

of zinc;

of arsenic;

Zinc being as 34·5, the oxigen in the oxide, from the analysis of Proust, is to the chlorine as 7·5 to 34·4; and the arsenic being as 21·9, the oxigen, from the analysis of the same chemist, is to the chlorine as 7·3 to 33·6. The analyses of the oxides of the other metals being at variance with those of the chlorine combinations, I was induced to make the following experiments, with the hope of discovering the cause of the difference.

of lead;

100 grains of lead, which had been precipitated from the nitrate of lead by zinc, were dissolved in nitric acid, and thrown down by carbonate of potash. This precipitate of carbonate of

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* Nicholson's Journal, vol. XXXVII, p. 375.

lead was well washed and dried, and heated to dull redness for a quarter of an hour in a platina crucible: by this treatment all the carbonic acid was expelled; the remaining yellow oxide weighed 107·7 grains, and it dissolved in muriatic acid without effervescing, and without affording any residue of brown oxide. Hence, the yellow oxide of lead appears to contain 7·15 per cent of oxigen. And this proportion of oxigen in the oxide compared with that of chlorine in plumbane, lead being as 97·2, appears to be in the ratio of 7·5 to 33·8, instead of that of 15·6 the estimate of Klaproth, or of 11·2 the estimate of Dr. Thompson to 33·8. Klaproth might have been misled by considering the hydrated oxide as a true white oxide free from water.

(Oxide of lead)

of antimony;

(Oxides of antimony.)

According to Mr. Proust, the peroxide of antimony contains 23 per cent of oxigen, and the protoxide 18*. I have repeated this chemist's experiments; my results, in which the peroxide is concerned, agree with his; but there is not the same concordance in those relating to the protoxide. The protoxide I used was either prepared by the decomposition of the butter of antimony, or of the sulphate, by a boiling solution of carbonate of potash. This oxide, in its purest state, I have always found, as Mr. Proust describes it, of a light fawn colour before fusion, and afterward in mass of a gray colour, and of a radiated crystalline texture. 100 grains of it that had been fused were heated in the state of powder with strong test nitric acid in a platina crucible: when nitrous gas ceased to be produced, the excess of nitric acid was expelled by a gentle heat, and the oxide was heated to dull redness; the increase of weight after this was equal to 10·4 grains: nitric acid was again added, and the process repeated, but without any alteration of weight being produced. Hence, as the peroxide contains 23 per cent, the protoxide seems to contain 15 per cent; which proportion of oxigen very nearly agrees with that of chlorine in the butter of antimony; for, antimony being as 42·5, the former is to the latter as 7·5 to 34·6, instead of 33·6. I put some confidence in this estimate of the proportion of oxigen in the protoxide, not only on account of its agreement with the analysis of the butter of antimony, but because it was confirmed on the repetition of the experiment.

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* Journal de Physique, Tom. LV,

Klaproth

Klaproth concludes from his experiments, that the oxide of bismuth, prepared by means of nitric acid, contains 17·7 per cent of oxigen; and in consequence this oxide has been considered distinct from that which is formed by direct calcination of the metal, and which contains a much smaller proportion. But there is reason to believe, that this difference does not really exist, and that there is only one known oxide of bismuth, and that Klaproth's oxide was a hydrated oxide; for I have found that 100 grains of bismuth, converted by nitric acid into oxide, precisely in the same manner as the protoxide of antimony was more highly oxidated, gained only 11·1 grains. Klaproth did not heat his oxide to redness, and hence apparently the discordance. From the above result, which I have confirmed by repetition of the experiment, oxide of bismuth seems to contain 10 per cent of oxigen; and bismuth being as 67·5, the oxigen in the oxide is to the chlorine in the butter of bismuth, as 7·5 to 34·2.

and of bismuth.
(Oxide of bismuth.)

6. *On the Relation between the Proportion of Sulphur in the Sulphurets, and the Proportions of Chlorine in some of the Combinations of Chlorine and the Metals.*

The last section afforded proofs of the useful application of the general analogy of definite proportions in correcting the results of chemical analyses. In the present section, it is my intention to pursue a little farther the plan that I have adopted in the preceding, and to apply another test to the analyses of the combinations of the metals and chlorine, by comparing some of them with the combinations of the same metals and sulphur.

Proportions of sulphurets compared with those of the compounds of oximuriatic acid.

I was first led to examine the sulphurets of tin on a different account. Aurum musivum, it has been observed, is formed when stannane is heated with sulphur. According to Mr. Proust, this substance is a sulphuretted oxide of tin. Were this opinion correct, an argument might evidently be deduced from it in favour of the existence of oxigen in chlorine. To satisfy myself respecting this, I endeavoured to ascertain whether any sulphureous acid gas is produced by the decomposition of aurum musivum by heat, as it is commonly asserted. I heated to redness in a bent luted green glass tube, connected with a pneumatic mercurial apparatus, about 20 grains of aurum musivum, prepared by the decomposition of stannane

Examination of aurum musivum.

with

with sulphur, no more gas was produced than the expansion by heat occasioned, sulphur sublimed, and a gray sulphuret of tin remained. These results I have several times obtained, and not only with aurum musivum prepared as the preceding, but with some also made according to WOULFE's process. As no sulphureous acid gas was produced, and as sulphur sublimed, it may be concluded, that aurum musivum differs merely from the gray sulphuret in containing a larger quantity of sulphur. My next object was to ascertain the exact proportion of sulphur in both these sulphurets, for the sake of comparison with the combinations of tin and chlorine.

Component parts of the gray sulphuret of tin,

100 grains of tin in a finely-divided state, as precipitated from the muriate of this metal by zinc, were heated in a glass tube intimately mixed with sulphur, the combination of the two was accompanied with vivid ignition, the sulphuret formed weighed 127·3 grains, and, broken, it appeared perfectly homogeneous; it was pounded, and again heated with sulphur; but the excess of sulphur being expelled, the fused sulphuret had not increased in weight. The second time I made this experiment, I obtained the same result.

and of aurum musivum.

50 grains of aurum musivum, purified from mixed sulphur by exposure in a close vessel to a dull red heat, were decomposed by a bright red heat in a small green glass tube nicely weighed, and having only a very small orifice; the loss of sulphur, by conversion into the gray sulphuret, was equal to 9·3 grains. Hence, as 40·7 grains of gray sulphuret contain 8·72 grains of sulphur, 50 grains of aurum musivum appear to contain 18·02 grains.

Ratio in which sulphur combines with bodies, compared with oxigen, and oximuriatic acid.

The ratio in which sulphur combines with bodies is to that in which oxigen and in which chlorine combine, as 15 to 7·5 and 33·6. This appears from the proportions of the constituent parts of sulphuretted hidrogen and sulphureous acid gas; for I have found 100 cubic inches of the former to weigh 36·64 grains, and 100 of the latter 68·44 grains. In the comparison, therefore, between the sulphurets of tin and the combinations of this metal and chlorine, 15 by weight of sulphur are equivalent to 33·6 of chlorine. And the tin being as 55, it appears from the analysis of the gray sulphuret and stannane, that the sulphur is to the chlorine as 15 exactly to 33·4; and from

from the analysis of the other two compounds, aurum musivum, and the liquor of Libavius, as 15·5 to 33·5, or as 31 to 67.

Sulphurets of iron,

The proportions of sulphur in the two sulphurets of iron do not accord with the proportions of oxigen in the oxides, or of chlorine in the chlorine combinations; but I am yet ignorant of the cause of this difference.

of lead,

100 grains of lead, heated with sulphur in a glass tube, afforded, in two trials, 115·5 grains of fused sulphuret. Hence, lead being as 97·2, the sulphur is to the chlorine in the respective combinations as 15·09 to 33·8.

of antimony,

Sulphuret of antimony contains 25·9 per cent of sulphur. Hence, antimony being as 42·5, the sulphur in the sulphuret is to the chlorine in the butter of antimony, as 14·86 to 34·6.

and of bismuth.

100 grains of bismuth heated with sulphur afforded 122·3 grains of sulphuret. Hence, bismuth being as 67·5, the sulphur is to the chlorine as 15·08 to 34·2.

In the following table, the proportions are collected in which chlorine, sulphur, and oxigen combine with several metals; the numbers representing the metals are kept constantly the same, for the greater facility of comparison.

Compounds of metals with oximuriatic gas, oxigen, and sulphur.

Copper	60	+ 32·77 chlorine	= cuprane.
		+ 67·20 ditto	= cupranea.
		+ 7·79 oxigen	= orange oxide.
		+ 15·00 ditto	= brown oxide.
Tin	55	+ 33·40 chlorine	= stannane.
		+ 67·00 ditto	= stannanea.
		+ 15·00 sulphur	= gray sulphuret.
		+ 31·00 ditto	= aurum musivum.
		+ 7·50 oxigen	= protoxide.
		+ 15·20 ditto	= peroxide.
Iron	29·5	+ 33·60 chlorine	= ferrane.
		+ 55·50 ditto	= ferranea.
		+ 8·00 oxigen	= black oxide.
		+ 13·20 ditto	= red oxide.
Manganese	28·4	+ 33·60 chlorine.	
Lead	97·2	+ 33·80 chlorine	= plumbane.
		+ 15·09 sulphur	= sulphuret.
		+ 7·50 oxigen	= yellow oxide.
Zinc	34·5	+ 34·40 chlorine	= zincane.
		+ 7·50 oxigen	= oxide.

Arsenic 21·9 + 33·60 chlorine = arsenicane.
+ 7·30 oxigen = white oxide.
Antimony 42·5 + 34·60 chlorine = antimonane.
+ 14·86 sulphur = sulphuret.
+ 7·50 oxigen = protoxide.
Bismuth 67·5 + 34·20 chlorine = bismuthane.
+ 15·08 sulphur = sulphuret.
+ 7·50 oxigen = oxide.

7. *On the Action of muriatic Acid on some Combinations of Chlorine and Metals.*

Action of muriatic acid on some compounds of oximuriatic acid and metals.

Sir HUMPHRY DAVY has pointed out in a great variety of instances the existence of an analogy between chlorine and oxigen. He has shown, that the former, united with certain inflammables, constitutes, like the latter, acid compounds; and combined with metals, as it has already been observed, substances similar in many respects to metallic oxides.

I have kept this analogy in view in my inquiries; and, directed by it in my experiments, I have obtained some results which appear to me to coincide with it.

Thus having been led to try the action of muriatic acid on different combinations of the metals and chlorine, I have found many of them capable of uniting with this acid, and of forming compounds not dissimilar to some of those consisting of acids and metallic oxides.

Corrosive sublimate, stannane, cuprane, and the combinations of chlorine with antimony, zinc, lead, and silver are all soluble in different degrees in muriatic acid.

Corrosive sublimate.

Corrosive sublimate, which is but sparingly soluble in water, and still more sparingly in the sulphuric and nitric acids, is, I have ascertained, very readily soluble in muriatic acid. 1 cubic inch of the common strong acid takes up about 150 grains of this substance, and when gently heated, a quantity far more considerable, about 1000 grains. The compound thus formed solidifies on cooling into a crystalline fibrous mass, of a pearly and brilliant lustre. It is decomposed by heat, the acid being first expelled; and when exposed to the atmosphere, it effloresces, and appears to lose its acid; for, afterward analysed, it is found to be pure corrosive sublimate.

Compounds of silver, of copper, and of lead.

When I first tried the action of muriatic acid on the different combinations of chlorine already mentioned, I was not aware, that KLAPROTH had before observed the solubility of horn silver in this acid, and Mr. CHENEVIX that of cuprane. Horn silver, cuprane, and horn lead, are precipitated from muriatic acid unaltered by water. Both the hot saturated solutions of the two last compounds deposit crystals on cooling; those from the solution of the former are of an olive green colour, and of a prismatic form, and consist of cuprane and muriatic acid; those from the latter are small white brilliant plates.

None of the compounds absorb muriatic acid gas.

Finding the combinations of the metals and chlorine so generally soluble in liquid muriatic acid, I expected, that some of them might absorb muriatic acid gas; but none that I have tried have possessed this property, not even the liquor of Libavius. Indeed this is not singular, for water is necessary to the composition of many saline bodies; neutral carbonate of ammonia and nitrate of ammonia, for instance, cannot be formed without the presence of water. Neither is the precipitation of cuprane, horn silver, and horn lead from muriatic acid by water extraordinary; there are several salts containing metallic oxides which are liable to the same change, the oxides having less affinity for the acid, than water has.

Precipitation by water not extraordinary.

The action of muriatic acid may explain many phenomena.

The action of muriatic acid on the combinations of the different metals and chlorine will, I have little doubt, afford, when more minutely investigated, explanations of many phenomena, which are not yet well accounted for. Before I conclude, I shall mention only one instance, to which it already appears to be applicable. Mr. PROUST has observed the decomposition of calomel by boiling muriatic acid, and its conversion into corrosive sublimate and running mercury. Now calomel being insoluble in muriatic acid, these changes evidently appear to be owing to the strong attraction of the acid for corrosive sublimate, which has been already shown to exist.

Decomposition of calomel by it.

VIII.

VIII.

On the Coral Fishery in the Sicilian Seas: *by* ALFIO FERRARA, *M. D.*

> *Mylarum at pontus, Drepanique, et stricta Pelori*
> *Claustra ferunt avidis ramosa corallia nautis.*
> *Flaccomius in Sicelid.*

Communicated by the Author.

Coral a subject of natural history worthy notice.

HAVING for a long time employed myself in the study of the various natural productions, with which the sea that bathes the Sicilian shores abounds, the coral was the first object to attract my notice. This beautiful and elegant ornament of the sea could not fail of deserving first to come under my examination. I have been frequently present at the fishing of it, near the coast of Sicily: I have contemplated it in the very bottom of the *sea*, on its native spot: I have gathered it from stones, and shells, and other marine substances, recently taken out of the sea: I have had it worked in my presence: I have analysed the several varieties of it: in fine, I have extended my researches to whatever would give me the least insight into the nature of this substance, comparing the results of my own observations with every thing the ancients and moderns have written on the subject, and consulting in every point the treasures of natural history, with which the present day has been so abundantly enriched by the accurate experiments and luminous theories of the many great men of the last century.

The author's study of it.

Object of the present paper.

I have endeavoured in the present memoir to establish a clear and precise notion of the origin, increase, and nature of coral. This work has been the more pleasing to me, as I flatter myself I have been able not only to confirm by my own observations what has been already written on the subject by former Philosophers and Naturalists, but to add some new facts, that may tend to elucidate the history of this marine production, which has at all times as much occupied the researches of naturalists, as it has engaged the admiration of the fair sex, with whom the beauty of its colour, and brilliancy of its texture, have rendered it a favourite ornament of dress.

Supposed to be a plant by the ancients,

The ancients, attending only to its external form, conceived coral to be a plant; to which from its ramifications it bears some resemblance, and named it lithodendron, or stony plant, on account of its hardness. It was so called by Dioscorides and Pliny. These authors and their contemporaries did not attempt to contradict by the most trifling examination, what the poet Ovid (his head full of transformations) had asserted : that under the water it was a soft plant, but, immediately on being taken from the sea, became hard. This opinion prevailed for a long time, and was encouraged in later times by many great naturalists. Of this number was the celebrated Cesalpino.

and many moderns.

Our Baccone, who took much pains to investigate the nature of coral, could not divest himself of this idea ; but, gifted as he was with great sagacity and penetration, not being convinced, either from his own observations or those of others, that coral was a mere plant, and still less that it was a stone, he imagined, that the milky juice, which drops from the pores of fresh coral, was its seed ; which, being dispersed in the sea, is precipitated and gradually accumulated in a regular form in the capsules nature provides for it*.

Baccone's opinion of it.

This opinion, tending to alienate naturalists from the belief of the vegetable nature of coral, was entirely removed by the publication of the valuable and erudite work of the celebrated conte Marsilli, entitled Storia del Mare ; who, led away by his imagination, or rather deriving little aid from the state of natural philosophy at that time, suggested the idea, that the movable substances at the extremity of the branches were the octopetalous flowers of the coral, and thus revived the old opinion.

Count Marsilli's.

Tournefort, who, in the pursuit of his favourite study of botany, had remarked the vegetation of stones in the grotto of Antiparos, eagerly adopted this idea ; and was followed by Ray, Boerhaave, Klein, and many others of that time.

Embraced by Tournefort and others.

No sooner had naturalists begun again to take up the observations of Baccone, than they discovered in the hard substance of coral a sort of earthy concretion : but this not being sufficient to induce them to expunge it from the list of vegetable substances, they considered it as a marine plant encrusted with calcareous earth deposited by the sea. Lehman was of this opinion, to which the mineralogist Baumer was also much inclined.

Supposed to be a plant encrusted by calcareous earth.

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* See Rechescher sur le Corail, and Museo di Fisica.

Ferrante Imperato first supposed it to be the habitation of worms.

This opinion generally adopted.

Our Ferrante Imperato, in his work on natural history (which, like many other works of the ancients, has been almost buried in oblivion, though well deserving our attention from its containing the principles of many important truths, which have since been brought to light), had already supposed, that some of the species of coral were merely the habitation of marine worms. This opinion had so much of probability, that it has always been entertained by naturalists since; and the discovery of the polypi assists to explain on solid principles the true nature and origin of coral: and on this account the works of Peyssonnel, Jussieu, Guetard, Trembley, Reaumur, Donati, Ellis, Pallas, Cavolini, Spallanzani, and many others on coral, became so interesting. Coral is found round nearly all the Mediterranean islands. Pliny and Dioscorides speak much in praise of that found in the Sicilian seas* in their time. It is fished for at present on every part of the shores of Sicily.

Places where the coral is collected.

Requires eight years for its reproduction.

The Messineze collect a great quantity in those straits, even as far as Melazzo; but the Trapanese, who are chiefly employed in working the coral, not only fish it in the neighbouring seas about the Eolian and other islands, but extend their search to all the Southern shores as far as Cape Passaro, and beyond Siracuse, and even to the coast of Barbary. They are obliged to occupy so large an extent of sea; as they cannot fish again on the same spot for several years, the re-production of coral requiring a great length of time, even nearly eight years. I have myself collected it on the shores of Catania, and thence as far as Taormina.

Instrument used to get it.

The instrument with which the coral is detached from the bottom of the sea has been known a long time. It is composed of a large wooden cross, having fastened to each of its four extremities nets sufficiently capacious to enclose the coral, which is broken from its root by a large stone hanging from the centre of the cross. The instrument is let down by two ropes from the boats employed in this fishery into the sea, and after remaining

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* See Dioscorides, lib. 5; Plin. lib. 32. Pliny says, *Laudatissimum in Gallico sinu circa Stoechadas insulas, et in Siculo circa Heliam, ac Drepanum.* Some commentators, not finding the name of Helia, have called it Aeolias; but the true name is Helia, for the island opposite Trapani was anciently so called. Pliny himself names it in his 3d *book Hieronesus.*

remaining a sufficient time it is drawn up by a windlass. The Trapanese claim the invention of this machine.

Coral adheres to some hard substance.

From my own observations, and from the most accurate information I have been able to obtain from the people employed in this fishery, I am persuaded, that the coral grows indiscriminately on all hard substances, as rocks, shells, &c.—I have seen it attached to an earthen vessel, which had at some time fallen into the sea, and was taken out in my presence. The usual appearance of coral is that of a tree without leaves. It never grows to a greater height than twelve inches, and is seldom an inch thick. The direction of its branches extends always forwards from the spot to which the root is attached; therefore when it grows on the top of a cavern they spread downwards; if from a horizontal surface upwards: most commonly however the branches extend downwards, which enables the nets to enclose it with greater facility when detached by the stone.

Its figure and size.

Branches broken off fix on some hard substance.

They are frequently perforated by worms.

It has been constantly remarked, that the broken branches of coral attach themselves to some hard substances where they continue their growth. It is very common to find many branches of coral, when taken out of the sea, perforated in several parts. There can be no doubt, that this is the work of the lithophagi; worms which attack even the hardest substances, for it is well known that they pierce and destroy the hardest carbonate of lime. The coral (isis nobilis, Linnei) which is most eagerly sought after, is of a fine red colour. Artists and ladies give it the preference. It improves the charms of a beautiful face. Naturalists describe all the varieties; two original colours in coral may be established, white and red, as the two extremes, the gradations of shade from the one to the other producing infinite varieties, among which five principal may be distinguished.

Red most esteemed.

Varieties.

1st. The deep red coral resembling in colour minium. This is considered as the most perfect sort; in fact, it is the largest and most dense, and receives the highest polish. It is commonly called the male coral.

2d. Red coral. This is more or less clear, but always less brilliant than the first variety.

3d. Flesh coloured coral. The ancients call it light red.

4th.

Varieties.

4th. Dull white coral; by some it is called fawn coloured, from its resemblance to the colour of the fawn.

5th Clear white coral. All these varieties are found in the seas round the island, sometimes on the same spot. The first and second are not so abundant or common as the others.

Appearance of coral when fished up.

The extremities of coral, when extracted from the sea, are swelled and rounded, resembling juniper berries. Probably these were the berries remarked by Pliny, which he considered as the fruit of the coral; although in his work he asserts that they are white and soft under water, and become hard and red out of it. I am inclined to believe, either, that he wrote from the reports of others, or that he has mistaken for them the red globules formed by the artist. These extremities when pressed, give out a white unctuous fluid resembling milk, which has a sour taste. It was formerly thought to be the seed and nutritious juice of the coral plant.

Fluid expressed from it.

Coral hard in the sea, and red

The centre hardest.

Cortical part.

The largest pieces have an appearance of joints.

Its component parts.

The colour apparently owing to iron.

Coral formed by polypi.

The substance of coral is hard as well in the sea, as when out. The red kind is red from the first, and it is a singular circumstance, that the ancients should have entertained these two erroneous opinions, which the most simple examination would have falsified.—The central part or axis of the coral is hard, of a firm solid texture, even, and lamellated; and hence capable of taking the finest polish. This is enclosed by a paler coloured bark of a granulated texture, interspersed with holes in the form of stars with eight rays. In the coral of the largest size sometimes is found a kind of joint or union between the different pieces of which it is composed, these having the appearance of tubes of some length, lying one above the other. In the analysis of coral we obtain a small quantity of gelatinous animal matter, a large proportion of carbonate of lime, and a little iron. The different colours of this beautiful marine production seem to depend on the different degrees of oxidation of the iron, and various proportions of it in union with the animal matter. The discovery of polypi gave the clearest idea of the origin and growth of coral. These animals, the last in the scale of animated nature, form for themselves small nests sufficiently solid to shelter and protect them. These soft and delicate animals, surrounded by an element in a constant state of agitation, and exposed to the attacks of their numerous enemies, were instructed by nature to form for themselves a covering capable of resisting the percussion of the sea, and affording them a retreat in the moment of danger.

The polypi described.

These coralligenous polypi are only a few lines in length, their bodies elongate and ramify into eight delicate threadlike branches around the mouth. These are the arms and legs of the animal, which it can extend and spread out at will to a considerable distance in search of its food. They are analogous to the horns of the snail. The curious manner of propagation of polypi, so different from that of other larger and more perfect animals, is well known; on examining minutely the gelatinous bodies of these polypi, a great number of grains, or little buds, are discernible, covering the surface; these elongate themselves, increase in thickness, diverge and spread in all directions, and become young polypi. Scarcely are these developed before a new series of sprouts appears from their small bodies by the increase and growth of the small buds on their surface. By this rapid succession the family is propagated in every direction, forming as it were a genealogical tree of existing generations. It is well known how from the soft nature of their bodies these animals are enabled to unite and engraft with each other in the same manner as plants; and one branch of these animalculæ so engrafted lives and regenerates another. Even one single animal may detach itself from the family tree, and establish on another spot a new family with its various branches. While large animals have bones for the support of the softer parts, and shell fish are protected by their shells, the coralligenous polypi make use of a certain proportion of earth to incorporate with and give firmness to their form.

Growth of the polypi.

Immediately as a polypus has fixed itself on a hard body, it begins to lay the foundation of its future generation. If you only take some stones from the bottom of the sea round Sicily, you will find on them small branches of red coral, and round red spots, which are the first depositions of the coralligenous polypi. In the same way as the bones of the larger animals are formed by the gradual deposition of the earthy particles separated from their food by vessels adapted to this purpose, so is the covering of these polypi formed by the carbonate of lime mixing and encrustating with the gelatinous matter, which is so abundantly secreted by their delicate bodies, and gradually incases them except the mouth. If a branch of coral newly gathered is immersed in a vessel full of sea water, these animals are perceived issuing from the stellated holes, their mouths gradually appearing first, and then their silklike arms extend, in this manner putting on the appearance of octopetalous flowers, by which the ingenious count Marsilli was deceived.

The multiplication of polypi, of which I have treated, explains admirably the arboraceous form of coral, as also the increase of the branches detached from the trunk. I have before me a fine specimen of the *antiphates*, the black coral of the ancients, in which the extremity of a branch has united with the principal trunk, and the polypi are seen bedded in it.

Black coral of the ancients.

Classification of coral.

From what we have seen, I think the term zoophyte inapplicable to coral; it is neither an animal plant, nor a plant animal: Nor can it be called a zoolite; as it is certainly not a stony animal. It is with more propriety a *polipaio*; which, on account of its form, and to distinguish it from the other analogous works of polypi, might be called *polipaio dendroide.* In using this nomenclature, we must be careful not to adopt the false idea, that the *polipaio* resembles a wasp's nest; the wasps may at will leave their nest, but the *polipaio* is a part of the animal, from which it cannot be detached. Thus the *polipaio dendroide* is an accumulation of ramified polypi, incorporated with the solid substance, in the same manner as the shell of some animals and the bones of others. The above erroneous opinion cannot be entertained by any one, who observes, that in coral the gelatinous membrane of the polypus is continued into the solid earthy part, the same as in bones. Herissant has already pointed out this mistake.

Its hardness supposed to be progressive,

but this not confirmed by experience.

It is to be inferred from the analogy of coral with bone, that, as it does not arrive at once at a state of maturity, but by degrees, its hardness must also be progressive. However probable this idea may be, it has not been confirmed by experience. I have particularly remarked the small quantity of iron obtained in the analysis of the red coral, I have always found it combined with the gelatinous animal substance in the state of oxide. Not to extend this paper too much, I shall omit the results of various experiments I have made; but they have led me to conclude, that the ferruginous substance is phosphate of iron, that is, the oxide of iron united with phosphoric

The colour of coral ascribed to phosphate of iron.

phoric acid, which it is well known gives the red colour to the blood of animals*. The phosphate of iron therefore, which in animals has the property of giving the lively red colour to the blood, and even the vermilion hue to the skin, serves to colour the solid part of coral, and give it the brilliant sanguineous tinge.

Attempt to account for the difference of texture and colours.

The first variety, as I have remarked, is esteemed the most perfect; it is more solid than the other kinds, of a finer and more compact texture, and hence takes a higher polish. In the other kinds, in proportion as the bright colour fades, these qualities gradually decrease, so that the white sort, which is the softest and lightest, is very unfit to be wrought, and takes but a trifling polish. The deficiency in the quantity of phosphate of iron diminishes the colour, and at the same time decreases the density of its texture; or perhaps the light texture by its porosity permits the water to wash away the colouring matter, and consequently that which would tend to bring it to perfect maturity.

The red parts always most solid.

To this may be attributed the peculiarities of some corals, in which the trunk is red, and the branches white; or the branches red within, and externally white; or the branches half white and half red, which is often seen in coralligenous productions; but the red part always proves of firmer texture than the others.

Coral as an ornament

in some respects superior to gems.

While naturalists have been employed in investigating the origin of coral, and the nature of its growth, each applying it to different purposes; the fair sex, occupied by the natural desire of pleasing, have been much indebted to the brilliant colour and fine lustre of this marine production. Coral formed into beads is worn as an ornament of the neck and arms; and there is no doubt, that the lively colour of coral gives additional grace to a fine face and beautiful complexion, which cannot be obtained by the use of the precious stones, so that these can only be considered as ornaments of luxury and show. The ladies who are always led away by fashion, because they consider it as depending on the existing taste of the other sex, laid aside this beautiful ornament, to load themselves with

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* This is at least highly questionable. See Journal, p. 31, of the present volume, and p. 48 of the preceding. C.

jewels brought from distant countries. Thus coral gave place to other ornaments, the rage of pleasing being only gratified by variety. Works of Amber have latterly obtained a very high estimation from the softness of its substance and its transparency*.

Amber lately become fashionable.

This substance, which for a time was in high repute, and which the discovery of the precious stones had almost thrown into oblivion, has of late, by the accustomed versatility of capricious fashion, recovered its former value, and has rivalled in price even the ornaments composed of jewels.

Coral superior.

When the value of female ornaments shall depend no longer on the price or scarcity, but on the effect they produce on the complexion, all will yield to the natural beauty of coral. Most certainly Galatea, emerging from the ocean, would select from the numerous offerings of the nymphs the lucid branches of the coral to adorn herself with, which would alone assimilate with the roundness of her lips, and with the vermilion of her cheeks.

Method of working coral.

The working of coral consists in removing the outer bark, and exposing the interior solid and highly coloured part, which takes a fine polish. The coarse part of the bark being removed by the file, it is rubbed with tripoli powder, and lastly, with a metallic earth, which gives the polish. Some bring it to the finest polish imaginable by the use of the oxide of tin.

Uses to which it has been applied.

The ancients ornamented their swords, bucklers, and helmets with coral; this custom is still in vogue in some part of Asia, where coral is as much esteemed as in the time of Pliny. The soothsayers and priests of that age attributed many mystic properties to it; hence they were in the habit of wearing coral, as well from religious motives, as from regard to its beauty. Paracelsus recommends it to be worn round the necks of infants, as an admirable preservative against fits†, sorcery, charms, and even against poison. Many other follies of that man are still prevalent, and of great credit with the common people; and it is very usual in the inland parts of Sicily, to see

Superstitions respecting it.

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* See Memor. sull Ambra di Sicilia, 8vo, Pal. 1801. Written by my brother, ab Francesco Ferrara, profes. in the univers. of Catania.

† The negroes in the West Indies say, that the colour of coral is affected by the state of health of the wearer, it becoming paler in disease.

children

children wearing amulets of coral round the neck for the above purpose. In the cities it is worn by many in the shape of a horn, as a protection against the influence of evil eyes. It was even believed, that coral would drive away devils and evil spirits, hence perhaps arose the custom of making crowns of it. Nor have the medicinal properties of coral been less exaggerated, as may be sufficiently seen in the writings of Pliny and Dioscorides. It certainly may be considered as an absorbent, it is used in dentifrice powder, in the Alkermes for indigestion, and in the Troches of Carabe.

First wrought by the Trapanese.

Engraved.

The Trapanese appear to have been the first who worked the coral, being induced thereto by the great quantity of it found in their seas. It is asserted, that Antonio Ciminello, a Trapanese, was the first who discovered the art of engraving coral*. In the time of king Alphonso the coral fishery was so assiduously, and so advantageously pursued by the Trapanese, that the ministers of that king proposed to subject the fishery to a tax†. In the last century, when it was again proposed, instead of a tax, which probably would have ruined this branch of industry, king Ferdinand instituted some very useful regulations in favour of it.

Beside forming necklaces and bracelets, the Trapanese have the art of engraving it in the same manner as they do amber and shells, and most certainly many of these works display great spirit, boldness, and grace in the execution, talents natural to the genius of the Sicilian nation.

IX.

On the medical Effects of the Bark of the Piscidia Erythryna of Linnæus, or Jamaica Dogwood. In a letter from WILLIAM HAMILTON, Esq.

To Mr. Nicholson.

Nevis, the 20th of July, 1812.

SIR,

Bark of the root of Jamaica Dogwood used for poisoning fish.

YOUR readers may perhaps have accidentally heard of the remarkable effects produced upon fish, by mixing a strong

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* Orlandini Descrizione di Trapani.

† Capit: è Costituz. del Regno. See also, Muta capit: 49 del Re Giacomo G. 1.

strong infusion of the bark of the roots of a tree, well known by the name of the Jamaica Dogwood, with the waters of the ocean. This process, well known to the planters and others here by the name of fish poisoning, has been too frequently described to render any particular account from me necessary.

Its effects tried by the author on himself.

However, the effects upon fish appeared to me so singular, that I was led to try how it would act upon the human subject; and as, from the very strong and general prejudice entertained against this plant, I was not likely to succeed in persuading others to submit to my trial of its effects upon them, I was necessarily reduced to make trial of it in my own person.

A tincture the best preparation.

To detail the various experiments which I have made, with the view of ascertaining whether this plant possessed any, and what medical virtues, would be superfluous here; and I shall only observe, that I have at length discovered the tincture to be the least exceptionable form of exhibition. This is prepared by macerating an ounce of the dried bark of the root, in fl℥vj of rectrified spirit and fl℥vj of water, or which will amount to the same thing, in fl℥xij of proof spirit, for three days, and straining; when a tincture will be obtained of a fine clear topaz colour, somewhat resembling that of fine old Madeira Wine; when this tincture drops on any substance, a white resinous film is found remaining after the spirit has evaporated, and a milky fluid is formed by the admixture of water; thereby showing, that a resinous substance is one of the active constituents of the bark. The smell and taste of this are not disagreeable; and I find, that from flʒij to flʒiv taken in a flask of water at bedtime, produce an immediate sensation of warmth in the stomach, quickly succeeded by an universal glow on the surface, together with a profuse diaphoresis, and followed by an agreeable, tranquil, and refreshing sleep; without occasioning any of those distressing sensations, which opiates so frequently produce.

Mode of preparing it.

One of its active principles resinous.

Its dose and effects.

Externally cures toothache.

In odontalgia from a carious tooth, where the nerve is exposed, a little of this tincture introduced into the cavity, produces instant and most commonly permanent relief. Upon the whole, I regard it as well deserving of farther research, as it promises to add a very valuable medicine to the class of anodynes.

Some of the

Mr. Carlisle, of Soho Square, has been furnished with some

of

of the dried bark of this plant, which is the piscidia erythryna of Linnæus, and grows most abundantly in some parts of this island. Before I conclude I cannot help remarking, that this is one of the deciduous plants, no leaves existing on it during the period of flowering, which is in the month of April.

bark sent to Mr. Carlisle.

The plant deciduous.

I have the honour to remain,
In extreme haste, your's truly,
WILLIAM HAMILTON.

This shrub is the icthyomatheia of Brown—see his history of Jamaica. Brown in the same place notices a shrub growing at Surinam, the leaves and smaller branches of which are employed for a similar purpose, with the bark of the root of the Dogwood. He calls it a Cytisus, which I strongly suspect to be erroneous. Some of your readers can perhaps favour me with the real name, habitat, and other particulars of this plant.

Mentioned by Brown.

Inquiry after another plant.

X.

A Correspondence between DR. BOSTOCK *and* DR. MARCET, *on the subject of the uncombined Alkali in the Animal fluids.*

To Mr. Nicholson.

SIR,

THE attention which I have for some time paid to the subject of animal chemistry, caused me to read with much interest the controversy, which was carried on through the medium of your journal, between Dr. Pearson and Dr. Marcet, respecting the nature of the uncombined alkali in the serum of the blood. I was induced to make a considerable number of experiments upon the subject, the result of which had led me to decide in favour of Dr. Pearson's opinion; but having communicated my doubts to Dr. Marcet, he repeated and extended his former experiments in such a manner as, I think, firmly to establish the fact, that the alkali is soda. The detail of these experiments, as contained in the following letter of Dr. Marcet to me, I have his consent to transmit to you for publi-

Controversy on the alkali in the animal fluids.

First supposed to be potash.

But since proved to be soda.

publication; and, I believe, you will agree with me in the opinion, that they must entirely set the question at rest.

I am, Sir,
Your obedient Servant,
J. BOSTOCK.

Knotshole Bank, near Liverpool,
Aug. 22d, 1812.

Dr. Marcet to Dr. Bostock.

" *London, August the 19th,* 1812.

" MY DEAR FRIEND,

Controversy on the alkali in the animal fluids.

" I feel much indebted to you for the remarks you have made, and the doubts you have expressed, in some of your last letters to me, respecting the nature of the uncombined alkali in the incinerated salts of serum; they have induced me to reconsider the question, and to add to my former inquiry on that head, a few new results, which, I flatter myself, will remove every shadow of doubt, which may remain on your mind in that respect.

Why no new data brought forward in the last letter.

" In my reply to Dr. Pearson, in March last, I abstained purposely from bringing forward any new data, because the chief object of that letter was to vindicate former statements and inferences; and to show, that there had not been, as was argued by my opponent, any blunder in the mode of reasoning by which I arrived at my conclusions. Indeed, it appeared to me hardly necessary to push the inquiry any farther; and I must own that, from the manner in which Dr. Pearson had thought proper to carry on the controversy, in his two letters on the subject*, I should have felt great reluctance to resume the discussion, had it not been for your interference.

Source of Dr. Bostock's doubts on the subject.

" Your objection, or rather your scepticism, arose from your having found in a mass of salts from serum, (by the successive agency of acetic acid, alcohol, and tartaric acid,) such quantities of potash, as appeared to you to show, that the uncombined alkali was potash, and not soda; and you were farther confirmed in this belief by observing, that the alka-

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* See this Journal for February and May last.

line residue obtained by heating the acetat to redness, was deliquescent. You will see, however, by the following statements, that you were mistaken in your inference; and you will, I make no doubt, admit, that the potash, which you found in the alcoholic solution, must have been in the state of muriat; and that the deliquescent quality of the alkaline residue must have arisen from your acetat having been but imperfectly decomposed, on account of the too low degree of ignition to which you had exposed it, and perhaps also (as you have since yourself observed) in consequence of the presence of muriatic salts. But your experiments appear to show, that the proportion which the muriat of potash in the blood bears to the muriat of soda, is greater than I had at first imagined; and that we had both underrated the power of alcohol to dissolve muriat of potash.

Large proportion of muriate of potash in the blood,

" As to the point at issue, however, namely, the nature of the uncombined alkali, in the incinerated salts of blood, the experiments upon which I think myself warranted to repeat, with increased confidence, my former opinion, that the alkali is soda, and not potash, were conducted in the following manner.

The uncombined alkali is soda.

" After evaporating some human serum to siccity, incinerating the residue, dissolving in water the soluble saline substances contained in the incinerated mass, filtering this solution, and evaporating it again, the alkaline mass of salts thus obtained was treated with acetic acid, and afterwards digested with 5 or 6 times its weight of alcohol of the specific gravity of 0·815. The highly deliquescent residue, deposited by the evaporation of the filtered alcoholic solution, was then made red hot in a platina crucible, and kept for a few minutes in a state of igneous fusion. A carbonaceous alkaline mass remained in the crucible, which, after being exposed to the air for 48 hours, in a room without fire, and in damp though warm weather, did not exhibit the least vestige of deliquescence. This mass, the quantity of which amounted to 4 or 5 grains, being dissolved in a little water, was divided into four portions; *a*, *b*, *c*, *d*.

Experiments proving this.

" The portion *a*, being examined by re-agents, exhibited the following properties.

" 1. It contained abundance of muriatic acid.

" 2. When suffered to evaporate spontaneously in a glass capsule, it left, at the end of 12 hours, a dry efflorescent crystalline substance, which consisted principally of feathery crystals, amongst which were discerned groups of rectangular plates, and a few minute cubes.

" 3. The presence of potash in this crystalline mass was made obvious, both by the tartaric acid, and by oxymuriat of platina, though not so much so by the latter of these tests.

" The portion *b*, was saturated with sulphuric acid, and submitted to spontaneous evaporation. The result was a rim of confused crystals, surrounding a group of regular efflorescent prisms of glauber, being (at least some of them) terminated by distinct dihedral summits, and having sufficient magnitude to be identified by the naked eye, even at the distance of a few yards; they were made to crystallize over and over again, always with the same result; but in some of these crystallizations, a few crystals of sulphat of potash also appeared, the form of which was not equivocal.

The portion *c*, being treated with nitric acid, yielded by evaporation great numbers of rhomboidal crystals, perfectly distinct to the naked eye, and amongst which no form, at all resembling that of nitre, could be detected.

" The portion *d*, being treated with oxymuriat of platina, the usual crystalline appearance of potash-muriat of platina took place immediately; but by slow spontaneous evaporation, other and more abundant needle-shaped crystals of soda-muriat of platina made their appearance.

" My conclusion therefore, (which I hope will now also be your's) is precisely as before; namely, that the potash which exists in the animal fluids, is in the state of muriat, and that the whole of the uncombined alkali is soda; and as it is a known fact, that muriat of potash is in some degree soluble in alcohol, the circumstance which led you into error is readily explained.

" I have only farther to add, that the fact, which I have endeavoured to establish by a specific inquiry, ought to have been inferred from principle; for it is well known that potash has a stronger attraction for the muriatic acid than soda; and indeed I understand that it is a common process, in some manufactories

factories, to obtain soda by the action of potash-lye on muriat of soda.

"Believe me, ever, &c. &c.
"ALEXANDER MARCET.

"P. S. Since the above was written, I have, in consequence of your suggestion that the blood of Graminivorous animals might perhaps yield potash instead of soda, on account of their living, exclusively upon vegetable food, examined bullock's blood, with a view to ascertain this circumstance; and as there was no difficulty in procuring any quantity of that blood, I had some gallons evaporated, from which I procured some ounces of salts, in order to satisfy those who think that nothing certain can be inferred from experiments upon a small scale. However, the results were precisely similar, except that the crystals of sulphat and nitrat of soda, obtained by the processes above detailed, were of much larger dimensions than in any of my former experiments."

Soda from Bullock's blood in large quantity.

XI.

On the Culture and Preparations of Hemp in Dorsetshire, and on the Growth of Sea Cale: by H. B. WAY, Esq.*

DEAR SIR,

AS you informed me, when you were lately in Dorsetshire, that the Society of Arts, &c. were anxious to obtain information concerning the culture and preparation of hemp in this neighbourhood, I am induced to send you some account thereof.

Information respecting hemp desirable.

I fear my memorandums on the subject will not be worthy the notice of the society, and I should scarcely have ventured to have put pen to paper upon it, if I had not uniformly found that the persons who are concerned in the growth and management of that article are shy of giving information. If what I have sent should induce persons equal to the task, to make the needful inquiries in this county, Somerset, Suffolk,

In what parts of England

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* Trans. of the Soc. of Arts, vol. XXXI, p. 63.

chiefly cultivated.

and Norfolk, (which I believe to be the parts of England where hemp is most cultivated,) and make the culture more generally known that it now seems to be, I shall be much gratified I hope, if you again visit this neighbourhood, to show you a very fine crop of wheat on the field where you last year saw the persons employed in collecting the male hemp; also another large field of exceeding good wheat, that produced hemp last year, neither of which have had any fresh manure upon them, since the hemp was taken from the fields. I have added some observations on the growth of Sea Cale: this useful vegetable, growing naturally on some of the cliffs near Bridport Harbour, and being one of the most valuable esculent plants that I know, I have found the culture of it in the kitchen garden more easy to manage than has been generally supposed.

Wheat after it without manure.

Sea cale valuabler and not difficult to cultivate.

I have sent different specimens of the seed, and some of the natural soil, for inspection:

And remain, Dear Sir,
Your friend and obdient Servant,
H. B. WAY.

Bridport Harbour, March 1st, 1811.

Account of the Culture and Preparation of Hemp in Dorsetshire.

Preparation of ground for hemp.

Hemp is usually sown about the 15th of May, on the best arable land, on which about twenty cart-load of good rotten dung has been spread, say about a ton to the load. This is well ploughed in, and the ground well ploughed two or three times, and well dragged and harrowed, to get the soil as fine as possible, and about two bushels of seed, or two and a half, sown to the acre. What produces no seed, called by some male or summer hemp, and by others cinner hemp, is drawn about five or six weeks after the plant comes up. It is at that time in blossom When drawn, it is tied up in bundles, and carried to some meadow land, and there spread to ripen: when ripe and dry, it is bundled and stacked. What stands for seed has no flower that can be discovered; it is the female hemp, and is generally ripe early in September; when it is drawn, bundled up,

Quantity of seed.

Drawing the male hemp.

The female.

up, and stowed up in the field, for the seed to dry and harden, when it is thrashed out in the fields. Most commonly in Dorset the seed is sold on the spot, at from 2s. 6d. to 7s. per bushel; an acre of hemp produces eighteen or twenty bushels. In Somerset they have sometimes thirty bushels of seed to the acre. In the sowing season I have known 21s. per bushel paid for seed. When thrashed the hemp is carried to the meadows, and spread to ripen as the other, and stacked in the same way, to prepare it for sale; it is sent to the houses of the poor in the parishes round which it is raised, to be what is called scaled; that is, each separate stalk of hemp is broken in the hand, and the hemp, which is the outside rind or bark, is stripped off; in which state it is sent to market. The scaling is the employment of old men, women, and children, and of the whole of the labouring family in the evening, as in winter they make but poor wages of it; and one principal inducement for them to do it is, that the woody parts of the hemp make them a fire, but it soon burns out. Complaints are made of a great deal of the hemp being often wasted from improper management, and want of care in the scaling of it. At the Comptons and Bradford, a good deal more hemp would be raised if they could get it scaled, which they find much difficulty in doing; and if it were possible to construct a mill that would swingle it at a moderate expense, on some such plan as the flax swingling mills, and to afford some encouragement to the erecting them, as well as flax swingling mills, it would encourage the growth of both articles materially. An acre of hemp in a good season will produce 14, 16, or 18 weights, of 32lb. to the weight, in Dorsetshire; in Somersetshire they reckon their weight two pounds less, and they sometimes get as much as 35 weights to the acre. The price of the weight of hemp is from 16s. to 20s. per weight. The rotation of crops as follow:

Seed.

The hemp thrashed and scaled.

Complaints respecting this.

A mill for the purpose desirable.

Produce per acre.

Rotation of crops.

On ground well manured,	Hemp.
	Wheat.
	Barley or Oats.
	Clover with the above.
	Wheat.

Barley or Oats.
Ground well manured, Hemp.

But sometimes they dress the ground well for hemp every third year. The quantity of hemp sown in Dorset, is very trifling in comparison to what is sown in Somerset. In the former it is chiefly confined to eight or nine parishes; whereas very large quantities are raised in Somerset, in the parishes of Misterton, Crewkerne, Hinton St. George, Lopen, Seavingtons, Ilminster, Stocklinch, Donyatt, Kingstone, Shipton, Beauchamp, Barington, South Petherton, Martock, Norton, Chiselborough, Stoke-under-Ham, Montacute, Odcombe, the Chinniocks, the Cokers, the Comptons, Bradford, and a great many other parishes. Mr. Emanuel Pester, of Preston, near Yeovil, is in the middle of the hemp and flax county; and he can doubtless obtain and give every information that may be wished on the subject, being so extensively engaged in agricultural pursuits himself, and so competent to give that sort of information wanted. A bounty of 3*d.* per stone on hemp, and 4*d.* per stone on flax, was for many years given by government, but is now discontinued; it was paid by the clerk of the peace for the counties, and as the late Mr. Wallace managed that for the county of Dorset uncommonly well, it is most probable, that a very correct return for the county of Dorset could be obtained from the office of the clerk of the peace for this county, of the quantity raised each year of both articles, during the continuance of the bounty; also from Devon and Somerset similar returns could be got. There are large quantities of hemp raised in Suffolk, the writer thinks, near St. Edmund's-Bury and Stow-market, in that county. He has been told they make linen so fine of hemp, as to be worth 5*s.* and 6*s.* per yard, and used for shirts in preference to Irish, being considered much more durable and better, so much so, as to induce the Irish to imitate the fabric, and stamp the cloth, Suffolk hemp. It is also raised in Norfolk, in the neighbourhood of Lynn and Wisbeach, but it must be watered and prepared in some other way; indeed he is convinced that all the hemp imported from the Baltic is prepared differently from the mode used in Dorset and Somerset, and must have been swingled before it was sent to the different ports it was shipped at for this country. The giving the former bounty on the growth, and

Hemp and flax much cultivated in Somerset.

Bounty formerly given on them.

Very fine linen from Suffolk hemp.

Bounty should

and increasing it on hemp and flax, would encourage the growth; but if given on the number of acres sown, the grower, as his ground would be in high order for a crop of turnips and wheat after, might be careless about his crop of hemp, as the bounty, to be worth notice, must be worth more than the value of the seed in common years and the labour of sowing.

be on the produce, not the measure of the land.

Hemp in this county and the next is never sown in new ground fresh broke up, but flax always by choice, when fresh ground can be got. Mr. John Pitfield is going to break up great part of the West Clift at Bridport Harbour, and sow it with flax this season. The writer, while on the subject of hemp, is led to mention, that when travelling in the year 1792, in the province of Massachusets, near Boston, in North America, he was assured that considerable quantities of hemp were raised in the township of Sunberry, about ten miles from Boston; and that it was always raised on the same ground every year, no other crop being sown in their hemp lands, and that it was manured every year, at the rate of about ten tons of manure to the acre of hemp. Respecting seed, he cannot learn that there is any for sale at Bridport, with the buyers who purchase it up for the growers at the hemp harvest, and he expects that very little can be got from the growers round here. Somersetshire is a more likely place to get it, as he has known some of the hemp farmers to have upwards of a hundred acres of hemp in one season. Round this they generally are only in a small way. A change of hemp-seed is much wanted in Somerset and Dorset. Trials have been made two or three times to get it from Russia, but it is not possible to get new seed from the interior early enough in the fall at the shipping ports, and some old seed which has been shipped has not answered the purpose; if new could have been got, it would as generally have been used for a change, as the new Riga barrel flax seed is by the flax-growers. As the seed sown in Russia was considered a good sample, and its appearance much liked, possibly it might, at a future period, be obtained in the fall from Odessa, or some other port on the Black Sea; as it is understood that a good deal of hemp shipped at Riga and St. Petersburgh grows much nearer to the Black Sea than the Baltic; or possibly the seed of the Italian hemp raised in the neighbourhood of Bologna, or that of America, might be obtained

Flax sown on new ground, hemp not.

Hemp sown constantly on the same land.

Change of seed wanted.

Suggestions for obtaining it.

A previous crop of vetches.

Hemp alternated with turnips.

Manure for hemp.

obtained in time to answer. Perhaps tares, called by some vetches, might be cleared from the ground early enough for manuring and sowing the ensuing crop of hemp, and vetches might make it worth the farmer's attention; to this an objection was stated, which I do not just now remember. On talking with the gentleman before-mentioned, and stating the American practice, with what had passed on it with my neighbours, he said, he had long been persuaded, that it was a good practice; and that he had the last season a very good crop of hemp on a piece of ground that had hemp the year before, and that he did not let the hemp stand for seed, but had it all down at the usual time for drawing the summer or male hemp, and the ground immediately sown with turnips, which were fed off with sheep, and the ground then slightly manured, and hemp sown again at the proper season; and that he had then, October 27, 1808, a piece of turnips after his hemp, which were worth 6*l.* per acre. It is to be observed, that the acre here meant is the British acre of one hundred square poles, three hundred and four square yards each. The manure mostly used for hemp is good rotten stable dung, which is much preferred to any other, though lime is frequently used; but manufacturers pretend to assert, (with what foundation I cannot say), that they can distinguish a material difference in the quality of the hemp, where lime has been used instead of dung; as from lime they say hemp is more harsh and brittle, and not of such a soft silky quality as where dung has been used. The writer has endeavoured to throw together every thing that occurs to him on the subject of the culture of hemp, which, from being born and residing great part of his life in a part of the county where it has been extensively cultivated for ages, he has been able to collect; but where it is not very easy to obtain direct information, as both the growers and manufacturers are very shy of giving any, under an idea that it might injure their own interest by assisting to extend the culture to other countries. He believes that his statement may be depended upon; but he is no farmer, and therefore the loose hints thrown together here on the subject may not be so clearly and satisfactorily explained as he could wish; but if they in the smallest degree assist in encouraging the growth of an article so essential to the welfare and prosperity of the kingdom, it will afford him the most heartfelt pleasure.

H. B. WAY.

Account of the Culture of Sea-Cale, or Sea-Kale.

Cultivation of sea-cale.

The mode which I consider the best for the culture of sea-cale is to draw lines in a very dry soil and dry situation, on ground with a southern aspect, about two feet one way by about eighteen inches the other, and where the lines cross, to put in three or four good perfect seeds in a square or triangle, about three inches apart. This may be done any time in November or December in open weather; and it will require no other care afterwards but keeping the ground clear from weeds till the autumn of the following year, when all the plants but one of the finest in each square may be taken up, which if wanted will serve to form other beds set the same distance apart. The ground in the intervals of the plants should be dug in the spring and fall of the year, taking care not to injure the plants. The leaves should be left on the plants till they fall off naturally, which will not in general be sooner than the latter end of November. In the autumn of the second year, the same attention should be paid to the plants, and to remove the dead leaves.

Blanching.

In the third year, about the middle or latter end of November, when the leaves had been cleared away, and the ground dug, each plant should be covered over close with a tub, pan, a heap of small stones, coarse cinders, or coarse bark, raised about ten or twelve inches over the crown of each plant, and from about the latter end of February to the latter end of March, the plants will be very fine and fit for use. I prefer that which has been blanched with our round sea-gravel, about the size of large peas or beans, to any other mode whatever. The plants should be cut but once in a year, as cutting them oftener weakens and lessens the size of the plants. If it is not desired to have the plants large, they may be blanched and cut a year sooner.

Sandy soil best for it.

I have sent a specimen of the sandy soil in which it grows naturally here, as I think the generality of gardeners are too careful, and manure the ground too highly for it. In the month of April last, after cutting my plants, I covered the ground all over, at least six inches above the crown of the plants, with this earth, they soon shot up through it, and never looked finer, or produced a larger quantity of good seed than that year.

should be covered when cut.

I am thus particular in order to show, that this vegetable will succeed as well, if not better, in poor ground than in rich; provided the soil be dry, and care taken in the management, I speak from the long experience, having been well acquainted with the management of this valuable plant from my youth. When I cut the sea-cale for use, I immediately draw up the earth with a trowel, so as completely to cover the whole of the plant; this I fancy makes them grow more luxuriantly. This plant, if properly managed, is superior to asparagus, and if more is cut than wanted for immediate use, it will keep for some days in a pan of cold water, but of course it cannot be better than when recently cut. It precedes the use of asparagus, being ready for the table in February and March.

H. B. WAY.

XII.

On the perfumed cherry, Prunus mahaleb: by Mr. Tollard, *senior**.

The perfumed cherry tree ornamental,

particularly adapted to chalky land,

and its wood very useful.

THE perfumed cherry is a pleasing tree for shrubberies. Its flowers are white, and diffuse a very pleasant smell. It rises as high as twenty feet, grows in poor land, and appears particularly suited to a chalky soil. In this respect it is a valuable tree, as scarcely any other thrives in a soil of this kind; except the Scotch fir, *pinus sylvestris*, and the *salix capræa*. The wood of the *prunus mahaleb* is smooth, close grained, takes a good polish, and is useful in turnery, cabinet-making, &c.

May be grafted with other cherries.

Seed sown in autumn.

Any sort of cherry may be grafted with success on a stock of the *prunus mahaleb*. It is propagated by seed, which is sown in the course of the autumn. It thrives so much in chalky and marly soils, that extensive plantations of it have been made, since this property has been discovered.

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* Sonnini's Bib. Phys. Econ. Feb. 1810, p. 82.

Notice from a Work of Monsieur LELIEUR, *on the hereditary Diseases of Fruit Trees: by the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. &c.*[*]

M. LELIEUR, a French gentleman who holds the office of Administrator of the Parks and Gardens of the crown, has lately published a book on the diseases of Fruit Trees.

Hereditary diseases in the peach tree.

In this he asserts, that the disease called in French *le blanc,* or *le meunier,* which shows itself by a mealy whiteness on the leaves of the peach tree, or on the fruit itself in blotches, that destroy the flavour, is an hereditary disease: that plants raised from the kernels of trees subject to this disease, will produce plants in like manner infected, and which will communicate the disease to grafts taken from sound trees inserted in them; and that grafts from diseased trees will certainly be diseased, although taken from branches that are quite free from it.

Gum also hereditary.

He attributes the same hereditary continuance to the *gum,* a disease more mischievous possibly than any other, to our grafted and budded stone fruits; and he is of opinion, that this disease also may be entirely avoided, by grafting from trees that never have been subject to its attacks.

The importance of these facts to the interests of horticulture, will, it is hoped, justify the writer for offering this short account of them to the society, though they are taken from the Moniteur of the 7th December, 1811, the book not having been yet brought into this country.

The mealy disease not contagious.

The mealy disease, he says, is certainly not contagious, and he instances a fruit-wall at *Versailles,* on which are many curious *peach trees,* some of which are much damaged by it, while others are intirely free from it.

XIII.

SCIENTIFIC NEWS.

Medical and Chemical Lectures.

Medical and chemical lectures.

ON Monday, October 5th, a course of lectures on physic and chemistry will re-commence in George Street, Hanover Square, at the usual morning hours, viz. the Thera-

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[*] Trans. of the Hort. Soc. Vol. i. App p. 27.

peutics at eight, the practice of physic at half after eight, and the chemistry at a quarter after nine. By George Pearson, M. D. F. R. S. Senior physician to St. George's Hospital; of the College of Physicians, &c.

Clinical lectures are given as usual on the patients of St. George's Hospital every Saturday morning at nine o'clock.

Lectures on Surgery, Physiology, and Pathology.

Lectures on surgery, physiology, and pathology.

Mr. A. Carlisle, F. R. S. professor of anatomy in the royal academy, and surgeon to the Westminster hospital, will begin his course of lectures on the art and practice of surgery, and the sciences connected therewith, on Monday, October the 12th, at half after eight, P. M., at his house in Soho Square.

The introductory discourse is open to all professional students, and the subject to be continued on Mondays, Wednesdays, and Fridays, at the same hours.

The diseases and accidents allotted to the province of surgery will be amply treated of, and illustrated by cases from the lecturer's experience. A compendious view of the animal economy will be adduced to illustrate the several processes of disease, and of recovery.

The operations of surgery, and the anatomy of the affected parts, are to be demonstrated.

Surrey Institution.

Lectures at the Surrey Institution.

We understand the following arrangements have been made for lectures at the Surrey Institution, in the ensuing season; Mr. COLERIDGE on the Belles Lettres, to commence on Tuesday, the 3d of November, and to be continued on each succeeding Tuesday; Mr. MASON GOOD on the philosophy of physics, to commence on Friday, the 20th of November, and to be continued on each succeeding Friday; and Dr. CROTCH on music, to commence early in 1813.

London Hospital.

Practice of physic.

Dr. Buxton's autumnal course of lectures on the practice of medicine will be commenced on Thursday morning, the 1st of October, at 11 o'clock.

On the apparent figure of Stars, &c.

g.1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

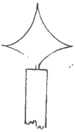


Fig. 7.

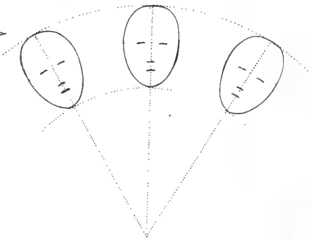


Fig. 8.

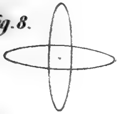


Fig. 9.

Fig. 10.

Fig. 11.

Fig. 12.

Fig. 13.

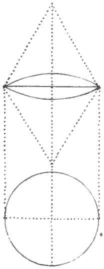


Fig. 14.

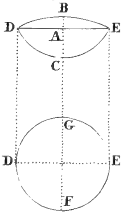
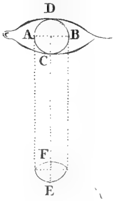
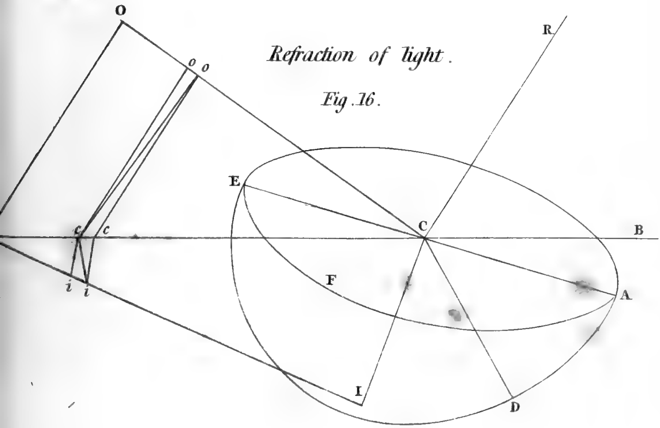


Fig. 15.



Refraction of light.

Fig. 16.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1812.

ARTICLE I.

A Continuation of Experiments on the soniferous Vibrations of the Gasses, &c. by Messrs. KERBY *and* MERRICK.

To William Nicholson, Esq.

SIR,

THE following experiments, on the musical sounds of the gasses, were performed with new apparatus, similar to that which I described in a preceding communication to the Philosophical Journal, vol. XXVII, p. 269. To those who may be disposed to construct apparatus for the same purpose, a statement of its dimensions will not be unacceptable. Such, however, as would possess it without the trouble of fitting it up themselves, may procure it of Mr. Bancks, 441, Strand.

Bellows.

The bellows are made of three small pieces of mahogany, each six inches long, three wide, and three tenths of an inch in thickness. They are connected by folds of thin leather, glued round their edges. The requisite pressure on the bellows is given by a spring of brass wire. A kind of fusee was added, to equalize the blast; something like a contrivance for the same purpose in Mr. Liston's perfect organ :—on trial it was found in this case, to be of no advantage.

Pipe.

Its pitch.

The organ-pipe employed is of the kind called the stopped diapason: its width is 0·57 of an inch, depth 0·71, and its length, without the plug by which it is tuned, 5·15;—the thickness of its sides 0·15, and the width of the *mouth* 0·16. When tuned a minor tone above one of Hawkins's new C tuning-forks, the length of the vibrating column of air was found to be 4·1 inches; the barometer being at 29·60, and F. thermometer at 65°.

Air pump and guage.

Thermometer.

Receiver.

Vapour.

Exhaustion.

Sulph. acid used to absorb vapour.

Ice produced in 2′.

A long barometer-gauge was added to the air-pump, having a scale movable by an endless screw for adjusting the zero to the surface of the mercury in the basin below it. In noting the experiments, the height of the mercury in the gauge was subtracted from that of a barometer suspended in the same room. Having by accident broken the small thermometer attached to the bellows-frame, we registered the temperature from another thermometer placed outside of the receiver. The capacity of the glass receiver is 275 cubic inches, and that of the effective part of the pump barrel 26·7. Nothing but pomatum was used between the brass plates and the receiver, on account of wetted or oiled leathers being known to afford a great deal of vapour. Notwithstanding this, the gauge was depressed by vapour; for, on exhausting the receiver, the gauge indicated two tenths of an inch less than the barometer, but when again exhausted, after placing a cup with sulphuric acid in the receiver, the difference of the two was only 0·06, and in one case we could perceive no difference. For this reason, in all the experiments, except those on water, alcohol, ether, and oil of turpentine, a glass containing five or six ounces of sulphuric acid was placed by the bellows in the receiver. With this pump, and using the acid instead of muriate of lime, water has been frozen by Mr. Leslie's process in two minutes, while the thermometer in the room was at 67°.

Plate.

Plate V fig. 1 (opposite page 240 of the present volume) represents the apparatus made use of for transferring liquids into the exhausted receiver. It consists of a small glass tube, graduated, which screws on the cock above the transfer-plate: the top of this tube is closed by a piece of ground glass smeared with pomatum. To the other end of the cock, under the plate, a small semispherical brass dish is screwed, to catch any liquid liqnid that falls through the cock, and prevent its injuring the bellows.

Every time, before the receiver was exhausted, the wooden pipe was carefully tuned to 0·2250 of a monochord or sonometer, divided decimally, the monochord being tuned accurately to a C fork. The box of this monochord is made of straight-grained deal, and is 36 inches long, three inches wide, and 2·5 deep. Over two immovable bridges, placed 30 inches asunder, a steel wire, 0·017 of an inch in diameter, is strained by two endless screws, placed at the extremities of the box, which act like the screws of a modern English guitar. A long wire is preferable to a short one, because a small alteration of the tension or temperature will cause a less perceptible difference in the pitch of the sound it produces. Lord Stanhope used steel wire on his curious monochord, finding that it did not keep continually lengthening, as brass or iron wires do when the tension is considerable. A curious experiment is related of the Stanhope monochord, which I have never yet seen explained. Two equal wires were put on it, and brought in unison with G, an octave below the treble cliff. One of the wires was then shortened as little as the eighteen-thousandth part of an inch, and this was said to produce invariably an audible *beating*, which could be very sensibly *felt* with the finger as well as heard! What was the cause of this beating? The length of the G wire was 20 inches, which could be divided by that instrument into 360000 equal parts, consequently the length of the altered wire was 359999 of those parts. Now, the vibrations of strings, which differ in length only, being in the inverse ratio of their lengths; if we assume 180 as the number of vibrations in 1″ of that G at concert pitch, the shorter string will make only 180·0005 vibrations in 1″, and consequently not a single beat can be produced by such an imperfect unison in half an hour* ! The beating that was produced, therefore, remains unaccounted for.

Monochord.

Its wire.

Should be of *steel*.

Beats.

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* Sauveur, Chladni, and Dr. T. Young, consider every *Ut* or C as a power of 2, taking the fundamental C for unity. At this pitch, middle C makes 256 "acoustic vibrations" in 1″: Sauveur's experiments, in 1700, give 244. Euler and Marpurg attribute to the same C 236 and 250 in 1″; Cavallo gives 256·8; Smith 247; Sarti 262; Robison 240; Hawkins 238·6; and Farey 241·5.

Concert pitch.

Tension of the wire.

Doctor Crotch remarks*, that a monochord-wire should be stretched equally at both ends, or else it will be inaccurate.

Chladni and Jacquin's experiments.

Their apparatus.

Length of the pipe;

pitch $\frac{11331}{5{\cdot}9.}$

The only experiments on the sounds of the gasses, with which I am acquainted, and which have preceded ours, are those made by professors Chladni and Jacquin at Vienna several years ago. Many objections might be made to their apparatus. It consisted of an open organ-pipe of pewter, fixed within the neck of a glass receiver, furnished with a stop-cock above the pipe, and a bladder on the outside. When the apparatus was sufficiently filled with gas, the blast was excited by pressing the bladder : this was done over water. The temperature during their experiments was from 54° to 59° of Fahrenheit. The length of the vibrating column of air in their pipe was about 15 *centimetres*, or 5·9 inches ; hence it would produce a sound of three octaves higher than *Ut* 3, or the tenor-cliff C. Their results will be mentioned farther on.

Priestley and Perolle's exp.

The experiments of Priestley and Perolle, with a bell rung by wheel-work, had for object only to determine the *intensity* with which sound is transmitted by different kinds of gas, and are therefore dissimilar from those which I shall now describe.

Exp. 1.

Nitrous oxide.

Explanation of the tables.

1. The receiver being exhausted till the gauge stood only 0·44 of an inch lower than the barometer, *nitrous oxide*, produced by decomposing nitrate of ammonia, was transferred from the gas-jar into the receiver of the pump in four successive quantities. After each transfer, the scale of the gauge was adjusted, and the movable bridge of the monochord slid till the wire and pipe were in unison. While this was doing, the gauge ascended a small quantity, as we had anticipated, from the absorption of vapour, by the sulphuric acid. To save room, as the mode of operating was uniform, I shall dispose each gas in a separate table of five columns, the first, from the left, showing the number of successive quantities, and the name of the gas ; the second, the temperature ; the third, the quantity of rise ; the fourth, the pressure after that rise was observed ; and the fifth, the monochord lengths corresponding with the pitch of the organ-pipe. No settled portion of time was allowed for the gauge to ascend. The sound of this was louder and deeper than that of any other gas, and in quality of tone

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* Elements of Thorough Bass, &c. 4to. 1812.

(timbre) resembled the sound produced by a bad-toned bagpipe. The pitch was a little more than a major third below that of atmospheric air: our preceding experiments with an *open* pipe and less accurate apparatus gave 3d.—comma.

1. Nitrous oxide - -	67	·07	8·17	·27350
2. - - - - - - - -	67	·11	15·52	·28000
3. - - - - - - - -	67	·20	23·02	·28550
4. - - - - - - - -	67	·25	29·77	·28675

We thought the acid in the receiver was become a little more opaque, but no froth appeared on its surface, as it had in some of the experiments. On opening the stop-cock in the transfer-plate, and working the pump, the sound of the pipe became more acute, like the sound of a violin-string, which is slowly shortened by sliding the finger.

Expt. 2. Carbonic acid.

2. *Carbonic acid,* disengaged from chalk by dilute sulphuric acid, and collected over water. The tone of this gas was weak and reedy*.

1. Carbonic acid -	61·5	·30	10·23	·2740
2. - - - - - - -	——	·12	14·25	·2772
3. - - - - - - -	——	·34	25·96	·2850
Atmos. air - - -	——	—	28·80	·2850

Exp. 3. Carbonic acid and hidrogen.

3. This gas remaining in the receiver, the pump was worked till the pressure on the gauge was 14·34, when a quantity of hidrogen gas was added. The sound became more acute with a slide, and clearer and smoother than the sound of carbonic acid alone.

Carbonic acid - - -	61·5		14·34	·2760
Hidrogen added - - -			28·80	·2115
Do. remained - - -		.65	28·15	·2125

The pitch of these gasses was not altered by working the pump till the pressure was 14·46; but on filling up the receiver with atmospheric air, the sound was depressed to ·2215, and the gauge rose 0·30 in a very short time.

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* Chladni found the pitch of this gas to be almost a major 3d below that of atmospheric air, a result which accords with this experiment.

4. *Chlo-*

Exp. 4. Oximuriatic gas.

4. *Chlorine* gas, obtained from oxide of manganese and muriatic acid, and collected over water. In this experiment the gauge was not used. The receiver was filled by three successive and nearly equal quantities of the gas. The pitch was not quite a minor 3d lower than that of air.

1. Chlorine gas - -	67	——	——	·25400
2. - - - - - - - - -	—	——	——	·27675
3. - - - - - - - - -	—	——	——	·27900

Exp. 5. Olefiant gas.

5. *Olefiant gas,* or supercarburetted hidrogen, produced by boiling, in a glass retort, alcohol and sulphuric acid.

1. Olefiant gas - - -	60	·09	7·39	·23400
2. - - - - - - - - -	59	·13	15·65	·23825
3. - - - - - - - - -	59	·16	22·62	·24100
4. - - - - - - - - -	59	—	29·18	·24120

Its pitch.

Exp. 6. Olefiant and oximuriatic.

6. The pitch, taking the mean length, is almost a major semitone below that of atmospheric air. Having noted this experiment, the pump was worked till the pressure was only 15·68, and three measures of *chlorine* gas were added, in succession, to the *olefiant gas,* which remained in the receiver.

1. Chlorine added - -	55	2·70	19·48	·25650
2. { - - - - - - - -	54	- - -	26·14	·26000
{ remained - - -	54	4·39	21·76	·25550
3. { - - - - - - -	53	- - -	27·75	·27400
{ - - - - - -	53	6·41	21·34	·26925

Pitch.

During this experiment, the barometer ascended from 29·18 to 29·75. The tone was very peculiar, and cannot be easily described. After each addition of gas, the pressure began to diminish, and the pitch to ascend. The mean length gives the pitch nearly a superfluous second lower than that of atmospheric air. The brass plate of the pump was found entirely covered with a purplish gray oil, which was extremely difficult to remove, and which was not quite removed for a long time after.

xp. 7. xigen gas.

7. *Oxigen gas,* obtained from oxide of manganese by heating it in an iron retort.

1. Oxigen gas - - -	69	.03	7·95	·23375
2. - - - - - - - - -	69	·10	15·81	·23600
3. - - - - - - - - -	69	- -	23·61	·23900

Here

Here the lever of the bellows breaking, we had to begin this experiment anew. The gas was pumped into a bladder and used again.

1. Oxigen gas - - -	69·5	·05	8·26	·23250
2. - - - - - - - -		·25	15·61	·23600
3. - - - - - - - -		·40	22·61	·23800
4. - - - - - - - -		- -	29·98	·23875

Its pitch.

A mean of seven gives the pitch a little more than a minor semitone graver than that of air—Chladni found it to be a semitone or nearly a tone; and our former experiments, with the little open pipe, make it not quite half a comma.

Exp. 8. Nitrogen gas.

8. *Nitrogen gas,* obtained from small pieces of lean muscular flesh (beef) and weak nitric acid, gently heated in a glass retort. The gas stood over water for twelve hours before it was used.

1. Nitrogen gas - - -	68	·08	7·72	·2185
2. - - - - - - - -	68	·14	15·20	·2250
3. - - - - - - - -	68·5	05	22·95	·2275
4. - - - - - - - -	69	- -	30·00	·2290

Its pitch.

This gas produced a very weak, dead sound; the same in pitch as common air. Chladni found it almost a semitone graver.

Exp. 9. Nitrogen and oxigen.

9. After working the pump till the pressure was only 22·5, *oxigen gas* was added to the nitrogen in the receiver, till the pressure was again 30·0, when the sound of the mixed gasses was 0·2335, or almost three commas below the sound of common air. Chladni found that a mixture of these two gasses gave a sound in unison with that of atmospheric air, being more acute than either gas alone. "But before the mixture of these fluids had become homogeneal by repeated pressions of the bladder, the sound was not appreciable, because the vibrations could not be isochronous." (Chladni, § 67.)

Exp. 10. Nitrogen.

10. *Azotic gas,* procured by setting fire to a piece of phosphorus in atmospheric air confined over water. It was inflamed with a burning lens. Before the gas was used it was left in sunshine, over water, for several hours.

1. Azotic gas - - -	60	·10	7·30	·2210
2. - - - - - - - -	—	·10	14·50	·2210
3. - - - - - - - -	—	·10	21·55	·2215
4. - - - - - - - -	—	·15	27·75	·2230
				——
Added common air - -	—	- -	29·40	·2225

(A little air added.)

Pitch.

A mean of the four gives the sound a little more than a comma more acute than the sound of common air.

Exp. 11. Sulphuretted hidrogen.

11. *Sulphuretted hidrogen,* obtained from powdered sulphuret of iron, a little water, and weak muriatic acid, gently heated in a glass retort, and collected over water. A large quantity of the gas was absorbed by the water in the pneumatic trough.

1. Sulphuretted hidrogen	62·5	·39	6·88	·0000
2. - - - - - - - -	61·5	·68	13·74	·2035
3. - - - - - - - -	61	·30	20·82	·2070
4. - - - - - - - -	58·5	·50	27·12	·2075

The least sonorous of the gasses.

This is the least sonorous of all the gasses that we have tried. The sound was hardly appreciable after the second tranfer of gas; and even after the third it was impossible to maintain a continual sound by the most rapid action of the bellows. The pitch of this gas, from a mean of the three lengths, is not quite a minor tone higher than that of common air, but is more than a major semitone. At the end of this experiment, the sulphuric acid placed in the receiver had a froth on its surface about a quarter of an inch high, and of various metallic colours. And we observed, that a part of the oily matter produced in the sixth experiment, and which so obstinately adhered to the plate, could be much more easily wiped off.

Its pitch.

Exp. 12. Hidrogen.

12. *Hidrogen gas,* obtained from water, bits of zinc, and sulphuric acid.

1. Hydrogen gas - -	61·5	·10	7·96	·111
2. - - - - - - - -	——	·08	15·36	·111
3. - - - - - - - -	——	·14	22·72	·111
4. - - - - - - - -	——	- -	28·86	·111

Its pitch.

The sound was weak, and more than an octave and comma above that of air. On working the pump till the pressure of the gas was 14·28, and adding breath till the pressure was again 28·86, the sound of the mixture agreed with ·1815 of the monochord; and the gauge soon rose again 0·2.

Breath added.

14. *Light*

Exp. 13. Light carburetted hidrogen.

13. *Light carburetted hidrogen,* produced by distillation from chips of deal.

1. Lt. carb. hidrogen - -	59	- -	- - -	·1075
2. - - - - - - -	—	11	14·92	·1075
3. - - - - - - a	—	18	21·55	·1075
4. - - - - - - -	60	- -	28·45	·1100
5. - - - - - - -	—	- -	29·13	·1120

Its pitch.

The pump being worked till the pressure of the gas was again 14·92, the sound was ·1060. The pitch, from a mean of the six monochord-lengths of wire, is more than an octave and two commas above that of air.

Exp. 14. Sulph. ether.

14. *Ether.* The graduated glass tube was screwed upon the stop-cock of the transfer plate, and filled with ether. The pressure on the gauge being 0·62, with no acid in the receiver, a small part of a cubic inch of ether, as shown in the left-hand column of the following table, was admitted into the receiver through the stop-cock.

·05	1. Ether - - - - -	60	0	1·48	·09650
·05	2. - - - - - - -	—	- -	2·05	·10320
·05	3. - - - - - - -	—	- -	3·09	·10387
·05	4. - - - - - - -	—	- -	4·30	·11000
·05	5. - - - - - - -	—	- -	5·30	·11290
·05	6. - - - - - - -	—	- -	6·10	·11450
- -	7. - - - - - - -	—	- -	8·77	uncert.
·00	Air added - - - -	—	- -	29·70	·22150

The receiver heated.

After the seventh transfer of ether, we applied a cloth dipped in warm water round the receiver. The gauge fell 0·38 ; and we imagined we could hear two sounds, about a fifth different in pitch ; the one a wheezing tone, the other much clearer. The hemispherical dish was about half full of liquid ether within the receiver. By a mean of six lengths, the pitch is almost an octave and major semitone more acute than the sound of atmospheric air. The air and ether vapour give a sound which is only about a comma-deficient minor semitone more acute.

Exp. 15. Alcohol.

15. *Alcohol,* introduced into the receiver in the same manner as the ether, sunk the gauge only 0·55, and produced no sound. When air was added to fill the receiver, the sound was 0·2260, and very indistinct. Thermometer in the room 60°, barometer 29·7.

Other expts.

Hot water, liquid ammonia, and oil of turpentine, were successively treated like the ether, and found to produce no sound, and but very little depression of the mercury in the gauge.

Air and hidrogen gas are the only elastic fluids that have not varied in pitch with a considerable variation of pressure.

At present, I shall not enlarge on these experiments; but subjoin a table, showing the *relative* lengths and vibrations corresponding with the sounds of the gasses when the sound of air is taken as unity. "Des faits, et point de verbiage, voilà la grande règle en physique comme en histoire." (Dalembert.) In the right-hand column of the table I have placed the logarithms of the intervals with air; for the value of any interval is the logarithm of its constituent ratio*.

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* See Dr. Smith's Harmonics, sect. I; and a Table of Intervals, by Mr. Farey, in the Edinb. Encyclopædia, vol. II, 1810.

Postcript. The pitch or value of a sound depends on the frequency of its vibrations. It has been asked—" Why should not the measure of an interval be the *difference* of the values of its terminating sounds? and consequently why should not intervals be compared by the differences of the values of their sounds?" In answer it has been said, that " the measure of an interval estimated in that manner would vary according to the unity of time chosen for representing the value of the sounds. For, let a and b be the numbers of vibrations of two sonorous bodies in one second; ma and mb will be the numbers of vibrations of these bodies in a time m times greater. The interval would then be measured in the first case by $b - a$, and in the second case by $mb - ma$, a quantity necessarily different." The following theorems respecting intervals, translated from A. Suremain Missery (1793) may be useful to some of your readers, who are not familiar with the subject.

" Considering intervals in one direction only :—

Theorems.

" I. The product of the constituent ratios of two or more different intervals is the constituent ratio of the interval which would be equal to their sum.

" II. The quotient of the constituent ratios of the two intervals is the ratio constituting the interval which would be their difference.

" III. Every natural power of the constituent ratio of an interval is the ratio constituting the interval which would be a multiple of the first marked by the degree of the power.

" IV. Every natural root of the constituent ratio of an interval is the constituent ratio of an interval that would be an aliquot part of the first marked by the degree of the root.

" V. Any fractional power whatever of the ratio constituting an interval is the constituent ratio of the interval which would be a portion of the first marked by the exponent of the fractional power." 75. For authors on this subject, see Forkel's *Allgemeine Litteratur der Musik*, *kap.* I. of the second part; Leipsic, 1792.

Exps.	Aeriform fluids.	Mean lengths of wire.	Relative lengths.	Relative vibrations.	Logarithms of intervals.
1	Nitrous oxide gas -	·2814375	1·250833	0·799467	·0971995
2	Carbonic acid - -	·2787333	1·238814	0·807223	·0930063
4	Chlorine - - -	·2699167	1·199610	0·833590	0790473
6	Do. and olefiant - -	·2630500	1·169111	0·855351	0678558
5	Olefiant - - -	·2386125	1·058333	0·944882	·0246225
7	Oxigen - - - -	·2362857	1·050159	0·952237	·0212549
9	Do. and nitrogen -	·2335000	1·037778	0·963597	·0161044
3	Carb. a. & hidrogen	·2333333	1·037037	0·964286	·0157942
—	Common air - - -	·2250000	1·000000	1·000000	·0000000
8	Nitrogen - - -	.2250000	1·000000	1·000000	0000000
10	Azotic - - - -	·2216250	0·985000	1·015228	0065637
14	Ether vap. and air -	·2187500	0·972222	1·028571	0122344
11	Sulph. hidrogen -	·2060000	0·915556	1·092233	·0383153
12	Hidrogen - - -	·1110000	0·493333	2·027027	·3068595
13	Carburetted Do. - -	·1084167	0·481852	2·075326	·3170863
14	Ether vapour - -	·1063283	0·472570	2·116088	·3255337
14	Do. highest - -	·0965000	0·428889	2·331606	·3676552

I remain,
Sir,
Your humble Servant,
ARNOLD MERRICK.

Quern's Road, Cirencester,
22d Sept. 1812.

II.

On the secret and open Nectaries of various Flowers. In a Letter from Mrs. Agnes Ibbetson.

To Mr. Nicholson.

SIR,

On the nectary of flowers.

IN the exact dissection I have given of a flower in your Journal for July last, the explanations of the calyx, corolla, and stamen, were alternately given, their peculiar vessels described, and the separate cylinders, which convey those vessels to the stalk, accurately and exactly marked. There remains, therefore, of the flower but two parts to develope, the *nectary* and the *pistil*: the most important, indeed; and which have never, I think, been rightly explained, or properly delineated, particularly

larly the former. I shall therefore dedicate the greater part of this letter to this subject: first, to the display of the various functions of the nectary; secondly, the importance of this part of the flower to botany in general; thirdly, the description of the many different sorts of nectaries both concealed and open; and fourthly, the curious mechanism displayed in their various formation.

Honey supposed to be intended to invite insects, so that they might convey pollen from one flower to another.

It has been conceived, and frequently asserted, by our first physiologists, that the only purpose, or known use, of the honey found in plants, was to tempt the insect tribe to visit the flowers, that, while inserting their heads into the interior, they might, by rubbing against the stamen, take up some of the powder of the pollen, and convey it to the pistil in other flowers; and thus impregnate seeds, which, without their assistance, might not be able to procure the powder necessary to their completion. That nature has bestowed on the insect tribe the curious knowledge necessary to seek the honey in a flower*, and make their search thus serviceable not only to themselves but to botany, I have no doubt; but I am equally convinced, that few, very few, of the indigenous plants of any country, (even of the diœcian class,) require such assistance; and that if flowers were never removed from their native soil, they are all sufficient to perform every part which nature has assigned them, in fructifying their own seeds. It is very little known, (because it has never before been a matter of serious investigation,) how much motion indigenous plants possess. That they are scarcely ever still, is an absolute truth; and that on a warm day we need not seek in the plants of other countries that curious and regular motion to be found in each field in our own. That the pistil and stamen regularly bend to each other, so as to enable

This assistance seldom necessary.

Mobility of our native plants.

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* Any person, who has seen an insect seek the honey in an antirrhinum, will be convinced, that this knowledge is necessary; and that without it the insect would fly to any other part of the flower, but the one at which it opens: whereas it settles at once on the round top, fixes one foot on the opposite petal, which it pushes open, inserts the head and shoulders within the flower, lengthens the proboscis, and draws up the honey. All this is the work of a few seconds of time, and it is done in so perfect a manner, so immediate and so direct, that nothing but a thorough knowledge of the flower could enable the insect to act thus decidedly.

the female to acquire pollen sufficient to fructify its seeds, there can be no doubt with those who thoroughly watch flowers; also that the mechanism for the purpose exists in them, and is always found capable of performing its office in indigenous plants. If, therefore, the pistil and stamen can almost always suffice, without aid, to impregnate the seeds, is it likely, that so large and seemingly important a part as the nectary should be placed in *every flower*, when not likely to be necessary but to a very few? But the nectary has, in reality, a much more important task to perform, and its history is beautiful and perfect in all its parts. There is in every flower a concealed as well as an open nectary; its luscious juice is formed within the vessel of the line of life, and increases in sweetness as the plant advances towards flowering. It is this juice which appears at the head of the stigma in one or many glittering drops, which dissolve the pollen, conveying the joint mixture to the seeds, which it impregnates. Without this liquid, therefore, the seeds would not be completed. Hence the cause of nature's forming *two nectaries* in each flower, which are the sacred deposits of this juice; the one that insects can attain, the other closed to them, and so well guarded when not *wholly closed*, that death generally follows the attempt to seize it: for though the insect tribe are taught to find the open one, yet if they could take all the honey thus deposited, the seeds could not be impregnated. Thus Nature, ever indulgent, bestows as long as the *general good* will allow; but gluttony brings its own punishment.

Nectary not formed for one purpose only.

Why two nectaries are necessary.

Nor are the purposes already mentioned the only ones for which the nectary was designed by nature, and bestowed on flowers. The various alteration of the juices; the first feeding of the embryo of the seeds and buds; the innumerable combinations formed in the interior of plants, of which the nectarious juice is the basis; and the decomposition of water, which the solar microscope so admirably shows to be constantly going on in plants; must owe much to the juices of the nectary. I have observed for some time past, that the line of life (the source of the nectary) bleeds whenever it arrives at that part of the stem where the leaves shoot, and the buds come forth; and have continually seen it ooze out when greatly magnified: but I was most curious to know whether this bleeding was the *cause* of that change of colour, which so frequently takes place in many different

Farther use of the nectaries.

Bleeding of the line of life.

different plants at that time, when even the stalk, as well as the axilla of the leaves, is reddened by its tint. I took out, therefore, the whole cylinder belonging to the line of life, and procured a drop or two of this luscious liquid. I then divided the wood from the same plant, and by pressure obtained a little sap : and the mixture of the two immediately changed the colour of the liquids, and converted the light green of the sap to a dark red colour. That the sap running in the *perfect wood* is almost always a pretty powerful alkali, I have long been convinced (differing from that found in the alburnum :) and that the buds, on the contrary, are more or less acid, may be easily shown by cutting and then immersing them in a weak alkaline liquor. The direct change of colour produced in the wood by passing a feeble electric stroke through a plant also proves it, since it reddens the wood vessels, but has no such effect on the buds. We may therefore, I think, conclude, that the reddening of the stalk is caused by the bleeding of the line of life, and the mixture of the *acid* and *alkali* in the bosom of the leaf: and we may also look to the nectary for three of the most powerful effects observable in the life of a plant.

Effect of the acid and alkali in colouring plants.

I now turn to the third division of my subject, and shall show the various species of nectary found in plants, both concealed and open. I need not here represent the manner in which I dissect flowers, as my former letter has shown it, and proved, I hope, how impossible it is, that after cutting them in three *different directions,* I should be mistaken in the formation of their various parts ; since in such a *display* they would mutually be the means of detecting each other, if not critically exact in their delineations. All flowers, with respect to the situation of their seed vessels, may be divided into three distinct kinds: those which have their seed-vessels above the flower ; those in which the same part is found much below, and those in which the germe occupies nearly the centre. In the first and second the secret nectary is generally found in a deep cavity either above or below the seed-vessel, according to its situation in the flower ; but in the last it is so various, that there is no giving rules to find it. In a monopetalous corolla the secret nectary generally forms a little box under the seed-vessel, which has sometimes the stamen opening into it, and is therefore discovered by drawing out the filaments, and finding their ends

The situation of the secret nectary in monopetalous plants, steeped in this luscious juice, when there is not any perceptible in the *cup* of the flower. Another sort of secret nectary is found in most triandrian and hexandrian plants; which have their seed-vessels very low in the stem, and the concealed nectary reaching from the bottom of the corolla to the germe. In some flowers it is above two inches deep; nor is it useless in this situation; it is not only ready to rise to the stigma to impregnate the plant, but it has various vessels passing through the exterior of the seed-vessel (where it joins the nectary) to nourish the embryo till the flower decays, and the rest of the luscious juice evaporates. In the tetradynamian flowers, as well as the geraniums and some others, the *apparent well* is on one side of the stem, reaching from the corolla to a certain mark, or to a stipula; and always distinguished by one of the leaves of the calyx turning *up*, while all the rest turn *down*. The open nectaries of these flowers are as various as the plants. In most of the geraniums it is a *trough* between the corolla and stamen, rising round the pistil, and oozing up from the well below. In the triandrian plants the second nectaries are generally either cavities at the bottom of the stamen, or vessels managed within the corolla. The admirable double nectary of the iris developes much of the intentions of Nature in its formation, since its secret repository is not quite closed, but is sure to catch the insect that attempts to seize its contents. The open nectary is in some species a beautiful fringe with a wide vessel down the reflected petal, and in others it consists of three honey-bearing excrescences flowing from the same source: but the secreted juice is found in a deep cavity in the stem, having a trough at the top, and within the flower, to hold the precious liquid. The two points of this trough secure the insect as it crawls down between the petals, when, not contented with the feast the open one bestows, it stretches out its proboscis to get at the honey within the trough. How often have I stood contemplating this picture, and seen the insect try to insinuate itself lower and lower, though apparently well informed of the danger it was encountering! I found the other day, in a yellow iris on the border of the river, a bee, which, having ventured too far, was caught by the projecting points of the trough, which, like the stamen of the berberris and apocynum androsæmifolium, had got it fastened between them: and it would there have died, had

triandrian and hexandrian plants,

and in tetradynamian plants.

The double nectary of the iris.

The nectary seizing an insect.

had I not released it; for the warmth or moisture of its body so contracted the spiral wire in the petals, as to press them tight against the insect. Nor could it ever have regained its liberty. It was really astonishing to see the flower able to resist the struggles of so large a creature, but the very violence of its exertions seemed to increase its danger, by pressing from below such a quantity of liquid, that it was almost drowning in honey. The best way of getting at the secret nectary of the iris is not by the flower, but by cutting the stem where it is marked for the beginning of the seed-vessel, as that points out also the termination of the nectary.

Nectaries in the diadelphian class.

The two nectaries in all the diadelphian class are most admirably managed. To look at the exterior of the flower it would appear almost impossible to find room sufficient for one, and yet all those flowers possess a very perfect double nectarium, with all the various vessels its different offices require: a deep trough within the cylinder of the males is filled even to half the height of the filaments with this precious juice; and that the bees also may have their share, there is a nectarious ball on each side, on which the banner is fastened, thus serving a double purpose. As the insects draw the honey forth, it is constantly replenished from the trough below; and the exquisite beauty of the contrivance is completed by its not only being ready for supplying the stigma, should the weather be unfavourable to the ripening of the pollen (as in this case it loses much of the nectarious juices by evaporation); but it is also all that time feeding the embryo in the seeds; which have innumerable vessels running through the pod, and most plainly to be perceived in the solar microscope, to imbibe this juice for the nourishment of the young plant; but no sooner does the cylinder of the stamen decay and fall off, and the pods increase, than the skin thickens, the vessels disappear, and the embryo no longer receives nourishment except from the stalk, and its own nourishing vessels.

Didynamian plants, and those with naked seeds.

In the didynamian plants, and in all those flowers which have uncovered seeds, the nectarious juice is secreted in a box in the large part which lies directly under the seeds; while the open nectary is either found at each corner, or opposite to the seeds. In giving the exact picture of the flower of the peach, in the Journal of July last, dissected in three different ways, I left a square at the bottom of the seed-vessel not allotted to any

use;

use; it is this which forms the nectarious box, while the interior lining, or inward cylinder of the stamen, forms the open nectary of all the icosandrian class. Wherever this yellow matter is found, (which appears something like softened bees' wax) it always indicates the nectary, as does also a very brilliant white substance found at the top of the seed-vessel in all pentandria digynia plants, which is also a never-failing sign of its presence. If the dissector finds it difficult to discover the secreted juice, there is a little insect, which may be seen with a small magnifier, that always leads to it. It is of various shades of brown, raises its tail, and the half of its body above, smoothing down its very short wings. It lives wholly on the nectary of plants, and is found there alone. When a drawing, or a sort of map is taken at the bottom of each flower, showing the manner in which the vessels branch off to form their appropriate ingredient, it is exactly seen, that not the breadth of a hair is left unemployed and unallotted, and that the most marked adjustment takes place in this respect: and this is done without in the least disfiguring the parts; all appear as perfectly to coincide as if this alone was the object in view, and yet for what great ends, what noble designs! I often throw down my pen and pencil in despair, ashamed of the folly of attempting to give an idea of works almost too great for man even to conceive. It is certainly true, that we are more struck with the power of the Almighty, when we contemplate his small works, than in the view of his larger chefs d'œuvre: when each diminutive object is so highly finished, it would appear the peculiar care of providence; and yet on farther examination of these minor objects, we find all equally perfect and beautiful.

A yellow matter, an indication of the nectary.

Insect living in the nectary of plants.

Beautiful adjustment of the flower.

To give the double nectary in all plants would be an endless business, equally tiresome to the reader and writer; but I think I shall have given (with the following examples) sufficient to prove the truth of all I have advanced—"That there is a concealed as well an open nectary in all flowers;" and that the situation of both is generally regulated by that of the seed-vessel, which appears to connect it indifferently with calyx or corolla, stamen or pistil: but, however this may seem a matter of no moment, I doubt not it is of the *ūtmost consequence* to the parts, and regulated with the greatest judgment; and that, when I am still better acquainted with the subject, I shall discover the neces-

Nectary connected with the calyx, &c.

necessity of this variation in the situation of the nectaries: for there is not any thing more striking in dissecting plants, than the simplicity and ease with which the cause is discovered in the effect. As a specimen of the nectary in the calyx, I shall give the stock (pl. iv, fig. 1.) Here the two opposite leaves serve as the hidden nectary, and the excrescence alternating the stamen forms the open one. For an example of the nectary in the corolla, I shall give the fritillaria: in this a sort of basin, fig. 2, corresponds with the secret nectary under the seed-vessel. In the stamen it is either at the bottom of the filament, fig. 3, AA, or joined in a more conspicuous manner, as in the corn flower, fig. 4, BB, which is distinguished by many curious circumstances peculiar to itself, which I shall enlarge upon another time, when I give the dissection of the stamen. In all pentandria digynia plants, we have examples of the nectary joined to the pistil and seed-vessel, as at fig. 5: CC being the open, and DD the closed nectary. Here we also see a proof of that brilliant white matter, which always announces the luscious juice. I shall present also one example of a plant having its seed-vessel above, in the passion-flower, fig. 6. Here the nectary is sure to be placed in the following manner: the open one at EE, the closed one at F, perfectly secreted from all danger, yet corresponding with the other nectary, and free to communicate its juices for the benefit of the seeds, and repairing any excess of evaporation lost on the stigma. The secret nectary in the iris I have already shown to be a species of well, see fig. 7, from H to H, while the corners of the trough in the flower are seen at II. The secret nectary in the geranium is shown, fig. 8, from K to K; and the open one, a trough round the pistil at LL. The two nectaries in the silene, cucubalis, lychnis, &c., are displayed at fig. 9: MM being the secret one, NN the open nectary. Those in all flowers, which have uncovered seeds, are seen at fig. 10; PP the open one, OO the secreted box. I have avoided giving those most commonly known, as I preferred the nectaries which have not yet been noticed by botanists, to show more certainly, that two are found indifferently in *all plants*.

Nectary in the corn flower.

Conclusion of the subject.

I shall now conclude my letter with a few words on the mechanical properties of the nectary. There is, I believe, no nectary, that has not the power of giving out its juices, or retaining them,

them. In the delphinium, fig. 11, I have seen it when almost full and turning down, still retain its liquid; nor could I at first conceive how this was managed, till, on dissecting it, I found that there was an inner lining, which contracted at R, when the round part was much distended; and thus prevented the expenditure of its juices. At fig. 5, the nectary has not only the power to retain its juices, but to throw or eject them on the stigma; which I have repeatedly seen it do on a warm sunny day. I have before observed how uncommonly perfect the mechanism is in most flowers in very hot weather, when the spiral wire seems full of vigour, and all its various offices are shown with double force. It is on such a day the liquid of the pistil melts the pollen with more ease; for in general the drop appears on the stigma but one hour, and then retires. This also is the mechanism of the pistil; the curious motion in the nectary of the *ranunculus* is known to a few; the leaf, which is formed to cover it, always encloses it tight if the wind blows, but, on the contrary, admits the rays of the sun to it, if it is a *fine warm day*. There cannot be a more excellent barometer, for it will denote each cloud and each change of the atmosphere; and there are many flowers which draw on, and put off their cover, whenever a threatening cloud appears. In watching flowers very exactly, it is really a perpetual source of astonishment—the varying mechanism is so great, that no person who would take the trouble of sitting by a plant for a few hours, could ever after admit a doubt of its being governed by the mechanical powers.

Mechanical motion of the nectary.

Your humble Servant,
AGNES IBBETSON.

III.

Chemical Researches on the Blood, and some other animal Fluids. By W. T. BRANDE, Esq. F. R. S. *Communicated to the Society for the Improvement of animal Chemistry, and by them to the Royal Society.*

(Concluded from p. 32.)

Researches on the colouring matter of the blood.

1. TO procure this substance for experiments, I generally employed venous blood, which had been stirred during its

Method of collecting the colouring matter of blood.

its coagulation; the fibrina is thus removed, and the colouring matter diffused through the serum, from which it gradually subsides, being difficultly soluble in that fluid; on decanting off the supernatant serum, the colouring matter remains in a very concentrated form. When other modes of procuring it were employed, they will be particularly mentioned; but as I have not found the serum which is retained interfere much with the effects of various agents upon the colouring principle, the method just noticed was commonly adopted.

It appears to consist of globules,

not soluble in water,

2. When the colouring matter thus collected is microscopically examined, it seems, as Lewenhoeck first observed*, to consist of minute globules. These are usually described as soluble in water, a circumstance which my own observations led me to doubt, and which the more accurate experiments of Dr. Young, an account of which, intended for publication, he has kindly permitted me to peruse, have completely disproved.

though the colouring principle in them is.

3. The effect of water upon the red globules is to dissolve their colouring matter, the globule itself remaining colourless, and, according to Dr. Young, floating upon the surface.

The aqueous solution decomposed by heat,

This aqueous solution is of a bright red colour, and not very prone to putrefaction. When heated, it remains unaltered at temperatures below 190° or 200° Fahrenheit; at higher temperatures it becomes turbid, and deposits a pale brown sediment: if in this state it be poured upon a filter, the water passes through without colour, so that exposure to heat not only destroys the red tint, but renders the colouring matter insoluble in water.

alcohol, and ether.

Alcohol and sulphuric ether added to this solution also render it turbid, and when these mixtures were filtrated, a colourless and transparent liquor was obtained.

The precipitate partly soluble in acids.

4. The matter remaining upon the filter was insoluble in water, in alcohol, and in sulphuric ether; but when digested in dilute muriatic or sulphuric acid, a portion was taken up forming a brown solution. I regard this soluble portion as a modification of the colouring matter produced by the operation of heat: the insoluble residuum had the properties of albumen.

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* Haller, Elem. Physiolog. Vol. I, p. 51.

Effects

M.J.Adams Improvement on the Reflecting Circle of the Borda.

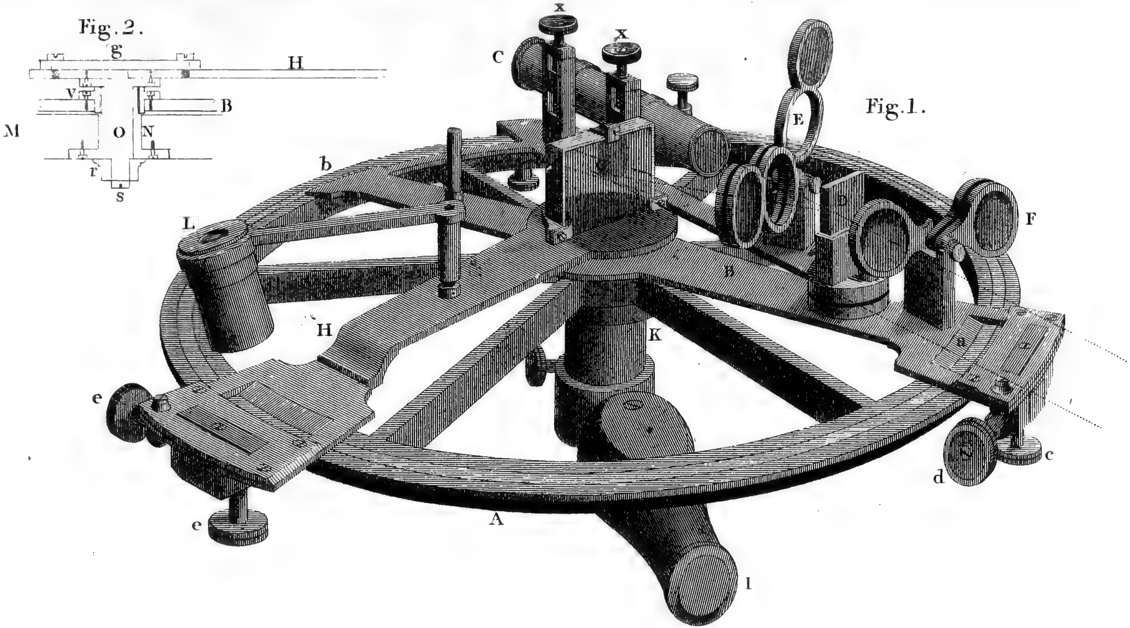

Fig.2.

Fig.1.

Drawn by J.Farey

Engraved by H.Cooper

5. *Effects of Acids on the colouring Matter.*

The colouring matter treated with muriatic acid:

A. Muriatic acid, poured upon the colouring matter of the blood, renders one portion of it nearly insoluble, and of a bright brown colour : another portion is taken up by the acid, forming a dark crimson solution when viewed by reflected light; but when examined by transmitted light, it has a greenish hue.

This solution remains transparent, and its colour is unimpaired by long exposure to light, either in contact with the air, or when kept in close vessels. At its boiling temperature the colour is also permanent.

Infusion of galls produces no change in this muriatic solution, nor is its colour affected by carbonated alkalis, even when added in considerable excess.

It is rendered brown red by supersaturation with caustic potash, but not with soda, or ammonia : these, and especially the latter, rather heighten its colour.

When considerably diluted with water its original colour is much impaired, and the green hue, which it always exhibits by transmitted light, becomes more evident.

In preparing this solution, I frequently employed the coagulum of blood cut into pieces, and digested in equal parts of muriatic acid and water, at a temperature between 150° and 200°. In three or four hours the acid was poured off, and filtrated. The clear solution was in all respects similar to that above described, although before filtration it appears of a dirty brown colour.

I evaporated a portion of this muriatic solution in a water-bath, to dryness; it retained its colour to the last, and left a transparent pellicle upon the evaporating basin, of a dirty red colour : this, when redissolved in muriatic acid, acquired its former tint, but the colour of its aqueous solution was nearer brown than red.

with sulphuric:

B. Sulphuric acid, diluted with eight or ten parts of water, forms an excellent solvent of the colouring principle of the blood.

It may be employed in a more concentrated state, but the bright colour of the solution is in this case apt to be impaired; and when more largely diluted with water, its action is slow

and

and uncertain. Either the sediment of the colouring matter from the serum, or the crassamentum of the blood, may be indifferently employed in forming these solutions.

When dilute sulphuric acid is added to the colouring matter, it renders it slightly purple; and, if no heat be applied, the acid, when poured off and filtered, is colourless; so that dilute sulphuric acid, when cold, does not dissolve this colouring principle.

One part of the crassamentum of blood cut into pieces was put into a matrass placed in a sand heat, with about three parts of dilute sulphuric acid. It was kept for twelve hours in a temperature never exceeding 212°, nor below 100°. After twenty-four hours the acid was filtered off, and it exhibited a beautiful bright lilac colour, not very intense, and tainted with green when viewed by transmitted light.

This solution is nearly as permanent as that in the muriatic acid. Some of it, which has been kept for a month in an open vessel, often exposed to the direct rays of the sun, is very little altered.

When diluted with two or three times its bulk of water, the lilac tint disappears, and the mixture is only slightly green.

When exposed to heat, the colour gradually changes as the acid becomes more concentrated by evaporation, and when reduced to about half its bulk, the lilac hue is destroyed.

The solutions of pure and carbonated alkalis, when added in excess, convert the colour of this sulphuric solution to brownish red; but in smaller quantities they merely impair it by dilution.

with nitric: C. Nitric acid, even much diluted, is inimical to the colouring matter of the blood.

A few drops added to the muriatic or sulphuric solutions gradually convert their colour to a bright brown, and larger quantities produce the same change immediately.

The action which this acid exerts upon the colouring matter under other circumstances is nearly similar, and always attended with its decomposition, so that my attempts to procure a red solution in this menstruum uniformly failed of success.

with acetic: D. Acetic acid dissolves a considerable quantity of the colouring matter of the blood; the solution is of a deep cherry red colour. When somewhat diluted, or when observed in tubes of about a quarter of an inch bore, this solution appears per-

perfectly green by transmitted light. In its other habitudes it nearly resembles the muriatic solution.

with oxalic, citric, and tartaric.

E. The solution of the colouring matter in oxalic acid is of a brighter red than those hitherto noticed; that in citric acid is very similar to the acetic solution, and with tartaric acid the compound somewhat inclines to scarlet. All these solutions exhibit the green hue, to which I have so often alluded, in a remarkable degree.

6. *Effects of Alkalis on the colouring Principle of the Blood.*

Action of the alkalis on the colouring principle.

The caustic and the carbonated alkalis form deep red solutions of this substance, which are extremely permanent.

1. Solutions of pure potash, and of the subcarbonate, take up a large proportion of the colouring matter of the blood. The intensity of the colour of this solution, when concentrated, is such, that it appears opaque, unless viewed in small masses, or in a diluted state, when it is of a bright red colour.

2. In soda and its subcarbonate the solution has more of a crimson hue, which colour is extremely bright in its concentrated state.

3. The solution in liquid ammonia approaches nearer to scarlet than that in which the fixed alkalis are employed.

4. When these alkaline solutions are supersaturated with muriatic acid, or with dilute sulphuric acid, they acquire a colour nearly similar to the original solutions in those acids, which have been above described.

5. Nitric acid, added in small quantities, or even to saturation of the alkaline menstruum, heightens the colour of the three compounds; but when there is a slight excess, a tint of orange is produced, which soon passes into bright yellow.

6. The alkaline solutions may be evaporated nearly to dryness without losing their red colour; during the evaporation of the ammoniacal solution, the alkali flies off, and a brown red solution of the colouring matter in water remains.

Examination of the affinities of the colouring matter.

Having ascertained the above facts respecting the colouring principle of the blood, I next proceeded to examine how far it was susceptible of entering into those combinations which are peculiar to other varieties of colouring matter.

These experiments I shall detail in the order in which they were made.

1. Some

Alumine does not form a permanent red with it.

1. Some pure alumine was added to a concentrated aqueous solution of the colouring matter of the blood, and after twenty-four hours, the mixture, which had been frequently agitated during that period, was poured upon a filter, and the residuum washed with hot distilled water.

The filtrated liquor had lost much of its original colour; the alumine had acquired a red tinge; it was dried at a temperature between 70° and 80°, during which it became brown.

2. Two hundred grains of alum were dissolved in four fluid ounces of a solution of the colouring matter, similar to that employed in the last experiment. The colour of the compound was bright red. Liquid ammonia was added, and the precipitate collected, and carefully dried. It was of a dirty red, and after some days exposure to light, became nearly brown.

From these, and other experiments, which I have not thought it necessary to detail, it appears that alumine will not form a permanent red compound with the colouring principle of the blood; I was therefore next induced to employ oxide of tin.

Muriate of tin combined with it,

3. Fifty grains of crystallized muriate of tin (prepared by boiling tin filings in muriatic acid, and evaporating the solution) were dissolved in four ounces of the solution of colouring matter, which immediately assumed a purple tint, and became afterward brown. It was diluted with twice its bulk of water, and put aside in a stopped phial. On examining it three days afterward, a small quantity of a bright red powder was observed at the bottom of the phial, which proved to consist of the colouring principle combined with the metallic oxide. A portion of this compound, which has been kept in water for some weeks, has undergone no change of colour; but when dried by exposure to air, it loses its brilliant tint, and becomes of a dull red hue.

and the acid separated.

To a compound solution of muriate of tin and colouring matter, similar to that employed in the last experiment, I added a sufficient quantity of solution of potash to decompose the salt of tin. The precipitate thus obtained was collected, and dried by exposure to the air of a warm room. It was of a dull red colour, and has undergone no apparent change by exposure to the joint action of light and air for three weeks.

4. Find-

4. Finding that supertartrate of potash exalted the colour of the blood, I endeavoured to form a compound of it with that substance and oxide of tin, and thus, in some measure, to imitate the process in which cochineal is employed for the production of scarlet dye; but although a bright red compound is produced, when it is dried at a very moderate temperature its colour becomes similar to that of the other combinations which I have described.

Attempt to combine it with supertartrate of potash, and oxide of tin.

These experiments I repeated in various ways, occasionally applying the salt of tin as a mordant to woollen cloth, linen, &c.; but the brilliancy of the colour was never permanent.

The experiments varied.

5. Having observed that infusion of galls and decoction of oak bark do not impair the colour of the blood, I conceived that solution of tannin might answer the purpose of a mordant, as it is effectually employed by dyers in giving permanence to some of their red colours.

Oak bark used as a mordant.

I accordingly impregnated a piece of calico with decoction of oak bark, and afterward passed it through an aqueous solution of the colouring matter of blood. When dry, it was of a dirty red colour, nearly similar to that which would have been obtained, had no mordant been applied: when, however, an alkaline solution of the colouring matter was employed, the colour was equal to that of a common madder red, and as far as I have been able to ascertain, it is permanent.

6. A solution of superacetite of lead was impregnated with the colouring matter of the blood. The compound was bright red: no spontaneous change took place in it, and on the addition of an alkali a white precipitate was formed, the fluid retaining its former tint.

Oxide of lead.

From this, and other experiments, in which it was attempted to combine oxide of lead with the colouring matter of the blood, it would appear, that there is no attraction between these two substances.

7. The most effectual mordants, which I have discovered for this colouring matter, are some of the solutions of mercury, especially the nitrate, and corrosive sublimate.

Solution of mercury.

Ten grains of nitrate of mercury, (prepared with heat, and containing the red oxide) were dissolved in two fluid ounces of a solution of the colouring matter of the blood. After some hours a deep red compound was deposited, consisting chiefly of the

Nitrate of the red oxide, and

the metallic oxide combined with the colouring matter, and a small portion of coagulated albumen. The remaining fluid had nearly lost its red colour.

of the black. The nitrate of mercury containing the black oxide produces nearly similar effects, excepting that the colour of the compound is of a lighter red.

Corrosive sublimate. When corrosive sublimate is added to the solution of the colouring matter, its tint is instantaneously brightened, and it becomes slightly turbid from the deposition of albumen. If this be immediately separated by a filter, the liquor which passes through gradually deposits a deep red or purplish insoluble precipitate, and if it now be again filtrated the liquid is colourless, the whole of the colouring principle being retained in the compound which remains upon the filter.

These salts used as mordants. By impregnating some pieces of woollen cloth with solution of nitrate of mercury, or of corrosive sublimate, and afterward steeping them in an aqueous solution of the colouring matter of the blood, I succeeded in giving them a permanent red tinge, unalterable by washing with soap; and by employing the ammoniacal solution of the colouring matter, calico and linen may be dyed with the same mordant.

In these experiments I was much satisfied by the complete separation of the colouring matter from its solutions, which after the process were perfectly colourless.

VII. Some Remarks on the preceding experimental Details.

Analogy between coagulum of chyle and curd of milk. From the experiments related in the second section of this paper, it appears, that sulphuric acid effects changes upon the coagulum of chyle, similar to those which Mr. Hatchett has observed to result from the action of dilute nitric acid upon the coagulated white of egg. This last substance, however, is not convertible into gelatine by means of sulphuric acid, whereas in these respects the curd of milk resembles that of chyle: this circumstance, as well as the more ready solubility of the coagulum of chyle in dilute, than in concentrated acids, points out a strong analogy between these two bodies.

Sweet taste of chyle. The sweet taste of chyle naturally suggested the idea of its containing sugar*; but I am not aware of any direct experi-

ments

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* Fordyce on Digestion, 2d Edition, p. 121.

ments which have demonstrated its existence, and have therefore detailed minutely such researches as I have been enabled to make upon the subject, hoping at some future period to render them more complete.

No gelatine in blood.

The experiments to prove the nonexistence of gelatine in the serum of blood will, I trust, be deemed sufficiently decisive: they show, that that abundant proximate principle of animals is not merely separated from the blood, in which it has been supposed to exist ready formed, but that it is an actual product of secretion.

It is a product of secretion.

The blood contains but little iron, and this not peculiar to the red part.

The proportion of iron afforded by the incineration of several varieties of animal coal is much less considerable than we have been led to expect, and the experiments noticed in the fifth section show, that it is not more abundant in the colouring matter of the blood, than in the other substances which were submitted to examination; and that traces of it may be discovered in the chyle, which is white, in the serum, and in the washed crassamentum or pure fibrina.

The colouring principle of a peculiar nature;

The inferences to which I have alluded in the first section of this paper are strongly sanctioned by these facts, and coincide with the opinion, which has been laid before the Royal Society by Dr. Wells*, respecting the *peculiar nature* of the colouring principle of the blood, and support the arguments which are there adduced.

independent of iron: and probably may be useful in dying.

That the colouring matter of the blood is perfectly independent of iron, is, I conceive, sufficiently evident from its general chemical habitudes, and it appears probable that it may prove more useful in the art of dying, than has hitherto been imagined, since neither the alkalis nor the acids (with the exception of the nitric) have much tendency to alter its hue. The readiness, too, with which its stains are removed from substances to which no mordant has been applied, seem to render it peculiarly fit for the purposes of the calico-printer. I have not extended these experiments, nor have I had them repeated on a sufficient scale to enable me to draw more general conclusions respecting the possibility of applying them with advantage in the arts: this would have led me into too

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* Phil. Trans. 1797.

wide

wide a field, and one not immediately connected with the objects of this society: the subject, however, appears important.

So employed by the Armenians.

It is not a little remarkable, that blood is used by the Armenian dyers, together with madder, in the preparation of their finest and most durable reds*, and that it has even been found a necessary addition to insure the permanency of the colour†. This fact alone may be regarded as demonstrating the non-existence of iron as the colouring principle of the blood, for the compounds of that metal convert the red madder to gray and black.

Menstrual fluid.

While engaged in examining the colouring matter of the blood, I received from Mr. William Money, house surgeon to the general hospital at Northampton, some menstruous discharge, collected from a woman with prolapsus uteri, and consequently perfectly free from admixture of other secretions. It had the properties of a very concentrated solution of the colouring matter of the blood in a diluted serum, and afforded an excellent opportunity of corroborating the facts respecting this principle, which have been detailed in the preceding pages. Although I could detect no traces of iron, by the usual modes of analysis, minute portions of that metal may, and probably do exist in it, as well as in the other animal fluids which I have examined; but the abundance of colouring matter in this secretion should have afforded a proportional quantity of iron, did any connection exist between them. It has been observed, that the artificial solutions of the colouring matter of the blood, invariably exhibit a green tint when viewed by transmitted light: this peculiarity is remarkably distinct in the menstruous discharge‡.

Rapid reproduction of perfect blood.

I hope that some of the facts furnished by the above experiments may prove useful to the physiological inquirer: they account for the rapid reproduction of perfect blood after very copious bleedings, which is quite inexplicable upon that hypothesis which regards iron as the colouring matter, and may

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* Tooke's Russian Empire, Vol. III, p. 497.

† Aikin's Dictionary, Art. Dying, and Philos. Magazine, Vol. XVIII.

‡ I could discover no globules in this fluid; and although a very slight degree of putrefaction had commenced in it, yet the globules observed in the blood would not have been destroyed by so trifling a change.

perhaps lead to the solution of some hitherto unexplained phenomena connected with the function of respiration. There can, I think, be little doubt, that the formation of the colouring matter of the blood is connected with the removal of a portion of carbon and hidrogen from that fluid, and that its various tints are dependent upon such modifications of animal matter, and not, as some have assumed, upon the different states of oxidizement of the iron which it has been supposed to contain.

Formation of the colouring matter.

IV.

Remarkable Effects of the spontaneous Rise and Overflow of heated Soap Lie in a metallic Pump. In a Letter from R. B. with Remarks by W. N.

To Mr. Nicholson.

SIR,

I WAS much pleased at the sight of your advertisement on the wrapper of your last number, in which you promise an extensive Index to the whole Journal. I have sent my subscription to the publisher; and, as an old correspondent, who has been your disciple from the very first, I think it incumbent on me to look round for a few philosophical facts, and give my assistance to your work as far as my ability may extend. I wonder that you, who are known to be so intimately acquainted with the practice of all our manufactures, have never given us a detail of what may be called the philosophy of a workshop. In the daily performance of manipulations, and the adoption of expedients to ensure success, the manufacturer, by the chance of events, and under the necessity of making repeated trials, is continually, and for the most part unconsciously, availing himself of the tenacities, the toughness, the brittleness, the excitement and conducting of heat, the production and condensation of elastic fluids, the statical and hydrostatical powers, with the chemical energies of bodies, in such a variety of ways, as would, and often does, afford the most certain instruction to theorists, and not unfrequently the means of improving, in return, the very art under contemplation. I write these reflections as they flow from my pen, and as a kind of preface to a fact

Index to the Journal.

Philosophy of a workshop.

Introductory remarks.

fact with which I find myself embarrassed. You have often obliged your correspondents with disquisitions or investigations in cases like the present. Whether this may lead to any useful result beyond the mere fact, I know not: I am sure you will admit it to be curious; and if you have not seen it, I beg you will take the trouble to do so, and oblige me, and your other readers, with your opinion of its cause.

Description of a boiler containg soap and lie:

and a copper pump to draw off the lie from beneath the soap.

When the hot lie was first pumped, the effect was not remarkable,

but after a few strokes it flew out spontaneously,

and continued

Some time ago I visited an extensive soap manufactory, in which there were several large boilers, each capable of holding a tun of soap. One of them was in a state to have the lie drawn off from beneath the soap, which formed a fluid mass between three and four inches thick, above a depth of perhaps four feet of lie. I speak from memory throughout. There was a wooden back or vessel on one side to receive the lie, and a copper pump was lowered down into the boiler from a situation in which it had hung suspended above by a tackle. I think the barrel of the pump might be about four inches in diameter, terminating in a small copper cistern at top, with a spout of about three inches bore, proceeding from one side of the cistern close to its bottom. I do not remember whether the pump was primed or not, but I think not, and suppose it may have had metallic valves, and perhaps a hempen packing, instead of leather, which could not be used with soap lie. The pump was, I doubt not, lowered so as to rest on the bottom of the boiler, for it could not else have been steady—and a workman began to pump. The lie came out of the spout in a stream which did not half fill the bore, and it fell at no considerable distance from the spout. During the whole experiment the soap and the lie in the boiler appeared level and motionless, neither circulating nor showing any other sign of its high temperature, the soap at top preventing even the steam from appearing. But after a very few strokes the lie dashed out of the spout with a sudden noise, and flew to the opposite side of the receptacle in a stream hot, smoking, frothy, and filling the bore of the spout. The workman left off pumping, and the stream continued with unceasing violence and rapidity. I was astonished, and stood gazing at this striking effect, my mind being engaged with a mixture of wonder at the phenomenon and puzzle at its continuing, when the apparent cause had ceased to act. But after a considerable time, I turned to the assistants, and asked, "how it

was

was to be stopped?" Three men immediately came with buckets of cold water, which they dashed against the pump, and the stream immediately slackened, but did not quite cease. It was recovering strength, and I have no doubt would have arisen to its former violence if a fourth bucket of water had not been brought in time, and dashed against the pump, upon which it entirely ceased. These are the facts; to which I will only add, that I think the immersed part of the pump was between three and four feet, and the spout might have been between two and three feet above the surface of the fluid in the boiler. Why hot soap lie, in these or any other circumstances, should spontaneously rise and run with violence out of an aperture more than two feet above its quiet level, is an event upon which I cannot make even a probable conjecture.

till cold water was thrown against the pump.

I am, Sir,
Your obliged Reader,
R. B.

Reply. W. N.

Some years ago, an application was made to me to report professionally upon a pump, which was described to me; and it was added, that it produced the effect of raising water to a greater height, by what is called suction, than 33 or 34 feet. My report was, that the pump was a bad one, and that it was incapable of producing the pretended effect. Some time afterward, however, the same person called on me, and said, that the parties who had commissioned him to apply to me had been deceived by the inventor, who had made a small hole in the suction-pipe. This led to observation on my part, that my general conclusion upon the merits of the pump would have been the same, if this fact had been stated along with the others; but that it would have been accompanied with an indication of the time and place where that trick had been played a century before; which does in truth, by mixing air with the column of water, enable it, (because upon the whole lighter,) to rise to a greater height than mere water; but that the effect, taking height and quantity into the consideration, would be less than that of a common pump.

Account of a pump which raised water by suction higher than 34 feet.

pump. I relate this, not because I infer the existence of any trick in the remarkable fact so clearly and fully described by correspondent, but because it appears to indicate the solution. I have not, however, thought the analogy so perfect as to preclude the necessity of visiting a soapwork, and of making an experiment which seems altogether to clear up the subject.

The facts at the soap manufactory were precisely as he describes them; and very striking indeed, notwithstanding my expectation having been previously raised by R. B.'s description. The course of reasoning suggested by them were as follows:

Inferences respecting the cause of the continued rise and flow of the heated lie. Steam was first produced under the partial vacuum of the pump, and continued from the thin space between the bottom of the suction-pipe and of the boiler. This rendered the column in the pump lighter, and caused its rise to continue.

The lie and melted soap upon the surface, though strongly heated, were not sufficiently so to cause steam to be produced under the pressure of four feet of fluid, added to that of the atmosphere. But, when a part of this last pressure was taken off by the action of the pump, the fluid in contact with the bottom, and immediately under the suction-pipe, was enabled to give out steam; and the thin portion of fluid lying between the metallic edge of the pipe, and the bottom of the boiler, would be more particularly so disposed (it being a well-known fact, that such thin films are very readily converted into steam, as is strikingly shown by heating water in a glass vessel with a small metallic ball or piece of glass lying in it; most of the bubbles seeming to spring from these small bodies.) The progressive increase of steam will render the fluid in the pump-barrel frothy and lighter than the dense lie in the boiler; and as soon as the reaction of its specific gravity becomes so much as that a column of the whole length of the pump shall be lighter than a column of the dense lie, of no greater length than answers to the depth of the boiler, this last will predominate, and cause the other to ascend without the aid of the pump. Now the first developement of the steam was effected by the action of the pump, in taking off atmospheric pressure; but as soon as the pump became very hot, and the steam very copious, the pressure of the inclosed column may be conceived to have become much less than is requisite to allow the continued developement of steam; and the column being allowed to flow out sideways, instead of ever rising to a counterpoise, the diminished pressure, and also the afflux of the hot frothy lie, becomes permanent. And the velocity of emission will be governed by the height to which the lie is enabled or allowed to rise in the trough above the

the level of the spout. I am greatly disposed to think the condition that the heated lie must all pass between the two hot metallic bodies, the lower edge of the suction-pipe, and the bottom of the boiler, is of great consequence to the whole effect.

Experiment. Water boiled in a vessel, in which an upright glass tube was placed, stood higher in the tube.

In order to bring the subject in some measure to the test of experiment, I took a tubular glass vessel, ten inches long, and one inch internal diameter, having a small enlargement, or foot to stand upon, as a chemical measure. By a smart stroke with the end of a pointed file, I drove the bottom in, by making a large hole in the middle without breaking the other parts. In this state it was capable of standing on its bottom as before. A tin vessel with a flat bottom was then filled to the depth of three inches with water, and the glass vessel being set upright in the water, the whole was placed on the fire. As soon as the liquid began to boil, the bubbles were most plentiful in the tube, and the water stood at rather less than three quarters of an inch higher in the tube than in the exterior vessel. When the boiling was very rapid, the difference was rather less, and when the tube was raised a little from touching the bottom, the difference ceased. Upon taking the tube out, and letting it cool, and then setting it as before in the boiling water, the difference or rise did not take place, nor was any boiling seen within the tube till after the lapse of about half a minute; and upon inverting the tube, so that its mouth touched the bottom of the tin vessel, and the foot was uppermost, the water within it did not stand so high, nor boil so fast, as when the contrary position was adopted. And lastly, the difference appeared rather more considerable when the boiling was moderate, than when the whole mass of the fluid was full of steam-bubbles by rapid ebullition.

Particular circumstances of the experiment.

Inferences. The higher station in level of the water in the tube was occasioned by steam produced most plentifully between the lower part of the tube, and bottom of the vessel.

Though the preceding experiment is not so apposite and conclusive as it might have been made, by using a greater depth of a saline fluid, and allowing a side aperture for the same to flow out as in the pump; yet it appears to me to show with sufficient precision, 1. That the column of fluid in the tube was of less specific gravity, from the greater admixture of steam-bubbles, than a like column of the exterior heated fluid, and therefore stood higher. 2. That the excessive quantity of steam was produced from the thin film of water between the foot of the tube, and bottom of the tin vessel; and that less steam being produced when the mouth of the tube was downwards, and the film less extensive, the internal fluid did not stand so high. 3. That it would have stood higher, if there had been a side aperture; because this would have caused a rapid access of new water to the film, and carried all the bubbles inwards. 4. That upon raising the tube, the film lost its thinness, and the effect ceased. 5. That the effect was less when the exterior fluid was rendered lighter by strong boiling, which filled it with steam-bubbles. 6. And that the cold tube produced no effect, but required to be first heated to the boiling point, before any steam could be produced in contact with it.

V.

On the Combination of Chlorine with Oil of Turpentine. In a Letter from Mr. R. PORRETT, *jun.*

To Mr. Nicholson.

SIR, *Tower, Oct.* 2, 1812.

Reference to Mr. J. Davy's paper. Liq. Libav. and oil of turpentine.

I HAVE just read Mr. John Davy's paper, published on the 1st of last month, in No. 151 of your Journal, on the combination of chlorine; and my attention was particularly engaged by that part of it in which he mentions an experiment of his on the action of Libavius's liquid, or stannanea, on oil of turpentine, and states, that the oil became viscid, less volatile than before; that it had little taste or smell; and that its solution in alcohol, when dropped into water, occasioned a cloudiness in that fluid.

Analogous experiments formerly made by the author.

This experiment of Mr. J. Davy's recalled to my mind some analogous experiments which I made long before his, and in which I formed, as I have some reason for believing, a similar fluid to that obtained by Mr. J. Davy; and as that gentleman expresses a wish, that a subject so curious might engage the attention of chemists, I think this a fit opportunity for requesting your insertion of those experiments in your valuable Journal. Imperfect as they are, they may thus afford some information, and will become more generally known than they can be by the communications of them which I have made in conversation

versation with my chemical friends, and in a lecture on chlorine and muriatic acid, which I delivered to the Mathematical Society on the 22d of February last.

Date of the experiments.

To avoid the suspicion, that any part of what I have now to communicate is borrowed from Mr. J. Davy's paper, I think it best to transcribe, verbatim, my memorandum of the first experiment, in which I combined chlorine with oil of turpentine. It was performed some time between the 24th of July, and 1st of November, 1808, those being the dates of the experiments immediately preceding and following that which I am about to transcribe, and which in my memorandum I had omitted to date. The following is the copy of the memorandum.

" *Experiment with Oil of Turpentine.*

Oxymur. gas and oil of turpentine in the state of vapour. A heavy, thick oil was formed, &c.

"Wishing to ascertain the effect of oxymuriatic gas on oil of turpentine, I caused the vapour of the latter and the oxymuriatic gas to pass together through a glass tube conducting into a glass receiver. There was formed by this process a very heavy white oil, which sunk immediately in water, was as thick as otta of roses; smelt and tasted very much like nutmegs, but communicated rather a more caustic sensation to the tongue. I did not observe any disengagement of gas, arising from the chemical combination that was going on; very little was expelled from the receiver, and that, I believe, was only oxymuriatic. I did not collect and examine it, to ascertain whether any other gas was present, but the smell proved that that gas was evolved; and as it only took place when the extrication of the oxymuriatic acid from the materials that produced it was violent, there is some reason for thinking it was not contaminated with any other gas, which might be supposed to be formed from the oil of turpentine. The change produced on the oil of turpentine in this experiment is very great, from an exceedingly light and thin fluid to a very heavy and viscid one, differing likewise totally in its smell and taste."

Repetition of the experiment.

I have since formed a similar fluid by passing a large quantity of chlorine gas through a small quantity of oil of turpentine. The product of this experiment I exhibited to the Mathematical Society during the lecture before mentioned; and I have a little now remaining in my possession. Its taste is the same as of that obtained in the first experiment; viz. a bitter aromatic, uniting the flavour of hops with that of nutmegs. I have remarked, that the flame arising from its combustion is edged with green.

Compound of the fluid with sulphur.

Still more recently, viz. on the 15th of January last, I obtained a compound of this fluid with sulphur, by acting on oil of turpentine by Dr. Thompson's sulphuretted liquid, or sulphurane. This compound is decomposed by solution of potassa, which dissolves the sulphur, and separates the peculiar fluid, which then exhibits, as far as I have been able to judge, all the characters of that formed by the direct combination of chlorine with oil of turpentine. This experiment, I recollect, appeared to me, at the time that I made it, as very favourable to the opinion formed by Sir Humphrey Davy of the nature of Dr. Thompson's sulphuretted liquid.

Whether this peculiar fluid be the same as that of Mr. John Davy.

I cannot positively say whether the peculiar fluid formed in the three experiments just described is identical with that produced by Mr. John Davy from the mutual action of stannanea and oil of turpentine. Reasoning from analogy would induce me to conclude that it is; but some differences in our descriptions of those fluids oblige me to suspend forming a decided opinion, until the fluids themselves have been compared.

I am, Sir,
Your most obedient Servant,
R. PORRETT, Jun.

VI.

On the Electrical Effects produced by Friction between Bodies. In a Letter from J. A. De Luc, *Esq. F. R. S.*

To William Nicholson, Esq.

SIR,

Reference to former papers.

IN my former papers I have communicated to you my remarks on Dr. Maycock's electrical system; and I come now to his paper in your Journal, No. 144, concerning the *production of electrical excitement by friction*. This paper concludes by the following very judicious remark, which induces me to offer here to him my ideas on the same subject.

" It

Theory of electrical excitement deserves attention.

"It will afford me much pleasure (says Dr. Maycock) "should these observations call the attention of your readers to "the *theory* of *electrical excitement*. I trust that, while we are "successfully employing the powers of electricity in chemical "analysis, we shall not altogether neglect to investigate the "means by which these powers are called forth, and the laws "by which their action is regulated. It has, with much injustice, been objected to *theoretical* pursuits, that they lead "to none of the *practical* advantages, which interest the happiness of society. The remark is indeed true, if applied to "particular discoveries; but these are to be considered only as "the elements from which physical science first took its origin, and by which it is daily nourished and supported. Let "it never be forgotten, that our most perfect instruments, "those which promote no less our comfort, than they tend to "advance our intellectual improvement, are the invaluable "fruits of philosophy." Journ. vol. XXXI, p. 309.

Effects of friction before examined.

1. In quoting this passage with approbation, I cannot, Sir, but express again my regret, that Dr. Maycock appears to have no knowledge of my papers in your Journal; for they might have given him the opportunity of useful examinations between us. For instance, in your No. 126, for January, 1811*, is my paper under the title of *Experiments, showing the effects of Friction between bodies*; which experiments might have afforded him what he wishes to find in your readers, viz. some *remarks* to be compared with his *theory*. But if he reads my present paper, there will be only a little time lost, and the examination may now be effected more directly between us in your Journal.

Dr. Maycock's opinion of these effects.

2. Dr. Maycock's system on the effects of *friction* is derived from his opinion, which, in my former papers, I have proved to be unfounded, viz. that the *electrical* effects produced by the association of *two* proper *metals* appeared only when they came to be *separated*. Had Dr. Maycock known these papers, he certainly would have thought it proper to answer me, before he took his system as a principle in explaining the effects of *friction*, as he does thus in vol. XXXI, p. 305. "It "must be obvious, that, while we are drawing one body over

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* Vol. XXVIII, p. 1.

"another, a number of points in the surface of the *rubber* are "first brought into *contact* with a corresponding set of points "in the surface of the *body rubbed*; that they are then *sepa*"*rated* from them, and brought into *contact* with another set of "points; and so on, until the one body has passed entirely over "the other. Now, at each *separation*, if the bodies be of different "kinds, whether conductors or nonconductors, the general "law, we have stated, must operate, and opposite *electrical* "*states* must be excited in the *separated* particles. So far, "therefore, the *excitement* by *friction*, and the *excitement* by "*contact* and *separation*, appear to be referrible, in a general "manner, to the same principle. We shall now proceed to a "more particular *consideration* of the subject."

This theory difficult to prove.

3. To this *consideration* I shall soon come; but I must first observe, that it would be very difficult to prove that theory by ascertaining the effects of the *friction* in different points of the *rubber* and the *body rubbed*, in order to find out their *progress*. We see, upon the *whole*, that one is become *negative*, when the other is made *positive*; but nothing can indicate whether these effects are produced during the *contact*, or only at the *separation*. Therefore the decision of this point must proceed from other phenomena, and Dr. Maycock affords me an opportunity of discussing this point by the passage which follows that above quoted.

Principal facts relative to excitement by friction, according to him.

"The principal facts (he says) relative to the *excitement* of "bodies by *friction*, may be expressed by the five following "propositions. 1. To produce *excitement* by friction, it "is essentially necessary that one of the bodies employed in "the operation be of the class of *electrics*. 2. If two *electrics*, "or an *electric* and an *insulated conductor* be employed, the one "body will, after the operation, indicate an electricity opposite "to that which is indicated by the other. 3. The effect of "*friction* performed with one combination of *dissimilar* bo"dies is different from that which is produced by any other "combination. 4. The *friction* of two bodies, *similar* in all "respects to one another, produces *no excitement*. 5. If the "*rubber* of an *electrical machine* be insulated, only a very "slight charge can be accumulated in the prime conductor; "and, under such circumstances, the action of the machine "soon ceases altogether."

5. I

Errour in the distinction between electrics and conductors, all bodies conducting more or less: but on some the effects are not propagated.

5. I shall first observe, that, had Dr. Maycock read my paper on the effects of *friction*, to which I shall here refer on many points, he would have seen the errour of the first electricians in their distinction of bodies, which he continues to admit, that of *electrics* opposed to *conductors* ; as if the former only had the faculty to be *electrified* by *friction*. With respect to *electricity*, there is no other distinction than that of more or less *conductors*, which explains all the phenomena. From the property of *absolute nonconductors*, as are *resinous* bodies, whatever change is produced in the *electrical* state of their surface, either by *friction*, or by *communication* with an *electrified* body, it is not *propagated* on them ; and this is their only distinctive property with respect to *electrical* phenomena. The difference, therefore, between these bodies, and the *imperfect nonconductors*, is this ; that the changes produced on some points of the latter, either by *friction*, or by *communication* with an *electrified* body, are propagated on their surface, slowly on some, as *glass*, or almost instantly on the *best conductors*, such as *metals*.

Motion of the electric fluid along different bodies.

6. From this determination of the effects of the different *conducting* faculties of bodies, united with that of the nature of the electric fluid, which Dr. Maycock has not thought necessary to investigate, I derived in the same paper (pp. 3 and 4) the following theory of the effects of *friction*, which is to be compared with the phenomena. "The *electric fluid* resides "on all terrestrial bodies, every particle of *air* included ; being "retained upon them by a mutual attraction, which, however, "differs in degree ; some attract the *electric fluid* only when it "comes into *contact* with them ; but then it *adheres* strongly "to the parts which receive it, or moves but very slowly along "their surface ; which therefore are *nonconductors* : others "receive it at more or less *distance*, and it is *propagated* more "or less rapidly along their surface. *Glass*, though absolutely "*impermeable* to the *electric fluid*, permits it to move with a "sensible progress along its surface."

Theory of the effects of friction.

7. After these definitions of the *nature* of the *electric fluid*, and of its *motions* along different bodies, I thus define the effects of *friction*, connected with these premises. "*Friction* "excited between two bodies, has no other effect than that of "*disturbing* the natural *equilibrium* of the *electric fluid*, which "*equilibrium* tends always to be produced among all bodies, accord-

"according to its actual, but local (in a certain extent) quanti-"ties on them, and in the ambient *air*. If both the bodies which "exercise *friction* on each other are *good conductors*, the *equi-"librium* being constantly restored, this disturbance is not "perceived: but if one has more disposition than the other to "attract the *electric fluid* thus agitated, with the faculty of "transmitting it to its remote parts; when the bodies are "separated, either suddenly, or in general before the *equili-"brium* of the *electric fluid* is restored between them, one is "found *positive*, as having acquired a proportional quantity of "this *fluid*, greater than the *ambient air*, and the other *negative*, "as having lost that quantity." This is the theory of the effects of *friction*, which, in the same paper, I compare with direct experiments: but before I come to that comparison, I must explain the general plan of those experiments, and its motive.

Motive and plan of Mr. De Luc's experiments.

8. The obscurity which reigned on the effects of *friction* proceeded from a circumstance wanting in most of these experiments; they require the *insulation*, not of *one* only of the bodies, but of *both*, either *conductor* or *nonconductor*; else the whole of the reciprocal effect cannot be discovered. I had found this necessity by many experiments made with large bodies, with which I could exactly follow the motions of the *electric fluid*. But I could not suppose it easy for every experimental philosopher to procure this apparatus, which I had partly constructed myself; therefore I attempted to produce a small apparatus, containing in itself all the parts of the large one, which might easily be obtained by every experimental philosopher; and having succeeded, I thus introduced, in the same paper, this new plan of experiments on *friction*. "Mr. "Cavallo has given a table containing the results of his experi-"ments of this kind, wherein is found, that certain bodies "become either *negative* or *positive*, according to those by "which they are *rubbed*. *However, there remained to be known* "*what effect was produced on each of the bodies which exercised* "*that friction*. This has been one of the objects of my expe-"riments; for which purpose I kept *insulated* both bodies, "exercising *friction* on each other, applying *electrometers* to "both."

The apparatus.

9. Then follows, in the same paper, the description of the appa-

apparatus with which these experiments were made: its figure, which is at the head of the paper, is half the size of the apparatus itself; and it may be seen, in that figure, that it is, in fact, a very small *electric machine*, with a *revolving part* and a *rubber*: but it is so constructed, that both these parts may be easily changed, for producing *friction* between different bodies, the effects of which are always shown by the *gold leaf electrometers*. I do not think it necessary to compare directly every part of these experiments with Dr. Maycock's theory; he is so intelligent, that, had he read my paper, he would have found himself those relating to the objects on which we dissent; therefore, I shall only indicate briefly some of these points.

Dr. Maycock's fourth proposition refuted by experiment.

10. The fourth proposition of Dr. Maycock's theory, above quoted, is the following: "The *friction* of two bodies, *similar* "in every respect to one another, produces no *excitement*." This is the immediate consequence of his theory, but is contrary to mine: here, therefore, is afforded a criterion between them; and he might have found the decision in my paper. There, after having explained my theory,—that, in the *friction* between two bodies, which operation agitates the *electric fluid* on their surface, the body which is the most disposed to seize upon that fluid, and to transmit it to its remote parts, becomes *positive*, and the other *negative*,—I added: "This holds, not "only between bodies of *different natures*, but even between "the *same kinds* of bodies, if one be made to pass in *length* "over *one part* only of the other. This effect cannot be ob-"served with perfect *conductors*, as on them the *equilibrium* of "the *electric fluid* is instantly restored; but there is a known "experiment with two pieces of the same *silk riband*, in "which, by making one piece pass rapidly in *length* on *one* "*part* only of the other, the former becomes *positive*, by car-"rying off some *electric fluid* from the latter, which thus is "rendered *negative*, by losing that fluid."

Experiment with silk ribands,

11. These experiments I have repeated many times; by using pieces of wide and strong *silk riband* about a yard long, at the extremities of which were fixed proper pieces of wood, to keep them stretched; one being held very steady, while somebody made the other pass rapidly on one part of the former: then applying each of them instantly to the top of a

gold leaf electrometer, the *riband* which has moved is found *positive*, and the other *negative*. I must observe, that this experiment cannot succeed, but when the air is very *dry*, commonly in winter, at the time that a divergence produced in the *gold leaves* by any cause is long preserved; else the effects produced on the ribands is soon dissipated.

with glass,

12. I have produced the same effect by the *friction* between other bodies absolutely *similar to one another*, namely, *glass* and *glass*; as may be seen in Exp. 3 of the same paper. The *revolving body* was a *glass* cylinder, and the *rubber* a piece of the same *glass*. Now, the *revolving glass*, as the *riband* which passed in *length* over the other, carried off some *electric fluid* from the immovable *rubber*, and immediately transmitted it to the prime conductor of the small machine; so that, at every revolution, the *gold-leaves* connected with it increased in *divergence*, and at last *diverged* much as *positive*.

and with dissimilar substances.

13. All the experiments related in that paper demonstrate the same theory concerning the effects of *friction*; but I shall only indicate them shortly, as the details may be seen in the paper itself. In Exper. 1, a *brass* rubber acting on a *glass* revolving cylinder, the *brass* became *negative*, and the *glass* was made *positive*. This is the same effect produced by a *metallic amalgama* laid on the rubber of the electric machine. In Exper. 4, a *sealing wax* rubber applied on the same revolving *glass* cylinder, the *sealing wax* becomes *negative*, and the *glass* is *positive*. The latter, as being a better *conductor*, carries off a greater part of the *agitated electric fluid*. In Exper. 5 is seen a very singular case. Having used for *rubber* a piece of *India-rubber*, on the same revolving *glass* cylinder, according to the degree of *pressure*, sometimes the *glass* became *positive*, and the *rubber* then was *negative*; at other times the former was *negative*, and the latter *positive*. This case shows, that, between the *same bodies*, when they have a disposition to *adhere* to each other, *friction* may have inverted *electrical* effects, according to the degree, or parts, that the adhesion takes place.

A metal, when insulated, rendered either positive or negative, according to circumstances.

14. I come now to very remarkable changes in the *electrical* effects of *friction*, according to other circumstances. It has been seen above, in Exper. 1, that a *brass* rubber, applied to the revolving *glass* cylinder, became *negative*, and the *glass* was

was made *positive*. But in Exper. 6, the same *brass* rubber being applied to a revolving cylinder of *sealing wax*, the latter was made *negative*, and the *brass* became *positive*. Thus, therefore, *brass*, though the best *conductor* as a *metal*, when it is *insulated*, and thus retains the effect produced on it by *friction*, shows, that it is rendered either *positive* or negative, according to the body which exercises friction upon it.

Sealing wax rendered positive by friction with certain bodies.

15. With respect to *sealing wax*, which is our common test to discover whether our *electroscopes* indicate the *positive* or *negative* state by their *divergence*; because *sealing wax*, when rubbed with the hand, or some cloth, becomes *negative*; exper. 7 proves, that *sealing wax* itself is made *positive* by *friction* with certain bodies. In this experiment, the same revolving cylinder of *sealing wax*, which before was become *negative* by a *brass* rubber, was made strongly *positive* by the *India-rubber*.

Other experiments, showing different effects on the same bodies.

16. Exper. 8 is farther illustrative of these differences of *electrical effects* produced by *friction* on the *same* bodies, according to those which exercise *friction* on them. The object of that experiment is one of the *India-beads*, the size and colour of a cherry, used by Indian women in necklaces or other ornaments, which consist of an inspissated vegetable oil. One of these *beads* I made to revolve by a glass axis, and applied to it successively a *brass* rubber, and a *sealing wax* one: the *brass* rubber rendered it *negative*, and became itself *positive*; but the *sealing wax* rubber made the same bead *positive*, becoming itself *negative*.

Deductions from these experiments.

16. All these experiments prove, first, that the distinction between *electric* and *anelectric* bodies was illusory; that none, in their natural state, are either *positive* or *negative*. With respect to *friction*, these experiments demonstrate, that this operation has no other effect than that of disturbing the *equilibrium* of the *electric fluid* on their surface, one of which, according to circumstances, retains *more* and the other *less* of that *fluid*.

17. If Dr. Maycock happens to see this abstract of the experiments contained in my former papers in your Journal, I think he may find, that every thing belonging to *electrical phænomena* is much clearer than he had imagined: he, however, encouraged natural philosophers to collect all the *known facts* under

under some *theory*, as tending *to advance our intellectual improvement*; and he will now judge whether I have accomplished this purpose.

Supposed obscurity with respect to the action of the galvanic trough.

18. The last part of his paper will lead us to another field, where he finds *much obscurity*, but on which I think light will appear. This part relates to what he calls the *galvanic battery*, saying: "that all the opinions, which have been proposed to "account for it, are unavoidably *hypothetical*, and indeed very "*unsatisfactory*; and that, therefore, every *fact*, which relates "to it, deserves *attention*, although its application may not be "clearly perceived." This gives me hope that he will consider what I shall here explain; expressing, however, again my regret, that he has not known my paper in your Journal on the *galvanic pile*, an apparatus in which the causes and effects may be easily followed; but I hope to make them clear, even in the apparatus of *troughs*, the only one Dr. Maycock seems to have used. I therefore shall copy first what he says of his experiments.

Dr. Maycock's experiments with it.

"I filled one of the new *porcelain troughs* with an *acid fluid*, "so that the *metallic plates*, and their connecting *arcs*, were "completely covered. In this state, a *trough* of 10 *pairs of* "*plates*, 3 inches square, decomposed water very rapidly. "Anxious to know how far the division of the *trough* into "*cells* is requisite, I placed the *metals*, connected by the *bar*, "in a *trough* without *partitions*, and filled it with the same "kind of *acid*,—but no action ensued. The action which "took place in the first experiment appears *inconsistent* with "all our *theories*; and it seems not a little curious, since a "*communication* between the *cells* is not an impediment to "action, that no action was evinced in the second experiment. "It would afford me much pleasure, should these observations "call the attention of your readers to the theory of *electrical* "*excitement*." It has certainly been the case with me, and I shall now explain how I find his experiments *consistent* with each other, and also with my theory.

Attempt to explain the apparent inconsistency in them.

19. In the first of these experiments, the *trough* with *partitions* produced a series of *ten* distinct *pairs* of the *two metals*, which, being formed of plates 3 *inches square*, were sufficient to produce the effect described; as the *liquid* was a *conductor*, which transmitted undisturbed the effect of each *pair* to the next

next on both sides; as does the *wet cloth* in the *galvanic pile*. But when the *plates* were entirely *immersed* up to the *bars* in the *liquid*, the latter being a *conductor* which embraced the whole, every difference between the *metals* in each intermediary *pair* was destroyed, and the effect was reduced to that of *one* single *pair*.

Apparatus to imitate the phænomena of the electric eel.

20. This will be shown by an analogous experiment, which, for another purpose, I made some years ago at Berlin, related in p. 253 of the 2d vol. of a work under the title of *Traité élémentaire sur le Fluide électro-galvanique*, published at Paris, in 1804. I had then in view the phænomenon of the *electric eel*; that fish which produces the *shock* while in *water*. I tried to imitate that *eel* by a *galvanic pile*, composed of 30 groups of *zinc* and *silver*, separated by pieces of cloth imbued with salt water. These groups were held together by 3 glass rods, so kept together as to leave no projection outwards, and resembling so far an *electric eel*. With this pile I made the following experiments:—1. It being held upright, I received a strong *shock* from it: having applied to it the usual *glass tube with water*, the *gasses* were produced in that tube. 2. I laid the pile on my table; it continued to produce the *shock*. 3. I laid it in a narrow *wooden trough*, with a little *water* at the bottom; the *shock* was less. 4. I poured successively more *water* into the *trough*: in proportion as the *water* rose round the *pile*, the *shock* was less; and at last, when the *water* covered it entirely, not only there was no more *shock*, but, having applied between its extremities a *glass tube with water*, no *gas* was produced. The *electrical eel*, therefore, has no perceptible analogy with the *galvanic pile*, though the effects are similar.

Experiments with it.

This, I think, will show Dr. Maycock the manner in which his two experiments are *reconciled* with each other, and are *consistent* with my *theory*. It will also give me much pleasure, sir, if Dr. Maycock, finding any objections to my explanation, will transmit them to me through your valuable Journal; for I have a great regard for him, though not personally acquainted with him.

I have the honour to be,

Sir,

Your obedient, humble Servant,

J. A. DE LUC.

Windsor, October the 5th, 1812.

VII.

METEOROLOGICAL JOURNAL,

		BAROMETER.			THERMOMETER.					
1812.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain.	
8th Mo.										
AUG. 29	N W	29·99	29·98	29·985	54	52	53·0	—	·16	☽
30	N E	30·04	29·99	30 015	61	53	57·0	—	·14	
31	N	30·15	30·04	30·095	60	48	54·0	—		
9th Mo.										
SEP. 1	N E	30·18	30·15	30·165	59	48	53·5	—		
2	S E	30·15	30·10	30·125	62	54	58·0	—		
3	S W	30·10	29·95	30·025	62	53	57·5	—		
4	S E	29·95	29·89	29·920	63	45	54 0	—		
5	N E	29·96	29·89	29·925	65	46	55·5	·28		●
6	N E	30·07	29·96	30·015	67	40	53·5	—		
7	S E	30·09	30·07	30·080	67	41	54·0	—		
8	E	30·07	29·97	30·020	69	50	59·5	—		
9	S E	29·99	29 97	29·980	68	47	57·5	·41		
10	S	30·17	29·95	30·060	67	47	57·0	—		
11	N W	30·28	30·17	30·225	68	42	55·0	—		
12	W	30·26	30·22	30·240	69	44	56·5	—		
13	Var.	30·18	30·14	30·160	71	42	56·5	—		☾
14	Var.	30·18	30·07	30·125	69	40	54·5	·53		
15	N W	30·07	29·94	30·005	68	41	54·5	—		
16	S W	29·94	29·82	29·880	72	47	59·5	—		
17	Var.	29·96	29·82	29·890	62	41	51·5	—	·15	
18	N W	30·20	29·96	30·080	60	37	48·5	—		
19	S W	30·14	30·12	30·130	60	46	53·0	·48		
20	S W	30·12	30·03	30·075	69	45	57·0	—		○
21	W	30·03	29·97	30·000	73	47	60 0	—		
22	S W	29·95	29·91	29·930	68	50	59·0	—	·34	
23	S W	29·95	29·93	29·940	57	44	50·5	·57		
24	N W	30·06	29·95	30·005	56	34	45·0	—		
25	Var.	30·04	29·99	30·015	60	40	50·0	—		
26	W	30·07	30·04	30·055	63	53	58·0	·23		
		30·28	29·82	30·040	73	34	54·93	2·50	·79	

The observations in each line of the table apply to a period of twenty-four hours beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

REMARKS.

Eighth Month. 30. Very showery. Between 4 and 5 p. m. a sudden *tornado* (as it seems by the description given) crossed the village in a direction from N. E to S. W. which left behind it considerable traces of its violence : a large quantity of wheat in sheaves was carried over a hedge into a neighbouring field : a fence was levelled, and about seventy *oak hurdles* torn out of the ground, some of which were seen tumbling over in the air, and fell at two hundred yards distance.

Ninth Month. 12. Misty morning : much dew. 13, 14. The same in the evening, a dense *stratus* reflecting on its surface with much brilliancy the orange colour of the western sky. 15. Hoar frost in the pastures : a *stratus* at night as before, the wind coming about to the eastward soon after it was formed. 16. Cirrus with cirro-stratus and cumulus. 17. Rain most of the afternoon; a rich crimson tinge on the lower surface of the clouds at sunset. 18. At sunset the sky was extensively coloured with orange, surmounted by a distinct blush of red : the colour was reflected in the E. horizon. 19. a. m. much hoar frost. 22. a. m. clear at first, but soon overcast, with rain· 25. Hoar frost. 26. Cirro-stratus.

RESULTS.

Easterly winds prevailed the fore part, and westerly the latter part, of this period.

Barometer : highest observation 30·28 inches ; lowest 29·82 inches,

Mean of the period 30·040 inches.

Thermometer : highest observation 73° ; lowest 34°.

Mean of the period 54·93°.

Evaporation 2·50 inches. Rain 0·79 inches.

Plaistow. L. HOWARD.
Tenth Month, 8, 1812.

VIII.

On the primitive Crystals of Carbonate of Lime, Bitter-Spar, and Iron Spar. *By* WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S*[*].

Angles of crystals corrected by the author's goniometer.

WHEN I formerly described to the Society a goniometer[†] upon a new construction for measuring the angles of crystals, I expressed an expectation, that we should thereby be enabled to correct former observations made by means of less accurate instruments. I took occasion to mention one instance of inaccurate measurement in the primitive angle of the common carbonate of lime; and I have had the satisfaction to find the necessity of a correction, in that instance, confirmed by Mons. Malus, and admitted by the Abbé Haüy, in a work[‡] published nearly at the same time.

Haüy's accuracy in general surprising.

It is by no means my design to detract, in any degree, from the merit of that justly celebrated crystallographer, to the surprising accuracy of whose measurements I could, in various instances, bear testimony. I hope, on the contrary, that in bringing forward two more observations similar to the preceding, and intimately connected with it, I shall offer what will not only appear interesting to crystallographers in general, but will be peculiarly gratifying to the Abbé Haüy.

The same primitive form assigned by him to three substances:

In his Traité de Minéralogie, and again more recently in his Tableau Comparatif, the same primitive form is assigned to three substances very different in their composition to carbonate of lime, to magnesian carbonate of lime (or bitter-spar) and to carbonate of iron.

Identity of composition should accompany identity of figure.

It has been objected to Mons. Haüy, that according to his method identity of form should be accompanied by identity of composition, unless the form were one of the common regular solids. For though in this case any geometrician would readily admit it to be very probable, that many different substances might occur in assuming the same form of cube, of octo-

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[*] Philos. Trans. for 1812, p. 159.

[†] Philos. Trans. 1809, p. 253; or Journal, vol. XXV, p. 192.

[‡] Tableau Comparatif des Résultats de la Crystallographie et de l'Analyse Chimique.

hedron,

hedron, or of dodecahedron, &c.; there does not appear a corresponding probability, that any two dissimilar substances would assume the same form of a particular rhomboid of 105° and a few minutes, to which no such geometric regularity, or peculiar simplicity, can be ascribed.

The forms of the three carbonates similar, but not identical.

But though so accurate a correspondence, as has been hitherto supposed to exist in the measures of the three carbonates above-mentioned, might be justly considered as highly improbable, no degree of improbability whatever attaches to the supposition, that their angles approach each other by some difference, so small as hitherto to have escaped detection. And this, in fact, I find to be the case.

Least measure of an angle usually employed by the author.

Since the angles observable in *fractures* of crystalline substances are subject to vary a little at different surfaces, and even in different parts of the same surface (as is evident from the confused image seen by reflection from them) I shall not at present undertake to determine the angles of these bodies to less than five minutes of a degree. This, indeed, is the smallest division of the goniometer that I usually employ, as I purposely decline giving so much time to these inquiries, as would be requisite for attempting to arrive at greater precision.

Primitive angle of carbonate of lime,

The most accurate determination of the angle of carbonate of lime is probably that of Mons. Malus, who measured it by means of a repeating circle, and found it to be 105° 5′. And this, indeed, is the result to which I formerly came by a different method*. If it differ in any respect from this quantity, I am inclined to think that it will more likely be found to be deficient by a few minutes, than to exceed the measure here assigned; and accordingly to differ still more widely from those angles which I am about to mention.

of bitter-spar,

In the magnesian carbonate of lime, or bitter-spar, the primitive form is well known to be a regular rhomboid, as well as that of carbonate of lime, and so nearly resembling it, as to have been hitherto supposed the same. I find, however, a difference of 1° 10′ in the measures of these crystals; for that of the magnesian carbonate is full $106\frac{1}{4}$°, as I have observed with uniformity, in at least five different specimens of this substance, obtained from situations very distant from each other.

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* Phil. Trans. 1802, p. 385; or Journal, vol. IV, p. 150.

and of iron-spar.

The primitive angle of iron-spar is still more remote from that of the carbonate of lime, which it exceeds by nearly two degrees. I have examined various specimens of this substance, some pure white, others brown, some transparent, others opake. That which gives the most distinct image by reflection is of a brownish hue, with the semitransparency of horn. It was obtained from a tin mine, called Maudlin Mine, near Lostwithiel, in Cornwall. By repeated measurement of small fragments of this specimen, the angle appears to be so nearly 107°, that I cannot form any judgment whether in perfect crystals it will prove to be greater or less than that angle.

In this instance the carbonate of iron is nearly pure, and so perfectly free from carbonate of lime, as to render it highly probable, that in other specimens, having the same angle, but containing also carbonate of lime, or other ingredients intermixed, the form is really dependent on the carbonate of iron alone.

Pearl spar. Mixtures probably most liable to curved surfaces.

It appears, however, not unlikely, that when substances, which agree so nearly in their primitive angle, are intermixed in certain proportions, they may each exert their power; and may occasion that confused appearance of crystallization with curved surfaces, known by the name of pearl-spar. I cannot say that I have made any accurate comparative analyses, which may be adduced in support of the hypothesis, that mixtures are more subject to curvature than pure chemical compounds; but it is very evident, from the numerous analyses that have been made of iron-spar by other chemists, how extremely variable they are in their composition, and consequently how probable it is, that the greater part of them are to be regarded as mixtures; although it be also possible, that there may exist a triple carbonate of lime and iron as a strict chemical compound.

Perhaps some carbonate, modified by manganese, exists.

It seems not unlikely, that there may hereafter be found some carbonate allied to the preceding, which may owe its form to the presence of manganese; but notwithstanding the liberality which happily prevails in general among those who have it in their power to assist in such inquiries, I have not had the good fortune to meet with any such compound; and I am unwilling, merely in the hope of making such an addition,

tion, any longer to defer communicating an observation, which I hope will be of real utility in the discrimination of bodies that differ so essentially in their composition.

IX.

Account of an Economical Lamp for producing Heat, with a considerable saving of Oil. In a Letter from a Correspondent, L. O. C.

To Mr. Nicholson.

SIR,

IF you should not think the following experiments too trifling for insertion in your excellent Journal, it is probable, that such of your readers as are in the habit of using an Argand lamp in their chemical researches may feel interested in them. They certainly have some claim to attention in an economical point of view: and this, I think, no one will deny, when he is informed, that he may procure a lamp at one tenth of the price of the cheapest Argand lamp; which will produce an equal, if not a greater degree of heat; and effect a saving of at least one third of the quantity of oil consumed.

Introduction. Economical lamp costs one tenth of an Argand lamp, and saves one third of the oil.

Experiment 1. a. An Argand lamp, which is supplied by a fountain, so that the oil can never become very hot, was made to burn as bright and strong as possible, without smoke; and was found to consume 447 grs. of oil, in an hour. Two thousand grains of rain water were exposed to the heat of this lamp in a glass matrass; they boiled in seven minutes.

Exp. 1. Argand's lamp consumed 447 grs. of oil per hour, and boiled 2000 grs. of water in 7 m.

b. As I suspected that the lamp did not produce its greatest effect, on account of the wick having been in the oil for a length of time, I caused a new one to be put to it, and every precaution was taken to make it burn as powerfully as possible. It now consumed 500 grs. of oil in the hour, and made 2000 grs. of water to boil in 6$\frac{1}{3}$ m.

Same lamp, newly trimmed and filled, consumed 500 grs. per hour, and boiled the water in 6$\frac{1}{3}$ min.

Exp. 2. I caused a small tin lamp to be made, with four burners; and having a tube in the centre, to convey air to them; each of the burners containing eight threads of cotton, of the diameter of $\frac{1}{46}$ of an inch. This lamp, when burning as strongly as possible without smoke, consumed in one hour 200 grs. of oil, and made 2000 grs. of water boil in 10 m. (See plate 5, fig. 3 and 4; and the description at the end of this letter.)

Exp. 2. A lamp with 4 separate small wicks, &c, consumed only 200 grs. of oil per hour, and boiled the water in ten min.

It saved more than one third of the oil.

We may here observe, that the Argand lamp expends, in raising 2000 grs. of water to the boiling point, 52·1 grs. of oil; and that the lamp just described requires only 33·3 grs. to produce the same effect, which is a saving of 18·8 grs. or more than one third.

Exp. 3. Another lamp, but with the same cotton, in 8 separate wicks, consumed 300 grs. of oil per hour, and boiled the water in 6½ min.

Exp. 3. Although the lamp described in the last experiment did not produce an effect nearly equal to an Argand lamp, in a given time; yet I could clearly perceive that the principle upon which it was constructed was good, and capable of being carried to a much greater extent. I therefore ordered another to be made, in every respect similar to the former; except in the number of burners, which were increased to eight, each of which contained four threads; so that the quantity of cotton in the wick of each lamp was exactly the same. This lamp consumed 300 grs. of oil in an hour, and made 2000 grs. of water boil in 6½ m. Its power, therefore, was equal to that of an Argand lamp; and if we take the quantity of oil consumed by an Argand lamp in heating to ebullition 2000 grs. of water in 6½ m. at 45 grs. (which is evidently too small a quantity) and that consumed by this lamp, in producing the same effect, at 30 grs. the quantity required by the two lamps to produce a given effect, in a given time, will be in the ratio of 2 to 3.

Its effect was therefore equal to that of the Argand lamp; but one third of the oil was saved.

Exp. 4. smaller wicks consumed more oil, without increasing the effect.

Exp. 4. a. The same lamp was used in this experiment, as in the preceding; but as four threads appeared to fill the tubes too tight to admit the free ascent of the oil, only three were put into each of them; and a glass, 1½ in. in height, and 2 in. in diameter, was placed over the flame, as in an Argand lamp. When thus adjusted it consumed 320 grs. of oil in an hour; and caused 2000 grs. of water to boil in 6½ m.

When the chimney glass was shortened, the effect was increased:

b. As the effect appeared to be somewhat diminished by the elevation of the matrass to too great a distance from the flame of the lamp, this glass was removed, and another, only 1 in. in height, substituted in its stead. The flame did not appear to be as vehement now as before: it nevertheless produced a greater effect, which was owing, no doubt, to the bottom of the matrass being nearly in contact with it. Only 276 grs. of oil were consumed in the hour; and 2000 grs. of water boiled strongly in 6¼ m.

and the saving nearly one half.

This appears to be the preferable mode of using the lamp, as it thus produces a greater degree of heat than an Argand lamp, even

even under the most favourable circumstances; and consumes, in the same time, 224 grs. less of oil, which is a saving of about $\frac{9}{20}$, or nearly half. If we calculate the quantity required by each to produce the same effect, it will be still more in favour of mine.

Exp. 5. a. As the advantages arising from bringing the substance to be heated, as near as possible to the flame of the lamp, appeared to be so considerable, it was natural to conclude, that similar effects would attend the shortening of the glass of the Argand lamp, or the removing it altogether. Accordingly, 2000 grs. of water were exposed to the heat of this lamp, when burning as clear as possible, without a glass. The water did not boil in less than 7¾ min. This, therefore, did not appear to be any improvement.

Exp 5. The effect of Argand's lamp is less when the glass is taken away.

b. I added to the Argand lamp a glass 2 in. in diameter, and 1½ in. in height, from the base of the flame. Its power was considerably increased, for it made 2000 grs. of water boil in 6⅓ m. and consumed only 400 grs. of oil per hour.

But a short glass of 1½ inch long, saves one fifth of the oil in the A. lamp.

Those who do not think it worth their while to procure a lamp similar to that which I have recommended, will at least find it a considerable improvement, to reduce the glasses of their Argand lamps, to the dimensions here specified, which will cause a saving of one fifth of the quantity of oil at present consumed.

Exp. 6. One of my principal objects, in the construction of this lamp, was, that I might be able always to procure an equal degree of heat; and one that could be augmented or diminished uniformly. The following experiments were instituted with a view to determine, whether it possessed these properties or not. I have arranged them into a table;

Exp. 6. The heat of the new lamp is steady, and is proportioned to the number of burners.

	m.	m.
8	3½	
7	4	4
6	4¾ hardly	4⅔
5	6 nearly	5·6
4	6¾	7
3	9½	9⅓
2	15	14
1	30	28

the first column of which indicates the number of burners employed; the second, the time required to raise 1000 grs. of rain water to the boiling point; and the third, the time required by calculation to produce the same effect; taking the first experiment for the standard, viz. that with eight burners, the lamp will make 1000 grs. of water boil in 3½ m., which is very nearly half the time which it requires to make 2000 grs. boil.

The real times are copied exactly from the memoranda I made while performing the experiments; and I was much surprised to find how nearly they coincided with the times by calculation: especially since the watch used had no seconds hand. In the last experiment but one the water never boiled strongly; and in the last it only just simmered. I have added a sketch and description of the lamp, for the use of those who may wish to procure one like it. Its dimensions are exactly double those of the drawing; with the assistance of which any tinman will be able to make it; and I should recommend it to be made with a reservoir, to prevent the oil from being much heated, as I have found by experiment, that this circumstance considerably increases the quantity of oil consumed in a given time, without adding, in the least degree, to the heat of the lamp. The price of one, similar to the drawing, will not exceed 1s. 6d. exclusive of the glass, which is about one tenth of the price of the commonest Argand lamp that I have seen.

Hot oil is consumed faster without augmenting the effect. Hence a reservoir is advisable.

I am, Sir,
Your constant Reader,
L. O. C.

Description pl. V, fig. 3, 4. A the body of the lamp; B. B. the burners; C. tube for conveying air to the flame; D. D. D. small feet which raise the body of the lamp, and allow the free access of air to the tube C.; E. E. bended wires, which support the glass F.; G. small tube for filling or emptying the lamp*.

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* It would afford an acceptable result, if the writer would also measure the light afforded by the lamps under comparison, as well as the heat. As the method of shadows, though very simple and easy, is certainly not in general use, I would here repeat, that when the shadows of the same object, projected upon a wall or surface by two lights, are equally dark, the lights themselves are equally intense—that, if not, the darkest shadow will be projected by the interception of the brightest of the lights; and that, if this brightest light be then removed farther from the wall, till both shadows become equally dark, and the distances of the lights from the wall be in that situation measured, the intensity of each

X.

Description of a new Construction and Arrangement of the Keys of Musical Instruments, invented by JOHN TROTTER, *Esq.* (W. N.)

General account of keyed instruments.

IN the piano forte and other instruments, in which the notes of the musical or diatonic scale are afforded by wires, strings, pipes, or other parts of invariable figures, lengths, or other affections, and produced by the action of keys,-it is well known, that one principal series has its notes designated by the first seven letters of the alphabet, A, B, C, D, E, F, and G; and that the intervals of gravity or acuteness are all (nearly) equal to each other, except the interval between B and C, and that between E and F, which are (nearly) half the magnitude of the others; and that by interposing other notes at (nearly) equal distance between each of the greater intervals, a series of such smaller intervals (called the intervals of semitones) is constituted, and affords the number of twelve notes in all; out of which it is practicable, by commencing from any individual note, to take a regular diatonic series, having its notes and half notes disposed in a similar manner to those of the principal series before mentioned. The present occasion does not require any illustration of the nature of the musical scale, or the degrees of imperfection to which the secondary scales of fixed instruments are unavoidably subject, and the remedy usually afforded in part by temperament. Practical musicians are aware, that the white keys of a piano forte, and the lines and spaces of a music book, are appropriated to the notes of the principal series, called the natural scale; and that the auxiliary half notes are given by black keys on the instrument, and marked in the book by the same character as denotes the note next ascending or descending, but have the modifying character of flat or sharp prefixed in the cliff or elsewhere. That this mode of notation is unscientific and awkward; and that the correspondent structure of the keys is irregular and embarrassing, will scarcely be controverted by any one who shall maturely consider the subject. In the natural

The principal series, or natural notes expressed by simple characters, and afforded by white keys.

Notation by flats and sharps condemned.

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each will be in proportion to the square of its distance. For example; if two lights give shadows equally black or dark, when their distances from the wall or surface are respectively five and seven feet, the intensity, or quantity of light emitted from them, will be respectively as 25 (or 5×5) and 49 (or 7×7).—N.

series, every character simply indicates its note; but if we take the fundamental note one tone higher, viz. from C to D, then two sharps, namely *f* and *c*, will be marked in the cliff; and the same characters, in effect, will no longer indicate the same notes, or require the same keys to be touched. If we assume the fundamental two tones above C, four sharps will be marked in the cliff, &c. And it is accordingly found, that in transposing and performing on the different keys, the fingering is very different in each. Mr. Trotter has found a simple and effectual remedy for this last difficulty, by constructing the keys in such a manner that no preference as to white keys is given to any particular series; but every series is fingered precisely in the same manner. He places the white and black keys alternately in succession for every one of the twelve semitones; and the consequent rule is, that for the sharp series ascending from any note whatever, the key note itself is to be touched, and then the two keys next ascending of the same colour, and these are to be followed by the four keys next ascending of the opposite colour. Thus in fig. 5, pl. V, the key of the Note C being black is to be followed by the black keys of D and E, and afterwards by the four white keys of F, G, A, and B. And it is easy to show, that in the flat series a rule no less simple and universal prevails throughout, viz. that the key note, and the next above, and two next below it, are to be of the same colour, and the other three of the opposite colour.

Mr. Trotter places a black and a white key in regular succession for all the twelve semitones; which affords a simple and invariable rule for performing in every key of music.

Illustration by the engraving.

In the same figure, where W, W, &c. represent the white keys, and N, N, &c. the black keys as usual, the letters *n*, *n*, represent the same black keys continued, and a little depressed below the white ones. By this means the performer has his election to touch either N or n of any key at pleasure; and as the nature of the passage may require. This construction is shown in profile at fig. 6.

Performance on the instrument by a professional man.

I was present when a professional gentleman, who had practised on this improved instrument two days, performed several pieces of music upon it, which were of difficult and rapid execution. He spoke with much approbation of the facility and advantages it affords, and particularly remarked, that the improvement *n*, *n*, &c. allows certain passages to be performed in a superior manner, which, if fingered by means of the faces

N. N.

N. N. &c. would be much less manageable. Not being myself a performer on any keyed instrument, I can only express my approbation in general terms. Letters patent have been granted for the invention.

XI.

A new Compensation Pendulum, without Joints or Surfaces bearing against, or moving upon each other. In a Letter from a Correspondent. (R. B.)

To Mr. Nicholson.

SIR,

Pendulums of compensation.

SINCE the invention of pendulums of compensation for changes of temperature, there nave been a number of excellent contrivances, by means of the contrary expansions of metallic bodies, for keeping the centres of oscillation and suspension at an invariable distance from each other. In the quicksilver pendulums, or any of the contrivances which require the use of a fluid, it may reasonably be supposed, that the changes, which are to compensate each other, do take place at the same time as the temperature becomes altered; and that the quantity and effect of the compensation continue the same for any length of time, during the existence of the machinery. But in pendulums composed altogether of solid materials, both these results have been called in question. In the gridiron pendulum, it has been insisted, that the adjustment of the expansions is by the construction of holes and pins, not capable of extreme exactness; and that the variation, to which the pressure of contact may be liable, and the stickage of the bars, (which rub against the frame) may be more than sufficient to counteract and give uncertainty to all small variations, required to be compensated. And in all constructions of bars of steel and brass, soldered or connected lengthwise, it has been doubted—not only whether the wire-drawing and upsetting of the elastic metals themselves, under a state of such severe force, may not allow for all small changes, without perceptible flexure,—but likewise whether the flexure, which in larger changes is perceived, may not become altered, after a course of time, and long exposure to the effects of heat and cold. Perhaps, Mr. Nicholson, you, or your readers, may be inclined to

Mercurial.

Gridiron pendulum objected to.

Expansion bars supposed to alter by the forcible action of their parts.

to think these speculations the mere creatures of theory; and may, from the actual performance of many surprising time-measurers, be disposed to conclude, that they may be practically of no consequence. But I would urge, Sir, that it is by this kind of minute and scrupulous investigation, that discoveries come to be made; and that, while the best makers hold different opinions, and with all their skill (which I truly honour) cannot foretel, beyond a certain point, whether the work they are upon will answer their expectation or not,—we may actually expect still to make discoveries in this useful and curious pursuit. And while we make our experiments and operations under the guidance of some probable hypothesis, or doubt, or indication, the objects we produce may, in many instances, be found to possess unforeseen advantages, or serve to establish new truths, perhaps not suspected by the operator himself.

Disquisitions upon minute changes in bodies useful.

New pendulum.

I have been desirous of constructing a pendulum, composed of expansion bars, not subject to the violence of soldered or adherent faces pulling against each other, or the uncertainty of contacts, the rub of surfaces, the stickage of joints, or the bearing parts of levers. The following has been going for several years.

Construction. Two pair of strait bars, each of brass without, and steel within, are rivetted together, and each pair to the other at one end only. Flexure allowed at certain thin parts, &c.

In the annexed drawing, (Fig. 2, plate v.) f g and i h represent bars of brass, respectively united by rivetting and soldering with two bars of steel at the ends where they touch, but their faces do not touch except near the ends, as shown in the drawing.

At the extremity g l n i of the system, all the four bars are connected, as is also shown in the cross section p; but at the other extremity the bars f k are connected, and so likewise are the bars m h; but there is no union between k and m. At o, o, the bars are filed away or notched, so as to leave each of them thin on the side farthest from the middle line of the system. a d shows part of the pendulum rod, of which a is the point of suspension, connected by a spring with b, a clamp adjustable by sliding along the face of f g, and fixable in any required position by the screw c. And d is part of the lower rod, broken off to save room in the plate instead of being continued down to the ball.

Particular description of the

The effect of this combination may be thus explained: When by an increase of temperature the brass bars f g and i h become longer effect upon change of temperature.

longer than their correspondent steel bars k l and m n, the whole of the flexure will take place without any strain or tension of any practical importance, in the thin parts at o, o, and the faces k m will be brought nearer together, and so likewise will a and d ; by which means the pendulum rod will be shortened, and this shortening may, by the due adjustment of the clamps, be made precisely to compensate for the lengthening of the rod, caused by its direct expansion by the same increase of temperature.

Hardy's balance.

I believe the happy expedient of confining the flexure to a thin part of the bar was first used by Mr. Hardy, in his expansion balance.

Principles of computation, for the quantity of effect.

In order to show the quantity of effect, let fg, kl (Fig. 2*) represent one pair of the bars, of which og, cl are the thin parts, and co the depth of the notch ; and suppose oe to be the effect of the expansion by one degree of heat in the thin brass part beyond that of the steel bar; which will be 0·00000331 in unity.

Or, go being equal to co, the quantity eo will be= 0·00000331 × depth of notch.

But the whole excess of expansion will be greater in proportion as the whole bar is longer than co, or the depth of notch, that is, 0·00000331 × length of bar = whole excess of expansion.

And as this excess will cause an angular deviation in the line co to ce and beyond, and will also cause a similar deviation in gf, the linear deviation of the extremity f will be greater than that of o, in proportion as the whole bar is longer than co, or the depth of notch ; that is, depth of notch : length of bar :: excess of expansion = 0·00000331 × length of bar : linear

$$\text{deviation of } f = \frac{\text{length of bar}^2 \times 0{\cdot}00000331.}{\text{depth of notch.}}$$

In order to adjust the compensation, the effective length of the bar is variable by means of the clamps, and the deviation of the parts of the pendulum rod, above and below the set of bars from a precise right line, would not exceed ten minutes, if the parts were inflexible, and the bars very short ; but the spring of suspension is practically sufficient to preserve the right line.

XII.

Abstract of an Essay on the Construction and Effects of the Pneumatic Tinderbox, by Le BOUVIER DESMORTIERS*.

Production of fire by compression.

THE inflammation of spunk in the pneumatic tinderbox, by the compression of air alone, is a phenomenon, with which chance, the father of discovery, has lately enriched natural philosophy. Many have reasoned on its cause; which some consider to be caloric, others electricity; but no one, that I know of, has attempted to support his opinion by experiments†. Without bias for any hypothesis, I have made some researches on the construction and effects of the pneumatic tinderbox, the results of which shall be the subject of the present paper. In the first part, I shall consider what relates to the structure of the instrument; in the second, I shall give an account of the experiments, that tend to the discovery of the cause of its effects.

The piston generally too long.

I. The first construction of these tinderboxes was a little faulty in the piston being commonly eighteen or twenty lines long. This was said to be necessary, that the air might not escape, when the piston was in action; for, if there were any point not accurately fitted to the inside of the tube, the air escapes, and the spunk does not kindle.

Half an inch, with a tube of six inches, sufficient.

The goodness of the instrument does not depend on the length of the piston, but on the accuracy with which it fills the bore of the tube; with a tube well bored and a piston of six lines, the air will no more pass than with a piston of twenty. Accordingly, for a tube of six inches I have reduced the piston to six lines, which adds an inch to the column of air, and diminishes the friction two thirds, so that the effect of the tinderbox is more certain, and it is more easily used. With a little dexterity you may kindle the spunk by holding the tube in one hand and pushing the piston with the other, without being obliged to rest it on a table, or any other solid body. Mr. Dumotiez, a skilful maker of philosophical instruments, is so fully convinced of the advantage of short pistons, that he now makes them of these dimensions.

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* Journal de Physique, Vol. LXVII, p. 125.

† See Journal, Vol. XX, p. 278; and Vol. XXI, p. 234.

They

Short pistons of advantage in other machines.

They should be employed also in the syringes of air guns*, of fountains acting by compressed air, of the apparatus for artificial mineral waters, of fire-engines, which are worked with so much labour, and even of air-pumps. As the shortening the piston is an advantage to the pump, we obtain a greater effect with less labour, and in a shorter time, than with long pistons.

The chamber must be air-tight, but not essential for the piston to be so.

It is essential too, that the instrument does not leak at the part where the spunk is placed, because there the transient action of inflammation takes place, and a slight emission of air would prevent the effect. But this effect is produced, though the piston does suffer the air in the tube to pass it. To satisfy myself of this, I made the following experiment, at which they who have seen it were greatly surprised.

Four grooves made in the piston did not prevent the effect,

In the length of the piston I made a groove a quarter of a line broad. The spunk took fire as before. Three other grooves were added successively opposite one another, so as to divide the piston into four equal parts; and still the spunk took fire†. When the grooved piston is moved backwards and forwards in the tube, the air may be heard entering or issuing out; and the friction is so slight, that the effect of the instrument is easily obtained by pushing it with the hand. This kind of piston would be preferable to those that fit accurately, if a solid substance were employed, hard enough to resist the continual friction of the air passing through the grooves, if I may be allowed the expression. The grooves in leather pistons soon alter their shape, and spread so as to allow the air to pass in too large quantity.

but one of larger dimensions did.

The piston with four grooves acting very well, I made one with a single groove, of dimensions equal to the other four, and what I foresaw actually took place: there was no inflammation. The following are the reasons of this difference.

Why the small grooves do not prevent the action,

The extremity of the grooved piston exhibits the area of a circle, the peirphery of which touches the interior edge of the grooves. The column of air contained in the tube rests almost wholly on this base. There are only the parts corresponding to

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* In the air-guns of Germany, which are the best we know, the piston of the syringe is extremely short.

† I tried this experiment with Mr. Bancks, at his house in the Strand, and we found it succeed completely with a common condensing syringe of his making.—C.

the grooves, that are continued through the length of the piston, and communicate with the external air. When the piston is pushed with sufficient velocity to kindle the spunk, the parts of the column corresponding to the grooves rush into them with equal velocity; but the friction they experience in passing through such narrow tubes occasions a resistance to their passage, a kind of choaking, that suffers only a part to escape, while the column resting on the area of the piston is pushed entirely toward the extremity of the tube, where the spunk to be kindled lies.

and the large one does.

In the piston with a single broad groove, the area of the circle, on which the column of air rests, is much smaller, consequently the column itself is less. The resistance the air experiences in passing through the groove is next to nothing; for we hear no noise on moving the piston backward and forward; and as air expands in all directions, when the piston is moved, the column resting on the area of the circle, resting at the same time laterally on that which answers to the groove, it recedes from all the points of contact, and flows entirely through the channel it finds open. It is so true, that it wholly flows out, that the piston, when it touches the extremity of the tube, remains there; while with other pistons a sufficient quantity of air is retained to occasion a spring and repel them.

Choice of the touchwood.

I think it proper to say a word or two on the quality of the spunk. The driest, softest, and least impregnated with nitre, should be chosen. In that of the best quality a piece will not always be found equally good throughout. Some contains a great deal of nitre, and is kindled with more difficulty*. This may be known by the cool taste it leaves on the tongue; or by kindling it: for when it has taken fire the nitre melts, and sometimes throws out sparks, that may be dangerous when they spirt out of the instrument, particularly if made with a cock. As it is usual to blow on the spunk, to try whether it be kindled, a spark may be thrown from it into the eye. This painful accident once happened to me.

Danger of blowing on it.

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* Spunk is prepared from agaric, which is first boiled in water; beaten well when dry; steeped in a strong solution of saltpetre; and lastly dried in an oven. If the solution of nitre be too strong, the agaric is loaded with this salt, which retards its inflammation.

They who imagine, that electricity kindles the spunk, consider these sparks as an incontrovertible proof of their opinion. I think they are mistaken in this case; yet I must not conceal a fact communicated to me by Mr. Veau-Delaunay, which seems to confirm this opinion, of which he is a partisan. Out of twelve times, when he operated with the instrument without any spunk in it, he saw sparks emitted three times. There are strong reasons, however, for suspecting, that electricity is not the cause of the inflammation here. These I shall give in the second part of this paper, concluding the present with an important observation on the construction of pistons.

Electricity supposed to be the cause of the fire.

If we could find an elastic substance sufficiently compact to be turned in a lathe, we should have perfect pistons, that would spring and adapt themselves to the inequalities of the tube, without suffering a bubble of air to escape. I have made some with caoutchouc, softened before the fire, in order to give it a degree of elasticity more obedient to the inequalities of the tube. But on attempting to turn it in a lathe, it bent under the tool. Even the edge of a razor would not take hold of it; so that the piston remained uneven and almost ragged, and yielded like soft wax under the fingers. In this imperfect state it so far prevents the air from escaping, that a column of three inches is sufficient to kindle the spunk; but after a few strokes of the piston the heat dilates it to such a degree, that it cannot be moved without considerable force. If a drop of oil be put on it, it moves easily; but this soon spoils the instrument; for the oil dissolves the caoutchouc, and forms a varnish, which, as the piston grows hot, makes it adhere still more strongly to the sides of the tube.

On the construction of pistons.

Attempt to use elastic gum for them.

Might not these inconveniences be avoided, by arming the piston rod with caoutchouc, and covering this with leather? If this process succeeded, it might be applied with advantage to all sorts of pumps.

This might be covered with leather.

II. To attain, if possible, a knowledge of the principle of inflammation in the pneumatic tinderbox, four things are to be considered—the materials of the tube, the matter contained in the tube, the materials of the piston, and the friction. Among the materials of the piston I include the grease, with which it is coated, to make it move more easily, and render it fitter to intercept the passage of the air.

What is the cause of the inflammation?

In

Is it electricity?

In examining the question whether the spunk be kindled by electricity, I consider

Arguments against this.

1st, That no part of the instrument is insulated; and that insulation is a necessary condition for producing sensible electricity with any of the machines we know. I say machines that we know, because the animal electricity, that manifests itself without insulation, is an exception to our mechanical means, and cannot here be taken into consideration.

2dly, The friction of the piston, which is a greasy body, against a metallic substance, is not calculated to produce electricity.

3dly, Experience demonstrates, that, unless during storms, the atmosphere seldom exhibits any signs of electricity at the height in which we breathe it; and that we must search for them with instruments in a more elevated region, or when electric clouds are passing over our heads. How then shall we estimate the infinitely small quantity of electric matter in a cubic inch of air, or even less, which the instrument contains.

Fourthly, It is not without great difficulty, that we can kindle spunk with strong electric sparks. I have discharged a large jar on spunk strewed with powdered resin, and it has remained unkindled, though the resin caught fire, and burned entirely away.

In a metallic condenser we cannot see what takes place.

As long as the instrument was made with metallic substances only, we were obliged to confine ourselves to the exterior marks of inflammation alone, without being able to assign the true cause, or at least furnish proofs of it. For to guess is not sufficient in natural philosophy; we must demonstrate, in order to give to facts that degree of certainty, which befits science; and this we cannot do here, without seeing what passes at the very point of inflammation.

We can in a glass tube.

(Glass flutes.)

The means are very simple. Nothing is necessary, but to substitute a glass for a metal tube. Those found in the shops being too slight, I applied to Mr. Laurent, the inventor of glass flutes, requesting him to procure me tubes of a similar quality. This artist, as much distinguished by his civility as by his talents, furnished me with three, which I fitted up. The first, eight inches long by eight lines in diameter, did not kindle the spunk. The second, nine inches long by six lines and three quarters in diameter, kindled it completely. This being destroyed

stroyed by accident, I tried the third, eight inches long by seven lines in diameter, which succeeded equally well.

Appearances. A bright light if the compression be rapid:

When the instrument is made to act, and the spunk kindles, we see a bright flash, that fills the capacity of the tube; and this light is so much the more vivid, in proportion as the compression is more rapid. If the compression be less powerful, the spunk does not kindle, but we perceive in the upper part of the tube a light vapour, that falls in undulations on the piston. When this has disappeared, if we draw back the piston, the vapour will reappear, as long as there is any air in the tube. These effects may be produced several times in succession, merely by pushing the piston with the hand. This vapour is so thin and diaphanous, that it is not perceptible in a strong light. It requires a sort of twilight to see it well.

a light vapour, if not.

Whence arises this vapour?

But whence arises this vapour, and what is its nature? Assuredly it is not furnished by the materials of the instrument; it can only proceed, therefore, from what it contains, from the atmospheric air. Now, according to the present state of our knowledge, the air contains only nitrogen, oxigen, and a very small portion of carbonic acid; all gasiform substances, which are kept in this state by the great quantity of caloric that penetrates them, and are consequently heavier than it*. But in compressing the air contained in the tube, what is the substance that must first give way? Is it not that which is lightest, the caloric, that general solvent, that principle of fluidity and volatilization, which gives wings even to metals to raise themselves in the air? Is then the vapour in question caloric, rendered visible by the approximation of its particles, which are compressed by the surrounding air, as air becomes visible in passing through liquids? This idea, which I am far from presenting

Is it the matter of heat condensed?

It is not aqueous vapour.

* The air likewise, in its ordinary state, contains twelve grains of water in a cubic foot. This small quantity of water, reduced to the proportion of the quantity of air contained in the apparatus, contributes nothing to its effect: for the heat produced by the friction could at most reduce it to vapour, and in this state it would not kindle the spunk. If the vapour seen in the tube were water in a state of expansion, when it fell on the surface of the piston it would condense there, and appear in the state of a liquid. But the surface of the piston always remains dry, though on moving it the vapour appears and disappears several times.

as a

as a thing proved, acquires more probability from the following experiments*.

Trials with other gasses.

I substituted hidrogen for common air, and the vapour showed itself as before; but the spunk did not take fire. With carbonic acid gas, and with nitrogen the effects were the same. The latter, which contained a little nitrous gas, gave a somewhat denser vapour. Oxigen, lightly compressed, yielded a vapour more rare and transient than that from common air. It had scarcely fallen on the piston, when it rebounded and disappeared. When I compressed oxigen with a proper force for producing inflammation, the spunk, which commonly takes fire only at the anterior part, was almost entirely burned: yet for this experiment I used a copper instrument, the piston of which lost air so much, that it would no longer kindle spunk [with common air].

The vapour cannot arise from the grease employed.

Perhaps it will be said, that the vapour came from the greasy matter on the piston, which adheres to the sides of the tube; and that it is expanded by the heat produced by the friction. To this I answer, in this case, 1st. The vapour should not show itself before the greasy matter is deposited on the sides of the tube; yet it appears at the first stroke of the piston, before the tube becomes greasy. 2dly, It should show itself below the piston, in the part which the piston has left; but, on the contrary, it always shows itself above. 3dly, There is no vapour, when the piston loses much air, if the friction be ever so rapid. 4thly, The vapour should be more apparent, when the piston exerts its friction throughout the whole length of the tube, than when it is confined to a small part of its upper extremity; yet the reverse frequently happens. 5thly, When the air is entirely decomposed no more vapour appears, but it shows itself again, if ever so little fresh air be introduced.

It contains no acid.

As it was essential to ascertain whether the vapour did not contain an acid principle, I fastened to the surface of the piston, with a little green wax, a piece of muslin dipped in infusion of litmus, and afterward dried. After twenty strokes of the piston the colour was not changed. I put on a second piece of muslin larger than the first, and the edges of which were loose.

This

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* Mr. De Luc ascribes the ignition to the condensation of the matter of heat. See Journal, vol. XXI, p. 234.—C.

This was burned all round, without the colour of the rest being altered. Lastly, a third piece, which was wet, experienced no change of colour.

General inferences.

From these experiments it follows, that no acid principle is developed: that all aeriform substances, as well as common air, produce a light vapour: that no other g s, except oxigen and common air, kindles the spunk: that oxigen produces a much more powerful combustion than common air, consequently oxigen acts an important part in the inflammation: that as it can exert its action only when set free by the decomposition of the common air, of which it constitutes a fourth part, it follows, that the air contained in the tube is decomposed by the simple force of compression: that the vapour produced is not owing to the oxigen, since it shows itself equally in gasses that contain no oxigen: that this vapour is the effect of some agent common to all gasses: and that we may presume it is caloric itself, rendered visible by the sudden approximation of its parts in a small space, where it rises to a temperature that is increased in the oxigen, so as to kindle the spunk*.

Luminous meteors independent of electricity,

I am equally induced to believe, since the air, and it is the same with all gasses, is decomposed by rapid compression, that the luminous meteors frequently perceived in hurricanes are not always the effects of electricity. I have observed several times, on these occasions, that Saussure's atmospheric electrometer affords no signs of any. I will mention a particular instance, as it occasioned me no less surprise than damage.

in high winds.

In the beginning of the year 1803, being at my country seat, toward evening a violent wind arose, which continued increasing for two hours to such a degree, as to blow down about sixty trees of prodigious size and height in an ornamental plantation. It threw them one upon another in a row, and some of them were broken off. Those that were torn up by the roots brought up the earth with them to the distance of

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Vapour of a different kind.

* It sometimes happens, that the spunk is turned black without kindling. In this case, as well as when it is kindled, if we draw back the piston in the tube, a dense vapour, that may be smelt, issues out, which is not of the same nature as the former. That shows itself before the inflammation; this always succeeds it. That is the principle of the inflammation: this a product furnished by the combustion of the spunk, of which it has the smell.

fifteen

fifteen feet. The clouds flew with extreme rapidity, and twice I saw flashes of light from them. I raised my electrometer, armed with its conductor two feet long, but the balls still continued in contact.

If these researches afford nothing more than conjecture, they will have at least the advantage of serving as a guide to more enlightened observers, whose labours may extend our knowledge of a very obscure subject, to elucidate which is difficult.

XIII.

Analyses of Minerals. *By* Martin Henry Klaproth, Ph. D. &c.

(*Concluded from vol. XXXII, p.* 384.)

Magnesite.

MAGNESITE from Styria*.

Magnesia	48
Carbonic acid	49
Water	3
	100

Magnesian limestone.

Gurofian (so named by Karsten from the place where found).

Carbonated lime	70·5
magnesia	29·5
	100

Wavellite from Barnstable,

Wavellite from Barnstable, in Devonshire.

Alumine	71·5
Oxide of iron	0·5
Water	28
	100

and South America,

Wavellite brought from Hualgayoc in South America, by Humboldt.

Alumine	68
Silex	4·5
Oxided iron	1
Water	26·5
	100

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* For a paper on this stone by Messrs. Haberle and Bucholz, see Journal, vol, XXXI, p. 269.

Siliceou

Siliceous guhr from the Isle of France.

Siliceous guhr.

Silex	72
Alumine	2·5
Oxide of iron	2·5
Water	21
	98

A green mineral, having the appearance of a sandstone, from Spessart.

Green sandstone.

Silex	85·25
Alumine	1
Oxide of iron	7
Water	5
	98·25

Hepatite from Andrarum.

Hepatite.

Sulphated barytes	85·25[*]
———— lime	6
Oxidulated iron	5
Alumine	1
Charcoal	0·50
Loss, including water and sulphur	2·25
	100

Botryolite[†].

Grapestone.

Silex	36
Lime	39·5
Boracic acid	13·5
Oxide of iron	1
Water	6·5
	96·5

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[*] In the J. de Phis. 25· 8. This being evidently wrong, the figures probably had fallen out and been replaced erroneously by the pressmen; and, as the loss is noticed, the whole sum should of course be 100. From the appearance of the figures too, there being a vacancy for one, I have little hesitation in correcting it as above.—C.

[†] See Journ. vol. XXVI, p. 273, for a description of it.—C

Zircon

Jargon.

Zircon from the Circars, in the East Indies, Sp. grav. 4·5.

Zirconian earth	64·5
Silex	32·5
Oxide of iron	1·5
	98·5

Red garnet from Greenland.

Red garnet from Greenland.

Silex	43
Alumine	15·50
Magnesia	8·50
Lime	1·75
Oxide of iron	29·50
——— manganese	0·50
	98 75

Kannelstein.

Kannelstein*.

Silex	38·80
Lime	31·25
Alumine	21·20
Oxide of iron	6·50
Loss	2·25
	100

Opake black tourmalin of Haüy.

Common schoerl from Eibenstock.

Silex	36·75
Alumine	34·50
Magnesia	0·25
Oxidulated iron	21
Potash	6
Oxide of manganese, a *trace*	
	98·5

Opake black tourmalin of Haüy.

Common schoerl from Spessart.

Silex	36·50
Alumine	31
Magnesia	1·25
Oxidulated iron	23·50
Potash	5·50
Oxide of manganese, a *trace*	
	97·75

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* This appears very different from the kannelstein analysed by Lampadius. See Journ. vol. XXIII, p. 231.—C.

Common hornblende from Nora.

Lamellar amphibole of Haüy.

Silex	42
Alumine	12
Lime	11
Magnesia	2·25
Oxidulated iron	30
Manganese	0·25
Water	0·75
Potash, a *trace*	
	98 25

Basaltic horneblende from the country of Fulda, found in volcanic basaltic substances.

Supercompounded crystalized amphibole of Haüy.

Silex	47
Alumine	26
Lime	8
Magnesia	2
Oxidulated iron	15
Water	0·5
	98·5

Common black augite from Rhorgebirge, in Franconia.

Black augite from Franconia.

Silex	52
Lime	14
Manganese	12·75
Alumine	5·75
Oxide of iron	12·75
——— manganese	0·25
Water	0·25
Potash, *a trace*	
	97·75

Common green augite.

Virescite of Delametherie.

Silex	55
Magnesia	13·75
Lime	12·50
Alumine	5·50
Oxide of iron	11
——— manganese, *a trace*	
Water	1
	98·75

Black augite from Frascati. Pyroxine of Haüy.

Black crystallized augite from Frascati*.

Silex	48
Lime	24
Magnessia	8·75
Alumine	5
Oxide of iron	12
———manganese	1
Potash, *a trace*	
	98·75

Black garnet.

Melanite†.

Silex	35·50
Lime	32·50
Alumine	9
Black oxide of iron	24·25
Oxide of manganese	0·40
	101·65

Gadolinite.

Gadolinite from Bornholm.

Yttria	60
Silex	22
Oxidulated iron	16·5
Water	0·5
Oxide of manganese, *a trace*	
	99

Fettstien.

Elæolite‡.

Silex	46·50
Alumine	30·25
Lime	0·75
Oxide of iron	1
Potash	18
Water	2
	98·5

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[*] See Journ. vol. XXVII, p. 148.

[†] Ib. p. 151.

[‡] See a description of it, Journ. vol. XXVI, p. 384.

Apatite

Apatite.

Apatite in mass from Uto.

Phosphated lime	92
Carbonated lime	6
Silex	1
Oxide of manganese, *a trace*	
Loss in roasting	0·5
	99·5

Bituminous slate.

Brandschiefer, or bituminous schist, from Wologda. 200 grs. yielded

	c. inch.	grs.
Sulphuretted hidrogen gas	80 =	28·8
Empyreumatic oil		30
An oil as thick as pitch		5
Ammoniacal water		4
Charcoal		20
Silex		87·5
Alumine		6·5
Lime		10·5
Magnesia		1
Oxide of iron		3
		196·3

Water of the Dead Sea.

Water from the Dead Sea. Sp. grav. 1·245*.

Muriated magnesia	24·2
——— lime	10·6
——— soda	7·8
Water	57·4
	100

Sulphate of zinc.

Crystallized vitriol of zinc, from Rammelsberg.

Oxide of zinc	27·5
——— manganese	0·5
Sulphuric acid	22
Water	50
	100

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† For a very full account of an analysis of this water by Dr. Marcet, see Phil. Trans. for 1807, Part II; or Journal, vol. XX, p. 25.—C.

Red silver ore.

Rothgultigerz.

Silver	60
Antimony	19
Sulphur	17
Oxigen	4
	100

Ore of lead

Fibrous arseniated phosphated lead, from Rosier, near Pontgibaud, in Auvergne.

Oxide of lead	76
Phosphoric acid	13
Arsenical acid	7
Muriatic acid	1·75
Water	0·50
Loss	1·75
	100

Ores of titanium.

Iserine.

Oxidulated iron	72
Oxide of titanium	28
	100

Granular titanited iron.

Oxidulated iron	85·5
Oxide of titanium	14
———— manganese	0·5
	100

Pitchlike iron ore.

Ferruginous pecherz, or piciform iron, from Freyberg.

Oxide of iron	67
Sulphuric acid*	8
Water	26
	101

Ironglance.

Octaedral crystallized volcanic eisenglanz. Sp. gr. 3·88.

Oxidulated iron	66
Silex	29·50
Alumine	4
Potash	0·25
	99·75

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* So in the French.—C.

Tin pyrites.

Tin pyrites.

Copper	30
Tin	26·5
Iron	12
Sulphur	30·5
	99

Realgar.

Realgar.

Metallic arsenic	69
Sulphur	31
	100

Orpiment.

Orpiment.

Metallic arsenic	62
Sulphur	38
	100

Sphene from Salzburg.

Ore of titanium.

Oxide of titanium	46
Silex	36
Lime	16
Water	1
	99

Meteorolite of Lissa,

Meteoric stone that fell at Lyssa, in Bohemia, the 3d of September, 1808*.

Iron	29
Nickel	0·50
Manganese	0·25
Silex	43
Magnesia	22
Alumine	1·25
Lime	0·50
Sulphur and loss	3·50
	100

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* See Journal, vol. XXXI, p. 224.

Meterolite

Smolensko, Meterolite that fell in the government of Smolensko, the 15th of March, 1807*.

Iron	17·60
Nickel	0·40
Silex	38
Magnesia	14·25
Alumine	1
Lime	0·75
Oxide of iron	25
Sulphur, manganese, and loss	3
	100

and Stannern. Metereolite that fell near Stannern, in Moravia, the 22d of May, 1808†.

Silex	48·25
Alumine	14·50
Lime	9·50
Magnesia	2
Iron	23
Loss, including sulphur and manganese	2·75
	100

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* See Journal, vol. XXV, p. 59.

† See Journ. vol. XXXI, p. 229; and for analyses of this stone by Mr. Moser and Mr. Vauquelin, vol. XXV, p. 56 and 58.

Crystallographie

Crystallographic Models, exhibiting the forms of Crystals, their Production, Geometrical Structure, Transitions of Forms, and mechanical Dissections. Intended to illustrate the Science of Crystallography, after the Method of Haüy. *Accompanied with a Treatise elucidating the Elements of that Branch of Knowledge.* By FREDERICK ACCUM, M. R. I. A. Operative Chemist and Lecturer on practical Chemistry, and on Mineralogy and Pharmacy.

> "*In future* the name of God, will be as distinctly written on a crystal, *as it has* hitherto been in the Heavens." Philosoph. Journal, vol. ix, p. 87.

To Mr. Nicholson.

SIR,

Cultivation of mineralogical science.

THE general attention which of late years has been paid to the science of minerals cannot have escaped the notice of the most superficial observers. No department of natural history has been cultivated with more ardour and success than mineralogy, and in none have the cultivators of science been more numerous, both at home and on the Continent. It embraces a wide circle of votaries among the curious and wealthy classes of the community, and it is intimately connected with that laudable passion for exploring the productions of nature which characterises the age in which we live.

Symmetrical solids or crystals.

Indeed, under whatever points of view we examine the shell of our globe, we are struck with the variety of its productions. When we cast our eye over the substances which compose the collections of mineralogists, or the cabinets of the curious, we behold a vast number of bodies, which are regularly shaped, and exhibit the forms of geometrical solids. The substances are called crystals.

General facts respecting crystals. Varieties in the same substance.

When we examine the constitution of crystalline solids by the methods of chemistry, we become convinced, that the same identical substance, or material, does assume different figures, which frequently bear no such resemblance to each other, as would seem to indicate their relation. And chemistry, or the chemical art, is also capable of causing bodies to assume symmetrical forms; and the figures of these are likewise liable to be altered by circumstances, which affect the crystallizing process. Sugarcandy, for example, usually crystallises in oblique four-

sided

sided prisms with wedge shaped summits; but it is also met with in six-sided prisms variously modified. Alum crystallises in octahedrons, but it likewise assumes the shape of a cube. It is found nevertheless, that a certain number of figures are peculiar to each particular crystallisable material; and the crystals of that substance assume some one of those forms, or their modifications, and no other.

The elementary parts are disposed in symmetry according to laws

This however is not all. When we penetrate into the interior structure of these solids, we become convinced, that their mechanical elements are symmetrically placed according to certain laws, which have their measure and their value. Their aggregation is absolutely geometrical, and appears as if it had been effected by instruments guided by skill and intelligence.

—deducible mathematically.

To exhibit these laws of crystalline architecture, is the province of crystallography. This science has in our time been so successfully cultivated, that it gives a dignified aspect to the philosophy of minerals, as grounded upon the results of the most elaborate and skilful analysis. By these we are enabled to calculate with the fewest possible data, simple in the extreme, and mathematically certain, the vast variety of forms of crystals, with a like degree of accuracy as astronomers attain in calculating the motion of the heavens.

Great advantages of models for expla ning this doctrine.

But as the knowledge of crystallography in its improved state abounds in mathematical and algebraic calculations, and cannot therefore be studied with success by such as are unacquainted with the mathematics; it has been proposed to illustrate its elements by the help of geometrical models, which, in other departments of knowledge, are so singularly useful in rendering mathematical demonstrations obvious to the senses. Undoubtedly the human mind is capable of receiving information from the mathematics with much greater facility for demonstrations afforded by tangible solids, than by mere reasoning from designs drawn upon a plane surface. It requires an eye familiarised with the rules of linear perspective to comprehend the diversified and often complicated forms of angular polyhedrons represented by projections by straight lines only, which must naturally cross each other in many directions in the representation of crystalline bodies.

Advancement

The general advancement of science and arts must be greatly dependant on the facility with which their practical means can be obtained. Less than thirty years ago there were not three places at which the ready prepared materials of philosophical chemistry could be purchased in this great metropolis. There was but one maker of turning lathes; philosophical instrument makers were very few; and there were no steam engine makers, agricultural implement manufactories, &c. in London, with which we now so plentifully abound. I would submit to your consideration, Sir, that he who establishes a place of fabrication or deposit of an article of use to the sciences, which could not before be purchased, is a benefactor to the public; and under this point of view, I offer you the present notice, as a piece of scientific news, though it is likewise of a private commercial nature. I have, with considerable expense and attention, prepared a set of models of crystals, partly solid and partly dissected; and have made arrangements, which enable me to supply the public. The dissected crystals are so constructed, that they can readily be taken to pieces and built up again in various ways, to give the untutored eye a distinct conception of the laws of that geometry, which are followed by the integrant particles when they combine, and the orderly arrangement of which produces symmetrical crystals. And this in fact constitutes the basis of the science.

of science from the completion and sale of the useful means of practice.

Dissected models by the author.

A single glance at the dissected models will enable the student to comprehend why crystals are always rectilineal bodies, bounded by planes; and whence that immense variety of actually existing crystalline forms is derived, with which the mineral kingdom has hitherto astonished the world.

I have likewise composed a treatise, which will form a work distinct in itself; but nevertheless so composed, that it may serve as an index of reference to its models through the work*.

Treatise referring to the models.

And as the series of solids to be finished on the present occasion will be limited, such individuals, who are desirous of receiving sets of them, will have the goodness to favour the author with their orders, either in a direct way, or through the medium of their booksellers.

Subscription proposed.

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* This work, which is in the press, will shortly be published by Messrs. Longman, Hurst, Rees, Orme, and Brown, Paternoster-row. The copper plates for the work are engraved by Lowry; and the lineal projections by Berryman and Brandstone.

This condition is essential, because the author presumes he could otherwse employ his time and labours with more advantage to himself, and the public. Farther information may be had at the laboratory, where several thousand models, both solid and dissected, are ready for inspection.

I am, Sir,
Your obliged servant,
FREDERICK ACCUM.

Old Compton Street, Soho,
October 25th, 1812.

Queries. By Inquisitor.

To Mr. Nicholson.

SIR,

Queries in natural history.

THE following queries are submitted to the scientific eye of the perusers of your Journal, and an answer solicited by

INQUISITOR.

QUERIES—Is there a species of lichen, or of any other cryptogamin plant, in the form of a powder, of a light azure colour?

Or do the ova of any insect, or the insects themselves, exhibit this appearance?

Where is the description of such plant or insect to be found?

☞ *For want of room, the accounts of Kirchoff's discovery of a process for converting starch into sugar; with the experiments and inductions of other chemists, are deferred to our next.*

Dr. Pearson's reply to Dr. Marcet came too late for the present month.

The communication and drawing from Mrs. Ibbetson came duly to hand, and will be published in our next number.

ERRATA IN THE LAST NUMBER,

P.	L.		
146	18	For "*rectrified*"	read *rectified.*
	27	" fl℥ij to fl℥iv"	fl℥ij to fl℥iv.

Fig. 1.

Fig. 2.

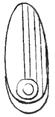


Fig. 3.

Fig. 4.

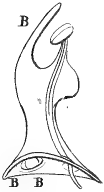


Fig. 5.

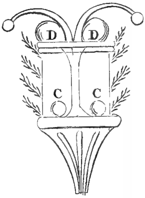


Fig. 6.

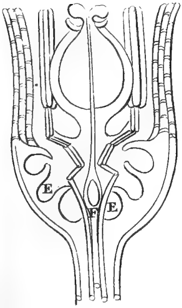


Fig. 7.

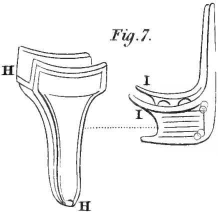


Fig. 8.

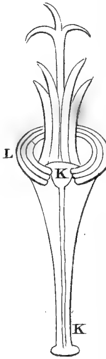


Fig. 11.

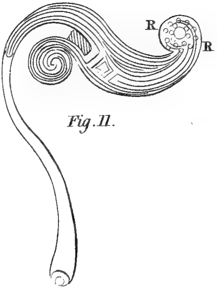


Fig. 9.

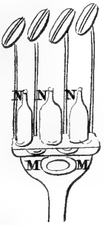
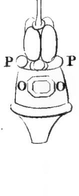


Fig. 10.



Mrs Ibbetson's designs of the secret & open nectary of many flowers.

Sonorif. vibr. of Gases
by Mess.rs Kerby & Merrick.

Fig. 1.

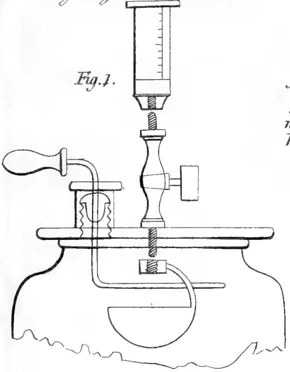


Fig. 2.

Compensation Pendulum. by R. B.

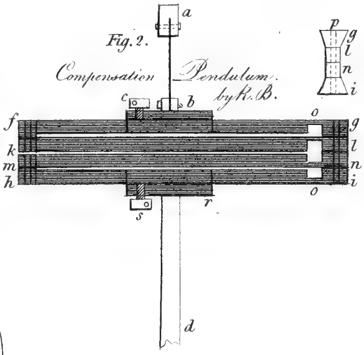
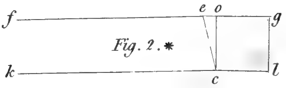


Fig. 2. *



Economical Lamp.

Fig. 3.

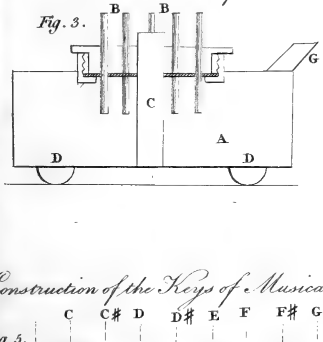
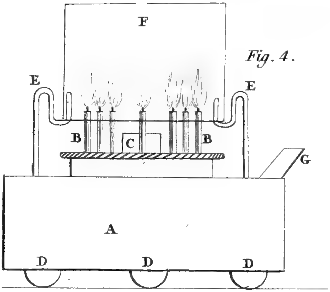


Fig. 4.



Construction of the Keys of Musical Instruments, by Jn.o Trotter Esq.r

Fig. 5.

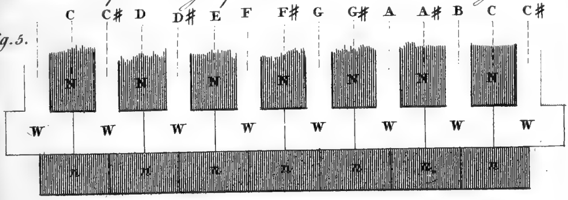


Fig. 6.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1812.

ARTICLE I.

On the Growth or Increase of Trees: by Mrs. AGNES IBBETSON.

To Mr. Nicholson.

SIR,

I HAVE before proved, that there is a vital principle in all plants, from which all flowers proceed; from which the seed is formed, and from which the interior bud is protruded. I have also shown, that in all plants which rise yearly from the earth, whether annual or perennial, the buds shoot from the root; but in all trees and shrubs, from the nearest line of life, which is that *vital part* adjoining the pith. The next matter of importance to the development of the nature of trees is to know and understand, as exactly as possible, how they *increase in size.* That the wood is enlarged by an additional cylinder each year, we are well apprised; and that a new shoot is formed each spring and autumn, we also know: but here our knowledge ends. No one has ever attempted to inquire in what manner that stripe is added, or what preparations nature makes for the purpose: satisfied with the result, they seek no farther, though, without knowing it, the *existence of a tree* is

To show the manner of the increase of a tree.

unintelligible to us. Nor are we informed how nature, in so hasty a manner, can protrude such a length of shoot as is often seen, in the autumn particularly. And yet all this is of the utmost importance to be known; it is that leading ray, which should enlighten all the rest, and give a more perfect perception of the formation of that extraordinary production called a tree, which perhaps may be truly said to collect within itself more wonders than any other matter whatsoever, and which nothing but the custom of viewing daily could enable us to see without constantly increasing astonishment; a being endued with life, and yet governed by mechanical powers; capable of selecting from the juices of the Earth the quantity of sap necessary to its increase, and yet drawing only that, and adapting its increase to the quantity drawn; elaborating its own juices, and by this means rendering them more suitable to the tender existence of the new bud, fit to invigorate the flower, and prepare it for the perfecting the seed; enabling it, by mechanical means, to support its leaves, that no rain water may drop from them on those below, which, if not provided against by nature, would soon putrefy the lower part of the foliage*; but by their varied motion, and mechanical action, so manage, as to throw off to a distance the water thus gathered: enabling the leaves to turn in such a direction, that each may partake of that light absolutely necessary to the welfare and health of the whole, and though producing a deep shade for the solace and refreshment of man, yet each leaf capable of placing itself so as to receive rays of that vivifying matter, light, which we every day learn is more necessary, not only to the animal creation, but also to the vegetable world.

The mechanical powers of a tree.

For a considerable period my time has been dedicated to the studying the new shoot in trees, watching its daily progress, marking it with threads, and then dissecting it in various states of augmentation. By these means I have, I flatter myself, gained a tolerably perfect knowledge of the whole proceedings,

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* The contrivance selected by nature to enable the leaves of trees to throw off the rain water which is not necessary to them, is to be found in the gatherers: they have moments of shaking, which seems to be caused by some sudden effect of the spiral wire. I have repeatedly placed a paper windmill to ascertain whether it was the effect of wind, and found it not so.

and no part of botanical physiology is more worthy a minute inquiry. It is an easy matter to establish a beautiful theory to captivate the imagination, though without elucidating a single fact: but to understand every part of the formation of a plant, interior as well as exterior; to dissect and watch its various states and changes; to examine thoroughly how it passes into each, and what has been the general effect produced in the vegetable by such alterations; to collect by dissection and by culture its habits and powers—these are the requisites, and all this must be gained by examination and study, before we can at last form a theory founded on truth, and learn to know what a plant really is. This is my aim, and upon this I have already employed fourteen years of the most unremitted application. I shall now show the manner of a tree's increase in every way; how the spring and autumn shoots are protruded; what is the difference of various trees in this respect, and also the changes produced in the new shoot, when compared with the older parts of the tree; how the yearly stripe in the wood is contrived, with many other particulars, as they may occur to me.

Necessary manner of study.

If a tree be examined about the beginning of August, it will be perceived, that a sort of screw is forming at the end of the last year's shoot. Each different tree has its own peculiar screw, appertaining to the whole genus. Thus, in the poplar it is long and scattered; in the oak, short and close. It is found by many deep rims, which are partly the outward marks indicating the bud, but perfectly divided all across the plant, one from the other, within as well as without. When you take off the bark and rind from the screw, you find the interior wood swelled with the buds of the year, which are to develop the next spring, and will then be arranged and placed in the bark of the screw. It is now that you see, in the most pointed manner, all I have before shown respecting the buds; viz. that the wood vessels open and disperse to let them out; that the buds possess all this time no other covering than a few coats of alburnum, and have no scales till they reach their cradles in the bark; and that it is the thickened form of this bark in each screw, which allows of the concealment of the buds, where they remain till they have woven their scales or winter covering. I have said, that the screw is a collection of rings or links; there is also

The screws which indicate the shoots.

a part attached to each, capable of increase, and which draws out like a telescope; this increase is generally the usual distance allowed to new shoots in every sort of plant between bud and bud, and of course varies according to the tree. When the screw is formed, and the buds arranged in each, then the shoot begins to push; and here again great variety is discovered—in some trees a quarter of a screw divides, and then runs up to the end of the shoot, forming a long distance between the buds in each new division, and thus continues to develop till all the different *links* are expanded. In the horse-chesnut it will separate into various pieces; its leaves and buds growing from each extremity, equally developing it both ways; but it may always be known which link, or which *part of the screw* is drawing out, by the youngest leaves being in *that place*. In the ash, nearly half the screw first shoots up to the termination of the new branch, and then continues to unfold that piece of the screw, till it is all expanded; it then completes the formation of its *winter bud*, and when *that* is once *protruded* in a plant, it never shoots a piece beyond it *that season*. This is the case, I believe, in every forest tree: in all trees the new shoot differs from the rest, not only in the manner of placing its leaves, but in the appropriate distance of the buds. In the oak the leaves are alternate, and there is not half an inch distance between them: but in a new shoot two leaves come out almost opposite, or within a quarter of an inch, and then pass on a full inch before they recommence their former progress. The first shoot of the elm is very different; the leaves are all twice the size of any other in the tree, and the distance of the leaves is in proportion. Also the screw almost always develops below, at the part where the new shoot begins, and very rarely at the extremity of the branch: besides, nature establishes a curious difference between shooting from an embryo, when a tree is first formed, and pushing its half-yearly branch in spring and autumn. In the first, while yet in the seed, it forms many buds, and while it is developing its seminal leaves, many more are added to the number: from this preparation the branch shoots at once, without waiting to arrange them in screws, because they then may be said to shoot, like an herbaceous plant, from the root, which of course they can never do afterward; and must be protected by the sheath the leaves always lend to young

The shooting of the screw.

The new shoot differing from the old.

Shooting without screws.

young plants of every kind. It is wonderful to see how nature adapts her proceedings to the case in point, and how you may make her vary her modes by changing or altering the situation to which she is exposed. This is the reason that makes me so unwilling to trust to any knowledge gained by placing a plant in an unnatural position, which is certainly the case when we stop the sap; arrest the flow of the blood; varying the growth of any particular part, or play any tricks of that kind in order to benefit by the means she will adopt to right herself; but we are not *enough* acquainted with the *whole arrangement* of plants, to improve by such a mode of practice: the result is only formed to lead us into errour; we misapply the cause, and build a theory on falsehood. The only proper way of studying plants is *constant watching* and *dissection*. The person who will not give up *some years* to the study, should not attempt it—but, to return to my subject. It is not only the shoots from the embryo that come up without a screw; it is the case also with *hungry branches*; these hasty productions are seldom seen in forest trees, though minor trees and shrubs are very subject to them. Whatever part of the tree may be the base from which these branches shoot, a quantity of buds is first formed at that place; and it is, perhaps, this very cause, that makes them run up so hastily. The buds being ready, they soon appear at the extremity of the twig, one by one, till they have expended all that were assembled—it is the same also when a stool is hewn for procuring trees, or when a pollard is fresh cut; the large space allows room for such a number of buds to form, that it appears no longer necessary to arrange them in that exact manner; but they run up hastily, and are soon seen rising alternately at the end of the twigs, and developing both buds and leaves. I have sliced several pollards and stools in this situation, just as they were going to shoot; and the buds have so crowded on each other, that it has been absolutely impossible to count their numbers. This manner, however, of shooting never takes place above once; the second time always comes with the screw as usual.

Necessary to give much time to the study.

Manner of shooting in stools and pollards.

I shall now show how the yearly increase of the stripe in the wood is contrived, which forms the horizontal addition to a tree in width: it is, if possible, attended with more curious circumstances than the increase of the new branch—but I know not any thing more difficult to discover, or that has cost me so much trouble to gain; as it requires so perfect a knowledge of the formation of the tree, and the general disposition of the several parts in each different wood:—but the dissecting and comparing the shoots of autumn and spring, by fresh vegetable cuttings, and watching in trees their increase, has at last enabled me to effect it; and it will be much *more easy* for a person to follow me, now the *matter is known*, than first to make the discovery. Choose a tree of any kind that you can cut to pieces, take off a large branch near the stem at the beginning of August; between the wood and bark a row of alburnum will be found—it is distinguished by being of a clearer and softer substance than any other in the tree: it is this alburnum which is deposited each season, half a circle at a time, and which the next season becomes wood. You will then find the bark and rind are retired back at the south side of the tree, leaving a diminutive space between the alburnum and bark, which is preparing for the season's increase. It is this which causes them to be so easily severed, and makes this the proper season for barking. Take a vegetable cutting of the branch, and examine the alburnum in the solar microscope; it will appear perfectly clear and free from all vessels, and to be merely what I before announced it, a jelly of sap. Continue to cut fresh specimens, and display them daily before good magnifiers, and they will soon show the sap-vessels beginning to run through this stripe of alburnum, and the bastard vessels shooting also across it, but in a contrary direction. In a fortnight's time, that part which was alburnum is now become perfect wood, and the jelly of sap will appear to be forming beyond it, filling up that place from which the bark had receded for the purpose, and forming a new circle of alburnum, which the next autumn, in its turn, will be converted into complete wood. This must at once show how the wood and bark are protruded in trees; and end that eternal dispute, whether the bark make the wood, or the wood the bark. It is certain, that they are of a totally different nature, and yet in one respect agree in their formation. That it is the juices which form the softer part of each; that these coagulate, and then wait for the growth of the separate vessels, which shoot out vessel within vessel, thus lengthening as necessary, and protruding

Yearly increase of sap.

truding like the new shoot of the spring and autumn, and drawing out like a telescope. This is the manner in which the wood vessels increase; the bark vessels are rather different, as I shall explain at another opportunity. But this is not all which is of consequence to the subject—the retiring of the bark-vessels to make way for the new row of alburnum is managed in various ways in different trees. In most fruit trees the bark-vessels bend up, receding from the part the alburnum is to occupy, and then pushing out towards the rind, and thus increasing the circle. In forest trees the smaller cross vessels break away, and leave all the circular ones to retire towards the rind. But whichever way they act, I have a specimen which elucidates each fact, and makes it beyond contradiction: and it may easily be seen, that constantly taking the cutting of a branch every season from the same tree, its increase, and the manner of it, must be exactly noted: but it sometimes happens, that the season is unfavourable, and that the severity of the weather so checks the sap that should form the new row of alburnum in March, that it rises not sufficiently to deposit so large a semicircle; then the old remains on that side, and causes that appearance sometimes found in wood, which presents the yearly circle incomplete; but it occurs not often, especially in *indigenous trees.* Nature performs her part too perfectly, unless we make her fail by removing various trees and shrubs from a more favourable climate to our own—then I have seen it produce a *strange effect.* I have many specimens, in shrubs particularly, where the pith has been wholly on one side, almost joining the bark, though twenty or thirty circles, well defined, have shown three quarters of the year; but the winter quarter has been as void, as if it should never have had a mark—this must be wholly owing to its missing its spring shoots, from the coldness or damp of our climate. The exact manner, in which every branch in a tree tells its own age, is also a curious fact. I have before observed, that the trunk of a tree shows exactly how long it has been planted, but the branch shows only the seasons it has grown, one row for each year. I have taken a whole tree in this manner, examining each division; and the exact way in which it answers to the time of its shooting is curious to see—the autumn shoot, however, is so much wider than the

The retiring back of the bark.

The pith seldom in the middle of a tree.

the spring*, that the pith is seldom in the middle of the trunk, except the tree is very strong, and in a very sheltered situation. But there is another point worthy of attention. If a deep indenture is made in a tree, the mark will go on increasing as long as the tree continues to grow, just as in a range of circles, an angle increases from the centre to the circumference. Thus, if I form a cross on the stem of a tree, twenty years hence that mark will show exactly what increase that stem has made in the middle; and, by the number of coats laid on it in the wood part, how many years since it was first indented. But it certainly appears, on first consideration, most wonderful, that it should do so, considering the extreme change each fibre undergoes, and how often every part must be moved to let the buds pass out from the interior; but on examining a tree *barked*, the miracle ceases—when once an impression is made, every succeeding cylinder is so contrived, that it must enlarge the mark by the progressive motion of the parts, the very thin layers that are added each year, and the forcible and perpetual compression the whole undergoes. It is the same with many natural marks formed by the missed buds or bulbs, the existence of which I have before shown, or any other accidental impression in a tree. This natural effect was productive of a very curious consequence during the time of irreligion and riot in France. A poor widow cleaving a tree to procure some fire-wood to sell, found the mark of a cross in the interior of the trunk of an ash—she never looked at the rind to seek a correspondent impression, but took it for a miracle, a declaration of the Almighty. All the people crowded to see it; the widow was soon enriched, and it had a better effect on their morals, than all the edicts in favour of religion afterward promulgated by Bonaparte, or the horrid experience of times divested of all piety.

The effect of cutting marks on a tree.

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* It is astonishing, how many exotic shrubs, and even trees, grow only in the autumn, and miss their spring shoots, and have, therefore, the pith quite on one side. I have traced this in a number, and, by taking them at the proper time in several different specimens, secured the most absolute proof, that this is the manner in which they increase: this truth, therefore, like the coming out of the bud from the interior, cannot be denied, since no tree or shrub can be examined, without proving it.

But

No torpidity in wood.

But that wood ever so old should get into that torpid state described by some botanists, is certainly a very gross mistake: as soon as the sap ceases to flow, the pipes decay, the rot is introduced, and death ensues: for when all motion ceases in the wood, it can no longer divest itself of those minute parts, which, accumulated, would soon cause its ruin. There are little fibres which join together the bastard vessels, and are constantly renewed every few years, decreasing in length as the compression of the wood makes it necessary. Their motion, therefore, in letting out the buds also divests them of their *extraneous matter*, which would otherwise fill up the places left for the new shoot of alburnum: but let the age be ever so advanced, the stems will throw out new branches, the line of life new buds. I have a log of a tree adjoining the trunk, with above ninety yearly circles—where there are two or three large buds protruding, and the wood vessels making way, as in quite young trees: but that there is some age at which the wood ceases to form in width, there can be no doubt. I think I have traced its manner of proceeding in this respect—but I have so seldom an opportunity of gaining a fit specimen from a very old tree, to ascertain the truth, and am so unfortunately circumstanced for procuring any thing of the kind, (although ever so much wanted,) that few would have the courage to study, so surrounded with obstacles. In a specimen known to be between two and three hundred years old, I have got a vegetable cutting: for eighty years it proceeds in the common manner—then the rows increase, not in the usual place, but *between* the *others*, forming five between each row—this continues for near thirty years; then it passes on to the old place between the bark and the wood, and increases only on the south side of the tree *each autumn*, without any sort of addition in the *spring*, or *north quarter*. This goes on about sixteen or eighteen years, when an entire stop to the growth seems to take place in *width*—it may then be supposed, that the tree, having attained its perfect size, stops for a certain number of years, and then begins to decline, still throwing out buds and branches, and never too old for this, since the oldest possible tree, if freed from rot, and having the *exterior pared*, and a plaster put on, would form new wood, and generate a quantity of buds. I have tried this in such extremely old subjects, that I am convinced it is part of the

Get rid of extraneous matter.

Shooting on the south side only.

the wonders of the vegetable; and that if, therefore, trees were taken care of, they would die only at a very old age. But few of our trees are allowed to gain maturity: we, indeed, use them so shockingly ill, that there is no chance of their reaching to such a time of decay. If noblemen and gentlemen, instead of planting such a number of trees, would lessen the number, and take care of those growing—be as saving of them as of their game—make it the business of the land steward, or bailiff, or gamekeeper, to see that no trees are damaged, or allowed to decay before their time—that the unprofitable branches are lopped, the cankered arms cut off, the withered tops curtailed—that the trunk is not allowed to form holes, or to split, without being joined and plastered—that they are, when first growing, cleared, the sun, air, and wind admitted to them (for to this last they owe their being saved from vermin and blights)—But I mean not now to enter on a farther discussion of this matter—my present subject is not the preservation, but the increase of the tree, which strictly examined by the rules, and in the manner I have advised, will, I flatter myself, be found exactly conformable to truth, and delineated with as much precision as the difficulty of the subject will permit.

Ill usage of trees.

I am, Sir,
Your obliged Servant,
AGNES IBBETSON.

Description of the Drawings.

Drawing to illustrate the manner in which trees increase in growth.

I shall now give the drawings, grieved that it is not in my power to show them in their natural state; for to argue from living specimens at once makes all contradiction impossible, and is as delightful to the teacher as to the instructed. Pl. VI, fig. 1, is the screw of the beech with the winter bud already formed: it is much magnified, and the three leaf-stalks show the manner in which the old branch shoots in the beech, while fig. 2 is the way the new shoot throws out its leaves; in the old parts the leaves are in threes or fours; in the new shoot the leaves are always alternate, with a distance of an inch and a half between. Fig. 3 is part of the same branch laid open, with the buds in their cradles, and with the divisions that show completely how they are to shoot at BBB: and CCC, buds more advanced, with the line of life leading to each bud at DDD. Fig. 4 are three screws of the same without the outward marks of the buds (that

(that it may not confuse), only showing the piece which will draw up or increase: there should be an inch and a half between each in the natural size; from E to E is the lengthening piece. Fig. 5 shows the way the wood increases in the contrary direction, I mean in width. FF is the row of alburnum deposited *last* autumn, and to be completed *this*, which is now done at GG; while the bending up of the bark-vessels at HH allows the sap to deposit a new row of alburnum at II, which is also seen at fig. 6, where the bending of the bark-vessels leaves it free. They are soon straightened at K, by the enlargement of the side of the circle, which the next spring will be made even by another row on the north side. Fig. 7 shows the increase on one side only, when the climate prevents exotics from receiving their spring shoots in width, and this is no very uncommon case. I have many specimens of the kind by me. Fig. 8 shows the manner of forming the circles—when a tree is past eighty, it then marks its lines *between* the others. But I have to get more specimens, which will complete my knowledge in this respect, which is yet *partly* but conjecture, and therefore not wholly to be trusted to. Fig. 9 is the manner in which the wood-vessels draw one out of the other; but as they lengthen, the upper ones soon decrease to the smallest size.

Drawing to illustrate the manner in which trees increase in growth.

II.

Some Horticultural Observations, selected from French Authors. By the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P. R. S. &c.**

Peaches.

THOUGH the English excel in many branches of horticulture, there are others in which they are materially outdone by the French. Absolute perfection in any branch of an art, so extensive as that of gardening, cannot be obtained by a person, who allows his talents to range over every part of it. This the French knew long ago, and have regulated their practice accordingly. The English have not yet begun this subdivision of skill. Our fruit gardeners, who carry every sort of fruit to market of a good quality, cannot be said to have

Attention to a particular branch of gardening alone leads to perfection.

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* Trans. of the Hort. Soc. vol. i. App. p. 4.

brought any one kind to absolute perfection. In *France*, whole villages are employed in the culture each of one single kind of fruit. In consequence of this arrangement, the fruits, under the management of individuals, who for many generations have exerted their whole energies to this one point only, are brought to a degree of perfection, which can never be attained in a garden, where fruits and vegetables of all sorts must be provided by one man, for a large and opulent family, or for a weekly market.

Peaches only cultivated at Montreuil.

At *Montreuil**, a village near *Paris*, the whole population has been maintained, for several generations, by the cultivation of *peaches*, which is their sole occupation. It is there alone, where the true management of this delicious fruit can be studied and attained; for it is impossible, from written precepts, to acquire the whole art. The modes of winter and of summer pruning† are varied not only according to the differences of soil and of exposure, but even according to the state and constitution of each individual tree.

Some of the best raised from the stone.

Some of the best of their fruits are never budded, but always reared from the stone; the rest are budded on stocks of a half wild *peach*, called *peche de vigne*.

Almond the best stock for budding.

Peach trees, budded on an *almond* stock, are larger and more durable than others; but they require a deep and light soil, and do not *fruit* so soon. The best *almonds* for stocks are the

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* An English tourist tells us, that he had stored his carriage with *peaches*, which he thought excellent; when he arrived at *Montreuil*, the inhabitants there, who offer their fruit for sale to travellers, told him that he would, if he tasted one of theirs, throw those he had got out of his chaise; which, in fact, he did, as soon as he had tasted a *Montreuil* peach.

Two modes of bearing in fruit-trees.

† Fruit trees may, in respect to their mode of bearing, be divided into annuals or biennials. *Figs*, *walnuts*, &c., are annuals, that is, they bear their fruit on the branches of the present year; *peaches* and *pears*, &c., are biennials, their fruit is produced on wood of the second year's growth. In this case much advantage is derived from the practice of rubbing off the leaf buds of the fruit-bearing branches, leaving only as many as are wanted to produce wood for the succeeding year. This, no doubt, is the *taille d'été* of the French; it does not only leave the remaining wood to grow stronger, and to ripen sooner, but it materially increases the size of the fruit. The French use this method with their *figs*, as is noticed in page 254.

red

red-shelled sort, and some prefer the bitter; but it is more difficult to succeed with these, than with the soft-shelled almond.

Apricot and Plum stocks.

Stocks of the *apricot*, and of the *prune de St. Juliers*, produce smaller trees, that bear sooner, but do not last so long, and, of course, answer better in a shallow soil.

Season of budding.

The season of budding depends on the weather being more or less wet; the end of *July*, in ordinary years, is proper for the *plum* stock, that for the *apricot* and the *almond* stock is later: and for the young *almond* stock, the middle of *September* is the most proper.

Raising stocks.

In order to provide stocks, the fruit stones are sown in baskets; which, when the tree has attained a proper size, are sunk in the ground where it is intended they should grow, provided the soil is deep; for shallow soils the young plant is taken up, and its larger roots cut off, which forces it to throw out lateral roots, and in the event to become a more productive bearer.

The trees in blossom should be protected from frost.

The climate of *France* is certainly better suited to the culture of the *peach*, than that of *England*, as some sorts produce their fruit there in perfection on espaliers, and a few on standards in the open air. The people of *Montreuil* are, however, abundantly more careful, than we are, to protect their trees from the action of frosts, during the time of flowering: at that time a very slight degree of frost is apt to seize upon the pistil; and if the sun shine upon the flower before it is entirely thawed, this organ loses its power of receiving the pollen, and the flower, in consequence, drops off without setting its fruit.

Modes of doing this.

To guard against this, the tops of the peach walls are furnished with long wooden pegs, or with iron wall-hooks, on which planks are fixed; and on them straw mats are hung in such a manner as to be rolled up or let down at pleasure.

Those who do not use this precaution, light fires with damp straw in such a manner, that the smoke may pass over the flowering branches at sun-rise. This intercepts, in some degree, the direct rays of the sun, and, by its gentle warmth, thaws the frozen pistils by gradual and slow degrees; others fasten the branches cut from ever-green trees, with their leaves upon them, in front of the *peach* trees, to break off the cold air.

The fruit should not ripen on the tree.

Peaches are never eaten in perfection if suffered to ripen on the tree; they should be gathered just before they are quite soft, and kept at least twenty-four hours in the fruit chamber.

Figs.

Figs cultivated at Argenteuil.

The inhabitants of *Argenteuil,* near *Paris,* derive their chief support from the culture of *fig* trees; near that town are immense fields covered with these trees, on the sides of hills facing the South, and in other places sheltered from the North and the North-west winds.

The branches buried to protect them from frost.

In the autumn the earth about the roots of these trees is stirred and dug; as soon as the frosts commence, the gardeners bend down the branches, and bury them under six inches of mould, which is sufficient to preserve them from being frozen.

The branches must be entirely stripped of their leaves before this is done; the gardener then, taking hold of the top of each branch, bends it down gradually, and with much care, to prevent its breaking, placing his knee or his hand under such parts as resist the most; the branches that will not bend low enough to be buried are cut off close to the ground.

A fig-tree will remain buried in this manner seventy-five or eighty days without harm; when the season is mild, the gardeners uncover them, especially in times of warm rains, but on the first symptoms of frost they are again buried. Severe frosts sometimes reach them, but the branches only are destroyed. The roots produce a new crop in the summer; but these do not bear fruit till the next year, and are more tender and liable to be killed by frost during the next winter, than older and more woody branches.

Leaf buds pinched out from the fruit buds.

In the spring the trees are carefully inspected; and where a double bud is observed, the gardeners, who are able to distinguish a leaf-bud, which is more sharp, from a fruit-bud, which is rounder, pinch out the leaf-buds without hurting the fruit-buds; these, as they receive the sap prepared by the plant for two purposes, produce fruit of double the ordinary size; this is done at *Paris* between the first and the tenth of *June*; but these leaf-buds may be suffered to expand a little, till they can be distinguished with certainty; they must not be all destroyed at the same time. In cool seasons, the ripening of of the fruit is hastened by inserting a drop of oil in the eye, from the point of a pen, or tooth-pick. Artificial ripening.

It is necessary in dry seasons to water *fig trees*; the nature of the plant requires to have its root cool, while its head is exposed to the hottest sun. In dry seasons they require water. If planted against the south wall of a house near a spout that brings water from the roof, it thrives luxuriantly. Good situations. Figs do well also in a paved court; the stones keep the ground under them moist and cool, while the surrounding buildings reflect and increase the heat of the sun's rays.

Apricots.

Our gardeners believe that the *Moor-park apricot* is the fruit called *abricot pêche* by the French; but this is a mistake, the *abricot pêche* is a large tree, which may be raised from the stone without grafting: it ripens later than the rest, not till the end of *August*. The stone is so soft, that a pin will pierce through it: the kernel is bitter. Apricot.

Pears.

The *crassane* may be improved, and all its harshness destroyed, by grafting upon the *doyenné*, a pear known in our gardens. Pear.

Apples.

The *golden pippin* (*reinette d'Angleterre*) is described not only as an excellent fruit, but as a very productive bearer; in *England* it appears to be in its last stage of decay. It is probable that trees decay by age sooner in colder than in warmer climates. Golden pippin.

The French do not suffer their *apple* or their *pear* trees, to form wild heads as ours do, and shade all things planted near them; their standard trees, of all kinds, when in gardens, are trained in such manner as to cast the least shade possible. A form like a pyramid, called by them *quenouille*, is very generally used. Shape given to standards.

Plums.

The *green gage*, called in French *la reine Claude*, is much improved, if grafted on an *apricot* or a *peach* stock*. Green gage.

Maize,

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* The name of *green gage* is said to have originated from the fol- Etymology.

Maize, Egg Plant, and Sweet Potatoes.

All these plants are reared for use in some kitchen gardens of *France*, though probably not in many.

Indian corn.

Maize is sown in the ground, without heat; when the spike is about half an inch thick, it is eaten fried in butter, as artichokes are, or made into pickle with vinegar.

Egg plant.

The *egg plant* is called in the gardens *la plante qui pond.* The seeds of this, as of the other varieties of solanum, are sown on a hotbed, in March; the plants, when ready, are transplanted into pots, and plunged in a gentle heat; after the plant has advanced considerably, it may be placed in the open air. The fruit is much used for ragouts in Provence.

Sweet potato.

The *sweet potato** is planted on a hotbed in the middle of *April*, in about six inches of mould: when the shoots are eight or ten inches long, they may be taken up, and replanted in a bed of light mould, in the open air, about eighteen inches deep: all the leaves, except the uppermost, are first to be taken off, and the shoot then buried so deep, that the small bunch of leaves only appears above ground.

In *October* the tubers are ripe and ready to be dug up; in doing this, the greatest care must be taken not to wound the skin, as the slightest scratch disposes them to rot.

They must be kept free from frost and damp; if exposed to either of these, they exhale an odour like that of the rose, and rot immediately. Both the yellow and the red variety are cultivated in *France*; the red is preferred.

Strawberries.

Alpine strawberry.

The French cultivate the *alpine strawberry* in the mode recommended by Mr. A. KNIGHT in the Horticultural Transactions†, and find the fruit so much better when produced by

lowing accident. The *Gage* family, in the last century, procured from the monks of the *Chartreuse*, at *Paris*, a collection of fruit trees: these arrived at their mansion of *Hengrave Hall*, with the tickets safely affixed to them, except only the *reine Claude*, the ticket of which had been rubbed off in the passage. The gardener being, from this circumstance, ignorant of the name, called it, when it bore fruit, the *green gage*.

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* Convolvulus *batatas*, *L.*

† Journal, vol. xxix, p. 214.

seedlings

seedlings of the first year, that they seem to prefer the *alpine* to all other sorts, and to be supplied at market with the fruit of it in every month of the year, by the use of some heat in the winter.

Sowing the seed.

The seeds, they say, may be sown either in a little heat, or in the open air, but always in the shade; they should be sown in sifted mould, and scarcely covered; have a thin layer of moss strewed over them, and they should be frequently moistened. Fresh seed grows up in eighteen days: old seed is much slower. The runners must be carefully removed.

The market gardeners near *Paris* sow theirs twice a year, in *March*, and toward the end of *August*; in six weeks they are large enough to be transplanted, which is done at eight inches apart. Those sown in *March*, fruit in *May* and *June*; those sown in *August*, the spring following. See *Traité des Arbres*, p. 9. I rather suppose that the plants sown in *March* give their fruit in autumn.

It is good to sow *strawberries* within the distance of five or six feet from a north or a west wall; in the latter case, the moss is absolutely necessary. The plants grown from the *Marc* sown seed must be well watered through the summer; in hot weather twice a day, if they are expected to bear in the autumn. The *French* seem to find the *August* sowing most productive. Even in the autumn, in the almanac called *Le bon Jardinier*, the author tells us to sow the seeds of *strawberries* in *February*, if we have not done it in the preceding *August*.

Hautbois and Chili strawberry.

The *hautbois* is called in *French*, *caperonier*; it is lately only that we have observed an hermaphrodite variety, which bears abundantly; in fact, the plant is polygamous: this the *French* have long known, and they say that the *Chili strawberry* is also polygamous, and that the females may be made fertile by the impregnation of the male flowers of the *hautbois*.

III.

Farther Experiments and Observations on the Action of Poisons on the Animal System. By B. C. Brodie, *Esq. F. R. S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society*[*].

Former observations on poisons.

I. SINCE I had the honour of communicating to the Royal Society some observations on the action of certain poisons on the animal system[†], I have been engaged in the farther prosecution of this inquiry. Beside some additional experiments on vegetable poisons, I have instituted several with a view to explain the effects of some of the more powerful poisons of the mineral kingdom. The former correspond in their results so nearly with those which are already before the public, that, in the present communication, I shall confine myself to those which appear to be of some importance, as they more particularly confirm my former conclusions respecting the recovery of animals apparently dead, where the cause of death operates exclusively on the nervous system. In my experiments on mineral poisons, I have found some circumstances wherein their effects differ from those of vegetable poisons, and of these I shall give a more particular account. Whatever may be the value of the observations themselves, the subject must be allowed to be one that is deserving of investigation, as it does not appear unreasonable to expect, that such investigation may hereafter lead to some improvements in the healing art. This consideration, I should hope, will be regarded as a sufficient apology for my pursuing a mode of inquiry by means of experiments on brute animals, of which we might well question the propriety, if no other purpose were to be answered by it than the gratification of curiosity.

Recovery from apparent death.

Effects of mineral and vegetable poisons differ:

medicine may be improved by the investigation.

Former account given more at large:

In my former communication on this subject, I entered into a detailed account of the majority of my experiments. This I conceived necessary, because in the outset of the inquiry I had been led to expect, that even the same poison might not always operate precisely in the same manner; but I have since had abundant

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[*] Phil. Trans. for 1812, p. 205.

[†] Phil. Trans. for 1811, p. 178; or Journal, vol. XXX, p. 295, 324.

abundant proof, that in essential circumstances there is but little variety in the effects produced by poisons of any description, when employed on animals of the same, or even of different species, beyond what may be referred to the difference in the quantity, or mode of application of the poison, or of the age and power of the animal. This will explain the reason of my not detailing, in the present communication, so many of the individual experiments from which my conclusions are drawn, as in the former: at the same time I have not been less careful to avoid drawing general conclusions from only a limited number of facts. Should these conclusions prove fewer, and of less importance than might be expected, such defects will, I trust, be regarded with indulgence, at least by those who are aware of the difficulty of conducting a series of physiological experiments; of the time which they necessarily occupy; of the numerous sources of fallacy and failure which exist; and of the laborious attention to the minutest circumstances, which is, in consequence, necessary, in order to avoid being led into errour.

but the effects of poisons differ but little.

Hence fewer experiments are detailed.

Difficulty of physiological experiments.

II. *Experiments with the Woorara.*

In a former experiment I succeeded in recovering an animal, which was apparently dead from the influence of the essential oil of bitter almonds, by continuing respiration artificially until the impression of the poison upon the brain had ceased; but a similar experiment on an animal under the influence of the woorara was not attended with the same success. Some circumstances led me to believe, that the result of the experiment with the woorara might have been different if it had been made with certain precautions; but I was unable at that time to repeat it, in consequence of my stock of the poison being exhausted. I have since, however, been able to procure a fresh supply, and I shall relate two experiments which I have made with it. In one of these, an animal apparently dead from the woorara, was made to recover, notwithstanding the functions of the brain appeared to be wholly suspended for a very long period of time; in the other, though ultimate recovery did not take place, the circulation was maintained for several hours after the brain had ceased to perform its office.

Artificial respiration recovered an animal from the poison of bitter almonds; but not from that of woorara:

yet it has finally succeeded with this.

Exp. 1. A cat poisoned with woorara.

Experiment 1. Some woorara was inserted into a wound in a young cat. She became affected by it in a few minutes, and lay in a drowsy and half-sensible state, in which she continued at the end of an hour and fifteen minutes, when the application of the poison was repeated. In four minutes after the second application, respiration entirely ceased, and the animal appeared to be dead; but the heart was still felt acting about one hundred and forty times in a minute. She was placed in a temperature of 85 of Fahrenheit's thermometer, and the lungs were artificially inflated about forty times in a minute.

The heart continued acting regularly.

When the artificial respiration had been kept up for forty minutes, the pupils of the eyes were observed to contract and dilate on the increase or diminution of light; saliva had flowed from the mouth, and a small quantity of tears was collected between the eye and eyelids; but the animal continued perfectly motionless and insensible.

At the end of an hour and forty minutes, from the same period, there were slight involuntary contractions of the muscles, and every now and then there was an effort to breathe. The involuntary motions continued, and the efforts to breathe became more frequent. At the end of another hour the animal, for the first time, gave some signs of sensibility when roused, and made spontaneous efforts to breathe twenty-two times in a minute. The artificial respiration was discontinued. She lay, as if in a state of profound sleep, for forty minutes, when she suddenly awoke, and walked away. On the following day she appeared slightly indisposed; but she gradually recovered, and is at this time still alive and in health.

Exp. 2. A rabbit.

Experiment 2. Some woorara was applied to a wound in a rabbit. The animal was apparently dead in four minutes after the application of the poison; but the heart continued acting. He was placed in a temperature of 90°, and the lungs were artificially inflated. The heart continued to act about one hundred and fifty times in a minute. For more than three hours the pulse was strong and regular; after this it became feeble and irregular, and at the end of another hour the circulation had entirely ceased. During this time there was no appearance of returning sensibility.

The circulation of the blood may be maintained in an animal from

from which the brain has been removed for a considerable, but not for an unlimited, time. We may conclude, that in the last of these experiments the animal did not recover, because the influence of the poison continued beyond the time during which the circulation may be maintained without the brain.

The circulation cannot be kept up above a certain time without the brain.

III. *On the Effects of Arsenic.*

Two opinions of the effect of arsenic.

When an animal is killed by arsenic taken internally, the stomach is found bearing marks of inflammation; and it is a very general opinion, 1, that this inflammation is the cause of death: 2, that it is the consequence of the actual contact of the arsenic with the internal coat of the stomach. But in several cases I have found the inflammation of the stomach so slight, that on a superficial examination it might have been easily overlooked; and in most of my experiments with this poison death has taken place in too short a time for it to be considered as the result of inflammation: and hence we may conclude, that the first of these opinions is incorrect; at least as a general proposition.

Death is not the result of inflammation of the stomach.

Many circumstances conspire to show, that the second of these opinions also is unfounded.

The inflammation that exists not caused by the contact of the arsenic.

In whatever way the poison is administered, the inflammation is confined to the stomach and intestines—I have never seen any appearance of it in the pharynx or œsophagus.

Mr. Home informed me, that in an experiment made by Mr. Hunter and himself, in which arsenic was applied to a wound in a dog, the animal died in twenty-four hours, and the stomach was found to be considerably inflamed.

If applied to a wound, the stomach becomes inflamed.

I repeated this experiment several times, taking the precaution always of applying a bandage, to prevent the animal licking the wound. The result was, that the inflammation of the stomach was commonly more violent and more immediate, than when the poison was administered internally, and that it preceded any appearance of inflammation of the wound*. Some

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* Since the greater part of my experiments on this subject were made, I have seen an account of an inaugural dissertation on the effects of arsenic, by Dr. Jaeger of Stuttgard. Dr. Jaeger has come to conclusions similar to those above stated, that in an animal killed by arsenic, the inflammation of the stomach is not the cause of death, and that the poison

Dr. Jaeger of the same opinion with the author.

Vegetable poisons act on the system through the blood:

so does arsenic.

Some experiments are already before the public, which led me to conclude, that vegetable poisons, when applied to wounded surfaces, affect the system by passing into the circulation through the divided veins. From this analogy, and from all the circumstances just mentioned, it may be inferred, that arsenic, in whatever way it is administered, does not produce its effects even on the stomach until it is carried into the blood.

How far the blood is necessary to life.

But the blood is not necessary to life, except so far as a constant supply of it is necessary for the maintenance of the functions of the vital organs. The next object of inquiry therefore is, when arsenic has entered the circulation, on what organs does it operate, so as to occasion death?

Arsenic causes a slough, not as a caustic, but by killing the part.

Acting as a poison, it does not destroy the life of every part at once.

When arsenic is applied to an ulcerated surface, it produces a slough, not by acting chemically, like caustics in general, but by destroying the vitality of the part to which it is applied, independently of chemical action. This led me at first to suppose, that, when arsenic has passed into the circulation, death is the consequence, not so much of the poison disturbing the functions of any particular organ, as of its destroying at once the vitality of every part of the system. The following circumstances, however, seem to show, that this opinion is erroneous. In an animal under the full influence of arsenic, even to the instant of death, some of the secretions, as those of the kidneys, stomach, and intestines, continue to take place in large quantity; and the muscles are capable of being excited, after death, to distinct and powerful contractions by means of the Voltaic battery.

Exp. 3. Arsenic applied to a wound in a rabbit.

Experiment 3. Seven grains of the white oxide of arsenic were applied to a wound in the back of a rabbit.

In a few minutes he was languid, and the respirations were small and frequent. The pulse was feeble, and after a little time could not be felt. The hind legs became paralysed*. He

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poison does not produce its fatal effects until it has entered the circulation. I have to regret, that I have had no opportunity of seeing the original of this dissertation.

The influence of the brain less easily con-

* I have observed, that, where the functions of the brain are disturbed, paralysis first takes place in the muscles of the hind legs; afterward in those of the trunk and fore legs; and last of all in the muscles of the ears

He grew insensible, and lay motionless, but with occasional convulsions. At the end of fifty-three minutes from the time of the arsenic being applied, he was apparently dead; but on opening the thorax, the heart was found still acting, though very slowly and feebly. A tube was introduced into the trachea, and the lungs were artificially inflated; but this appeared to have no effect in prolonging the heart's action. On dissection, the inner membrane of the stomach was found slightly inflamed.

Exp. 4. Arsenic injected into the stomach of a dog.

Experiment 4. Two drams of arsenic acid dissolved in six ounces of water were injected into the stomach of a dog, by means of a tube of elastic gum, passed down the œsophagus. In three minutes he vomited a small quantity of mucus, and this occurred again several times. The pulse became less frequent, and occasionally intermitted. At the end of thirty-five minutes the hind legs were paralysed, and he lay in a half sensible state. At the end of forty-five minutes he was less sensible; the pupils of the eyes were dilated; the pulse had fallen from 140 to 70 in a minute, and the intermissions were frequent. After this, he became quite insensible; convulsions took place, and at the end of fifty minutes, from the beginning of the experiment, he died. On opening the thorax, immediately after death, tremulous contractions of the heart were observed; but not sufficient to maintain the circulation. The stomach and intestines contained a large quantity of mucous fluid, and their internal membrane was highly inflamed.

The experiments repeated with similar results.

These experiments were repeated, and the results, in all essential circumstances, were the same. The symptoms produced were, 1, paralysis of the hind legs, and afterwards of the other parts of the body; convulsions; dilatation of the pupils of the eyes; insensibility; all of which indicate disturbance of the functions of the brain: 2, a feeble, slow, intermitting pulse, indicating disturbance of the functions of the heart. Where the heart has continued to act after apparent

ears and face. These facts seem to show, that the influence of the brain, like that of the heart, is not propagated with the same facility to the distant as to the near organs; and this is farther confirmed by cases of disease which occasionally occur, in which, although the paralysis is confined to the lower half of the body, the morbid appearances met with on dissection are entirely confined to the brain.

veyed to remote parts.

death, I have never, in any one instance, been able to prolong its action by means of artificial respiration. 3, pain in the region of the abdomen; preternatural secretion of mucus from the alimentary canal; sickness and vomiting in those animals which are capable of vomiting; symptoms which arise from the action of the poison on the stomach and intestines. There is no difference in the effects of arsenic, whether it is employed in the form of white oxide, or of arsenic acid, except that the latter is a more active preparation. When arsenic is applied to a wound, the symptoms take place sooner than when it is given internally; but their nature is the same.

Arsenic applied to wounds acts most speedily.

Effects of arsenic.

The symptoms produced by arsenic may be referred to the influence of the poison on the nervous system, the heart*, and the alimentary canal. As of these the two former only are concerned in those functions which are directly necessary to life, and as the alimentary canal is often affected only in a slight degree, we must consider the affection of the heart and nervous system as being the immediate cause of death.

In every experiment which I have made with arsenic, there were evident marks of the influence of the poison on all the organs which have been mentioned; but they were not in all cases affected in the same relative degree. In the dog, the affection of the heart appeared to predominate over that of the brain, and on examining the thorax immediately after death, this organ was found to have ceased acting, and in a distended state. In the rabbit, the affection of the brain appeared to predominate over that of the heart, and the latter was usually found

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The heart in some respect independent of general affection of the nervous system.

* When I say, that a poison acts on the heart, I do not mean to imply, that it necessarily must act directly on the muscular fibres of that organ. It is highly probable, that the heart is affected only through the medium of its nerves; but the affection of the heart is so far independent of the affection of the nervous system generally, that the circulation may cease although the functions of the brain are not suspended, and the functions of the brain may be wholly suspended without the circulation being at all disturbed. In proof of the first of these propositions, I may refer to my former experiments on the upas antiar, in which the sensibility of the animal continued to the very instant of death; and respiration, which is under the influence of the brain, continued even after the heart had ceased to act. In proof of the second, I may refer, among many others, to the experiments detailed in the Croonian Lecture for 1810.—[Phil. Trans. for 1811, pp. 36, 211; or Journal, vol. XXIX, p. 359.]

found acting slowly and feebly, after the functions of the brain had entirely ceased. In the rabbit, the effects of the arsenic on the stomach and intestines were usually less than in carnivorous animals.

Action of arsenic less simple than that of most vegetable poisons.

The action of arsenic on the system is less simple than that of the majority of vegetable poisons. As it acts on different organs, it occasions different orders of symptoms; and as the affection of one or another organ predominates, so there is some variety in the symptoms produced even in individual animals of the same species.

Appearances after death.

In animals killed by arsenic the blood is usually found fluid in the heart and vessels after death; but otherwise all the morbid appearances met with on dissection are confined to the stomach and intestines. As this is the case, and as the affection of these organs occasions remarkable symptoms, it may be right to mention the result of my observations on this subject.

State of the alimentary canal.

In many cases where death takes place, there is only a very slight degree of inflammation of the alimentary canal: in other cases the inflammation is considerable. It generally begins very soon after the poison is administered, and appears greater or less, according to the time which elapses before the animal dies. Under the same circumstances, it is less in graminivorous, than in carnivorous animals. The inflammation is greatest in the stomach and intestines; but it usually extends also over the whole intestine. I have never observed inflammation of the œsophagus. The inflammation is greater in degree, and more speedy in taking place, when arsenic is applied to a wound, than when it is taken into the stomach. The inflamed parts are in general universally red, at other times they are red only in spots. The principal vessels leading to the stomach and intestines are turgid with blood; but the inflammation is usually confined to the mucous membrane of these viscera, which assumes a florid red colour, becomes soft and pulpy, and is separable without much difficulty from the cellular coat, which has its natural appearance. In some instances there are small spots of extravasated blood on the inner surface of the mucous membrane, or between it and the cellular coat, and this occurs independently of vomiting. I have never, in any of my experiments, found ulceration or sloughing

sloughing of the stomach or intestine; but if the animal survives for a certain length of time after the inflammation has begun, it is reasonable to conclude, that it may terminate in one or other of these ways,

Sloughing seldom, if ever, the direct consequence of the application of arsenic to the stomach or intestines.

I am disposed to believe, that sloughing is very seldom, if ever, the direct consequence of the application of arsenic to the stomach or intestines. Arsenic applied to an ulcer will occasion a slough ; but its action in doing this is very slow. When I have applied the white oxide of arsenic to a wound, though the animal has sometimes lived three or four hours afterward, and though violent inflammation has taken place in the stomach and intestines, I have never seen any preternatural appearance in the part to which it was applied, except a slight effusion of serum into the cellular membrane. Arsenic speedily produces a very copious secretion of mucus and watery fluid from the stomach and intestines, which separates it from actual contact with the inner surface of these organs, even though taken in large quantity and in substance ; and in animals which are capable of vomiting, by much the greater part is rejected from the stomach very soon after it has been taken in. Hence though a few particles of arsenic are sometimes found entangled in the mucus, or in the coagulum of extravasated blood, and adhering to the inner surface of the stomach, I have never seen it in such a quantity as might be supposed capable of producing a slough.

Supposed sloughs from its action on the stomach.

In one instance, where a dog had swallowed a large quantity of arsenic in substance, a brown spot, about an inch in diameter, was observed after death on the inner surface of the cardiac extremity of the stomach, having so much of the appearance of a slough that at first I had no doubt of its being so; but on examination this proved to be only a thin layer of dark-coloured coagulum of blood, adhering very firmly to the surface of the mucous membrane, and having a few particles of arsenic entangled in it. On removing this the mucous membrane still appeared of a dark colour ; but this was also found to arise from a thin layer of coagulum of blood between it and the cellular coat. The mucous membrane itself was inflamed ; but otherwise in a natural state. I have observed a similar appearance, but occupying a less extent of surface, several times. In the Hunterian Museum there is a human stomach, which was preserved

to show

to show what was considered as a slough produced by the action of arsenic. On examining this preparation, I found that the dark-coloured spot, which had been supposed to be a slough, was precisely of the same nature with that just described.

The affection of the stomach and intestines from arsenic sometimes kills ultimately.

Although the affection of the stomach and intestines from arsenic is not the cause of death under ordinary circumstances, it is reasonable to conclude, that it may be so in some instances, if the animal survives the effects produced on the organs more immediately necessary to life. Mr. Henry Earle informed me of an instance, in which this appeared to be the case. A woman in St. Bartholomew's hospital, who had taken arsenic, recovered of the immediate symptoms, but died at the end of four or five days. On examination after death, extensive ulcerations were found of the mucous membrane of the stomach and intestines, which we can hardly doubt to have been the cause of death.

Important judicial inquiry. How far is it possible to ascertain poisoning by arsenic from examination of the dead body?

It is an important matter of inquiry, as connected with judicial medicine, how far may the examination of the body, after death, enable us to decide, whether an animal has died of the effects of arsenic? On this subject, however, I have only a few remarks to make.

The inflammation from arsenic, occupying in general the whole of the stomach and intestine, is more extensive than that from any other poison with which I am acquainted. It does not affect the pharynx or œsophagus, and this circumstance distinguishes it from the inflammation which is occasioned by the actual contact of irritating applications.

But little in general is to be learnt from the examination of the contents of the stomach after death. When arsenic has been taken in substance, small particles of it are frequently found entangled in the mucus, or in the extravasated blood; but where this was not the case, I have never known, in an animal that was capable of vomiting, that arsenic could be detected in the contents of the stomach after death, though examined by the most accurate chemical tests. As some substances when taken internally are separated from the blood very soon afterward with the urine, I thought it probable, that arsenic might be separated with the urine also; but Mr. Brande (to whom I am indebted for assistance on this, as well as on many

many other occasions) could never detect the smallest trace of arsenic in it.

(To be concluded in our next.)

IV.

Description of an Apparatus by means of which all bad Smell may be avoided in manufacturing Prussian Blue: by Mr. D'ARCET*.

Manufacture of Prussian blue offensive. One source obviated.

THE manufactures of Prussian blue diffuse to a distance two kinds of bad smell. The first, that produced from the combustion of animal matter, is easily avoided by covering the crucible with a dome, at the summit of which is the chimney of the furnace, and setting fire to the vapours emitted from the crucible, as soon as they are hot enough to burn.

Another from the potash.

The second source of the bad smell is found in the use of the potash of the shops, which contains more or less sulphate of potash. When the mixture of blood and potash is calcined, the temperature is high enough for the sulphate to be decomposed and converted into sulphuret, by means of the animal charcoal mingled with it: whence it follows, that the Prussic liquid always contains hydrosulphuret of potash in solution; and that, when this liquor is mixed with the solution of alum and sulphate of iron, a large quantity of sulphuretted hidrogen gas is evolved, which is extremely fetid, and diffuses itself to a distance, tarnishing plate and spoiling meat, that is within its sphere of action.

Apparatus to obviate this.

By means of the apparatus about to be described, these inconveniences may be avoided, and the sulphuretted hidrogen gas evolved on the mixture of the two liquids may even be turned to advantage.

Described.

Pl. VII, fig. 1, *a*, is a tub of white wood, well hooped, and firmly supported by two pieces of wood, that raise it from the ground, and preserve the bottom from rotting.

b is a hemisphere of thin copper, of the same diameter as the tub, and serving as a cover to it. It fits into it up to the

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* Ann. de Chim. vol. LXXXII, p. 165.

rim that appears in the figure. Before the cover is put on, the edge of the tub and the lower face of the rim are to be coated with potter's clay well diluted, which will render the juncture perfect.

c is a copper tubulure, through which the stem of the beater *h* is to be passed, before the cover is put on the tub.

h elevation of the beater. Toward the upper end of the stem is seen a piece of skin fastened to it. When the beater is placed in the tub, and the stem passes out through the cover, the lower part of the skin is fastened to the rim of the tubulure, and thus the communication with the air is prevented, without prejudice to the movement of the beater. The skin used should be soaked in oil, that it may not be injured by the liquids put into the tub.

g plan of the foot of the beater.

d funnel through which the different solutions are poured into the tub.

l a wooden plug used to stop the neck of the funnel.

i a cock, or spigot and faucet, by which the Prussian blue is drawn off from the tub, after the solutions have been well mixed in it.

n a small tub, sunk into the ground, into which the result of the mixture runs. As the liquid Prussian blue runs into this, it is dipped out with a ladle into a bucket, to be carried to the casks, in which it is to be washed with a large quantity of water.

e a curved tube fixed to the dome.

f a tube of the same diameter fixed in the ground. The dotted lines, terminating at *m*, point out the situation of this tube, which is placed parallel with the surface of the ground, and terminates in the ash-pit, near the grate of the furnace where the prussiate of potash is prepared. When the cover is put down on the tub, the tube *e* should enter into the tube *f*, and the juncture is to be luted with a little potter's clay.

Mode of using it.

Fig. 2 represents the apparatus put together, and ready for use. When the solutions are prepared, the door of the ash-pit, in which the tube terminates, is to be shut close; the plug *l* is to be taken out of the funnel, and the solution of alum and sulphate of iron poured in. A workman mounts a little stool, takes hold of the stem of the beater *h*, and begins to agitate the liquor

liquor in the tub. Two others pour the prussic liquor gently into the funnel *d*, and the workman who holds the beater moves it in all directions, that the liquors may be intimately mixed. A little of the mixture is drawn off occasionally by the cock *i*, filtered through blotting paper, and examined, to find whether a sufficient quantity of prussiate of potash have been poured in: if not, more is added; and, when it has reached the point of saturation, no farther addition is to be made, but the stirring of the mixture with the beater is to be continued about ten minutes.

The ash-pit of the furnace being closed, the draught of the fire causes the outer air to enter through the tube of the funnel *d*; this air mixes with the gasses extricated from the mixture, and the whole is conveyed through the tube *e m* underneath the grate of the furnace, where the sulphuretted hidrogen takes fire, loses thus its offensive smell, and serves also to keep up the heat of the crucible.

When the stirring of the mixture is finished, the tub may be emptied by the cock, and a new mixture immediately commenced.

The cover of the tub need not be taken off, except when the apparatus wants repair. When it is left some time out of use, the tub should be kept full of water, and this water may be employed afterward for lixiviating the residuum of the calcination of the blood and potash.

It has been employed with success.

The apparatus here described and figured I have caused to be fitted up at the paper-hanging manufactory of the brothers Jacquemart. It has succeeded completely, no inconvenience has been found in its use, and it has entirely freed the work-shops and neighbourhood from the bad smell diffused by mixing the prussic liquor, and the solution of alum and sulphate of iron.

Note of the French Editors.

A similar apparatus at another manufactory.

An apparatus of this kind, but by no means so well constructed, has also been employed with success for several years in a manufactory of Prussian blue in St. Nicholas-street.

V.

Extract from a Letter addressed to Mr. D'ARCET *by Mr.* DUFAUD, *Director of the Iron Works at Montalaire, near Creil**.

SIR,

I HAVE undertaken, with the greatest pleasure, the experiments on sawing hot cast iron, that you recommended to me: I have followed your instructions; my trials have been attended with the most complete success, and I hasten to give you an account of them.

Sawing heated cast iron suggested.

These experiments were the more interesting to me, as I have since applied them to practical purposes.

Experiment.

My first trial was made with the support of a grate, 108 mil. [4·25 in.] thick. This piece of cast iron was heated in a forge fire with coal: and as soon as it had acquired a sufficient degree of *incandescence* [this is the French term] it was placed on an anvil, and I sawed it with a common carpenter's saw, without any difficulty, and without any injury to the saw, which I dipped immediately into cold water. The carpenter continued to work with the same saw, without having any occasion to repair it.

The iron broke, as wood would have done, for want of support at the end.

In this my first trial a little accident occurred. The end of the iron I was sawing off not being supported, it broke, when 20 or 25 m. (about a line) remained to be cut through; but this slight defect I immediately removed with the saw. Convinced of the ease with which a common saw would cut hot cast iron, I afterward applied it to the demands of the ironworks.

Second experiment.

I had occasion to shorten a pivot of 135 m. [5·3 in.] in diameter; but, afraid of its breaking if I cut it cold, an operation besides very tedious and uncertain, unless executed in a lathe, I had resolved to cast another, when the experiment I have just mentioned determined me to saw it.

Having marked the place of section with red lead, I placed the pivot in a reverberatory furnace; and when I thought it sufficiently hot, I had it taken out of the furnace, and placed on an iron support, so that the two ends had equal bearings.

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* Ann. de Chim. vol. LXXXII, p. 218.

In four minutes, with two saws, which I used and cooled alternately, the piece was cut off, to the great astonishment of my workmen, who found the saws unhurt.

Third experiment.

The same day I performed a still more difficult operation. I had an anvil, which I was about to cast afresh, because it was 41 m. [1·6 in.] too thick, so that it could not be placed in its bed.

I marked the place of the saw kerf with red lead. The two cuts to be made were 217 m. [8·5 in.] long, by 189 m. [7·4 in.] high; and the thinness of the piece to be cut off required precision. This anvil was heated in a reverberatory furnace, in the same manner as the pivot; and, when sufficiently hot, two workmen took hold of it with a strong pair of tongs, and laid it on a block of cast iron. It was cut with much ease and precision by the same saws that had been used in the preceding instance.

General remarks.

In the course of these experiments I remarked,

1, That hot cast iron may be sawed as easily, and in the same space of time, as dry wood.

2, That, to diminish the resistance, the saw should be set fine.

3, That iron heated in a furnace saws more easily than if heated in a forge: and the reason is simple; in a furnace it is heated equally throughout, while in a forge the part near the tewel is almost in a state of fusion, while that opposite to it is scarcely red-hot.

4, That the iron must not be made too hot; for, if its surface be too near a state of fusion, the saw will be clogged, and the process will not go on well.

5, That the saw should be moved very quickly, because then it will be less heated, make its way better, and the cut will be more clean and exact.

6, Lastly, that the iron should be so placed as to have a firm bearing every where, except where the saw is to pass, otherwise it is liable to break before the cutting is finished.

These, Sir, are the whole of my experiments and observations; and I shall be well pleased if they answer your views.

The practice may be of ex-

It is the more to be wished, that this method of cutting cast iron should be rendered as public as possible, as it may be happily

pily applied in many arts. I thank you much for having suggested it to me, for I shall find frequent occasions for it.

tensive use.

Note by Mr. D'ARCET.

Several years ago Mr. Pictet observed a workman saw a cast iron pipe in the workshop of Mr. Paul, of Geneva. He had lately occasion to mention this to Mr. Thenard, who afterward communicated it to Mr. Mollard. Mr. Mollard, struck with the uses to which it might be applied, tried it at the Conservatory of Arts and Trades on pieces of cast iron 7 cent. [2·75 in.] square, and on plates of different thicknesses.

Practised some years ago at Geneva.

Experiments at Paris.

Mr. Mollard used a common saw, and succeeded perfectly with these various pieces, without injuring its teeth. He observed, that the iron should be heated only to a cherry red; and that it should be cut briskly, using the whole length of the saw. Mr. Mollard afterward found, that this process was known to a workman of Mr. Voyenne, who practised it in fitting the cast iron plates used for making stoves. It is probable, that this simple operation may be known in other workshops; but it is lost, as it were, since persons of distinction in the arts are generally ignorant of it.

Instructions.

Known to others.

We see, that the experiments mentioned in Mr. Dufaud's letter confirm the account of Mr. Pictet, and the trials of Mr. Mollard: of course there remains no doubt of the possibility of cutting cast iron when hot, or of the utility of the process.

The practicability demonstrated.

We conceive it would be practicable to employ it in the fabrication of iron cannons, for cutting off the cap of the piece, and even for removing the square piece left at the extremity of the button, which serves for mounting it on the boring machine. Perhaps advantage might be taken of the red heat, which the cannon retains long after it is cast, for sawing off the cap in the mould itself, its upper part only being removed.

Uses to which it may be applied: in making cannon,

The same process would certainly furnish an easy and ready method of cutting a cannon to pieces, and thus rendering it unserviceable; or facilitate its melting in the reverberatory furnace, when required to be cast afresh. Perhaps it might be employed also to ascertain the different ranges of a piece of cannon, shortened by little and little. It seems to us, the knowledge of a practice applicable to so many purposes of the arts cannot be too generally made known.

destroying or remelting them,

and ascertaining their ranges.

VI.

On the liquid Sugar of Starch, and the transmutation of sweet Substances into fermentable Sugar : by Mr. VOGEL. *Abridged by* Mr. BOUILLON-LAGRANGE[*].

Sugar had not been chemically compounded.

NO chemist has hitherto been able to form sugar by chemical agents. It is true, that Fourcroy and some others supposed, that at some time or other we should perhaps effect the conversion of starch into sugar, as the component parts of these two substances come infinitely near each other.

Supposition that it might be formed from starch,

"Starch," says Fourcroy, "announces itself as a little less carbonated than gum : we may say, that it comes very near to saccharine matter ; and we shall see hereafter, that it appears in fact capable of forming it by a particular alteration of its own substance[†]."

and from gum.

Under the head *gum*, the same chemist expresses himself as follows. "It is not improbable, that art may effect the conversion of gums into saccharine matter ; and already I have several times remarked, that an aqueous solution of gum, through which oximuriatic gas is passed, acquires a saccharine taste, mixed with a strong bitterness. This view of the subject, at present quite novel, will lead to many researches, and to useful results."

Said to have been done :

It is even pretended, that several authors say they have effected this transmutation of fecula into saccharine matter : but how is it possible, that they should have succeeded, and been silent on a fact of such importance ?

but by no one before Kirchoff.

On looking over what has been published by natural philosophers, it appears incontestable, that it was reserved for Mr. Kirchoff, of the imperial academy of Petersburgh, to convert starch into gummy matter, and this into saccharine matter[‡].

His

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[*] Ann. de Chim. vol. LXXXII, p. 148.

Component parts of starch.

[†] According to Messrs. Gay-Lussac and Thenard, starch is composed of

Carbon········ ········	43·55
Oxigen········ ······	49·68
Hidrogen········ ······	6·77
	10·000

Starch ren-

[‡] Mr. Bouillon Lagrange has already found means of renderin

His discovery pursued by Vogel.

His discovery, which opens a new career to vegetable analysis, and may lead to interesting results, has induced Mr. Vogel to pursue these new facts. His first experiments, some particulars of which he has given in the Journal de Physique, differ scarcely in any thing from those of Mr. Kirchoff, except in his observing, that part of the saccharine matter is formed in the course of two hours boiling, and that the proportion of two hundredths of sulphuric acid produces more than that of one hundredth, the quantity mentioned by the chemist of Petersburgh.

Since that time Mr. Vogel has followed up his experiments with more care, in order to acquire an intimate knowledge of the saccharine matter, and the mode of its formation.

Not ready formed in the starch.

To remove every idea of the saccharine matter being the result of simple extraction; a matter that, having escaped fermentation, was concealed by the starch; he washed the starch with a stream of cold water, before he made use of it.

Method of producing it.

When well dried and reduced to powder, he mixed 2 kil. [4lbs. 6½ oz. avoird.] with 8 kil. of Seine water, acidulated with 40 gr. [0·02 of the weight of the starch] of sulphuric acid at 56° [1·631].

He then boiled the mixture in a silver basin for thirty-six hours. There is no danger of its burning, except during the first hour, when it must be kept constantly stirring with a broad wooden spatula. After that time the mixture grows much more fluid, and requires only to be stirred occasionally.

It is essential to keep up the quantity of water, by adding fresh as it evaporates.

Clarification.

After this boiling, it is to be clarified when cold by means of charcoal and chalk, and the whole is to be filtered through flannel.

Evaporation.

The liquid having been evaporated nearly to a sirupy consistence, it must be left to cool, that more of the sulphate of lime may fall down; after which the clear liquid is to be decanted off, and the evaporation finished.

A silver vessel preferable to tinned copper.

The sugar thus obtained with two hundredths of sulphuric acid in a silver basin was much more saccharine, and less high coloured, than that made in a basin of tinned copper.

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starch soluble in cold water by a slight torrefaction, and thus assimilating it to mucilages. See *Bulletin de Pharm., tom. iii, p.* 395. dered soluble in cold water, Lead may be used.

In general the latter cannot be used for the purpose, the tin being strongly attacked by the long continued boiling. A leaden vessel has been substituted for it with success.

Quantity of sugar produced.

The 2 kil. boiled with two hundredths of sulphuric acid yielded, in several comparative experiments, sometimes a little less, sometimes a little more than 2 kil. of sirup at 33° of the areometer [1·295]; so from a mean of them we may conclude, without any material errour, that starch yields its own weight of sirup*.

Many sweet substances contain no sugar.

As many substances have a decidedly sweet taste, for instance sugar of milk, the sweet matter in liquorice, the sweet principle of Scheele (formed during the action of fat oils on litharge in making plasters), without however, containing an atom of sugar, Mr. Vogel thought it necessary to ascertain, in the first place, whether the sweet liquor from starch contained real sugar.

Sugar from starch fermented,

For this purpose he mixed some yeast with 200 gr. [3089 grs.] of sirup of starch in warm water, and put the whole into a phial, communicating with the pneumatic apparatus, by means of a sigmoid tube.

giving out carbonic acid,

Fermentation soon took place, with a very brisk extrication of carbonic acid gas.

and yielding alcohol.

The 200 gr. of sirup yielded by the fermentation upwards of 5 lit. [near 6 quarts] of carbonic acid gas; and a notable quantity of alcohol was obtained by distillation.

Sirup of starch contains gum.

It is certain, that all sirup of starch contains more or less gum, the quantity of which varies extremely, according to the time of boiling, and the weight of the acid employed.

Result of its evaporation.

The most saccharine sirup evaporated slowly in a stove, and dried in tin moulds, afforded a perfectly transparent elastic substance, in every respect similar to the paste of jujubes.

May be employed in pharmacy; but it is deliquescent.

The author has no doubt, that apothecaries may avail themselves of the sirup of starch, for all this kind of gummy saccharine medicaments, particularly those that may remain in a soft state; for the sirup of starch, thus reduced to a solid state, attracts moisture from the air.

Fecula of potatoes equally yields sugar.

Mr. Vogel substituted the fecula of potatoes for starch, and equally obtained a very saccharine gummy sirup.

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* Starch boiled eight hours with four hundredths of sulphuric acid yielded the same results.

The gum was separated by boiling the sirup in a close vessel with alcohol at 30° [0·868.]

The matter on which the alcohol had no action, and which was found in the most perfect sirup to the quantity of two tenths, was very viscous. Being dried and powdered, it exhibited all the characters of gum arabic, namely, its solubility in cold water, forming a thick mucilage, insoluble in alcohol.

The gum similar to gum arabic,

The only character, that appears to distinguish this matter from gum arabic, is its not forming mucous acid with nitric acid.

but does not yield mucous acid.

It has been asserted, however, that the gummy matter precipitated from sirup of starch is a compound of starch, water, and sulphuric acid.

The gum said to be a compound,

To satisfy himself on this head, Mr. Vogel poured a small portion of alcohol into sirup of starch. The precipitate first formed was composed of sulphate of lime and gum. When this was separated, he poured more alcohol into the sirup that had been decanted from it. The second precipitate was gummy matter, unmixed with sulphate: its solution in water was no longer rendered turbid by muriate of barytes.

but this disproved.

The author, however, was not content with this experiment; for it might be objected to him, that the sulphuric acid, being chemically combined with the gum, would not quit it to unite with the barytes. He dissolved this gum therefore in barytes water evaporated to dryness, and gave the mass a strong red heat in a platina crucible: thus the sulphric acid should have been set free, and no doubt would have seized on the barytes. Besides, this sulphate would have been decomposed by the carbon of the gum, and converted into a sulphuret: but muriatic acid poured on the calcined matter extricated nothing but carbonic acid gas, and not an atom of sulphuretted hidrogen gas that could be rendered sensible by paper impregnated with acetate of lead.

Farther confirmation of its containing no sulphuric acid.

Besides, the gum distilled on an open fire did not give out any sulphurous acid, or sulphuretted hidrogen gas.

It is not therefore a hydrate of starch combined with sulphuric acid; which affords us a fresh proof, that we must take care not to frame hypotheses before we consult experiment.

Hypotheses not to be hastily formed.

He made the same trials with the sirup deprived of gum by alcohol, which did not precipitate the muriate of barytes; but he

No sulphuric

acid in the sirup.

he could not discover in it the least trace of combined sulphuric acid.

Action of acids on other substances tried.

These experiments could not fail gradually to lead to an examination of the action of acids diluted with water on some other substances. Sugar of milk first drew his attention; and with the greater reason, as we have already announced this substance to become more soluble in water after it has been treated with acid*.

Sugar of milk treated with sulphuric acid.

Mr. Vogel boiled 100 gr. [1545 grs.] of sugar of milk with 400 gr. of water, and 2 gr. of sulphuric acid at 56° [1·631], for three hours, adding more water as it evaporated. After having saturated the excess of acid by carbonate of lime, he filtered.

The liquid, though clear, was slightly coloured. Evaporated slowly in a stove, a thick brownish sirup remained, which concreted into a crystalline mass at the expiration of a few days.

Saccharine product.

This matter resembling soft sugar has a much more saccharine taste than the most concentrated aqueous solution of sugar of milk. From this extremely saccharine taste the author was led to suspect, that a real sugar had been formed, capable of giving rise to the alcoholic fermentation.

Fermented,

In fact this product mixed with yeast diluted with water was scarcely placed in favourable circumstances for the alcoholic fermentation, before it commenced in a very brisk manner; though sugar of milk never ferments, as is well known to all chemists, and has been recently placed beyond all doubt by the numerous experiments of Mr. Bucholz†.

and yielded alcohol.

This fermented liquor yielded a considerable quantity of alcohol. On varying the proportions of sulphuric acid to three, four, and even five hundredths, very saccharine crystals, that ran into fermentation with extreme facility, were constantly obtained, particularly with five hundredths of acid.

Nitric acid has not the same effect.

With two or with four hundredths of nitric acid the sugar of milk could not be converted into a fermentable sugar.

Muriatic acid has.

Acetic acid has not.

Three grammes [46·3 grs.] of muriatic acid converted the sugar of milk into a very saccharine sirup capable of the alcoholic fermentation; while 2 gr. [30·89 grs.] of radical vinegar made no alteration in the sugar of milk.

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* See Delamétherie's *Journal de Physique*, July, 1811.

† See Delamétherie's Journ. de Physique, for December, 1811.

All

All these sirups reduced to the crystalline state differ from sugar of milk, not only in being susceptible of the alcoholic fermentation, but also in being very soluble in alcohol, a property that sugar of milk does not possess. Evaporated to dryness by a gentle fire, a white, granular, and extremely saccharine mass is the result.

The substance crystallized differs from sugar of milk.

It remains to explain the manner in which sulphuric acid acts on starch and sugar of milk, to take from them the principle that masks the saccharine substance, or to convert them into fermentable saccharine matters. The author confesses, that it is difficult, and out of his power, to give a clear and plausible theory of this metamorphosis; and, if he risk some notions on this subject, it will be with much reserve.

Theory difficult.

Many are disposed to adopt the opinion, that sugar exists ready formed in starch, and that the sulphuric acid only dissolves or destroys the principle that holds it enchained.

Supposition, that the sugar was formed in the starch.

It is obvious, that this reasoning is in a considerable degree vague; and besides, that it is founded on no experiment, direct or indirect. In this hypothesis too we must imagine a compound altogether new, sugar combined with a substance that renders it insoluble in cold water; and sugar has never yet presented us with such a compound.

Objections.

Others have supposed, that heat alone is capable of effecting this conversion of fecula into saccharine matter; a fact which, if it were confirmed, might throw fresh light on the saccharine fermentation of Fourcroy.

Supposition, that the conversion is effected by heat alone.

Accordingly starch has been boiled with water four days in succession, till it became extremely fluid. The filtered liquor was evaporated, and the result was a thick mucilage, very bitter, without the least taste of sugar. The starch remaining on the filter resisted the action of boiling water, and exhibited a very hard horny matter.

This disproved.

It remains to be examined, therefore, whether the sulphuric acid, or the starch itself, be decomposed.

Is the acid, or the starch, decomposed?

To judge by the letter from Petersburgh, the Russian chemists seem to suppose, that a decomposition of the sulphuric acid takes place.

The Russians seem to think the former.

To account for these phenomena, we should operate in close vessels. Accordingly, the author introduced into a tubulated receiver

Experiment

with sugar of milk.

receiver a hundred grammes of sugar of milk, four of sulphuric acid, and four hundred of water. To the neck of the retort was adapted a tubulated receiver, from which proceeded a sigmoid tube, opening under a jar filled with water.

After boiling for three hours, no gas had come over, except the air contained in the vessels. A piece of blue paper introduced into the neck of the retort was not reddened. The water that had passed into the receiver was without taste, did not redden litmus paper, had no smell of sulphurous acid, and did not precipitate lime-water, muriate of barytes, or acetate of lead; consequently it contained no sulphurous, sulphuric, acetic, or carbonic acid; in short, it was nothing but pure water.

Barytes-water traversed by the bubbles, extricated during the process, was not rendered turbid in the least, and the gas that had passed into the jars was nothing but the air of the vessels.

The sugar of milk decomposed, not the acid.

It is evident, that the sulphuric acid had not undergone the slightest decomposition: nevertheless, the sugar of milk was decomposed; it had a much more saccharine taste, and after saturation with chalk it fermented very readily with yeast.

Experiment repeated in close vessels.

It was necessary, therefore, to examine the decomposing action of the sulphuric acid on the substances in question. For this purpose the same experiment was begun afresh in close vessels, with 100 grs. of sugar of milk, 400 grs. of water, and 4 grs. of sulphuric acid. During the process no gas was evolved, as in the preceding experiment.

The liquid was then concentrated in a dish accurately weighed, after having added 5 grs. of potash to saturate the acid.

The mass thus evaporated to dryness should have weighed 109 grs. in consequence of the 100 grs. of sugar of milk, 4 grs. of sulphuric acid, and 5 grs. of potash employed; but it weighed only 98 grs. consequently there was a loss of 11 grs. This experiment was repeated twice more, and there was still a loss of 9 or 11 grs. giving a mean of 10 grs.

This loss is too great to be ascribed to any errour in the weighing, which was conducted with the greatest care.

Water apparently formed.

Hence we must conclude, that this diminution of weight is occasioned by a quantity of water formed at the expense of the sugar of milk; and this with the more reason, as no gas,

gas, no acid, and no other volatile substance, was extricated during the boiling.

All these experiments with the sugar of milk were equally repeated with starch, except that a much larger quantity of water was added to prevent it from burning. The results were the same as those obtained with sugar of milk.

Starch afforded similar results.

Conclusions.

From all that has been said, it follows :

General conclusions.

1. That starch and the fecula of potatoes, boiled with water acidulated with sulphuric acid, are converted into a liquid saccharine matter, the quantity of which corresponds with the weight of the starch employed.

2. That this saccharine matter is susceptible of the alcoholic fermentation.

3. That the sirup of starch is composed of gummy matter and saccharine matter in variable proportions.

4. That the sirup evaporated slowly in a stove exhibits an elastic substance, perfectly transparent.

5. That the gummy matter exhibits all the characters of a true gum, except that of forming mucous acid by means of the nitric.

6. That neither this gum, nor the saccharine matter, holds sulphuric acid in combination.

7. That the heat of boiling water alone is insufficient to convert starch into saccharine matter, as nothing is obtained but a bitter matter, and a horny substance insoluble in boiling water.

8. That sugar of milk treated with two, three, four, or five hundredths of sulphuric acid is converted into confused crystals, which have an extremely saccharine taste, and are susceptible of the alcoholic fermentation.

9. That this saccharine matter does not contain any sulphuric acid in combination.

10. That the muriatic acid effects the same changes in sugar of milk.

11. That neither the nitric nor acetic acid converts sugar of milk into fermentable sugar.

12. That sugar of milk thus converted into fermentable sugar becomes very soluble in alcohol.

13. That sulphuric acid is not decomposed in its action on starch and sugar of milk: and that, from the facts mentioned, it is much more probable, that the acid takes from these substances oxigen and hidrogen in the proportions necessary to form water.

VII.

Abstract of a Paper on the Deliquescence of Bodies; by Mr. GAY-LUSSAC*.

Deliquescence reducible to general principles.

ON the 17th of May [1812] I communicated to the Society of Arcueil some observations on the property that bodies have of attracting the moisture of the air, and which is more particularly designated in chemistry by the name of deliquescence. This property, hitherto badly analysed, may be reduced to general principles, by which we may easily ascertain what bodies possess it, the variations it undergoes according to the temperature, and the degree of the hygrometer at which it begins to manifest itself.

Owing to the attraction of a substance for water.

Mode of ascertaining it.

Exists where it had not been suspected.

As the deliquescence of a body is owing to its affinity for water, and as the effect of this affinity is to diminish to a certain degree the elastic force of the vapour contained in a determinate volume of air, it is very essential, both for knowing whether deliquescence can take place, and for obtaining comparable results, to place each body in an atmosphere completely saturated with moisture. Thus we find, that muriate of soda, sugar, &c., are very deliquescent; and that even nitre, and many other substances, in which this property had not been observed, possess it more or less.

Temperature to be attended to.

We cannot thus ascertain the degree in which a substance is deliquescent: but, to accomplish this, we must first observe, that, the deliquescence of a substance depending on its affinity for water, and this affinity itself being strikingly modified by heat, it is necessary to consider each temperature in particular.

Method of finding the de-

Suppose then a given substance, solid or liquid; and we wish to know its degree of deliquescence in an air saturated with

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* Ann. de Chim. vol. LXXXII, p. 171.

gree in which a substance is deliquescent.

with moisture at 15° of the centigrade thermometer [59° F.]. If it be solid, first make a saturated solution of it in water at 15° [59° F.], and boil the solution[*]. If it boil at 100° [212 F.] the boiling point of pure water, the substance is not deliquescent: but if it do not boil at so low a degree, it is more deliquescent in proportion as the boiling point rises higher above 100°. Thus muriate of soda will be very deliquescent in air saturated with moisture, for its solution in water at 15° [59° F.] will not boil below 107·4°. [225·32° F.]. Nitre, too, will be deliquescent, but much less than the preceding salt, as its solution at 15° boils at 101·4° [214·52 F.].

The theory agrees with experiment.

Treatment of slightly deliquescent substances.

Experiment here perfectly agrees with the theory; but to have a good view of the deliquescence of nitre, and of all substances like it feebly deliquescent, they must be taken in small separate parcels: in this state they will be found to dissolve completely, while large crystals would only be covered with a liquid stratum, or would dissolve very slowly.

Importance of temperature.

Nitre.

It is easy now to perceive how important it is to attend to the temperature; for as heat greatly favours the combination of salts with water, the boiling point of each solution will vary according to the temperature at which it is made. Thus nitre, which is but slightly deliquescent at 15°, the saturated solution of which boils at 101·4, would be greatly so at the temperature of 100° [212°], as the solution saturated at this temperature would boil only at 110° or 112° [230° or 233·6° F.]

Salts not deliquescent.

Acetate of lead and corrosive sublimate do not perceptibly retard the boiling of water; and accordingly they are not at all deliquescent.

Liquids boil at a lower temperature in

In ascertaining the boiling point of saline or acid liquors, I observed a singular phenomenon, that deserves to be made known. It consists in this, that water, or any other liquid,

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[*] I shall here observe, that, instead of taking the boiling point of each liquid, it would be more accurate to take the force of its vapour at the temperature at which we would determine its degree of deliquescence, because the elevation of the boiling point is not proportional to the quantity of salt held in solution. Similar means should necessarily be employed to know the force with which solids attract the vapour of water, without any change in their state ensuing, as would take place with lime and salts deprived of their water of crystallization. This subject is treated at length in my original paper.

does

metal, than in glass, unless this contain some insoluble powder.

This may affect the graduation of thermometers.

does not boil so soon in a glass vessel as in one of metal, unless filings of iron, copper, or some other metal, powdered charcoal, or pounded glass, be put into the former. The difference of temperature for water reaches to 1·3° [2·34° F.], and sometimes even more. This fact is of the more importance in the graduation of thermometers, as we may observe a similar difference between two of these instruments made with equal care, but the upper points of which were taken one in a glass, the other in a metallic vessel. It is true, that if care be taken not to immerse the ball of the thermometer in water, the difference will be less.

No salt lowers the boiling point.

I have found too, that no salt possesses the property of lowering the boiling point of water, though Mr. Achard has asserted the contrary.

Degree of the hygrometer at which a salt will deliquesce ascertainably.

Knowing the boiling point of each solution, by means of which we have a measure of the deliquescence of the salt, and of its affinity for water, we may go farther, and ascertain the degree of the hygrometer at which deliquescence begins to take place. All that is necessary is to place the hygrometer under a jar moistened with the saline solution, and observe the degree it will point out at the expiration of a few hours. Thus it will be found, that with a solution of muriate of soda, saturated at 15° [59°], the hygrometer will stop at 90°; with a solution of nitre made at the same temperature, it will stop at 97°, or thereabout, &c.

Muriate of soda.

Hence we may conclude, that muriate of soda will not be deliquescent below 90° of the hygrometer, but will begin to be so at this point, and become much more deliquescent beyond it. When a table indicating the degrees of the hygrometer corresponding to the boiling point of a certain number of salts is constructed, we may determine the degree of the hygrometer at which all the others will begin to be deliquescent, as soon as we know the boiling points of their aqueous solutions. I need not observe, that what is applicable to deliquescent salts is likewise so to all the solid or liquid bodies that have any affinity for water.

Sulphuric acid absorbs more than 15 times its weight of water.

On these principles we shall find, that concentrated sulphuric acid is capable of taking more than fifteen times its weight of water from air completely moist. In setting out from this property of various saline solutions having different degrees of elasticity at the same temperature, it is easy to determine with precision for every temperature, and every degree of the of the hygrometer, the quantity of vapour contained in a given volume of air; which Saussure could not do, notwithstanding his accuracy, on account of the imperfection of his processes.

Quantity of vapour in air ascertainable.

This method, which I have already pointed out, consists in taking liquids, from which nothing but water is separated by heat, and boiling them at very different temperatures; for instance, sulphuric acid more or less diluted; placing the hygrometer underneath jars wetted with each of these liquids; and observing the degree at which it stands. On the one hand we know from my experiments the density of aqueous vapour, which is to that of air as ten to sixteen; on the other we know the boiling point, or elasticity of each liquid enclosed under a jar with the hygrometer: consequently we have all the necessary data for the solution of the problem in question. On this I am at present employed, and I trust it will not prove uninteresting to the science of hygrometry.

Process.

The author employed on it.

VIII.

Remarks on the Correspondence between Dr. Bostock *and Dr.* Marcet, *on the subject of the uncombined Alkali in the animal Fluids. In a letter from* George Pearson, *M. D. F. R. S. &c.*

To William Nicholson, Esq.

George Street, Hanover Square, Oct. 27, 1812.

SIR,

IN your Journal for the present month I read the letter of Dr. Marcet, addressed to his friend, Dr. Bostock; in which he offers the evidence of some experiments to prove, that the potash which exists in the animal fluids is in the state of muriat (muriate), and "that the whole of the uncombined alkali is soda."

Introduction, by reference to Dr. Marcet's letter to Dr. Bostock, and the remarks thereon by Dr. B.

It appears, that Dr. Bostock was of opinion, that the supposed uncombined alkali was potash, according to my provisional conclusions, and not soda: but on the representation of evidence just mentioned, he has changed his opinion, and therefore has become the vehicle of Dr. Marcet's letter to the public;

public; confiding, as he says, Mr. Editor, that you will assent, that the evidence offered "*must entirely set the question at rest.*"

Statement that new facts, instead of conclusions supported by authority, ought to have been produced.

One authority declaring the question to be entirely set at rest, and the other affirming that every shadow of doubt is now removed, although I was not ready to believe, as I have had occasion to assert, that more than provisional conclusions are likely to be obtained, I at least expected to find some new contravening testimony. This I was prepared to acknowledge; for, reasoning merely from the known facts, I should have felt no humiliation if new evidence indicated adverse conclusions—"Nos non judicis sed indicis personam sustinemus." (*Bacon.*) But on examining the evidence, which it is asserted has produced conviction, "removed every shadow of doubt," and "set the question at rest," I was unable to perceive any new facts to alter my former conclusions; hence I might have replied merely by a counter-declaration and reference to my unanswered experiments and inferences. As, however, this mode of procedure may be deemed neither decorous to my opponent, and the testimony produced of respectable personal authority, nor satisfactory to the public, I respectfully offer the following brief exposition and remarks.

Dr. Marcet's process stated.

The process, which Dr. Marcet says authorises his confidence in former conclusions, was this. The saline matters of the serum of blood were procured by evaporation to dryness, incineration, dissolution in water, filtration, evaporation again to dryness, dissolution in acetic acid, dissolution again of the desiccated acetic compound in alcohol; evaporation of this to dryness, and fusion. The fused mass, amounting to about four grains, was divided into four parts, a, b, c, d.

I. a. 1. "contained abundance of muriatic acid."

2. Dissolved in water, and suffered to evaporate spontaneously, an efflorescent mass of feathery crystals was afforded.

3. Tartaric acid and oxymuriate of platina manifested the presence of potash.

Now, I can only infer from these experiments, that a muriate was present, probably either of soda, or of potash, or of both—that potash was present combined, but with what substance is quite equivocal; being only a small fractional part of a grain, it may be united to a double salt, although weakly, yet so as

to be

to be no longer deliquescent. It may also be united to muriatic or other acids, especially the sulphuric and carbonic; but here is no evidence of soda in a free state, and even only equivocal evidence of it as united to muriatic acid.

II. The portion b, with sulphuric acid, gave sulphate of soda, and sulphate of potash.

Here he testimony is equivocal; for the soda may be, and most likely was, from the decomposition of muriate of soda by the sulphuric acid. And the sulphate of potash may arise from the decomposition of potash united to some acid, such as carbonic, muriatic, &c. united, though weakly, to the other salts.

Hence I perceive no evidence of soda in a free state.

III. The portion c, with nitric acid, afforded rhomboidal crystals, and no prismatical crystals.

I will not repeat the objections I urged to any conclusion from the form of crystals, especially in such minute portions of matter as a small part of a grain, set forth so fully in a former paper; but it may be right just to remark, that this experiment is inconclusive and unsatisfactory: 1. because if all the crystals were nitrate of soda, then all the saline mass must have been soda; and 2dly, if only a part was soda, and the rest was muriate, then this must have been decompounded by the nitric acid: but 3dly, if this could happen, then the whole of the rhombs might be from the decompounded muriate of soda: 4thly; if the whole of the crystals were rhombs of nitrate of soda, what became of the cubical crystals of muriate of soda?

IV. The portion d, with oxymuriate of platina, gave a precipitate of potash oxymuriate of platina, and by evaporation soda-muriate of platina.

Here the questions occur, 1. what are the proofs of soda muriate of platina? 2. What are the proofs, that soda muriate of platina was from free soda, and not from muriate of soda?

Why the alkaline mass did not deliquesce.

To omit nothing supposed to be favourable to the adverse party, it must be noticed, that "the carbonaceous alkaline mass above spoken of after fusion did not deliquesce on exposure to even damp air." I never met with such a result, at least with expectorated matters, and dropsical fluids; and if no deliquescence took place with the salts of serum of blood, it is not unreasonable to account for it from the very small proportion

portion, probably not one fourth of a grain, or at most half a grain of alkali, in the whole mass; and this by fusion might be united to form a compound unknown.

Objections to Dr. Marcet's inference.

To the inferences of my adversary I also object. 1. That it is assumed, without testimony, that alcohol dissolves a large proportion of muriate of potash. It is, I believe, admitted, that this menstruum dissolves none at all; but if this be an errour, I demand the proof.

2. It was not admitted, as I reasoned, that acetate of soda is nondeliquescent; and therefore the proof I offered of the alkali being potash from the deliquescent property of the acetate was eagerly seized to expose my ignorance, by exultingly exclaiming, that I had committed *a palpable errour.* I acknowledge, that I had taken for granted, with most chemists, what I subsequently admitted was not a fact; but I am now in a doubtful state of mind, with regard to this property; for professor Berzelius confidently assures me, that acetate of soda was found, by repeated experiments, to be uniformly nondeliquescent: and on observing, that in my experiment I had found it otherwise, we agreed, that probably the different results were owing to the soda I used containing a proportion, however minute, of potash, and which I could not perceive by tartaric acid, whereas that he employed was exempt. If this be true, it will be a stronger proof, that the alkali is potash, than the united testimonies produced to prove that it is soda.

3. Dr. Marcet argues, that from principle it may be inferred, that soda, and not potash, is the impregnating alkali, because the latter attracts muriatic acid more strongly than the former. This is true in the circumstance of single elective attraction; but any reasoning from this law, when more than one menstruum is present, and two or more bases, is fallacious; especially when the different substances present are not certainly known. And here I must observe, that I have never contemplated potash as existing in an uncombined state in the animal fluids, but in reality in combination with a destructible acid, or with animal oxide. This acid, from some trials I was inclined to propose, is the malic acid; but I did not venture to offer it to notice, although I did not abandon the notion: however, I find from the conversations of professor Berzelius, now in London, that he coincided with me in an analogous, if not a simi-

lar

lar result. "You very nearly," said he, "made a capital discovery; for I have ascertained, that it is *the lactic acid* in union with the alkali of the animal fluids." I hope the British public will soon be edified by the translation from the Swedish language of a work of this most acute chemist, and, as I hear, by a most able editor. Hence much light will be afforded, especially in animal chemistry. This fact is, however, only within the record before us, to repel any *a priori* conclusion from a case of simple elective attraction. I had long considered the case of this kind noticed by my opponent, for it was too glaring to be passed by. If reasoning from principle could be depended upon, I would argue; that, as all animals either immediately or mediately live upon vegetables, and as vegetables very generally contain potash combined with acids, or other things destructible by fire, it is reasonable to conclude, that the fluids of animals must be impregnated with potash in such a state of combination.

I know it has been argued by some able chemists, that the potash must be united to muriatic or sulphuric acid, and soda must be united to some weaker acid, such as carbonic, lactic, acetous, malic, &c., agreeably to the assumed law, that the stronger menstruum unites with the stronger basis, and the weaker menstruum with the weaker basis. But there are so many exceptions to this rule, that it cannot be justly termed a law.

Mere general statement of the result inadmissible testimony.

Lastly, in his P. S. Dr. Marcet says he has instituted the process above examined on a very large scale, on a large quantity, some gallons, of bullocks' blood, with the same results as on small quantities of animal matter. I believe such evidence is inadmissible; for if mere general statements of results be received as testimony, much errour will be liable to be introduced; as the public in these cases cannot be in possession of the means of repeating the experiments, and judging of their accuracy. It is to be regretted, that the author did not render his experiments instructive, by the necessary detail; however, if they were a mere repetition of former ones, the questionable fact would still remain undetermined.

The chemical world may now perhaps be furnished with the means of judging whether or not Dr. Marcet has removed every shadow of doubt by legitimate inductive reasoning.

Authority no proof.

My opponent, not content with proofs by experiment, has endeavoured to command assent by a most respectable authority of opinion. But *Truth* is not the daughter of mere human authority, but of *Time*, producing testimonies of sense and of reason.

I beg to have permission to make a very few remarks, which, although justifiable, yet being personal, will afford but lenten entertainment, and still less instruction, to the public. In making this authority the vehicle of his letter, my opponent thinks proper to express disapprobation of my mode of controversy, and to more than insinuate, I should not have been honoured with further notice, but for the "interference" of his friend. Accordingly, but for this fortunate circumstance the public would not have been instructed by his letter before us. This conduct, I own, I think is rather selfish; for a public-spirited man will always make sacrifices of his feelings for the benefit of the republic. It is, however, good, that the *interference* overcame the resolution after four months' obstinate resistance. The head and front of my offending was, it seems, to the extent of an attempt to be jocular, in which I never meant to inflict any wound on the feelings. It grieves me, certainly, to find, that some of my expressions were construed, insidiousness—"Non vulnera fidelia amantis, sed oscula blandosa malignantis." In the endeavour to expose the inefficiency of the proposed method of investigation, and to honour illustrious chemists, whose successful methods were unworthily disvalued, I preferred the manner of controversy complained of, to the alternative—a serious remonstrance. For as my affectionate friend, the Prince of Philologists, (now no more!) was wont to say, "Cantantes minus via lædit."

Reasons why the author preferred a jocular tone.

In conclusion; I would fain hope, that, if this warfare must be continued, special care will be employed, that nothing be said or arise, which can reasonably excite* painful sensations in either party. And if it be agreed, that our axioms and conclu-

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* I have no where charged Dr. M. in the terms alleged, that he had committed *blunders*. I can well spare the word blunder from my vocabulary, having little use for it, although by the law of retaliation amply justifiable.

sions

sions are but inductive reasonings, according* to the known facts, which therefore are liable to be subverted by the facts being multiplied, whatever be the issue, no humiliation ought to be experienced, as the parties will moult no feather.

I have the honour to be,
Dear Sir,
Your most faithful Servant,
GEORGE PEARSON.

IX.

On Hygrology, Hygrometry, and their Connexions with the Phenomena observed in the Atmosphere. By J. A. De Luc, *Esq.* F. R. S.

To W. Nicholson, Esq.

SIR,

Atmospheric phenomena important to the knowledge of the nature of water:

IN the third part of my paper on the *electric column*, published in your Number 124, for December, 1810, where I have considered that instrument as an *aerial electroscope*, I have shown the importance of studying all the *atmospheric* phenomena, before a final decision could be obtained of the question agitated for some time, on the *nature* of *water*; whether it is a *compound* or a *simple* substance; a question which embraces the whole *theory of chemistry*. Especially I hope to have made it evident in that paper, that, since *atmospheric* phenomena are to be considered in the solution of the above question, we ought to study particularly all those of the *electric fluid* in the atmosphere; to which we might be led by the phenomena of the *aerial electroscope*, provided we did not connect them with arbitrary hypotheses, nor forget to take into consideration the *nature* of the *electric fluid*, which, from the great phenomena of *lightning* and *thunder*, has evidently a great share in *meteorological* appearances. My papers, Sir, in

and particularly those connected with electricity.

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* Experientiæ ordo primò lumen accendit, deinde per lumen iter demonstrat, incipiendo ab experientia ordinata et digesta, atque ex ea educendo axiomata, atque ex axiomatibus constitutis rursus nova experimenta.—Nov. Organum F. Baconis.

in the last numbers of your Journal, were destined to show, from our own experiments and atmospherical observations, what are the *nature* of the *electric fluid,* and its interference in meteorological phenomena ; and I now come again to the same subject, under another point of view.

Rain not from moisture in the air :

1. My observations of the *aerial electroscope,* published in your No. 124, show, that the changes in the phenomena exhibited by this instrument have no connexion with the state of *moisture* in the *ambient air.* I proved also, in the same paper, this important point in meteorology, that *rain* does not proceed from a *moisture* actually existing in the atmosphere. This, if it be certain, overturns the new *theory of chemistry ;* for thus *rain* cannot proceed from any other cause than that of a *decomposition* of the *atmospheric air* itself, a fluid *sui generis,* the *ponderable* part of which must be *water.*

but the ponderable part of the air itself.

Grounds of the conclusion.

2. But this conclusion rested on the indications of the *hygrometer,* Mr. De Saussure's observations, and my own, on high mountains; in the very region of the atmosphere we saw the *clouds* forming around us, and pouring *rain,* while an instant before our *hygrometers* testified, that there was very little *moisture* in the *air.* But here a question arises : is the *hygrometer* an instrument to be depended upon, for the purpose of indicating the real quantity of *moisture,* or *evaporated water,* mixed with the *air,* in the place where it is observed ?

Can the hygrometer be depended on ?

This an important question.

3. This, Sir, is a very important question, as well in natural, as in experimental philosophy ; and I wish, through your valuable Journal, to attract the attention of your readers to this instrument. I had very little hope of success on this point, when I wrote my preceding papers in your Journal ; because, from a circumstance which I shall explain hereafter, none of my *hygrometers* could be found ; but it is not the case now.

Progress made in the inquiry.

4. I had already made some progress in the correspondent researches of the indications of the *hygrometer,* and the phenomena of *rain and fair weather,* when, in 1786, I published in London my work, *Idées sur la Météorologie** ; but I had carried them much farther, when I delivered to the Royal Society my papers on *hygrology* and *hygrometry,* published

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* This work may be had of Messrs. Dulau and Co, booksellers in Soho Square.

in the *Phil. Transactions* for 1790 and 1791; the subjects of which I shall here shortly explain, for those of your readers who do not possess the *Phil. Trans.*

The hygrometer a perfect measurer of moisture in the air.

5. There is no physical instrument, the name of which terminates in *meter*, as used for *measuring* the *intensity* of the *cause* acting upon it, so deserving that name, as the *hygrometer* described in these papers; for this instrument alone has the property of measuring the *whole extent* of the cause which influences it; which *extent* is comprised between two natural and opposite *extreme points*, one of which I shall first describe: it is *extreme dryness*, or absence of all *moisture*; which, therefore, is an absolute 0. I have proved, in the above papers to the Royal Society, that this point is effectually obtained, by placing the *hygrometer* in a close vessel, filled previously with a sufficient quantity of fresh calcined *lime*, taken *red-hot* from the kiln.

Point of extreme dryness.

Principle on which the ascertaining it is founded.

6. The principle which led me to this method is, that, *evaporation* being produced by *heat*, if *red-heat* is not destructive of a *hygroscopic body*, it must occasion the *evaporation* of all the *uncombined water* the latter contains in its pores. And by previous experiments on various bodies of that kind, I found, that *lime*, passing from *red-heat* to *extreme moisture*, increased in proportion of nearly *half* its *weight*. I fixed therefore, upon *lime*, and I employed a large vessel, which I filled with *red-hot lime*. When it was cool, that vessel having at the top small openings for introducing the *hygrometers*, (after which they were closed, and opened only for taking them out,) I took thus the point 0 on a great number of various sorts of *hygrometers*, of which I shall speak hereafter. I have described this vessel in the Phil. Transactions; it is cylindrical, 1 foot diameter, and 3 feet high; I have it still, and when I place in it one of the *hygrometers*, the 0 of which had been fixed in it 10 years ago, I do not find any sensible difference in this point. Thus, therefore, the point of *extreme dryness* is perfectly ascertained.

Not varied by time.

Point of extreme moisture.

7. As to the opposite point, that of *extreme moisture*, I have proved in the same paper, that it was surely obtained by immersing the *hygrometer* into *water*; where it soon attains a point, beyond which it does not go, whatever length of time it remains

Scale.

remains there. This point I have called 100, and the scale is divided into 100 parts.

Inquiry after the fittest substance for hygrometers.

8. Another important object treated in the same paper, and which occasioned me much labour, was, of what substance the *hygrometer* should be constructed. On this particular point I related a long series of experiments, occasioned by the first results I obtained by trying many kinds of *animal* and *vegetable* substances: some of which could be used in thin *threads,* torn in the *length* of their *fibres*; and also in thin *slips* cut *across* the *fibres.* Now, I found, that when used in the *length* of the *fibres,* their *lengthening* by *moisture decreased,* and at last they were even *shortened,* while the same substances cut *across* the fibres continued to *lengthen :* which at first embarrassed me very much*.

Exp. to find whether substances cut lengthwise imbibed moisture while shortening.

9. I could not decide immediately from these observations, whether the substances taken in *length* continued to imbibe *moisture,* while, however, their *length* was *decreasing*; and in order to ascertain this necessary point, I contrived a vessel, described in the same paper. In that vessel I enclosed together several pairs of *hygrometers,* made of the same substances ; in one, it was used in the *length,* and in the other *across* the *fibres*; and a *beam,* indicating the 500th part of a grain, to which I

Reason why the substance should be cut across the grain.

* The reason of the difference in the successive expansion by moisture of the same fibrous substances, taken in the *length* and *across* their *fibres,* proceeds from the nature of these substances. The main *fibres* in their *length* are united by *fibrils,* which are seen when we split these bodies. These small *fibres* form with the larger ones a sort of *meshes,* similar to those of a *net.* The first effect of *moisture* is on the *longitudinal* fibres, which it *lengthens ;* but when it penetrates the *meshes,* it *widens* them, and thus *shortens* the body ; as the *length* of a *net* is *lessened* by stretching it *across. Moisture* therefore acts in two opposite ways on the *fibrous substances* taken *in length,* differently in its progress on the same substance, and differently also in different substances. And besides, the whole lengthening is very small in all of them. Now, one of these effects is suppressed by taking the same substances *across* the *fibres,* namely, that which acts on the length of the latter ; there remains only that which acts on the *breadth* of the *meshes,* which, if not absolutely proportional to the increase of *moisture,* is never in an opposite sense. Besides, there is a great gain with respect to the extent of the lengthening, and therefore of the *degrees* of the *hygrometer ;* for instance, a *slip of whalebone,* by passing from *extreme dryness* to *extreme moisture,* increases nearly *one ninth* in *length.*

suspended very thin *shavings* of the same substances as the enclosed *hygrometers*; which *shavings* indicated, by the increase of their *weight*, the *weight* of the *water* which penetrated them. I had a *lime-vessel* by which I first produced *extreme dryness* in the vessel containing the instruments; and when I had observed them in that state, and taken off the vessel containing the lime, I had also a manner of increasing *moisture* by degrees in that of the instruments, observing at each step the motions of the *hygrometers*, and the increase of weight of the *shavings*.

Results.

10. The general results of this experiment were the following:—1. That substances taken in *length* continue to imbibe *moisture*, though they cease to *lengthen*, and some even begin to *shorten*. 2. That *slips* cut *across* the *fibres* continue to *lengthen* so long as the *moisture* increases. 3. That the *slip* of *whalebone* follows very nearly in its *lengthening* the rate of the increase of *moisture*, indicated by the increase of *weight* in its *shavings*. From this last result, and from the great elasticity of this substance, which makes it always sensibly return to the same *length* with the same *degree* of *moisture*, I fixed on a *slip* of *whalebone* for my *hygrometer*.

Whalebone preferable.

The instrument little known:

11. Such was the point which I had attained, when I delivered my papers to the Royal Society; thus concluded by the determination of an absolute and comparable *hygrometer*, which was wanting in the set of meteorological instruments commonly observed: but by an unlucky circumstance, it still remains little known, and thus enters very seldom into the considerations concerning meteorological systems. I had directed, in the construction of that instrument, a very able German instrument-maker in London, Mr. Haas; but after he had sold a few, he was engaged to go to Portugal, with a pension from the government; and since that time, no other instrument-maker had undertaken to construct it. But lately a Hanoverian gentleman, Mr. Hausmann, who lives now at Cumberland lodge, near Windsor, seeing that it was a very important instrument for meteorology, has undertaken its construction, and having succeeded, he is disposed to make it for those experimental philosophers, who may wish to have it.

but now may be bought.

The quantity of water in air answering to

12. So far, however, as may be seen in the above account of these experiments, I had only obtained a *ratio* between the

quantities

given degrees of the hygrometer still remained to be found.

quantities of *moisture*, and the *degrees* of my *hygrometer*; or what *part* each *degree* was of the *whole*: but I had not obtained a knowledge of the *absolute* quantity of *evaporated water*, which, in a given bulk of *air*, corresponded to these *degrees*; a knowledge very essential in the investigation of the *cause* of rain. I saw that this was at least necessary for obtaining more certainty in meteorological conclusions. I relied in this respect on Mr. De Saussure's experiments, as I had not yet had time to undertake them myself; but I thought then to repeat the same experiments, for the following reasons.

Mr. De Saussure's experiments objected to.

13. Mr. De Saussure had made these experiments with his *hair-hygrometer*, which was so dissimilar to mine in the rate of *lengthening* with the same *increases* of *moisture*, that his results could not be immediately applied to my instrument. But especially, he had made all these observations in the course of one day; so that he could only obtain a few immediate points of comparison, whence he deduced a general law of the correspondence of the *degrees* of his *hygrometer* with the *quantities* of *evaporated water* in a given *bulk of air*. This was a first reason why some natural philosophers did not admit the results of his experiments. There were also some other reasons, which I shall hereafter mention: but these results were so important in meteorology, as he himself explained, that I resolved to repeat the same experiments in such a manner, as to remove all the objections, which I clearly saw could only affect the exactness of his experiments, but not their main results. I shall now mention all these objections, and the manner in which I proposed to remove them.

The author resolved to repeat them.

1st. objection.

14. The first objection, as I have said above, was the short time employed in his experiments, to which he had been obliged by the nature of his vessel: I therefore wanted to use a vessel in which I could prolong these operations as long as I should find it necessary. A second objection had been made against the manner in which he first produced *extreme dryness* in his vessel, which was by new-calcined *salt of tartar*; a substance which has *chemical affinities* with *water*, and might absorb *air* with it: I wanted therefore to use new-calcined *lime*, as I had used it for fixing the point of *extreme dryness* on my *hygrometers*. Lastly, there was an objection against the manner by which he had determined the *quantities* of *evaporated water*

2nd. objection.

3d. objection.

water in his vessel: it certainly could not be very exact; but it was sufficiently so, for the final and most important conclusions of a first attempt of these experiments. However, these objections had rendered the greatest number of experimental philosophers inattentive to this great step concerning meteorology, so that it was almost forgotten. This was my first motive for undertaking the same experiments with the precautions above explained.

The experiments difficult.

15. I found this attempt much more difficult than I had expected; for it cost me more than two years in useless trials, for obtaining, first, a vessel which would remain *air-tight* during all the time that these experiments should require. At last, however, I succeeded, and the experiments themselves took me afterward more than one year. These experiments are related in a work which I published at Paris, in 1803, under the title of *Traité élémentaire sur les Fluides expansibles:* but on account of the present circumstances of Europe, and this work being in French, a few copies only are come to England. This, Sir, makes me desirous to consign to your Journal a short account of these experiments.

Related in a French work.

Object of the experiments.

16. My purpose was to ascertain what quantities of *evaporated water* in a known *space* of *air* corresponded to each *degree* of my *hygrometer*; and I determined, that this *space* should be one *cubic foot*. My first success in overcoming the difficulties was that of obtaining a vessel, which would remain *air-tight* during the whole course of these experiments. I found, that no vessel could be rendered air-tight so long, which had a large opening at the top; and that therefore this opening should be only what was necessary to introduce the instruments into it. I then procured a *glass vessel*, about 23 inches high, and 8½ in diameter, the opening of which at the top was only 2½ inches in diameter. I measured the *capacity* of this vessel; it was not quite one *cubic foot*; but I ascertained the differences to which I was to proportionate the quantities of *evaporated water*, so that they might be as 1 *grain* in a *cubic foot*.

Vessel for making them in.

Method of ascertaining the quantity of water evaporated in a vessel with certainty.

17. Before that time, I had found a sure method of ascertaining the quantities of *water* successively evaporated in a vessel, without opening it; in order to prevent any exchange of the internal with the external air, lest the latter should introduce

duce some moisture with it. This method was to enclose equal quantities of *water* in very thin and small *glass bubbles*, with a neck drawn to a very small point, easily sealed with the flame of a taper; and before this last operation, I determined the quantity of *water* that each contained, by a beam which indicated 1000th part of a *grain*. These *glass bubbles* were placed in the upper part of the vessel on a circular stand, and I had, outwards at the top, a mechanism for breaking them without opening the vessel. This method I applied to the glass vessel above mentioned.

Mr. de Saussure showed, that rain is not condensed by cold.

18. Such were the means which I employed for ascertaining the quantities of *evaporated water* in a *cubic foot* of air, acting on the enclosed *hygrometer*. But these experiments required another condition, which Mr. de Saussure had already introduced in them: because those natural philosophers, who attributed *rain* to the *moisture* in the atmosphere, had supposed, that this *moisture* was condensated by *cold*. Mr. De Saussure had sufficiently proved, that it was not the case, by observing the effects of the changes of *temperature* on his enclosed *hygrometer*. I was therefore to introduce the same condition in my experiment, and for this purpose I enclosed also in my vessel a *thermometer* with Fahrenheit's scale. Lastly, as I intended to make the same observations on every successive *grain* of *evaporated water*, which would take a very long time; having previously found that *extreme moisture* was produced in the vessel by a small number of *grains* of *water*; and even that they could not undergo great changes in the degree of *heat*, without some *water* being deposited in the sides of the vessel: this obliged me, in order to obtain the same temperatures in the observations of the effects of each successive *grain* of evaporated water in the vessel, to make these experiments only in the spring and the autumn; because, in these seasons, I could obtain naturally almost every day in my room the temperatures of 50, 55, 60 of Fahrenheit, on which I fixed for all these experiments. By this method I was sure, that the temperature would be always the same in every part of the vessel it being that of the air in the room.

Thermometer enclosed in the vessel.

Extreme moisture produced by a few grains of water to a cubic foot of air.

The experiments confined to spring and autumn for uniformity of temperature.

Two series of experiments made.

19. I made two series of these experiments; one beginning in the autumn of 1795, and ending in January, 1796; the other beginning in the autumn of 1796, and terminating in February,

February, 1797: each of them began by producing *extreme dryness* in the vessel, and proceeded by the *evaporation* of successive grains of water; observing afterward the changes produced on the *hygrometer* at the three fixed *temperatures*. In the course of these experiments I had a proof, that the vessel remained *air-tight*. For in order to ascertain the effects of the increase of water at the three temperatures, I consecrated many days, even weeks, to the observation of each step, by repeating it many times; which made both sets of experiments last near 6 months: however, I found no sensible difference in these observations from the first to the last day, with every quantity of water; and in ending them, I had an immediate proof, which it would be too long here to explain, that the *aqueous vapour*, which had been produced in the vessels, had added its expansibility to that of the *air* originally enclosed in it.

The vessel remained air-tight.

The experiments many times repeated.

20. This, I think, was a complete determination of the correspondence between the *degrees* of my *hygrometer*, and the *quantities* of *evaporated water* in one *cubic foot* of air, at the observed degrees of *heat*. I then undertook to derive from these experiments general rules of *hygrometry*. These deductions begin at p. 325, of the 2d vol. of the above-mentioned work; they are given in 13 successive tables, of which I shall only mention two.

Rules of hygrometry deduced from the experiments.

21. In *table* ii. are united the results of both experiments, (which differ very little from each other), reduced to their mean terms. Each set began at the point of *extreme dryness* in the vessel; a point where the *hygrometer* stood at 0 in both. At that point, no *moisture* being in the vessel, the change of *heat* from 50 to 60 of Fahr. produced no change in the *hygrometer*. During both sets of experiments, the limits of the *evaporation* in the vessel were the same: 5 *grains* only of *water* could remain *evaporated* at the temperature of 50; 6 *grains* at that of 55, and 7 *grains* at 60. Beyond these quantities, at the respective *temperatures*, a certain quantity of *water* was deposited on the sides of the vessel in the form of *dew*; but when this effect took place at the temperature of 50, the *dew* was dissipated when the *heat* of my room came to 55; and when it happened at 55, it was dissipated when the *heat* in the room arrived at 60.

Limits of evaporation at different temperatures.

22. Thus therefore we have the natural *limits* of the quanti-

ties

ties of *evaporated water* that can subsist in one *cubic foot* of *air* with these three degrees of heat; but by the rate of its progress, this correspondence may be continued to higher and lower temperatures, as I shall explain, after the following indication of the immediate effects observed on the *hygrometer* of each increase of 1 *grain* at the three *temperatures*. In the first two columns of the table, the points of the *hygrometer* cease to be indicated at the period when *dew* appeared on the side of the vessel.

Table of moisture indicated by the hygrometer at different temperatures.

Grs. of water in 1 cubic foot.	Points of the hygr. at temp. 50°.	Points of the hygr. at temp. 55°.	Points of the hygr. at temp. 60°.
	°	°	°
1	15·2	14·5	13·9
2	29·9	28·5	27·6
3	51·6	47·2	43·2
4	74·9	64·1	55·
5	89·8	78·6	68·3
6		93·9	82·1
7			96·6

Remarks on this table.

23. This table shows the progress of the effects on the *hygrometer* of the *evaporation* of the successive *grains* of water. These increases were stopped, as I have said above, by some water being deposited on the sides of the vessel. This effect took place for the 6th *grain* with the temperature 50°, and for the 7th *grain* at 55°: however, this happened only when the *grains* were entirely evaporated, during which time the *hygrometer* had moved; but there was no fixed point to be obtained correspondent to the new *grain* of *water*, since a part of it at last was deposited on the sides of the vessel.

Account of the other tables in the work.

24. The tables which follow this, in my work, serve to combine these results, by the rules of interpolation, for obtaining the intermediate terms not given by the experiments; and also to continue the same series, on one side, up to 98 of the *thermometer*, and on the other, for a particular purpose, down to 0. The table ix., which is the result of all these combinations, is constructed in such a manner, as to afford immediately the answer to the following questions, very important in meteorology.

Questions an-

I. A point having been observed on the *hygrometer* in the open

open air, what are the quantities of *evaporated water* in one *cubic foot* of that *air*, at any given *temperature* ?

swered by table ix.

II. The points of the *hygrometer* and *thermometer* having been observed, what is the quantity of *evaporated water* in one *cubic foot* of that part of the atmosphere ?

III. The points of both instruments having been observed, to what degree ought the *thermometer* to fall, in order that the *hygrometer* should arrive in that air at 100 ; which point it must attain before there is any precipitation of *water ?*

No degree of cold in the air can produce clouds and rain.

25. The answers to these questions, from the immediate results of my experiments, led to this first conclusion ; that no diminution of *heat* in the atmosphere could occasion in it the precipitation of such a quantity of *water* as to produce *clouds* pouring *rain* ; which confirmed me in the opinion already expressed in my work, *Idées sur la Météorologie*, that the *aqueous vapour*, constantly ascending in the atmosphere, ceased in great part to act on the *hygrometer*, being converted into an *aeriform fluid*, namely, the *atmospheric air*, and that *clouds* and *rain* were produced by the decomposition of this *fluid*.

Aqueous vapour ceases to act on the hygrometer from its being converted into atmospheric air.

26. Such was the conclusion of all the above *hygroscopic* experiments ; and with respect to *atmospheric* phenomena, it coincided with the observations of Mr. de Saussure and myself in the high regions of the atmosphere. Having both long inhabited our mountainous country near the Alps, we had separately followed the same meteorological observations with our *hygrometers*, and we had absolutely ascertained these two points.—1. That the more we ascend in the atmosphere, the *dryer* the air is observed ; and that even, in clear weather, it is *dryer* in the night than in the day. 3. That *clouds, rain, hail,* and *thunder*, are produced in certain *strata* of the atmosphere which were *clear* a moment before, and in which one *cubic foot* of *air* did not contain above *two grains* of *water*. Having both separately, at different times, and also in different parts of the mountains, made the same observations, and published them separately, I cannot suppose, that their results can be contested. Thus it is certain, that *rain* is *not* produced by a *moisture* existing in the atmosphere ; and consequently that it proceeds from a *decomposition* of the *air* itself.

Conclusions from hygroscopic experiments agreed with the atmospheric phenomena of the Alps.

The same deductions formed separately by the author and Mr. de Saussure.

27. From what I have said so far, it may be judged, that the

Proofs, that

whole

the modern theory of chemistry is erroneous.

whole of this work was intended to prove, how erroneous was the modern *theory of chemistry*, the foundation of which is to suppose, that *water* is a compound of *two* substances, called by its authors *hidrogen* and *oxigen*, and that the *atmosphere* is principally composed of two *fluids* called by them *hidrogen air* and *oxigen air*; a system in which, for the explanation of the greatest atmospheric phenomena, which ought to have been their first objects of comparison, those of *clouds* and *rain*, they had been reduced to suppose a *condensation* of the *aqueous vapour* by *cold*, which supposition the above experiments prove to be absolutely erroneous. This is the only point, which I have here considered; and indeed it is sufficient to overturn the whole theory: but in other parts of the work I entered into the examination of all its parts, beginning with the original experiments from which the *composition* of *water* had been concluded; and in analysing these experiments I made it manifest, that, far from being satisfactory, there were many unwarrantable hypotheses to be made, in order to connect the *facts* with the *conclusion*.

Berthollet's attempt to defend it.

28. When my work had been published at Paris, Mr. BERTHOLLET, one of the authors of that *chemical theory*, attempted, in the *Annales de Chimie*, and in another French Journal, to defend the only resource of that theory, namely, that *rain* was the effect of the *condensation* by *cold* of the *aqueous vapour* existing in the atmosphere. He acknowledged however two points, first, that my experiments with respect to the effects of *evaporated water* on the *hygrometer* at different temperatures had been made with an uncommon accuracy; and that I had thus demonstrated the errour of those, who attributed *evaporation* to a *dissolution* of *water* by *air*. These were two important concessions; but being loth to abandon his theory, and totally unacquainted with meteorological phænomena, he attempted again, as it was absolutely necessary for the support of his theory, to explain *rain* by the *cold* condensating the *aqueous vapour* in the atmosphere; thinking that by transporting the condensation to *very high regions* of the air, no objection could be made from immediate facts: but he was mistaken; since Mr. de Saussure and myself had proved, from immediate facts, that the upper regions of the atmosphere are *dryer* than those that we can attain.

29. I

29. I answered, in the *Annales de Chimie*, to every part of Mr. Berthollet's objections; and neither himself, nor any other experimental philosopher, has ever replied; while, on the contrary, many have abandoned the fundamental part of that theory, the *composition* of *water*: and indeed, one of its first inventors, with whom, having seen his experiments, I had acquiesced in his conclusion, and for a time maintained it, I mean Dr. Priestley, made me himself abandon it, on account of new chemical results obtained in his experiments, which he opposed to Mr. Berthollet.

Answered by the author.

The composition of water given up by one of its first supporters.

30. I have been induced, Sir, to give you this abstract of a work little known in England, in order the more to fix the attention of natural philosophers on the *hygrometer*, of which I have thus proved the importance in natural science. It is difficult to abstain from making *theories* on the first phenomena we observe of a new kind, or from admitting those which appear probable to us; and I have said above, that I had at first acquiesced in that of the *composition* of *water*: but by the progress of experiments, new facts are discovered, and correct the *theories* too soon admitted. My long study of every branch of meteorology, being united with the experiments related in this paper, which indeed were directed to that object, have demonstrated to me this great point in natural philosophy—that it is impossible to attribute *rain* to a *moisture* actually existing in the atmosphere; which alone entirely refutes the new *chemical theory*. Moreover, all the experiments on the combinations of *gasses* with other bodies concur to show, that the *ponderable* part of these fluids is *water*. Lastly, in the above mentioned work I proved, as I have done succinctly, Sir, in my paper published in your Journal for December, 1810, that, when we consider the atmospheric air as an *aeriform fluid*, though never mixed but with a very small quantity of *aqueous vapour*, all the *atmospheric* phenomena are explained.

We are too hasty in forming hypotheses.

I may conclude, therefore, that *meteorology* makes an essential part of *natural philosophy*, and that it is not so obscure as it is commonly thought.

I have the honour to be,
Sir,
Your obedient, humble Servant,
J. A. DE LUC.

Windsor.

X.

METEOROLOGICAL JOURNAL.

1812.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
9th Mo.										
SEP. 27	S W	30·04	29·77	29·905	68	59	63·5	—	·04	☽
28	S E	29·90	29·67	29·785	63	53	58·0	—		
29	N E	29·90	29·90	29·900	57	55	56·0	—		
30	E	29·90	29·75	29·825	59	55	57·0	·20		
10th Mo.										
OCT. 1	W	29·92	29·75	29·835	62	48	55·0	—	·28	
2	W	29·98	29·92	29·950	64	48	56·0	—	·01	
3	S E	29·98	29·90	29·940	61	41	51·0	·02	·09	
4	S W	29·90	29·78	29·840	67	44	50·5	—		
5	N W	29·78	29·31	29.545	69	55	62·0	—	·23	●
6	S E	29·32	29·12	29·220	64	41	52·5	—	·22	
7	Var.	29·32	29·12	29·220	57	45	51·0	—	·05	
8	S E	29·40	29·32	29·360	62	40	51·0	—	·06	
9	N W	29·40	29·35	29·375	53	46	49·5	·13	·07	
10	S	29·35	29·28	29·315	55	43	49·5	—	·32	
11	W	29·28	29·23	29·255	53	38	45·5	—	·06	
12	Var.	29·23	29·16	29·195	51	37	47·0	—	·08	
13	S E	29·16	28·83	28·995	57	45	51·0	—	·60	☾
14	S W	29·14	28·83	29·985	51	42	46·5	·03	·10	
15	W	29·39	29·14	29·265	53	43	48·0	—	—	
16	W	29·56	29·39	29·475	56	34	45·0	—	—	
17	S W	29·56	29·05	29·305	55	39	47·0	—	·29	
18	W	29·17	28·74	28·995	61	50	55·5	—	·44	
19	S W	28·81	28·53	28·670	59	49	54·0	·09	·37	
20	N W	29·54	28·81	29·175	67	42	54·5	—	—	○
21	N W	29·74	29·50	29·620	53	37	45·0	—		
22	S W	29·45	29·35	29·400	59	46	52·5	—	·02	
23	W	29·92	29·45	29·685	54	41	47·5	·10		
24	S W	29·92	99·74	29·830	56	41	48·5	—	—	
25	S E	29·50	29·40	29·450	56	41	48·5	—	·31	
26	S W	29·74	29·72	29·730	53	38	45·5	·06	—	
		30·04	28·74	29·468	69	34	51·46	.63	3·64	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Ninth Month. 26. Windy. Some rain in the night. 28 a. m. Very foul sky. 28, 29, 30. Rain at intervals in very small quantity.

Tenth Month. 1. A thunder-storm about 1, p. m. which was chiefly in the W. with heavy showers. 4, 5. Much dew. A storm of wind about midnight on the 5th. 6. Windy. 7. Misty morning: the trees dripping. 8. Rainbow, several times repeated between 8 and 9, a. m. Showers followed. 10. Rainbow, p. m. 11, 12. Rain in the night, misty morning. 13. *Cirrostratus* and *Nimbus* a. m. sunshine, and showers: a wet night. 16. Sunshine, with *Cumulostratus.* 17. Misty morning. 18. Squally during the night, with heavy showers. 19. Thunder and lightning about 2 p. m. Very heavy squalls with rain. 20. Sunshine a. m. much wind. 21. Clear and calm this evening. 22. a. m. Overcast, windy. In the evening a wet squall, with some lightning. 24. No swallows have been since the 19th or 20th. 25. A few swallows appeared again to-day.

RESULTS.

Prevailing winds westerly.

Barometer: highest observation, 30·04 inches; lowest 28·74 inches;

Mean of the period 29·468 inches.

Thermometer: highest observation 69°; lowest 34°.

Mean of the period 51·46°.

Evaporation 0·63 inches. Rain 3·64 inches.

The evaporation was much greater, during the above period than the amount here stated; as appears by observations as the Laboratory. It was probably not less than 2 inches. The situation of the guage had been changed.

L. HOWARD.

Plaistow,
Eleventh Month, 18, 1812.

XI.

On the Nature and Detection of the different Metallic Poisons. In a Letter from Mr. CHARLES SYLVESTER. *To William Nicholson, Esq.*

DEAR SIR,

Nature and detection of the different metallic poisons, a subject of inquiry.

ALTHOUGH an inquiry relative to the nature and detection of the different metallic poisons would need no apology, at any period, for its introduction to the readers of your Journal, yet I feel it to be the less necessary, at the present moment, from the great interest which has, of late, been manifested for investigations of this nature; and am induced, therefore, to send you the following essay, which was, for the most part, prepared for a particular purpose many months ago. Should it tend to lessen any of the difficulties attendant on a subject of such importance, or at all interest those who have devoted themselves to researches of this description, it will fully repay me for the labour of transcribing it.

I remain, dear Sir,

Your's very faithfully,

CHARLES SYLVESTER.

Derby, Nov. 16th, 1812.

I. Arsenic.

White oxide of arsenic.

As a poison the most virulent, and, at the same time, the one most to be dreaded, from its extreme insipidity, and the consequent readiness with which it may become an instrument in the hands of the murderer, or be received into the system by accident, is the white oxide of arsenic. For the detection of this substance, when thus admitted, various processes have, at different times, been recommended; and the papers of Dr. Bostock, published in the 5th vol. of the Edinburgh Medical and Surgical Journal, have, very generally, been thought to contain the best directions for this purpose. After various experiments he decides in favour of sulphate of copper, and carbonate of potash, which precipitate the white arsenic under the form of *Scheele's green.* It cannot fail to have occurred, however, to every one who has repeated these experiments, that the phenomena produced in this process are very much too ambiguous to enable a man, where the life of an individual is at stake,

Detection of it.

Dr. Bostock's method generally preferred.

Objection to it.

stake, to swear positively, that arsenic has been detected in his operations. The alkali employed, whether arsenic be present or not, uniformly occasions a precipitate, by detaching the oxide of copper from its combination with the sulphuric acid. The colour of the deposit, it is true, is not absolutely the same in both cases; but, when it is recollected, that experiments of this kind are, for the most part, conducted on solutions coloured to a greater or less degree from the matters found in the stomach or intestines, this minuteness of distinction will be deemed scarcely appreciable by the eye even of the most experienced operator. Such uncertainty ought, most assuredly, not to attend investigations of this nature; and the process of Dr. Bostock, therefore, becomes objectionable from this circumstance.

Process lately recommended by Mr. Hume.

The method of detecting arsenic next deserving of remark, is one lately recommended by Mr. Hume, of London. It consists in adding a quantity of subcarbonate of soda to a solution suspected to contain this metal; and afterward presenting to it a small piece of nitrate of silver. It is far preferable, Mr. Hume finds, to employ the latter salt in a solid form; and he recommends an angular piece, of the size of a pin's head, or thereabouts, held at the surface of the fluid, on the point of a knife. The existence of arsenic will be shown by a yellow precipitate, which falls down in rather abundant quantity.

Objection to it.

Whenever arsenical mixtures are operated upon, which have but little contamination with foreign ingredients, this process will, undoubtedly, succeed very well; but if ever muriatic acid be present, and this is always the case where materials from the stomach are mixed with the fluids under experiment, the test is then wholly useless, as a muriate of silver will be immediately formed, and the yellow compound, said to be so unequivocal in its indication of arsenic, of course be prevented from appearing. Neither of the processes yet spoken of, therefore, can receive that confidence, which ought to attach to investigations of such high importance.

The difficulties proved by experience.

This is not a mere speculative difficulty, but was fully proved to me during a course of experiments made some time ago, in consequence of a case of poison which came under my notice; and having, with the assistance of my friend, Mr. James Oakes, devoted, at that period, a good deal of attention to the subject, with a view, if possible, to supply the deficiency, we were led to the use of two

A different

process recommended.

two reagents, which, I think, will not only be found free from the objections applicable to those already mentioned, but appeared to combine most of the advantages requisite in operations of such extreme delicacy. The reagents employed were the acetate of copper, and oxyacetate of iron. For the preparation of them it is merely necessary to decompose oxysulphate of iron, and sulphate of copper, by acetate of lead, adding the latter until a turbidness ceases to appear. The resulting mixtures should contain as little of the original ingredients in combination as possible; particularly the iron test, since an excess of the oxysulphate, as was observed in our experiments, suspends the action of the acetate, and prevents its combination with the arsenic. The presence of acetate of lead is objectionable from its causing a precipitate with arsenic, which cannot be distinguished by the eye from sulphate of lead. When the two acetates are properly prepared, they combine with arsenic with considerable facility; that of iron producing a bright orange yellow deposit, and that of copper, green. The decomposition, however, especially of the former, does not appear to be complete till they have been suffered to stand a few seconds. One of the great advantages attending these reagents is, that their action is independent of the use of alkali, which, in the two former processes, from the precipitate uniformly occasioned by its presence, throws considerable uncertainty over the results of an experiment; and where the mixtures are coloured, as will always be the case, in a greater or less degree, in examining the contents of the stomach, must rob these methods of the whole of their value. With the tests here recommended, the *colour* of the compounds produced is not of that primary importance; for, since almost all their combinations, particularly those of copper, are soluble in water, except the one produced by an union with the white oxide of arsenic, the appearance of any precipitate may, without much risk, be referred to the presence of this metal. Of the two, experience has confirmed us in a preference of the acetate of copper, partly from its more sensible action on arsenical mixtures, and in some measure, also, from the easier mode of its preparation. As the oxyacetate of iron, however, may sometimes be occasionally resorted to, in order to afford additional evidence of the accuracy of an experiment, it may be necessary to add, as a farther

Two tests proposed.

Advantages in the use of these tests.

Of the two, the acetate of copper preferable.

Preparation of the oxyacetate of iron.

farther direction for its preparation, that the oxysulphate from which it is obtained, should be made by dissolving iron *with the aid of heat* in nitric acid, afterward precipitating the oxide, and redissolving it in sulphuric acid. The salt thus formed contains the metal at a *maximum* of oxidation.

Reduction of the arsenic to be preferred when practicable.

The whole of the above processes for the detection of arsenic of course refer to the cases where it has been exhibited only in a fluid state. Whenever it can be accomplished, however, by far the most satisfactory means of arriving at a knowledge of the presence of this substance, is to reduce it to a metallic state, which may be readily effected, either by subliming it in a glass tube with the aid of charcoal, or exposed between two plates of copper, according to the plans recommended in chemical works.

II. Corrosive Sublimate.

Oxymuriate of mercury.

Muriate of tin recommended by Dr. Bostock.

Objection.

For the discovery of corrosive sublimate, the methods almost exclusively resorted to until very lately were its precipitation by means of one of the carbonated fixed alkalis, or by lime water, which detach it under the form of an orange-coloured, or orange yellow, sediment. Dr. Bostock has since recommended muriate of tin; but, to the use of this test there is considerable objection, inasmuch as a precipitate, very similar in appearance to the one obtained from mercury, is always occasioned whenever muriate of tin comes into contact with a solution containing water. This could not fail to render the result of any experiment ambiguous; but should it so happen, that, from a particular circumstance, the employment of the muriate might be rendered at all desirable, its effect upon the fluid suspected to contain corrosive sublimate should be collated with the appearance produced from its mixture with an equal quantity of water, since the precipitate occasioned in a mercurial solution is remarkably more abundant than in the latter case, and sufficient to dispel all uncertainty arising from this cause.

This, in some degree, surmounted.

Galvanism supplies a much better taste.

But a test, at once the most easy of application and satisfactory, is furnished by means of galvanism, in which the mercury is separated in a metallic state. This experiment can be made by any person, and almost in any situation. It is merely necessary to take a piece of zinc wire, or in its absence a piece of iron

iron wire, about three inches in length, bent twice at right angles into a shape something like the letter U, but with a flattened bottom*. Its width should be about equal to the diameter of a common gold wedding ring; and the two ends of the bent wire must afterward be tied to a ring of this description. This being accomplished, take a plate of glass not less than three inches square, lay it as nearly horizontal as possible, and on one side drop some sulphuric acid, diluted with about six times its weight of water, till it spreads to the size of a halfpenny. At a little distance from this, towards the other side, next drop some of the solution supposed to contain corrosive sublimate, till the edges of the two liquids join together, After this is done, let the wire and ring, prepared as above, be laid in such a way, that the wire may touch the diluted acid, while the gold ring is in contact with the suspected liquid. If the most minute quantity of corrosive sublimate be present, the ring, in a few minutes, will be covered with mercury on the part which touched the fluid. It might be proper to filter the mixture before submitting it to experiment, or otherwise to pour it clear from the top; since calomel, which is so frequently taken as a medicine, might possibly be present, and give rise to these appearances. The insolubility of this substance, however, enables us easily to avoid it by the precautions here suggested.

III. Lead.

Lead.

Cautions against its use.

Why not uniformly injurious in these cases.

Although lead is not so virulent a poison as either arsenic or corrosive sublimate, its effects upon the animal economy are so greatly to be dreaded, that those liable to its influence in manufactures or domestic life, cannot be too much cautioned against it. The use of lead, in the construction of water cisterns, pumps, and conduit pipes, would, at first thought, appear highly objectionable; and in many instances it is, no doubt, productive of injury. The reason of its not being uniformly so, has been ingeniously pointed out by Gayton de Morveau. Most mineral waters contain a greater or less quantity of some salt formed by sulphuric acid. This acid is not only

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* The Greek Π is no doubt the figure intended.

the means of precipitating any lead which may happen to be dissolved in the water, but has the effect also of completely coating the interior surface of the vessels with the sulphate thus formed, which is a substance not liable to decomposition, and therefore defends the lead from all future action of any solvent in the mineral water.

Devonshire colic.

The dreadful disease called the *Devonshire colic* was clearly shown by Sir George Baker to be occasioned by the lead constituting the lining of the cider press, and other vessels, and which was dissolved by the acetic acid developed during fermentation. The acetic acid is here asserted to be the solvent, because the malic acid forms a salt with lead which is insoluble.

Great injury done by wine merchants.

The effects of this metal have been still more conspicuous in its use by wine merchants to correct the acidity of wine. The practice was at one time so common in France, that in a particular year, when much sour wine prevailed, many thousands of people are said to have fallen victims to its influence; and had it not been for the interference of government, it is impossible to say how widely this dreadful evil might have extended itself.

Lead present in new rum.

In the new rum of our West-India colonies the presence of lead has been marked by still more deadly consequences. It became a matter of great surprise, however, that, after this liquor had been kept in casks for twelve months, it lost its deleterious qualities. The lead employed in the vessels for the manufacture, but more especially in those for the distillation of rum, could not fail to introduce this metal in great quantity through the medium of the acetic acid, which is a constant product of fermentation; and had it not been for a circumstance about to be mentioned, it is difficult to conceive where the calamity might have terminated. Nature, however, in the shape of accident, stepped in as mediator, and redeemed the lives of those destined to drink the fascinating potion. It was before observed, that the rum lost its poisonous property by remaining a certain time in the casks; yet, although the fact was known, and the evil remedied, many years ago, I am not aware that any one has accounted for the change produced in the liquor. About two years since, my friend, Dr. Forester, of this place, gave an interesting lecture, on behalf of the Literary and Philosophical Society, upon the subject of poisons; and

How this is remedied by keeping.

and it was not till then, that I became acquainted with the curious facts above mentioned. It immediately occurred to me, that gallate of lead was insoluble; and I lost no time in making some experiments, to ascertain the fact. The method by which I prepared some gallic acid for the purpose may, perhaps, be new, and not wholly uninteresting, to some of your readers. My first step was to make a strong tincture of nutgalls in proof spirit. To this was added, by little at a time, a nearly saturated solution of isinglass, till the whole of the tannin was precipitated. The liquor separated from the coagulum at first appeared rather opaque, but without colour. By standing at rest for a few days, a deposition of flocculent matter took place, consisting of gelatine and tannin, which left the liquor transparent and colourless. This I considered as a solution of gallic acid, nearly pure. At all events, it did not contain any substance which prevented its being an excellent test for iron or lead. I soon found, that in very dilute solutions of lead, where sulphuric acid, or a sulphate, produces no visible precipitate, the presence of this metal was made sensible by the aid of the gallic acid. This confirmed my suspicions on the subject, and left me in no doubt as to the real cause of the rum losing its pernicious qualities; for, since the joint existence of lead and gallic acid in any fluid is impossible, from the formation of an insoluble gallate, the lead of the rasa becomes precipitated by the gallic acid furnished by the oak cask. These facts supply an excellent, though indirect, method of ascertaining, in many instances, whether lead be dissolved in wines. If, on testing the wine with iron, it is found to contain gallic acid, we may safely infer, that no lead is present; but if no gallic acid be detected, then either this acid, or the sulphuric, may be added, which will precipitate the lead in the form of a white powder. Sulphuret of potash, or lime, may also be employed, which will occasion a blackish deposit.

Mode of preparing gallic acid.

A sensible test of lead.

Means of detecting lead in wine.

The prevailing use of lead in dairies is very objectionable, especially when the milk is immediately used as an article of food. On the separation of the curd and butter, the dissolved lead will, no doubt, exist in the whey. When milk is kept too long in warm weather, the acetic acid is formed, which takes up the lead; and it is a fact well known in dairies, that milk remains sweet longer in leaden vessels than any other. This is,

Lead should not be used in dairies.

is, mistakenly, attributed to the *coolness* of the lead; but the true cause is as above mentioned. The evil would, in all probability, be much more considerable, were it not for the presence of the saccholactic acid, which takes the lead from the acetic acid, and forms an insoluble compound.

IV. Copper.

Copper.

The only other metallic substance likely to be taken into the stomach is copper; and for this the beautiful blue colour produced in its solutions by pure ammonia, is the most decisive and satisfactory evidence that can be required.

Ammonia its proper test.

General test of metallic poisons.

As a general reagent, either for the present metal, or for lead, mercury, or arsenic, none, in point of delicacy, can exceed liquid sulphuretted hidrogen. It detects the smallest quantity of metallic matter present in any mixture; and although the coloured media, in which experiments of this sort are generally obliged to be made, prevent that reliance upon the mere colour of a precipitate requisite to give this test an exclusive preference; yet it may frequently abridge the labour of an operation very considerably, and at once decide whether the poison has been metallic or otherwise. In conducting this sort of experiments, the recommendation of Dr Bostock, to view the result by *reflected*, and not by *transmitted*, light, is highly important; and in no case, perhaps, ought a decision to be given without comparing the effect of every test on the suspected mixture with the phenomena it presents in fluids of known composition.

General precautions.

XII.

On facilitating the Emission of Roots from Layers. *By* T. A. KNIGHT, *Esq. Pres. H. S**.

Experiments on the generation and motion of sap in trees.

IT is my custom, annually, to repeat every experiment that occurs to me, from which I have reason to expect information either in opposition, or in favour, of the opinions I have advanced respecting the generation and motion of the sap in trees;

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* Trans. of the Hort. Soc. vol. I. p. 255.

trees; and one of these experiments appearing to point out an improvement in the propagation of such trees by laying, as do not readily emit roots by that process, I send the following statement, under the hope that it may be acceptable to the Horticultural Society,

Sap descends from the leaves through the bark.

I have cited, in a former communication*, a part of the evidence, upon which I have inferred, that the sap of trees descends from their leaves through the bark; and I shall here only observe, in support of this opinion, that, if a piece of bark be every where detached from the tree, except at its upper end, it will deposit, under proper management, as much, or nearly as much wood, upon its interior surface, as it will if it retain its natural position; and that the sap which generates the wood, deposited in the preceding circumstances, must descend through the bark, as it cannot be derived from any other source.

Proof of this.

Sap employed in the formation of new roots in a layer.

When a layer is prepared, and deposited in the ground, the progress of the sap, in its descent towards the original roots, is intercepted upon the side where the partially detached part, or tongue, of the layer is divided from the branch; and this intercepted sap is, in consequence, generally soon employed in the formation of new roots. But there are many species of trees, which do not readily emit roots by this mode of treatment; and I suspected that, wherever roots are not emitted by layers, the sap, which descends from the leaves, must escape almost wholly through the remaining portion of bark, which connects the layer with the parent plant. I therefore attempted, in the last and preceding spring, to accelerate the emission of roots by layers of trees of different species, which do not readily emit roots, by the following means, having detached the tongue of the layers from the branches in the usual manner.

In layers where new roots had not been formed,

Soon after midsummer, when the leaves upon the layers had acquired their full growth, and were, according to my hypothesis, in the act of generating the true sap of the plant, the layers were taken out of the soil; and I found, that those of several species of trees did not indicate any disposition to generate roots, a small portion of cellular bark only having

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* Horticultural Transactions of 1811; Journal, vol. XXXII. p. 350.

issued

issued from the interior surface of the bark in the wounded parts. I therefore took measures to prevent the return of the sap through the bark, from the layers to the parent trees, by making, round each branch, two circular incisions through the bark, immediately above the space where the tongue of the layer had been detached; and the bark, between these incisions, which were about twice the diameter of the branch apart, was taken off. The surface of the decorticated spaces was then scraped with a knife, to prevent the reproduction of the bark, and the layers were recommitted to the soil; and at the end of a month I had the pleasure to observe that roots had been abundantly emitted by every one. In other instances I obtained the same results, by simply scraping off, at the same season, a portion of the bark, immediately at the base of the tongue of the layers, without taking them out of the ground.

the return of the sap to the tree was prevented by removal of bark,

and roots were produced.

By the preceding mode of management, the ascending fluid is permitted to pass freely into the layer to promote its growth, and to return till the period arrives at which layers generally begin to emit roots: the return of the sap through the bark is then interrupted, and roots are, in consequence, emitted; and I entertain little doubt that good plants of trees, of almost every species, may be thus obtained at the end of a single season. I wish it, however, to be understood, that my experiments have been confined to comparatively few species of trees; and that I am not much in the habit of cultivating trees of difficult propagation.

Effect of this management.

XIII.

On the Cultivation of the Jamrosade (Eugenia Jambos L.) in the National Garden at Paris. Abridged from the account given by M. THOUIN, *in the Annales du Museum, V.* 1, *p.* 357.

By RICHARD ANTHONY SALISBURY, *Esq. F. R. S. &c.*[*]

THE *jamrosade,* or *eugenia jambos* of Linné, is one of those trees, the fruit of which is seldom brought to perfection in *Europe.*—In *Hindostan,* where it grows wild, it is called *jambos,*

Jamrosade, or rose apple.

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[*] Horticult. Trans. vol. 1, Appendix, p. 11.

or *jambose*, and in those colonies where it is cultivated, *jamrosade*, or *rose apple.* There are several varieties, differing in the size and colour of their fruit; some red, or reddish; others white and smaller. *Rumph* calls the last variety *Jambosa Sylvestris alba*, and this is the tree I now propose to describe.

The species being already well known and figured, I shall only mention the differences peculiar to this variety with white fruit, its habit at *Paris*, and the method there adopted to make it produce fruit.

Described.

Our tree is at present about 11 feet high, with a stem 2 inches and a half in diameter at the base, branching from below the middle into a pyramidal head. The leaves are undivided, smooth, opposite, of a deep green, coriaceous, and not unlike those of some *Peach trees*, but larger. The buds push forth young leaves in the beginning of summer, of a most lively red, which change gradually to their permanent deep green colour. The bunches of flowers also appear at this period, terminating the branches, from 2 to 6 being clustered together. Petals, 4, greenish white, about as large as those of *apple* blossoms. Stamens very numerous, in a tuft half as long as the petals, their filaments pale violet colour towards the top, where they diverge, their anthers yellow. Pistil, longer than all, is inserted like the stamens, petals, and 4 divisions of the calyx upon a globular germen, which swells into a green fruit, gradually changing to white with a pale rose coloured tinge on the side exposed to the sun.

The fruit.

In size and shape, the fruit is not very unlike a *medler:* its flesh rather firm, but easily broken, from 2 to 3 lines thick, slightly acid, and perfumed with a smell approaching that of the rose, from which it has acquired the name of *rose apple* in some of the French colonies; in the middle are several nuts, easily detached from the flesh; if there is only a single nut, it is spherical, but when more are perfected, as is often the case, they become angular in the parts which touch each other. The shell of the nut is thin and brittle, inclosing a greenish white kernel, which easily breaks into irregular pieces. The cavity of the kernel, varying in size and figure, but more or less oval, is lined with a brown pellicle, which adheres very slightly. These fruits ripen from *September* till *December*, and though not actually nutritious,

tious, their perfumed flavour renders them very agreeable to most palates.

Its introduction into France.

The individual one above described was brought from *Hindostan* in 1765, by the abbé Gallois, and placed in the late Mr. Lemonnier's stove at *Versailles*. Though very young, by plunging it in the tan-bed, it soon flowered, but never ripened fruit till 1786. When it had attained the height of 6 feet, it was transplanted into a small box, and exposed gradually to the open air, during two of the hottest months of the year, but afterward removed back to the tan-bed.

Attempt to treat it more hardily.

In 1794 this tree was added to the National collection, and being stout and vigorous, I determined to treat it more hardily. During winter, instead of the tan-bed, it stood on the floor of the stove, but near the flue, and during summer it was exposed to the open air, in a sheltered southern exposure, not housing it till *October*. This method of culture, however, did not agree with it; for, soon after being put out, most of the leaves fell off, and those which remained, as well as the ends of its branches, turned yellow; a plain indication of its sufferings from the cold nights. Nevertheless, the great heat of our *Paris* summer soon restoring it to its ordinary vigour, numerous young shoots, and many flowers pushed out, but they fell off without producing fruit. In this way, I persisted to cultivate this tree till last spring (1801), being anxious to try, if in so many years, it might not be habituated to our climate; but it annually underwent the same alteration of sickness and health already detailed*.

Treatment to make it produce fruit.

At this period, wishing to make it produce fruit, I thought all that might be necessary would be a large portion of air with very great heat. For this purpose, it was left in the great stove at the foot of a very white wall, which, by reflecting the rays of the sun, increased the heat still more, and the tree was so placed as to receive the rays perpendicularly. The air was suffered to blow freely round it, and it was deluged with water, in consequence of the great evaporation produced by so much heat and air.

Successful.

My wishes were thus completely fulfilled; the tree grew most luxuriantly, being covered in *June* with numerous flowers, which were

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* This account does as great honour to the candour of one of the first gardeners in the world, as his detail of the insertion of the several parts of the flower does to his botanical abilities.—*Sec.*

were rapidly fecundated, the greater part of them being succeeded by ripe fruits, of which I gathered more than 40. Some of the finest are preserved in the gallery of Natural History; of others, which fell off, I have already sown the seeds: and others still on the tree will be suffered to remain till they drop off spontaneously, that I may be quite certain their seeds are perfectly ripe. From an examination of the kernel, which soon changes to a hard, horny substance, it is not surprising, that all the seeds imported from abroad have hitherto failed, unless they have been sent packed in earth; and I therefore deemed it necessary to sow them in a few days after they fell from the tree.

Seeds sown.

Treatment of the seeds.

To make success in this point doubly sure, I employed a method, the good effects of which I have often experienced. This was, after taking the nuts out of the fruit, to put them in my breeches pocket for 2 or 3 days. This sort of animal bath is preferable to the custom which has hitherto prevailed of immersing many seeds of hot climates in pure water.

Mode of sowing.

I finally sowed these nuts about half an inch deep in pots of earth, plunged in a very gentle hot bed. At the approach of frost they will be removed to the tan-bed of the stove, when the essential point to attend to, will be to moderate the humidity, heat, and light, so that the young plants may not appear till spring.

The plants may probably thrive in a temperate stove.

I dare not hope that this tree will soon be naturalized to live in the open air in any part of *France*; for, its buds (gemmæ) have no scales; but we may reasonably expect, that the plants raised from seeds here will not be so delicate as imported plants, and that they may succeed in a temperate stove, or orangery: nay, it is even possible, that such plants may survive through winter in some of the warm spots under our southern maritime alps, or in the island of *Corsica*. For this purpose, they should be planted with *orange* trees, *citron* trees, and *guava* trees, among which the *jamrosade* thrives in its native country, or such colonies as it has been transported to.

XIV.

XIV.

Letter from Dr. Tuthill on the Sugar from Potato Starch. To Mr. Nicholson.

DEAR SIR,

HAVING learned, that professor Berzelius had brought intelligence to this country of a very remarkable change produced in wheat starch by the action of dilute sulphuric acid at a high temperature, as discovered by M. Kirchoff, of the imperial academy of St. Petersburgh, I was desirous of ascertaining whether the fæcula of other vegetables, submitted to the action of the same fluid, at the same temperature, would exhibit a similar phenomenon. For this purpose I took eight pounds and three quarters of potatoes, grated them, and placed the pulp on a sieve. Cold water was then slowly poured upon this pulp as long as it passed turbid through the sieve, and the liquor was suffered to stand in the vessel that received it till it became clear. On pouring off the clear liquor, the fæcula of the potatoes was found at the bottom of the vessel; and, when dried by a very gentle heat, weighed a pound and a half. To this fæcula were added six pints of distilled water, and a quarter of an ounce by weight of common sulphuric acid in an earthen vessel furnished with a cover. The mixture was kept boiling for thirty-four hours without intermission, the vessel being covered, and the loss by evaporation carefully supplied by the frequent addition of distilled water, so as to preserve the same quantity as at the commencement of the operation. For the first twelve hours I could perceive no change in the sensible properties of the vapour. At the expiration of twenty-four hours the liquor had evidently become saccharine, and this quality continued to increase as the boiling was prolonged. Thirty-four hours after the commencement of the ebullition, half an ounce of finely-powdered charcoal was added, and the boiling continued for two hours longer. The acid was then saturated by lime that had been very recently burned, and the boiling continued for half an hour; after which the liquor was passed through a piece of calico, and the substance remaining on the filter washed by the repeated effusion of warm water. This substance, when dry, weighed seven eighths of an ounce, and consisted of charcoal and sulphate of lime. The clear liquor was now evaporated in a water bath to the consistence of sirup,

Invention of sugar from starch.

Potato starch.

A pound and a half of starch, and six pints of water, and one quarter of an ounce of sulphuric acid were gently boiled thirty-four hours.

In twenty-four hours the fluid was saccharine.

After 34 hours an ounce and a half of charcoal was added.

After two hours more the acid was saturated by lime.

Insoluble precipitate.

Clear liquor evaporated.

rup, and set aside to crystallise. In eight days it was converted into a crystalline mass, having nearly the sensible properties of common brown sugar mixed with a little treacle. The weight of the saccharine matter thus obtained from eight pounds and three quarters of potatoes, and which I conceive to be intermediate between cane sugar and grape sugar, weighed one pound and a quarter.

Afforded sugar equal to one seventh part of the weight of the potatoes.

One pound of this crystallised saccharine matter was now redissolved in four pounds of distilled water, and by the addition of a quarter of an ounce of yeast submitted to alcoholic fermentation. In ten days the temperature having varied from 44° to 54°, the smell of the liquor first indicated that the alcoholic fermentation was just beginning to pass into the acetous. The whole was then instantly submitted to distillation, and the process continued till a pint and a half of fluid was collected in the receiver. This on being redistilled produced two ounces and five eighths by measure of dilute alcohol, of which a cubic inch, the mercury in Fahrenheit's thermometer standing at 45° and in the barometer at $22\frac{19}{20}$ inches, weighed 245 grains. I have therefore concluded from the accurate experiments of Sir Charles Blagden, that the two ounces and five eighths of dilute alcohol thus obtained contains fourteen drachms by measure of proof spirit.

I am, dear Sir, very truly your's,

October 28, 1812. G. L. TUTHILL.

Soho Square.

SCIENTIFIC NEWS.

New Explosive Compound.

Notice has been received from the Continent of a new explosive compound, upon which Sir Humphrey Davy has made some experiments; and it has also been produced by others. The present short statement is all that I can on this occasion insert.

Nitrate of ammonia is to be dissolved to saturation in water, and exposed in a basin to a low temperature, such as that of ice, or rather the freezing mixture of ice and salt.—A vessel containing oximuriatic gas is then inverted in the solution. The gas becomes absorbed, and the solution ascends; and, after one or two hours, a small portion of heavy oil is found at the bottom of the basin. Of this oil if a quantity of the size of a pin's head be put into contact with olive oil, a violent and dangerous explosion takes place.

A friend who repeated this experiment, used a four ounce phial of the gas and put his olive oil in a small platina spoon. The spoon was destroyed by the explosion.

Manner of the Growth of Trees

By Mrs A: Ibbetson.

Fig. 2.

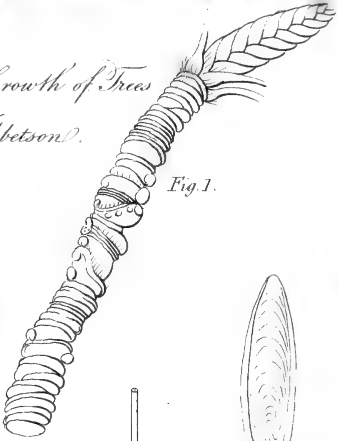

Fig. 1.

Fig. 8.

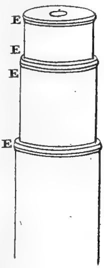

Fig. 4.

Fig. 9.

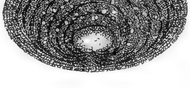

Fig. 7.

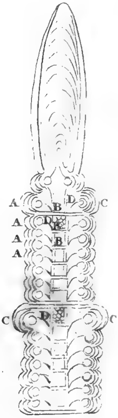

Fig. 3.

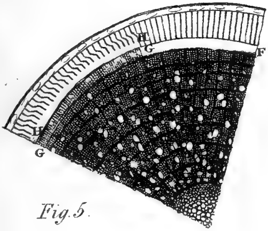

Fig. 5.

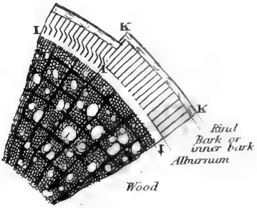

Fig. 6.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XXXIII.

ARTICLE I.

Observations on the Measurement of three Degrees of the Meridian, conducted in England, by Lieut. Col. William Mudge. By Don Joseph Rodriguez. *From the Philosophical Transactions for* 1812, *p.* 321.

Problem. To ascertain the figure and magnitude of the earth

THE determination of the figure and magnitude of the earth has at all times excited the curiosity of mankind, and the history of the several attempts made by astronomers to solve this problem might be traced to the most remote antiquity. But the details of the methods pursued by the ancients on this subject being extremely vague, and their results expressed in measures of which we do not know the relation to our own, in fact give us very little assistance in learning either the figure or dimensions of our globe.

was considered by Huyghens and Newton,

It was not till the revival of science in Europe, that the two great philosophers, Huyghens and Newton, first engaged in the consideration of this question, and reduced to the known laws of mechanics, the principles on which the figure of the earth should be determined.

as determinable by attending to its rotation;

They demonstrated, that the rotatory motion should occasion differences in the force of gravity in different latitudes, and consequently, that parts of the earth in the neighbourhood of the equator should be more elevated than those near the poles.

which, on the hypothesis of the mass being uniformly dense, and imperfectly fluid, would elevate the equatorial more than the polar regions.

The most simple hypothesis, which first presented itself to their imagination, was that which supposed the earth to be throughout composed of the same kind of matter, and its surface that of a spheroid generated by revolution round its axis. This hypothesis, adopted by Newton only as an approximation to the truth, is, in fact, perfectly consistent with the equilibrium to which particles in a state of paste, or of tardy fluidity, would arrive in a short time after their present motion was impressed; and the eccentricity derived from this hypothesis is at least not very remote from that which actually obtains in the present state of consistence and stability which the earth has since acquired.

But geological observations show that the (external part of the) earth is not homogeneous.

But the homogeneity of the matter, of which the earth consists, is at variance with all geological observations, which prove evidently that at least 5000 toises of the exterior crust is formed of an immense mass of heterogeneous matters, varying in density from each other; and upon the supposition of a state of fluidity of the whole, it should follow, that the strata should successively increase in density from the surface towards the centre, that the more dense would accordingly be subjected to less of centrifugal force, and consequently that the spheroidical form resulting from this cause would be less eccentric than would arise from a state of perfect homogeneity.

Solution of the problem, by measuring several arcs of the meridians.

The most simple, as well as the most effectual means of verifying the hypothesis respecting the figure of the earth, is to measure in the two hemispheres several arcs of its meridians in different latitudes, at some distance from each other. On this subject it must be allowed, that the Academy of Sciences at Paris set the example, in giving the original impulse to the undertaking, and not only commenced, but put in execution those parts of the plan which were most difficult and most decisive.

The first measurements, as well as the vibrations of pendulums, showed that the polar regions are flattened.

The results of the first measurements made of different arcs on the meridian of different parts of the world, were found to be perfectly conformable to the expectations of Huyghens and of Newton, and also with experiments made on the vibration of the pendulum in different latitudes; and they left no doubt that the earth was in fact flattened at the poles;

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establishing thereby one point extremely interesting in natural philosophy.

Inaccuracy of the earlier measures.

These results, however, did not correspond with sufficient accuracy for ascertaining with precision the degree of eccentricity, or even the general dimensions of the earth; as might naturally be expected, when we consider the necessary imperfection of the means then employed in these operations, and the great difficulties that are to be encountered.

For the purpose of making a nearer approximation to the true dimensions of the earth, and of verifying former measurements, it is necessary, in some instances, to repeat them, and also to make others in different situations, which may be expected to be improved in proportion to the progress that is made in the means of perfecting the several departments of science.

Grand undertaking of admeasurement in France and Spain;

At the commencement of the French revolution, men of science took advantage of the general impulse which the human mind received in favour of every species of innovation or change, and they proposed making a new measurement of an arc of the meridian in France, for the purpose of establishing a new system of weights and measures, which should be permanent, as being founded on the nature of things.

directed by a commission of the academy,

A commission, composed of some of the most distinguished members of the Academy of Sciences, was charged to form the plan of these operations, which were to serve as the basis of the new system. They invented new instruments, new methods, new formulæ; and in short almost the whole of this important undertaking consisted of something new in science.

and performed by Delambre and Mechain.

Two celebrated astronomers, Delambre and Mechain, were engaged to perform the astronomical and geodetical observations, and these they continued as far as Barcelona in Spain. The details of their operations, observations, and calculations, were subsequently examined by a committee of men of science, many of whom were foreigners collected at Paris, who confirmed their results, and by the sanction of such an union of talents, gave such a degree of credit and authenticity to their conclusions, as could scarcely be acquired by other means.

Continuation of the line by Biot and Arago to Formentera.

Since that time, in the year 1806, Messrs. Biot and Arago, members of the National Institute, were sent into Spain for the express purpose of carrying on the same course of opera-

tions

tions still farther southward, from Barcelona as far as Formentera, the southernmost of the Balearic islands. Fortunately this last undertaking, which forms a most satisfactory supplement to the former, was completed by the month of May, 1808, at a period when political circumstances would not admit of any further operations being pursued, as a means of verifying the results, by measuring a base which should be independent of those formerly obtained in France.

Verification of the Lapland measures by the Swedish academy.

In the year 1801, the Swedish Academy of Sciences, encouraged by the success of the operations conducted in France, sent also three of its members into Lapland, to verify their former measurement, taken in 1736, by new methods, and by the use of new instruments, similar to those which had recently been used in France, and of which the National Institute made a handsome present to the Swedish Academy. The results of this new undertaking, which terminated in 1803, were drawn up by M. Svanberg, and are highly interesting, by their exactness, by the perspicuity of the details, and even a certain degree of novelty given to the subject by the arrangement adopted by the learned author M. Svanberg.

The agreement of these new measures in confirming the general results

was contradicted by the results of the admeasurements made in England

under circumstances of pe-

These new measures were found to confirm, in a remarkable manner, the general results of those which had preceded, and gave very nearly the same proportion for the eccentricity and other dimensions of the globe, so that there would not have remained the smallest doubt respecting the figure of the earth being flattened at the poles, had there not been a fourth measurement performed in England at the same time as that undertaken in Lapland, the results of which were entirely reverse. This measurement, which comprised an arc of 2° 50′, was undertaken by Lieut. Col. Mudge, Fellow of the Royal Society, with instruments of the most perfect construction that had ever yet been finished by any artist, contrived and executed for that express purpose, by the celebrated Ramsden. The details of the observations and other operations of Lieut. Col. Mudge, may be seen in the volume of the Philosophical Transactions for the year 1803; and one cannot but admire the beauty and perfection of the instruments employed by that skilful observer, as well as the scrupulous care bestowed on every part of the service in which he was engaged. Bengal lights were employed on this occasion, as objects at the several stations, culiar advantage;

stations, and their position appears to have been determined with the utmost precision by the theodolite of Ramsden, which reduces all angles to the plane of the horizon, and with such a degree of correctness, that the error in the sum of the three angles of any triangle, is scarcely, in any instance, found to exceed three seconds of a degree, and in general not more than a small fraction of a second.

Accordingly the geodetical observations were conducted with a degree of exactness, which hardly can be exceeded; and even if we suppose for a moment, that the chains made use of in the measurement of the bases, may not admit of equal precision with the rods of platina employed in France, nevertheless, the degree of care employed in their construction, in the mode of using them, and the pains taken to verify their measures was such, that no error that can have occurred in the length of the base, could make any perceptible difference in the sides of the series of triangles, of which the whole extent does not amount to so much as three degrees.

but which indicate, that the earth is more elevated at the poles than at the equator.

Nevertheless, the results deduced by the author, from this measure alone, would lead to the supposition, that the earth, instead of being flattened at the poles, is, in fact, more elevated at that part than at the equator, or at least, that its surface is not that of a regular solid. For the measures of different degrees on the meridian, as reduced by Lieut. Col. Mudge, increase progressively toward the equator.

Table of the measures of a degree of lat. in England, which *increase in going southward.*

The following table of the different measures of a degree in fathoms, is given by the author in his Memoir.

Latitude.	
52° 50′ 30″	60766
52 38 56	60769
52 28 6	60794
52 2 20	60820
51 51 4	60849
51 25 18	60864
51 13 18	60890
51 2 54	60884

This singular result,

The singularity of these results excites a suspicion of some incorrectness in the observations themselves, or in the method of calculating from them. The author has not informed us in

his

his Memoir, what were the formulæ which he employed in the computations of the meridian; but one sees, by the arrangement of his materials, that he made use of the method of the perpendiculars without regard to the convergence of the meridians; and although this method is not rigorously exact, it can make but a very few fathoms more in the total arc, and will have very little effect on the magnitude of each degree. It is therefore a more probable supposition, that, if any errors exist, they have occurred in the astronomical observations. But it is scarcely possible to determine the amount of the errors, or in what part of the arc they may have occurred, excepting by direct and rigorous computation of the geodetical measurement. I have therefore been obliged to have recourse to calculations, which I have conducted according to the method and formulæ invented and published by M. Delambre.

if erroneous, is most probably so from the celestial observations.

The means generally employed for finding the extent of a degree of the meridian, consists in dividing the length of the total arc in fathoms, by the number of degrees and parts of a degree deduced from observations of the stars; but if these observations are affected by any error, arising from unsteadiness of the instrument, from partial attractions, or from any other accidental causes, then the degrees of the meridian will be affected, without a possibility of discovering such an error in this mode of operating. It is consequently necessary, in such a case, to employ some other method, which may serve as a means of verifying the observations themselves, of detecting their errors, if there be any, or at least of shewing their probable limits.

The usual method of finding the degree, by dividing the total arc in fath. by its measure in deg. and parts, will not detect the errors of observation.

My object therefore is to communicate the result of calculations that I have made, from the data published by Lieut. Col. Mudge in the Philosophical Transactions: and I hope to make it appear, that the magnitude of a degree of the meridian, corresponding to the mean latitude of the arc measured by this skilful observer, corresponds very exactly with the results of those other measurements that have been above noticed.

Calculations by the author,

In M. Delambre's method nothing is wanting but the spherical angles, that is to say, the horizontal angles observed, corrected for spherical error. Moreover, for our purpose, we have no occasion for the numerical value of the sides of the series of triangles, but only for their logarithms. Thus the logarithm

by means of the spherical angles in Delambre's method.

logarithm of the base measured at Clifton, as an arc gives us that of its sine in feet or in fathoms, so that by means of this latter logarithm, and the spherical angles of the series of triangles, we obtain at once, and as easily as in plane trigonometry, the logarithms of the sines of all their sides in fathoms.

After this, it is extremely easy to convert them into logarithms of chords or of arcs, for the purpose of applying them to the computation of the arcs on the meridian or azimuths. I give the preference to taking the logarithms of the sides as arcs, because the computations become in that case much more simple and expeditious.

Reduction of the northern base to toises.

Near to Clifton, which is the northern extremity of the arc, in a situation elevated 35 feet above the level of the sea, a base was measured of 26342,7 feet in length, the chains being supposed at the temperature of 62° Fahrenheit, or $13\frac{1}{3}$° Reaumur.

For reducing this base to toises, we have the proportion of the English foot to that of France, as 4 : 4,263, so that if p be taken to express the fractional part of the French foot, corresponding to English measure, then log. $p=9{,}97234{,}46587$,

and then log. of $26{,}342{,}7=4{,}42066{,}02860$,

and hence the log. of the base in toises will be found equal to 3,61485,36943, and the number of toises corresponding is 4119,5 taken at the same temperature, which corresponds to $16\frac{2}{3}$° of the centigrade thermometer.

This base we must consider as an arc of a circle, and it is easy to reduce it to the sine of the same arc, according to the method given in a note at the end of this memoir. The logarithm of the *sine* of the base in toises is found to be 3,61485,35800.

from which and the spherical triangles the portions or intervals of the stations with their azimuths were computed.

With this quantity as base, and by means of the spherical triangles given by Lieut. Col. Mudge in his paper, I have found the logarithmic sines in toises of all the sides of his series of triangles, and have subsequently reduced them to logarithmic arcs of the same, which enable me to complete the rest of the calculation. With these we may compute any portions of the meridian, or successive intervals of different stations expressed in toises, and in parts of the circle, or their respective azi-

muths,

muths, having regard always to the relative convergence of different meridians.

The author has made observations for determining the latitude of the two extremities of his arc, and has also determined the azimuths of the exterior sides in his series of triangles by means of the greatest elongation of the pole star.

In the calculations that I have made, I began at Clifton in Yorkshire, the northern extremity of the arc, and for this purpose the following are the data furnished by Lieut. Col. Mudge.

Col. Mudge's data,

Latitude of Clifton reduced to the centre of the station 53° 27′ 36,″62.

Azimuth of Gringley, seen from Clifton, and reckoned from the north toward the west 256° 17′ 25″.

Azimuth of Heathersedge seen from Clifton, and reckoned in the same direction 118° 8′ 8″,81.

from which the computations were begun,

With these data, and the two tables of spherical triangles and the logarithms of their sides expressed in arcs, the intervals between Clifton and the two stations Gringley and Heathersedge were found in toises and in seconds of a degree, as well as all the corrections to be made on the first azimuths increased by 180°, as azimuths of Clifton seen on the horizon at these latter places.

and continued through the whole series.

The same process was continued for the following stations in succession, all the way to Dunnose in the Isle of Wight, which is the southernmost extremity of the series.

In this manner we have the latitudes and azimuths of each station, by means of two or three preceding stations, and consequently we have a verification of all the calculations that have been before made by Lieut. Col. Mudge.

The results of my calculations are contained in the two folowin tables.

First Table of Distances in Toises and in Seconds of a Degree on the Meridian, comprised between the westerly Stations in the Series of Triangles.

Table of distances of the westerly stations on the Meridian;—in toises and in seconds.

Names of the Stations.	Arcs in toises.	Arcs in Seconds.
Clifton -	0,0	0,0
Heathersedge -	6834,324	430,9928
Orpit -	15818,489	997,5928
Castlering -	19801,1934	1248,8226
Corley -	14295,384	901,6207
Epwell -	22327,008	1408,2543
Stow - -	9555,479	602,7284
Whitehorse -	18799,645	1185,8656
Highclere -	14990,567	945,6354
Dean Hill -	16105,614	1016,0180
Dunnose -	23529,886	1484,4531
Sum total -	162057,5437	10221,9837

Second Table of successive Intervals between the Eastern Stations.

Table of the intervals between the E. stations.

Names of the Stations.	Arcs in toises.	Arcs in Seconds.
Clifton -	0,0	0,0
Gringley -	2809,105	177,149
Sutton -	10838,816	1061,931
Holland Hill	4681,190	295,2251
Bardon Hill	18092,261	1141,0462
Arbury Hill	27956,417	1763,2683
Brill -	22374,106	1411,2769
Nuffield -	14350,3834	905,2155
Bagshot -	12137,933	765,6822
Hindhead -	14449,2027	911,5140
Butser Hill	7853,644	495,4551
Dunnose -	20514,036	1294,1974
Sum total -	162057,0941	10221,9607

The arithmetic mean of those results gives the mean degree =57073 toises, of lat. of the whole arc.

Now if we take the arithmetic mean of the sums contained in the two tables, we have for measures of the entire arc, comprised between the stations of Clifton and Dunnose, the following quantities 162057,32 toises, and 10221,972 seconds of a degree, or 2° 50′ 21″,972. By dividing the former of these by the second, we get the measure of a degree, corresponding to the mean latitude of the whole arc, equal to

57073,74

57073,74 toises,, or 60826,34 fathoms, at the temperature of $16\frac{2}{3}$° of the centigrade thermometer, the latitude being 52° 2′ 20″.

But by dividing the whole arc into two nearly equal parts, the northern mean deg. proves= 57068,41 toises;

The station at Arbury Hill happens to be very nearly in the meridian of Clifton and Dunnose, and divides the interval between them into nearly equal parts. The measures of that part of the arc, which lies between Arbury and Dunnose, is by the tables 91679,47 toises, and 9783″,34 seconds, or 1° 36′ 23″,34 of the common division of the circle. The mean latitude of the arc is 51° 25′ 21″. And the measure of 1 degree corresponding to it is 57068,41 toises.

and the southern mean deg.=57080,70 toises.

In the same manner the measure of the arc comprised between Arbury Hill and the northern extremity at Clifton, is 70377,85 toises, and 4438,63 seconds, or 1° 13′ 58″,63. Its mean latitude is 52° 50′ 32″. And we have for one degree of the meridian, corresponding to this latitude, 57080,70 toises.

and the degrees increase in going northward,

Hence, if we divide the entire arc into two equal parts, we deduce the following values of a degree corresponding to the middle of the whole and of its parts.

Latitudes.	
51° 25′ 20″	57068
52 2 20	57074
52 50 30	57081

in perfect conformity with the theory and the results of other admeasurements.

These values are, as appears, perfectly in conformity with the theory, and with the results of other measures that have been taken in different parts of the northern hemisphere; but, in order to place that agreement in a more distinct point of view, I shall show how nearly these estimates agree with the elliptic hypothesis, by comparing them with those measures of a degree, on which we can place the greatest reliance for exactness.

Inquiry into the errors which led to the former conclusions.

Now, if we compare the results of these calculations with those deduced by Lieut. Col. Mudge from his observations, we shall see the probable source of those *errors*, which it appears to me have led him to false conclusions. It has already been observed, that the station at Arbury Hill divides the whole arc into two parts nearly equal, and that it is also nearly in the meridian of the two extremities at Dunnose and Clifton. It was, in all probability, this circumstance which determined the

the author to observe the latitude of Arbury Hill, as he would then have two partial arcs independent of the whole and of each other.

For determining the angular extent of these arcs, Lieut. Col. Mudge observed the zenith distances of several stars on the meridian above the pole, by means of a large zenith sector constructed by Ramsden, with the same pains that he had bestowed upon the theodolite. Lieut. Col. Mudge paid all possible attention, and took all such precautions as might naturally be expected from an observer of his experience and address. Nevertheless the results of his observations made on different stars, differ no less than 4 seconds from each other. But, by taking a mean of all, the dimensions of the three arcs reduced to the centre at each station are as follows,

Facts. The angular extent of the arcs were observed by Col. Mudge with a zenith sector; in which the results vary 4 sec.

Between Clifton and Dunnose	2^0 50′ 23″,35
Clifton and Arbury	1 14 3 ,40
Arbury and Dunnose	1 36 19 ,95

Means of observation in deg. and pts.

The extent of the first arc, in linear measure, is 1036339½ feet English, and when this is reduced to toises, we have for the lengths of the three arcs from Lieut. Col. Mudge's measures,

From Clifton to Dunnose	162067,3
Clifton to Arbury	70380,2
Arbury to Dunnose	91687,1

The same arcs in toises:

These last values exceed those resulting from my computations, the first by 10 toises, the second by 2, the third by 8 toises; and these differences arise from the convergence of the meridians, which the author thought might safely be neglected, and in fact it does not make a difference that is perceptible in the value of a degree upon the meridian. For the difference of 8 toises, in the distance between Dunnose and Arbury, makes but 5 toises difference in the value of a degree upon that arc, and the difference of 10 in the whole distance from Dunnose to Clifton, makes 3½ in the measure of each degree on that arc. So that, as far as this source of disagreement is concerned, the author's results and mine would not be found to differ materially from each other.

are not materially differing from the computed tables.

But,

But, if we attend to the angular dimensions of the several arcs, as deduced from observations and from calculation, these will not be found to agree so nearly.

The following table will shew the differences in each instance.

But in deg. and pts. they differ very considerably.

Clifton and Dunnose	2° 50′ 23″,35 observed
	2 50 21 ,97 calculated
Difference	+ 1 ,38
Clifton and Arbury	1° 14′ 3″,40 observed
	1 13 58 ,63 calculated
Difference	+ 4 ,77
Arbury and Dunnose	1° 36′ 19″,95 observed
	1 36 23 ,34 calculated
Difference	− 3 ,39

These differences are really considerable, and are capable of producing important errors in the results dependent on them.

In the first place we see, that the southernmost arc between Dunnose and Arbury is smaller than it would appear by computation, by as much as 3″,4, and when this deficiency is combined with an excess of 8 toises in the linear dimensions of the same arc, it makes as much as 40 toises difference in the estimated length of a degree. The reverse of this occurs in the northern portion of the arc comprised between Clifton and Arbury Hill. This is larger than it ought to be by 4″,77, and hence the value of a degree on the meridian turns out too small by about 62 toises in its linear dimensions. Fortunately however, the excess of the total arc is extremely small, as it does not exceed 1″,38, so as to make but 5 or 6 toises difference in the length of a degree observed on the meridian, and corresponding to the mean latitude of the arc examined.

namely 40 toises in a degree.

The excess of the total arc is, however, much less

Hence the apparent increase in the degs. is to be ascribed to error of obs. of lat. at Arbury Hill.

From what has been above stated, it seems almost beyond a doubt that it is to errors in the observations of latitude, that the appearance of progressive augmentation of degress towards the equator, as represented by Lieut. Col. Mudge in his paper, are to be ascribed, and that it is especially at the intermediate station

tion at Arbury Hill, that the observations of the stars are erroneous nearly 5 seconds, notwithstanding the goodness of the instruments, and the skill and care of the observer. But, before I insist farther on this head, I will answer one objection that may be made to the principles of the method that I have pursued in this Memoir.

Those astronomers, who have hitherto undertaken the measurement of degrees of the meridian, have deduced their measures by simply dividing the linear extent by the number of degrees and minutes found by observation of the fixed stars taken at the two extremities of the arc. This is indeed the most simple that can be adopted ; and it has the advantage of being independent of the elliptic figure of the earth, especially in arcs of small extent. The elements dependent on this figure, are too uncertain to be employed in calculating the angular intervals in the short distances between successive stations, even as a means of verification, without risk of committing greater errors than those to which astronomical observations can be liable. Accordingly one cannot safely make any use of it in cases where great accuracy is required.

Objection. The elements dependent on the ellipticalfigure, are too uncertain to be employed in calculating the intervals, &c.

I must admit the justness of this objection, and must therefore shew the extent to which it really applies to the present subject.

In the first place, I may suppose, that in consequence of some fault in the instrument, with respect to vertical position, construction, or some accidental derangement, there is an error of some seconds in the observations of the fixed stars. How is this to be discovered? This is not to be done by comparing the value of a degree on the meridian, as deduced from these observations, with the results of other measurements in distant parts of the globe. For if we find that these degrees so taken do not agree in giving the same ellipsoid, we are not to attribute all the differences to irregularities of the earth, without supposing any error on the part of the observer, of his instrument, or of other means employed in his survey.

but, on the other hand, if diff. measures give diff. ellipsoids—it must not be admitted that the whole variation be ascribed to the fig of the earth and none to the errors of obs.

But this, in fact, is what has generally been done. It must, however, be acknowledged, that the majority of observers have not been in fault, as they could do nothing better ; but too much reliance has been placed on the goodness of their instruments, their means, and other circumstances. It is true that

that irregularities of the earth and local attractions may occasion considerable discrepancies which are even inevitable; but before we decide that these are the real source of disagreement, we ought carefully to ascertain that there are no others.

(*To be continued.*)

II.

On the Roots of Trees. By Mrs. AGNES IBBETSON.

To W. Nicholson, Esq.

SIR,

The difficulties attending giving the root.

IN my last letter, it was my endeavour to give as exact an account as possible of the increase of trees, both in length and breadth; that which they made in spring and autumn, and that which (nearly at the same time) enlarges the trunks. I shall now venture on a more difficult task, the delineation of the root, which I have long delayed; for whenever I was on the point of attempting it, I feared I was inadequate to the undertaking, and put it off another year, till further *dissection*, and a more *thorough knowledge*, should satisfy me that I was capable of giving an account that would please myself, and do justice to the great object of my pursuit. For many years together I have recurred to the subject, studying it with the most indefatigable industry, and seeking in nature only for information: but for the last six months the quantity of roots, both fresh and dry, that I have dissected, the innumerable cuttings that I have subjected to the solar microscope of the roots of different trees of every age and size—in short, the endeavours I have made to collect facts sufficient to prepare myself to give an exact account of the laws by which the root is regulated—the power which governs it in its *exterior*, as well as *interior* form—the parts which compose, and the mechanism which moves it, has at last given me courage sufficient to venture on my task; and if I do not thoroughly satisfy my readers, I shall still show many things *perfectly unknown*; and, at a future time, I shall hope to add circumstances that may render it more complete, and more worthy the *attention* of the *public*: at least I can promise, that I shall advance nothing but what *all* may ascertain the truth of, nor enter into any detail that may

may not be proved to be just and true, by those who will take the trouble of seeking, both in dissection and practical gardening, that knowledge, which constant labour and watching has procured me.

Explanation of the root of a tree.

The first thing that strikes the mind with astonishment in the dissection of roots, is that excessive motion to which they are subject ; each fibre, and each sap-vessel must be capable of changing its place, and of creeping individually into another situation ; and yet so admirably is the tout ensemble contrived to make but *one whole*, that it rarely differs from that *form* and *fold*, which is allotted to that species of tree. The root of a tree is that part which is the *foundation* of the sap-vessels. I have said, that each *sap-vessel* of the *stem* is joined about two inches above the earth, to *two sap-vessels* of the root ; and so wholly and individually do they belong to each other, that they cannot be divided, without causing the destruction of both ; the root may, indeed, sometimes shoot out another sap-vessel, I believe ; but the stem-vessel cannot shoot (in this situation, and thus aggregated) another root. This vessel has its little branch, flower, and fruit proportioned to its size ; it is impossible to know what each stem cylinder, with its accompanying root, will produce, because it cannot be traced higher in the tree than the trunk ; but from the root to that part I have often followed it in one lengthening string. Monsieur de St. Aubert, (who is pursuing the same course of study as myself,) confirms what I have now written, by dissections published just after my opinions, in this respect, appeared in your Journal ; and any one that *studies from dissections*, must, I trust, be of the same manner of thinking, the truth so plainly appears in them.

Radicles, with their mechanism.

To the root is added many radicles with all the mechanism necessary to collect and throw up the nourishment procured from the earth around. That every plant has the power, from all the variety of soils and decomposed matter, to select that which best suits its nature, and convey it to the bottom of the root, where all the juices meet, and are properly compounded and assimilated to the nature of the plant, is a certain truth : this general reservoir is found at the part where the root begins *to contract*, to form the sap-root. It is known by the quantity of alburnum laid up there. I have long been convinced, that alburnum is the congealed juice into which all the various nourishment of the earth turns; part forms into sap, part into the jelly of wood; for the sap is nothing but this juice in a dissolved state, and the wood nothing more than alburnum, having the wood and bastard vessels lengthening by degrees, and running through it. It is impossible, that any wood vessels can be formed by this juice, but the sap may easily be converted into that jelly-like substance, which forms the *alburnum*, and the rest of the process may be seen to pass under your eye in the solar microscope; that is, the bastard vessels may be seen to lengthen, and the sap-vessels to pierce through the softer substance, for the completion of the wood.

First part of the root.

The root may be divided into three parts; the first part shows the difference between the stem and root, the latter having *double* the *number* of wood vessels, and no pith; for after the first three or four years, the pith always disappears in the root of trees, and the line of life occupies the *centre* in its stead; indeed, as the chief use of the pith is to moisten the wood vessels, that they may bend in every direction, and thus facilitate the exit of the buds; and as the roots of trees have few buds after that time to throw from the root, the pith would no longer be of use in the centre; the bark and inner bark are nearly the same as in the stem, and the row of alburnum rather larger—that the wood should be *double* in the root, to what it is in the stem—and that it should increase according to the increasing branch, is the most absolute proof that the sap flows in the wood, since no other part would produce nourishment sufficient to support the tree. But no person who dissects trees, can doubt this truth, as the immense sap-vessels, and their being loaded *with sap*, must carry conviction to the most incredulous, provided they see it properly magnified.

No pith after the third year.

Second part of the root.

The second part of the root is that which appears to be the reservoir. It has all the parts already mentioned, except that the bark is narrower, and that the part usually occupied by the alburnum, has from three to five rows of that matter, instead of one; they are wide and juicy, and the quantity most irregular. I have often seen them *almost heaped together*, forming at once from five to seven rows; but I never saw more. (See fig. 1. Pl. 8.) The alburnum is loose and thin, and far more watery and unfinished in its appearance, than in the stem, or first

first root. The tap root is the third division—it is of the utmost consequence to attend to the shoots that belong to the different roots—it is the tap root which always forms the leading shoot of the tree; and if it is cut, it will, without doubt, spoil that part, by forming *two middle stems* to the tree; at least I have generally found this to be the case; and as the beauty of a tree depends much on the perpendicular height of its single pillar, the custom they have in most nurseries of curtailing the tap root, is a most vicious one. A row of alburnum is seldom found in this part of the root; for it increases this way but once in seven or eight years—its growth is, indeed, in a different manner, shooting from the end; for if I sever the smallest piece from the tap-root, it will very soon throw out two ends, and if these are cut, two more will be added to each, and it then ceases to shoot perpendicularly; losing its form, and then growing like a common root, whereas a *tap-root* draws out at the end like a telescope, one inch each shoot; and if it is dissected *with care*, two or three of these divisions will be found. What is the use of the tap-root? By shooting perpendicularly down, to fix the tree *firmly* in the ground, and keep it straight in that position; then it is surrounded by radicles which perpetually pump up from every different soil, as it proceeds in depth, what other roots cannot attain, matter which, mixed with what the higher grounds bestow, serves to bring a variety to compound the different ingredients required for the various nourishment of the tree, probably minerals are wanted to form the juices of the *bark*; and I doubt not that the deep descent of the tap-root is most necessary to the *health* and *vigour* of the tree. How improper, then, is the custom of cutting it, and curtailing also many of the other roots, each of which has its appropriate branch, which will, of course, suffer in decay for the delapidations produced by the ignorance of the gardener. But the loss of the tap-root can never be remedied. It can no longer serve as a *deep well*, to gain not only a quantity of moisture from the number of rills it may meet with in its descent, but also matter from a variety of soil, and innumerable productions it passes in its way. The tap-root is, then, like the radicles, only a larger pump to collect and throw up all that it can select of water and other juices. The second part of the root is the reservoir for collecting the materials;

Third division, or tap-root.

Use of the tap-root.

Never to cut the roots.

rials; and the third part is the laboratory for forming each different gas and juice necessary to the health and habits of the tree. I may well add a fourth; for the radicles are the collectors sent out on every side to seek fresh provisions, to augment the stores, and increase the riches of this little habitation. They have all the mechanism appropriated to the purpose, and the bark and rind are so joined, as to serve, not only to protect and defend them from the stones and insects within the earth, but also to pierce and make way for them through the hardest materials; for they possess that softening power which enables them, I may say, to eat their way into every substance. It cannot be doubted, that they possess this faculty as well as *buds*, since I have perpetually found them dividing roots, piercing through the hardest wood, and even separating *stones* where any little defect assisted them. That a tap-root, or *any root* that is injured, should be *cut off*, there can be no doubt, since the danger of the rot is greater than any other inconvenience—but the greatest care (when trees are to be transplanted) should be taken not to hurt the roots, and if any radicles can be preserved, by wrapping them up in fresh earth, it should be done; for if they will live a little time, it *will be a great gain* to the tree; and here is the advantage of having the pit ready dug, and removing the plant, with all the earth around it—it preserves the few radicles alive, and enables them directly to perform their office of pumping moisture and nourishment from the earth—but if the tree is taken out some hours before it is replaced, all the *radicles* are sure to die; and if the tap-root is also injured, no wonder they never make fine trees; or that those planted by nature are always found superior. The reason that throwing a quantity of water into the pit has been found serviceable, *is*, that it supplies moisture, and quickens the growth of the new radicles; and what is still more advantageous, and should be *constantly done*, a large barrow of *good mould* should be thrown on the roots, and about the radicles; for a young and tender shoot, if it has to pierce through clods of earth in its sickly state, will certainly fail. It is like easily digested meat to a weak stomach—if you load it with heavy food at first, it destroys it at once; but let it gain strength and vigour, and a well-conditioned *radicle* will pierce through stone walls in time. It may be supposed, that according to the va-

Defect of cutting the tap-root.

The radicles.

Root to be cut if injured.

Necessity of removing trees quickly.

riety

riety required to compound the juices of the tree, such is the depth or shallowness of the root. The oak and ash are two of the deepest rooted of our forest trees, and should not, therefore, be planted close together—they may injure by intersecting each others roots—indeed, the greatest care should be taken, that even trees of the same kind (much more if they are not so) should not shoot their branches so as to cross or lie on each other. It is inconceivable the mischief they do, whether root or stem branch—in the first, it is not so easy to guard against the evil; but at their original planting, great care should be taken to place their roots regularly and even in the ground, and not allow them to cross, in which case nature herself, with the utmost diligence, will avoid another root's covering them. But it sometimes happens both above and underground—and in the first, *when seen*, it should directly be remedied, for nothing brings the rot so soon. They either both contract into so small a compass as to injure each other, or one gets the better and destroys the other, or the dispute carries the rot into both—for they will not continue to lie one on the other, without receiving, or doing injury: but, first losing bark and rind, the upper one, pressing on the other, in a few years pierces it, and then the trial of strength begins between each separate set of the sap-vessels. I have some curious specimens of this kind, well worthy being presented to the public attention, as giving a thorough insight into the nature of a tree, and as admirably pointing out the consequential parts of a plant, which, of course, are always the last to give way.

Not to cross their roots,

and not to cross stem branches.

I shall now turn to the manner in which the roots of trees are folded (if I may so express myself) in most forest trees, such as the elm, the oak, the ash, &c. They are laid exactly like a circular fan, their folds meeting in the centre, and apparently doubled over at the bark. This is admirably seen in the oak, still better in the lime tree: but most visible in a good double microscope—and so excessive is their predilection for this shape, that cut them ever so straight, nay, *plane* them smooth, and in a few hours (if the wood retains any vigour of muscle) the ribs will again evidently appear to be rising, and the finger which passes over, will be able to mark its motion, or at least will feel the height the muscles have gained. In some roots I have measured the rising, and found it to be above

Various folds of the root.

the tenth of an inch in twenty-four hours. Think what it must be in fresh and living plants! See the different sorts of figures into which the oak, &c. folds from that of the firs. The first fig. 2. BB, the second fig. 3. CC. showing it more plainly than in the circle. Let not the reader suppose, that this is the common warping of wood; it is the regular *fold*, always in one figure, and which goes off long before the wood is dry, and only retains it, like the animal muscles, a little while after life has ceased to linger in it. It is the last power of the muscular fibre of the wood, or rather of the spiral wire. I have not yet mentioned this as forming part of the root, because it does not (as in the stem) occupy a separate division in surrounding the nearest sap-vessels to the vital part. But in the wood of the root it circulates round *every sap-vessel*; separating each cylinder of wood, and meandering *on it* from one sap-vessel to another. (See fig. 4.) Besides penciling out the folds in three distinct rows of spiral wires, in each yearly increase, as at fig. 5. I doubt not, therefore, that it is the spiral wire which causes this peculiar motion, and I am the more persuaded of it, because, though the motion of the firs acts in such a reverse manner, yet the spiral wire accompanies it, so as equally to affect the motion, though in a perfectly *different direction*. What, then, can be said against the spiral wire being the cause of all motion in *plants?* The more I see of vegetable life, the more I am convinced of this reality. From the first I trusted to nature to *prove her own truths*; and she will do it, because I am most careful never to *make one for her*. The spiral wire never retains its power of motion above thirty-four hours after it is taken from a plant, and it is nearly the same with the root. I shall now indicate the extreme difference observed in the roots of firs, when compared with that part in other trees. I must beg Pliny's pardon for detecting him in an error when he says, "that he saw a fir tree planted, whose tap-root measured twelve feet." Now unfortunately, no fir has *any* tap-root. Such is the aversion their roots have to piercing the earth, that in young trees they will frequently be found with their roots bent back, and thus forming a hook or loop. (See fig. 6.) The roots of the Weymouth pine and silver fir, generally divide into *threes*, and run an amazing way horizontally under ground; but the Scotch and Spruce firs run

Folds caused by the muscular fibre.

Spiral wire which causes it.

No tap-root in firs.

in every direction, and a very little depth for such large and high trees. The division of the root, indeed, takes place in all the firs much higher in the stem than in other trees. None of the firs have any reservoir of alburnum, as is found in the second part of the root of the oak, &c. or I have not yet been able to discover it, from their not having a tap-root to guide me in the search; but I shall look more carefully. I have, however, dissected more than four dozen fir trees of all ages and sizes, growing naturally, and transplanted, not only in seeking *that*, but the tap-root also, but in neither have I succeeded. The tap-root in other trees, though, from being removed, it has branched and lost its shape, yet is always known; and so it would be in the firs, I doubt not, *if they had any*. But in all I have seen, there is not the smallest appearance of one, and if one of the side roots has been by accident turned down, its increased shape, on one side, shows that it was originally a side root forced into another situation—yet the *exact Evelin* also is mistaken. There is some variation in the bark and rind—they are not exactly like those of the stem. At first I thought they were not composed of leaves, as the coverings of the trunk of all firs are (see my letter on the subject in your Journal, 33;) but that was undoubtedly my mistake—they are formed of leaves, but thinner than the stem. The greatest part of the bark division is engrossed by that curious matter which separates the inner bark from the alburnum, and even in old roots is discovered to be of the most silvery whiteness; and its situation has so changed its very *nature*, that, instead of a thin division of hard rough wood, it appears like the most beautiful soft white leather. Surely its changing *thus* must show the excessive power of the juices of the firs in softening and tanning leather, and its vast superiority over the oak bark, or that of any other tree now made use of for the purpose—for though the oak has a very thin layer of this same matter to keep the debilitating juices from the wood, it is not softened and emolicated by its liquid, nor has it attained any thing like the suppleness or delicacy of that which encloses the fir, (as all may see that examine it) though they are both woods originally of nearly the same degree of roughness and hardness. I most sincerely wish that Sir H. Davy, or his brother, would turn their attention to this subject—it is very unlikely that nature would

Dissected many firs.

Bark and rind not quite the same.

Superiority of the juices of the fir.

Wish that some chemist would try it.

give

give such a proof of this, if it was not thoroughly worthy the strictest attention. I have not yet described all the motion that belongs to wood in the roots: as this is equally found in firs as in all forest trees, I have retained it as the last explanation. It is inconceivable what alteration *two inches,* or *even one,* will sometimes produce in a piece of root. I have seen the pith change its place two or three times in as many inches. I have now a piece by me where all the ovals are within one another, and of sixty in number, change in less than three inches in length to three regular circles, each circle bearing interlacing figures of fifty-nine each. Conceive what must be the motion of wood, that would, in so short a space, produce such a revolution of *form* and *figure!* it was only in tracing each sap-vessel in length, that I could persuade myself of the reality; the stems of trees are infinitely more quiet in this respect, and generally retain the pith at the same side, unless some cause, such as rot, or any injury happening to force a change. But it is very different in the root; that perpetual motion appears necessary to it is certain, and I doubt not contributes to its health; and, by seeking further, we may be the means of developing many of the disorders of wood. How often we find, that laying the earth lighter on the roots gives fresh vigour to the tree: this knowledge of its motion will open a source of refreshment of great consequence, I should hope, to their general health. In short, the more we are acquainted with their inward structure, the more we shall be able to administer to their diseases. In this situation it may be compared to the advice given in a surgical case by a person a proficient in anatomy, and one wholly ignorant of it—he who is best acquainted with the formation is more likely to hit on the real disorder. May we not, therefore, hope, that, by gaining a thorough knowledge of the interior of plants, it may, in time, lead us to an acquaintance with their diseases, and give us some notion how to remedy them. I am collecting a set of specimens to show the different disorders in trees and their causes; and when I am advanced enough, shall lay them before the public.

Knowledge of the interior of plants

of use in gardening.

I am, Sir,
Your obliged Servant,
AGNES IBBETSON.

The

The roots of fruit trees, herbaceous and annual plants, will be given in my next letter.

References to the Engraving, Plate VIII.

Fig. 1. View of the five rows of alburnum found in the second part of the root.

Fig. 2. View of the sort of folding of the oak and all forest trees. Whether the shape of the wood be circular, oval, or any other shape, it always folds in this manner.

Fig. 3. View of the manner of folding of the firs : it all joins underneath, though so much doubled under.

Fig. 4. View of the piece of wood showing the spiral wire running round the sap-vessels as it does in every slender cylinder of the wood. They are so thin, that more than 150 may go to an inch in length.

Fig. 5. The wood showing the penciling of the folds by the spiral wire on the oak.

Fig. 6. The hook of a young root of fir.

Fig. 7. The root of the Weymouth pine reduced.

Great care should be taken, if the tree must come from the nurseries, not to plant them too old. It is astonishing, when you dissect wood, what a difference there is between wood thus planted, and wood never removed and growing from the seed ; there is a regularity in the latter, an evenness of grain ; particularly if it is all the same degree of *hardness* ; not a piece almost iron in one place, and perfectly *soft* in another ; it is all equally firm and solid. But I have repeatedly found in trees planted, large alternate layers of hard and soft wood, that must make it almost useless to the carpenter. There is also another defect, which arises from allowing people to cut off large branches from trees : the piece thus exposed will either decay and get the rot, or will grow as hard as stone in the middle, while all the circular part will be as soft as pith : if, therefore, three or four large branches are cut from a fine oak, at ever such a distance of time, it will render that tree extremely inferior, in point of wood, to that which never lost a large branch. If a branch grows too low, it should be cut off at first shooting, when it can have no bad effect. There is not in nature any thing which deceives so much, as *buying trees* while standing, unless the person is very knowing in wood, so various are the hidden

hidden and concealed infirmities they are filled with, that, till sawn out into boards, the juice is not to be known, and the wisest may be taken in.

III.

*Popular Statement of the beautiful experiments of Malus, in which he has developed a new property of light.**

A solar ray is thrown in the meridian, to make an angle of 19° 10′ with the horizon: it is then reflected by a clear glass perpendicularly down:—and then by another clear glass, so as to pass with the same inclination to the horizon as before. Nothing remarkable happens if the ray be thus directed either N. or S. But if E. or W. the second glass reflects no light at all.

LET a solar ray be directed by means of a heliostat into the plane of the meridian, so that it shall form with the horizon an angle of 19° 10′. Then fix a glass, not silvered, in such a manner, that it shall reflect this ray vertically downwards. If below this glass a second glass be placed, exactly parallel to the former, the latter will make, with the descending ray an angle of 35° 25′, and reflect it again parallel to its first direction. In this case, nothing remarkable is observed.

But if this second glass be turned, with its face directed towards the east or west, but without altering its inclination with respect to the vertical ray, it will no longer reflect a single particle of light, neither from its first nor its second surface.

And if with the same inclination preserved with regard to the vertical ray, the face be turned towards the south, it will begin to reflect again the usual proportion of incidental light.

In the intermediate positions, the reflection will be more or less complete, accordingly as the reflected ray approaches more or less the plane of the meridian.

Under these circumstances, where the reflected ray comports itself so differently, it nevertheless constantly preserves the same inclination with regard to the incidental ray.

We, therefore, in this instance see, that a vertical ray of light falling on a transparent body, acts in the same manner when its reflecting face is turned towards the north or the south, and in a different manner when this face is turned towards the east or the west; although these faces still continue to form, with the vertical direction of this ray, an angle of 35° 25′.

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* See also Philos. Journal, XXX, 95. 161. 192.

These

Mrs Ibbetson on the Roots of Trees.

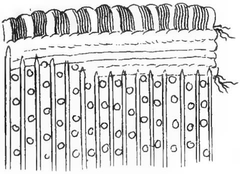

Fig. 1.

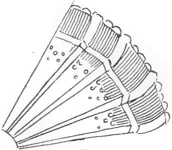

Fig. 2.

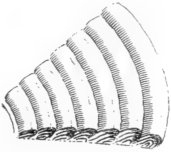

Fig. 3.

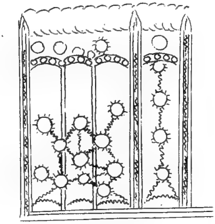

Fig. 4.

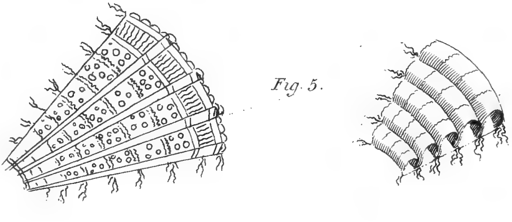

Fig. 5.

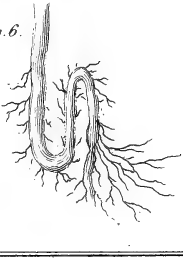

Fig. 6.

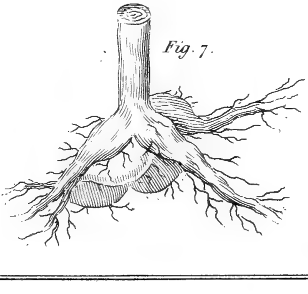

Fig. 7.

These observations lead us to conclude, that light acquires, under these circumstances, properties independent of its direction, with regard to the surface which reflects it, but exclusively relative to the sides of the vertical ray, which are here the same for the north and south sides of the ray, but different with regard to the east and west sides.

Light thus acquires properties relative to the sides of the ray.

By giving to these sides the name of poles, Malus has given the name of *polarisation*, to that modification which imparts properties to light, which are relative to these poles. And he says, that he has hesitated to admit of that term in the description of the natural effects now under consideration, until the variety of the phenomena obliged him to make use of it.

Modification, which the author calls polarisation.

Let us again, says he, consider the apparatus of which we have been speaking. If to the solar ray which has passed through the first glass, and of which a part has been reflected, a silvered glass be presented, which shall reflect it perpendicularly downwards, a second vertical ray will be obtained, which has properties similar to the first, but in a directly opposite manner.

The light transmitted through the first glass is polarised differently.

If a glass be presented to this ray, forming with its direction an angle of 35° 25′, and if, without changing this inclination, its faces be alternately turned towards the north and south, east and west, the following phenomena will be observed. There will always be a certain quantity of light reflected by the second glass, but this quantity will be much less when the faces are turned towards the north and south, than when they are turned towards the east and west.

The reflection will be much less when the second glass faces north or south,

In the first vertical ray, exactly the contrary may be observed. The *minimum* of reflected light took place when the second glass was turned towards the east or the west. Thus, in abstracting from the second ray the quantity of light which comports itself like a common ray, and which is reflected equally under both circumstances, it will be seen, that this ray contains another portion of light, which is polarised in a manner exactly contrary to that of the vertical ray reflected by the first glass.

which is directly contrary to what happens with the first vertical ray.

In this experiment a silvered mirror was used, merely in order to dispose the two rays parallel to each other, and under the same circumstances, in order to render the explanation more clear. The action of metallic surfaces being very weak with regard

The silvered glass is not requisite, and has little effect on the polarisation.

regard to the polarisation of the direct ray, their influence may be neglected.

Statement of the effect in general terms.

This phenomenon in the last analysis may be explained in the following manner: If a ray of light fall on a plate of glass, and form with it an incidence of 35° 25′, all the light which it reflects is polarised in one direction or manner. And the light which passes through the glass is composed, first, of a quantity of light polarised in a direction or manner contrary to that which was reflected, and having a proportion to that quantity, and secondly, of another portion not modified, but which preserves the characters of direct light.

The rays receive the same modification as by the double refraction.

These polarised rays have precisely all the properties of those which are modified by the crystals which have double refraction; and, accordingly, what the author has said of them elsewhere may be applied without restriction to the former.

The author, by continuing his experiments on the polarisation of light, has observed the following facts:

Iceland spar used to distinguish the condition of the transmitted rays.

I consider, says he, in order to fix the ideas, a vertical ray as polarised with regard to the plane of the meridian, and I place beneath this ray a glass, not silvered, in such a manner, that it can be turned round upon the ray so as constantly to make with its direction an angle of 35° 25′. In order to analyse the light which is transmitted through this glass in its different positions, I place beneath it a rhomboid of Iceland spar, directing its principal section in the plane of the meridian. I shall call the *plane of incidence* that which passes through the vertical incident ray and the ray reflected by the glass.

Statement of the effect as shewn by the chrystal.

The ray presents different phenomena according to the motions or positions given to the glass itself. When the glass has made a quarter of a revolution, it no longer reflects a single particle of light, and the ray that it transmits to the lower crystal is refracted in the usual manner; and, subsequently, the reflected light diminishes, and the refracted light increases, from the first position of the glass, until the plane of incidence has described an arc of 90 degrees. The ray refracted in the ordinary way by the rhomboid, also increases from the former unto the latter position; but the extraordinary ray only increases until the plane of incidence has arrived at an angle of 45 degrees. It is then diminished, and becomes nothing when the glass has performed one quarter of a revolution. Supposing, then,

then, that the glass makes one entire revolution, the reflected light will have two *maxima* answering to the positions N. and S. and two absolute *minima* answering to the positions E. and W. The transmitted light, and that which is refracted in the ordinary manner by the rhomboid, have two *minima* answering to the positions N. and S. and two *maxima* answering to the positions E. and W. but the light refracted extraordinarily has four absolute *minima* answering to the positions N. S. E. and W. and four *maxima* answering to the positions NW. SE. NE. and SW.

A metallic mirror substituted instead of the second glass.

It polarises the light with very striking variations, governed by the angle of reflection.

In the place of the moveable glass, but under exactly the same circumstances, let a metallic mirror be substituted, of which the plane or incidence constantly makes an angle of 45° with that of the meridian. When this mirror is inclined only a few degrees with regard to the horizon, the light which it reflects is entirely polarised, like the incident light with regard to the plane of the meridian. If the inclination be augmented, it reflects, first, a certain quantity of light polarised with regard to the plane of the meridian; secondly, another quantity of light polarised with regard to the plane of incidence; and, lastly, a certain inclination may be attained, by which the light is completely polarised with regard to the plane of incidence. Beyond this limit, the light polarised with regard to the plane of the meridian begins to reappear, and the intensity of the light polarised with regard to the plain of incidence, diminishes until the mirror becomes vertical. Metallic bodies act, therefore, exactly in the same manner as transparent bodies on the light which they reflect; but transparent bodies totally transmit the light which they polarise in one direction or manner, and reflect that which is polarised in a contrary manner, while metallic bodies reflect the light which they have polarised in both directions or manners.

The facts contained in this memoir point out the methods to be followed, in order to obtain, in the different cases, an exact measure of the phenomena. They resolve all that is problematical in this theory, and establish, in a decided manner, the following consequences—

All bodies polarise the light, but the effect is governed by

That all bodies in nature, without exception, polarise completely the light which they reflect under a determined angle. That angles of reflection peculiar to each. That within and beyond this angle the light receives this modification in a less complete manner.

It is essential to the reflecting force. Polished metallic bodies, which reflect more light than transparent bodies, do also polarise it in a greater degree. This modification is essential to the forces which produce reflection.

All the hypotheses to explain reflection are insufficient. Lastly, these new phenomena have advanced us one step nearer the truth, by confirming the insufficiency of all the hypotheses which philosophers have formed in order to explain the reflection of light. For example, it is certain, that not one of them tend to explain, why the most intense ray of light, when it is polarised, can, under a certain inclination, pass through a transparent body, and be totally deprived of the partial reflection to which ordinary light is subject.

IV.

Some Account of the Teak Tree of the East Indies. By Dr. WILLIAM ROXBURGH.

Introduction. THE durability of Teak wood for ship-building is well known to every one in India, and its qualities are so much valued in England, that considerable quantities are imported. The Society for Encouragement of Arts, from whose 30th volume the following paper is extracted, express their opinion, that this tree may be successfully cultivated in our West Indian and African settlements. And though it must be admitted, that the true national policy of an empire must ever be to establish those public resources which are least subject to the contingent events arising from the local distance of colonies, and consequently, that our great efforts ought to be to encourage the growth of native oak; yet it must be nevertheless admitted, that every possible means of insuring our supplies, and encouraging our settlements, ought to be adopted.

Great value of the teak tree of the East Indies. The timber of the teak tree is in India what oak is in England; it is, however, unnecessary to enlarge on their comparative value, because oak will not grow in India: our attention ought, therefore, to be confined to teak alone, not only as being by by far the best wood we yet know of in this country for ship-building, but also for the house-carpenter, and almost every other work where strong, durable, easily-wrought, light wood is required. The advantages to be derived from the cultivation of so valuable a tree, where nature has not bestowed it, must therefore be obvious to every one; particularly in Bengal, where it grows well, and the demand is so great. The teak tree is a native of Pegu.

Encouraged by government.

Government, sensible of what is here stated, have long given every possible encouragement for an extensive propagation. But to render it still more general, the native land-holders must be made sensible of the advantages they may expect to derive from large plantations thereof.

Its growth is rapid.

The growth of the tree is rapid, and at all ages the wood (from various experiments) appears excellent. Some trees in the Honourable Company's Botanic Garden, brought from the Rajahmundry Circar in 1787, were, in 1804, from three to upwards of four feet in girth, at three and a half feet above ground, and high in proportion*. These plants were about twelve months old when sent from the coast, so that their present age is about seventeen years. A tree promising so much advantage in so short a space, compared to what the oak requires in England to become serviceable in the marine yard, makes it highly worthy of every attention and encouragement. A few observations on rearing the plants from the seed seem necessary, as I have often known seeds from the same tree succeed with one person, and totally fail with another.

Seeds, and manner of planting, &c.

The nut in which the seeds are lodged, is exceeding hard, contains four cells, and in each is lodged a single small seed. It has been ascertained, that they perfectly retain their vegetating power in the growth, even as far as eighteen months; however, it is advisable to sow them about the beginning of the first periodical rains, or north-westers, after they are taken ripe from

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* The largest of those trees measured, at three feet and a half above the ground, in February, 1796, forty-two inches in circumference. The same tree was, in February, 1804, fifty-two inches in circumference at the same place, which gives an annual increase of one inch and a quarter. However, while the trees are younger, and in a more favourable soil than where this tree stands, their yearly growth is from two to three inches, which is fully double the increase of oak in England.

from the tree in October. If sown about this period, or rather before than after, in well-shaded beds, about an inch asunder, and covered with about a quarter of an inch of earth, with a little rotton straw or grass spread over the earth, to keep the beds in an uniform state of humidity, by gentle waterings, should the weather prove dry; most of the nuts will be found to produce from one to four plants, in from four to eight weeks. However, it sometimes happens, that many will remain in the ground until the commencement of the second rains, nay even of the third; however, this is rare, yet it will be adviseable to sow the seed on a spot that can be spared, at least until the rains of the second season are well advanced; by not attending to this circumstance, many have thought the seed bad, consequently caused the ground to be dug up for other purposes.

Appearance of the plants.

Time of transplanting.

The plants, when they first make their appearance, are very small, scarce so large as a cabbage plant when it first springs from the earth; their growth is, however, rapid. When they are about one or two inches high, they ought to be transplanted into other beds, at the distance of about six inches from each other, there to remain until the beginning of the next year's rains, when they are to be planted out to where they are to remain, or they may, when from two to four inches high, be planted out at once to where they are to grow; and it is not perfectly clear but by so doing they succeed better; as in taking up plants of any considerable size, say from one to two or more feet high, the roots are very apt to be injured, particularly the sap root, which retards their growth much, nay often kills them.

Considerations relating to the proper soil.

About Calcutta they thrive luxuriantly in most places where they have been tried, and any tolerable degree of care taken of them; so that the only observations which seem necessary to be made on this head, are to avoid sowing the seed, or planting in such places as are low, or subject to be inundated; to keep them clear from weeds, and sparingly watered during dry weather, for the first year only. In a good soil, not much overrun with that coarse, white-flowered grass, called by the natives Woola (Saccharum,) they will scarce require any care whatever after the first six months, from the time of being planted out where they are to stand. They will then be about eighteen months old, supposing them to have been transplanted twice; and

and in that time they will, in general, be from five to ten feet high, according as the soil is favourable, and out of all danger, except from north-westers.

Distances and thinning the plantations.

With respect to the distance at which plants ought to stand in plantations, every one's judgment can direct. The oak requires a great space, as the crooked parts thereof are the most valuable, and required for the knees and other curved timber in ship-building; but teak is naturally a straight-grained tree, and only used in Bengal, or at least in general, for the straight work, Sissoo being commonly employed for knees and other crooked timber; hence it may be concluded, that the straighter the teak trees grow, the more eligible for every purpose for which this timber is generally employed in Bengal. They do not, therefore, require to be planted at a great distance, suppose from six to ten feet, in quincunx order; by being so close they grow straighter, and protect one another while young, which is particularly wanted where violent gusts of wind, such as our north-westers, prevail. When the trees grow up, they can be thinned out to advantage, as the timber of the young trees will answer for a variety of uses. The seed of this tree we have now in such abundance, as to render a few hundred plants, in the hundred biggahs, of little or no importance; and if the ground on which they are planted is not of the best sort, the more necessity there is for planting close.

Suppose the trees planted in quincunx order, eight feet asunder, a Bengal biggah (which I believe is generally reckoned a square of one hundred and twenty feet) will hold about three hundred and ten trees.

Particular directions for the time of cutting down, &c.

It will be necessary, during the first ten years, to cut down about half of them, say one hundred and seventy, to give the rest more room; they are worth one rupee each.

Again, at from ten to twenty years, reckon half (eighty-five) of the remaining one hundred and seventy to be cut down, to make still more room for the rest, they will be worth four rupees each.

And again, at from twenty to twenty-five years, it may be necessary to thin them still more, say to another half, (or one-eighth of the original number) which will be worth eight rupees each. The remaining forty-two trees, when full grown, say in thirty years, may be expected to have, on an average, shafts

shafts or trunks thirty feet long, and at least four feet in circumference, which gives, according to the bases of timber merchants' measurement, a girth or square of twelve inches. The dimensions of such a piece of timber will therefore be thirty square feet, or three quarters of a ton, which, at one rupee (or about two shillings and sixpence sterling) per square foot, the average price of Pegu teak in this place (Calcutta) for some time past, will amount to thirty rupees per tree. Nor is it likely that the price of this indispensable commodity will fall; our growing trade, and consequent increase of shipping, gives reason to think it will rather rise in price. Let us, however, be on the safe side, and say, that each of the last-mentioned forty-two trees will be worth only twenty rupees each.

Value of the produce.

From the above statement, the value of a biggah of land, planted with teak trees, will produce, during thirty years, as follows:

	Rupees.
In the first ten years 170 are cut, and reckoned to be worth one rupee each, is - - - - -	170
In the next ten years 85 more are cut, and worth four rupees each, is - - - - - - -	340
In the next following five years 43 more are cut, and worth eight rupees each, is - - - - -	344
At the end of thirty years, the remaining 42 trees are reckoned worth 20 rupees each, is - - -	840
Total produce at the end of thirty years - - -	1694

Independent of the branches, many of the largest of which will be fit for knees, and other crooked timbers, of small dimensions, consequently of considerable value.

Reduction of rent, expences, &c.

From the above sum of 1694 rupees is to be deducted the rent of land for the before-stated time, together with the expence of planting, hedging, and taking care of the young plants during the first few years; after that they will require little or no care.

	Rupees.
The former let us suppose to be three rupees the biggah, which is certainly an high rent, and will amount, in thirty years, to - - - - - - -	90
Charges of planting and hedging, say - - -	20

Wages

Wages of one man, per biggah, which is fully sufficient, for the first five years, at 36 rupees yearly - -	180
For the next twenty-five years, allow one man to three biggahs, is for one biggah twelve rupees, or for twenfive years - - - - - - -	300
Total charges of one biggah for thirty-five years -	590
Deducted from rupees 1694, leaves a clear profit of	1104

Potatoes, leguminous, and culinary plants, meliorating crops, may, with advantage to the plantation[*], be reared in constant succession, on the same ground, during the first two or three years, or until the tops of the trees are too large to admit of their growth. The produce thereof will help to defray the expence of labouring the ground during that period; afterwards, as already observed, little more will be required than keeping up a fence round the plantation, to keep cattle and idle people from hurting the trees, till they are so large as to be out of all danger.

The ground may be profitably cultivated, and with advantage to the plantation.

A period of thirty years is only brought into the foregoing calculation, though it may well be imagined, that when in a healthy state, they must continue to gain considerably, both in size and quality, for a much longer period. In the *Bath Papers on Agriculture and Planting, Vol. 7, Article* 1, *Letter the Fourth*, a single oak tree is traced to have taken seventy-five years in acquiring a single ton; whereas in another seventy-five years, the same tree gave seven times as much in quantity, besides the increase in value as naval timber.

The observations relate to a period of thirty years. Subsequent years would give higher profit.

In addition to the remarks already made, it may be proper to add the following extract of a letter from Thomas Barnet, Esq. to G. H. Barlow, Esq. Chief Secretary to the Government, dated 8th November, 1799.

" A few years ago, a number of teak tree plants were, by

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[*] About six years ago my gardener trenched a piece of useless ground behind some cottages, and planted it with refuse elm suckers; thus prepared, the poor people availed themselves of the circumstance, set the ground with beans and potatoes, and have continued to crop it ever since; this has been of service to them, and of infinite benefit to the trees, which, by means of this annual culture, have outstript their undisturbed brethren, and almost doubled their contents. *Bath Paper, Vol.* 6, *p.* 17.

"orders of Government, I believe, disseminated in different "parts of the country, for the propagation of teak timber. "Amongst others, a few plants were sent to Rampore Bauleah; this was in 1795. These plants have throve in a sur"prising manner, and are, at this time, between twenty and "thirty feet high, and near a foot in diameter; the wood of "the hardest kind, and, as far as can be judged at present, "greatly superior to the teak of Pegu."

WILLIAM ROXBURGH.

Calcutta.
To C. Taylor, M. D. Sec.

V.

On some Combinations of Phosphorus and Sulphur, and on some other Subjects of Chemical Inquiry. By Sir HUMPHRY DAVY, *Knt. LL. D. Sec. R. S.*

1. *Introduction.*

The compounds of phosphorics and sulphur establish the notions of definite proportions in combinations, &c.

IN this paper I shall do myself the honour of laying before the Society the results of some experiments on phosphorus and sulphur, which establish the existence of some new compounds, and which offer decided evidences in favour of an idea that has been for some time prevalent amongst many enlightened chemists, and which I have defended in former papers published in the Philosophical Transactions; namely, that bodies unite in definite proportions, and that there is a relation between the quantities in which the same element unites with different elements.

I shall not enter into a minute detail of the methods of experimenting that I employed; I shall confine myself to general statements of the facts. The common manipulations of chemistry are now too well known to require any new illustrations: and to dwell upon familiar operations, would be to occupy unnecessarily and tediously the time of this learned body.

2. *Of some Combinations of Phosphorus.*

Two distinct

In a paper read before the Royal Society in 1810, I have de-

described the mutual action of phosphorus and oxymuriatic gas or chlorine. I have noticed two compounds which appear to be distinct and peculiar bodies, formed by the union of the gas and the inflammable substance. One is solid, white, and crystalline in its appearance; easily volatile, and capable of forming a fixed infusible substance by uniting with ammonia. The other is fluid, limpid as water, and, as I have since found, of specific gravity 1·45; it produces dense fumes by acting upon the water of the atmosphere, and when exposed to the atmosphere gradually disappears, leaving no residuum.

combinations of phosphorus and chlorine.

1. Solid, white, volatile: and when comb. with ammonia, infusible.

2. Limpid, sp. gr. 14·5, forming dense fumes with the water of the atmos. and evap. wholly.

The composition of the white sublimate is very easily ascertained by synthetical experiments, such as I have described on a former occasion in the Transactions. By employing chlorine dried by muriate of lime in great excess, and making the experiments in exhausted vessels, and admitting solution of chlorine to ascertain the quantity of gas absorbed, I have ascertained, that three grains of phosphorus unite with about twenty grains of chlorine to form the sublimate.

The first, or the sublimate, contains 3 of phosph. and 20 of chlorine.

If the phosphorus be in great excess in the experiment of its combustion in chlorine, some of the liquor is formed with the sublimate; but to obtain it in considerable quantities, phosphorus should be passed in vapour through heated powdered corrosive sublimate. A bent glass tube may be used for the process, and the liquor condensed in a cold vessel connected with the tube.

The second, or the liquor, is most plentifully formed by passing vapor of phosph. through hot corros. subl.

I have not been able to determine its composition by synthetical experiments; but by pouring it gradually into water, suffering the water to become cool after each addition of the liquor, and then precipitating the solution by solution of nitrate of silver, I have ascertained the quantity of chlorine and of phosphorus it contains. 13·6 grains, treated in this way, afforded 43 grains of horn silver.

It contains 3 phosph. and 10 chlorine, or half the quantity contained in the sublimate.

It is evident, from this analysis, compared with the result of the synthetical experiments on the sublimate, that the quantity of phosphorus being the same, the sublimate contains double as much chlorine as the liquor.

When phosphorus is heated in the liquor, a portion is dissolved, and it then, when exposed to the atmosphere, leaves a film of phosphorus, which, when the liquor is thrown on paper, usually inflames: a substance of this kind was first procured

The liquor will dissolve phosphorus.

by MM. Gay Lussac and Thenard, by distilling phosphorus and calomel together; and it may be produced in the experiment with corrosive sublimate, if sufficient heat be used to sublime the phosphorus, or if there be not an excess of the corrosive sublimate. I have made no experiments, in order to ascertain the quantity of phosphorus the liquor will dissolve.

The sublim. combines with water, and after evap. affords sol. of phosphoric ac.

When the white sublimate is made to act upon water, it dissolves in it, producing much heat. The solution evaporated affords a thick liquid, which is a solution of pure phosphoric acid, or a hydrate of phosphoric acid.

The liquor, by similar treatment, affords crystals.

When the liquor is treated with water in the same way, it furnishes likewise a thick fluid of the consistence of syrup, which crystallizes slowly by cooling, and forms transparent parallelopipedons.

Gas emitted during its combustion,

which is hydrophosphorous acid.

This substance has very singular properties: when it is heated pretty strongly in the air, it takes fire and burns brilliantly, emitting, at the same time, globules of gas, that inflame at the surface of the liquid. This substance may be called *hydrophosphorous* acid; for it consists of pure phosphorous acid and water. This is proved by the action of ammoniacal gas upon it; when it is heated in contact with ammonia, water is expelled, and phosphate of ammonia formed; and it is likewise shewn by the results of its decomposition in close vessels, which are phosphoric acid and a peculiar compound of phosphorus and hydrogen.

Ten parts in weight of the crystalline acid I found produced about 8·5 parts of solid phosphoric acid, and the elastic product must of course have formed the remainder of the weight, allowing for a small quantity of the substance not decomposed.

Properties of the gas.

The peculiar gas is not spontaneously inflammable; but explodes when mixed with air, and heated to a temperature rather below 212°.

Its specific gravity appeared from an experiment in which a small quantity of it only was weighed, to be to that of air nearly as 87 to 100. Water absorbed about one-eighth of its volume of this gas. Its smell was disagreeable, but not nearly so fetid as that of common phosphuretted hydrogen.

When it was detonated with oxygen, it was found that three

three of it in volume absorbed more than five in volume of oxygen, and a little phosphorus was precipitated.

When potassium was heated in contact with it, its volume increased rapidly till it became double, and then no further effect was produced. The potassium was partly converted into a substance having all the characters of phosphuret of potassium; and the residual gas absorbed the same quantity of oxygen by detonation as pure hydrogen. When sulphur was sublimed in the gas over mercury, the volume was likewise doubled; a compound of phosphorus and sulphur was formed, and the elastic fluid produced had all the characters of sulphuretted hydrogen.

The gas consists of 4·5 oxyg. and 22·5 phosphorus.

It appears from these experiments, that the peculiar gas consists of 4·5 of hydrogen in weight to 22·5 phosphorus; and its composition being known, it is easy to determine the composition of the hydrophosphorous acid, and likewise the quantity of oxygen required by a given quantity of phosphorous acid to be converted into phosphoric acid; for, for every volume of gas disengaged, a volume of oxygen must have been fixed in the phosphoric acid.

Taking hydrogen as unity, the proportional no. of combustion of phos. will be 20.

And calculating for 174 grains, 30 parts of oxygen must be fixed in the 150 parts of phosphoric acid, and 20 parts of phosphorus disengaged in combination with 4 parts of hydrogen; and on the idea of representing the proportions in which bodies combine by numbers, if hydrogen be considered as unity, and water as composed of two proportions of hydrogen, 2, and one of oxygen 15[*], phosphorus will be represented by 20.

Compound of chlor. and phos. acted on by water give mur. acid gas and oxyg. combines with the phos.

When the compounds of chlorine and phosphorus are acted on by a small quantity of water, muriatic acid gas is disengaged with violent ebullition, the water is decomposed, and it is evident that for every volume of hydrogen disengaged in combination with the chlorine, half a volume of oxygen must be combined with the phosphorus[†]; and the products of the mutual decomposition of water, and the phosphoric compounds

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[*] Supposing 100 cubical inches of the gas to weigh 27 grains.——— 27—4·5 the weight of 200 cubical inches of hydrogen = 22·5 grains.

[†] This mode of estimation is the same as that I have adopted on a former occasion, except that the number representing oxygen is doubled to avoid a fractional part.

of

of chlorine are merely the phosphoric acid from the sublimate, and the phosphorous acid from the liquor, and muriatic acid gas; so that the quantity of phosphorus being the same, it is evident that phosphoric acid must contain twice as much oxygen as phosphorous acid, which harmonizes with the results of the decomposition of hydrophosphorous acid. For supposing water to be composed of two proportions of hydrogen, and one of oxygen, and the number representing it 17; then 174 parts of hydrophosphorous acid must consist of two proportions; 34 parts of water, and four proportions of phosphorous acid, containing 80 of phosphorus and 60 of oxygene; and three proportions of phosphoric acid must be formed, containing three proportions of phosphorus 60, and six proportions of oxygen 90, making 150.

And the facts shew that phosphoric acid contains twice as much oxygen as phosphorous acid.

It is scarcely possible to imagine more perfect demonstrations of the laws of definite combination, than those furnished in the mutual action of water and the phosphoric compounds. No products are formed except the new combinations; neither oxygen, hydrogen, chlorine, nor phosphorus is disengaged, and therefore the ratio in which any two of them combine being known, the ratios in which the rest combine, in these cases, may be determined by calculation.

Water and the phosphor. compounds shew completely the laws of definite combination.

I converted phosphorus into phosphoric acid, by burning it in a great excess of oxygen gas over mercury in a curved glass tube, and heated the product strongly. I found in several processes of this kind, that for every grain of phosphorus consumed, four cubical inches and a half of oxygen gas were absorbed; which gives phosphoric acid as composed of 20 of phosphorus to 30·6 of oxygen; a result as near as can be expected to the results of the experiments on the sublimate and the hydrophosphorous acid.

Phosphoric acid produced by combust. in oxyg. contains 20 phos. and 30 6 oxyg.

Unless the product of the combustion of phosphorus is strongly heated in oxygen, the quantity of oxygen absorbed is less, so that it is probable that phosphorous acid is formed, as well as phosphoric acid.

Phosphorous acid is usually described, in chemical authors, as a fluid body, and as formed by the slow combustion of phosphorus in the air; but the liquid so procured is, I find, a solution of a mixture of phosphorous and phosphoric acids. And the vapour arising from phosphorus in the air at common temperatures,

Common phosphorous acid is impure.

peratures, is a combination of phosphorous acid and the aqueous vapour in the air, and is not, I find, perceived in air artificially dried.

In this case, the phosphorus becomes covered with a white film, which appears to be pure phosphorous acid, and it soon ceases to shine.

A solid acid volatile, at a moderate degree of heat, may be produced by burning phosphorus in very rare air, and this seems to be phosphorous acid free from water; but some phosphoric acid, and some yellow oxide of phosphorus, are always formed at the same time.

Nature of the peculiar gas,

The peculiar gas differs exceedingly from phosphoretted hydrogen formed by the action of earths and alkalies and phosphorus upon water; for this last gas is spontaneously inflammable, and its specific gravity is seldom more than half as great, and it does not afford more than 1·5 its volume of hydrogen when decomposed by potassium; it differs in its qualities in different cases, and probably consists of different mixtures of hydrogen with a peculiar gas, consisting of 2 parts of hydrogen and 20 of phosphorus; or it must contain several proportions of hydrogen to one of phosphorus.

denominated hydrophosphoric.

I venture to propose the name *hydrophosphoric* gas for the new gas; and according to the principles of nomenclature, I have proposed in the last Bakerian lecture, the liquor containing 20 of phosphorus to 67 of chlorine may be called *phosphorane,* and the sublimate *phosphorana.*

3. *Of some Combinations of Sulphur.*

Sulphuretted hydrogen and sulphureous acid.

I have shewn, in a paper published in the Philosophical Transactions for 1810, that sulphuretted hydrogen is formed by the solution of sulphur in hydrogen, and I have supposed that sulphureous acid, in like manner, is constituted by a solution of sulphur in oxygen. There is always a little condensation of volume in experiments on the combustion of sulphur in oxygen; but this may fairly be attributed to some hydrogen loosely combined in the sulphur; and to the production of a little sulphuric acid by the mutual action of hydrogen, oxygen, and sulphur.

It is only necessary, if these data be allowed, to know the difference between the specific gravity of sulphureous acid gas and

and oxygen, and sulphuretted hydrogen and hydrogen, to determine their composition.

Weights of the gases,

In the Philosophical Transactions for 1810, page 254, I have somewhat under-rated the weights of sulphuretted hydrogen and sulphureous acid gases; for I have since found, that the cubical inch measures, employed for ascertaining the volumes of gas weighed, were not correct. From experiments which I think may be depended upon, as the weights of the gases were merely compared with those of equal volumes of common air, I found that 100 cubical inches of sulphureous acid gas weighed 68 grains at mean temperature and pressure, and 100 cubical inches of sulphuretted hydrogen 36·5 grains, and the last result agrees very nearly with one given by MM. GAY LUSSAC and THENARD, and one gained by my brother Mr. JOHN DAVY.

distinctly set forth,

If 34, the weight of 100 cubical inches of oxygen gas, be subtracted from 68, it will appear that sulphureous acid consists of equal weights of sulphur and oxygen, an estimation which agrees very nearly with one given by M. BERZELIUS; and if 22·7, the weight of 100 cubical inches of hydrogen be subtracted from 36·5, the remainder 34·23 will be the quantity of sulphur in the gas; and the number representing sulphur may be stated as 30; and sulphureous acid as composed of one proportion of sulphur 30, and two of oxygen 30; and sulphuretted hydrogen as composed of one proportion of sulphur, and two of hydrogen.

and their numerical composition.

From the experiments of MM. GAY LUSSAC, it appears that sulphuric acid decomposed by heat affords one volume of oxygene to two of sulphureous acid: from this it would appear to be composed of one proportion of sulphur to three of oxygen. I have endeavoured, in several trials by common heat and by electricity, to combine sulphureous acid gas with oxygen, so as to form a sulphuric acid free from water, but without success; and it is probable, that three portions of oxygen cannot be combined with one proportion of sulphur, except by the intermedium of water. Mr. DALTON has supposed, that there is a solid sulphuric acid formed by the action of sulphureous acid gas upon nitrous acid gas. But I find, that when dried sulphureous acid gas and nitrous acid gas are mixed together, there is no action; but by introducing the vapour of water, they

Other experiments considered.

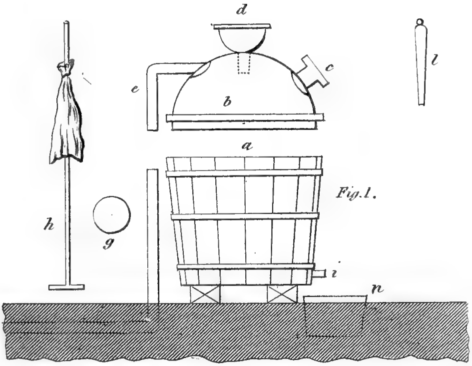


Fig. 1.

Manufacture of Pruſsian blue.

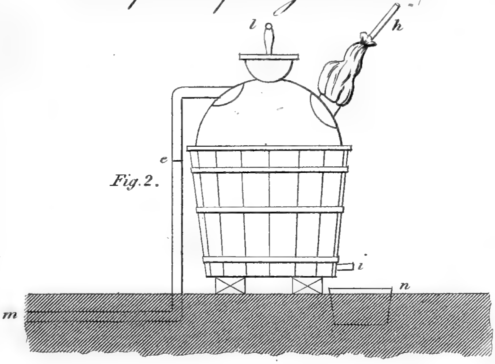
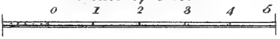


Fig. 2.

Scale of Feet.



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they form together a solid crystalline hydrate; which when thrown into water gives off nitrous, and forms a solution of sulphuric acid.

Chlorine and sulphur.

I have referred, in the Philosophical Transactions, to the combination of chlorine and sulphur. I have been able to form no compound of these bodies, which does not deposit sulphur by the action of water. When sulphur is satured with chlorine, as in Dr. THOMSON's sulphuretted liquor, it appears to contain, from my experiments, only 67 of chlorine to 30 of sulphur.

4. *Some general Observations.*

Observations. The proportions in phosphoric and sulphuric acid are the same, though the attractions greatly differ.

It is a fact worthy of notice, that phosphoric and sulphuric acids should contain the same quantity of oxygen to the same quantity of inflammable matter; and yet that the oxygen should be combined in them, with such different degrees of affinity. Phosphorous acid has a great tendency to unite with oxygen, and absorbs it even from water: and sulphureous acid can only retain it when water is present.

Most precipitates from water contain that ingredient.

The relation of water to the composition of many bodies has already occupied the attention of some distinguished chemists, and is well worthy of being further studied; most of the substances obtained by precipitation from aqueous solutions are, I find, compounds of water.

Thus zircona, magnesia, silica, when precipitated and dried at 212° still contain definite proportions of water. And many of the substances which have been considered as metallic oxides, that I have examined, obtained from solutions, agree in this respect; and their colours and other properties are materially influenced by this combined water.

Instance.

I shall give an instance. The substance which has been called the white oxide of manganese is a compound of water and the protoxide of manganese, and when heated strongly, it gives off its water and becomes a dark olive oxide.

Contraction of the pure earths by heat arises from the expulsion of water.

It has been often suspected, that the contraction of volume produced in the pure earths by heat, is owing to the expulsion of water combined with them. The following fact seems to confirm this suspicion, and offers a curious phenomenon.

Zircona, precipitated from its solution in muriatic acid by an alkali,

alkali, and dried at a temperature below 300°, appears as a white powder, so soft as not to scratch glass. When heated to 700° or 800°, water is suddenly expelled from it, and, notwithstanding the quantity of vapour formed, it becomes at the moment red hot. After the process, it is found harsh to the feel, has gained a tint of gray, its parts cohere together, and is become so hard as to scratch quartz.

SCIENTIFIC NEWS.

Geological Society.

Geological Society.

AT the meeting of this Society, Dec. 4th (the president in the chair) the reading of a paper by William Phillips, Esq. M. G. S. " on the views of Cornwall," was begun.

The regular or metalliferous views of Cornwall are found, with few exceptions, to run east and west. The known length of many of these views is considerable, amounting, in some instances, to two or more miles ; but their actual termination at either extremity has in no case been satisfactorily ascertained; all that is known being, that they gradually become so poor and narrow, as to make it no longer worth the miner's while to pursue them.

The dip or descent of the veins varies more or less from perpendicular, inclining towards the north or south, which inclination is called the *underlie* of the lode.

The depth of the veins is still less known than their longitudinal extent, not an instance having occurred of a vein being fairly worked out: many veins have indeed been relinquished, but only on account of the expences of working them exceeding the produce. The deepest mine now in work in Cornwall, is Dolcoath, some of the workings of which are 228 fathoms below the surface.

The usual width of the veins that are worked, varies from one foot to three ; in particular instances, however, portions of veins occur twenty-four feet, and even thirty feet wide ; and, on the other hand, a vein of tin, not three inches wide, has been followed with profit.

The

Geological Society.

The substances that accompany the metallic ores (or the vein-stones) vary considerably, not only in different mines, but in different parts of the same vein; and it is from these, and not from their metallic contents, that the miner's nomenclature of the veins is derived.

Gossan is a friable substance of a loose texture, consisting of clay, mixed more or less with silicious matter, and coated or tinged with oxide of iron. Its colour varies from light yellow to deep and brownish black. A *gossany* lode is more common than any other, and is considered as promising both for copper and tin.

When quartz predominates, the vein is called *sparry*; and if the quartz is considerably compact, it is looked upon as a very unfavourable indication, more especially if the vein becomes narrower as it descends.

If iron pyrites abounds, the vein is said to be *mundicky*. When this substance occurs at a shallow level, it is considered as not unpromising, more especially if mingled with copper ore as it descends.

A vein containing a large proportion of chlorine, is termed a *peachy* lode, and promises for tin rather than copper.

A vein is said to be *flookany* when one or both of its sides is lined with bluish white clay. It sometimes is so abundant, as to occasion considerable difficulty and expence to prevent it from slipping down, and obstructing the works.

When the contents of a vein consist of a hard compact substance, of a greenish or brownish colour, which appears to be chiefly a mixture of quartz and chlorite, the vein is denominated *caply*. Tin is often found in it, copper rarely.

When the ore, whether of tin or copper, is found in detached stones or humps, mixed loosely with the other contents of the vein, it is termed a *pryany* lode.

A vein abounding in blende, is called a *Black Jack* lode, and is considered as unpromising for tin, but a good sign for copper.

When a vein contains granite in masses or blocks, or in a state of semi-decomposition, it is termed a *growan* lode; and is generally considered as more promising for tin than for copper. Of late, however, many rich veins of copper have been found in the granitic districts of Cornwall.

The experienced miner by no means implicitly relies on even the

Geological Society.

the most promising symptoms, for all of them at times are found to mislead. The following, however, are those, in favour of which he is more especially prepossessed. All gosany lodes in general; the early discovery of pyrites with portions of yellow copper ore, also of blende and of gelina; and the cutting a good course of water, especially if it be warm.

The discovery of veins is effected in various ways.

The ancient method of *shoding* or tracing up water-courses, when pieces of ore are found to occur among the rolled stones in their channels, is now rarely resorted to. The common method is to work drifts across the country from north to south, by which all veins in the district thus examined, are sure to be cut through. Veins are often found in driving adits and levels for the working of known lodes; and not unfrequently are stumbled upon by mere accident in digging ditches and foundations for walls.

December 18.

The president in the chair.

The continuation of Mr. Phillips's paper on the Veins of Cornwall, was read.

The contents of a vein may be divided into those which are valuable, and those which are not so: the latter forming generally by far the largest portion, are technically called *deads,* and are left in the vein both to avoid the unnecessary expence of raising them to the surface, and for the very important purpose of preventing the two walls of the vein from collaping, and thus destroying the works: in addition to the deads, strong pieces of timber are frequently made use of. Sometimes large wedged-shaped fragments of rock, called by the miner *horses,* occur in the vein, partially cutting off the regular contents of lode, though seldom, if ever, entirely obstructing it. Veins of copper ore are, however, particularly liable to capricious and total obstructions, without any obvious cause. In proportion as the rock becomes harder, the vein always becomes more narrow.

One of the first objects in opening a new mine, is to drive an *adit* or horizontal gulley from the lowest convenient level, for the purpose of carrying off all the top water. One adit often serves two or three mines; and there is one, called the deep adit,

Geological society.

adit, which opens on one of the creeks of Falmouth harbour, the entire subterranean length of which is about twenty-four miles.

Copper veins, which, fifty years ago, were considered by the Cornish miners to be peculiar to Schist, have, of late, been found in the parishes of Givenass and Redruth to pass freely from Schist into Granite, and back again to Schist without any deterioration. The texture and hardness of both rocks is liable to considerable variation, affecting, of course, the profit and progress of the miner often in a very remarkable degree. Two shafts of Fluel Alfred were sunk in Schist, and the cost of one did not exceed 5l. per fathom, while that of the other amounted to 55l. for the same length.

The metalliferous, or east and west veins, are crossed by others, the direction of which is nearly north and south. These latter are called *cross courses,* and rarely produce copper or tin, or any other metallic substance. The principal practical advantage derived from these veins, especially when consisting of clay, is, that they oppose an effectual obstacle to the passage of water, and therefore the miners do not willingly pierce them without some adequate object in view. The disadvantage of them is, that they not only interrupt the course of the metalliferous veins, having them from a few inches to several fathoms; but not unfrequently totally impoverish them, so that a long and costly search after the heaved part of a vein, often terminates in the mortifying discovery, that it is not worth pursuing, as was most strikingly exemplified in the corresponding veins of Huel Jewel and Tol Carn.

There is another species of vein called a Contre or Caunter, the direction of which is, for the most part, NE. and SW. These are mostly, if not always, metalliferous, and often remarkably rich, of which the mines of Huel Alfred and Herland have afforded most splendid instances.

Phosphorescence of Bodies.

Deseignes continues his experiments on the phosphorescence of bodies. It was formerly published, that he found, that by violently compressing water in a glass tube, by a blow, it be-

came

came luminous. He has made the same experiment with a great number of liquids, which became luminous by the same treatment; such as olive oil, volatile oils, alcohol, sulphuric ether, acetous, and a saturated and boiled solution of potash, &c.

He ascertained, at the same time, that the temperature of all these liquids was at the same time raised.

Solid bodies likewise become luminous by compression. He filled the same tube with powdered chalk, and gave it a blow in the dark. The whole mass was then penetrated with a strong light, which disappeared like a flash of lightning.

He had the same results from flowers of sulphur, dried sulphate of magnesia, nitrate of potash, black oxide of manganese, ashes, powder of mica, and of vegetable coal, &c. and, in a word, every thing that was at hand.

The same bodies, struck with an hammer or an anvil, likewise gave atmospheric light; but particularly fluat of lime, phosphate of lime, and caustic lime; but sulphur, the metallic oxides by calcination, and burned alum, gave a very feeble light.

This difference seemed to him to have arisen from water in the solid state contained in these bodies. He ascertained this by the following experiments. He poured a few drops of water upon caustic lime, and it became very luminous by the blow. And the same effect was produced on other bodies.

Other experiments showed, that this light produced by compression is not electrical, but arises from the sudden approach of the particles of the bodies to each other.

Questions from a Correspondent on Subjects tending to encourage the Iron manufucture of this Country.

Has the making of iron had a gradual increase since 1800? and since that year, have considerable works been erected or established in addition throughout the United Kingdom, and principally where?

What

What quantity of iron may be now made in England, Wales, Scotland, Ireland—this is, iron into *bars*?

What may be the quantity in same manner of *cast iron* into various purposes for domestic and general use?

No doubt the British ore, if worked by wood, could produce equal iron to any we import; but is the quality of iron improved by working with coal at the present period, and is it possible by coal to make it equal to the foreign we import?

Is any quantity of British cast ware exported, as well as British iron in bars, and where to principally?

Is it not possible to manufacture all the iron we require for home consumption and exportation amongst ourselves? and by what means could it be adopted?

Does not the duty on the importation of foreign iron act as a bounty upon our own? or does the importation of foreign iron interfere with that of our own manufacture? and what means would be the most effectual to depress the importation of foreign, by a competition in the manufacture of our own?

Is it possible to increase the making of iron in the United Kingdom adequate to all wants and exportation without the danger of exhausting our own native resources, if the iron trade could be made the principal staple of the country?

In what part of the United Kingdom can the Iron be made the cheapest?

What may be the whole aggregate quantity of Iron, *cast in bars* in various iron utensils; and the whole manufacture of iron ware, from *native ore,* throughout the United Kingdom?

If the greatest quantity of iron is made in Wales, what may be the expence of conveying it up to London?

Meteorological Table for **1812**. *Extracted from the Register kept at Kinfauns Castle, the residence of Lord Gray, three miles almost due East from Perth, N. Britain, about ninety feet above the level of the ray.—Lat. 56° 24′.*

Communicated by his Lordship.

1812,	Morning, 8 o'clock. *Mean height of.*		Evening, 10 o'clock. *Mean height of*		Depth of Rain	No. of days.	
	Barom.	Ther.	Barom.	Ther.	In : 100	Rain or snow.	Fair.
January.	29·92	30·60	29·94	30·70	·72	7	24
February.	29·64	37·55	29·67	37·31	2·16	17	12
March.	29·96	33·46	29·99	33·30	·86	17	14
April.	30 09	38·40	30·11	36·71	1.38	8	22
May.	30·02	48·20	30 02	45·45	1·41	17	14
June.	30·01	54·17	30·02	52·00	2·89	12	18
July.	30·04	55·22	30·05	52·97	2·56	15	16
August.	30 09	55·10	30·19	53·16	2·38	11	20
September.	30·03	52·00	30·03	49·00	1·06	6	24
October.	29·47	45·00	29·50	45·10	3·18	16	15
November.	29 89	37 76	29·91	38·10	3·50	14	16
December.	30 09	34·00	30 14	35·00	·65	9	22
Average of the year	29·937	43·43	29·953	42·40	22·75	149	217

* * *

In the course of the present month, will be published, in one volume octavo, a Treatise on the Motion of Rockets, together with the Theory and Practice of Naval Gunnery; by W. Moore, of the Royal Military Academy, Woolwich.

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