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Engraved by C. Knight, from an original in the possession of M. Colnaghi.

Mr Ramsden.

London Published Aug^r 31 1803 by A. Tilloch, Carey Street.

THE
PHILOSOPHICAL MAGAZINE:

COMPREHENDING
THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
AGRICULTURE, MANUFACTURES,
AND
COMMERCE.

BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

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

VOL. XVI.



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THE
PHILOSOPHICAL MAGAZINE.

I. *Researches respecting the Organization of Leaves.* By
A. JURINE, Member of the Society of Physics and Natural
History at Geneva*.

WHEN two naturalists study the same subject without communicating to each other the result of their labours, two certain advantages arise to science. The first is, that when they agree in their manner of considering the same object, their opinion excites a greater degree of confidence: the second is, that when their opinions differ it induces them to make new researches.

The important discoveries published by Saussure, Hedwig, and other botanists, in regard to the organization of leaves, inspired me with a desire of examining after them objects so interesting. I therefore repeated their observations, and made new ones, which gave me different results. Of these I proposed to publish a detailed account, when I read in the *Journal de Physique* the memoirs of C. Mirbel on the anatomy of vegetables; and as my ideas in regard to these objects approach very near to his, I shall here only give an extract, as it were, of my researches on that subject, excluding all hypothetical reasoning.

Though the opinion of the celebrated men above mentioned is of great weight, I will venture to say that it ought not to be so far respected as to make one afraid of entertaining a contrary opinion, when it is founded, as I believe, on facts well established. When my ideas, therefore, are different from theirs, I shall enter into some details, in order to show better on what the diversity of our opinions is founded.

The existence of the epidermis of leaves is a question of too much importance in the physiology of vegetables to be neglected. I have therefore examined it with the greater care, as C. Mirbel has given an opinion entirely different from that of most botanists.

* From the *Journal de Physique*, Ventose, an. 11.

De Saussure found that the covering of leaves, to which botanists have given the name of *epidermis*, is not a simple membrane, but a real bark composed of three distinct parts; namely, *an epidermis properly so called, a cortical reticulation, and cortical glands**.

He defines the epidermis in the following words †:—"It is an exceedingly fine membrane, always transparent and colourless, in which no fibre, no pore, and no organization can be discovered;" though he allows that this membrane opens opposite to absorbent and excretory vessels.

Hedwig gives the name of *pellicle* of the leaves to what Saussure calls their bark. The *cuticular lymphatic vessels* of the former compose the cortical reticulation of the latter; and the evaporating *pores* or *conduits* of Hedwig are the cortical glands of Saussure. In regard to the epidermis, as this author does not mention it, there is reason to believe that he has confounded it with the pellicle ‡.

To enable the reader to comprehend better what I have to say in regard to the epidermis, I shall define what I mean by *utriculæ*, as I consider this definition necessary to prevent misconception.

The utriculæ are membranous vesicles filled with a particular juice, and contiguous to each other, in a greater or less extent of their surface, according to the form which they affect. The utriculæ constitute the greater part of leaves. They vary in their form and size; some of them being round, elongated, prismatic, or irregular.

All leaves seem to have a bark or exterior covering destined to contain the parenchyme and the vessels. In several plants indeed, and particularly the orchis, this kind of bark is seen to separate itself spontaneously by the effect of a peculiar alteration in the leaf. The surface of the leaves may also be frequently removed by employing caution: some then think they see the epidermis in this fine pellicle; and they are the more convinced of it, as they compare it to that of animals. But if this pellicle be examined with attention, it will be found, as I have always seen, that it is formed by the exterior stratum of the utriculæ, the lateral faces of which are contiguous. I shall, however, except the leaf of the *fritillaria* §, from which

* Observations sur l'Ecorce des Feuilles et des Petales; Geneve 1763.

† Chap. v. page 94.

‡ Sammlung seiner zerstreuten Abhandlungen und Beobachtungen über Botanische-œconomische Gegenstände D. I. Hedwigs; Leipsic 1790.

§ *Fritillaria regalis* Linn.

I was able to separate a pellicle, which carried with it only the exterior face of these utriculæ, as seen fig. 3 and 4.*

To find the epidermis of Saussure I repeated the same processes which he employed, varying them different ways; but instead of epidermis I always observed that the surface of the leaves was formed only by the external face of the exterior utriculæ; that this face was distinguished by slight rugæ, produced, no doubt, by exposure to the air; that it was indeed a little thicker than the other faces of the same utriculæ, and on that account exceedingly proper for discharging the functions of an epidermis. It needs not be objected that this thickness supposes an epidermis connected with the exterior face of the utriculæ, for in this manner we ought to admit nothing which is not supported by rigorous proof: but though I was never able to separate the epidermis from the surface of the leaves, I am in doubt whether I should thence conclude that it does not exist.

C. Mirbel entertained the same opinion as I do † in regard to the non-existence of the epidermis in leaves, and expresses it in the following manner:—"The exterior cells (which are my utriculæ) closely united exhibit contiguous surfaces, which many authors have considered as an epidermis. It was natural that they should believe in the existence of this organ, since they considered the vegetable as composed of fibres, vessels, and utriculæ. All these parts, in their opinion, being only joined or weakly united, ought to have a common connection or covering to retain them in their respective places: but, according to the facts which I have established, the existence of the epidermis does not appear to be necessary; and the more I observe, the more I am convinced that it does not exist. The sides of the cells exposed to the contact of the air and of the light undergo an alteration to which the interior parts are not exposed; they become dry, and are even separated sometimes from the cellular tissue. It is thus that they become a distinct covering, and it is then that they are destroyed: but if this pretended epidermis be carefully removed in the green parts, its continuity with the cellular tissue will be perceived; the vascular reticulation of Saussure is seen adhering to that exterior membrane; and this reticulation, when better observed, appears to be only the lateral sides of the cells, while the epidermis or membrane, which serves them

* The different figures referred to in this article will be found in Plates I and II.

† Journal de Physique, Prairial. an. 9, p. 443.

as a base, is nothing else than the junction of the exterior sides*.”

The observations even of Saussure seem to confirm my opinion in regard to the non-existence of the epidermis, since he expresses himself in this manner †:—“The system of the vessels which constitutes the bark of the leaves, and which touches on the one hand the parenchyme and the ribs of the leaves, and on the other the epidermis properly so called—this system of vessels generally adheres to its epidermis much stronger than to any thing else. It, however, sometimes happens that a part of the reticulation remains adhering to the parenchyme of the bark; but this is very rare. Continued maceration in cold or boiling water till the leaf is dissolved, separates very rarely the cortical reticulation from its epidermis. I never saw this separation take place but in the *sonchus spinosus*.”

May it not thence be inferred that this author was never able to separate the epidermis without removing with it the vessels, except in the leaves of the *sonchus*? This appears to me evident. But if I have always found in this fine pellicle the utricular organization, and if I am able to prove that the vessels of its cortical reticulation are nothing else than the lateral faces of the exterior utriculæ, may I not reasonably conclude that this epidermis is merely the external face of the exterior utriculæ?

The *cortical reticulation* of Saussure, covered by the epidermis, is composed of fibres or filaments, which by anastomosing form the meshes of this reticulation, and the form of these meshes varies according to the different plants.

This author says ‡: “In all the leaves which I have observed, four, five, six, and even seven filaments terminate at one and the same mesh: though six be the most frequent number, it does not thence follow that these meshes are regular hexagons; for the fibres which compose this reticulation are subject to such frequent inflections, and proceed winding in so irregular a serpentine manner, that in the greater number of species the meshes have no regular or constant form. The filaments which compose these meshes

* In my opinion, C. Mirbel is mistaken when he applies to the epidermis of leaves what Malpighi said respecting the epidermis of trees, namely, that it is formed by the desiccation of the utricule. Malpighi expresses himself in regard to leaves as follows, p. 37: “Tota exaratorum moles quibus folia commentantur, laevi superextensa corticuli seu epidermide abducitur, quæ subiectorum colorem refert, ipsaque contenta custodit et continet.”

† Page 40 and 41. ‡ Page 32 et seq.

appear transparent in their axes, and they are seen to anastomose with each other wherever they meet, and not to cross or to form knots in any manner. As these characters are suited to vessels filled with a transparent fluid much rather than to simple solid fibres, I have always considered them as vessels, and shall give them that name in future. The fineness and transparency of these vessels induce me to consider them as lymphatic vessels."

Hedwig observed the same reticulation, which he says is formed by vessels to which he has given the name of *cuticular lymphatic vessels*.

These two authors have committed an error in taking for vessels the lateral faces of the utriculæ which form the surface of the leaves; but this error, which is owing to an optical illusion, was easily committed. If the pellicle of a leaf, indeed, be removed, and be examined with the microscope, it will readily be conceived, that if the utriculæ which compose this pellicle be viewed from above, their lateral or perpendicular faces will appear shortened; and besides, as these membranes may be more or less inclined on account of their flexibility, the result will be, that the lower ridges will appear to be close to the upper corresponding ones, which will give the illusion of vessels of different diameters, the transparency of the axes of which will depend on that of the membrane comprehended between these ridges.

Fig. 10 will serve to give a better idea of the optical illusion which I have here endeavoured to explain.

As the lateral faces of the exterior utriculæ thus present themselves under the appearance of vessels, it is seen that the result will be a kind of reticulation, the meshes of which will vary according to the form of the utriculæ: for example, they will be rectangular, hexangular, or irregular, according as the utriculæ represent a parallelepipedon, a hexaëdral prism, or any other irregular form.

From what has been said, I entertain no doubt that if the authors I have quoted had examined the pellicle of the leaves in profile or in vertical sections, instead of observing them in front or from above, they would have found that what they take for vessels are merely the lateral faces of the utriculæ.

C. Mirbel in his memoir above mentioned seems to ascribe the errors committed on this subject to the same cause. He says, "The difficulty of observing, by the help of the microscope, the relation which exists between the planes placed in different directions, and at unequal distances,

tances, has fascinated the eyes of observers. The membranes which form the sides of the cells have always been taken for vessels or for fibres."

The utriculæ which compose the surface of the leaves have in general a form different from that of the utriculæ of the parenchyme, and a direction which is often opposite to them. In the *fritillaria*, for example, the form of the exterior utriculæ approaches that of a parallelopipedon very much elongated, in the direction of the length of the leaf (fig. 1. A, and fig. 5.), while the interior utriculæ are nearly spherical, as seen in fig. 7, 8, and 15.

The *erythronium dens canis* has its exterior utriculæ also elongated in the direction of the length of the leaf; they cross at right angles those of the parenchyme, which are cylindrical, and which consequently have their direction according to the breadth of the leaf. This disposition is not well observed but in the superior surface of the leaf.

The leaves of the *lavatera triloba* have their exterior utriculæ festooned like those of fig. 14; but the interior ones exhibit the form of a cylinder, the length of which is in the direction of the thickness of the leaf.

The exterior utriculæ of the leaf of the *silphium perfoliatum* are also festooned; while the utriculæ of the parenchyme are cylindric on the superior side of the leaf, and irregular on the inferior, like those of fig. 18.

In the *orchis maculata* the exterior utriculæ of the leaf are remarkable by their size, and particularly by their height: they alone constitute a great part of the thickness of the leaf.

I might multiply these examples; but I think I have said enough to prove what I have advanced, namely, that the exterior utriculæ of the leaves are in general very different from the utriculæ of the parenchyme.

Having examined the existence of the epidermis and that of the lymphatic vessels which form the cortical reticulation of the leaves, I shall proceed to the cortical glands of Saussure, or the evaporating pores of Hedwig; but I shall first say a few words of the manner in which the exterior utriculæ of the petals present themselves.

In most flowers the exterior utriculæ of the petals are conical; they rise in the form of teats, more or less prominent, on which the rays of light are broken and reflected in such a manner as to produce to our eyes that dark rich velvety appearance which gives them so beautiful an aspect.

To give an idea of the effect produced by these mammel-

lous utriculæ I shall compare them to that of velvet, which exhibits that appearance merely in consequence of the projection of the threads of silk above the woof.

Petals with a smooth surface do not exhibit the same phenomenon, because their exterior utriculæ being plane do not reflect the rays of light like the conical utriculæ; and in flowers, the petals of which are velvety above and smooth below, the utriculæ are seen to exhibit both these forms.

The surface of leaves is furnished with a very large quantity of small whitish points, which are scarcely apparent to the naked sight, but which may be clearly distinguished by the help of a good magnifying glass. These points are the *cortical glands* of Saussure, the *milliary glands* of Guettard, the *evaporating pores* of Hedwig, and the *cortical pores* of Decandolle.

Saussure says that the cortical glands are small oval bodies, often transparent and at other times opaque, surrounded by a fibre or vessel of the same form, and with which several vessels of the cortical reticulation anastomose. At the two extremities of each gland is a very delicate vessel, which traverses in a straight line the interval between the gland and the vessel that surrounds it, with which it anastomoses. He considers these glands, on account of their constant position near the surface of the leaf, as organs destined for preparing or secreting the peculiar juices which form the matter for transpiration, or for preparing and assimilating to vegetables the vapours and exhalations which they absorb by their leaves.

Guettard, without penetrating into the organization of these glands, contents himself with giving them the appellation of *milliary*, on account of their great number in certain leaves.

Hedwig first found that these organs are so many small apertures, which he calls *evaporating pores* or *conduits*. He says that each pore is surrounded by a circumference, and that the interval comprehended between the pore and the circumference is the reservoir of the pore. In regard to the use of these pores, he has no doubt that they serve for evaporation or absorption.

Decandolle has also given them the name of *cortical pores*, and seems to have adopted the opinion of Hedwig in regard to the uses of them.

Senecier, in his *Traité* on the Physiology of Vegetables, says that he was never able to discover, even with the help of the best microscopes, the pores of Hedwig, which

which I can ascribe only to an ambiguity occasioned by the meaning of the word, since the pores of Hedwig are only the cortical glands of Saussure, with which the illustrious author of that physiology seems to have been very well acquainted.

Mirbel calls these pores *exterior pores*, in contra-distinction to those with which the interior cells are perforated*: "Each pore †," says he, "is an oblong fissure surrounded by an elliptical area at which the neighbouring cells terminate. In these pores I can see only cells having a particular disposition, the exterior side of which is rent in a longitudinal direction. When the vegetable is exposed to the air and to light, circumstances necessary for transpiration, the fluids proceed in abundance towards the surface, the cells become elongated to receive them, and pierced to afford them a passage. Hence the formation of the cortical pores: but this elongation of some cells is, in a certain measure, only accidental; it is not invariably determined by the organic plan of the vegetable."

The diversity of opinions which prevails among the authors above mentioned induced me to study these organs with great attention, that I might endeavour to discover their real structure, and in this manner facilitate the explanation of their uses.

I have found that the surfaces of almost all leaves are perforated with a great number of small apertures, to which I shall retain the name of *pores*.

The size of these pores varies very much, according to the plants. In the orchis and lily kind they are very large, while in the sow-bread, the jessamine, the oak, &c. they are very small.

The number of them varies also according to the leaves. Hedwig counted 577 in a square line of the *lilium bulbiferum*. I counted 140 in the same extent of a leaf of the fritillaria, and 150 in that of the aloe; but in leaves where they are very small I was not able to count them very exactly, because they are too numerous.

In most plants the pores have an oval form, the direction of which seems to be subordinate to the form of the exterior utriculæ. Thus in the lily kind, the utriculæ of which are elongated, the large diameter of the pore proceeds in the direction of the length of the utriculæ, fig. 1 and 5; and in the leaves, the utriculæ of which are festooned, the pores extend in every direction, fig. 14. This rule, however, is

* *Journal de Physique, Fructidor, an. 9, p. 212.*

† *Ibid. Prairial, an. 9, p. 444—448.*

not constant; for in the white serapias, the utriculæ of which are also festooned, the pores are disposed very regularly in the direction of the length of the leaf.

All leaves are not in the same manner provided with pores: some have pores on both surfaces, others have them only on one; and some are entirely destitute of them. At the end of this memoir I shall give a catalogue of most of the plants which I have examined, and which I have arranged in three tables according to these divisions.

Trees in general have pores only in the interior surface of their leaves. Those, however, of the fir and the larch have them on both their surfaces; and the *juniperus sabina* and the common *juniper* exhibit pores only on the upper face.

But pores are not found in leaves alone: they are found in the seminal leaves, in most herbaceous stems, and in that of the *cactus flagelliformis* which has no leaves; they are also disseminated throughout the bractæ, stipulæ, calyces, and in the corolla without a calyx: I have even found them in the corolla with a calyx, though in less quantity; but I will not assert that they are all provided with them. In a word, I have observed them in the antheræ, the pistils, and pericarpium of the *lilium bulbiferum*; and I have no doubt that they are found also in the parts of fructification of other plants.

The existence of pores in the leaves having been fully established by various observations, I shall now show the manner in which I found them to be formed.

Were we to suppose that the pores are apertures formed by chance in the surface of the leaves, we should be in an error. Nature had some end in forming them, and the attentive observer will soon perceive that it has given to these pores a peculiar organization. Each pore indeed, when observed with the microscope, seems to be placed in the middle of a body nearly spherical, composed of two small uniform utriculæ, perfectly similar, opposed to each other by their concave faces and united by their extremities, the result of which is a vacuity or interval of an oval form, to which I have given the name of pore, fig. 1, 5, and 6.

These reniform utriculæ, which I shall call *conjugate*, concur with the exterior utriculæ towards the formation of the surface of the leaf; they are so closely united, that when the pellicle is removed they are removed with it, except in the fritillaria and aloe; which arises, no doubt, from the pellicle being formed, as already said, merely by the superior face of the utriculæ, the lateral surfaces of which adhere to the parenchyme

parenchyme of the leaf, and thus retain the utriculæ *conjugate* or *conjoined*.

To enable the reader to comprehend better what is here said, I shall refer to fig. 1, which represents a portion of the leaf of the fritillaria with the pellicle removed at B. It will first be observed that the leaf is greener in that part; that the lateral surfaces of the exterior utriculæ remain adhering to the parenchyme, and that they retain the utriculæ *conjoined*, in the centre of which the pore is seen.

Fig. 2 exhibits this pellicle removed, and pierced with as many oval apertures as there were *conjugate* utriculæ in the leaf.

The size of these apertures clearly proves that, besides the pore, a part of the *conjugate* utriculæ remains exposed to the air, which thus concurs with the exterior utriculæ to the formation of the surface of the leaf. Fig. 5 and 6 give a pretty correct idea of the manner in which the *conjugate* utriculæ are enveloped by the exterior utriculæ, and of the part of these utriculæ which remains uncovered.

The apertures here mentioned are not of the same form in the different plants: in the fritillaria they are always oval, while in the aloe they are square, fig. 13, which I can ascribe only to the form of the exterior utriculæ, which are elongated in the fritillaria and hexagon in the aloe.

The *conjugate* utriculæ are not united to the parenchyme by their lower part, as the exterior utriculæ are; below them is found a vacuity, as seen fig. 7, which communicates on one side with the pore, and on the other with the *utricular interstices*, which I shall speak of in describing the parenchyme. This vacuity is very remarkable in the leaf of the aloe, both on account of its size and its funnel-like appearance.

The *conjugate* utriculæ are filled with a juice which seems to be of the same nature as that of the utriculæ of the parenchyme; in general it is green and sometimes transparent. It often contains a great number of small globules, which render these utriculæ opake: but if the utriculæ be slightly compressed, all these globules unite as several globules of oil would do, and thus communicate to the utriculæ a part of their transparency.

Every time I observed the pores with the microscope, taking care to moisten the pellicle, they always appeared to me to be almost always filled with a black matter, fig. 1 and 14, which is nothing else than a small bubble of air retained there, and which I was enabled to detach by slightly compressing

pressing the pores with a fine needle, which restored to them their transparency. Are these bubbles atmospheric air, or a peculiar air produced by the plant itself? The following experiment may serve as an answer to this question.

I examined different pellicles taken from the surface of the leaves. I successively compressed the pores, and always observed the bubbles of air which issued to decrease gradually in volume, and at length disappear entirely. Whether they remained attached to the utriculæ, or to the point of the needle, or whether they rose to the surface of the water in which these pellicles were immersed, is not the absorption of this air by water peculiar to carbonic acid gas?

Hedwig entertained no doubt that the pores had the faculty of being able to shut themselves, to oppose the introduction of the floating particles disseminated in too great quantity throughout the atmosphere.

Krocker asserts* that the contraction of the pores of the leaves of the *amaryllis formosissima* takes place in a very evident manner; which induced me to examine this fact. For this purpose I placed in the focus of the microscope a fragment of the pellicle of one of these leaves, moistening it sufficiently to obviate its desiccation, and I viewed it very attentively without observing any contraction in the pores; but having substituted for it another fragment of pellicle, placed dry on the glass, I then saw the pores gradually contract and close themselves entirely; which I can ascribe only to the shrinking of the *conjugate* utriculæ, produced by the evaporation of their juices: for, having moistened this pellicle, the pores gradually returned to their natural state.

The result of this observation may give us an idea of what the pores of vegetables may experience when exposed to continued drought; and make us comprehend, at the same time, the beneficent influence which rain and abundant dews have on leaves.

We have considered the pores and *conjugate* utriculæ under a general acceptation: let us now speak of an exception which relates to the family of the gramineous plants, and which did not escape the acuteness of Hedwig.

* *De Plantarum Epidermide—Specimen inaugurale, &c. auctore Antonio Krocker. Halæ, 1800, p. 11.* Sæpius apertas (rimas) majores in plantis conspexi, quæ paulo post adhibito microscopio, ipsarum contractionem evidentissimè monstrabant. Observatori quamvis rusti, hoc phenomenon in foliorum tenuium epidermide amaryllidæ formosissimæ, quam maximè perspicuum adparet.

The *conjugate* utriculæ of gramineous plants represented in the pellicle of maize, fig. 9, form a narrow elongated body, instead of being nearly spherical as in other plants. It is placed in the middle of a kind of square area, produced by the disposition of the surrounding utriculæ. The *conjugate* utriculæ are applied to each other by their interior faces so exactly, as to make the pore disappear, and to exhibit in its place nothing but a longitudinal line, at the extremities of which is observed a small circle, which Hedwig considered as an aperture, and which, in my opinion, is formed only by the juice contained in the utriculæ, since I was able to make the appearance of this circle vanish by slight compression. Though the form of the *conjugate* utriculæ in the gramineous plants is always such as I have described, I have however remarked, both in the leaves which envelop the ear of maize, or in the interior face of the sheath of the leaves, and also in that of the leaves of the sugar-cane, *conjugate* utriculæ sufficiently reniform to construct the pore, as seen fig. 11. I have even observed some which had a perfect resemblance to the *conjugate* utriculæ of other plants, as represented fig. 12. Such a variety in the form of the *conjugate* utriculæ of the same plant is very remarkable. The details I have given, in regard to the structure of pores, seem to prove that these organs are essential to the greater part of vegetables: I however foresee that it may be objected, that they are not indispensably necessary; since it is said that by blanching a plant may be deprived of its pores. I shall observe in reply, that this assertion is not verified by my observations; for I have found pores on the blanched leaves of an orchis, which had vegetated below a piece of blackened pasteboard, on the blanched leaves of lettuce taken from the heart of the plant, and on the blanched stems of radishes, French beans, and potatoes, which had grown up in a dark cellar.

It may be objected also, that pores exist only eventually in vegetables, since aquatic plants, which never have any, assume them in those parts which are exposed to the air, and that plants which have pores lose them by vegetating in water.

I shall observe in reply, 1st, That if the leaves of the flower of the mesophyllum have pores, while those of the stem are without them, it is a proof that this flower was destined to rise above the surface of the water, and not a consequence of its vegetation in the air; that if the pedicle of the water-ranunculus has pores, while its stem is deprived of them, it is because it ought to live in the air, and not
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because it has lived there : 2d, That if the leaves of the nenuphar have pores on their superior surface and not on the inferior, it is because the former is intended to exist in the air, and the other in the water.

I shall here add, that the leaves of the nenuphar are provided with pores in the upper surface before they reach the surface of the water; and I have observed that the lower surface of the same leaves had not acquired pores, though they vegetated a long time in the air, in consequence of the desiccation of marshes; which proves that the contact of the air has no influence in the formation of pores which are seen on vegetables destined to be provided with them.

We are told by C. Decandolle, that having caused mint to vegetate under water, it shot forth leaves destitute of pores. To this observation I shall oppose one which I made on the leaves of the narcissus, which after having shot forth in water were furnished with as many pores as if they had vegetated in the open air, as I had reason naturally to expect, since the leaves, still contained in the bulb, exhibited to me their pores already completely formed.

It appears then that the existence of pores does not depend either on the light or the air; but that these organs are co-existent with the other parts of the vegetable.

[To be continued.]

II. *A general View of the Coal Mines worked in France, of their different Products, and the Means of circulating them.* By C. LEFEBVRE, Member of the Council of Mines, of the Philomatic Society, &c. &c.

[Continued from vol. xv. p. 345.]

Department of Oise.

NOTHING has hitherto been found in this department but very abundant strata of highly pyritous peat. Such are those in the commune of Beaurain, Guiscart, and Muyracourt, Fretoy, and several other places in the environs of Noyon. This peat can be considered only as a bad kind of fuel. By subjecting it to proper treatment, to decompose the pyrites, sulphate of iron (green copperas) and even sulphate of alumine (common alum) may be obtained from it. It is susceptible of spontaneous inflammation when exposed to the air in masses. It is much employed for manure, either before or after incineration.

Department

Department of Orme.

No coal mines are worked in this country. A great many indications have been announced, and some of them deserve to be examined, particularly that in the environs of Sécz, at Fontaineriaut, which has already been partially worked, and afforded some hopes.

Department of Ourthe.

This country (33) is one of the richest in coals in Europe, and the mines have been worked since the earliest periods. A great many coals are dug up in the neighbourhood of Liege, and even within the precincts of that city the pits are carried to a great depth; and powerful machines are employed to clear these immense cavities of water, and to draw up the coals.

The product of these mines is estimated at 43 millions of myriagrammes. They furnish coals of every kind. The mean price of those of a good quality is ten cents per myriagramme at the pits.

The coal mines in the department of Ourthe ought to supply Batavia; at least in competition with the English mines. If the suppression of the numerous tolls which shackle the navigation of the Meuse is strictly maintained, it will secure this advantage to the country of Liege.

Department of the Pas-de-Calais.

The coal mines of Hardinghen (34), seven leagues north-east of the port of Boulogne, are the principal ones worked in this department. The annual product of the mines in this department amounts to from six to nine millions of myriagrammes. The coals in general are not of so good a quality as those of the departments to the north of Jemappes; but mixed with a little of these coals they are of excellent use for forges: when delivered at the ports of Boulogne, Gravelines, and Dunkirk, they are sold for eight cents per myriagramme.

Department of the Puy-de-Dome.

The cantons of Montgie, Brassac, Auzat-sur-Allier (35) situated above the Issoire, exhibit several important coal mines, worked for a long time, and particularly those of Salles, Combelle, and Barre. That of Grosmeuil, which had been long abandoned on account of its being inundated with water, has been lately resumed by a company who have found means to overcome this difficulty. The products of these coal mines amount to about a million of myriagrammes

myriagrammes per annum. The coals cost about 15 cents per myriagramme at the pits. The mean price at Paris is 33 cents.

Departments of the Pyrenées, Upper, Lower, and Eastern.

These three departments have no coal mines worked. Indications have been announced in the department of the Eastern Pyrenées near Prades, and in the environs of Livia, in that of the Lower Pyrenées, at a small distance from Salies; but hitherto they have afforded very little hopes.

Departments of the Upper and Lower Rhine.

The coal mines worked in these two departments (36) are not of much importance. They are, however, valuable for the resources which they afford to the cantons where they are situated. The product of them amounts to about 200,000 myriagrammes per annum.

At Lamperlosch, in the canton of Soultz, in the Lower Rhine, strata of sand containing asphaltum are worked. This bituminous substance is separated, by particular processes, from the earthy matters with which it is united, and employed in commerce. It is applied to the same purposes as pitch, and when mixed with grease is used for daubing over the pivots of machines, axles, &c.

Department of the Rhine and Moselle.

Several researches have been made in this department in the hope of meeting with coals; but it does not appear that any mines have yet been worked in it. Some of the coals it consumes are brought from the departments of La Saarre and La Roër, but the greater part come from the mines situated on the right bank of the Rhine. It is estimated that these mines furnish annually 150,000 myriagrammes.

Department of the Rhone.

Coal mines are known in several places of this department (37), and particularly in that part which borders on the department of La Loire. The annual product, however, of these mines amounts only to 50 or 60,000 myriagrammes per annum.

Department of La Roër.

Very important coal mines are known at Eschweiller (38), Cornelins-Munster, Weisweiller, Bardenberg, and Heyden. The strata of Weisweiller are reserved. The product of the

other mines amounts to about 20,000,000 of myriagrammes per annum. The mean price of these coals at the mine is 11 cents per myriagramme.

[To be continued.]

III. *Observations and Experiments on the Light emitted by rotten Wood in the different Kinds of Gas, and in Fluids.*
By C. W. BÖCKMAN, of Carlsruhe*.

NOTWITHSTANDING the great number of accurate experiments and of ingenious observations which have been already made known by several eminent philosophers in regard to the light emitted by rotten wood in the different kinds of gas and other mediums, it is still difficult to explain this phænomenon in a satisfactory manner; and this difficulty is increased, because great variations occur both in the observations, and in the consequences deduced from them.

Thus Spallanzani found a perfect analogy between the luminous appearance of rotten wood and that of phosphorus †; and conjectures, that by the putrid fermentation of the wood its hydrogen and carbon come more readily into contact with the oxygen of the atmosphere; and that this combination is a slow combustion, which occasions the luminous appearance of the wood. In the non-respirable gases this, according to Spallanzani's opinion, cannot take place for want of oxygen; and he infers that every kind of rotten wood is not luminous, because the necessary quantity of hydrogen and carbon does not always happen to be extricated at the same time.

On the other hand, Mr. Carradori ‡ is of opinion, from other experiments, that rotten wood emits light without this slow combustion taking place, and that the non-respirable gases make on the wood only a transient impression capable of preventing the efflux of light, which, on the contrary, is promoted and increased by the peculiar action of oxygen gas. The observed decrease in the volume of the oxygen gas he considers as not decisive, because this de-

* Scherer's *Allgemeines Journal der Chemie*, vol. v. no. 1.

† See an Essay on the Phænomena of natural Phosphorus in Atmospheric Air, Oxygen Gas, and other Kinds of Gases, by L. Spallanzani. Gren's *Annalen der Physik*, vol. i. p. 1.

‡ See *Annalen der Physik*, vol. i. p. 2.

crease is produced by many substances without combustion, or without being exactly phosphorus. The above theory respecting this luminous appearance he thinks not altogether improbable, because the wood at the period when it begins to be luminous has, for the most part, lost its resinous particles, and therefore contains little hydrogen or carbon. He is of opinion also that rotten wood approaches nearer to phosphorescence the more it loses its inflammable matter, and that on this depends its susceptibility of absorbing and retaining light. According to Carradori's meaning, however, there is a greater difference between this natural phosphorus and that of Kunckel.

Humboldt*, that assiduous and philosophic observer, deduces from his well known experiments that the luminous appearance of rotten wood in general is possible only during its contact with oxygen gas; and that the wood, which loses its phosphorescence in the non-respirable gases, acquires it again immediately by the access of new oxygen gas.

In the last place, M. Gärtner†, in consequence of his interesting experiments on the luminous appearance of rotten wood, considers a certain degree of moisture as a necessary condition, and is of opinion that oxygen gas is less essential, even though the phosphorescence is promoted by it. But as this phænomenon differs so much from all the hitherto known processes of combustion accompanied with an extrication of light, he proposes this question: May not this phænomenon have more relation to the process of animal respiration than to real combustion? Or whether the luminous appearance of wood be not produced by the union of phosphorus and carbon in a certain proportion still unknown to us? But even if it should be admitted that during the process of emitting light water is decomposed, it is difficult, according to his opinion, to determine what becomes of the liberated hydrogen. M. Gärtner therefore considers it as still impossible to give a satisfactory explanation of the phænomena which occur during this process.

In consequence of the numerous experiments which I have made for several years past on Kunckel's phosphorus in the different kinds of gas, the most remarkable of which I have already communicated to the public in a particular treatise, I was desirous to see what phænomena would be exhibited in them by phosphorescent wood, and also in

* See *Versuche über die Chemische zerlegung des Luftkreises*; ix über die entbindung des Lichtes, p. 209.

† See his *Essay* in Scherer's *Journal der Chemie*, vol. iii. part 1.

other mediums : I hoped also that during these researches I might fall upon some new fact or idea, as is often the case, which might serve to confirm or to throw some new light on either the one or the other of these opinions. As soon therefore as I had procured some phosphorescent wood I began with it a series of experiments, a part of which, with the consequences deduced from them, I shall here lay before the public, after I have made a few previous remarks.

In regard to the wood itself, it was part of the old rotten trunk of a beech-tree, moderately moist, and without any particular mouldy smell. It was not luminous throughout, but emitted light only from its surface to the depth of a few lines. The luminous parts appeared to have lost in a considerable degree their resinous particles. They were friable, full of fibres, and whiter than those parts of the wood which emitted a weaker light, or had no light at all. I preserved the rotten wood in moist filtering paper in a cellar the temperature of which was from 10 to 12 of Reaumur; and in this place I made my observations during the night. The colour of the light was exactly the same as that exhibited by the light of artificial phosphorus in atmospheric air.

I used for my experiments, in general, small bell glasses capable of containing from 8 to 14 cubic inches each, having a neck at the top exactly shut by corks boiled in wax, through which passed a varnished wire. I stuck a piece of phosphorus on the wire in the inside of the bell; filled the vessel, according to the nature of the gas to be employed, either with water or quicksilver; and then placed it on the pneumatic tub. By the pressure which these fluids exercised on the wood, small air bubbles, which must have been contained in the substance of the wood, from time to time, escaped; and therefore before each experiment I took care to immerse the wood in water till no more air ascended, and by these means prevented the gases from being rendered impure.

Experiment I.

I filled a bell with atmospheric air, and preserved it closed by means of water. During the first two days the rotten wood remained luminous; on the third the light was somewhat fainter; on the fourth it had considerably decreased; and on the seventh the light had entirely disappeared. The wood, when taken out and exposed to the atmospheric air, emitted no light either when dry or when moistened with water. I then introduced into the remaining gas a piece of wood which was strongly phosphorescent: it emitted a
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bright light, and even at the end of twenty-four hours I could observe no decrease of any consequence.

The remaining gas, when subjected to examination by means of Fontana's eudiometer, in which I mixed it with the same quantity of nitrous gas, showed a decrease of 30 degrees. A taper immersed in this gas was immediately extinguished. Phosphorus evaporated strongly in it, and it rendered lime water pretty turbid, without a considerable quantity of the gas being absorbed.

Experiment II.

I filled a bell glass with oxygen gas prepared from oxide of manganese well washed with milk of lime. The wood immersed in it did not emit a stronger light than in atmospheric air or water. At the end of forty-eight hours the light seemed to decrease a little, and on the sixth day it was about a third weaker. The phosphorescence afterwards slowly decreased; on the 14th day it ceased entirely, and was not afterwards revived in the open air. The volume of the gas decreased very little, scarcely 0.2. Having put another piece of wood into the remaining gas, it continued to emit light without being weakened.

On trying the gas which remained in an eudiometer, it showed only a decrease of 21 degrees; and in about 15 seconds, when the red vapour of the nitrous acid had disappeared, I observed a faint whitish vapour from the gas floating over the water. For the sake of comparison I tried a portion of the same oxygen gas in a similar bell closed in the same manner with water, but in which no wood had been immersed, and found that in the same eudiometer it showed a diminution of 266 degrees. Artificial phosphorus, when placed in the remaining gas, became luminous and evaporated. A taper immersed in it was immediately extinguished: it was not inflammable, had no considerable smell of mouldiness, and rendered lime water turbid; but it was not absorbed by it in any considerable degree.

Having repeated this experiment several times, I obtained similar results, or results very little different. The phosphorescence of the wood, however, decreased once on the fourth day, and ceased totally on the seventh; though the gas, when subjected to proof, showed a diminution of from 80 to 120 degrees. This difference may have arisen chiefly from a difference in the nature of the wood; for it is not possible to obtain two pieces exactly the same in every respect. On the wood which had emitted light in oxygen gas I observed no mouldiness, nor any perceptible alteration.

It did not appear that the want of moisture was a principal cause of the cessation of the phosphorescence; for I found the wood often moist in a greater or less degree, and especially when it came in contact with the water by which the mouth of the bell was closed.

Experiment III.

I filled several bell glasses with azotic gas as pure as possible, which I had separated from atmospheric air by long continued agitation of an amalgam of lead, or by six months action of a solution of alkaline sulphuret, or by moist garden earth, and which tried in an eudiometer mixed with nitrous gas exhibited no diminution. The phosphorescence of the wood in this gas continued at first without any decrease, and as strong as in oxygen gas; but after from one to four hours it became weaker in the different bells: in some it ceased entirely at the end of an hour and a half, in others not till the end of from five to fourteen hours; the cause of which, in all probability, was the unavoidable diversity in the nature of the pieces of wood. After 24 hours I introduced into several of these bells from half a cubic inch to an inch of fresh azotic gas; but in neither of these cases was there the least appearance of light. But having introduced, with proper care, a new piece of wood, it emitted, in these as well as in the other vessels which had received no mixture of new azotic gas, as strong a light as in atmospheric air, and continued undiminished for some time. In some of the bells it was not extinguished till the end of 2, 4, or 5 hours, though no oxygen gas had been introduced.

Experiment IV.

I put into some of the bells along with the rotten wood small bits of phosphorus; and having introduced some of the above azotic gas, the wood and the phosphorus both began to be luminous. At the end of an hour the luminous appearance of the wood was considerably weakened, and it at length decreased so much that its light could no longer be distinguished any more than that of the phosphorus. At the end of 24 hours, when the light of both substances had already ceased for a considerable time, I introduced, with proper caution, a new piece of wood into the gas in which the former still continued luminous. A proof that by this operation no atmospheric air had been introduced was, that the phosphorus remained dark, and I could observe no luminous vapour in the glass. In about
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an hour and a half the phosphorescence of the wood had for the most part ceased. At the end of 24 hours I therefore introduced another piece of wood, which exhibited the same phænomena as the preceding. This operation was often repeated in the same gas. When the luminous appearance of the wood became weak, it recovered nearly its original splendour, in the course of a few minutes, on placing it in atmospheric air. I tried the remaining gas in the eudiometer, but could observe no decrease; which proves that the gas had remained free from any mixture of oxygen gas.

When a piece of luminous phosphorus is placed near the wood, it is difficult to determine the moment when the light of the latter becomes entirely extinct: for I found that the phosphorus generally remains luminous a considerable time longer than the wood, and even after its light is extinguished a somewhat luminous vapour arises; so that on account of this vapour it is not easy to ascertain when the phosphorescence of the wood ceases. It is equally difficult to observe, whether, on the admission of oxygen gas to azotic gas containing wood and phosphorus, the light of which is extinct, both these substances begin to be luminous at the same time, or not; for at first the gas is entirely illuminated by the luminous vapour; and besides this, the surface of the rotten wood becomes entirely luminous, in consequence of the phosphoric particles deposited on it; and hence it is difficult to determine whether the light proceeds from itself or from these particles. I often found such pieces of wood when taken out entirely penetrated with particles of phosphorus. This observation may be of utility to the future observer.

Experiment V.

I filled a common bell glass with phosphorated azotic gas, in which a considerable quantity of phosphorus had remained several weeks, at the temperature of from 14 to 24° of Reaumur, and in which fresh phosphorus neither evaporated nor became luminous. A piece of wood placed in this gas continued at first luminous, without any decrease of intensity. In about half an hour however its light became weaker, and in an hour entirely disappeared. Next evening I introduced into the same gas a fresh piece of wood, and observed the same phænomena. On introducing more phosphorus it emitted as little light as before.

Experiment VI.

I prepared impure phosphoric azotic gas by combustion and long exposure to heat, with a sufficient quantity of phosphorus shut up in a close vessel with atmospheric air. In this gas a piece of rotten wood continued luminous for an hour. Having introduced another piece of wood, the phosphorescence was the same as before; and at the same time artificial phosphorus emitted no light.

Experiment VII.

Rotten wood appeared phosphorescent in hydrogen gas, prepared from iron and sulphuric acid, in which phosphorus emitted no light; but in the course of 30 or 40 minutes it considerably decreased, and at length became entirely extinct. By the contact of atmospheric air the light was in some measure revived. As often as a fresh piece of rotten wood was introduced into the remaining gas, it became luminous. This experiment I several times repeated with the same result.

Experiment VIII.

Having placed rotten wood in carbonic hydrogen gas, prepared from the saw-dust of the beech tree, it became luminous at first, as in atmospheric air; but after 45 minutes the phosphorescence gradually decreased, and in about an hour entirely ceased. Every time I introduced a fresh piece of wood into the remaining gas I observed the same result. Artificial phosphorus in this gas gave no signs of light whatever.

Experiment IX.

I introduced rotten wood into phosphorated hydrogen gas above a year old, during all which time a considerable piece of phosphorus had remained in it, and which had been continually exposed to the solar heat. In this gas the wood continued luminous without any decrease of its intensity. At the end of an hour the light began to decrease, and in an hour and a half it had almost entirely ceased. Fresh wood introduced into the remaining gas exhibited the same phenomena: artificial phosphorus however gave no signs of light.

Experiment X.

I prepared fresh phosphorated hydrogen gas, which, as is well known, is so unfavourable to the luminous property of phosphorus, even in small quantity, in azotic gas or atmospheric

spheric air: in this gas the rotten wood was exceedingly luminous. Even at the end of an hour and a half I observed no decrease of the light; and it did not cease entirely till the end of several hours. Having introduced a piece of fresh wood, at the end of 24 hours it was as luminous as in atmospheric air, and the case was the same with the 4th and 5th piece which I afterwards brought into contact with it. At the conclusion of this experiment I could easily inflame the single bubbles of gas by means of a burning coal.

Experiment XI.

I introduced into a common bell glass over mercury strong fuming sulphurized hydrogen gas, disengaged from a solution of alkaline sulphuret and tartaric acid. A piece of rotten wood placed in this gas immediately ceased to be phosphorescent. If taken out when the light began to be extinguished, the light could in some measure be revived by washing it with water in atmospheric air. A fresh piece of wood introduced exhibited the same phenomenon. Want of oxygen gas was not, in some cases where this experiment was repeated, the cause of the light being suddenly extinguished; for the gas was not always perfectly pure. Artificial phosphorus would even at times emit in it a faint vapour.

Experiment XII.

In carbonic acid gas prepared with proper care from chalk and sulphuric acid diluted with water, and in which phosphorus, partly by a natural heat and partly by gentle heating in a vessel with hot water, had entirely ceased to be luminous, a piece of rotten wood retained at first its full phosphorescence: the phosphorescence however in the course of four or five minutes considerably decreased, and in 15 or 20 minutes no light was to be seen. Wood which had thus lost all its luminous appearance, when washed with water, seldom recovered its phosphorescence: if however it retained any light when taken out, it was somewhat strengthened in atmospheric air, but after some time greatly decreased, and at length entirely disappeared.

Rotten wood exhibited almost the same phenomena in carbonic acid gas, prepared with great care, but in which no phosphorus had previously been exposed.

Experiment XIII.

Having brought nitrous gas, mixed with about 0.11 parts of azotic gas, into contact with a piece of rotten wood,

wood, it at first appeared perfectly luminous: the light however speedily decreased, and in general ceased entirely at the end of from a minute and a half to three minutes: it was seldom renewed by washing the wood with water in atmospheric air. When a fresh piece of wood was introduced into this gas, its phosphorescence and the duration of the light were the same as before.

Experiment XIV.

I brought muriatic acid gas into contact with phosphorescent wood, and observed that in the course of from one minute to a minute and a half its light ceased entirely. This phenomenon took place several times, as often as a new piece of wood was brought into contact with the gas. It appeared to me, in this experiment, that moist wood sooner became dark than dry wood. The phosphorescence could not be revived by the usual means.

Experiment XV.

Rotten wood, placed in ammoniacal gas newly prepared, shone from one and a half to six minutes; and the decrease of the light was pretty speedy. When I took the wood from the gas, I observed that it had a strong smell of ammonia; and after being washed with water it assumed, in a considerable degree, its luminous property. The moister the wood, the more the phosphorescence decreased; and the gas was absorbed by it in the same proportion.

Experiment XVI.

Rotten wood appeared phosphorescent in newly prepared muriatic gas a shorter time than in ammoniacal gas; and when the light became extinguished, I was not able to revive it. A part of the gas was absorbed by the somewhat moist wood.

[To be continued.]

IV. *Of the general Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, and the Circumstances by which the former are influenced*.*

§ 1. ALL spirituous liquors may, with respect to their strengths, be regarded as compounds of two ingredients, alcohol, or pure spirit of wine, and water; and as differing

* From Atkins and Coy's Essay on this subject, of which we gave some account in our last volume.

only from each other in the proportion in which these substances enter into their composition. The former of these fluids, considered as in a state of chemical purity, or altogether unmixed with any heterogeneous substance, is, however, by no means well known. The most highly rectified spirit which has ever yet been procured has probably still contained no inconsiderable quantity of water, which it is the object of the process of rectification to separate; and though we obtain spirit which is more and more dephlegmated as we advance in our knowledge of practical chemistry, yet we have reason to believe that the ultimate point of absolute purity has never yet been attained.

§ 2. The latter of these ingredients being of no value, it follows that every such compound must, *cæteris paribus*, be appreciated by the quantity of the former which it contains; and we could therefore at once estimate its value if we could determine that quantity. It is not, however, absolutely necessary for this appreciation that we should be in possession of the actual quantity of alcohol which is contained in any given liquor: if we can discover the proportion which that quantity bears to the quantity entering into the composition of any other given liquor, we shall be in possession of its relative value, when compared with that of such other.

§ 3. This proportional value, therefore, will be no less truly ascertained, if, instead of considering alcohol as our standard in this respect, we should take spirit of an inferior strength as being so, and appreciate all spirituous compounds by reference to the quantity of such standard spirit which would be capable of producing or being produced by the given compound, by the addition of water to the strongest of the two till they were reduced to the same degree of strength. The real quantity of alcohol, properly so called, which is contained in any mixture, being, from our ignorance of this fluid in a state of chemical purity, impossible to be ascertained, we have naturally been obliged to have recourse to the latter mode of appreciating the values of spirituous liquors by reference to the relative proportions in which this hitherto unknown substance enters into their composition, which are obtained by comparing all of them with some other spirit of known strength as a standard; and this has accordingly become the practice in every country in which these kinds of liquors form an important article of commerce. The strength of this assumed standard is merely arbitrary, it being sufficient for all purposes that it be only certainly and precisely fixed. This, however, has unfortunately

unfortunately not hitherto been the case in this country, and is still less so in any other: the cares of government, in general, afford to those who are occupied by them but little leisure for abstruse research; and the appreciation of quantity and quality in general has hitherto, therefore, been in a great measure left to individuals.

§ 4. Flavour, odour, colour, and consistence, are the objects of our external senses, and the quality of a liquor in these respects is discoverable by their assistance alone; but a minute difference in the strengths of two kinds of spirit, which are otherwise similar, is not so easily detected: there are means of communicating to a liquor an apparently different strength from that which it really possesses, so long as its taste, smell, and appearance, are relied on as criteria by which it is to be estimated.

§ 5. Water and spirit of wine are of very different specific gravities: that of the former being, in a great measure, fixed and invariable at given temperatures, whilst that of the latter is liable to so much uncertainty that it has not hitherto been ascertained what is the real weight of alcohol, properly so called; the thing itself being, as we have before stated, scarcely known. A very few years ago it was conceived that a spirit whose specific gravity was 820* at 60° of Fahrenheit's thermometer was as perfectly free from any admixture of water as it was possible to render it; and yet we are now able with ease to procure it lighter: the specific gravity of the best alcohol from Apothecaries' Hall being very commonly considerably less. In some cases even a much greater degree of dephlegmation has been attained. Mr. Lewis, of Holborn, a very scientific rectifier, has obtained spirit whose specific gravity was less than 811 at 60°; and Dr. Black is said to have procured it so light as 800, or weighing only 4-5ths of the weight of an equal measure of water.

§ 6. When two fluids of different weights are mixed together, we may easily conclude that the specific gravity of the compound will bear some relation to that of its component ingredients; and it appears, therefore, to have been a very obvious idea that the relative proportion of each in such a mixture would be thus to be discovered. In the present improved state of science we see so much further than our

* It has been most usual with writers on specific gravities to consider that of distilled water as unity: we, however, have found it more conducive to the facility of denominating them, to consider it as 1000, and which is accordingly done throughout this tract. "Eight hundred and twenty," for example, is more easily expressed in words than 820.

predecessors, that, without considering the elevation of the ground on which we stand, we are almost induced to doubt whether their intellectual powers were equal to our own. The want of resource which our ancestors seem to have discovered in their attempts to ascertain the strengths of these liquors, by the shaking them in a phial, firing them over gunpowder, and a thousand other still more fallacious means, when the object which they had in view was so much more accurately and easily attainable by the simple operation of weighing them, really appears, at first sight, somewhat remarkable. The consideration of all the details relative to this subject involves a number of intricate points; yet the merely ascertaining the weight of a given measure of any liquor by a common pair of scales would doubtless have afforded a better indication with regard to its strength than any of the other modes of estimating it which are unconnected with the consideration of this property. It is now, however, sufficiently agreed upon, that the best method of ascertaining the relative values of spirituous liquors, with regard to their difference of strength, is by means of their specific gravities; and the principles of this method, therefore, will form the subject of the present tract.

§ 7. If a given bulk or measure of water and alcohol remained unchanged in every temperature, and whether these two fluids were mixed or separate, the ascertaining the real specific gravity of the latter itself, or the estimation of the quantity of each in any compound of the two, would be a matter of no difficulty: the simple rules of alligation would give all that could be required in this respect. This, however, is not the case: a variation of temperature, or the mixture of two spirituous compounds of different strengths, or of any such liquor with water, occasions a change in the aggregate bulk of the whole which is necessary to be taken into consideration; and it will therefore be requisite to treat separately of the effect of each of these operating causes.

§ 8. It has long been known that all bodies in general, whether solid or fluid, expand by heat and contract by cold; and it follows that the same quantity of any fluid which, when at an elevated temperature, would fill a measure of given dimensions, must fall short of doing so if the temperature should be lowered; and, *e converso*, the same quantity which would be contained in the measure when cold would when heated be more than sufficient to fill it, though the absolute weight of the whole of the fluid would still continue unchanged. In speaking, therefore, of a measure
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of any liquid, we indicate nothing with respect to its real quantity, unless we at the same time express the temperature at which it is to be measured.

§ 9. If an equal change took place in the bulks of alcohol, and of water, and of every compound of them, by the same elevation or depression of temperature, the application of the necessary correction for this circumstance would be still easy. The difference in this respect is, however, very considerable: water increases only about 1-300th of its bulk by an elevation of its temperature from 40° to 80° of Fahrenheit's thermometer, whilst alcohol would, by a similar change of temperature, increase in measure no less than 1-43d of the whole, or seven times as much as the other; and liquors of intermediate strengths would be affected in some intermediate degree. To estimate, therefore, the difference in the measure produced by this cause, we must know the strength of the liquor; whilst this, on the other hand, is only to be determined with reference to the former.

§ 10. The effect of which we are next to speak is of a still more curious nature. When two fluids which are capable of chemical combination are mixed together, it generally happens that either heat or cold is produced, the temperature of the compound differing from the mean temperature of its ingredients. The former is most commonly the case; and, when so, it happens in the greater number of instances, and perhaps in all, that a diminution of the bulk or measure of the compound also takes place, which is proportional to the heat so produced: probably in consequence of this separation of caloric.

If 18 gallons of water be mixed with the like quantity of the strongest spirit of wine, the mixture will become considerably warmer, and we shall only get about 35 gallons of the diluted spirit instead of 36: and this kind of effect is also produced in less degree by the addition of water to any weaker spirit, or the mixture of two such liquors of different strengths; the resulting compound being always found to occupy less space than the substances forming it did when separate; and its specific gravity being therefore greater than would be inferred by mere arithmetical calculation from those of its ingredients. This "concentration," as it is very properly called, must of course be considered in the estimation of the strength from the specific gravity of a liquor; and the consideration of it is attended with the same difficulty, as has already been mentioned in the last section, with respect to the effect produced by change of temperature.

§ 11. The uniformity of the relation between the specific gravities and the strengths of spirituous liquors depends on a supposition that they are either altogether composed of alcohol and water, or at least in such a state of purity as to be free from every adulteration which can materially change the specific gravity, whilst the quantity of the former continues the same; for, unless this be the case, we shall of course be unable to deduce their strength from their weight. The substances likely to be found in spirituous liquors, where no fraud is suspected, are, essential oils, sometimes empyreumatic, mucilaginous or extractive matter, and perhaps some saccharine matter. The effect of these, with the exception of the latter, is perhaps scarcely such in the course of trade as to be worth the cognizance of the excise, nor could it easily be reduced to any certain rules. Essential and empyreumatic oils are nearly of the same specific gravity as spirit, or generally rather lighter; and therefore, notwithstanding the mutual penetration, will probably make but little change in the specific gravity of any spirituous liquor in which they are dissolved. The other substances are all heavier than spirit; the specific gravity of common gum being 1482, and of sugar 1606, according to the tables of M. Brisson. The effects of them, therefore, will be to make spirituous liquors appear less strong than they really are. With a view of determining this matter, Dr. Dollfuss evaporated 1000 grains of brandy, and the same quantity of rum, to dryness: the former left a residuum of 40 grains, the latter only of $8\frac{1}{2}$ grains. The 40 grains of residuum from the brandy, dissolved again in a mixture of 100 of spirit with 50 of water, increased its specific gravity .00041: hence the effect of this extraneous matter upon the specific gravity of the brandy containing it, would be to increase the fifth figure by six nearly, which is about equal to the effect which would be produced in the above-mentioned mixture, by the addition of a pint of water to 100 gallons of the spirit; a quantity much too minute for the attention of government. It appears, indeed, somewhat remarkable, both that so large a proportion of residuum should have been left, and that upwards of 1-4th by weight of this extractive matter should not have occasioned a greater difference than 1-2000th part in the specific gravity of this diluted spirit. Saccharine matter operates much more powerfully in this respect. If a quantity of sugar be dissolved in proof spirit, it will become very considerably increased in its weight, and consequently diminished in its apparent strength and value. There will perhaps, however,

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be but little danger of fraud of this nature, if the purchasers of spirituous liquors are to use the same means of ascertaining their strengths as the officers of the revenue; since that which would apparently diminish them to the one, would also lessen their value in the estimation of the other. The detection of it, if suspected, would of course be by evaporation.

§ 12. We are rather disposed to believe, that if fraud does in fact exist with regard to the adulteration of spirituous liquors, for the purpose of changing their apparent strengths, it is more frequently of a contrary description. It is well known that when alcohol is distilled with any of those acids which retain their oxygen least powerfully, the former is converted into æther; a very different fluid, which is in a great measure immiscible with either water or spirit, and which, being much lighter than either, will float on the surface if attempted to be combined with them. This effect, in a certain degree, appears not unfrequently to take place with respect to spirituous liquors. It is very common to use the mineral acids, particularly the sulphuric, in various processes relative to spirituous liquors, either to give them a "vinous flavour," as it is called, or for neutralizing an alkali with which they have previously become charged in their rectification: and the experiments of the authors of this tract have shown them that it often happens in this case that the superior portion of the product, after it has stood for some time, is considerably the lightest; being doubtlessly composed, either wholly or in part, of a kind of semi-ætherial liquor, or dulcified spirit, which does not intimately combine with the other portions of it. It appears to be a fact well known in the trade, that there are some kinds of spirit which will not bear dilution; that is, which if mixed with water produce compounds which are by no means of such a degree of strength as would have been inferred from the apparent strength of the liquor before such mixture: a circumstance which must proceed from the same cause. The mode of detecting this kind of adulteration, or of counteracting its effect, is by taking the sample for the proof from the lowest part of the vessel in which it is contained.

§ 13. Having thus generally treated of those operating causes which are capable of influencing the weight of spirituous liquors, and which must, therefore, be separately considered when it is required to deduce their strengths from an examination of their specific gravities, we shall proceed to speak more particularly of the modes of comparison

parison hitherto used, and conclude with giving some rules for the adaptation of the valuable tables of Mr. Gilpin to the present standard, together with two short tables of our own, by which the comparative strength and value of any spirit may be found, when its specific gravity and temperature are given.

[To be continued.]

V. *On the Quantity of Iron in Cotton and Linen Cloth: Evil Effects, simple Means of eradicating, &c.: and Observations on Bleaching, the Result of long Experience.* By NICHOLAS GRIMSHAW, Esq. Member of the Dublin Society*.

EVERY one who has attended to the process of bleaching, must have observed a buff hue in the cloth after it had been spread on the field, or immersed in oxygenated muriatic acid (bleaching liquor).

Having thought much on the subject, tried experiments innumerable, and being of opinion that iron† was the cause, I, on the 22d of September last, cut a yard of calico off one of the pieces then in the first bleaching-liquor, which had been previously boiled in potash, which piece (as well as many of the 100 then in the same kieve) was a deep buff. The yard of calico was put into a hot solution of sumach (astringent), and it soon became a deep

* From the *Transactions of the Dublin Society*, vol. i. part 2.—That iron is very generally diffused throughout the globe, being frequently found mixed with sand, clay, chalk, and in the ashes of vegetables, and even in the blood of animals, in such abundance, that some authors have attributed both the colours of vegetables, and of the vital fluid itself, to the iron contained in them, is no new discovery: but, that the bad colour of linen and cotton cloth is owing to the evil effects of the iron contained in the vegetables, was first thought of by the author of the following essay, which is a strong proof of the great utility of chemical knowledge in bringing manufactures to perfection.

† Since my arrival in Dublin, Mr. Higgins, professor of chemistry, &c. accompanied me to a neighbouring bleach-green: we cut a bit of calico off a piece then in the first bleaching liquor, and which had had one boil in potash: it was buff, and on trying it with marine acid and prussiate of potash it became blue: a convincing proof that iron was present. The following experiments also were made in the presence of Mr. Higgins, and his very respectable operating pupils, in the laboratory of the Dublin Society. Six ounces of unbleached calico was burned to tinder in a crucible; digested it with marine acid, and filtered it; poured prussiate of potash on the solution, and found a copious precipitation of prussiate of iron; filtered the liquor, having previously weighed the filter (45 grains), and found when dry it had retained eight grains of the prussiate of iron.

Six ounces of linen, treated in the same manner, produced six grains of prussiate of iron.

drab colour: it was then dyed in madder, which brought it to a deep purple; and on bleaching it for a fortnight, I found the colour as permanent as if a solution of iron (iron-liquor), or sulphate of iron (copperas), had been used as a mordant, but more muddy.

I was then satisfied that a considerable quantity of iron was present, not only in the raw material of cotton, but in consequence of some weavers making or allowing their dressing to remain in iron pots, which they generally call sowings in Ireland, and sowlings in Lancashire.

The dressing is nearly the same in both countries, as it consists of the farina (flour) of wheat, oats, or potatoes, and, when sour, is capable of dissolving iron. This is well known from iron liquor used by calico printers being made with vegetable acid, in which they put iron hoops. This iron liquor is the mordant for black and purple, and is dyed in logwood or madder; and when, in consequence of accident in printing, it is necessary to discharge the black or purple colour before they are dyed, if recourse is had to oxygenated muriatic acid, an iron mould is produced, that is, the colour becomes buff or gold; but use sulphuric or marine acid, and the colour is discharged. I have mentioned this circumstance for the purpose of showing why some pieces assume a much deeper buff than others, when put into the oxygenated muriatic acid, as the quantity of iron put on the warp by the weavers will depend on their using or not using iron pots, the strength of the acid of their dressing, the rust or cleanness of their pot, and consequently the quantity of iron it holds in solution.

The experiment however, before mentioned, induced me to take 100 pieces which had been once boiled in potash and washed, and immerse them in sulphuric acid (bleachers sours), and after remaining about 12 hours they were washed, and again boiled in potash.

After being washed and without being put on the bleach-green, they were put into the oxygenated muriatic acid for the usual time (about 12 hours), and came out perfectly free from any buff appearance; which convinced me, as it must every one who knows its use, that the sulphuric acid had divested them of iron, and consequently had left none to be oxidated by the oxygenated muriatic acid, or the oxygen of the atmosphere. They were again boiled and immersed in the bleaching liquor alternately, until they had six boils, and five bleaching liquors; and on the seventh day, without being put on the grass, they were the whitest and strongest pieces I had ever seen: and during the process appeared uncommonly clear; for after the fourth boil they

they were sufficiently white for every purpose except printing. More than 5000 pieces have been bleached at my works (at White House, near Belfast) in the same manner, without any appearance whatever of buff, as before mentioned: only half the usual quantity of oxygenated muriatic acid has been used; the receiver affording sufficient for seven to eight parcels of calicoes, which before was only equal to three or four—So much less was required to bring the liquor in the kieve to its usual strength, (which was ascertained by the test of solution of indigo in sulphuric acid,) and so little had the liquor been exhausted by oxidating the iron in the cloth, &c. in lieu of acting on the colouring part.

It has been long known, and as long lamented by the calico printers, that yellow stains have appeared after the printed pieces have been dyed, and that such stains cannot be bleached out without reducing and materially injuring the colours. Prior to the commencement of the process I have described (I mean, the use of sours previous to the cloth being exposed to the oxygen of the atmosphere, by being spread on the field, or immersed in the oxygenated muriatic acid) a considerable quantity of calicoes had those stains; but I am happy to state, that on dyeing in the same parcels, and the same coppers, pieces of the old process, and pieces of the new, the latter were perfectly clear, and the first stained.

This defect continued as long as any of the old bleach remained, but with them stains entirely disappeared. The new bleach were perfectly clear, by lying two or three days on the grass, after dyeing: the old not in as many weeks.

It is unnecessary to say to men of practice and science, that the same process of bleaching is applicable to linen and to cotton, linen requiring only a stronger process and more time, or that iron oxidated and iron moulds are the same; and that the quality of cloth is in consequence injured, as they are generated by combustion in consequence of the union of oxygen gas: hence the quantity of bucks (rotten linens) and linens also resuming a yellow hue after being bleached and exposed to the air, which acts on the iron remaining in the cloth.

The eye of the scientific mind will discover at the first glance, that a solution of sulphuric acid will take up or dissolve iron in the cloth, and render it miscible, or soluble in water, if applied previous to its becoming an oxide.

Let us then look at what the bleacher has to contend with and to eradicate: and it will be found he has the resin or colouring matter of the flax or cotton, iron, and the

weaver's dressing as before mentioned; to which may be added a little butter or tallow. An addition of the last article takes place by the dropping of the candles during the winter.

The first object then is to divest the cloth of the sowings. This is done by steeping in water, summer heat (76° of Fahrenheit), and washing.

The second is to dissolve the grease applied by the weaver, and some of the resinous part. This is done by a boil or buck in alkali, and washing.

The third is to divest the cloth of iron; which is most effectually done by steeping in sours for 10 or 12 hours, and washing immediately.

After another boil, the bleacher may proceed as before mentioned; or proceed in any other manner his experience or opinion may direct—as he will derive all the advantages stated to result from the use of sours previous to exposure to air or bleaching liquor: let his after-process be any one of those now in use.

But I by all means advise him to throw his cloth loose into the steeping-kieve, furnace, sours, and bleaching liquor, during the early part of the process, so that the cloth may be equally acted upon; by which means he will avoid those dark clouds which must always appear when the cloth has been kept in the band during the above processes.

Those who use the rope-net and crane (and every one ought to use them) will find no additional trouble, and will be much pleased with the evenness of the pieces.

Let the first sour be strong, and wash well: the other sours may be continued in the usual stages of bleaching, using one less in consequence of the previous sour*.

The strength of the boils should be proportioned to the quantity of resin or colouring matter of the cloth; consequently the first should be the strongest, as the resin or colouring matter of the cloth is capable of saturating a greater quantity of alkali than it is afterwards: and it should be recollected that the cloth possesses, in the first instance, the grease of the weaver's dressing, which, by uniting with the alkali, becomes saponaceous (soapy), and is in consequence easily washed off.

The alkaline hydrometer should by all means be used by the bleachers to ascertain the strength of their leys: they will in a moment see the number of ounces of alkali to a gallon, and can by a simple table† see how many gallons are necessary, so as to give them a certain weight to each

* Muriatic is preferable to sulphureous acid, especially for the after-sours, as it possesses the power of dissolving oxides of iron (iron moulds).

† See the table, p. 39.

piece, and which should always be proportioned to the thickness of the cloth, and, as before mentioned, to the quantity of colouring matter to be dissolved.

Those who use the bleaching liquor, either early or late in the process, should immerse or steep the cloth in it after it had been boiled and washed, but never to put the cloth from the field into it, as they will find, as I have done, that the quality will be destroyed.

I have likewise found that cloth, either linen or cotton, neither stiffens with the callender or beetling, if dried after being only washed out of the bleaching liquor; it must therefore be boiled in water or ashes: if the latter, it ought to be put on the grass for a few days previous to finishing; and I advise the linen bleachers to give at least one boil in ashes after the last bleaching liquor, not only to prevent the cloth being soft or slack after finishing, but as the best means of preventing mildews (so much complained of) by the attraction of moisture.

If any further proof was necessary to confirm what has been before stated relative to the utility of early souring, it will every day be found in the process of dipping or dyeing china blue, which is performed by alternate immersion in a solution of lime or ashes and sulphate of iron (copperas), by which the iron is precipitated on the cloth; it is easily removed by immediate souring: but if the cloth is exposed for some time to the air, the iron cannot be dissolved without injury to the texture or fibres, and the colour.

I therefore entreat the bleachers of linen, calico, or muslin, will try the effects of souring after the first boil, in the manner before mentioned; and after the second boil that they will pursue such process as they have most improved on (either bleaching on the field entirely, or the alternate process of bleaching on the field after a few boils, and then using the bleaching liquor with lime or ashes, in their receivers): this will most effectually enable them to compare the relative progress, &c., and which will be very obvious in favour of early souring.

In a conversation a few days ago with Mr. Whiteman, of Lisburn, on the subject of bleaching, he told me that his muslins had the buff appearance I had described, not only in the process of bleaching, but that he had then some unsaleable, even at a loss of 10 per cent., in consequence of that buff or muddy appearance, a specimen of which he gave me, and in which a considerable quantity of prussiate of iron has been found.

The muslins, calicoes, and linens, bought in the white for printing at my works, are generally stained and bad whites

after printing and dyeing. Those bleached after the use of sour, as described, are always free from stains, and very clear whites.

I am very happy to have it in my power to add the following very respectable scientific authorities in favour of my practice, &c. to whom I communicated the result, viz. —Wm. Higgins, Esq. M. R. I. A. Professor of Chemistry, &c. Richard Kirwan, Esq. F. R. S. M. R. I. A. &c. and to James McDonald, Esq. M. D. &c. Belfast, who has attended the process at my works, and whose Report is before the Linen Board.

On Soap, substituted for Bran in Bleaching. By the same.

THE legislatures of Great Britain and Ireland have adopted wise and salutary measures for increasing the quantity of food, by offering bounties on importation; they have likewise lessened its consumption by stopping the distilleries. It becomes then the indispensable duty of all, especially in periods such as the present, to avoid the smallest waste or misapplication of food. Bran, if given to horses, saves corn; if to horned cattle, it produces milk, butter, or beef; but when used by calico printers, it is worse than throwing it away; for in most cases it is not only unnecessary and expensive, but injurious. There is still a stronger reason for avoiding the waste of it, on which at present it is better to be silent. Prejudice, therefore, will give place to public good, and the calico printers may rely on the following process, as it has been practised for more than a year by a house of long experience and proficiency in that business.

Let six or seven pounds of black or soft soap be well dissolved in hot water; pour about two-thirds of the above into a copper of hot water (180 to 190° of Fahrenheit): when the calicoes or muslins which have been dyed in madder are washed as for branning, give them five or six ends over the wince, taking only three pieces of calico or six of muslin at a time, that they may be even. Renew your copper with the remainder of the soap, and through one copper may be done twenty-seven or thirty calicoes, and about double the number of muslins. Let them be washed as after bran, and pinned on the grass: if the cloth has been well bleached, muslins will be white, on an average, in two days, calicoes in four. The colours will have a much finer hue than by the use of bran.

N. B. Soap has been used for taking out stains, but by branning before soaping the stain has been more fixed: therefore bran should not be used.

A TABLE ascertaining the Quantity of Ashes to be used, according to the Hydrometer, for 100 Pieces of Cloth.

Oz. marked by the hydrometer.	Gallons to be used for a 6 oz. buil.	Gallons to be used for a 5 oz. buil.	Gallons to be used for a 4 oz. buil.	Gallons to be used for a 3 oz. buil.	Gallons to be used for a 2 oz. buil.
8	75	62 $\frac{1}{2}$	50	37 $\frac{1}{2}$	25
8 $\frac{1}{2}$	70 $\frac{1}{4}$	58 $\frac{3}{4}$	47	35 $\frac{1}{4}$	23 $\frac{3}{4}$
9	66 $\frac{1}{4}$	55 $\frac{1}{2}$	44 $\frac{3}{4}$	33	22 $\frac{1}{4}$
9 $\frac{1}{2}$	63	52 $\frac{1}{2}$	42 $\frac{1}{2}$	31 $\frac{1}{2}$	21
10	60	50	40	30	20
10 $\frac{1}{2}$	57	47 $\frac{1}{2}$	38	28 $\frac{1}{2}$	19
11	54 $\frac{1}{4}$	45	36	27	18
11 $\frac{1}{2}$	52 $\frac{1}{2}$	43	34 $\frac{1}{2}$	26 $\frac{1}{4}$	17 $\frac{1}{4}$
12	50	41 $\frac{1}{4}$	33	25	16 $\frac{1}{2}$
12 $\frac{1}{2}$	48	40	32	24	16
13	46	38	30 $\frac{1}{2}$	22 $\frac{3}{4}$	15 $\frac{1}{4}$
13 $\frac{1}{2}$	44 $\frac{1}{4}$	36 $\frac{1}{2}$	29 $\frac{1}{4}$	21 $\frac{1}{4}$	14 $\frac{1}{4}$
14	42 $\frac{1}{4}$	35	28 $\frac{1}{4}$	21	14
14 $\frac{1}{2}$	41 $\frac{1}{4}$	34 $\frac{1}{4}$	27 $\frac{1}{2}$	20 $\frac{1}{2}$	13 $\frac{3}{4}$
15	40	32 $\frac{1}{2}$	26 $\frac{1}{4}$	20 $\frac{1}{4}$	13
15 $\frac{1}{2}$	39	32	25 $\frac{3}{4}$	19 $\frac{3}{4}$	12 $\frac{1}{2}$
16	37 $\frac{1}{2}$	31 $\frac{1}{4}$	25	18 $\frac{3}{4}$	12 $\frac{1}{4}$
16 $\frac{1}{2}$	36 $\frac{3}{4}$	30 $\frac{1}{4}$	24 $\frac{1}{2}$	18 $\frac{1}{2}$	12
17	35 $\frac{1}{4}$	29	23 $\frac{3}{4}$	17 $\frac{1}{2}$	11 $\frac{1}{2}$
17 $\frac{1}{2}$	34 $\frac{1}{2}$	28 $\frac{1}{2}$	22 $\frac{3}{4}$	17	11 $\frac{1}{4}$
18	33 $\frac{1}{2}$	27 $\frac{1}{2}$	22 $\frac{1}{2}$	16 $\frac{1}{2}$	11
18 $\frac{1}{2}$	32 $\frac{3}{4}$	26 $\frac{3}{4}$	21 $\frac{1}{2}$	16	10 $\frac{3}{4}$
19	31 $\frac{3}{4}$	26 $\frac{1}{4}$	21	15 $\frac{3}{4}$	10 $\frac{1}{2}$
19 $\frac{1}{2}$	30 $\frac{3}{4}$	25 $\frac{1}{2}$	20 $\frac{1}{2}$	15 $\frac{1}{4}$	10 $\frac{1}{4}$
20	30	25	20	15	10
20 $\frac{1}{2}$	29 $\frac{1}{4}$	24 $\frac{1}{4}$	19 $\frac{1}{2}$	14 $\frac{1}{2}$	9 $\frac{3}{4}$
21	28 $\frac{1}{4}$	23 $\frac{3}{4}$	19	14 $\frac{1}{4}$	9 $\frac{1}{2}$
21 $\frac{1}{2}$	27 $\frac{3}{4}$	23	18 $\frac{1}{2}$	13 $\frac{3}{4}$	9 $\frac{1}{4}$
22	27 $\frac{1}{4}$	22 $\frac{1}{2}$	18 $\frac{1}{4}$	13 $\frac{1}{2}$	9
22 $\frac{1}{2}$	26 $\frac{3}{4}$	21 $\frac{3}{4}$	17 $\frac{3}{4}$	13 $\frac{1}{4}$	8 $\frac{3}{4}$
23	26	21 $\frac{1}{4}$	17 $\frac{1}{2}$	13	8 $\frac{1}{2}$
23 $\frac{1}{2}$	25 $\frac{1}{2}$	21	17 $\frac{1}{4}$	12 $\frac{3}{4}$	8 $\frac{1}{4}$
24	25	20 $\frac{1}{2}$	16 $\frac{1}{2}$	12 $\frac{1}{2}$	8
24 $\frac{1}{2}$	24 $\frac{1}{2}$	20 $\frac{1}{4}$	16	12 $\frac{1}{4}$	8
25	24	20	16	12	8
25 $\frac{1}{2}$	23 $\frac{1}{4}$	19 $\frac{1}{4}$	15 $\frac{3}{4}$	11 $\frac{3}{4}$	7 $\frac{3}{4}$
26	23	18 $\frac{3}{4}$	15 $\frac{1}{2}$	11 $\frac{1}{2}$	7 $\frac{1}{2}$
26 $\frac{1}{2}$	22 $\frac{3}{4}$	18 $\frac{1}{2}$	15 $\frac{1}{4}$	11 $\frac{1}{4}$	7 $\frac{1}{4}$
27	22	18	15	11	7
27 $\frac{1}{2}$	21 $\frac{1}{2}$	17 $\frac{3}{4}$	14 $\frac{1}{2}$	10 $\frac{1}{2}$	7 $\frac{1}{2}$
28	21 $\frac{1}{4}$	17 $\frac{1}{2}$	14 $\frac{1}{4}$	10 $\frac{1}{4}$	7
28 $\frac{1}{2}$	21	17 $\frac{1}{4}$	14	10	7
29	20 $\frac{3}{4}$	17	13 $\frac{3}{4}$	10 $\frac{3}{4}$	6 $\frac{3}{4}$
29 $\frac{1}{2}$	20 $\frac{1}{4}$	16 $\frac{3}{4}$	13 $\frac{1}{4}$	10	6 $\frac{1}{4}$
30	20	16 $\frac{1}{2}$	13 $\frac{1}{2}$	10	6 $\frac{1}{2}$
30 $\frac{1}{2}$	19 $\frac{1}{2}$	16 $\frac{1}{4}$	13	9 $\frac{1}{2}$	6 $\frac{1}{4}$
31	19 $\frac{1}{4}$	15 $\frac{3}{4}$	12 $\frac{3}{4}$	9 $\frac{3}{4}$	6 $\frac{3}{4}$
31 $\frac{1}{2}$	19	15 $\frac{1}{2}$	12 $\frac{1}{2}$	9 $\frac{1}{2}$	6 $\frac{1}{2}$
32	18 $\frac{3}{4}$	15 $\frac{1}{4}$	12 $\frac{1}{4}$	9 $\frac{1}{4}$	6 $\frac{1}{4}$

VI. *Of the Herring Fishery. Translated from an Essay in Dutch, entitled "Beschryving van de Haringvischerye*."*

THE herring inhabits the Northern seas, and probably amidst the great Ice islands, spawns in the months of August and September, and multiplies so astonishingly, that notwithstanding the great destruction of them by the fish of prey and men, the species is not sensibly diminished. The herring belongs to that class which emigrate. They make their appearance yearly in prodigious numbers. The great shoal, in its progress from the North, divides into two principal branches; the right wing goes westward, falls towards the coasts of Iceland in the month of March; the left inclines to the eastward. These two grand divisions are afterwards split into several subdivisions: some bend their course towards Newfoundland, others towards the coasts of Norway, and partly fall into the Baltic through the Sound; while another part turns the north point of Shetland, where it stretches along the coast, until it joins the division (through the Belt) which entered the Baltic. They separate again, to cover the coasts of Holstein, the Texel, the Zuiderzee, &c.

The westerly column, or right wing, which is also the greater, goes on straight forward towards the Orcades (where the Dutch fishers impatiently lie in wait for them), and from thence to Scotland, where they again separate, one wing steering by the coast of Holland, England, and France, the other taking the route of Ireland. After passing all these islands, they again meet, and form into a column, which stretches along the Atlantic ocean and disappears. But what justly challenges our admiration is, that after separating into so many different branches, they know how to rally their scattered squadrons, and find the way back to their native abode. The time of meeting, and the place of rendezvous, are settled, so that after the general retreat not one straggler is to be met with in these seas.

How regular soever the period of yearly emigration appears, it is not free from anomalies. It may appear surprising that these animals, who are secured from the persecution of their enemies, in the unfathomable depths of the Northern ocean, by an impregnable rampart of ice, should forsake their safe retreat yearly, in myriads, exposed on our coasts to great and unavoidable havoc. Is it not a striking

* From the *Transactions of the Dublin Society*, vol. i. part 2.

instance of the goodness of divine providence, to draw this prodigious swarm of useful fish into our nets? This explanation, however pious, affords little light to the natural historian, who may be inclined to ask: Since divine providence is so gracious in this respect, why does it not send us a troop of whales to furnish us with train oil at less labour and cost than we can now procure it? The celebrated burgemaster of Hamburg, Mr. Anderson, is of opinion, that the emigration of herrings is owing to the overflow of young ones, who, not finding sufficient room in their old habitations, sally out in quest of new settlements, as so many colonies. This opinion does not at all account for the phænomenon. How can periodical emigrations, always taking place at stated seasons, proceed from so uncertain a cause as the mere accident of an overflowing population?

How will this hypothesis account for the constant adherence to the same track, the separation and reunion of the main body, at stated places and at stated times? Whereas mere want of room would drive them indiscriminately towards any or all places. 2dly, So far from being the effect of excessive multiplication, it seems to be the motive of it. They are not the only species which undergoes long voyages for the sake of propagation. Many birds of passage, such as the woodcock, wild-goose, &c. do the same. In fact, the herrings multiply more on the route than they do before it; we know that many kinds of fish leave the sea, and seek the rivers to deposit their spawn: so that one very probable cause of the emigration is, the instinct of emigration; the second, the plenty of food which the quickening influence of spring prepares, by the swarms of insects and flies. The king or leader of herrings is much larger than the common, being two feet long; fishers think it criminal to destroy it; the whole column follows and observes his motions. In a streight they contract, in an open sea they expand their order of march with admirable dexterity, without slackening their pace. They live on small fishes and young crabs, as appears from their jaws being furnished with teeth. Lewenhoeck found in the stomach of a herring the indigested remains of a little fish.

To form some conception of the innumerable multitude of herrings that fill the seas, extending more than the breadth of England and Ireland together, and in order to give a clear insight into this profitable branch of trade, we shall first treat of the ships and implements used in the fishery: 2dly, of the time and manner of taking them: 3dly, of the regulations and right of carrying on the fishery, the
gutting,

gutting, salting, packing, &c. : 4thly, of the different sorts and appellations of herrings, which make an article of commerce : 5thly, of the inspectors and overseers of this commerce.

The vessels employed in this fishery are, time out of mind, called *buixzen*. The English use a kind of vessels carrying from 60 to 70 tons. The Dutch vessels are from 25 to 30 lasts, some are 40, but seldom so much. Each of these has ten, twelve, or fourteen men aboard, who are hired at so much per week, except the steersman, who receives five florins for each last of herrings. The crew receive, over and above their pay, a present of herrings proportioned to the take, which present is the only wages of the younger part of the crew or apprentices. A Dutch fishing smack costs new about nine thousand florins; the costs of fitting for two voyages are about six thousand florins, and for three voyages, about eight thousand. Mr. Semeyns computes the expense of fitting out a vessel of 60 lasts (including prime cost) to amount to 7,530 florins, to make three voyages in the course of a year.

Here follows an accurate list of all the vessels sent out yearly, from 1763 to 1776, on an average of thirteen years.

	1763	1764	1765	1766	1767	1768	1769
Rotterdam,	7	6	6	2	2	2	2
Schiedam,	5	6	7	11	11	10	9
Ulaardingen,	64	71	69	60	58	61	64
Maaslandsluis,	7	14	15	14	15	12	12
Delfshaven,	7	9	9	8	7	7	7
Delft,	0	0	0	0	0	0	0
Enkhuizen,	40	40	40	40	41	41	41
De Ryp,	14	14	14	14	16	16	14
	144	160	160	149	150	149	149
Besides jaegers	13	15	17	17	17	17	20
	1770	1771	1772	1773	1774	1775	1776
Rotterdam,	3	5	7	7	6	6	6
Schiedam,	8	7	7	5	2	2	2
Ulaardingen,	62	64	68	76	74	66	84
Maasslandsluis,	14	14	15	18	18	17	21
Delfshaven,	7	7	7	7	7	6	6
Delft,	0	1	0	0	0	0	0
Enkhuizen,	41	41	42	42	44	41	41
De Ryp,	14	14	13	13	14	16	16
	149	153	149	168	165	154	176
Jaegers,	20	20	23	20	20	20	22

In 1774, the number of vessels sent out amounted to 165, and in 1776 to 176. From this brief exposition of the number of vessels employed in the fishery, it appears that a great decline has taken place since the year 1601, when 1500 vessels were sent out yearly from the United Provinces. The cause of this decline seems to be, the greater attention which other nations pay to this branch of fishery. It remains, however, certain, that the Dutch method of curing is superior to that of their neighbours. The diminution of trade was necessarily accompanied by a proportional diminution of national profit; formerly thousands were supported by this branch who are now out of employment. Government omitted nothing that could revive this declining branch of trade. Thus, on the 19th of May 1775 the government offered a premium of five hundred florins for any ship which should be employed two years successively in the herring fishery, and for the second two years four hundred florins: yet some abuses happened in consequence; for some, having got the premiums, discontinued to send out their vessels. As the selling prices of herrings vary with the season and plenty, it is scarce possible to ascertain with exactness the profits of one voyage. We shall, however, attempt an average account, from which it will appear that the herring fishery is worthy of every encouragement, as a branch of industry highly advantageous to the community. A buss of thirty-two lasts, fitted out for three voyages, costs, as we already observed, between sixteen and seventeen thousand florins; each last of herrings contains twelve tons, and each ton about eight hundred herrings: the whole lading then of the buss amounts to about 380 tons; and if each herring be estimated at half a stiver (i. e. twenty florins the ton), the amount will be 7600 florins, which multiplied by three voyages yields a sum total of 22,800; from which 17000 florins being deducted for prime cost, a surplus remains of 5800 florins for each buss, all net profit:

Which multiplied by 160, the number of busses yearly sent out to the fishery, yields little short of a million of florins.

Let us in the second place examine, with what instruments, and after what manner, this amazing quantity of herrings is taken as soon as the busses are all assembled. They take their course from Shetland, N. N. E. and cast the nets off Fairhill on St. John's night, the 25th of June, after midnight. The fishery is always carried on at night, as well to obviate the inconvenience of the fish discovering
the

the nets, as also to entice them by the light of the lanterns, of which they are fond, and towards which they make. Mr. Spon, in his Travels through Italy, makes mention of a similar artifice used by the fishers on the coast of Dalmatia; they carry lanterns in order to entice the sardines, which are there in great abundance.

The nets are very long, consisting of fifty or sixty webs, fortified with narrow net meshes, in order that the herring may be entangled by his gills. The mouth of the net may be fortified with good hemp, or strong Persian silk, as being more durable than hemp, being capable of holding good three years: moreover they are tanned, or coloured brown with smoke, that they may be the less perceptible by the fish. The nets are thrown out at sun-set, betwixt two busses, which, on account of their size, require much time and labour. They are fastened, and buoyed up with casks to prevent sinking, which serve as a distinguishing mark, and also, by reason of their weight, instead of an anchor. To prevent them from getting entangled in one another, the busses should be so arranged that each may preserve its nets free. During night the fishes run into the nets spread out for them, and about five or six in the morning they are hauled up. This labour will give full employment for three hours, as the take is commonly from three to ten lasts, and sometimes even fourteen lasts. The day is taken up in dressing the fish: they begin the operation by cutting out the gills, as those parts are liable to speedy putrefaction: they are then put into barrels and salted: all that are taken under five lasts are thus prepared for the market. The overplus, which they call *stabers*, are also gilled, lightly salted, and thrown into boats to be sent ashore after the first salting. The herrings are left on deck throughout the following night. On the second morning they are properly packed, and the barrels, being filled up, are placed in the hold. In the beginning a part of the take is sent ashore in the lighters called *jaegers*. When the herrings are fully cured, the buss herself goes towards land, discharges her freight, and lays in provision for a second expedition. As experience has taught, that herrings caught at certain seasons keep longest and are best for use, the time for beginning the fishery is fixed by law. Before the 25th of June, all masters of fishing smaeks, previous to their departure from Holland, are bound over not to trespass this ordinance, and at their return are obliged to declare upon oath, that they have not, by their own deeds or by the ministry of others, contravened the law. Testimonials of this are given
to

to each ship, marked with the place of destination, to the end that none be deceived, or the trade suffer by improper goods. The place of the fishery changes with the seasons. From St. John's to St. James's, i. e. from the 24th of June to the 25th of July, the seat of the fishery is between Fairhill and Shetland; during this period herrings of the best quality are taken: from St. James's day to the 14th of September, the pursuit is carried on to the north of Scotland, and from thence to the 25th of November, along the coasts of Yarmouth and Norfolk. All the herrings taken the first three weeks after the 25th of June are cured and packed up together, unsorted, and sent to Holland by swift-sailing vessels called jaegers, after which all the herrings taken are carefully sorted and separated into three divisions: maatijes herrings, full, and shotten herrings, which are all separately cured and packed up in distinct barrels.

In the maatijes herrings is found neither roe nor milt. They are very fat and palatable, but do not keep well. Full herrings are those that are full of milt or roe, and in their most perfect state: this sort is fittest for market and preservation. The third sort consists of such as having cast their spawn or roe are spent, or are on the point of spending: this last sort is inferior in quality, and cannot be kept so long as the former, or full herrings.

The lading of the busses on their return to Holland consists of those three sorts, which are again inspected, packed, and salted afresh, before they are sent to any foreign market. By this fresh packing fourteen sea barrels are reduced to twelve, which make up a last. In order to bring this branch of commerce to a flourishing state, the governments of this and many other countries have made sundry regulations concerning the manner of cutting out the gills, salting, and packing.

The English have always looked on the commerce of Holland with an eye of envy, which often bursts out into open acts of violence, never omitting any opportunity of disturbing, and, if possible, of ruining our herring fisheries; the more so as Dutch herrings have always had the preference of the English, as well as of those of every other nation. In order to cut off all pretext of quarrel, our fishers are forbidden to cast their nets within ten miles of the English shore; which prohibition is the less detrimental to the fishery, as herrings of the best quality are taken at such a distance from the shore.

Those which come into the bays of Norway, Shetland,
and

and Ireland, being of an inferior quality, are less fit for preservation or salting; for which reason it is forbidden by an ordinance, dated 1620, to take any herrings at the fore-mentioned places. Among the regulations that have been made for the support of the herring fishery, the following are the principal ones.

The appointment of a hearmeester, or overseer, at all the landing places where herrings are brought in, to take strict care that the herrings should get a second salting and packing: to him is also intrusted the inspection of the salt and coopers' work, or barrels in which they are packed. Their province is in a special manner to prevent foreign herrings being mixed with our own (in case any foreign cargoes arrive), and to have the barrel branded with such marks as may prevent a mistake on this head, that our trade may not suffer from the quality of such fish.

2dly.—He is to take care that all damaged herrings or of bad quality, e. g. such as take sick after having cast their fry, or on the eve of doing it, in a word, unmerchantable; be thrown aside in the packing, lest such bad fish corrupt the sound, or give them a bad flavour. That, moreover, the fish be properly salted and packed.—3dly. That the masters and crew of one buss do not put any hindrance in the way of another. In case they were unlucky, or could not succeed where they had cast nets, they must not remove to the ground of others to disturb their operations, nor damage their nets, boats, or other implements: in case they do, they must make good the damage. No buss employed in the fishery can be sold to foreigners, or hired out to them for the purposes of fishing. 4thly.—That the said overseer do inspect all the barrels before they are taken on board the busses, reject such as shall appear unfit, and mark such as he approves with the name of the cooper, and his place of residence; after having examined the quality of the timber, construction, hoops, &c. &c. 5thly.—He shall not suffer any buss out on the fishery before the 24th of June, and he shall require a declaration upon oath, before any of the herrings are landed, that there are none abroad taken before that period.

6thly.—He must take care that in salting herrings a distinction should be made. Thus, for example, the herrings taken between St. John's and St. James's day shall be salted with coarse and chosen salt. Those taken from St. James's day to the 14th of September must be packed up with the best fine salt. No herrings can be packed except such as are taken from the 14th of July to the 1st of January.

ary. Each sort to be packed up separately in barrels properly filled up, stopped, hooped, &c. Lastly.—No herrings can be sold or brought to market in this country that are not picked and sorted in the following manner. By this sorting and marking, the different kinds of herrings, and the time they were taken, are discriminated and named accordingly. Thus are to be met with in the market, St. John's herrings, St James's herrings, St. Bartholomew's herrings—none but these can be packed. The take of St. John is sent ashore in lighters, in order to be sold immediately for consumption. The St. James's herrings undergo a second packing, are reduced from fourteen barrels to twelve, then marked by the overseers: these are sent off in a commercial way.

The take of St. Bartholomew, the 28th of August, are marked with the arms of the city, and commonly sent to Cologne in Germany. The take of the 14th of September are likewise marked with the arms of the city, and sent generally to Rouen in Normandy: they are not marked until they have remained in the first pickle eight or ten days. Lastly.—They must have remained in salt ten days before they can be sold. No Scotch or other foreign herrings can be worked, cured, and packed, as if they were Dutch: they may be simply packed up in barrels without any stamp. The precaution on this head is carried so far, that no empty barrels of ours, marked as above, can be exported to any other country. This extract of regulations concerning the important commerce of the herring fishery, possessed as well by the States General as by the states of the province of Holland, is drawn from the Great Placart book, from the treaty with the city of Hamburgh, from the book of Handvoesten, all which the reader must examine, if he requires a more exact knowledge of the matter.

We have said above, that the inspection of the curing, packing, &c. is intrusted to overseers, who are appointed by government, and take up their residence conveniently to the harbours, or place where the business is going forward.

I have said already, and the common suffrage of all nations confirms it, that the Dutch herrings are the best. No other cause can be assigned for this general preference, than the scrupulous adherence to the regulations and provisions just now mentioned, it being by no means true, that the art of curing, salting, and packing herrings is confined to the Dutch alone. Other nations are as expert at doing all that as we can be; but in no other country is so much at-
tention

tention paid to this branch of commerce as in our republic, which is of so much the greater consequence to our state, as the necessary expenses of stores and fitting for the whale fishery are almost all defrayed from the profits of it. As long as these wise ordinances and regulations are punctually observed, and no breach of them allowed, notwithstanding the high wages, which may be considered as one cause of its decline, it may still flourish.

VII. *On the Boiling Point of Mercury, and the Fixing Points of Lead and Tin.* By Mr. JAMES CRICHTON, of Glasgow*.

To Mr. Tilloch.

SIR,
I HAVE to request that you will correct a small error in the account of my thermometer, inserted in number LVIII, (p. 147). The bar is composed not of *iron* and zinc, as printed by mistake, but of *steel* and zinc. The engraver has also made the scale to read from right to left, instead of the contrary; but this is not material.

I have it now in my power to send you the *results* of a number of interesting experiments respecting the boiling point of mercury, and the fixing points of lead and tin, which I think may answer some important purposes to the philosophical world. A detail of all the steps followed, which were similar to those stated in my last communication, would only take up time unnecessarily.

The steady uniform point at which block-tin fixes surpasses my expectations, and is far more determinate than that of water. Lead has not, like tin, the property of instantly depressing and as instantaneously raising the thermometer at the moment of congelation.

Having had occasion, in constructing some very high-ranged thermometers, to take for the purpose of graduation some point much higher than that of boiling water, as in prolonging a scale to 500 or 600° from so comparatively contracted a scale, errors must unavoidably be introduced, I had recourse to the writings of the most respected British and foreign chemists, to find from them the fixing points of lead and tin. In this search I was greatly disappointed, for they do not agree amongst themselves, varying so much as 30, 40, sometimes 70 degrees. Besides, having good reason to suspect that none of them were near the truth, I

* Communicated by the Author.

resolved to determine the fact by actual and careful experiments, the results of which were so different from what have been received, that I think myself called upon to state them.

In order to obtain the more certainty, I made on purpose three accurate thermometers, on which I could perfectly rely; and from their agreements I can with confidence say that mercury boils at 656° , lead fixes at 619° , and tin at 442° ;—their specific gravities being respectively thus: mercury 13,568, lead 11,346, tin 7,279, taken in distilled water at 62° . The gravities attributed to them by authors are nearly the same; which is so far satisfying.

When the experiments were made the barometer stood at 29.3 , 24 feet above the sea.

JAMES CRICHTON.

VIII. On the new Planet Pallas. By Baron VON ZACH.

THE proofs on which the improvement of our astronomy, both theoretical and practical, is at present founded, cannot be more conclusive; and the triumph which this science has obtained, even in the eyes of the public in general, cannot be greater than it is at present by the re-appearance of Pallas.

This small planet, after its discovery last year by Dr. Olbers, was scarcely observed four months when it approached so near the solar rays as to become lost in them; and having now emerged from them, after being invisible for six months, it has been again found, like a small point, hardly perceptible, among myriads of worlds, and exactly in the spot where its place was announced by theory. To this new discovery of the re-appearance of Pallas, the greatest possible physical and intellectual powers of man have contributed.

On the 18th of February, about $14^{\text{h}} 50'$, Harding, that expert observer of Lilienthal, was so fortunate as to find again Pallas, exactly in the place where it ought to be according to the calculations of Dr. Gauss, inserted in the *Monatliche Correspondenz* for December 1802. The following night he had the pleasure to ascertain its existence in the most satisfactory manner. On the 15th of February, about 15^{h} , he found it nearly over No. 36 of Poniatowsky's Bull, as a small star of the 12th or 13th magnitude, and knew it to be the planet, as he had observed

very accurately the night before No. 36 and the surrounding stars; but now saw a star in a place where 24 hours before none was visible. The next night this luminous point had moved as theory required, and on the 20th of February, about $15^{\text{h}} 30' 13''$ mean time, had advanced 55 seconds from No. 36 of the Bull, against which he saw it on the 15th of February.

Mr. Harding had the goodness to communicate to us immediately his important discovery. He informed us in his letters that he was not able to distinguish this planet with a three feet achromatic telescope, through which stars of the eleventh magnitude were clearly seen. Dr. Olbers, who on the 21st of February had the pleasure of again seeing his planet, wrote to us that he could distinguish Pallas very distinctly with a five feet telescope by Dollond; but at that time he considered it equal to a star of the twelfth or thirteenth magnitude, and its light equal to that of the fourth satellite of Saturn.

Dr. Gauss therefore, in the same manner as last year in regard to Pallas, is entitled to the greatest praise, and deserves our warmest thanks for the wonderful accuracy of his calculation, by the correspondence of which alone it was possible to discover again this planet among the innumerable host of telescopic stars. "Had there been an error of only from 30 to 40 minutes," says Mr. Harding, "in the calculations of Dr. Gauss, I much doubt whether I should have been able to discover Pallas again."

In the *Monatliche Correspondenz* for the month of March, page 277, we estimated the real apparent magnitude of Pallas, on its first re-appearance, to be equal to a star of the twelfth magnitude, and this conjecture has been found to be confirmed. In that number we gave Harding's chart of the probable orbit of this planet, in order to facilitate the finding of it, and this chart is now found to be its actual orbit; for the difference between the calculations and the observations hitherto made is so small, that it is not perceptible on our chart, though constructed on a very large scale, and nearly four times as great as that employed by Professor Bode for his celestial atlas. By the help of these charts, and the calculation of the orbit of this planet given in the *Monatliche Correspondenz* for December 1802, it will be possible, even without knowing that this calculation corresponds within two minutes with observation of the heavens, to find Pallas at all times when searched for with good telescopes.

Mr. Harding's two observations of the right ascension were as follow :

1803.	Mean time at Lilienthal.	A.R. ♀
Feb. 20	15 ^h 30' 13"	272 38 27
21	17 0 11	272 56 29

Dr. Olbers also, who possesses a wonderful dexterity in observing with the circle micrometer, has hitherto been able to obtain only two observations. This excellent observer, in a letter dated February 23d, says, " I find observations of Pallas very difficult, on account of its faint light, and therefore they are not very accurate. The declination, in particular, is somewhat doubtful. In the place where Pallas ought to have stood on the 23d of February there were four small telescopic stars, among which I however discovered Pallas as the brightest. On the 23d of February, at 10^h 19' 24" mean time, after six comparisons with No. 42 of Poniatowsky's Bull, the planet followed this star 2' 15.5" in time.

These two observations are as follow ; but Dr. Olbers still gives the declination as very doubtful :

1803.	Mean time at Bremen.	Apparent A.R. of Pallas	Apparent Declination of Pallas.	Stars with which compared according to Bode's Catal.
Feb. 21	17 ^h 6' 10"	272 56 45	7 31 14 N.	No. 36 Pon. Bull.
23	15 24 36	273 28 39	7 46 1	No. 42

Dr. Olbers, in a letter dated March 3d, says, " The weather has been very unfavourable for observing Pallas. Neither Harding nor I have been able to see it again, on account of the cloudy state of the atmosphere ; and as it is now moon-light, it will again be lost for some time." The indefatigable Dr. Gauss, however, could not withstand the great desire he had to undertake an improvement of the elements, intending at a future opportunity to correct them still further from new observations.

These six new elements are as follow :

Epoch meridian of Seeberg 1802 - - 143° 29' 17.27

Noon 1803 - - 221 28 54.0

D 2

Greater

Greater semi-axis	- - - - -	0.4426160
Daily tropical motion	- - - - -	769.4161''
Eccentricity	- - - - -	0.245619
Dist. from the sun 1803	- - - - -	301° 24' 13''
Ascending nodes 1803 stationary	-	172 28 8
Inclination of the orbit	- - - - -	34 38 19.8

A comparison of these elements with the preceding observations shows that the difference is very small.

For the observations of Dr. Olbers give:

1803.	Calculated A.R. of Pallas.	Calculated Declination.	Difference	
			in A. R.	in Declin.
Feb. 21	272° 56' 25.0''	7° 31' 29.8''	- 20.0''	+ 15.8''
23	273 28 46.8	7 45 52.3	- 8.8	- 7.7

IX. *On the Management and Improvement by Tillage of strong wet Loams in which the Clay greatly predominates*.*

HERE again I can meet the wishes of the Board, and give them the information they require from actual experiment, and on a very large scale. This soil, of which there are thousands of acres in Suffolk, is in that district called clay; but I believe there is no pure clay in that county. The lands having a greater or smaller portion of calcareous matter in them, are consequently of a better quality than the clays in Surrey and Sussex, though they were in no higher repute until within these twenty years. A manor farm near Eye was offered for sale about fifteen years since, when I purchased it, much against the inclination of my friends, who joined in the general cry of the farmers, and pronounced the lands the poorest that ever grew upon. This did not discourage me. I found a proportion of calcareous earth intermixed with the clay on the surface, and at two and three feet below the surface a stratum of fine marl in almost every field. The estate was so depreciated that I did not attempt to let it. Of the two hundred and thirty-five acres, of which the whole consisted, the major part was old sour grass lands, full of water quitch, overrun with black and white thorn, bushes, and pollard trees. I began my operations by grubbing the thorns and trees,

* From Mr. Close's paper published in the Communications to the Board of Agriculture, vol. iii. part 1.

levelling all the old fences, and filling up the ditches where they formed inconvenient angles and corners. Then the fields were squared; new ditches cut, four feet and a half wide at the top, one at the bottom, and three feet six inches deep, and planted with white thorn: expense per rod two shillings and three pence, exclusive of white thorn and bushes. Drains were then drawn out as deep as possible by the plough, dug with the common spade ten inches, then with a narrow spade ten inches*, and with a small gripping spade fourteen inches, filling the latter trench with haulm, and putting bushes on the top of it, covering the whole with mould. Heath, where it can be procured, is preferable. Expense per score yards four shillings and six pence. Marl pits were then opened, when possible, at the point where four of these new ditches met, by which the distance of carting was reduced; and with a few posts and rails the fences were made good, and a permanent pond of water secured to each field, after the marl carting was finished. Eighty loads was the quantity per acre, at two pounds twelve shillings and six pence per hundred loads.

This marl was spread on the old sour grass, and left to be pulverized by the winter's frost, and incorporated with the surface by the feet of the cattle and sheep. Early in the spring the lands, thus prepared, received one clean deep ploughing, and were drilled with oats; produce about thirteen quarters per acre. The second year drilled with peas; produce seven quarters and a half per acre: ever since which time they have been cropped regularly, and the produce has been immense. That the Board may form an accurate idea of the advantage derived from converting these old grass lands into tillage, I shall state the value of the farm per annum when I purchased it, and the money I gave for it; then the sum I received for my improvements; the rent at which I let it on a twenty-one years' lease; and the price I received for it when sold. There were two hundred and thirty-five acres, valued at one hundred and twenty pounds per annum, (the tenant paying twenty pounds a year land tax) and the price I paid for it three thousand three hundred pounds. I received for my improvements, of the tenant five hundred pounds, besides three hundred and fifty pounds which the old pollard trees sold for; let the farm for two hundred and twenty pounds, the tenant paying twenty pounds a year land tax, and covenanting to keep all

* In the old grass lands the sod was reversed and placed over to the bottom drain, which formed an effectual covering.

the buildings, &c. in perfect repair. I then sold it for six thousand five hundred pounds. The gross produce per acre of these old sour grass lands could not exceed twenty shillings; and little or nothing was expended on productive labour. Ever since they have been converted into tillage double the former gross income must have been expended for labour, and the aggregate produce at least six pounds and six shillings on every acre, including the whole rotation of crops. You have here, gentlemen, one strong proof of the utility of converting certain portions of grass lands into tillage. This farm is now in such a high state of cultivation, that any part of it might be converted into meadow land by the mode recommended in my former chapter, and would be worth two pounds per acre, as I shall demonstrate from actual experiments on rather inferior, though somewhat similar, soils.

When first I became rector of in the county of there were two fields of fifteen acres much impoverished by constant cropping and tillage, valued in that state at sixteen shillings per acre; soil a strong loam, and very wet. These fields I drained, to take off the surface water, as recommended in my first chapter; then fallowed and dressed them, sowing the hay seeds, &c. in August. The produce the first year was estimated by many farmers at four tons per acre; it was so stout that the mowers had double their usual wages, and it was impossible to spread it on the land. I have ever since, viz. for eight or nine years, let these lands at two pounds two shillings per acre. As the tenants have been frequently changed, the lands have been mown almost every year, and are now somewhat exhausted, but may be made as good as ever by a dressing, and by being fed with sheep one summer; or, if ploughed, would produce abundant crops for eight or ten years, and might then be converted into meadow by the process I have before recommended: the course of crops will be found at the end of this essay as best adapted to clayed loams. By the mode of cropping there recommended, a farmer may continue sowing wheat in any season. If too wet to work his pea, bean, or vetch land, his clover lands will be in order; if too dry for the latter, the former will work well, and he may be sure of putting in his seed in good time. It will be obvious, that when clover is sown with the spring corn, the three feet ridges, on which the turnips or cabbages grew, must be levelled, and five or ten feet ridges formed. The clover seed should be sown immediately before the last horse-hoeing. I could bring proofs
ad

ad infinitum to demonstrate the folly of old grass lands to remain untilled, under the foolish idea of not impoverishing the land. So far from being improved by such a state of rest, the lands, by being saturated with stagnant water, become worse and worse every year; and one hundred acres in this state, at seven shillings and six pence per acre, would starve the tenant and his cattle, bring a very poor rent to the landlord, and produce nothing to the public. Under the management I have recommended, the income of the landlord may be nearly tripled, and the produce to the tenant and the public increased in a tenfold degree. But I will, as the Board requires, substantiate these assertions by further experiments made under my direction on some lands in Sussex, belonging to his grace the duke of Richmond, and on others in Suffolk, the property of lord Rous. One will be an additional proof of the utility of breaking up old sour grass lands; the other, of the advantage of restoring lands exhausted by constant tillage into good and productive meadows. It may be necessary to say, that I applied to my two noble friends for an account of the following experiments, and received permission to transmit them to the Board. The farm of lord Rous consisted of two hundred acres of land, half of which was old sour grass, let for ninety pounds a year; but previous to my riding over it with his lordship, his steward had valued it at one hundred and fifty pounds a year, allowing a specific number of acres to be broken up every year, on condition that the tenant laid down with hay seed an equal number of acres of the old arable lands. I stated to lord Rous, that he could not have a full and fair rent for his farm under covenants, or without allowing the whole to be cleared, ditched, drained, marled, and ploughed, except about ten acres near the house. To these conditions he readily, and from conviction, acceded; and I valued the estate (which was then let at ninety pounds a year, and estimated with a partial privilege of breaking up the old pastures at one hundred and fifty,) at two hundred and twenty-five pounds; for which sum it was let to an unexceptionable tenant, so much approved of, that at the end of three years, before he had reaped the fruits of his own industry, his lordship offered him a much larger occupation, if he could find a tenant of character to repay him for his expensive improvements. Several candidates offered, out of which lord Rous selected one who paid the outgoing tenant five hundred and fifty pounds for his ditching, draining, and marling, and ex-

pended about two hundred more in similar improvements. Precisely the same system was adopted by these spirited farmers for improving these lands, as I have before given the particulars of in the account of the estate I purchased; and a recapitulation would be protracting this essay, which I wish should contain, in the most concise and plain terms, as great a mass of information as I am capable of furnishing. As his grace the duke of Richmond, with that exactness which marks his conduct in all matters of business, has furnished me with an account of his experiment, attested by his signature, I should not feel myself justified in deviating from it, even in form, but shall transmit it in the very words in which I received it: "I do certify, that some years ago above fifty acres of land that I wished to convert from tillage into pasture, which had been attempted several times by the common mode of sowing grass seeds with corn, without success, were (after two years successive turnips fed off) sown with hay seeds in a very large proportion, in August, without corn, as advised by the Rev. Mr. —; and the result was, two crops of hay the first year; one estimated at two tons per acre, and the other at one ton and a half. The land has since been dressed with old mortar and other manures occasionally, and the permanent improvement on the value of the land is estimated to amount to at least fifteen shillings per acre*.

"Jan. 23, 1801.

(Signed)

"RICHMOND."

These experiments exactly correspond in their result with those which I have stated from my own practice; and being made in different situations, they seem to be conclusive on the most important subjects selected by the Board as objects of their premiums. The characters of the noblemen under the sanction of whose approbation I have given you the particulars of the two last experiments, stand too high in the estimation of the country for honour, veracity, and minute exactness in every transaction of the least importance, to need any encomium from me. For the particular management of these experiments I shall refer you to those similar ones made upon my own lands, from which no variation was made except in the mode of draining the duke of Richmond's lands; which, to the best of my recollection, were made with bricks. Though the query respecting the utility of draining may be supposed in effect to

* By a letter previously received from the duke of Richmond, his grace informed me this was exactly double the value of the land.

be answered by its being the foundation of all the advantages which I have so minutely detailed, yet I must urge the necessity of this operation as the *sine qua non* of agricultural improvement; and add, that though the particular method of surface-draining strong clays and of clayey loams, where the substratum was less retentive of water, has proved both cheap and effectual, yet in all bogs, peats, gravels, or sands, where a large tract of land may be made firm and dry by one cut, Mr. Elkington's mode of tapping and boring is a very superior method of effecting this necessary and primary improvement. Upon rich black loams, sandy loams, good deep lands with a chalky substratum, peats, &c. (all soils capable of producing the natural grasses of this country) the same system as has been recommended in the preceding experiments will be found effectual for converting them into meadows or pastures; only varying the rotation of crops according to the table at the end.

Objections to sowing Grasses with any Crop of Corn, when it is required to obtain a Meadow; and a comparative Estimate of the two Methods.

Here again I can give you, my Lord, and the Board, information from actual experiment. A friend of mine wished to procure a good meadow or pasture around his house: he fallowed the land for barley; but the spring proving wet, and the soil being a strong loam, he could only put half of it in order for that crop, which was sown, and laid with clover and rye grass. The other part was fallowed, and sown in August with the sweepings of hay chambers, as I have recommended. The barley was a good crop, and the clover and rye grass were probably equal to the first year's cut of hay. The second year the artificial grasses began to fail; worse the third, fourth, and fifth. The sixth year, after having received two dressings, the spontaneous product of the soil began to give a fleece over the surface of the land. About ten years after these lands were sown, I saw this field, when the part sown in August was worth at least fifteen shillings per acre more than the part which had been sown with artificial grasses in the barley. Thus from actual experiments, numbers of which I could adduce, I conclude that sowing the sweepings of hay chambers in August is preferable to sowing artificial grasses in the spring with any crop of corn. Suppose the corn worth five pounds per acre, the difference in the produce of hay or feed in the second, third, fourth, and fifth years, would more than counterbalance

lance this; and the proprietor would find a permanent improvement in his land of from fifteen shillings to twenty shillings per acre*.

Many object to sowing such rubbish as the sweepings of hay chambers produce; and I wish most sincerely any method could be devised for procuring clean seeds of our best and natural meadow grasses. It is a great desideratum; and premiums to encourage agriculturists to save seeds of the fescues and poas, &c. and for the largest quantity of land sown with these seeds, and kept distinct, might be of infinite service. Until this can be effected, the plan I have submitted to you, my lord, appears to me the most eligible. It certainly has been crowned with the greatest success. Sowing rubbish in August is not of so great importance as in the spring. In the former season all the annual seeds vegetate; and if the beginning of the winter be mild, they will blossom; but they cannot perfect their seed, and the first frost destroys them. If sown in the spring, they vegetate, blossom, perfect and shed their seeds, and thus stock the land with noxious weeds. The facts I have stated must do away the objections to sowing rubbish. It is immaterial what you sow, if you do but obtain an abundant crop, and leave your land clean and in good order. Should it, however, be thought proper to sow the seeds with any corn, barley must have the preference. If sown with oats, and the land be prepared as it should be, viz. in high tilth and order, the oats will be so luxuriant as to smother and destroy the young plants. Lands intended for grass or meadow cannot be in too high a state of cultivation. The permanent improvement in the intrinsic value of the land will abundantly repay almost any expense. To improve the soil with this view, and then to exhaust it by a crop of corn at the time of sowing the seeds, appears to me a sure method of counteracting the very object in view. I have always advised the landlord to provide the hay seeds, that he might be sure, not only of the quality, but that the quantity should be sown. Indeed, as the improvement is a permanent one, if the tenant make a good fallow and dress the land well previous to sowing the seeds, or directly after carting the hay, he does his proportion. On these terms I have permitted many tenants to break up grass lands, on their cove-

* Had the barley been drilled, horse- and hand-hoed, and kept perfectly clean until July, and then sown with the common hay seeds, the only objection to the plan would arise from exhausting the soil by a crop, when your great and leading object is to make a permanent improvement.

nanting to lay them again in a specified number of years, preparing the land by a complete highly pulverized fallow, and sowing the hay and other seeds of the landlord's procuring, the first week in August; and after the first crop of hay to dress the lands with twenty loads of good dung, or compost, per acre, if the soil was of such a quality as not absolutely to require dressing previous to sowing the hay-seeds. A dressing before sowing, or after carrying the hay the first year, is absolutely necessary.

The additional rent must be regulated by the quality of the land, from ten shillings per acre to fifteen and twenty shillings. If the land be naturally very sterile, and difficult to till, as in the wilds of Sussex, and the tenant be obliged, by paring, burning, and fallowing, to purchase his first crop very dearly, no additional rent should be required, but he should be tied to such a judicious mode of cropping as would leave the lands in an improved state for the landlord. See the Table at the end. If the land, after being drained and marled, will produce a good crop of oats with one effectual ploughing, the tenant may pay an additional rent of ten shillings per acre. Where the lands will produce a good crop from one effectual ploughing, without the expense of draining and marling, the rent may be advanced fifteen shillings per acre; and on lands naturally very good, a landlord may expect an additional rent of twenty shillings per acre of grass land he may allow to be converted into arable.

The depth at which grass lands should be at first ploughed must depend on the nature of the soil. In my directions for improving sterile clays, I have, I trust, proved the utility of moving such soils to a great depth. Similar observations will apply to all clayey loams, where the surface and the substratum consist of the same component parts. On deep rich loams of all denominations deep ploughing will be advantageous; but when there are only four or five inches, or less, of good earth, and the under stratum be a rubbly chalk, gravel, or other unproductive soil, it should never be brought to the surface; and, being naturally pervious to water, it will not require being moved below the action of the plough.

In my practice I have almost invariably mown the grass the first year, and immediately dressed it well and fed it the second. My success, in every instance, has been great, and no inconvenience ever arose from this system. In some few cases I have seen much injury arise from feeding the first spring: the lands being in a high state of cultivation

in August, and the surface kept very light by the frosts and freeze of grass, the cattle have pulled up the young plants by the roots, and entirely destroyed them. The duke of Richmond's bailiff was very desirous to feed the young grasses before mentioned in the spring; the flock wanted the food, and there was an abundant crop. So urgent was the man, that the duke, who had promised that my directions should be implicitly obeyed, wrote for my consent; but to this plan I positively objected, declaring that, should the stock be allowed to feed the lands the first spring, I would not answer for the success of the experiment. His grace, who is always firm on such occasions, would not therefore give his consent to his bailiff's request. The result of the experiment was such, that neither the duke nor his steward ever regretted adhering to my advice.

[To be concluded in our next.]

X. *Observations on some Insects little known.* By
M. MEYER, of Göttingen*.

IN endeavouring to class insects which have not been accurately described by those who saw them, nothing more can be offered than conjectures. The few characters which are given must be employed in order to deduce from them something nearer the truth; and it then becomes the duty of future observers, when they have an opportunity of seeing new insects which have any similarity to those imperfectly described, to examine and establish the points in which they correspond. It is by these means only that we can attain to certainty in things which have before rested only on conjecture.

I have for some time past read, in different authors, accounts of insects, which attracted my attention the more, as noxious qualities were ascribed to them: I wished that I might be able to obtain better information than I found in the books where they are described respecting the genera to which they belong; and, having taken the trouble to determine this as far as possible, I here give the result of what appeared to me most probable. I am far from giving these observations as strictly agreeable to truth: on the contrary, I consider them as very imperfect; but it is only by making them known that I can obtain more certainty in regard to my conjectures.

* From *Magazin für das Neueste aus der Physik*, vol. ix. part 2.

The following account is to be found in Ulloa's Voyage to South America* :—" In the valleys of Popayan, in South America, there are found insects which are very remarkable on account of the poisonous juices contained in their bodies. Among these there is one called *coya* or *coyba*, of a fiery red colour, and not larger than a common bug. It is generally found under stones, and in the fields between the grass and other plants. When this insect is bruised on the skin of any animal its pernicious juice penetrates into the pores, becomes mixed with the blood and fluids, and occasions ulcers, which, if a remedy be not speedily applied, soon produce death. The only remedy for this evil is to set fire to the dried stems of a certain plant, and to keep the body of the patient over the flame till it begins to swell; an operation which the inhabitants of these districts can perform with great dexterity. It has been observed that no bad consequences ensue when this animal has been bruised on the palm of the hand; from which it is inferred, that the small quantity of poisonous juice is absorbed by the callous parts of the skin in the palm of the hand, and cannot penetrate into the blood. The natives, who travel through these districts, often bruise the insect in their hands in order to gratify the curiosity of travellers. But there can be no doubt that the *coya*, if bruised in a less callous hand, the skin of which is thinner or tenderer, would produce nearly the same effects as when bruised in other parts of the body.

" These who travel through these valleys, when they feel a sensation of puncture by an insect in the face or neck, take great care not to rub or to touch the part, because the least pressure bruises the *coya*, which when not bruised is harmless. On such occasions, they cause the natives who accompany them to examine the place; and if the latter observe a *coya*, they blow it off with their breath without touching it; by which means the travellers are freed from danger. The cattle in these valleys are led by instinct to use the same precaution; they breathe strongly on the herbs and grass on which they are about to feed. The mules, however, often eat the *coya*; after which they gradually swell, and at length die."

The province of Popayan, as is well known, is a district of New Grenada, and particularly remarkable because it is the only part of America where platina is found. I have seen no account of the *coya* in any other work, and there-

* Vol. i. p. 343.

fore I am much inclined to suspect the accuracy of the above. At any rate, very little can be gathered from this description; but, as the insect is compared to the bug, there is reason to conjecture that it may have some affinity with that species. It is, however, difficult to determine whether it be an acanthia, a cimex, or a reduvius. Besides, we have hitherto no instance that any insect belonging to this species can produce so noxious effects; and if the coya really exists, the account of it is probably exaggerated, as Ulloa's information was no doubt derived from the natives. Stoll* describes an insect of Surinam, which perhaps is the same as the coya, or has an affinity with it. He calls it the poisonous bug, and gives the following description of it:—"The feelers, which consist of five joints, are yellow; the eyes are very prominent, and behind these eyes are placed two small shining ones: the neck is united to the corslet by means of a small tube. The fore-legs are covered with black hairs; which gives them a very rough aspect. The corslet is of a shining appearance, black and arched: the membranous part of the sheaths of the wings is transparent, and the wings themselves are brownish yellow: the hind part of the body is of a dark yellow colour on both sides. It is said that this bug is furnished with a sting, and that the puncture it makes occasions unsupportable pain." I have mentioned this bug, in my Natural History of poisonous Insects †, under the name of *reduvius venenatus*. The *reduvius annulatus*, however, seems to approach nearer to the coya, because it is red; but this bug, as far as I know, is not a native of America, which is the country of the *reduvius venenatus*.

M. Schroter, superintendant of Weimar, is the first naturalist who gave an account of the *moschka*. It is to be found in the first part of his *Treatises on various Objects of Natural History* †. The account he gives is in substance as follows:—"The *moschka* has some similarity to the *musca nemorum* of Linnæus: it is, however, different. It differs from Schäfer's *æstrus* by its four eyes, as the latter has only three. It is very dangerous to cattle, and particularly to oxen. It makes a loud humming noise when it approaches them, falls suddenly upon them, and then drops on the grass. The cattle as soon as they hear this enemy at a distance betake themselves to flight, and cannot be restrained. This insect seldom appears, and flies with great

* Tab. xiii. f. 66.

† Part i. p. 141.

‡ Abhandlungen über verschiedene Gegenstände der Naturgeschichte, Halle 1776, 8vo.

rapidity: it does not long remain on the cattle, but throws itself on the grass, and therefore can with difficulty be caught. M. Schroter was able only once to obtain one of these insects. It is fond of frequenting those fields which are interspersed with clumps of trees. The head is oval, and tapers towards the mouth; the mouth consists of two moveable jaws, and the pincers are like those of the beetle. The whole head is covered with hair, but more so around the mouth than on the forehead. The hair on the upper part of the head stands erect like bristles, but that around the mouth hangs down. Its colour is whitish. Two large eyes stand on each side of the head, and two small ones in the centre. The large eyes are cut into facets, the small ones are plane. It has a white hairy band, which passes round the whole neck and ends at the extremities of the first two legs. The corslet is round, very black, and at the extremity towards the body covered with single, crooked hairs, white at the points. The two wings are white, a little brownish, and not much longer than the body. The hind part of the body is entirely covered with hair; the upper part towards the corslet is entirely black, interspersed with single white hairs; but the lower part and the rump are yellow. The whole body terminates in a point. On the lower part the insect is completely hairy and black, except on the rump, where it is yellow. The six legs are black: the hind thighs are covered with hair, and the tarsus has six joints.

M. Schroter reckons this kind of gnat among the Linnaean flies with silk-like naked feelers; and it certainly deserves to form a peculiar variety. He has given a representation of it, tab. i. fig. 6.

After him few seem to have paid any attention to this insect till it was again brought to notice by M. Bechstein. He introduces it under the name of the cattle fly, *musca nemorum* Linn., as an enemy to cattle, and says it has four eyes, and that the body is marked with yellow rings and three white bands.

From various grounds it appears that this insect, till its natural history has been more accurately examined, may be placed in the same class as the *rhingia* of Fabricius. Experienced entomologists will certainly do a great service to natural history by undertaking the examination of this subject. Should it be found that it really belongs to this class, it might be introduced into the system under the following characters:

Rhingia

Rhingia tinniens. R. hirsuta, atra, thorace glabrò, abdominis segmento primo albo, ano flavo.

Lepechin, in the first part of the Journal of his Travels through different Parts of the Russian Empire*, describes a spider, which, on account of its remarkable characters, deserves a place in our systems, of which it has hitherto been deprived. It has, perhaps, been overlooked, because Lepechin calls it a tarantula; a name very improper, since the spider described by him differs very much from the tarantula (*aranea tarantula*). At any rate, on account of this difference it ought to be distinguished by the name of the Tartarian tarantula. His description is as follows:

On account of the saline nature of the districts around Usoleia, a very dreadful kind of spider is fond of frequenting them. These animals reside in holes under the earth, and the passage which conducts to them does not seem to be above a quarter of an arschine in depth. They extend their webs on the ground before their hole, and devour without mercy the insects which fall into them. The exterior part of the body is entirely covered with soft hair, which gives them a horrid appearance. The head has the figure of a half pyramid, the sharp side of which represents the middle of it, and the bottom the forehead. The breast is covered by a roundish shield, whitish on the edges and furnished in the middle with black hair. Small white and almost imperceptible veins, which proceed from the white edge of the shield, meet in the centre. On the top of the head there are eight eyes, the smallest of which, four in number, occupy the lowest place before the mandibles: above these there are two large eyes, and above these two others of a moderate size. Besides these there are large, bright red eyes on the sides above the mandibles. The two mandibles are covered with soft hair, blackish at the ends, armed with small pointed and stiff hooks. The feelers consist of four joints; three of them are reddish brown; the other, which is the last, is black, and has a blunt point. On the back there are two rows of whitish points between the black ones.

The lower part of this spider is black and rough. The first joints of the legs are reddish brown on the lower side; the next is short, and black all around; the third is reddish brown, with a black band at the end; the fourth and fifth are grayish black, and the last is split in the form of a claw. The rump is furnished with six papillæ, two of

* German edition, Altenbourg 1774, p. 200.

which are on the sides, two below, and two between the upper and lower ones, which are shorter than the others. Though I made inquiries among the inhabitants with the greatest care, I could not find that there ever had occurred any instance of people being hurt by this formidable animal.

In p. 257 of the same work Lepechin gives a further account of this spider in the following words:—"In this place we learned some further particulars respecting the tarantula. Having dug up some of their nests, we observed what this animal uses as weapons against its enemies. When it finds itself deprived of every means of flight, it remains quiet, puffs itself up, and ejects from its back, as if through a syringe, a white juice to the distance of two arschines. I cannot, with certainty, say whether this juice be poisonous or not, because none of us were inclined to make the dangerous experiment. But we were assured by one of the cossacs who were on guard at Woguti, that a woman of Little Russia had the misfortune to feel the effects of this juice. She dug up a spider of this kind, and on turning it with a stick it squirted some of the juice on her hand, which in a short time became inflamed: the part swelled with unsupportable pain, and the most serious consequences might have ensued had not a remedy been speedily applied. The best antidote to this poison is the animal itself. It is put alive into oil, and kept in that manner for use. If the part exposed to the action of the juice be rubbed over with this oil, a cure takes place without any need of employing music; which, indeed, could not be obtained in these districts, as the whole music of the country people consists of a screeking noise produced by reeds, or the stem of the wild angelica (*angelica sylvestris*). These spiders were so quarrelsome, and greedy of devouring each other, that of twenty of them confined in a glass only one remained alive as conqueror. The black sheep, it is said, are great enemies of the tarantulas. These animals dig them up from the earth, and readily eat them; on which account these sheep are highly esteemed by the Calmucs, who are much afraid of the tarantulas, so that they never encamp in places frequented by them, but proceed to some other district, however tired their cattle may be.

Lepechin calls this kind of spider a tarantula, and consequently it must have some similarity to the tarantula of the system. According to Fabricius the tarantula has ten eyes,

in the following form, $\begin{matrix} \vdots \\ \vdots \\ \vdots \\ \vdots \\ \vdots \end{matrix}$; whereas that of Lepechin has only eight, as the large red eyes above the mandibles

will hardly be considered by entomologists as real eyes, but as prominences which have the form of eyes without possessing their properties. If this conjecture, which appears to me very probable, be well founded, and as the eight real eyes of Lepechin's spider, according to his own ac-

count, are arranged in this manner, $\begin{matrix} \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \end{matrix}$, it belongs to Fabricius's sixth family of spiders, where the *aranea clavipes*, *saccata*, and *fumigata*, are placed. In regard to form, it seems to have the greatest similarity to the *aranea clavipes*; from which, however, it is so different in many respects, even without taking into account the diversity of country, that few entomologists will be inclined to consider both as varieties of the same kind. The parts of the mouth are not different from those of spiders in general: what Lepechin calls feelers are, in all probability, nothing else than what entomologists have long and much better distinguished by the name of *palpi*; for it may with certainty be affirmed, that no *antennæ* have ever yet been found in spiders. It is seen, from what has been said, that Lepechin's tarantula certainly deserves to form a new kind in the system, which may be placed after the *aranea clavipes* as follows:

ARANEÆ tartaricæ oculis $\begin{matrix} \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \end{matrix}$.

A. hirta atra, thorace albo marginato, rotundato, abdominis dorso seriebus duabus punctorum albicantium, pedibus e rufescente-nigris.

.Habitat in Tartaria valde frequens.

Subterranea, retiaria. Hirta, atra. Thorax rotundatus, albo-marginatus, medio alter, pilosus, verulis albis. Mandibularum pars lateralis, maculis magnis, ocularibus, rubris. Palpi rufescentes, articulo extremo obtuso, nigro. Abdominis dorsum seriebus duabus punctorum albicantium. Femora priora subtus rufescentia; tibiæ tarsique annulis atris, rufescentibus et nigris. Anus papillis sex.

.Succo viroso albo prædita.

It is not improbable that this spider is of the same kind as that described by Laxman* under the name of *aranea Singoriensis*: at any rate there is great reason to make this conjecture, from what Gmelin says of this spider, in describing the *aranea tarantula* †: should this be the case,

* Nov. Comment. Petropol. vol. xiv. p. 602.

† See Systema Nat. Linn. 6d. 13, tom. i. pars v. p. 2956, n. 34.

Laxman's *aranea Singoriensis* ought not to be considered as a variety of the *aranea tarantula*, but rather as a variety of Lepechin's spider, or as a particular species. Whether the large red pair of eyes be really eyes, still remains a problem which deserves to be an object of future research to naturalists; and the question, whether the proper tarantula (*aranea tarantula auctorum*) be found in the southern parts of Russia, requires further proof before it can be answered in the affirmative.

In *A Voyage to Madeira and the Leeward Caribbean Islands*, by a lady, London 1792, 8vo. mention is made also of a tarantula, which, according to every probability, is nothing else than the *aranea clavipes*, or a large American kind of earth spider. It does not appear to be the *aranea avicularia*, at least according to the description of it, which is very defective, like all the descriptions of natural objects found in books of travels written by ladies. There is reason, however, to conjecture that it is not the real *aranea tarantula*, because this spider has never been found in the districts which the authoress visited. Of her spider she gives the following account:—"A kind of tarantula is found in Antigua in stony places and under old ruins. Its bite produces convulsions with strangury, and sometimes proves mortal. Music, however, produces no sensible effect on those bitten by it." It certainly is worth while to make some further researches respecting this spider.

In large collections there are generally found two varieties of the American bird spider (*aranea avicularia*); a larger than the common, which is dark brown, and a smaller, of a bright cinnamon colour, a very beautiful specimen of which I saw in the collection of Mr. Ortman, apothecary of Hamburg. The owner of it observed on the inside of the first joint of the fore-legs of some specimens a small, black, corneous, sharp-pointed spur. Is it not probable that this spur is a distinguishing mark of sex, and may not this mark be peculiar to the male? These questions can be best answered by comparative observations.

John Angelo Brunelli, in his description of the river of the Amazons, in the Transactions of the Institute of Bologna*, gives an account of an insect, which is so badly described that no entomologist can with certainty determine to what genus it belongs, though it is probable that it belongs to the *cymothora* of Fabricius: at any rate I am of opinion that we shall not be far from the truth if we con-

* Comm. de Bononicis. Scient. et Art. Institut. atque Acad. tom. vii. 1791, 4to.

sider Brunelli's insect as a little known species of this genus till the natural history of it has been better explained. The following is his account of it:—"The Brasilians give the name of *condiru* to an insect shaped like a small worm, which, when stroked with the finger in one direction, is smooth, but when stroked in the other feels so rough that it wounds the finger. It is greedy of blood from wounds; it is dangerous to the crocodile when wounded in the water, and is found in many places in multitudes. It makes its way into the limbs of the natives, and cannot be taken out without laying open the part: the men, therefore, must be very cautious when they go into the rivers."

XI. *Miscellanies in Natural History: viz. An Improvement in the System of the Mammalia; Observations on a living Opossum; and an Account of the third Generation of the Porcupine Man.* By Professor BLUMENBACH*.

THE system of the mammalia, which I have made the foundation of my *Manual of Natural History*, and which has been followed by various naturalists in their works, will, I hope, be rendered more agreeable to nature, and more perfect, by the following alteration, which has been occasioned, in particular, by the discovery of the *ornithorhyncus paradoxus*. The organs of motion are made the chief ground of these orders, because they soonest strike the attention, and are in the most intimate relation with the whole habits of the animals. I have, however, subdivided two of them, which comprehend a great variety, into two families, according to the diversity of their incisor teeth, and distinguished them by the known names of some Linnæan orders, that those whole classes are arranged as follows:

I. ORDER. BIMANUS.

1. Homo.

II. QUADRUMANA.

2. Simia.

3. Papio.

4. Cercopithecus.

5. Lemur.

* From *Magazin für den Neuesten Zustand der Naturkunde*, &c., by J. H. Voigt, vol. iii. 1802.

III. CHIROPTERA.

6. Vespertilio.

IV. DIGITATA.

Mammalia with detached toes on all the four feet. This order is divided, according to the diversity of the incisors, into the following families:

A. GLIRES with mouse-like incisors.

7. Sciurus.

8. Glis.

9. Mus.

10. Marmota.

11. Scavia.

12. Lepus.

13. Jaculus.

14. Hystrix.

B. FERÆ. The *rodentia*, properly so called, and some other genera with similar teeth.

15. Erinaceus.

16. Sorex.

17. Talpa.

18. Didelphis.

19. Viverra.

20. Mustela.

21. Ursus.

22. Canis.

23. Felis.

C. BRUTA. Without incisors, or at least without fore-teeth.

24. Bradypus.

25. Myrmecophaga.

26. Manis.

27. Tatu.

V. SOLIDUNGULA.

28. Equus.

VI. BISULCA. The ruminating animals with divided hoofs.

29. Camelus.

30. Capra.

31. Antilope.

32. Bos.

33. Giraffa.

34. Cervus.

35. Moschus.

VII. MULTUNGULA. Mostly large, or shapeless, bristly, thinly haired mammalia, with more than two claws on each foot; comprehending swine, for these properly have four claws.

- 36. Sus.
- 37. Tapir.
- 38. Elephas.
- 39. Rhinoceros.
- 40. Hippopotamus.

VIII. PALMATA. Web-footed mammalia, again divided, according to the diversity of their incisors, into the above three families.

A. GLIRES.

- 41. Castor.

B. FERÆ.

- 42. Phoca.
- 43. Lutra.

C. BRUTA.

- 44. Ornithorhyncus.
- 45. Trichechus.

IX. CETACEA.

- 46. Monodon.
- 47. Balæna.
- 48. Physeter.
- 49. Delphinus.

Observations on a living Opossum, Didelphis marsupialis.

Some months ago I obtained that wonder of all the land animals, as Mr. Lawson calls it, for which I was indebted to the kindness of an American friend, Dr. Tidyman, of Charlestown, in South Carolina.

It is about as large as a middle-sized cat. Its head is shaped like that of the fox: but its long snout, and the bare flesh-coloured nose turned somewhat upwards almost in the form of a snout, are nearly like those of a pig. The aperture of the mouth is exceedingly wide: the lower jaw is perceptibly shorter than the upper; and the upper angular teeth, even when the mouth is shut, are visible. The head is white, with a faint blackish stripe along the forehead, and the part between the fore corners of the eyes and the snout is of the same colour. Both sides of the mouth, and in particular the chin, are furnished with a great many long stiff hairs. The pupil of the eye is small, but the cornea is proportionally large and exceedingly convex, so that very little of the white of the eye can be seen; and
this,

this, with the dark brown colour of the iris, gives to the animal a lively appearance. Of a *membrana nictitans*, as among the quadrumana, scarcely any rudiment is to be seen. The ears are large, black, naked, and, according to appearance, merely membranous, without any cartilaginous folds, and therefore nearly like those of the bat; in my animal also, without the white border which is ascribed to others of this genus.

The neck is short and thick, and the same is the case with the rump, which is well covered with hair. Sometimes the hair on the back is long and erect, almost as in the badger; its colour is white mixed with black, and darkest on the shoulders.

The bag on the belly is very apparent by its prominence, especially when the singular *ossa marsupialia* or *cornua pelvis abdominalia* lie under it. The place of its aperture is marked only by a longitudinal fissure.

The tail is about the length of the body; it is almost naked, and as scaly as that of the rat, but a real *cauda prehensilis*.

The shoulders and fore legs are black, and covered with soft hair. The toes are naked, and of a flesh colour. The hind feet are furnished with detached toes with a small flat nail, but on all the other toes there are hooked claws of a white colour.

A figure of the animal, drawn from the life, may be seen in my *Abbildungen naturhistorischer Gegenstände*, tab. 54.

It is a real *animal omnivorum*, and can feed upon any kind of fruit; it is fondest of plums, and of other food, next to flesh, of fowl, game, and in particular of soup and bouilli. It chews its food with great deliberation, and catches the large pieces very dexterously with its fore feet; and it uses these feet with great address for dressing its snout, on which occasions it sits on its hind legs like a squirrel.

Its cry, which it seldom emits except when irritated, is a weak kind of grunting. It drinks very little, and sometimes not for several days. It seldom makes water, and even when in good health voids its excrements only once in four or five days. It however does neither in the place where it lies, but always retires to a corner of its kennel.

In general it preserves itself very clean; and on the whole is a quiet, good-natured animal; slow, and as it were cautious in all its motions; and of so strong a constitution that the people in America are accustomed to say,

“ If a cat, according to the proverb, has nine lives, the opossum has nineteen.”

I shall now say a few words respecting the oldest accounts and figures of this animal, which were published in Europe after the discovery of the New World.

The first person who made mention of it, as far as I know, was V. Pinzon, who accompanied Columbus on his first voyage of discovery. This notice is to be found in Herwag's Collection (*Novus Orbis*, the first edition of 1532, p. 121*.)

About the end of the fifteenth century one of these animals was brought alive to Seville, and presented to the king of Granada.

Peter Martyr, who saw a dead specimen of this animal, gave a more accurate account of the opossum, which he thus describes: “ *Monstrorum animal, vulpino rostro, cercopitheca cauda; vesperilionis auribus, manibus humanis, pedibus simiam æmulans, &c.*”

The name of *simivulpa* was first given to it by Gylli, in his edition of *Ælian*, 1553, 4to. p. 209; and this denomination was afterwards adopted by Gessner.

The oldest figure of it with which I am acquainted, but which is indeed very defective, is in the unfortunate Serretus's edition of Ptolemy, 1535, fol. tab. 28. It is there given as brought from the eastern coast of *Terra Firma*, with this inscription: “ *Reperitur hic animal habens reservaculum quo suos pullos secum portat, et eos non nisi lactandi tempore emittit. Tale regi Hispanicæ Granatæ oblatum est.*”

The first tolerable figure was given by Nierenberg, p. 156, if we except the woolly hair and the hind feet, which are entirely misrepresented.

The third Generation of the Porcupine Man.

The well-known astronomer J. Machin gave in the *Philosophical Transactions* for 1732† the first account of a boy of 14 years of age, afterwards called the *porcupine man*, whose whole skin, the head, the palms of the hand, and the soles of the feet excepted, was covered with corneous pegs, which gave the body an appearance as if covered with a coat of mail. He was not born with this cuticular defor-

* This very scarce *editio princeps* seems to be unknown to modern bibliographers best acquainted with the literature of voyages and travels. I obtained my copy through the kindness of Sir Joseph Banks.

† Vol. xxxvii. p. 299.

mity, which first made its appearance seven or eight weeks after birth, at which period the skin became yellow, and gradually continued to grow darker, till at length it became black, and soon after thicker and more corneous.

In his fiftieth year this man, who was now married and a father, exhibited himself in London, together with his son, who had the same deformity of skin. The celebrated Baker, who wrote on the microscope, gave at that time in the *Philosophical Transactions** an appendix to M. Machin's paper; and as the latter had given a representation of the hand of the father, the former gave a figure of that of the son from a drawing, an engraving of which may be seen also in Edwards's *Gleanings of Natural History*, p. 1, tab. 212.

This son afterwards married; and in the month of September 1801 I saw two of his sons perfectly like their father and grandfather, and consequently the third generation of this family so singular on account of this cuticular deformity.

The oldest was twenty-two years of age and married, the younger was fourteen. Both were stout, well made, and of an athletic constitution. The older was a good pugilist like his grandfather, who is said to have excelled in this gymnastic art. His face, the palms of the hands, and the soles of the feet were of the usual appearance, but seemed to me to be uncommonly red. The skin of the remaining parts of the body was covered with corneous excrescences, or pegs of greater or less size, and of a more or less horny nature. The longest, strongest, and hardest, were on the fore arm and thighs; the finest were on some parts of the lower belly. They were in general smaller on the younger brother, and in many places, such as the breast, soft. The largest excrescences were from four to five lines in length, and of an irregular prismatic form, with blunt edges, almost as if pressed flat. The thickest were about three lines in diameter; at the extremities in general split, and many of them diverging like a fork. On the other hand, I scarcely observed one of them of that cylindrical form ascribed to them by Baker, who besides supposed them to be hollow; at least such was the opinion of Haller, who considered this as a confirmation of Boerhaave's opinion in regard to the construction of the epidermis, as he says: "*In hoc puero tota superficies corporis abiit in congeriem tubulorum exstantium, callosorum, subinde renascentium, quod*

* Vol. xlix. part i. p. 21.

certe exemplum quasi de industria ad conformandam præceptoris sententiam factum est." Boerhaave says expressly of the epidermis: "Constat vasorum exhalantium et inhalantium innumerabilium extremis annulis, inter se connatis."

Where the excrescences were longest and thickest, they appeared to me to be like those which I have seen in the elephant under the forehead and above the trunk.

The colour of them in general appeared to be a chesnut or coffee brown. This however was the case at the surface, for in other parts the larger ones were rather yellowish grey.

The hair of the skin appeared sometimes as if grown into the horny substance of these excrescences.

Both the brothers, as well as the father and grandfather, had had the smallpox, in the last stage of which they lost the greater part of their excrescences; but they were soon gradually reproduced. In general they drop off singly from time to time, especially in winter; but new ones gradually grow up. When they are in any manner torn off, the skin which lies under them readily begins to bleed.

The skin on the top of the head before, and especially in the oldest, forms a kind of broad callosity, which has some resemblance to the *tofts* of the camel.

The perspiration of these two brothers exhibits nothing uncommon, no perceptible smell, &c. and during great heat or violent exercise they sweat like other men.

I am acquainted with only two cases which have a real analogy to that of the porcupine men from Suffolk. The one is the boy from Biseglia, of whom Stalp van der Wiel has given a figure and some account, in his *Observations**: the other is a female child, three years of age, at Vienna, whose history and an account of the cure have been published by J. A. von Brambilla†. In both the face was free from these excrescences, but the palms of the hands and the soles of the feet were the most covered with them. An observation made in regard to the boy corresponds exactly with a circumstance related of the porcupine man: "Delapsis venteribus, non illico succedebant squamæ, quibus avulsis mox effluebat sanguis:" and the case is the same with what Brambilla says of the girl: "she was born with a smooth and somewhat yellow skin, but in six weeks it became brown, and in the course of a year black and bristly." The last-mentioned child was freed from its bristly warts

* *Observat. part ii. p. 374.*

† *Abhandlungen der Josephinischen medicinisch-chirurgischen Akad. vol. i. p. 371.*

by the continued use of bathing and mercurials; and we are told by Baker that the first porcupine man twice employed salivation to cleanse his skin; that by these means the excrescences dropped off, and that the skin continued for some time as white and smooth as that of other people; but that soon after the cure it became covered with these horny excrescences as before.

Other instances of singular deformities in the skin are mentioned by Fabricius, Hildanus, Fourcroy, &c., but these are so different from that here alluded to, that they cannot be placed in the same class.

XII. *A Survey and Report of the Coasts and Central Highlands of Scotland; made by the Command of the Right Honourable the Lords Commissioners of His Majesty's Treasury in the Autumn of 1802. By THOMAS TELFORD, Civil Engineer, Edinburgh, F. R. S.*

[Continued from vol. xv, p. 311.]

The Fisheries.

IN what regards the fisheries, I beg leave to quote a passage from my last year's Report. "I believe it is generally admitted, that in the improvement of a country, the interference of government should extend only to the removing obstacles, and affording conveniences which are of a nature not easily to be surmounted by individuals, or any body of men who can be brought to act together; and where it is evident that by removing these obstacles and affording these conveniences, the exertions of individuals will be greatly facilitated, so as to promote the general good of the empire."

The objects connected with the Fisheries, which seem to come under the foregoing description, are, 1st, the want of a ready communication by water between the east and west coasts; 2dly, the want of communications by land from the low countries and the east coast, with the shores and fishing lochs of the west coast; 3dly, the inconveniences arising from the operation of the salt laws; and 4thly, the want of a harbour in Caithness.

The first and second of those objects have already been fully discussed under the heads of the Caledonian Canal, and the proposed Bridges and Roads. The third has been so often and thoroughly investigated, that I shall only in this place take the liberty of mentioning that all the information

I have

I have received tends to confirm the justice of the complaints against the laws now in force which regard salt.

As to the fourth object, the harbour of Wick in Caithness, which I examined, estimated, and reported upon last year, will remove the just complaints of want of protection on the N. E. coast.

The Emigrations.

That emigrations have already taken place from various parts of the Highlands, is a fact upon which there does not remain room to doubt: from the best information I have been able to procure, about three thousand persons went away in the course of last year; and, if I am rightly informed, three times that number are preparing to leave the country in the present year.

I shall not encroach upon your lordships' time by investigating all the remote or unimportant collateral causes of emigration, but shall proceed to that which I consider to be the most powerful in its present operation; and that is, converting large districts of the country into extensive sheepwalks. This not only requires much fewer people to manage the same tract of country, but in general an entirely new people, who have been accustomed to this mode of life, are brought from the southern parts of Scotland.

The difference of rents to the landlords between sheep and black cattle is, I understand, at least three to one, and yet on account of the extraordinary rise in the prices of sheep and wool, the sheep farmers have of late years been acquiring wealth. As the introducing sheep farms over countries heretofore stocked with black cattle creates an extensive demand for the young sheep from the established farms, it is possible that the high prices may continue until a considerable portion of the country is fully stocked: after this takes place, the quantities of sheep produced will bear a very great proportion to the demand, and then it is possible the prices may fall below the average value: in this case it is probable the farms will be subdivided, and a proportion of black cattle and cultivation be introduced in the lower grounds in the valleys, while the upper parts of the hills continue to be pastured with sheep. This I consider as the most improved state of Highland farming, and is consistent with a very considerable population: a beautiful instance of this is to be seen along the north side of Loch Tay. But improved communications, by means of roads and bridges, are necessary for this state of society; and for this reason I have said, that if these conveniences had been

been sooner introduced into the Highlands, it is possible this emigration might not have taken place, at least to the present extent.

The very high price of black cattle has also facilitated the means of emigration, as it has furnished the old farmers with a portion of capital which enables them to transport their families beyond the Atlantic.

In some few cases a greater population than the land can support in any shape, has been the cause of emigrations; such was the island of Tiree.

Some have, no doubt, been deluded by accounts sent back from others gone before them; and many deceived by artful persons, who hesitate not to sacrifice these poor ignorant people to selfish ends.

A very principal reason must also be, that the people, when turned out of their black cattle farms to make way for the sheep farmers, see no mode of employment whereby they can earn a subsistence in their own country; and sooner than seek it in the Low Lands of Scotland, or in England, they will believe what is told them may be done in the farming line in America.

What I have here mentioned appear to me to be the immediate causes of the present emigrations from the north-western parts of Scotland. To point out the means of preventing emigrations in future, is a part of my duty, upon which I enter with no small degree of hesitation. As the evil at present seems to arise chiefly from the conduct of land-owners, in changing the œconomy of their estates, it may be questioned whether government can with justice interfere, or whether any essential benefits are likely to arise from this interference.

In one point of view it may be stated, that, taking the mountainous parts of Scotland as a district of the British empire, it is the interest of the empire that this district be made to produce as much human food as it is capable of doing at the least possible expense; that this may be done by stocking it chiefly with sheep; that it is the interest of the empire the food so produced should not be consumed by persons residing amongst the mountains totally unemployed, but rather in some other parts of the country, where their labour can be made productive either in the business of agriculture, fisheries or manufactures; and that by suffering every person to pursue what appears to them to be their own interest, that although some temporary inconveniences may arise, yet, upon the whole, that matters will in
the

the end adjust themselves into the forms most suitable for the place.

In another point of view it may be stated, that it is a great hardship, if not a great injustice, that the inhabitants of an extensive district should all at once be driven from their native country, to make way for sheep farming, which is likely to be carried to an imprudent extent; that in a few years this excess will be evident; that, before it is discovered, the country will be depopulated, and that race of people which has of late years maintained so honourable a share in the operations of our armies and navies will then be no more: that in a case where such a numerous body of the people are deeply interested, it is the duty of government to consider it as an extraordinary case, and one of those occasions which justifies them in departing a little from the maxims of general policy: that for this purpose regulations should be made to prevent land-owners from lessening the population upon their estates below a given proportion; and that some regulation of this sort would in the end be in favour of the land-owners, as it would preserve the population best suited to the most improved mode of highland farming, such as is practised at Breadalbane, and to the establishment of fishing villages, on the principle laid down and practised so successfully by Mr. Hugh Stevenson of Oban, at Arnisdale on Loch Hourm.

In whatever light the foregoing statements may be viewed, there is another on which there can, I think, be no difference of opinion. This is, that if there are any public works to be executed, which, when completed, will prove generally beneficial to the country, it is advisable these works should be undertaken at the present time. This would furnish employment for the industrious and valuable part of the people in their own country, they would by this means be accustomed to labour, they would acquire some capital, and the foundations would be laid for future employments. If, as I have been credibly informed, the inhabitants are strongly attached to their native country, they would greedily embrace this opportunity of being enabled to remain in it, with the prospect of bettering their condition, because, before the works were completed, it must be evident to every one that the whole face of the country would be changed.

The Caledonian canal, and the bridges and roads before mentioned, are of the description here alluded to: they will not only furnish present employment, but promise to accomplish all the leading objects which can reasonably be
looked

looked forward to for the improvement and future welfare of the country, whether we regard its agriculture, fisheries, or manufactures.

Carlisle to Port Patrick.

The particulars of the report of the district of country which lies between Carlisle, the Solway Frith, and Port Patrick, are in a separate paper attached to this; but the following is an abstract of it:

1. To the expense of a new bridge over the river Esk at Gereston, to enable the city of Carlisle and the royal boroughs in Dumfriesshire to make a new line of road between Carlisle and Annan, by which 5 in 22 miles will be saved; and two brought into one stage	£. 4,500
2. To half the expense of a wet dock at Kirkcudbright, in order to render this harbour, which is the best in the Solway Frith, and the greatest rise of tide in Scotland, fit to admit ships of war, and in order to facilitate the intercourse with Ireland from this frith. The other half of this expense to be raised by the town	2,500
3. To the expense of a lighthouse to be erected on the Little Ross for the benefit of this port, to be lighted and maintained by the town of Kirkcudbright	100
4. To the expense of a bridge over the Dee at this place, provided that the surveyor of the general post-office shall think this the best line for a mail coach, the roads to be made at the expense of the town and county	2,500
5. To a buttress to the North Pier at Port Patrick, a small additional pier upon the Millstone Rock, making a further excavation of the bason, in case it should be deemed proper to improve this harbour, and keep it in repair	2,700
6. To part of the expense of making a harbour at Port Nessock, in order to obtain a more regular communication with Ireland than at Port Patrick, to afford a proper station for revenue cutters, and a place of refuge to vessels driven back from the Mull of Galloway, the remainder of the expense, 2,000l., being to be paid by the proprietor	3,000
7. To the expense of building a lighthouse on the Mull of Galloway, it being to be lighted and maintained by the commerce of the district	100
Carried over	15,400
	Brought

		Brought over	£. 15,400
s.	To a wooden pier or wharf 160 yards long, and 15 feet wide at the Cavin, in Loch Ryan, for embarking and disembarking troops, and for promoting the commerce of this part of the country		1,500
		Total	£. 16,900

London,
March 15, 1803.

THOMAS TELFORD.

General Statement of Funds necessary for the several Works mentioned in the foregoing Report.

Roads and Bridges.	{	For 1st three years public aid, annually	£. 20,000 - 60,000	} £.	192,000
		Do. land-owners, &c.	20,000 - 60,000		
		For 2d three years public aid, annually	12,000 - 36,000		
		Do. land-owners	12,000 - 36,000		
Naval Stations.	{	Aberdeen, five years public aid, annually	5,500 - 27,500	} 55,000	
		Aberdeen, city funds	5,500 - 27,500		
		Cromarty, two years	2,500 - - -		
Caledonian Canal.	{	Seven years public aid, annually	50,000 - - -	}	350,000
Fisheries.	{	Wick, three years public aid, ann.	2,000 - - -	}	6,000
Carlisle to Port Patrick.	{	Three years public aid, annually	5,650 - - -	}	16,950
					£. 624,950

London,
March 15, 1803.

THOMAS TELFORD.

Statement of the Annual and Total Sums to be advanced, beyond the Proportion to be contributed by the Land-owners.

1st Year.	Roads and bridges	-	-	£. 20,000
	Aberdeen	-	-	5,500
	Cromarty	-	-	2,500
	Caledonian canal	-	-	50,000
	Wick	-	-	2,000
	Carlisle to Port Patrick	-	-	5,650
				85,650
				Brought

		Brought over £.	85,650
2d Year.	Roads and bridges - - -	20,000	
	Aberdeen - - - - -	5,500	
	Cromarty - - - - -	2,500	
	Caledonian canal - - -	50,000	
	Wick - - - - -	2,000	
	Carlisle to Port Patrick -	5,650	
		-----	85,650
3d Year.	Roads and bridges - - -	20,000	
	Aberdeen - - - - -	5,500	
	Caledonian canal - - -	50,000	
	Wick - - - - -	2,000	
	Carlisle to Port Patrick -	5,650	
		-----	83,150
4th Year.	Roads and bridges - - -	12,000	
	Aberdeen - - - - -	5,500	
	Caledonian canal - - -	50,000	
		-----	67,500
5th Year.	Roads and bridges - - -	12,000	
	Aberdeen - - - - -	5,500	
	Caledonian canal - - -	50,000	
		-----	67,500
6th Year.	Roads and bridges - - -	12,000	
	Caledonian canal - - -	50,000	
		-----	62,000
7th Year.	Caledonian canal - - -	50,000	50,000
		-----	£. 501,450

London,
March 15, 1803.

THOMAS TELFORD.

[To be concluded in our next.]

XIII. *On the Theory of Combustion.*

To the Editor of the Philosophical Magazine.

SIR, Edinburgh, May 20, 1803.

As you have admitted Mr. Henry's claim in favour of his father's discovery, I trust you will do equal justice to an author, in whose cause I presume, though unauthorized, to appear. In your last number is a communication from Mr. Portal on the subject of combustion: he there mentions the opinion, that heat and light are different bodies, simultaneously separated in this process, without their hav-

ing any peculiar connection. This system he attributes to Dr. Thomson, author of a very excellent article on chemistry, in the Supplement to the Encyclopædia Britannica, and of a separate elementary work on the same subject. You remark in a note that this opinion was before published in Gren's Chemistry.

Long before the appearance of the latter work in England, and I believe before the publication of that shorter and more comprehensive system, which has been translated, and which you quote, the same opinion was published in a work entitled "Essays by a Society of Gentlemen at Exeter;" and those essays, which relate to philosophical subjects, were reprinted in this part of Britain, by what authority I am not informed. In one of these essays the peculiar and distinguishing properties of light and heat are pointed out. The author attempts to show that they are distinct bodies, by a variety of facts, and that, instead of being related, they are apparently antagonizing principles, repelling each other. He traces the principle of light, as it forms an ingredient in many different bodies, not only in chemical combination, but in a looser union, where its separation takes place without decomposition.

If this opinion be then established, that author must have the prior claim; and it is remarkable that, in the Encyclopædia, it is attributed to Dr. Parr of Exeter, though the name is omitted in Dr. Thomson's separate work. But with this conjecture I have no business: my wish is only to state the circumstances.

As I have paid some attention to this subject, I may trouble you with some further remarks on it, should you approve of the communication. I will now only intrude further, by requesting a place for this letter in your next; and am your sincere friend and well wisher,

T. R. O.

XIV. *Observations on the Processes of Tanning.*

By Mr. DAVY.*

I. *On the Preparation of Skin for Tanning.*

IN all the processes for forming leather, the skins are depilated, and freed from flesh and extraneous matter before they are submitted to the action of the tanning lixivium. In some cases, when large skins are employed, a slight

* From the Journals of the Royal Institution of Great Britain.

degree of putrefaction is induced, for the purpose of enabling the hair to be readily separated; but in general this effect is produced by a mixture of lime and water.

The process by putrefaction is so simple as to require no comment: the epidermis is loosened by it, and the cellular substance that constitutes the bulb of the hair softened in such a manner that it may be easily separated from the cutis or true skin.

When lime is employed, it has been generally supposed that it acts by destroying the epidermis, so as to render it soluble in water. This, however, does not appear to be the case: I exposed to two ounces of lime water four grains of epidermis, separated from cow skin, and which had been freed from loose moisture by blotting paper; but, after five days, it appeared rather of larger volume than before; and instead of having lost any weight, I found that it had gained very nearly half a grain.

The epidermis has been supposed to consist of coagulated albumen. In comparing its properties with those of the coagulated white of the egg, there was a striking analogy perceived between them: both were soluble in the caustic alkalis by long exposure, and were acted upon by the acids.

In examining the circumstance of the action of lime water, and of milk of lime, upon skin, I have always observed that the cuticle is rendered extremely loose and friable after this action: from which it is probably that it combines with the lime, so as to form an insoluble compound. This may be observed indeed in washing the hands with lime water: the cuticle becomes extremely rough and dry; whereas, after the action of weak alkaline solutions, which form soluble compounds with it, it is found smooth.

Not only the epidermis, but likewise the soft matter at the extremity of the hair, is acted upon by lime; and this effect must tend considerably to facilitate the process of depilation. Likewise the fat and oily matter adhering to the skin form saponaceous compounds with the earth, and these compounds are removed with other extraneous matter, before the skins are submitted to any new chemical agents.

It has been proposed to use the residuum of the tanning lixivium, or the exhausted ooze, for the purposes of depilation; but this liquor seems to contain no substances capable of acting upon the epidermis, or of loosening the hair; and when skin is depilated by being exposed to it, the effect must really be owing to incipient putrefaction.

Skins, after being depilated and cleansed, are in this country generally subjected to other processes of preparation before they are impregnated with the tanning principle.

The large and thick hides which have undergone incipient putrefaction are introduced for a short time into a strong infusion of bark, when they are said by manufacturers to be *coloured*; and after this they are acted upon by water impregnated with a little sulphuric acid, or acetic acid formed by the fermentation of barley or rye. In this case they become harder and denser than before, and fitted, after being tanned, for the purpose of forming the stouter kinds of sole leather. The acids are capable of combining both with skin and with tannin; and it would appear that, in this process, a triple combination must be effected on the surface of the skin, though from theory one should be disposed to conclude, that the interior part could be little modified in consequence of the colouring, and the action of the acids.

The light skins of cows, the skins of calves, and all smaller skins, are treated in a very different way, being submitted for some days to the action of a lixivium, called the grainer, made by the infusion of pigeons dung in water. After this operation they are found thinner and softer than before, and more proper for producing flexible leather. When the infusion of pigeons dung is examined, after being freshly made, it is found to contain a little carbonate of ammonia; but in a short time it undergoes fermentation, when carbonic acid and hydrocarbonate are given out by it, and a small quantity of acetic acid formed. The alkali in the grainer may probably have some action upon the skin; it may be supposed to free it from any oils or calcareous soap that remained adhering to it: but the great effect probably depends upon the complicated process of fermentation, during which the skin loses its elasticity, and becomes soft; and it is found by tanners, that dung which has undergone fermentation is wholly unfit for their use.

I have tried several experiments on different substances, as substitutes for the pigeons dung used in the grainer, but without gaining successful results. Very weak solutions of carbonate of potash and carbonate of ammonia seemed to soften considerably small pieces of skin that had been depilated by lime; but when they were tried by Mr. Purkis, in the processes of manufacture, the effects were less distinct. In the western counties of England the excrement of dogs

is employed instead of pigeons dung, and culver or the dung of fowls is in common use. The dung of graminivorous quadrupeds enters only slowly into fermentation, and it is not found efficacious in the process.

XV. *Notices respecting New Books.*

Connoissance des Tems, à l'Usage des Astronomes et des Navigateurs pour l'An 13 de l'Ere de la République Française (1805); publiée par le Bureau des Longitudes à Paris. De l'Imprimerie de la République.

THIS work was published for the first time in 1679. Picard, one of the greatest astronomers of the 17th century, and Lefebvre, edited the earlier volumes; Lieutaud began in 1702; Godin in 1730; Miraldi in 1735; Lalande in 1760; Jeurat in 1776; Mechain in 1788: in consequence of the absence of Mechain, Lalande resumed the editorship in 1795, and has continued ever since.

This work is divided into two parts: the first comprehends the calendar, that is to say, every thing relating to observations both at sea and at land; a catalogue of 600 principal stars for the 1st of January 1805, corrected by Michel Lefrançois de Lalande. A geographical table of longitudes and latitudes, corrected and enlarged by Buache, Mechain, and Lalande, according to the last voyages and observations which the French Board of Longitude receives from different countries. The six eclipses which will take place in the above year are carefully calculated, as well as eclipses of the stars,

The second part of the volume contains observations which render this work of more general utility to astronomers. Among the articles are a history and observations of the new planets and the last comets; a new catalogue of the stars hitherto known, and amounting to 13000, extracted from the 50,000 stars observed by Lalande the uncle and nephew; memoirs or observations by Von Zach and Ciccolini, Delambre, Messier, Mechain, Vidal, Flaugergues, Cousin, Lalande uncle and nephew, Burekhardt, Nouet, Chabrol de Muro, Thulis, &c. Also the history of Astronomy for the years 8 and 9, being a continuation of that published by Lalande for the preceding years, since 1782. The most curious articles in this volume are the observations of the new planet discovered by Dr. Olbers, to whom the National Institute has decreed the prize medal founded by Lalande for the best annual work on astronomy.

General Zoology, or Systematic Natural History, by GEORGE SHAW, M. D. F. R. S. &c. with Plates from the first Authorities, and most select Specimens engraved principally by Mr. Heath. Vol. III. Parts 1 and 2. Kearsley, 1802.

Having already noticed the two preceding volumes of this useful work, we shall only observe, that the one now announced seems to be executed with the same care and attention, and that the plates are engraved in the same style of excellence. The two parts of this volume contain amphibia; comprehending tortoises, frogs, toads, lizards, and serpents. As a specimen of the work, we subjoin the following account of the Coluber Naja:

The coluber naja, or cobra de capello, is a native of India, where it appears to be one of the most common, as well as most noxious, of the serpent tribe; very frequently proving fatal, in the space of a few minutes, to those who unfortunately experience its bite. Its remarkable form and colours are such as to distinguish it with great ease from almost every other snake. Its general length seems to be three or four feet, and the diameter of the body about an inch and quarter: the head is rather small than large, and is covered on the fore part with large smooth scales, resembling in this respect the majority of innoxious serpents; the back part, sides, and neck, with smaller ovate scales; and the remainder of the animal, on the upper parts, with small, distinct, oblong-oval scales, not ill resembling the general form of a grain of rice. At a small distance beyond the head is a lateral swelling or dilatation of the skin, which is continued to the distance of about four inches downwards, where the outline gradually sinks into the cylindrical form of the rest of the body. This part is extensible, at the pleasure of the animal; and when viewed from above, while in its most extended state, is of a somewhat cordated form, or wider at the upper than the lower part; it is marked above by a very large and conspicuous patch or spot, greatly resembling the figure of a pair of spectacles; the mark itself being white with black edges, and the middle of each of the rounded parts black. This mark is more or less distinct in different individuals, and also varies occasionally in size and form, and in some is even altogether wanting. The usual colour of the animal is a pale ferruginous brown above; the under parts being of a blueish white, sometimes slightly tinged with pale brown, or yellow: the tail, which is of moderate length, tapers gradually, and terminates in a slender, sharp-pointed extremity.

This

This formidable reptile has obtained its Portuguese title of *cobra de capello*, or *hooded snake*, from the appearance which it presents when viewed in front in an irritated state, or when preparing to bite; at which time it bends the head rather downwards, and seems hooded, as it were, in some degree, by the expanded skin of the neck. In India it is every where exhibited publicly as a show, and is, of course, more universally known in that country than almost any other of the race of reptiles. It is carried about in a covered basket, and so managed by its proprietors as to assume, when exhibited, a kind of dancing motion; raising itself up on its lower part, and alternately moving its head and body from side to side for some minutes, to the sound of some musical instrument which is played during the time. The Indian jugglers, who thus exhibit the animal, first deprive it of its fangs; by which means they are secured from the danger of its bite.

Dr. Russel, in his account of experiments made in India with this serpent, observes, that, as a general standard for a comparison of the effect of its bite with that of other poisonous serpents, he never knew it prove mortal to a dog in less than twenty-seven minutes, and to a chicken in less than half a minute. Thus, fatal as it is, its poison seems not so speedy in operation as that of the rattle-snake, which has been known to kill a dog in the space of two minutes.

In the month of June 1787, a dog bitten by a *cobra de capello* on the inside of the thigh howled at first as if in severe pain: after two or three minutes he lay down, continuing to howl and moan: after twenty minutes he rose, but with much difficulty, being unable to walk, and his whole frame appeared greatly disordered. He soon lay down again, and in a few minutes was seized with convulsions, in which he expired twenty-seven minutes after the bite.

A large and very stout dog was bitten by another *cobra de capello* on the inside of the thigh, which in a minute or two was drawn up; which is, in general, the first symptom of the poison having taken effect. He continued, however, nearly an hour longer, walking on the three remaining legs, seeming not otherwise disordered; but after this time he laid himself along in great inquietude, his head and throat being convulsed in an uncommon degree; he made several vain efforts to rise; his legs became both paralytic, and after continuing in this state near an hour he expired.

Nov. 11, a large dog was bitten by a *cobra de capello* which had been captive only two days. He complained a good deal at the instant of the bite, and the leg was drawn

up soon. In twenty-five minutes he was seized with convulsions, succeeded by stupor, in which state he lay for ten minutes: the convulsions, however, returned, and he expired in a quarter of an hour; being fifty-six minutes after the bite.

Aug. 9, a cobra de capello which had lost his two longest fangs, but retained two of the second order, was made to bite a very large stout dog. At first the dog complained loudly, though without drawing up the thigh, or showing any other symptom of poison: but, happening at this time to break loose, he was pursued, and, after a chase of an hour and a half, was brought back, much fatigued and heated. After resting a quarter of an hour, water was offered to him, which was refused, though he ate some morsels of bread thrown into it. About a quarter of an hour afterwards he became much disturbed, grew entirely outrageous, howling violently, snapping at and gnawing the stake to which he was tied with incredible ferocity. This continued about three hours, when, growing faint, his howlings grew weaker, his convulsions increased, and he expired in about four hours after the bite.

A pig bitten by a snake of this kind, which had been kept for more than six weeks, and fed only once in seven days with milk, became greatly disordered in twenty minutes, and expired in less than an hour.

A chicken bitten by a cobra de capello has been sometimes known to survive two hours.

Aug. 17, 1788, an attempt was made to make a cobra de capello bite another (of the variety called *nooni paragoode*) in the tail; but that part being found too small, the belly was bitten, a little above the vent. The bitten snake soon lost its former activity, and, when put under a glass, coiled itself up. In this state it was left, and after an hour and a quarter was found dead. On opening the belly, the parts immediately beneath the bite appeared much inflamed, though it could not be discovered whether the fangs had penetrated into the cavity.

A cobra de capello received by Dr. Russel from Ganjam, under the name of *sattunag*, was made to bite another remarkably large cobra brought from the same place under the name of *couthiah*. The poison was shed on the place, but no marks of fangs could be perceived, and the *couthiah* remained as well as before: this experiment was repeated with the same result, though a little blood as well as poison was found on the part bitten.

Some days after this, a cobra de capello (of the variety called

called *codum nago*) was made to bite the *coultiah* on the belly: both fangs visibly acted; blood appeared on the wound, but no other consequence followed. A *tar tutta* bitten immediately after in the same manner, died within two hours.

Chickens and pigeons bitten by a cobra de capello, whose fangs had been eradicated, suffered no symptoms of poison; but when poison taken from the same snake was inserted into their bodies, either by incision or puncture, they suffered the usual symptoms, and very often died.

Mr. JAMESON, author of the Mineralogy of the Scottish Isles, is about to put to the press a work entitled "A Natural History of Fossils, according to the System of the celebrated Professor Werner, of Freyberg." As Mr. Jameson studied two years under this illustrious naturalist, he hopes to be able to present to the public a work free from the errors and imperfections of Wiedemann, Emmerling, and Brochaut.

XVI. Proceedings of Learned Societies.

ROYAL SOCIETY OF LONDON.

THE following is a short account of experiments on the absorption of different gases by water, at different temperatures, and under different pressures, made by Mr. William Henry, and communicated to the Society. The processes were carried on by means of instruments invented by the author; plates and descriptions of which are annexed to the account of the experiments.

Mr. Cavendish, in pursuing his first experiments in pneumatic chemistry, ascertained many of the circumstances of the absorption of carbonic acid gas by water; and Dr. Priestley had noticed the relations of this absorption to pressure: more lately, the manufacture of artificial gaseous waters has called the attention of many chemists to this part of the subject; but the power of combination of other aëriform fluids with water, and the manner in which this power is modified by different causes, have been but little studied.

Mr. Henry, in the first part of the detail of his experiments, states, that the quantity of carbonic acid gas absorbed by water, is influenced materially by the quantity of common air, which may be either combined with the water, or
mixed

mixed with the gas; and the effect is in some measure proportional to the quantity of the residue, the absorption being always greatest when a large quantity of gas was agitated with a comparatively small quantity of water.

From various trials made with great care, Mr. Henry concludes, that, in judging of the influence of temperature, the experiments should be made on equal proportions of gas, and of water, and that in this case, with regard to carbonic acid gas, 1-fourteenth of the whole bulk, absorbable at 55°, is the diminution of the quantity of absorption produced by each elevation of 10° of Fahrenheit.

Of sulphuretted hydrogen gas, 100 parts of water, at 55°, absorb 86 parts, and at 85° 78 parts.

Of nitrous oxide gas, 100 cubic inches of water, at 55°, take up 50, and at 70° only 44.

The experiments on those gases which are absorbed only in small proportions by water the author could not conveniently make at more than one temperature.

He found that at 60°, 100 cubic inches of water absorb, of nitrous gas, 5 cubic inches; of oxygen, 2.63; of phosphuretted hydrogen, 2.14; of gaseous oxide of carbon, 2.01; of carburetted hydrogen gas, 1.40; of nitrogen gas, 1.20; of hydrogen gas, 1.08.

Mr. Henry mentions that during the absorption of large quantities of carbonic acid, sulphuretted hydrogen and nitrous oxide by water, an increase of temperature of about 3-fourths of a degree is perceived.

From the results of a great variety of experiments made in the more absorbable gases, and on oxygen and nitrogen gases, the author draws the following general conclusion with regard to pressure:

That under the same circumstance of temperature, water takes up the same volume of gas, whether it be condensed, or under ordinary pressure; but, as the spaces occupied by gases are inversely as the weights compressing them, it follows, that water takes up of gas condensed, by one, two, or three additional atmospheres, a quantity which is equal to twice, thrice, or four times the quantity taken up under the ordinary pressure. D.

GALVANIC SOCIETY, PARIS.

On the 25th of May the Galvanic Society made, for the second time, some experiments on a large scale at the veterinary school of Alfort.

Animals of all sizes, from the insect to the horse, were subjected to an apparatus composed of more than 2000 disks,

disks, formed into piles which communicated with each other. Besides other results, the following were observed :

1st, That a spark cannot be obtained, as in common electricity, at an explosive distance, but at the point of contact.

2d, That Coulomb's electrometric balance does not indicate electric tension in the ratio of the number of pairs of disks, or of the strength of the pile.

3d, That the power of a very formidable apparatus is required to kill a small animal.

4th, That Galvanism, applied after death, can determine movements of inspiration and expiration. If a lighted taper be placed near a small aperture made in the trachea, the flame at the moment when the Galvanism is applied is drawn into the breast by the impulse of the air passing into the lungs, and extinguished when the air issues.

5th, That contractions can be produced in the head and forehead of a large animal—a horse, for example—by placing it at a very great distance, and making it communicate with the pile by one conductor, while the other is formed by the common réservoir*.

XVII. *Intelligence and Miscellaneous Articles.*

A DEFENCE AGAINST FIRE.

PROFESSOR PALMER, of Hamburgh, has lately discovered a means by which all inflammable matters, such as wood, paper, linen, &c., can not only be secured from burning, but also be speedily extinguished when on fire. These means consist in a powder composed of one ounce of sulphur, one ounce red ochre, and six ounces of copperas water. To render wood incombustible, it is first daubed over with cabinet-maker's glue, after which the powder is strewed over it: and this operation, when the wood becomes dry, is three or four times repeated. When the powder is to be applied to linen or paper, plain water is employed in the room of glue: in other respects the process is the same, with this difference alone, that the operation is performed only once or twice. When the powder is used for articles already on fire, two ounces are sufficient to extinguish a square foot of surface. Professor Palmer intends to publish

* This sentence is ambiguous. We have translated it literally, however, from the French Journal in which it appeared.—EDIT.

a full account of his invention, and of the different methods of using it, and to show how at the time of large conflagrations it may be employed to most advantage to save the lives of men, and valuable articles.

A trial of this powder was made at Wolfenbuttel on the 11th of December, and it fully answered the expectation which had been formed of it.

SPONTANEOUS DECOMPOSITION OF A FABRIC OF SILK*.

On the night of March 19, 1802, during the session of congress at Washington, Jonathan Dayton, one of the senators then attending from the state of New Jersey, sustained a loss of a pair of black silk stockings in an uncommon manner. On undressing himself at bed-time, his stockings were the last of his garments which he took off. The weather being cold, he wore two pair, the inner of wool and the outer of silk. When he stripped off the silk stockings, he let them drop on a woollen carpet lying by the bed-side; and one of his garters, which was of white woollen ferretin, fell down with the stockings. The under-stockings, on being pulled off, were thrown at some distance, near the foot of the bed. He observed, on separating and removing the silk stockings from the woollen ones, that there was an unusual snapping and sparkling of electric matter. But as he had been long acquainted with the appearance, it attracted but transient notice.

He fell asleep, and remained undisturbed until morning, when the servant entered to kindle the fire. The man observed that one of the leather slippers, lying on the carpet, and partly covered by one of the stockings, was very much burnt. Mr. Dayton then rose, and found that the leather over which the stockings had lain was converted to a coal. The stockings were changed to a brown, or what is commonly called a butternut colour. And although, to the eye, the stitches of the legs, and even the threads of their clocks, appeared to be firm and entire; yet, as soon as an attempt was made to touch and handle them, they were found to be wholly destitute of cohesion, their texture and structure being altogether destroyed. Nothing but a remnant of carbonic matter was left, except that a part of the heel of one of the stockings was not decomposed.

Though this destruction of the stockings took place during the night, when nobody saw the manner and circumstances of the process, yet there was evidence enough of

* From the American Medical Repository, vol. v.

the evolution of much caloric while it was going on; for every thing in contact with the stockings was turned to coal or cinder. Beside the slipper before mentioned, the garter was burned. It had fallen partly on the carpet, and partly on and between the stockings. As far as it touched the stockings it was perfectly disorganized and carbonated, and immediately beyond that limit was as sound as ever. The part of the carpet, with its fringe, which lay between the stockings and the floor, was in like manner totally destroyed, just as far as it was covered by the stockings, and no further. The wooden plank, which was of pitch-pine, was also considerably scorched; and beneath the place where the thickest folds of the stockings had lain was converted to charcoal or lamp-black to a considerable depth. In throwing down the stockings when they were pulled off, it happened that about a third part of the length of one of them fell not upon the carpet, but upon the bare floor. This part of the stocking was decomposed like the rest, and the floor very much scorched where it had lain.

There was very little fire on the hearth, and the little there was was eight or nine feet distant. The candle had been carefully extinguished, and stood on a table in another direction, and about equally distant. Indeed, no application of burning coals or of lighted candles could have produced the effects which have been described. It would seem that the combustion, if it may be so called, proceeded from a surcharge of caloric, or electricity, in the silk, accumulated by means not well understood; and that, not being referable to any known external agent, it may, [in the present state of our information, be termed spontaneous.

The substances chiefly consumed were leather, wool, silk, and resinous wood. The linen lining of the slipper was indeed destroyed as far as the leather it touched was destroyed. But where it did not come in contact, it escaped, and the fire showed no disposition to burn even the linen beyond the boundaries prescribed to it on the leather.

What is the theory of this phenomenon? With what other facts is it immediately connected? Whatever men of science may determine on these points, one thing seems to be evident, that if spontaneous combustion can happen thus to bodies so little inflammable as leather, silk, and wool, that instances of its occurrence in bodies easier to burn are more frequent than is generally supposed.

ANTIQUITIES.

A number of curious remains of antiquity arrived at Portsmouth in one of the transports from Egypt; they are the property of the earl of Cavan, and were put on board a vessel to be conveyed to his lordship's seat at Fawley: among them are the following:—A case containing mummies of an antient Egyptian family, viz. a male, female, and two children: the male measures five feet nine inches in height; and as the upper half of the body had been stript of the linen swathes, the flesh, the nails of the fingers, and even the features, can be seen very distinctly: the arms are bent upwards, crossing each other on the breast, the fingers of the right hand touching the left shoulder, and the left hand clenched as if holding something. The female measures five feet six inches in height, and the infant children about twenty-two inches. Mummies of an ichneumon, a dog, two hawks, two owls, and six ibises, some of them in covered urns of red earthen ware; another complete mummy, with the external case beautifully painted with hieroglyphics; a bust of Isis; a large frog in gray granite; a large slab of whitish granite, with hieroglyphics cut in bas relief; a broken sarcophagus in black granite, and many antique fragments of marble porphyries, jaspers, agates, and masses of the various rocks of Upper Egypt, which will be highly interesting to the mineralogist, as well as amusing to the antiquarian; a perfect sarcophagus of red granite; its inside dimensions are six feet six inches long, two feet four inches wide, and one foot six inches deep: a large column of red porphyry; also, a bowl of red granite, its outside dimensions near six feet; it is cut out of the base of a Corinthian column; the mouldings are very perfect, and the whole height of the column must have been about 54 feet.

MEASUREMENT OF A DEGREE OF THE MERIDIAN.

Astronomers long suspected that there was an error in the measurement of a degree of the earth, made in Lapland in 1736 by Maupertuis, Lemonnier, Outhier, and Celsius; because that degree was greater than it ought to be according to all the other measurements. M. Melanderhiolm has found means to repeat this measurement. He informs me in a letter that M. Svenberg and three other astronomers have found the degree to be 57209 toises in lat. $66^{\circ} 20'$, which makes 196 toises less than by the measurement of the French astronomers, and gives for the flattening of the earth a 313th part. This agrees better with the other comparisons, and shows us that the figure of the earth is not so irregular as was supposed from the northern degree.

M. Mechain

M. Mechain set out on the 26th of April for Spain, where he intends to measure a triangle of 93,000 toises, terminating at the Balearian islands, and which will complete the grand and important measurement of the meridian which Mechain and Delambre made a few years ago. He is accompanied by M. Chevalier, Dezauche, and Mechain jun. This measurement will be very difficult: but no one is more capable than M. Mechain to overcome difficulties; and we shall have the 45th degree in the middle of the whole arc measured by the French. DE LALANDE.

HUMBOLDT'S TRAVELS.

The report some time ago circulated of the death of this celebrated traveller has been contradicted by some of the Berlin journals, which state, that letters have been lately received from him by some of his friends at Paris. These letters, which are dated at Lima, announce that he proposed to return to Europe about the month of September next.

NEW METAL.

Proust has discovered that the substance which he considered as a new metal, and to which he had given the name of *selene*, is nothing else than uranium.

ASTRONOMY.

Table of the geocentric motions of the two new planets for the month of July:

	A. R. of Pallas.			Declin. North.	A. R. Ceres Ferd.			Declin. South.
July 2	18 ^h	21 ^m	37 ^s	23° 1'	18 ^h	42 ^m	33 ^s	25° 22'
5	18	19	7	22 50	18	39	38	28 34
8	18	16	43	22 36	18	36	46	28 45
11	18	14	20	22 21	18	32	54	28 55
14	18	12	5	22 2	18	31	8	29 4
17	18	9	54	21 43	18	28	33	29 12
20	18	7	52	21 21	18	24	55	29 20
23	18	5	57	20 56	18	23	32	29 27
26	18	4	11	20 30	18	21	18	29 34
29	18	2	36	20 4	18	19	17	29 39

PHILOSOPHICAL LECTURES.

The rev. D. F. PRYCE, A. M. curate of Bathwick, has been for some time engaged in preparing a philosophical apparatus, on a very extensive scale, under the inspection of Messrs. W. and S. Jones, Holborn, on which he designs to read two or more courses of popular lectures annually in the city of Bath. The apparatus is in such a state of forward-
ness,

ness, as will enable Mr. Pryce to commence early in the ensuing winter. He is also in treaty for the erection of a scientific theatre, capable of holding about five hundred persons, on a plan similar to that at the Royal Institution.

Dr. PEARSON proposes, in addition to his summer course of lectures on physic and chemistry, to give the following on the cow-pock inoculation, at the Institution, (founded Dec. 1799, late No. 5, Golden-square,) No. 44, Broad-street, Golden-square. A lecture to be given at the Institution once or twice a week, according to the subject of it, for about twelve weeks.

The principal objects will be: 1. The history of what is known of the vaccina in cows. 2. The history of the discovery, introduction, and propagation of vaccine inoculation. 3. To show, in patients at the Institution, the progress of the inoculated cow-pock, through its stages of growing into a vesicle, constitutional disorder, scabbing process, deceduary carbuncle-like scab, with a view especially to make known the distinguishing characters of the vaccina. 4. To explain the unusual or accidental symptoms and effects of the vaccina: viz. eruptions, phlegimonous inflammation, erythema, axillary swellings, *essera vaccina*, pustule, ulcerations of inoculated parts, &c. 5. To explain the anomalous eruption of inoculated parts. 6. To explain the anomalous course of the inoculated vaccina. 7. Intervening disorders, especially the small-pox, measles, chicken-pox, contagious angina, hooping cough, tooth rash, red gum, &c. 8. Instances, by inoculation, of the small-pox and cow-pock at the same time in the same person. 9. The various modes of preserving vaccine matter. 10. The effects of various modes of inoculation. 11. The effects of matter at different ages of the vaccine-pock. 12. The effects of inoculation of persons who have undergone the small-pox or cow-pock. 13. The effects of inoculation when it fails to destroy the susceptibility of the small-pox. 14. The medical treatment and regimen during the cow-pock. 15. The effects on health subsequently to inoculation. 16. The vaccine inoculation instead of the small-pox, as vicarious of a disease in sheep.

PROPOSALS.—1. Subscribers for life, viz. of ten guineas, to the Vaccine Institution to be admitted gratuitously; as well as 2. Perpetual pupils to Dr. Pearson's lectures in general; and his 3. Other pupils on becoming perpetual, in addition to their present lectures, to be admitted on the same terms. 4. Those who are neither subscribers to the Institution, nor are pupils, as just mentioned, are to pay three guineas for a single course, or six guineas as perpetual.

XVIII. *On the Modifications of Clouds, and on the Principles of their Production, Suspension, and Destruction; being the Substance of an Essay read before the Askesian Society in the Session 1802-3.* By LUKE HOWARD, Esq.*

SINCE the increased attention which has been given to meteorology, the study of the various appearances of water suspended in the atmosphere is become an interesting and even necessary branch of that pursuit.

If clouds were the mere result of the condensation of vapour in the masses of atmosphere which they occupy, if their variations were produced by the movements of the atmosphere alone, then indeed might the study of them be deemed an useless pursuit of shadows, an attempt to describe forms which, being the sport of winds, must be ever varying, and therefore not to be defined.

But however the erroneous admission of this opinion may have operated to prevent attention to them, the case is not so with clouds. They are subject to certain distinct modifications, produced by the general causes which effect all the variations of the atmosphere: they are commonly as good visible indications of the operation of these causes as is the countenance of the state of a person's mind or body.

It is the frequent observation of the countenance of the sky, and of its connection with the present and ensuing phenomena, that constitutes the antient and popular meteorology. The want of this branch of knowledge renders the predictions of the philosopher (who in attending only to his instruments may be said only to examine the pulse of the atmosphere) less generally successful than those of the weather-wise mariner or husbandman.

With the latter, the dependence of their labours on the state of the atmosphere, and the direction of its currents, creates a necessity of frequent observation, which in its turn produces experience.

But as this experience is usually consigned only to the memory of the possessor, in a confused mass of simple aphorisms, the skill resulting from it is in a manner incommunicable; for, however valuable these links when in connection with the rest of the chain, they often serve, when taken singly, only to mislead; and the power of connecting them,

* Communicated by the Author.

in order to form a judgment upon occasion, resides only in the mind before which their relations have passed, though perhaps imperceptibly, in review. In order to enable the meteorologist to apply the key of analysis to the experience of others, as well as to record his own with brevity and precision, it may perhaps be allowable to introduce a methodical nomenclature, applicable to the various forms of suspended water, or, in other words, to the modifications of cloud.

By modification is to be understood simply the structure or manner of aggregation, not the precise form or magnitude, which indeed varies every moment in most clouds. The principal modifications are commonly as distinguishable from each other as a tree from a hill, or the latter from a lake; although clouds in the same modification, considered with respect to each other, have often only the common resemblances which exist among trees, hills, or lakes, taken generally.

The nomenclature is drawn from the Latin. The reasons for having recourse to a dead language for terms to be adopted by the learned of different nations are obvious. If it should be asked why the Greek was not preferred, after the example of chemistry, the author answers, that the objects being to be defined by visible characters, as in natural history, it was desirable that the terms adopted should at once convey the idea of these, and render a recourse to definitions needless to such as understand the literal sense, which many more would, it is concluded, in Latin than in Greek words.

There are three simple and distinct modifications, in any one of which the aggregate of minute drops called a cloud may be formed, increase to its greatest extent, and finally decrease and disappear.

But the same aggregate which has been formed in one modification, upon a change in the attendant circumstances, may pass into another.

Or it may continue a considerable time in an intermediate state, partaking of the characters of two modifications; and it may also disappear in this stage, or return to the first modification.

Lastly, aggregates separately formed in different modifications may unite and pass into one exhibiting different characters in different parts, or a portion of a simple aggregate may pass into another modification without separating from the remainder of the mass.

Hence, together with the simple, it becomes necessary to
admit

admit intermediate and compound modifications, and to impose names on such of them as are worthy of notice.

The simple modifications are thus named and defined :

1. *CIRRUS*. *Def.* Nubes cirrata, tenuissima, quæ undique crescat.

Parallel, flexuous, or diverging fibres, extensible in any or in all directions.

2. *CUMULUS*. *Def.* Nubes cumulata, densa, sursum crescens.

Convex or conical heaps, increasing upward from a horizontal base.

3. *STRATUS*. *Def.* Nubes strata, aquæ modo expansa, deorsum crescens.

A widely extended, continuous, horizontal sheet, increasing from below.

This application of the Latin word *stratus* is a little forced. But the substantive, *stratum*, did not agree in its termination with the other two, and is besides already used in a different sense even on this subject, e. g. *a stratum of clouds*; yet it was desirable to keep the derivation from the verb *sterno*, as its significations agree so well with the circumstances of this cloud.

The intermediate modifications which require to be noticed are :

4. *CIRRO-CUMULUS*. *Def.* Nubeculæ densiores subrotundæ et quasi in agmine appositæ.

Small, well defined roundish masses, in close horizontal arrangement.

5. *CIRRO-STRATUS*. *Def.* Nubes extenuata sub-concava vel undulata. Nubeculæ hujus modi appositæ.

Horizontal or slightly inclined masses, attenuated towards a part or the whole of their circumference, bent downward, or undulated, separate, or in groups consisting of small clouds having these characters.

The compound modifications are :

6. *CUMULO-STRATUS*. *Def.* Nubes densa, basim planam undique supercrescens, vel cujus moles longinqua videtur partim plana partim cumulata.

The cirro-stratus blended with the cumulus, and either appearing intermixed with the heaps of the latter or superadding a wide-spread structure to its base.

7. *CUMULO-CIRRO-STRATUS* vel *NIMBUS*. *Def.* Nubes vel nubium congeries pluviam effundens.

The rain cloud. A cloud or system of clouds from which rain is falling. It is a horizontal sheet, above which the

cirrus spreads, while the cumulus enters it laterally and from beneath.

Of the Cirrus.

Clouds in this modification appear to have the least density, the greatest elevation, and the greatest variety of extent and direction. They are the earliest appearance after serene weather. They are first indicated by a few threads pencilled, as it were, on the sky. These increase in length, and new ones are in the mean time added laterally. Often the first-formed threads serve as stems to support numerous branches, which in their turn give rise to others.

The increase is sometimes perfectly indeterminate, at others it has a very decided direction. Thus the first few threads being once formed, the remainder shall be propagated either in one, two, or more directions laterally, or obliquely upward or downward, the direction being often the same in a great number of clouds visible at the same time: for the oblique descending tufts shall appear to converge towards a point in the horizon, and the long straight streaks to meet in opposite points therein; which is the optical effect of parallel extension.

Their duration is uncertain, varying from a few minutes after the first appearance to an extent of many hours. It is long when they appear alone and at great heights, and shorter when they are formed lower and in the vicinity of other clouds.

This modification, although in appearance almost motionless, is intimately connected with the variable motions of the atmosphere. Considering that clouds of this kind have long been deemed a prognostic of wind, it is extraordinary that the nature of this connection should not have been more studied, as the knowledge of it might have been productive of useful results.

In fair weather, with light variable breezes, the sky is seldom quite clear of small groups of the oblique cirrus, which frequently come on from the leeward, and the direction of their increase is to windward. Continued wet weather is attended with horizontal sheets of this cloud, which subside quickly and pass to the cirro-stratus.

Before storms they appear lower and denser, and usually in the quarter opposite to that from which the storm arises. Steady high winds are also preceded and attended by streaks running quite across the sky in the direction they blow in.

The relations of this modification with the state of the
barometer,

barometer, thermometer, hygrometer, and electrometer, have not yet been attended to.

Of the Cumulus.

Clouds in this modification are commonly of the most dense structure: they are formed in the lower atmosphere, and move along with the current which is next the earth.

A small irregular spot first appears, and is, as it were, the nucleus on which they increase. The lower surface continues irregularly plane, while the upper rises into conical or hemispherical heaps; which may afterwards continue long nearly of the same bulk, or rapidly rise to mountains.

In the former case they are usually numerous and near together, in the latter few and distant; but whether there are few or many, their bases always lie nearly in one horizontal plane, and their increase upward is somewhat proportionate to the extent of base, and nearly alike in many that appear at once.

Their appearance, increase, and disappearance, in fair weather, are often periodical, and keep pace with the temperature of the day. Thus they will begin to form some hours after sun-rise, arrive at their maximum in the hottest part of the afternoon, then go on diminishing and totally disperse about sun-set.

But in changeable weather they partake of the vicissitudes of the atmosphere; sometimes evaporating almost as soon as formed, at others suddenly forming and as quickly passing to the compound modifications.

The cumulus of fair weather has a moderate elevation and extent, and a well defined rounded surface. Previous to rain it increases more rapidly, appears lower in the atmosphere, and with its surface full of loose fieces or protuberances.

The formation of large cumuli to leeward in a strong wind, indicates the approach of a calm with rain. When they do not disappear or subside about sun-set, but continue to rise, thunder is to be expected in the night.

Independently of the beauty and magnificence it adds to the face of nature, the cumulus serves to screen the earth from the direct rays of the sun, by its multiplied reflections to diffuse, and, as it were, economise the light, and also to convey the product of evaporation to a distance from the place of its origin. The relations of the cumulus with the state of the barometer, &c. have not yet been enough attended to.

Of the Stratus.

This modification has a mean degree of density.

It is the lowest of clouds, since its inferior surface commonly rests on the earth or water.

Contrary to the last, which may be considered as belonging to the day, this is properly the cloud of night; the time of its first appearance being about sun-set. It comprehends all those creeping mists which in calm evening ascend in spreading sheets (like an inundation of water) from the bottom of valleys and the surface of lakes, rivers, &c.

Its duration is frequently through the night.

On the return of the sun the level surface of this cloud begins to put on the appearance of cumulus, the whole at the same time separating from the ground. The continuity is next destroyed, and the cloud ascends and evaporates, or passes off with the appearance of the nascent cumulus.

This has been long experienced as a prognostic of fair weather*, and indeed there is none more serene than that which is ushered in by it. The relation of the stratus to the state of the atmosphere as indicated by the barometer, &c. appears notwithstanding to have passed hitherto without due attention.

Of the Cirro-cumulus.

The cirrus having continued for some time increasing or stationary, usually passes either to the cirro-cumulus or the cirro-stratus, at the same time descending to a lower station in the atmosphere.

The cirro-cumulus is formed from a cirrus, or from a number of small separate cirri, by the fibres collapsing as it were, and passing into small roundish masses, in which the texture of the cirrus is no longer discernible, although they still retain somewhat of the same relative arrangement. This change takes place either throughout the whole mass at once, or progressively from one extremity to the other. In either case, the same effect is produced on a number of adjacent cirri at the same time and in the same order. It appears in some instances to be accelerated by the approach of other clouds.

This modification forms a very beautiful sky, sometimes exhibiting numerous distinct beds of these small connected clouds, floating at different altitudes.

* At nebulæ magis ima petunt, campoque recumbunt.

Virgil. Georg. lib. i.
The

The cirro-cumulus is frequent in summer, and is attendant on warm and dry weather. It is also occasionally and more sparingly seen in the intervals of showers, and in winter*. It may either evaporate, or pass to the cirrus or cirro-stratus.

Of the Cirro-stratus.

This cloud appears to result from the subsidence of the fibres of the cirrus to a horizontal position, at the same time that they approach towards each other laterally. The form and relative position, when seen in the distance, frequently give the idea of shoals of fish. Yet in this, as in other instances, the structure must be attended to rather than the form, which varies much, presenting at other times the appearance of parallel bars, interwoven streaks like the grain of polished wood, &c. It is always thickest in the middle, or at one extremity, and extenuated towards the edge. The distinct appearance of a cirrus does not always precede the production of this and the last modification.

The cirro-stratus precedes wind and rain, the near or distant approach of which may sometimes be estimated from its greater or less abundance and permanence. It is almost always to be seen in the intervals of storms. Sometimes this and the cirro-cumulus appear together in the sky, and even alternate with each other in the same cloud, when the different evolutions which ensue are a curious spectacle, and a judgment may be formed of the weather likely to ensue by observing which modification prevails at last. The cirro-stratus is the modification which most frequently and completely exhibits the phenomena of the solar and lunar halo, and (as supposed from a few observations) the parhelion and paraselene also. Hence the reason of the prognostic for foul weather, commonly drawn from the appearance of halo †.

This

* The following passage is beautifully descriptive of the appearance of this modification by moonlight:

For yet above these wafled clouds are seen
(In a remoter sky, still more serene)
Others, detached in ranges through the air,
Spotless as snow, and countless as they're fair;
Scatter'd immensely wide from east to west,
The beauteous semblance of a flock at rest.
These to the raptur'd mind aloud proclaim
Their mighty shepherd's everlasting name.

Bloomfield's Farmer's Boy, Winter.

† The frequent appearance of halo in this cloud may be attributed to its possessing great extent, at such times, with little perpendicular depth.

This modification is on this account more peculiarly worthy of investigation. Little is yet ascertained of the relations of this and the last modification with the barometer, &c. although, as may be readily supposed, they have been found to accord with opposite indications of those instruments.

Of the Cumulo-stratus.

The different modifications which have been just treated of sometimes give place to each other, at other times two or more appear in the same sky; but in this case the clouds in the same modification lie mostly in the same plane of elevation, those which are more elevated appearing through the intervals of the lower, or the latter showing dark against the lighter ones above them. When the cumulus increases rapidly, a cirro-stratus is frequently seen to form around its summit, reposing thereon as on a mountain, while the former cloud continues discernible in some degree through it. This state continues but a short time. The cirro-stratus speedily becomes denser and spreads, while the superior part of the cumulus extends itself and passes into it, the base continuing as before, and the convex protuberances changing their position till they present themselves laterally and downward. More rarely the cumulus alone performs this evolution, and its superior part constitutes the incumbent cirro-stratus.

In either case a large lofty dense cloud is formed, which may be compared to a mushroom with a very thick short stem. But when a whole sky is crowded with this modification, the appearances are more indistinct. The cumulus rises through the interstices of the superior clouds, and the whole, seen as it passes off in the distant horizon, presents to the fancy mountains covered with snow, intersected with darker ridges and lakes of water, rocks and towers, &c. The distinct cumulo-stratus is formed in the interval between the first appearance of the fleecy cumulus and the

and that degree of continuity of substance which seems requisite to the phenomenon. There is also probably some additional peculiarity of structure in it not yet attended to.

The following lines of Virgil seem to relate to an effect of the cirro-stratus, which in this country is more often to be observed on the setting sun:

Ille ubi nascentem maculis variaverit ortum
 Conditus in nubem, medioque refugerit orbe,
 Suspecti tibi sint imbres: namque urget ab alto
 Arboribusque satisque Notus, pecorique sinister.

Georgic. lib. i.

commencement

commencement of rain, while the lower atmosphere is yet too dry; also during the approach of thunder storms: the indistinct appearance of it is chiefly in the longer or shorter intervals of showers of rain, snow, or hail.

The cumulo-stratus chiefly affects a mean state of the atmosphere as to pressure and temperature; but in this respect, like the other modifications, it affords much room for future observation.

Of the Nimbus, or Cumulo-cirro-stratus.

Clouds in any one of the preceding modifications, at the same degree of elevation, or in two or more of them, at different elevations, may increase so as completely to obscure the sky, and at times put on an appearance of density which to the inexperienced observer indicates the speedy commencement of rain. It is nevertheless extremely probable, as well from attentive observation as from a consideration of the several modes of their production, that the clouds while in any one of these states do not at any time let fall rain.

Before this effect takes place they have been uniformly found to undergo a change, attended with appearances sufficiently remarkable to constitute a distinct modification. These appearances, when the rain happens over our heads, are but imperfectly seen. We can then only observe, before the arrival of the denser and lower clouds, or through their interstices, that there exists *at a greater altitude* a thin light veil, or at least a hazy turbidness. When this has considerably increased we see the lower clouds spread themselves till they unite in all points and form one uniform sheet. The rain then commences, and the lower clouds, arriving from the windward, move under this sheet and are successively lost in it. When the latter cease to arrive, or when the sheet breaks, every one's experience teaches him to expect an abatement or cessation of rain.

But there often follows, what seems hitherto to have been unnoticed, an immediate and great addition to the quantity of cloud. At the same time the actual *obscurity* is lessened, because the arrangement, which now returns, gives freer passage to the rays of light: for on the cessation of rain the lower broken clouds which remain rise into cumuli, and the superior sheet puts on the various forms of the cirro-stratus, sometimes passing to the cirro-cumulus.

If the interval be long before the next shower, the cumulo-stratus usually makes its appearance, which it also does sometimes very suddenly after the first cessation.

But

But we see the nature of this process more perfectly in viewing a distant shower in profile.

If the cumulus be the only cloud present at such a time, we may observe its superior part to become tufted with nascent cirri. Several adjacent clouds also approach and unite laterally by subsidence.

The cirri increase, extending themselves upward and laterally, after which the shower is seen to commence. At other times the converse takes place of what has been described relative to the cessation of rain. The cirro-stratus is previously formed above the cumulus, and their sudden union is attended with the production of cirri and rain.

In either case the cirri *vegetate*, as it were, in proportion to the quantity of rain falling, and give the cloud a character by which it is easily known at great distances, and to which, in the language of meteorology, we may appropriate the nimbus of the Latins*.

When one of these arrives hastily *with the wind* it brings but little rain, and frequently some hail or driven snow.

In heavy showers, the central sheet once formed, is, as it were, warped to windward, the cirri being propagated above and against the lower current, while the cumuli arriving with the latter are successively *brought to* and contribute to reinforce it.

Such are the phænomena of showers. In continued gentle rains it does not appear necessary for the resolution of the clouds that the different modifications should come into actual contact.

It is sufficient that there exist two strata of clouds, one passing beneath the other, and each continually tending to horizontal uniform diffusion. It will rain during this state of the two strata, although they should be separated by an interval of many hundred feet in elevation. See an instance in De Luc, *Idées sur la Météorologie*, tom. ii. p. 52, &c.

As the masses of cloud are always blended and their arrangement destroyed before rain comes on, so the reappearance of these is the signal for its cessation. The thin sheets of cloud which pass over during a wet day, certainly receive from the humid atmosphere a supply proportionate to their consumption, while the latter prevents their increase in bulk. Hence a seeming paradox, which yet accords strictly with observation, that for any given hour of a wet day, or any given day of a wet season, *the more cloud the less rains*

* *Qualis ubi ad terras abrupto sydere nimbus
It mare per medium, miseris heu prescia longe
Horrescunt corda agricolis.*

Hence also arise some further reflections on the purpose answered by clouds in the œconomy of nature. Since rain may be produced by, and continue to fall from, the slightest obscuration of the sky by the nimbus (that is, by *two sheets* in different states), while the cumulus or cumulo-stratus, with the most dark and threatening aspect, shall pass over without letting fall a drop, until their change of state commences; it should seem that the latter are reservoirs in which the water is collected from a large space of atmosphere for occasional and local irrigation in dry seasons, and by means of which it is also arrested at times in its descent in the midst of wet ones*. In which so evident provision for the sustenance of all animal and vegetable life, as well as for the success of mankind in that pursuit so essential to their welfare, in temperate climates, of cultivating the earth, we may discover the wisdom and goodness of the creator and preserver of all things †.

The nimbus, although in itself one of the least beautiful clouds, is yet now and then superbly decorated with its attendant the rainbow; which can only be seen in perfection when backed by the widely extended uniform gloom of this modification ‡.

The relations of rain, and of periodical showers more especially, with the varying temperature, density, and electricity of the atmosphere, will probably now obtain a fuller investigation, and with a better prospect of success, than heretofore.

[To be continued; when Plates of the different Modifications will be given.]

XIX. *Researches respecting the Organization of Leaves.*

By A. JURINE, Member of the Society of Physics and Natural History of Geneva.

[Continued from p. 15.]

HAVING described the organization of the surface of leaves such as I observed it, I shall proceed to their interior organization, omitting at present the different vessels found there, and which will be described in the second part of this memoir.

The authors who speak of the parenchyme describe it in so many different ways that it seems difficult to form a just

* Nulla dies adeo est Australibus humida nimbis

Non intermissis ut fluat imber aquis:

† See on this subject Job, chap. xxxvii and xxxviii.

‡ *Biba ingens æra*, says Virgil, in commemorating the propitiousness of continued rain.

idea of it. Saussure expresses himself as follows:—"In observing the cortical reticulation I have often had occasion to study another reticulation which is placed immediately below the former; it is the parenchymatous reticulation. This reticulation has larger and straighter vessels, and its meshes are generally greater than those of the cortical reticulation. The vessels of the parenchyme are besides almost always coloured, and for the most part green. They are very rarely cylindric; in general they grow narrower and larger in succession, so that they resemble vessels contiguous to each other."

Hedwig says * that he has seen in the leaves of moss small ducts, disposed according to the length of the leaf, which anastomose laterally with other transverse or lateral ducts in such a manner as to form areolæ sometimes square and sometimes oblong, pentagonal, or hexagonal, which almost all contain a parenchyme, the form of which is globular, and which gives to leaves their colour.

Senebier gives his opinion in the following terms †:—The appellations of cellular tissue, cellular covering, and parenchyme, are given indifferently to that reticulation formed by transparent fibres or vessels filled with a green juice, which are anastomosed at the places where they meet, and swelled up in the intervals. Utriculi will in all probability be found there, though nothing is seen with the best glasses but the meshes of a reticulation. If we conceive some parts of vegetables composed of fibres, forming meshes with a granulated substance, we shall have some idea of the matter which constitutes the greater part of leaves and fruits. The parenchyme I am about to describe will be found in that in particular which fills up the meshes of the greater part of reticulations.

C. Mirbel considers the parenchyme of leaves as a cellular tissue formed of cells which are filled with a juice almost always coloured green. It does not consist, he says, of small bags or utriculi; it is a membrane which unlines itself in some measure to form vacuities contiguous to each other.

In all the leaves which I have examined I have always found that their parenchyme was composed only of an aggregation of utriculi closely united to each other, filled with a green juice by which they are coloured, and of which the form varies according to the different plants. For exam-

* Musci Frondosi, pars i. p. 24.

† Physiologie Végétale, tom. i. p. 161.

ple, they are nearly spherical in the *fritillaria*, elongated or cylindrical in the *lavatera triloba* and the *erythronium dens leonis*, prismatic in the *aloe*, and irregular in the *sylphium perfoliatum*.

The utriculi of the parenchyme of the same leaf are seen to vary also in regard to their form and size. Thus the leaves of the *sylphium* and the *impatiens balsamina* have the utriculi irregular on the inferior side and cylindric on the superior. Those of the *nenuphar* have them small and elongated on the upper side, and large and prismatic on the lower. Those of the *narcissus* and others of the lily kind have them round and small on both sides, but large and prismatic in the interior part of the leaf.

From what has been said, if we suppose the parenchyme of a leaf to be composed of spherical utriculi adhering to each other, it will readily be conceived that, as these utriculi cannot touch each other throughout their whole surface, the necessary result will be vacuities or intervals between them, which will have a communication with each other, as seen fig. 15. If the utriculi are irregular, as those of fig. 18, the vacuities in this case will be larger; and if the utriculi approach the cylindric or prismatic form, the vacuities will be the less sensible as the utriculi touch each other in a greater extent of their surface.

It is of importance to comprehend properly the aggregation of the utriculi; the vacuities resulting from it, which in future I shall call *utricular interstices*; and the communication of these utriculi with the pores, in order to account for the circulation of the air in the leaves.

That air exists in leaves is a truth fully confirmed; for, by compressing them in water, the air is seen to issue from them. By exposing them under water to the action of an air-pump they emit air; and if they be left in the water for some time they soon lose their opacity, become transparent by the introduction of water, which assumes the place of the air, and are precipitated to the bottom of the vessel. In a word, these leaves when again exposed to the air gradually resume their opacity and their natural colour; which can be ascribed only to the air which re-enters as the water is evaporated. But where is this air lodged? I shall reply from my observations, that it is contained in the *utricular interstices* of the parenchyme.

If a leaf of the *fritillaria* or the *portulaca* be examined with the naked eye, or, still better, with a magnifying glass, there will be observed in its interior part small luminous points, which are produced by the air contained in the *utricular*

cular interstices, and which reflect the light. To assure myself of it, I removed the pellicle of a leaf of the *fritillaria*, and then cut a piece of parenchyme, in which I observed several of these luminous points. I then placed it in the focus of the microscope on a drop of water: by the mere reflection of the light I distinguished very well these points: but when I observed them transparently they appeared to me opake; at which I was not surprised, for I had before remarked that globules of air seen in this manner always assumed that appearance. I gently compressed the parenchyme to force out the air, which I saw issue in the form of bubbles, and the water having then assumed its place rendered the interstices transparent from being opake.

I have endeavoured to represent the effect of these luminous points in fig. 1; but I have succeeded only imperfectly, since it was necessary to place at the surface of the drawing the luminous points which ought to be in the parenchyme.

If the petal of a rose for example, the irregularity of the parenchymatous utriculi of which give rise to large interstices, be slightly compressed under water, the air it contains will be seen to circulate with rapidity, following the different inflections of the interstices.

I have already said that the pores communicate with the utricular interstices. This will be proved by the following experiment.

I subjected to the action of an air-pump the leaves of the *geranium peltatum* and of the *rumex sanguineus*, which I placed in a vessel filled with water. On the first stroke of the piston the two surfaces of the leaf were covered with a dew arising from small air bubbles; I then ceased to pump; and re-establishing the communication between the exterior air and the receiver, I examined with attention these bubbles, which I saw decrease and entirely disappear: whence I conclude that this air had returned into the utricular interstices by the same way that it issued, that is to say, by the pores. To convince myself of this, I repeated the same experiment on the leaves of the *olea fragrans*, the upper surface of which is destitute of pores; and on the first stroke of the piston the inferior surface alone was covered with bubbles, which re-entered into the leaf in the same manner.

C. Mirbel reproaches Malpighi with having admitted utriculi in the formation of vegetables: he denies the existence of them, and asserts, that instead of utriculi there is only a cellular tissue, more or less elongated, composed of one piece, the cells of which are formed by the cellular
membrane

membrane unlining itself. By adopting this idea I do not know how we can explain the existence of utricular interstices, and assign a place to the air, since the parenchyme of the leaves will be only a continued whole formed of cells the sides of which are common.

The utriculi are filled with a particular fluid which I call the *utricular juice*. In leaves exposed to the action of light the utriculi assume a green tint, which seems to depend in an essential manner on small green globules which abound in the juice, as seen fig. 15, where the utriculi of the frutillaria are represented. These coloured globules are found also in the utriculi of the stems; and I have seen them very distinctly in the *cucurbita* and *tropæolum*, where they are less numerous than in the leaves, but larger and more apparent.

In the greater part of thick and fat leaves, the most interior utriculi, which are not coloured, seem to be destitute of these globules, or at least if they exist they are not sensible.

In the utriculi of the bulb of the lily kind, and in those of the root of the potatoe, these globules are very large, often angular, but transparent and colourless. It is probable that they constitute the farinaceous part of these roots.

I imagined for a long time that these globules were disseminated in the utricular juice; but by a more attentive examination I found that they were applied to the membrane of the utriculus without adhering to it, since, on tearing some of the utriculi in water, I saw the globules disperse themselves in the liquid.

It is to these globules alone that Hedwig applied the denomination of parenchyme, as I have already mentioned.

These globules engaged also the attention of Saussure; but he saw them only as small brilliant points, and the researches he made to discover their intimate nature and uses proved to be fruitless.

This author detached from an asparagus-leaf a fragment of its bark; and having viewed it with a microscope in the light of the sun, either by reflection or transparently, he observed a multitude of small brilliant points nearly circular, surrounded by an opaque circle and almost contiguous: the first idea he conceived was, that the bark of the asparagus was perhaps pierced with a multitude of small circular holes which afforded a free passage to the sun's rays; but having found a great quantity of these brilliant points in the parenchyme and cortical reticulation, he concluded that they did

did not belong to the epidermis. The result of his observations showed that these brilliant points were neither holes nor the orifices of vessels, for they were not altered by desiccation and maceration. "As they are not," says he, "resinous or gummy moleculæ, since they are unalterable in water and spirit of wine, what can they be?"

It appears to me that the formation of these globules is not owing to an inspissation of the utricular juice, and that the green colour which they assume by exposure to the light is only a modification depending on the effects of that fluid.

Besides the globules here mentioned, there are found in the utriculi of some plants singular organs with the uses of which I am unacquainted. These organs, represented fig. 15, A, and fig. 16, are small, prismatic, smooth, and transparent filaments of equal length, terminating in a point on each side, and united in a bundle to the number of forty or fifty, and even more.

In a leaf of the fritillaria which had been macerated for some time, these bundles are distinguished by the naked eye through the utriculi, like small, elongated, whitish, argenteous bodies, disposed in the direction of the length of the leaf.

I found them in the bulb of the *leucojum vernum*, of the *scilla bifolia* and *maritima*, in the stem of the *phytolaca decandra*, and in its leaves, where they are very apparent, especially when the pellicle has been removed from the inferior surface.

In the aloe, besides these bundles there are seen a great number of other prismatic filaments similar to the preceding, but insulated, larger, and lodged, as appeared to me, between the utricular interstices.

I saw also in the stem of the nenuphar other insulated filaments lodged also between the utriculæ, but cylindric, shagreened on the outside, and from the middle of which there proceed in general two or three ramifications, fig. 17.

Having accidentally touched my face with my hands while I was examining the aloe, I soon experienced a violent itching, which I ascribed to the entrance of these prisms into my skin. To ascertain this fact, I rubbed the back of my hand with a piece of a leaf of that plant; which occasioned a strong smarting pain followed by a cutaneous eruption. I repeated this experiment with the parenchyme of the *scilla maritima*, and found the same result.

If the leaves of the *narcissus*, *hyacinth*, *amaryllis formosissima*, and *scilla bifolia*, be cut transversely, there will be
seen

seen to issue from all the proper vessels a viscous and transparent juice, which contains a multitude of these prismatic filaments, which give to this juice an argentine colour.

Rafin, in his *Physiology of Plants*, says he found small prisms in the milky juice of the euphorbia. I found them also, but only in small number.

What are these prismatic filaments? How are they formed? Why are they found in some plants and not in others? In the last place, What is the use of them? These are questions which it is impossible for me to answer, and respecting which I have no data. I shall now proceed to the communication of the utriculi with each other.

Senebier says that "the utriculi form a kind of vessels composed of vesicles bound together. They have a pretty exact resemblance to a flexible tube, slightly choked at distances nearly equal, and nevertheless retaining a free communication throughout the whole length of the canal."

C. Mirbel discovered that "the membranous sides of the cellular tissue are in general perforated with pores, the apertures of which are certainly not the hundredth part of a line; that these pores are bordered with small, unequal, and glandulous rolls, which intercept and strongly refract the light when they receive its rays; that they establish a communication between one cell and another, and serve for the transfusion of the juices, which in this tissue is exceedingly slow.

To ascertain whether there was a direct and sensible communication between the utriculi, I employed various means. I cut a very thin piece of the parenchyme of the leaf of the fritillaria, which I chose in preference on account of its spherical utriculi, which are frequently united by a prolongation in the form of a neck, fig. 15, and which on that account have a greater resemblance to the choked tubes mentioned by Senebier. I placed it in the focus of the microscope; and having observed several complete utriculi, I compressed one of them slightly with a very fine needle, presuming that the liquid it contained would pass immediately into those adjacent to it, and which were open: but the pressure I continued to exercise on it made it burst, and the juice it contained was instantly dispersed. I repeated this operation several times, following very attentively the impulse of its utricular juice that I might observe its passage into the neighbouring utriculi, which, in my opinion, would have taken place had a free communication existed between them.

I repeated this experiment on other leaves the utriculi

of which were filled with a red juice, and always with as little effect.

As I was not able to succeed by these means, I had recourse to injection. I immersed the fresh cut extremity of different leaves, for twenty-four hours, in a decoction of Brasil wood: I then observed them, and found that the liquid had not penetrated the parenchyme beyond the cut surface.

I again immersed several fragments of leaves in the same decoction: I dissected them with attention, and observed that the colouring liquor had penetrated beyond the sections into the utricular interstices, that the membranes of the utriculi had been a little covered by them, but that the utricular juice had retained its natural colour.

I substituted for this decoction ink and a solution of the acetite of lead, which I precipitated by sulphate of potash, without obtaining from this process a more satisfactory result. I then placed in a decoction of Brasil wood, under the air pump, large fragments of the leaves of the aloe and of the fritillaria, and of the leaves of the mesembryanthemum and cactus. I exhausted the receiver; and taking the leaves from the liquid, I observed that in the leaves of the aloe the injection had penetrated at the sections only to the depth of an inch; that the leaves of the fritillaria were almost entirely penetrated by the injection, which in colouring it had given it that transparency which is observed in leaves that have remained under water; that in the cactus and the mesembryanthemum the injection had not penetrated beyond the cut part.

Though these injections had succeeded better than the preceding, the result however was the same, since I observed that the coloration of the leaves depended only on the liquid introduced into the utricular interstices.

If we admit that there is a free communication between one utriculus and another, how can we explain the different colours by which the streaked leaves are shaded? In the spotted orchis, for example, the spots are produced by utriculi which contain a red juice, while those found between the ribs have a green juice. In the red cabbage the juice of the exterior utriculi is violet, and that of the interior ones is transparent.

These observations induce me to believe that the utricular juice does not circulate in the utriculi, but that it is rather stagnant in them. How, indeed, can we suppose that the globules found in so great abundance in this juice, and the prisms met with in most plants, should pass from one utri-

culus to another, when we are not acquainted with the apertures proper for affording them a passage? It cannot, however, be doubted that the utriculi receive, by some way or other, the juice destined for them, since they are full of it, since they repair the daily loss of it which they sustain by evaporation, and since they are susceptible of great development. But what is the method employed by nature for this purpose? I confess that I do not know, having never been able to discover it, notwithstanding the perseverance of my researches.

C. Mirbel, having observed that the membranes of his cellular tissue were perforated with pores, established by their means the communication of the cells between each other, and the slow transfusion of the juice into that tissue.

A discovery of so much importance was worthy of attracting my notice; I therefore paid particular attention to it. I first examined the utriculi of the sugar-cane, and I indeed found that their membrane seemed to be perforated with a great number of small pores; but, decreasing or shading the light by placing my hand before the reflecting mirror, these pores appeared to me to be nothing else than elevated, whitish, and opaque points, which reflected the light; and this doubtless would not have been the case had they been apertures.

I examined these pores in the stems of the asparagus and the horse-tail, in consequence of what had been announced by C. Mirbel; but they are much less sensible in these plants than in the sugar-cane. I perceived them very distinctly in the utriculi of the stem of the white poppy, fig. 20, and in those of the pith of the elder, where they are large, elongated, and disposed in the direction of the breadth of the stem. Though these points, on the first view, have the appearance of apertures, there is reason to believe that they are only prominent semi-transparent points, since I have seen the shadow of them change its place according as I varied the light by moving the reflecting mirror.

These points appeared to me to form part of, and to belong to, the utricular membrane; for I could not detach them without tearing that membrane.

Though my opinion in regard to these pores be different from that of C. Mirbel, I do not pretend to give it as certain; for I must confess that the inspection of objects so small may be accompanied with some optical illusion. I shall however add, that I could not discover these pores in the utriculi of the parenchyme of the leaves; and I must remark, as a cause of error very easy to be committed in

examining these parts, the globules above mentioned, which are found affixed to the utricular membrane, but which can very readily be detached.

I do not consider the existence of these pores as certainly proved. In my opinion, these organs still require to be examined by naturalists; and if they find means to prove their existence, a considerable step will be made in the system of vegetation in regard to the circulation of the juices.

The leaves at their birth are composed of utriculi so small that they cannot be distinguished even with the help of a microscope; but in proportion as these leaves grow, the utriculi expand, and are distended by the addition of the juices with which they are penetrated.

I have several times followed the rapid development of the utriculi in the young leaves of the lily kind of plants, where it is very remarkable; and I have been astonished in particular, in examining the nenuphar, to see the utriculi of the umbilical cord of the seed, which are scarcely sensible at the time of flowering, develop themselves so much between that period and the time when the seed attains to maturity, that this cord is capable of surrounding it, and even of supplying it with a double covering. It must not, however, be believed that this development of the utriculi is without bounds: nature has nearly fixed its limits; so that after a certain term it sometimes happens, especially in leaves where the vegetation is strong, that the utriculi become spontaneously torn, as has been very justly observed by C. Mirbel.

I examined with great care in some plants of the lily kind the formation of the spontaneous lacerations, and of the vacuities thence arising, to which this author gives the name of *lacunæ*, and the following is the result of my observations:

These lacerations are effected in the parenchyme of the leaves according to the direction of the vessels, so that in a transverse section of the leaf they appear as irregular holes separated from each other by bundles of tracheæ: in observing these holes with the microscope, rags of the torn utriculi are very well distinguished, so that it may be asserted that in these leaves the lacunæ are only accidental.

The young leaves of the *scilla bifolia* have no lacunæ when they issue from the bulb: in proportion as they grow up the lacerations are produced; and when they attain to their natural size the lacunæ form longitudinal canals, the diameter of which gradually decreases as they approach the bulb, and which at length entirely disappear: in the place where

where the lacunæ terminate there are then observed large transparent reticulæ of a very loose texture, which become lacerated in their turn when the leaf acquires more increase. I observed the same disposition in the leaves of the narcissus, the hyacinth, and of leeks.

Were we to apply this system of lacerations and lacunæ to all stems and leaves which have canals in their interior part, it would, in my opinion, be giving it too much extent.

Since there are several which issue from the root with these canals, and the smooth close sides of which never exhibit any laceration of the utriculi, I therefore entertain no doubt that the canals found in some aquatic plants, such as the nenuphar, the mereophyllum, &c. are essential to their organization, and that they depend on a particular and predetermined arrangement of the utriculi.

Explanation of the Figures.

(See Plates I and II.)

Fig. 1. This figure represents a piece of a leaf of the fritillaria seen through the microscope. The side A is in its natural state, and shows the exterior utriculi, the form of which is a parallelopipedon elongated in the direction of the length of the leaf. Between these utriculi are remarked some of those small spherical bodies formed by the conjugate utriculi which constitute the pores.

The side B, deprived of the pellicle, which is transported to fig. 2, shows the parenchyme of the leaf of a green colour, much more intense than it appeared in A through the pellicle. This pellicle, which is formed merely by the upper face of the exterior utriculi, could not be removed but by the laceration of these utriculi: their lateral faces, which have remained adhering to the conjugate utriculi with which they are intimately united, are therefore seen on the parenchyme.

The bright points seen here and there dispersed on the surface of the figure, represent small brilliant points observed in the parenchyme, and which are produced by the air contained in the utricular interstices.

Fig. 2. This figure, as already said, is the pellicle removed from the surface B, fig. 1. The upper surface of the exterior utriculi of which it is formed exhibits slight undulations, produced, in my opinion, by its exposure to the air. The oval holes with which it is perforated correspond to the conjugate utriculi which have remained on the

parenchyme; and show that it is the part of these utriculi which concurs to the formation of the surface of the leaf.

Fig. 3. This figure represents a thin slice of the leaf, which has been cut perpendicularly in the direction of its length. The lateral faces of the exterior utriculi are seen lengthwise, united to the utriculi of the parenchyme by their lower face. The conjugate utriculi are seen in the direction of their height and large diameter: they are not united to the parenchyme.

Fig. 4. This figure exhibits also a very thin slice of the leaf, but cut across or in the direction of the breadth: for this reason the exterior utriculi appear in their small diameter as well as the conjugate utriculi, the union of which is shown by a line of separation.

The pellicle A, which has been detached from the exterior utriculi by laceration, shows that it is formed only by their superior face: it however sometimes happens that the conjugate utriculi remain adhering to it.

Fig. 5. This figure represents nearly the same object as fig. 1, but much larger in its dimensions. It is chiefly destined, as well as the following, to give a precise idea of the conformation of the conjugate utriculi, and of their connection with the exterior utriculi. The exterior utriculi are disengaged from the parenchyme to show more distinctly how the lateral faces of these utriculi, seen shortened, may have been taken for vessels: slight wrinkles, or undulations, as observed in fig. 1, are remarked on their upper face.

This figure shows also the part of the conjugate utriculi which is not covered by the exterior utriculi, and which thereby concurs to the formation of the surface of the leaf.

Fig. 6. This figure represents a slice cut vertically in the direction of the length of the leaf, with a certain inclination. The exterior utriculi are seen at the same time on their exterior face, and in the direction of their height; which evidently shows that what was taken for vessels is nothing else than the lateral or vertical faces of these utriculi. It is seen also how the exterior utriculi are applied to, and cover the conjugate utriculi.

Fig. 7. The section represented in this figure is the same as that in fig. 4; it has been magnified, to exhibit all its particulars. It may be considered the same also as the preceding, if we suppose it to be inverted to exhibit it in profile.

The superior face of the exterior utriculi is slightly convex:

vex : it exhibits a very sensible thickness, which is not found in the other faces.

The conjugate utriculi are seen in the direction of their height and of their small diameter. They are united by their extremities, and as they are seen shortened they appear under the form of two spherical bodies applied to each other : they are not united to the utriculi of the parenchyme, and therefore a considerable vacuity, which establishes a communication between the pore and the utricular interstices, is found below them.

Fig. 8. This figure is a repetition of fig. 3. It exhibits a very thin slice cut vertically in the direction of the length of the leaf, but it is seen under a scale of proportion much larger.

The conjugate utriculi which are seen in the direction of their height and large diameter mutually conceal each other ; so that only one of these utriculi is seen on its lateral face, covered by that of the exterior utriculus to which it is united.

Fig. 9. This figure represents the pellicle of maize, and exhibits an exception in regard to the pores which belong to the family of the gramineous plants. No pores indeed are seen ; and the conjugate utriculi, which are found lodged in a kind of square area produced by the disposition of the exterior utriculi, instead of being reniform as in the other plants, are cylindric, and applied to each other by their cylindric faces, in such a manner as to conceal every appearance of pores. At their extremity is seen a small circle, which is closed, as far as I could ascertain, by the juice contained in these utriculi.

The exterior utriculi, the form of which is a very elongated parallelopipedon, have their large sides festooned, and their small sides straight or rectilinear.

Fig. 10. This figure shows more distinctly than fig. 5 how the lateral faces of the exterior utriculi exhibit themselves under the appearance of vessels which form a kind of reticulation, the meshes of which are here hexagonal in consequence of the hexagonal prismatic form of the utriculi.

Fig. 11 and 12. The conjugate utriculi represented in these two figures evidently differ from those seen in fig. 9, though found in the same plant, either in the leaves which surround the ear, or in the interior face of the sheath of the leaves. In fig. 11 the pore begins to appear, because the utriculi become a little reniform ; and in fig. 12, where they are entirely so, the pore is very evident.

Fig. 13. This figure shows the pellicle of the leaf of the aloe,

aloe, which is composed of hexagonal utriculi, between which appear several square apertures. These apertures, similar to those of the fritillaria, fig. 2, correspond to as many conjugate utriculi which have remained on the parenchyme. The square form which these apertures constantly affect, seems to depend on that of the exterior utriculi.

Fig. 14. This pellicle belongs to the leaf of the digitalis purpurea: it consists of utriculi very much festooned, between which are seen the conjugate utriculi, the pores of which seem to be obliterated by a black matter, which is nothing else than air.

The surface of this pellicle is furnished with some conical hairs, very strong, and composed of several rings: they proceed from the middle of the utriculus, as if they were a prolongation of it.

Fig. 15. This figure represents the utriculi of the parenchyme of the leaf of the fritillaria: their form is nearly spherical; they are united to each other by a sort of prolongation in the manner of a neck, and separated in the rest of their extent by pretty considerable vacancies, which have a communication with each other. To these vacancies I have given the name of *utricular interstices*. Each of these utriculi is filled with a viscous juice, in which is found a great number of small green globules applied to the membrane of the utriculus: the upper ones appear more distinct, and the lower ones fainter.

The utriculus A is remarkable on account of the bundle of small prisms contained in its inside.

In fig. 16 these prisms are seen insulated and larger.

In fig. 17 is represented a different kind of these organs found in the nenuphar. Its exterior side is covered with points, and two ramifications proceed from its middle.

Fig. 18. On account of the singular form affected by the utriculi which compose the parenchyme of a great number of leaves, they have been called *irregular utriculi*. From their bodies proceed several arms in different directions, which unite with similar prolongations of the other utriculi; so that the parenchyme which they form becomes very lax, in consequence of the large interstices which they constitute.

These utriculi were taken from the parenchyme of the nectary of the narcissus.

Fig. 19. This figure exhibits the pellicle of a petal the utriculi of which rise above the surface in the form of conical papillæ, which produces a kind of dull velvety appearance, remarked in most petals,

Fig. 20. This figure represents a longitudinal section of the stem of the white poppy, the utriculus of which exhibited in a very striking manner the points considered by C. Mirbel as the pores of his cellular tissue: their form is elongated, and I saw them placed constantly in the direction of the breadth of the utriculus or of the stem.

XX. *Communication from Dr. THORNTON relative to the Pneumatic Practice.*

July 20, 1803.

MR. JOHN GREY, merchant, living at Newport in the Isle of Wight, was attacked by spasmodic asthma fourteen years ago, and has had paroxysms of this dreadful disorder, returning at first at uncertain intervals, until the year 1797, when these fits of difficult respiration recurred usually once, sometimes twice a week, leaving him in the intervals very languid and dispirited. The smallest exertion was a pain to him, and, to use his own expressions, life a burthen. Various physicians had been applied to without any essential benefit, and he despaired of ever getting free from this disease, when he was advised to come to London to be under my care. Upon his arrival in town, tonic medicines, such as he had taken before, were ordered, and he commenced with daily inspiration of four quarts of oxygen or vital air mixed with thirteen of atmospheric air. The immediate effect was a diminution of the violence of the paroxysms, which before lasted from two to three days; the expectoration was earlier, and the fits subsided sooner. Continuing this plan for a few weeks, the paroxysms of asthma wholly disappeared, and Mr. Grey was recommended to return home, and take down with him an eighteen gallon cask, with a tin pneumatic apparatus for inhaling the same; which arrived safe by the Newport waggon, the air being found to have lost none of its peculiar properties; as re-kindling a match when blown out, and burning iron like wood. Another cask was sent down, medicines discontinued, and for six months Mr. Grey continued perfectly free from asthma, at the expiration of which time he had a slight return; but resuming again the oxygen air sent for from town, he was again re-established in health. For five years Mr. Grey has continued free from asthma, except in the month of August, when he is slightly indisposed; but having always had recourse to the inhalation of the oxygen air, sent to him in an eighteen gallon cask, the disorder has been

been prevented forming, and he has continued free from asthma during the rest of the year.

Observation.—It is surprising that a remedy which is so safe, pleasant, and efficacious, and which recommends itself so strongly by innumerable facts, and the analogy of ordering sick people into the country, should not have made more progress in the medical world, especially in cases of asthma, which is so materially affected by changes in the atmosphere. At this time I shall only add another case.

Miss ——— was recommended to my care by the late Mr. Cruikshank for a humour she had been afflicted with upwards of twenty years. It was not an ordinary case, for the tumefaction of the legs was such as to make the leg the thickness of one's thigh; and so great was the discharge from this part, and the whole body, that the servant has been known to come in the morning with the mop, to mop under the bed, the whole of the under part of the bed being wet through. For more than two years this amiable lady daily inhaled the vital air, and the benefit was progressive until a complete cure was accomplished. This was more than five years ago, and the patient has since continued in perfect health.

At another opportunity I will trouble the readers of your Magazine with other facts of the same sort.

XXI. *An Essay on the Fecula of Green Plants.* By Professor PROUST*.

H. ROUELLE was the first who discovered in fecula a substance analogous to the gluten of farina. This substance since that time appeared problematical, only because few chemists tried to ascertain its real characters. The fecula of which it is the basis is, according to Fourcroy, either a supposed substance, or a substance too little examined to be placed among the number of the immediate products of vegetables. He even goes so far as to suppose that albumen, an animal substance which no one before him had supposed to exist in plants, is that which ought to be substituted for the glutinous part of green fecula.

Are albumen and fecula, then, found together or separate in the juice of plants? Such is the question I have proposed to myself, and which I shall endeavour to resolve in

* From the *Journal de Physique*, Pluviose, an 11.

the course of my observations on the *System of Chemical Knowledge*.

To save the reader from the trouble of recurring to that work, I shall copy the passage where the author collects the facts and arguments, in consequence of which he thinks himself authorized to entertain this opinion. This passage is very remarkable, by the opposition discovered in it between the manner in which he and the modern chemists characterize other vegetable products.

“Rouelle junior, who examined and carefully compared it with other animal matters, asserts, that he found it in coloured fecula, and particularly in that called the green fecula of plants. But the expression of fecula, given indifferently to the fibrous matter contained in the juice of plants and to starch, having induced chemists to consider the latter as a part of the remains of solid vegetable substances, there is reason to think that it was merely by analogy, and in consequence of some ambiguous properties, that Rouelle was of opinion that the green matter contained gluten. At least the experiments made since that period, and those which I repeated several times on these coloured feculæ, did not confirm this assertion; and nothing has really proved that gluten is one of the principles of the latter fecula.”

“The expression fecula,” says Fourcroy, “given *indifferently* to the fibrous matter contained in the juice of plants and to starch, having induced an opinion that the latter is a part of the remains of solid vegetable substances, there is reason to believe,” &c.

I shall first observe that this opinion is not correct. For example, chemists at present will never agree with Fourcroy, that the confusion which has been so justly ascribed to the improper use of words has induced those who preceded us to adopt these ideas. Our masters, it must be allowed, gave to things bad denominations; but they did not *confound* them more than we do.

At times even when every vegetable deposit was considered by them as fecula, the resemblance of names never deceived them so far as to make them compare starch to the remains of the solid parts of plants. In the first place, we are acquainted with no remains of that kind to which chemists could with reason compare it; and in the second, if any of them took the green fecula for a residuum, there was not one of them who was not perfectly acquainted with the whole difference between these feculæ or remains and starch: and since no confusion of this kind is found in their works, it is not just to reproach them with it;

for

for we need only cast our eyes on those of Rouelle, Macquer, Baumé, Sage, Parmentier, &c., to be convinced that the word fecula has not led these authors into comparisons so unworthy of their judgment as to arrange in the same class green fecula, the remains of solid parts, and starch.

We shall now proceed to green feculæ, and shall observe that in laboratories, in apothecaries' shops, and still less in the hands of a chemist so celebrated for exactness as Rouelle, the chopped straw of green plants has never been confounded with that beautiful liquid velvet expressed from their leaves, or with the emulsive product which passes in complete freshness through the cloth, and which by its excessive fineness, and the splendour of its colour, is so superior to their herbaceous filaments.

If it were true that the fecula is a body homogeneous with the rest of the plant, if it were possible to consider it only as a part which differs in no other respect than that of having been better pounded, would not complete trituration of the remainder be sufficient to convert it also into fecula? When a fresh herb is pounded, the pestle breaks and bruises its tissue, but does not pulverize it.

And this contusion of a few moments is too far from resembling dry pulverization to admit of any comparison between its fecula and moistened powder. If an aqueous plant, such as sedum for example, be bruised on a piece of marble by means of a roller, its expressed juice will give fecula. It is not to trituration that fecula is indebted for its velvety appearance, its fineness, and impalpability, which distinguish it from powder: it is molecular by its nature, and perhaps even crystallized in the fibrous meshes where vegetation deposits it.

"Rouelle asserts," says Fouréroy, "that feculæ contain a principle which may be compared to animal matters," &c. Rouelle has done more: very little disposed to be satisfied with simple assertions, he proved it not by *analogies* and *ambiguous properties*, but by a series of convincing facts, by comparisons which obtained general assent only because they united the greatest characters then known, and which are yet known, in animal substances. Otherwise whence could Rouelle deduce analogies to compare, as he does, green fecula to the gluten of wheat? What is there common in the appearance of these two products to serve as a basis for comparison? To find points of comparison, it would be necessary to examine in an intimate manner their composition and chemical properties; and this is what this laborious chemist does. Comparisons of this kind deduced from analysis served as a basis for the memoir which
he

he gave on green feculæ, and of which no mention is made in The System of Chemical Knowledge; no doubt because, according to the ideas of this illustrious author, Rouelle had confounded albumen with gluten, and the detail of his mistake must have appeared to him a matter of indifference to the history of chemistry.

Rouelle, however, found in the fecula of sorrel a product so strongly possessed of the chemical properties of albumen, that he particularly insisted on it, to call the attention of chemists to a substance so animalized; and as he afterwards extracted it from a plant which, according to Fourcroy, does not furnish the slightest vestige of albumen, it is now certain, as it was then, that Rouelle was the first who found in green juices and feculæ a *product* which, if it ought not to be distinguished by the name of *albumen*, has, however, all its properties in such a degree that it must appear no less proper to make a figure in the history of their discoveries than albumen itself.

It is to the same penetration that we are indebted for those astonishing relations exhibited by caseum and gluten, when they have both experienced a kind of fermentation which transforms them into that cellular, odorous, and savoury combination called cheese: and in this singular result gluten approaches nearer to its model the more carefully it has been washed. Macquer, by asserting, as is now every where repeated, that it is indebted for a part of these changes to a residuum of starch, had not correct ideas. Starch, a substance always inert in fermentation, in that of bread, and of beer, and even in germination, would only serve to retard that experienced by gluten itself, and consequently could obliterate only in part the traces by which Rouelle discovered the resemblance of these two products.

And even their analysis passes beyond the limits which had been assigned to them; for, when gluten has changed its insipid and viscous mucosity for the caseous state, when it has passed through all the periods of that fermentation peculiar to it to arrive at this state, it is found seasoned with those acrid and burning salts which form the principal merit of the cheese of Roquefort; salts which have no affinity with that which is added, and which are found equally strong in the curd that has been washed and left to its own fermentation.

In the cheese of gluten, indeed, as in that of animals, potash and sulphuric acid will detect that ammonia and vinegar which Vauquelin discovered. Is the ammoniacal acetate, then, one of the ingredients which season cheese?

I know

I know only that alcohol applied to pungent cheese deprives it of all its savour. An analysis directed to this point might give us curious results: but let us return to the green feculæ; let us try them by the test of modern chemistry; and, in particular, let us endeavour to discover whether albumen really exists where Beccari and Rouelle found gluten.

Green Fecula.

I. Fecula exposed to heat experiences a change capable of furnishing it alone with a decisive character in regard to its nature. I allude to that concreteness of which there are few examples among vegetable products; that agglutination which attaches its molecule to each other, and gives them the appearance of caseous curd. If fecula before this change passes easily through the cloth, it can no longer do so when boiled; a peculiar crispation has then deprived it of its tenuity; but heat does not coagulate the fibrous tissue. The fecula, therefore, in this point of view, cannot be compared to the torn straw of green plants.

II. Fecula separated from juices by filtration assumes in drying a corneous and elastic consistence. It becomes soft with difficulty in boiling water, but it does not acquire softness even at the end of a month: notwithstanding its humectation it always retains its corneous nature. It recovers its former state if bent, but absolutely refuses to crumble: all these are qualities which are not observed in dried ligneous pulp.

The feculæ of green and white cabbages, cresses, hemlock, &c. do not by these means lose their property of coagulating by heat. In warm water between 145 and 165 degrees, if two equal matrasses be immersed, one with diluted fecula and the other with the water of the white of an egg, the fecula becomes crisp and is collected in flakes, such as those seen in any juice exposed to heat in order to be clarified; but at that temperature albumen does not even lose its transparency.

III. Green fecula is nearly of the same weight as water; for that of plants which are not acid employs more than eight days to deposit itself.

If fecula washed and diluted be poured into three jars, and if a little alcohol be added to the first, a few drops of acid to the second; and if the third be placed between the other two for the sake of comparison, the fecula in the two former will be completely deposited in less than half an hour, while that in the third scarcely begins to fall. Alcohol and acids then have the power of coagulating fecula, but they do not exercise the same action on woody remains.

IV. A hundred parts of the dry fecula of hemlock transmit to alcohol from 15 to 16 of green resin. When taken from repeated infusions to which it has been subjected, it remains of an earthen gray colour, and alcohol is never able to bleach it. Sage, who was well acquainted with the nature of feculæ, found some which gave even a third of their weight of resin: to exhaust them with ease, it is necessary to throw them still moist into spirit of wine; the spirit then penetrates and attacks them in every point: but this is much more difficult when they have been rendered corneous by desiccation.

Parmentier, I think, was the first who doubted, and with reason for his time, that alcoholic tincture of feculæ is resinous, because it is not precipitated by water. However, if it be considered that water can never detach it from gluten, that alcohol, oils, and fat, have exclusively this property; and that this substance, when separated from alcohol and concentrated in itself, is a fat tenacious body insoluble in water, it will be found that there is no product in vegetables which it approaches so much as the resinous: but we shall here show, that to determine in it more perfectly this character, nothing is necessary but to furnish it with a little oxygen.

The oxygenated muriatic acid in a few days renders green resin white and firm; it then suffers itself to be drawn out in threads like boiled turpentine, and its dye mixes readily with water: but if the green part of feculæ belonged to those coloured juices which are found in the ingredients proper for dyeing, oxygen would not convert it into a resin. But at present, since observation has taught us that we ought no longer to establish between vegetable products such rigorous limits as formerly; since we see them so often confounded by intermediate qualities, we are not astonished to find that a resin carried to its maximum of divisibility can associate with water. Do we not see camphor, essential, animal, and vegetable oils, sarcocolla, &c. dissolve completely in water? We shall not, however, for this reason, take such products from that classification which has been assigned to them by analysis.

Green feculæ assume in oxygenated muriatic acid that colour of dead leaves which forms the mourning of vegetation during the winter, and their dye becomes very turbid in water. Let us deduce then from all this, that if the colouring part of feculæ cannot resist water when transmitted by alcohol, it is no less in regard to its other qualities a substance absolutely resinous; and though this product, one of the most curious in the vegetable kingdom, since it embellishes

bellishes it with its different shades, has not been introduced into the *System of Chemical Knowledge*, various chemists, such as Rouelle, Danel, Sage, Parmentier, &c. thought it worthy of their researches.

This resin, dissolved in potash, abandons it to attach itself to silk, and to give it a bright green dye; but it fades too much to become useful: its shade, however, resists verjus: but the preference which it at length gives to gluten over the vegetable fibre is agreeable to known principles; for it is to animalized substances in general, rather than to the fibres of flax, hemp, or cotton, that colouring bodies attach themselves. Fecula then has something in its nature analogous to wool, silk, &c.: it is gluten.

V. Let us now examine fecula in points of view more proper for unveiling new characters of animalization.

If fecula, either boiled or raw, be kept under water, it begins in less than twenty-four hours to emit a bad odour. It soon exhales an excrementitious putridity, which always goes on increasing, and to which one perhaps could not long be exposed without danger. The infectious miasm which it diffuses around it instantly obscures metallic writing, and its liquor speedily blackens plates of silver.

It is to the corruption of this principle, no doubt, rather than to any other, that are owing the pernicious exhalations of hemp and flax when watered. As running water, which is equally proper as stagnant for separating the filaments, speedily carries off their extractive juices, nothing but the green fecula which unites the fibres is susceptible of being destroyed by watering.

The liquor which at the end of a year floats above putrid fecula contains sulphurated hydrogen, carbonate of ammonia, and gluten dissolved by means of the latter.

It has this peculiarity also, that it retains its stercoraceous odour after long ebullition. The product of its distillation contains carbonate of ammonia joined to a principle of infection which does not blacken metallic solutions, and with the nature of which I am not acquainted. Acids are not weakened by precipitating fecula and becoming saturated with ammonia; which induces me to think, that if the effluvia of a mass of animal putridity can serve as a vehicle to the phosphorus and sulphur it contains, it is not indebted for its infectious quality to these combustibles alone; that there is a great difference, for example, between the odour of rotten fish or flesh, corrupted fecula or putrid cheese, and that of phosphorated and sulphurated hydrogen.

[To be continued.]

XXII. *A Survey and Report of the Coasts and Central Highlands of Scotland; made by the Command of the Right Honourable the Lords Commissioners of His Majesty's Treasury in the Autumn of 1802. By THOMAS TELFORD, Civil Engineer, Edinburgh, F. R. S.*

[Concluded from p. 81.]

APPENDIX.

Report from the Highland Society of Scotland.

Queries referred to in the annexed Report.

1st, DOES it consist with your knowledge, that the progress of improvement in the northern parts of Scotland is much retarded by the want of roads and bridges? If so, what lines would tend most effectually to open the country and promote the public good?

2d, Does the valley, which passes through the north of Scotland from the Murray Frith on the east to Loch Eil, and the Linnhe Loch on the west, appear to you to be well calculated for an inland navigation, if formed of a size sufficient to admit of large trading vessels and frigates? (I have, for the sake of distinction, named this navigation the Caledonian Canal.)

3d, Would this navigation, by opening a ready and safe communication from one side of the island to the other, prove the means of promoting the extension of the fisheries, and of throwing the industry and intelligence of the fishers who reside on the east coast upon the extensive fishing grounds along the west coast?

4th, Would the undertaking these public works at the present time, by affording employment to the people, giving them habits of industry, and furnishing them with capital, tend to check the spirit of emigration which now prevails, and, connected with the powers which would be furnished by using the water which flows down each extremity of the valley from the extensive lochs, prove the means of laying the certain foundation of future employment?

5th, If the executing these roads and bridges would prove the means of employing the people, improving the agricultural state of the country, and of extending the fisheries, the nation would evidently derive an increase of revenue and power; and the land-owners through whose estates the lines of road passed, and indeed the whole of the adjoining districts of country, would enjoy improved cultivation and

pasturage, increased incomes, and all the blessings which are derived from a facility of intercourse: is it not therefore the interest of the land-owners to unite with government in executing these plans? and should not the memorials and propositions to this purpose originate with the land-owners, and be transmitted by them to the lords of the treasury, who will, by comparing the memorials with the information contained in the surveys made by their directions, judge how far the public aid can be with propriety extended?

6th, If the opening the Caledonian canal upon the scale I have proposed would prove the means of facilitating the intercourse from the west of England and Scotland, and the whole of Ireland, with the northern parts of Europe, and likewise from the east side of Great Britain to America and the West Indies; is it not just and reasonable that the commercial interests should be united with the efforts of government in carrying the same into effect?

7th, In my last I neglected to state, in order to enable the Highland proprietors to contribute, without inconvenience to themselves, a moiety of the expense of making the roads and bridges necessary for the improvement of that part of the country, that they might be empowered by an act of parliament to sell land to that amount. This is reasonable, because the price would be applied to improve the remainder of the entailed estates, which would by this means be much improved in value, though somewhat diminished in extent.

Report of a Sub-Committee of the Directors of the Highland Society of Scotland, on Consideration of a Letter from Mr. Telford, Engineer, to Henry Mackenzie, Esq. one of the Directors of the Society; made to, and approved of by, the General Committee of Directors of the said Society, 10th December 1802: the Right Honourable Lord Macdonald, one of the Vice-Presidents in Office, in the Chair.

THE committee have fully considered Mr. Telford's questions, addressed to Mr. Mackenzie, one of the society's directors.

In answer to the first, they are persuaded that even the lines of communication by means of military roads in some parts of the Highlands, have been productive of benefit to the country, though, the motives which gave rise to their formation having no relation to objects of commerce and industry, the advantages derived from them are very imperfect.

perfect. The committee accordingly have no hesitation in declaring it to be their fixed opinion, that the want of further roads and communications in the Highlands has hitherto proved the greatest obstacle to the introduction of useful industry there, and that every attempt for that purpose must fail, until regular and easy communication is afforded from one part of the country to another, and more especially from the remote points where there is the best field for useful exertion to the present seats of capital and industry. With regard to what lines would tend most effectually to open the country, and promote the public good, the committee humbly report their opinion as follows:

The Highlands, as to this question, may be divided into three districts: the first, comprehending the west coasts of Argyle and Inverness-shire, as connected with each other; the second, including the county of Ross and a part of the county of Inverness; and the third, or northern district, comprising the shires of Sutherland and Caithness.

In the first of these districts, the utmost benefit would arise from drawing a direct line of communication from the west side of the Frith of Clyde nearly opposite to Greenock to the Bay of Strachur upon Loch Fyne, from whence there is already an excellent and well conducted road to Fort William. From this point the road may be easily continued by Loch Eil Side to Loch na Gaul, through Arisaig into Morer. Such a communication would tend very greatly to the success of the fisheries in the islands of Egg, Rum, Cana, Muck, Barra, and South Uist, all of which possess numerous lochs and fishing banks in and around them. The greatest advantages would arise from approximating these various fisheries and extensive coasts to the Frith of Clyde, where the fishing capital is at present almost exclusively resident. It is evident that nothing can more discourage the employment of that capital in those parts than that difficulty of approach, amounting almost to inaccessibility, which renders the communication of intelligence always slow and even often precarious.

In the northern district the lines of communication would, from the nature of the thing, be drawn to a different point. A central point at the south of that district is found at or near Invershin, to which place the Frith of Dornoch is navigable, and where a bridge can easily be thrown over, and from whence a direct and short communication could be made to Dingwall and Inverness. From this point several advantageous lines of road might be made, one stretching by the banks of Loch Shin through part of Assint to

Kylescow, another by the kirk of Lairg to the head of Loch Loxford, and a third from the kirk of Lairg by the west of Lochnaver to Tongue. Another road again would connect together the western and eastern extremities of this the northern coast of Scotland, proceeding from Loch Eriboll (at which place there is one of the finest harbours in the kingdom) by Tongue, Farr, and Thurso, to Honna on the east. From this point, where there is a ferry to Orkney, the road would return to Wick, and from thence along the east coasts of Sutherland and Caithness, crossing the river Fleet by a bridge, to avoid the little Ferry, till it terminated at Invershin. Such lines as the above would open the whole of these countries to all the trading capital of Inverness and the east coast of Scotland, as well as by the way of Fort William to that of the Clyde; and it is well known that all the way from the vicinity of Kylescow round to Wick, the fishing grounds are abundant and excellent.

As to the middle division, the committee would humbly suggest the utility of certain lines of intersection from east to west. One of these ought to be from the great military road between Fort William and Inverness in a western direction, such as may best afford an easy intercourse between both these places and the islands of Skye, Harries, and North Uist, as well as Loch Hourn, Loch Duich, and the other valuable fishing lochs in that vicinity. A second will lead from Contin (which has already a good road to Dingwall) by the south side of Loch Garve and the head of Loch Lickart to Achnashine, and from thence in one branch to Loch Carron and in another to Pollew. From one or other of these branches a road of important benefit might be drawn to Loch Torridon, a third road will extend from the port of Ullapool in Loch Broom to Invershin at the head of the Frith of Dornock.

When the lines of road now mentioned are completed, the course of post will become rapid and regular. From the neighbourhood of Skye to Greenock the mail would be conveyed in three days, while from Invershin to Edinburgh by Aberdeen, or to Greenock by Inverness and Fort William, it would be conveyed at furthest in four, and thus the most remote points of the Highlands would be brought within five days course of post, at the utmost, of Edinburgh and the Frith of Clyde. It may suffice for contrasting such a situation of the Highlands with that in which they are now placed, in respect to communication of intelligence, to relate what happened this very year. When, after the return of the Clyde vessels from a vain search for herrings

in the northern lochs, some considerable shoals having appeared, intelligence was dispatched to Greenock; but owing to the indirect course of the post, and the difficulties of some parts of the circuitous journey, several weeks elapsed before any advantage could be taken of the information*.

The lines that have been suggested, or nearly such lines, are, in the opinion of the committee, the radical lines of road, as they may be termed, from which in process of time various ramifications will be formed, when the benefits of these begin to be perceptibly diffused.

From consideration of the connection of the fifth question with what precedes, the committee in so far depart from Mr. Telford's arrangement as to put next in order the answer to it. They are fully persuaded of the reality of those views, both of public and individual benefit, which the statement of the question includes; and they think it highly reasonable that the land-owners should, according to their respective abilities, unite with government in executing these plans by contributing a certain proportional part of the expense, varying with the different circumstances of their several situations. But the committee humbly report their opinion, that it would be advisable for the lords of the treasury, after weighing such suggestions as have been made; and consulting their surveyor, to select the lines of road which more immediately, and in a national view, invite the public aid; and then, after the selection is made known, it will be the duty as well as the interest of land-owners, to come forward with their proposals, stating, with regard to each separately, those local considerations which seem to fix the proportion of public aid that may fairly be solicited.

In answer to query second, the committee have no doubt that the Caledonian canal, formed on the scale suggested (sufficient for the passage of large trading ships and frigates), will be attended with the greatest national advantage. In respect to these objects, indeed, the benefit must be so incalculably great, that this truly useful undertaking assuredly merits the attention and exertions of government. The committee have equally little doubt in concurring with the opinion inferred in the third question, that, by opening a free communication from the eastern to the western sea, it would be highly beneficial to the fisheries, particularly by transferring the skill in the cod and ling fishery, possessed by the people on the eastern coast, by whom it is certainly

* The ordinary course of the post is one week from Loxford to Tongue, and another from Tongue to Tain, being on the line by Inverness to Edinburgh.

better understood than by the natives of the western, from the former to the latter of these shores, where the field for its action is inexhaustible.

With regard to query fourth, the committee are well convinced that the undertaking these public works must produce the united good consequences of checking the spirit of emigration, by affording useful employment to a great number of people, of improving the habits of the country by teaching lessons of systematic industry, and of affording at once the excitement to undertake, and the intelligence as well as (to a certain moderate extent) the means required for instituting those fishing and manufacturing establishments, on which the future prosperity of the Highlands must be founded.

On the sixth question the committee have to observe, that they are fully aware of the commercial as well as other national advantages derivable from the Caledonian canal; but with regard to the question to what extent commercial men would be ready to contribute individually towards carrying the same into effect, the Highland Society can have no means of forming an opinion, other than by reference to that general spirit of liberal enterprise which distinguishes the commercial body.

Adverting to a supplementary suggestion from Mr. Telford, the committee apprehend that it would be highly expedient to introduce a clause into any act of parliament on the subject, authorizing and empowering proprietors of entailed estates either to sell lands for defraying the expense of contributing along with government to the making of roads and bridges in the Highlands, or in their option to make the same a debt, affecting the subsequent heirs of entail.

It has been stated to the committee, that tutors and curators of minor proprietors, and trustees holding possession of estates concerned in these improvements, might feel some hesitation in venturing on the necessary outlays, as entertaining a doubt of such acts of administration falling within their powers. The committee are humbly of opinion, that it would be proper to add to the clause already suggested, an enactment, that tutors and curators of a minor heir of entail, or trustees in possession of an estate already entailed, or which is directed to be entailed, should have the same power of selling lands or charging the estate that is by the act conferred upon heirs of entail themselves. As also that tutors and curators of minors possessed of unentailed estates, and trustees holding possession of such, should

should be entitled by their acts in the premises to bind the minor or trustee, and all successors to the estate.

A true copy from the record.

(Signed) *Lewis Gordon*, dep. sec.

Highland Society Hall,
Edinburgh, Dec. 10, 1802.

Highland Society Hall, Edinburgh, Dec. 23, 1802.

Minute of the Committee of the Directors of the Highland Society of Scotland, which formerly drew up Answers, in the Shape of a Report, to the Queries of Mr. Telford, Engineer, respecting the opening of Communications by Roads and Bridges in the Highlands, and by a Canal from Inverness to Fort William, upon considering a Letter from Mr. Telford, of date 14th December 1802, to Henry Mackenzie, Esq. one of the Society's Directors, owning Receipt of said Report, which he states to be able, full, and satisfactory, and that the "only Instance in which it is rather less explicit than he could wish, is with respect to the Road in the Middle Division, which should connect the Inverness and Fort William Road with Skye, &c. &c." as to which Mr. Telford wishes the Committee could say something more specific, and recommends their taking any Information which can be furnished by Mr. Donaldson, Surveyor of Military Roads, upon that Point: the Right Honourable Lord Macdonald, one of the Vice-Presidents in Office, in the Chair.

The committee, in their former report, have pointed out the great objects to which roads through the district in question should in their opinion apply; but there being a difference of opinion as to the precise lines of road by which those objects would best be attained, the committee do not feel themselves at liberty to specify those precise lines. They would take the liberty of suggesting the expediency of government employing some able surveyor or engineer of respectable character and abilities to report on the subject; and if, relative to the present point of inquiry, they are to say any thing more particular, they may mention that the objects of this line of intersection seem chiefly twofold, viz. to afford a communication to the head of Loch Hourne, a very valuable fishing loch, and also to Bernera, the nearest point to Skye; both which they apprehend may be attained by two ramifications of a road from the military road leading from Fort William to Inverness.

The committee, adverting to Mr. Telford's suggestion of an examination of Mr. Donaldson, called him before them; but found that he had never travelled any part of the country from Fort Augustus, westward, to Bernera or the lochs, and that his information was solely, as to that part of the country, derived from others.

A true copy from the record.

(Signed) *Lewis Gordon*, dep. sec.

XXIII. *Anatomical Observations on the Crocodile of the Nile.* By E. GEOFFROY.

THE following observations were read in the last sitting of the Institute of Egypt:—Two unfortunate combats, and the loss of the battle of the 30th of Ventose, year 9, gave us reason to apprehend that the enemy, favoured by the misunderstanding which prevailed between our chiefs, would at length tear from us the most valuable of our colonies, which had cost us so many efforts and sacrifices; in a word, that celebrated country Egypt, which we had explored in every direction, which we had seen covered with monuments coeval with the heroic ages, and the fertility of which had appeared to us superior to its reputation. At the moment when we were informed of our disasters, and when the report circulated of them immediately excited against us the whole population of Egypt, a crocodile was brought to me which had been carried alive to Cairo, and which had died three days before. At a more fortunate period I had ardently desired to dissect an animal so much celebrated by antient authors; but being at that time abandoned to those painful sensations which all the French experienced, I hesitated a moment whether I should undertake this labour. Foreseeing, however, that if I let slip this opportunity I might never have another, and being persuaded, as I always have been, that the courage proper for travellers placed in the same circumstances as those in which I then found myself, is that of resignation, I paid no attention to any thing but the crocodile then before me. But I was not able to proceed to a regular dissection, nor to extend my researches to all those organs which appeared to be worthy of notice, being prevented by a commencement of putrefaction which the crocodile had already experienced, and by the obligation I was under to save and to preserve the skin. Besides, as it had already been observed

²⁸ From *Annales du Museum d'Histoire Naturelle*, No. 7.

by several distinguished anatomists, I thought it sufficient to confine myself to the consideration of the organs which might have escaped their examination; so that what I now publish contains only some additions to the history of an animal known since the earliest ages.

I. *Of the Manner in which the Jaws are moved.*

Who could believe, considering the present state of science, that this question is still problematical? It has been combated by a great number of travellers and naturalists, but none of them, as will be seen, have completely solved it.

Herodotus is the first who asserts that the crocodile is the only known animal whose upper jaw is moveable on the lower, which remains fixed: his opinion has been followed by all the antients. Aristotle, Pliny, &c., and some of the moderns, such as Margrave, Oligerus, Jacobæus, Marmol, the illustrious Vesalius, and some Jesuit missionaries to Siam, who had an opportunity of seeing living crocodiles, or of examining them soon after death, all speak of them in the same terms; but little attention was paid to these testimonies. The first anatomists of the Academy of Sciences undertook to demonstrate the impossibility of the fact advanced by Herodotus, and the names of Perrault and Duverney tended to establish this opinion, which has been adopted by the naturalists who have since written on the crocodile.

It is no doubt very surprising that Perrault, known for his accuracy, and who carefully dissected a crocodile from the menagerie of Versailles, did not pay sufficient attention to the singular conformation of the head of this animal; and that he should have opposed with so much violence the opinion of the antients. He gives a minute description of the articulation of the jaws, without observing that it furnishes proofs against the fact which he proposed to establish; and he besides supposed that he had done it in a satisfactory manner by rectifying the errors of Marmol, which he falsely ascribed to Vesalius, and by proving, with reason, that the upper jaw is not, as in perroquets, separated from the cranium, but that it forms with the rest of the head one osseous piece.

Since men of such merit as Perrault, Duverney, and the other naturalists who have since examined crocodiles in collections, could doubt of a fact attested by so great a number of observers, this question must certainly be embarrassed by a difficulty which can be cleared up only by an exact description of the head, and of the organs by which it is moved.

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The question here is not merely to rectify an accredited error, and to defend the antients from the injustice done them by some of the moderns. I must also call the attention of naturalists to a singular fact in regard to organization. Nothing, indeed, can be more paradoxical than the head of the crocodile; all those parts which in other animals are on the sides, are in the crocodile thrown backwards. The temporal bone itself projects backwards a good deal beyond the cranium: it is elongated, and transformed into a double condyle, the functions of which it performs. Every thing has in some measure been said of the head of the crocodile, by considering it as composed merely of two jaws, for the cranium is so small and so displaced that it escapes the first examination. It is found below and a little before the occipital plate; the brain, or rather the ganglion, contained in its cavity, which is exceedingly narrow, is continued pretty far forwards, so that the organs of sight and of hearing are situated below and a little behind it.

Another anomaly equally worthy of remark is, 1st, That the lower jaw is a sixth longer than the upper and the cranium. 2d, That the lower jaw exhibits a cavity with two facets, where the horns of the temporal bone are articulated by a ginglymus. 3d, That the occipital condyle is on the same line as the four condyles of the temporal bones, so that the head is really retained towards its points of articulation as the lid of a box is by its hinge. 4th, That as the two jaws have only a simple motion from the top downwards, they cannot be moved separately to the right or to the left, to subject the aliments to a sort of trituration.

On examining a living crocodile, or one prepared, as is customary in collections, it is hardly possible to believe that the head terminates at the extremity of the jaws; one looks for the osseous box which contains the brain, and which in all other animals manifests itself externally under the form of a frontal protuberance. The observer thinks he sees it towards the anterior part of the neck, which is symmetrically swelled up, and which is generally taken for the complement of the head: but this swelling arises from the presence of the crotaphite muscles, which are pretty voluminous, and which in a great measure are lodged between the straight and oblique muscles.

Differences so great in the form of the head necessarily occasion others in the organs which correspond directly with it; and it is indeed found that the cervical column is composed of seven vertebræ, which are distinct, but combined in their articulation in such a manner that they are
not

not moveable on each other. The apophyses of these vertebrae are so multiplied, so long, and so close to each other, that the animal cannot bend its neck, and that the cervical column, in regard to its uses, must be considered as one bone. The straight and oblique muscles attached to it, and which have their second point of insertion towards the occipital ridge, raise up when they contract the head on the neck, by making it describe an arch of 45° . The skin is thin behind the occipital plate, and readily yields to all the movements given to the head: on the other hand, the lower jaw is, as it were, sheathed in a rugous and very little flexible skin. If we suppose a muscular force sufficiently strong to draw it downwards, it would be retained by its coverings: it is besides confined towards its posterior extremity; for the long apophysis situated beyond the articular facets approaches the skin by describing a curve exactly towards the point, where it is armed with a long scale. The latter opposes an almost invincible resistance to the elevation of the condyle, and consequently to the depression of the jaw: it is not however entirely fixed, especially in the manner understood by Marmol, who thought that it formed with the sternum one bone. Two small elongated muscles, by contracting, can give it a slight motion. The assertion of Herodotus, then, is almost strictly true: "The crocodile is the only animal known, whose upper jaw, between the branches of which is comprehended the cranium, is moveable on the under, which has a motion almost insensible."

II. *Of the Organs of Digestion.*

The ancients, and almost all the moderns, have stated that the crocodile has no tongue: it is indeed true that it does not appear outwardly, but, speaking in a physiological point of view, this animal is not destitute of a tongue. The whole skin comprehended between the branches of the lower jaw is clothed interiorly with spongy, thick and flabby flesh, which is inseparably attached to it throughout its whole extent: but this muscle or tongue is in some measure masked in the inside by a continuation of the general coverings. It is a yellowish shagreened skin, perfectly similar to that of the palate: it is pierced with a great number of small holes, which are the orifices of the glands with which the upper part is furnished. This tongue has the form of the head of a lance: its dimensions in the subject I examined, which was 2.10 metres long, was 0.15 in length by 0.05 at the root. Though it does not project forwards, I have no doubt that it serves to retain and convey the aliments into the œsophagus;

phagus; for it is fixed by its base to the broad piece of the os hyoides. When the latter then is drawn downwards, while the muscular fibres of the tongue at the same time contract, it forms itself into a ball, and, being drawn backwards by the muscles of the os hyoides, it necessarily carries with it in the contrary movement the aliments comprehended between it and the palate.

The os hyoides is composed of three pieces. The largest 0·10 metre by 0·07 is cartilaginous, and resembles the broad part of a wooden shovel: the posterior bottom is round, and the interior straight. The latter is inflected on the convex surface of the large piece, and it is in the groove formed by this inflection that the root of the tongue is inserted. It results from this disposition that the large piece projects beyond the root of the tongue by about a centimetre. This projecting edge or kind of ridge becomes a velum, which, when the os hyoides is carried backwards, closes the whole of the back part of the mouth, and sometimes also the posterior apertures of the nostrils.

Such is the mechanism which allows the crocodile when pursued and frightened, as I have had occasion to observe in Upper Egypt, to shelter itself, and to lie concealed in the river, and to be able to respire in it. It only thrusts out of the water the extremity of its muzzle where the nasal apertures are situated: the jaws are then open, without the water being able to penetrate into the œsophagus and trachea.

The horns of the os hyoides are two small arched and elongated bones about 0·07 metre in length.

The os hyoides is drawn backwards by four muscles, the exterior ones of which are round and the interior flat: it is drawn forwards by the contraction of the tongue.

Perrault gives to the œsophagus of the young crocodile which he examined a greater diameter than to the stomach. He compares the œsophagus of this reptile to the gizzard of a bird which feeds on grain; and he consequently supposes, what would be an anomaly too monstrous to be found in the animal œconomy, that digestion is effected in a great measure in the œsophagus. My observations are directly contrary to those of that celebrated anatomist. I found the diameter of the œsophagus to be 0·06 metre, and that of the stomach 0·17 by 0·15; for the form of this bag is that of an ellipsoid slightly compressed on the sides: in a word, it did not appear to me to have any resemblance to a gizzard. The velvety tunic was exceedingly thick; the muscular part was less so: the inside was filled with a quantity of small pebbles,

pebbles, the polish of which announced that they had served for the trituration of the alimentary matters. The stomach had over it a bag, which was terminated by the pylorus. In the intestines, which were 3·67 metres in length, nothing could be distinctly observed but the rectum, in consequence of its great thickness. The duodenum, a little below the pylorus, was remarkable by a double contour, which it made from the top upwards for the extent of 0·14: its folds, which touched each other, were united by an adipose membrane split in three different places. The rest of the intestines, in which no trace of a cæcum is observed, was strongly attached to the loins by means of the mesentery.

III. *Organs of Respiration.*

The flatness of the tail of the crocodile, and the membranes extended between the toes of its hind feet, sufficiently account for the decided taste which this animal has for rivers; but as the ears and back part of the mouth are each provided with a cartilage, which, when necessary, prevents the introduction of the surrounding liquid, I had reason to expect a similar relation between the pulmonary organs and those of natat on. I therefore always wished that I might be able to describe these pulmonary organs as compared with those of other lizards, in order to trace out the most essential anatomical characters by which the genus of the crocodile differs from various other families of reptiles. If I therefore give this description, it is not because most of the preceding anatomists have omitted to do it: on the contrary, we are acquainted with those of Vesalius, sir Hans Sloane, Perrault, Hasselquist, and that more minute by the Jesuit missionaries to Siam, to which Duverney has added, and which might be considered as complete.

The trachea opens in the centre of the broad piece of the os hyoides, and accompanies it backwards nearly (that I may employ the comparison already used) as the handle of a wooden shovel accompanies the lower part. A little before it divides itself into two branches, it is folded back, and turns to the right side, as is observed in several birds. Its length in a straight line, as far as the point of its bifurcation, is 0·38 metre. It is composed of complete, broad, cartilaginous rings, separated from each other by a very narrow membranous ring. I found only the first ten rings complete. Duverney, in the crocodile of the Academy, counted sixteen, the portions of which were united by a membrane. The Jesuits above mentioned found a greater number in the crocodiles of Siam. It is this membrane strongly

strongly distended, and made to vibrate in the manner of the parchment of a drum, by the interior air of the lungs, which causes the crocodile to emit that cry, or rather that bellowing noise, mentioned by Catesby, La Coudreniere, and Bartram. The fissure of the glottis is then shut by the muscular roll which borders it on each side.

The lungs are two conical bags, the summits of which are turned towards the head. Their interior surfaces, which rest on the œsophagus, retain the impression of it by a longitudinal furrow. Their length is 0.33 metre, and their breadth at the base 0.22. The figure given by Perrault represents them of an elongated ovoid form.

The lungs of lizards are only two elongated bags, 0.40 in length, and 0.11 in their greatest thickness; the interior sides of which are lined with small reticular carneous fibres and sanguiferous vessels. Those of crocodiles differ by the size of the membranous leaves with which they are furnished, and which form as it were several small walls. It is a vast reticulation, composed of a quantity of meshes similar to those which are seen in the second stomach of ruminating animals. Each of these meshes is the edge and entrance of a small bag, which opens into a second, and sometimes into a third. They are composed of two kinds of fibres: the first circular, and parallel to each other; the second perpendicular, which transversely intersect the former at right angles. The centre of each pulmonary bag, entirely empty, serves in some measure as a receptacle for the air. The cells in opening become filled with it. They then compress it by shutting, and convey it to the blood, as we may say, without the concurrence of those organs which press on the whole pulmonary mass. They repeat this play until the air contained in the whole lung is vitiated. The crocodile, then, is not forced to come and respire at the surface of the water till after a certain time has elapsed. This structure of the lungs, which makes the crocodile deviate from lizards, brings it near to the sea-tortoise. I shall have occasion hereafter to remark, that this is not the only relation which it has to these animals.

IV. *Of the Organs of Generation.*

These organs are so complex, and have so little relation to what is known in the mammalia, that authors, as we may say, have been afraid to describe them, and have scarcely given a slight sketch of them. It has been said that the crocodile is only a lizard of a monstrous size, and Linnæus has consequently arranged it in his system under the genus *lacerta*.

lacerta. What I have already said of the configuration of the head and lungs of this reptile removes it no doubt from that genus, but the consideration of the organs of generation will obviate all uncertainty in regard to these natural relations.

Most lizards, like serpents, are furnished with two yards, situated on each side of the anus. Properly speaking, they are only two cavernous bodies, formed by a slight cutaneous expansion, and terminated by two cartilaginous appendices. In the inside of them are found two glands, which pour forth a liquor in such abundance, that it has given rise to mistakes in regard to its nature, and made it be considered as the seminal liquor. These yards move in a sheath formed by a duplicature of the skin: they are terminated behind by an elongated muscle, always inclosed in a membranous vagina, which, by contracting, forces them to re-enter.

If the crocodile retains any part of this general plan of organization, the combination is quite different. It has only one yard lodged at the anterior part, and in the fold of a common cloaca: it is imperforated, entirely cartilaginous, and terminated by a kind of gland 0.03 in length. It has two glands on the sides of the anus, from which oozes a whitish liquor through two distinct orifices at a considerable distance from each other. These glands produce no protuberance, and yet the retractor of the cavernous bodies of lizards exists. It is even of so considerable a volume (0.40 in length, and 0.11 in its greatest thickness), that it is this muscle with its congenerate that swells up the anterior part of the tail, so that it cannot be distinguished by a diminution of volume from the rest of the body. This muscle is terminated by a sharp edge, or ridge, where it is articulated with the caudal vertebræ, and by a free and round edge on the opposite side. What is remarkable besides is, that it is contained, like the retractor muscle of the cavernous bodies, in a proper sheath of great thickness, and of a cartilaginous nature. This sheath is continued forwards in an aponeurosis, which spreads and is inserted on the pelvis; so that, as the uses of this muscle change with the general system of organization, they are confined to contributing merely to the lateral motion of the tail.

The testicles in some measure approach near to those of fishes: they are narrow and elongated. They are observed a little above and before the kidneys.

The semen is conveyed in two pretty large vessels, contiguous, and lodged behind the common cloaca. These vesicles are in part closed by a cartilaginous bag: they open
into

into the common cloaca by six or seven holes on each side, disposed in a circular manner around the urinary passage.

V. *Of the Liver.*

The liver is composed of two unequal lobes : one of them has the form of a parallelepipedon (0·14 metre by 0·09) ; the other is slender and more elongated (0·19). This viscus exhibited a very remarkable organization, which has never yet been noticed by any anatomist. The convex surface of each lobe is covered by a membrane, which is the aponeurosis of a muscle, the use of which I can hardly comprehend. This muscle, which begins at the posterior and inferior edge of each lobe, is inserted very near the pelvis, in the last piece of the sternum ; for it must be recollected, that the latter is prolonged beyond the ribs, and terminates in a large piece articulated with the bones of the pelvis. These two muscles, which have not yet been found in any other animal, produce by their contraction the depression of the liver, and thereby give more capacity to the breast. This use makes them have an affinity with the diaphragm : the points to which they are attached might induce a belief of the same thing *. The gall-bladder is ovoid, 0·08 of a metre by 0·03, and adheres to the right lobe of the liver.

VI. *Of the other Viscera.*

As these have been already so well described by most of the authors before mentioned, I shall give only their relative positions, because this information may furnish some useful hints for determining the different kinds of crocodiles.

The *heart*.—Its height is 0·07 metre, its base 0·05. The right auricle is larger than the left.

The *spleen*.—Oval elongated, 0·10 by 0·04. On the inferior face it is somewhat concave ; and on the upper rises into two ridges, one of which is very small.

The *kidneys* are composed of papillæ, and numerous sinuosities formed by a collection of glands, 0·11 metre by 0·05½.

I had not resolved to publish these observations till after my return from Egypt ; and at that time, notwithstanding the learned researches of several of my colleagues, they had still retained all their novelty. The object of Cuvier's excellent memoir is merely to establish the real differences be-

* C. Cuvier observed these muscles in the crocodile of St. Domingo. He proposes to describe them at more length in his *Comparative Anatomy*.

tween the crocodiles of the new continent and those of the old; and Daudin* has endeavoured, in particular, to enrich the history of the crocodile from the relations of travellers before unknown.

Since I have occasion to quote the latter work, I must rectify an error which concerns me, and which C. Daudin introduces in consequence of the respect which he is pleased to entertain towards me. He announces that "I attempted, during my stay in Egypt, to tame crocodiles after the example of the antients, and that my attempts were not crowned with that success which I expected." It is a duty which I owe to truth, to assert that I never made any attempt of the kind.

The following is the circumstance that gave rise to this report, which was indeed circulated at the time of our triumphs. The period when the army of the East had at its head a chief worthy of its great exploits, the English sent to combat us could then find no opportunity of gratifying the desire of injuring us with which they were tormented. Tired of cruising backwards and forwards to no purpose, they wished to amuse themselves, and thought they could reach us with very feeble weapons, by endeavouring to turn into ridicule the principal persons in the army. They made some caricatures, which they sent to England, and which were thence conveyed to France. I had the honour of attracting their notice. They introduced into the scene several crocodiles; and this ephemeron production gave rise to the mistake to which I allude.

Explanation of the Figures. (Plate III.)

Fig. 1. A A, the lungs.

B, the pericardium.

C C, the two lobes of the liver.

D, the diaphragmatic muscles.

E, the stomach.

F, the intestinal canal.

G, the sternum and its muscles seen interiorly.

H, the same organs seen exteriorly.

Fig. 2. The cranium of the crocodile of the Nile: It is here represented,

1st, To give an idea of the manner in which the crocodile raises its upper jaw on the inferior.

2dly, To show the two condyles of the horns of the temporal bone, and the cavity with two facets, where they are articulated.

3dly, To render sensible the differences which exist between the large teeth of the crocodile of the Nile and those of the crocodile of St. Domingo.

XXIV. *Observations and Experiments on the Light emitted by rotten Wood, in the different Kinds of Gas, and in Fluids.* By C. W. BÖCKMAN, of Carlsruhe.

[Concluded from p. 26.]

Experiment XVII.

I INTRODUCED phosphorescent wood under the receiver of the air-pump, placed a bit of it in a glass filled with water, and another piece on a dish not immersed in water. When the air began to be rarefied, a quantity of air not inconsiderable issued from the wood. The light of the wood seemed to be somewhat weaker than at first, and proportionably less than that of a piece placed without the receiver. When the quicksilver in the barometer connected with it sunk about four lines, on the re-admission of atmospheric air I thought I observed a considerable increase in the phosphorescence. The piece of wood immersed in water was entirely penetrated by that fluid: it therefore fell to the bottom, but emitted as strong a light as at first. This experiment was several times repeated with the same result.

Experiment XVIII.

Oval glass flasks, capable of containing five cubic inches, and of equal thickness, were filled; some with oxygen gas, some with azotic gas, and some with carbonic acid gas, and well stopped with corks, to which were affixed wires supporting pieces of phosphorescent wood. I then introduced all these vessels at the same time into water at 70° of Reaumur. In about three-quarters of a minute the light in the carbonic acid gas perceptibly decreased, and then that in the azotic gas; in one minute and a half it appeared weaker in the oxygen gas; and in two minutes and a half the least phosphorescence was not to be observed in any of the vessels. It could not afterwards be revived in any of the pieces of wood by any means whatever. This experiment was also repeated several times with the like result.

Experiment XIX.

Rotten wood emitted light in spring water, in boiled water, and in distilled water, kept in a close vessel till the experiment

ment was made at the temperature of from 8 to 12° of Reaumur, the same as in common air. The phosphorescence did not become weaker till the end of several hours, and in twenty-four hours it completely ceased. It could, however, be in part revived by wrapping up the pieces of wood in filtering paper; but when the water had the temperature of 45° the light was speedily extinguished, and could not be again revived.

Experiment XX.

Having introduced wood strongly phosphorescent into sulphurous acid, nitrous acid, and muriatic acid, the light either instantaneously ceased altogether, or in one or two minutes, and could not be again revived in any manner.

Experiment XXI.

I added a considerable quantity of water to the above acids, in such a manner that only a few drops of acid were put into a cubic inch of water. The light of the rotten wood decreased in three minutes, ceased in general in from six to ten minutes, and was not again visible when the wood was washed with water in the atmospheric air.

Experiment XXII.

Rotten wood appeared phosphorescent in muriate of ammonia, nitrate of potash, common salt, and tartaric acid; and even somewhat stronger in the second and third solution than in atmospheric air. The light continued longer than in common spring water.

Experiment XXIII.

The phosphorescence of rotten wood continued in a dilute solution of carbonate of potash as well as in liquid ammonia two or three minutes, and then became entirely extinct. In linseed oil the wood appeared phosphorescent without any diminution eighteen hours, and in thirty was entirely extinguished.

Experiment XXIV.

I immersed phosphorescent wood in spirit of wine, and found that it was extinguished in from four to eight minutes. In sulphuric ether the light strongly decreased after twenty minutes, and in a short time entirely disappeared. It could not be again revived by the common mode of treatment.

If these experiments, which I was not able to carry any farther for want of phosphorescent wood, be compared with

the experiments of other observers, the following differences will be found :

1st, According to the above experiments, rotten wood does not emit so remarkably clear a light in oxygen gas as Spallanzani observed. The light also was not so speedily extinguished, that is, in scarcely a minute, as found by Tychemsen. And as my observations perfectly coincide with the experiments of Humboldt and Gartner, Spallanzani's and Tychemsen's observations, which I will not venture to doubt, must have been attended with peculiar circumstances.

2dly, In regard to the diminution of the volume of gas, M. Gartner found it once in oxygen gas two-thirds of the whole. According to my experiments, however, it was always much less.

3dly, The residuum of the oxygen gas, in which the rotten wood had ceased to be luminous, was found by Gartner, on being subjected to proof, of such a nature that one measure of it mixed with a sufficient quantity of nitrous gas gave a diminution of 207° . I however found the diminution of the residuum in a bell glass to be 120° , and that in another only 21° , though our oxygen gas was prepared from the same material, namely, oxide of manganese, and of a quality equally good. This difference, perhaps, was owing to a difference in the size of the vessels employed, and even in the rotten wood.

4thly, The sudden extinction of phosphorescent wood observed by M. Humboldt in carbonic acid gas purified by means of phosphorus, I did not observe in exceedingly pure gas treated in the same manner.

5thly, M. Humboldt observed the same sudden extinction in azotic gas which had been freed by means of phosphorus from any oxygen it might contain. But this I was never able to observe, either in azotic gas as pure as possible, or in the same gas exposed to the contact of phosphorus.

6thly, In these experiments I did not pay very great attention to the absorption which might take place of the different kinds of gas, because in researches of this kind in regard to rotten wood no accurate results can be obtained. I however, in general, observed no diminution in the azotic gas ; which, on the other hand, according to Tychemsen's observations, was greater than that experienced by oxygen gas prepared from saltpetre.

7thly, M. Humboldt found that rotten wood was luminous only for a very short time in oil ; whereas Carradori's experiments and mine show the contrary.

8thly, According to M. Gartner, a piece of rotten wood

continued

continued luminous in hydrogen gas in which another piece had emitted light, a much shorter time than in gas which had not been before used. I do not remember to have ever found any remark of this kind in the works of others; and as I paid particular attention to this point, in regard to every other kind of gas, I did not find Gartner's observation confirmed. A great part of these variations, however, are probably owing to this circumstance, that the wood employed was not of the same kind, and had not the same degree of rottenness and moisture. All our experiments, however, coincide as much as could be expected from the use of a substance such as phosphorescent wood.

If we still further compare the phenomena of rotten wood with those of phosphorus, the following differences will be observed between these substances:

Phosphorescent Wood,

1st, Is luminous in oxygen gas at low temperatures.

2dly, It is phosphorescent in all non-respirable gases, at least a short time, and in several of them in a pretty continued manner; as in phosphorized oxygen gas, and phosphorized azotic gas,

3dly, In muriatic acid gas its light is soon extinguished.

4thly, Its phosphorescence in rarefied air is weaker.

5thly, It emits light in a Torricellian vacuum, according to the testimony of Carradori.

6thly, Its light becomes extinct both in oxygen gas and in other gases when heated.

7thly, By the process of its phosphorescence in oxygen gas carbonic acid gas is produced.

8thly, The phosphorescence of rotten wood can be extinguished several times successively in the non-respirable gases, whether they contain oxygen gas or not, without the property of the gas to maintain the phosphorescence of a new piece of wood introduced into it being perceptibly lessened.

9thly, Moisture promotes the phosphorescence of rotten wood, and is essentially necessary for that purpose.

10thly, Rotten wood emits light also under water, in oil and in other liquids. Its splendour in some of them is even heightened.

Kunkel's Phosphorus,

1st, It becomes luminous in oxygen gas only at a temperature of about 16° to 22° of Reaumur.

2dly, Of all the non-respirable gases as pure as possible, it

is luminous only in azotic gas, oxidated azotic gas, and muriatic acid gas.

3dly, In muriatic acid gas it inflames immediately of itself, and burns with great brightness.

4thly, The light of phosphorus is stronger in rarefied air.

5thly, It emits no light in vacuo.

6thly, On being subjected to heat, it inflames and burns with rapidity in oxygen gas; and in the non-respirable gases, not pure, its light is stronger.

7thly, By its phosphorescence in oxygen gas no carbonic acid is formed.

8thly, When artificial phosphorus has emitted light in the non-respirable gases not perfectly freed from oxygen gas, a fresh piece of phosphorus does not become luminous in them: azotic gas, however, is an exception, in which, after being purified, phosphorus becomes luminous for some time.

9thly, Moisture and wet are impediments to its being luminous.

10thly, Fluids are altogether contrary to the luminous property of artificial phosphorus.

Phosphorescent wood, therefore, differs essentially from artificial phosphorus by the conditions requisite for its being luminous; and therefore the assertion of Spallanzani, that the greatest analogy exists between the luminous phenomena of these two substances must lose some of its weight.

If I mistake not, the following probable conclusions may be deduced from the above experiments: that the extinction of the light of rotten wood in different mediums does not so immediately arise from a want of oxygen gas as from some change which the wood itself has experienced. For even if, according to the opinion of Humboldt, Spallanzani, and other philosophers, the oxygen gas concealed in different mediums be the immediate cause of its phosphorescence, in several experiments where rotten wood was immersed in different mediums I must have observed no light: at any rate, the phosphorescence must always have been weaker, and fresh wood repeatedly introduced would no longer have been luminous in it; which is contrary to what I experienced,

Besides, how difficult would it be to prove, with any degree of probability, that the undiminished phosphorescence of wood, which is observed in distilled water and in oil, arises from a small quantity of oxygen gas in these fluids? And is the luminous wood, placed in these fluids, capable of decomposing oxygen gas with which it is so little in contact,

tact, and which, as a component part in atmospheric air, must adhere to the azotic gas, and which is also in combination with the fluids themselves? Is artificial phosphorus, which in all probability has a greater affinity for oxygen than luminous wood, able to decompose the oxygen gas in water? May not its being oxidated under water arise rather from the slow decomposition of the water itself, than from the decomposition of the gasiform oxygen contained in it? If otherwise, phosphorus would be as luminous in water as wood; which, in the course of my numerous experiments, I did not find to be the case, though I had often in my hand flasks which contained half a pound of partly fresh melted and partly weak and strongly oxidated pieces of phosphorus: and in regard to the luminous stars which Messrs. Scherer and Jäger have described, and which I observed in boiling water in which phosphorus was put, I consider them to have been fine phosphoric particles which emitted light in the small air bubbles separated by means of the heat from the water. I observed also in the dark, on opening the above flasks, in which phosphorus had remained for some months, luminous vapours often arise from them. This luminous appearance, however, did not take place in water free from the contact of atmospheric air. It appears to me also, in consequence of several experiments, made with great care, on the quantity of oxygen gas decomposed during the combustion of phosphorus in atmospheric air, or in non-respirable gases in which a certain quantity of oxygen gas is mixed, possible to determine, in a certain degree, from the known quantity of the surface of the phosphorus, from the duration of the light and its intensity, how much oxygen gas is actually decomposed; and if this idea is not entirely groundless, it appears to me very improbable, that in the interstices of distilled water as much gaseous oxygen exists as is sufficient to account for the phosphorescence of rotten wood in it, according to the conjecture of M. Humboldt; and therefore it appears to me more probable that wood, to produce its phosphorescence, is in no immediate need of oxygen. I was induced, by some expressions of M. Humboldt, to enlarge more on this point than I otherwise should have done.

I am not much inclined to believe that wood, as a luminous substance, produces the observed diminution of oxygen gas. According to my opinion this is occasioned much more by the degree of the rottenness, as during all processes of fermentation and putrefaction the above gas is decomposed. M. Humboldt does not appear to me to have

adopted a proper method, when, in some of his processes, he destroyed the phosphorescence by sudden heating, in order that, by comparing it with that of other wood not treated in the same manner, he might observe whether it effects the diminution of the gas as a luminous or as a putrescent substance. For it appears to me very probable, that by such a violent exaltation of temperature, besides the phosphorescence, the previous degree of putridity is changed, and consequently that this wood can neither decompose nor absorb any more oxygen gas.

This alteration of phosphorescent wood, by means of which its light is more or less speedily checked by certain mediums, appears to me, with some probability, to depend on the circumstance whether they are calculated more or less to check the putridity, or, on the contrary, to promote it. The putridity, therefore, and the phosphorescence connected with it, must continue not only in oxygen gas but also in atmospheric air, and in weak solutions of muriate of soda and nitrate of potash. Like the phosphorescence of wood, it is more or less checked by want of oxygen gas, and consequently in all the non-respirable gases, and particularly in nitrous gas, and in carbonic acid gas on account of its peculiar property of opposing putridity; also by exposure to heat, on account of the desiccation connected with it; by concentrated or diluted acids; by tartarous acid, &c. The latter acts, perhaps, so far as it speedily attracts the water, and therefore desiccates the rotten wood. But whether sulphurized hydrogen gas, ammoniacal gas, muriatic acid gas, which are all speedily miscible with water, exercise a prejudicial action on luminous wood by absorbing its moisture and acting in the same manner as fluid ammonia or acids; or whether, by means of a peculiar antiputrescent property, they extinguish the phosphorescence so speedily, I will not venture to determine.

In a word, this theory harmonizes pretty accurately with the well known experiments on phosphorescence, which continues in mediums that promote putridity, and is interrupted by fluids that oppose it. According to the above-mentioned opinion of Carradori, this philosopher seems to have considered such action as possible, but he does not assign any cause.

The following observations of M. Humboldt, which he wrote however for a totally different object, I found so applicable, with a few variations which may be easily made, to the conjectures I have expressed on the luminous appearance of rotten wood, which continues in some mediums
and

and in others is weakened or completely checked, that I consider it as my duty here to insert them; for coincidence with the unprejudiced opinion of a great man is always advantageous to a writer, and when thrown into the scale adds no small weight to an assertion. M. Humboldt says, "When the equilibrium between the component parts of organized matter is destroyed, and the important chemical process of putrefaction begins, it is variously modified by the temperature and nature of the surrounding mediums. Every fermenting substance, therefore, changes every moment the state of its mixture; and as its natural phosphorescence depends on this change of mixture, every thing that relates to the one must increase or destroy the other. There are two conditions, therefore, under which rotten wood is extinguished, one of which has an immediate and the other a mediate action. The first is only oxygen gas; the other, heat, oil, acids, alcohol, &c. Decomposition with the disengagement of light ceases until a new accession of oxygen gas. But the putrescent substance in contact with oxygen gas is brought to a new state of mixture by elevation of temperature, and quits its former state of putridity, which is an essential condition of the disengagement of light."

If the determinate degree of putridity is totally changed or destroyed by certain mediums, the extinguished light of the wood cannot be again revived; but if these fluids have exercised on the wood only a weak action and for a short time, and if the degree of the putridity be therefore only as it were superficial, the phosphorescence in this case may be again revived and strengthened by means that promote putridity, and consequently by moistening, exposure to atmospheric air; &c.

I, however, freely confess that it still appears to me difficult to explain, in a definitive manner, how wood in this phosphorescent state is decomposed, and what its luminous appearance really is. I have however formed several ideas on this subject, but they do not appear to me to be yet fit to be laid before the public. Nor will I venture to determine how near the truth the opinions of Spallanzani, Carradori, Humboldt, and Gartner, approach. I am inclined to think that time and experience are still necessary to bring them to maturity. This much, however, is certain, that to produce such phosphorescence several particular circumstances are necessary, otherwise this phenomenon would occur much oftener in nature,

XXV. *Conjectural Observations on the Mammoth.* By
G. J. WRIGHT, Esq.*

THE direct or indirect benefit of mankind is universally allowed as the end of the creation of those various classes of animated beings which inhabit our globe. Among these some appear to be less entitled to arrangement under this axiom than others; or, in other words, the benefits to be derived from a certain class may appear more than counter-balanced by the evils incurred by their ravages: but, happily for us, the disproportionate increase of the various species of such, and particularly of the more destructive, is in a great measure prevented, not only by the indiscriminating rapacity of animals whose superiority of rank carries certain destruction wherever they haunt, but also by that wise provision of nature, which, the more effectually to curtail the diffusion of destructive animals, implants in other orders a peculiar specific and native antipathy, prompting them to unceasing warfare against such their appointed prey, whose undue increase, if not by some appropriate method prevented, would in process of time probably depopulate the world.

As then (for reasons which it is not our province to analyse) mankind are exposed to the ravages of such destructive beings, we must necessarily allow that class as most to be dreaded, against which we are acquainted with no effectual means of opposition from other animals, not excepting even man, who, though placed in the world as the lord of the creation, unassisted by those skillful manipulations in the arts by means of which he ranges unhurt amidst a thousand dangers, actually becomes the prey of tribes over which he is destined to rule.

If the above precautions are necessary in an æra when the greater part of the globe is replete with inhabitants, how much more requisite in the earlier ages, when, population being at a low ebb, the continuance of the human race in far separated climes may have hinged on the preservation of a few, and that at a time when these scattered individuals were unfurnished with the implements of security of which modern improvements have put us in possession!

And can we suppose for a moment, that while the increase of less hurtful creatures is limited by various means, the disproportionate extension of a class against which nei-

* Communicated by the Author.

ther brutal ferocity nor human skill can avail, should be left overlooked, and not provided against? I say, if we assent to such a supposition, we form a chasm in zoological economy which the wisdom of nature throughout her various analogies stamps as unwarrantable.

So important a query, one would imagine, would scarcely have been left undiscussed, as applicable to so formidable a race of beings as the serpent tribe, against whose undue increase*, however desirable, no effectual bar seems to be provided, no means of evasion successful, and against which the opposition of other animals is attempted but to their certain fatality.

In the primitive ages of the world, when the paucity of mankind must have allowed of the unlimited extension of all the classes of inferior beings†, the tribes of the serpent kind, if no natural means of opposition from other animals were furnished, must have increased to an incalculable degree both in numbers and size. Combining in themselves at once insatiable rapacity with incredible abstinence, their age and growth apparently unlimited, their amphibious nature allowing an extension of their ravages to the inhabitants of the watery element as well as those of the land, and at the same time suffering them alike to evade the premature view of their prey; with such noxious qualities they hold a scale in the creation perhaps not less formidable than would appear the combined disadvantages of all other ferocious beings. Nor can we suppose that the general deluge, which at once destroyed the several tribes of land animals, at all diminished the numbers of the serpent kind. We have no reason to conclude that amphibious animals were included in the ark any more than fish; and although naturalists agree that the largest species of serpents are most frequently found in fresh water, yet it does not follow that so weak a saline solution as the mixture of the rain with the waters of the ocean, which together composed the waters of the deluge, should prove disagreeable to them: it is true, some suppose the sea to have been more saturated with saline matter in the primitive ages than since‡, on account

* The smaller species casually become a prey to carnivorous birds and animals.

† The Indian missionaries reported that vast tracts of that country had long been uninhabitable by reason of the increase of the brute creation, so that whatever man cultivated for the support of life was destroyed without possibility of prevention,—*Encyclop. Britan.* vol. v. p. 743.

‡ Kirwan's *Geological Essays*, p. 377.

of the subsequent mixture of the quantity of rain which fell during the awful crisis alluded to: but I am far from adopting that opinion, although it would not militate against the supposition above hazarded; for, if a class of creatures not included in the ark escaped the ravages of the deluge, it follows that, if the same are still in existence, the waters which composed the deluge, whether saline or not, did not prove deleterious to them*.

* The salubrity of water not depending on its containing salt, but on its continual agitation, my own opinion is, that the sea was scarce at all saline before the deluge; and as it is necessary we should find some source for the saline matter therein contained, which the supposition of its rise from saline springs running into it, or mines of salt under its bed, is found inadequate to, so I imagine that the origin of the formation of muriatic acid (which combined with the alkaline and earthy matters at the bottom of the ocean forms the several marine salts contained in the same) is due to the putrefaction of the various animals floating in the ocean: but of this we must remain ignorant till experiment shall develop the actual elements of this acid so peculiar to marine situations: it is certain that wherever the external agents are most favourable to the putrefying process, there the sea is most salt; and this, as I think, depending conjointly on the greater evaporation which must needs happen wherever the temperature, as in hot climates, is most favourable to this process, together with the more speedy putrefaction of all animal and vegetable matters in equatorial latitudes. The deluge, in an indirect manner, was certainly a means of destruction of myriads of shell-fish, which, imbedded in the mire covering the surface of the earth, were by the induration of the same completely hemmed in on every side, forming extensive beds of that mixture of shells and earthy matters so frequently met with: but fish provided with fins to rise to the surface of the water, as also all that have the faculty of swimming, could not be included in this general destruction: hence the tribes of serpents and water-snakes would not be diminished by the waters of the deluge; which implies a still further necessity of some effectual means of their diminution; otherwise their numbers would present a most formidable obstacle to the new peopling of the earth after the deluge. With regard to fresh- and salt-water fish, though the former could not perhaps endure for a length of time so concentrated a saline solution as is our present ocean, yet we may reasonably conclude that both classes could exist in, and become naturalized to, a slightly saturated mixture, as must of necessity have been the waters which composed the general deluge.

In conformity with the above theory I consider the origin of sea salt to be in the sub-aqueous putrefaction of animal matters, especially during the decline of the deluge; and so far from the ocean being supplied from mines and springs of salt, I imagine the former to be merely the product of filtration, during the very gradual subsidence of the waters of the deluge into immense cavities, (which must needs have been found by the falling in of vast masses of earth, whose support worn down by the softening quality of the water no longer could resist the incumbent weight,) and that of the latter to arise of course from currents of fresh water, which, occasionally in wearing themselves an outlet, find a passage through these same primitive depositions of sea salt,

If then the disproportionate extension of the serpent kind is not provided against by any known means, and we allow as we do the superior necessity of some effectual restraint to the same, it behoves us to attempt to define the probable structure and habits of the animal which should be best able to quell this class of reptiles, not less prolific in themselves than they are obnoxious to all other creatures.

Such an one I presume to be the mammoth, whose stupendous appearance gives great air of probability that he is destined to oppose a class of beings unconquerable by other means. His allowed amphibious nature renders him a fit opponent to the serpent tribe: the position and form of his ribs, so well adapted to resist external pressure, render him calculated to oppose this class of reptiles, whose efforts to vanquish their prey are confined to attempts to entwine the same, and crush, as they do by their convulsive writhings, the bodies of the largest animals we are at present acquainted with. Nor can this strength, indicated in the position and structure of the ribs of the mammoth, be supposed to be for fitting him for resisting the pressure of water merely, since many of the tribes of fishes which visit the deepest parts of the ocean are unprovided with a similar barrier* for that purpose; at the same time that most of such as are furnished with ribs have them barely more than cartilage.

The tusks of the mammoth are admirably situated for tearing up bushes and digging in morasses, the better to dislodge his prey, the which if of shell-fish, as has been suspected †, would doubtless exhibit his teeth more abraded than they appear to be; nor is there in our present supposition concerning his diet any necessity for canine teeth, the absence of which in the mammoth gives great countenance to the idea that his food has been of a more soft consistence than the muscular flesh of land animals: the shortness of the neck would appear a wise provision in the case

* Many fish are without ribs; such are rays, sharks, pipe-fish, sun-fish, porcupine-fish, lump-fish, &c. &c.—*Cuvier, Comp. Anatomy.*

† Several reasons lead me to object to the idea of fish being the ordinary food of the mammoth. Both shell and flat fish inhabiting solely the water, the animal which might be designed to feed on such diet would doubtless be an aquatic and not an amphibious animal. In the class of amphibia it is observable, that according as either the water or the land is most genial to the habits of any one species of these, so does their structure more or less verge toward that of the fishy tribe, and the contrary. It therefore reasonably follows, that the anatomical structure of the mammoth, if destined to live on fish, and of course in the water, should approach more nearly to other of the aquatic class than its skeleton appears to do.

of its combating with the larger serpents, as the neck appears the only portion vulnerable in the mammoth by the writhings of those reptiles, which if perhaps not well defended by the tusks of the former, might endanger its strangulation: the prominent ridgy back, and allowance of considerable motion on the part of the head, would certainly be favourable circumstances when viewed in the present light. Whether the hide of the mammoth may or may not have been susceptible of being pierced by the teeth of serpents I will not pretend to conjecture; but if it were so, it is to be recollected that the bite of venomous serpents is dangerous in proportion as the animal bitten is more or less of a hot-blooded temperament*: hence the amphibious nature, together with the food of the mammoth, rendering him a cold-blooded animal, fits him still better as the opponent of the serpent tribe. 'Tis most probable, that as the quickness of sight in serpents is even proverbial, the enemy of these formidable reptiles would be endowed with equal perfection in some other organ the better to search them out: perhaps the notorious odour of the majority of serpents will warrant us in concluding that the organs of smell would be destined for that purpose. As a serpent when desirous of evading the view of its natural enemy would most probably retreat to the neighbouring swamps, burying himself with all possible speed therein, so the latter should be capable of exerting a considerable degree of activity for at least a short space of time; and in this respect also the mammoth well corresponds.

With regard to the strength of the opponent of this tribe of deleterious creatures, we must at once see the propriety of it; but referring to his bulk, it certainly appears not so immediately necessary, were it not that, upon more mature consideration, it seems requisite that this opponent of the serpent kind should feed on the flesh of the same; otherwise the putrefying carcasses of so bulky a class of beings as many of these are, would prove perhaps as deleterious to the inhabitants of the vicinity, human or brutal, as though their destruction had not taken place; not to mention also the necessity of his feeding on some such kind of diet to stamp him as a cold-blooded amphibious animal: otherwise he would not be adapted for the purposes of his creation, nor could he under contrary circumstances feed on the flesh of the venomous classes without deadly hazard.

If the mammoth is allowed to be the natural enemy of

* Asiatic Researches, vol. vi. p. 110.

the serpent tribe, it may be asked, why is he not in existence at this time for the same purpose? why is he peculiar to the new world, while serpents have always alike abounded throughout the globe? and whence comes it that the remains of the mammoth have been usually found in higher latitudes than are congenial to the constitution of the majority of the serpent tribe? In reply to these queries I observe, that we have no proof that the mammoth is not at this present time in existence, although in situations remote from the view of man, as the classes of serpents are also observed to retreat in proportion as the population of the climates they inhabit increases. But admitting that the mammoth has become extinct for some centuries, it argues not against the present conjectures; for like instances have occurred in other classes*, and this especially, as, however important the restraining of the propagation of the serpent kinds might have been in the earlier ages, it has been much less so in subsequent periods, when mankind have little to fear from the ravages of the brute creation.

Yet I confess I suppose the mammoth to be now in existence in the north-west parts of America, and perhaps equally savage and unpeopled regions of the old world; but till forced by famine to quit the accustomed haunts of his prey we must not expect to be visited by him: nor need we wonder that no accounts of such a formidable being have been handed down to us, as it is probable that few, and those perhaps the most ignorant and superstitious of savages of former times, ever witnessed the sight of this stupendous animal.

That the mammoth is peculiar to America is solely a conjecture. Till minute investigation warranted a contrary information, the various huge bones which have been at times discovered in different parts of the world have been uniformly referred to the elephant †; the which, if more carefully examined, would doubtless many of them be found to belong to the mammoth class. The bones of elephants, bears, whales, &c. &c. have been found in territories foreign to their temperament ‡; so also those of the mammoth may doubtless be expected to be found in most climates of the world.

That the remains of the mammoth should be met with in latitudes higher than the usual haunts of his prey, as supposed in the case of serpents, is not to be wondered at; for an animal, unless worn out with age, or prematurely de-

* Plin. Hist. Nat. lib. 8.

† Phil. Mag. vol. xv. p. 327.

‡ Kirwan's Essays, p. 73.

stroyed, must not be expected to die in the midst of plenty. But, in fact, the serpent tribe inhabit every climate more or less: hence it is probable that the rapacity of the mammoth, no longer to be satisfied with the sparing diffusion of the serpent class in the tropical countries, which for a lapse of time had supplied them with their, perhaps, only diet, reduced by their ravages to almost perfect extinction, induced the mammoth to visit regions unfavourable to his constitution, or at least to that of his prey: hence, in process of time, the class would be, perhaps, wholly annihilated through want of food, and the ills of an inhospitable climate. But such as have lately been found on the banks of the Ohio must indubitably have perished there through some sudden and unnatural cause, perhaps a partial deluge or hurricane; for, if an animal dies by natural causes, the carcase remaining on the surface of the earth putrefies and disappears, the skeleton only remaining, which in its turn also moulders away, leaving the teeth, as the portions which longest resist the action of air and moisture: but so small a substance as a tooth of the mammoth, especially if lying in swamps and morasses, may long escape detection, even till such time as the decomposition of its constituent parts leaves no further trace of its original form.

Thus, the mammoth may have existed in the old world as well as the new, although its remains should never have been found in the former: a circumstance, as above affirmed, dependent on the nature of the soil and situation of the place wherein the same may have perished, conjointly with the manner in which its destruction may have happened. Thus, if the period of the mammoth be referred to some centuries back, we cannot expect to find its remains except in situations unfavourable to the process of decay; that is, totally defended from the action of external agents: of course such only must be expected to be met with as have perished in warmer climates by some convulsion of nature, or in those frigid regions where putrefaction is unknown.

But now that naturalists are perfectly convinced of the existence of the mammoth, the remains of huge animals will not, as formerly, be referred to the elephant tribe without more accurate examination; the result of which will, in my humble opinion, prove that the mammoth has existed in most climates of the world; holding a scale in the creation not less beneficial to the animal world at large, than illustrative of those wise dispositions of the Creator which at once draw forth our admiration and gratitude.

Kennington Cross,
June 18th, 1803.

XXVI. *Catalogue of Animals belonging to the Class Vermes, found on the Coasts of Scotland.* By ROBERT JAMESON, F. R. S. F. A. S. Edinb. F. L. S. Lond. *Honorary Member of the Royal Irish Academy, &c.**

THE animals belonging to the class vermes, although holding so low a rank in the animal kingdom, are in many respects deserving of our particular attention: First, they point out to us the intimate connection that exists between the animal and vegetable kingdoms. It has been supposed by many naturalists, that the most perfect plants, and the least perfect animals, form the link by which these two great classes of organic beings are connected together. On a nearer examination, however, we discover that their greatest approximation is in the lowest members of each class; in the vermes of the one, and the cryptogamia of the other.

2dly, By tracing the difference of their structure, from the most simple of all the monas to the more complicated mollusca, many curious anatomical discoveries have been made; and there is reason for believing that the study of their œconomy will throw light on philosophy.

3dly, By an extensive and critical acquaintance with the animals of this class, the geognost will be enabled to determine many of the petrifications he meets with; and this knowledge will also, when other data fail, enable him to determine with certainty to what formation the rocks that contain these petrifications belong.

On the natural history of the British vermes little has been written. Pennant's outline contained in his *British Zoology*, and Ellis's excellent treatise on *Corallines*, are almost the only works that treat professedly on this subject. Dr. Shaw, by means of his interesting work *The Naturalist's Miscellany*, has contributed much in exciting a taste for such inquiries; and it is to be hoped that the concluding volumes of his great work on zoology will be rich in information respecting this interesting class of animals. The chemical analysis of several has been very ably executed by Mr. Hatchet, as detailed in his masterly memoirs in the *London Transactions*.

In the catalogue I now communicate I have only mentioned those species that are rather rare on our coasts.

* Communicated by the Author.

MOLLUSCA.

Tritonia.

T. papillosa. Cuvier. *Doris papillosa* of Syst. Naturæ.
verrucosa. Ib. *verrucosa.* Ib.

Both of these species are found on the rocks on Leith shore.

Doris.

D. argo. Lin.

This beautiful species occurs but rarely on our coasts. I have hitherto only found it on the sea-rocks in the neighbourhood of Leith.

Aphrodita.

A. scabra.
squammata,
imbricata.

Leith shore.
 Ibid. and Orkney islands.
 Ibid. ibid.

Amphitrite.

A. ventilabrum.

On sea-rocks near Queen's Ferry, on the banks of the Forth.

eristata. Muller. Leith shore.

It is interesting to observe the gradation from the coarse tube of the animals of this genus through all the varieties of more regular form observable in other mollusca, and in many insects, to the finished cell of the bee. Dr. Steffens is of opinion that these different figures are caused by the crystallization of the exuded matter. The reader is referred for the proofs of this opinion to his *Beiträge zur Kenntniss der innern Naturgeschichte der Erde.*

Ascidia.

A. rustica. Muller.

Adhering to the roots of the fucus *digitatus*, on Leith shore.

prunum. Id.

Attached to fuci. Ibid.

conchilega. Id.

Ibid.

Actinia.

A. gemmacea. Ellis.

Leith shore.

Pedicellaria.

P. globifera. Muller? On Leith shore, adhering to fuci.

Muller's *globifera* is characterised "capite spherico, collo nullo." The species found on Leith shore wants the neck, and may probably turn out a new species.

Medusa.

Medusa.

M. æquorea. Muller. Seas around the Shetland and Orkney islands.

The animals of this genus possess a powerful stinging quality, and emit a strong alkaline odour. Dr. Steffens conjectures that the stinging quality may be owing to the presence of an uncombined alkali.

Asterias.

A. aculeata. Shetland and Orkney islands.
caput Medusæ. In the sea off the main land. The Argus of the Shetland islands.

Echinus.

E. cidaris. Island of Fula, the most western and recluse of the Shetlands.
placenta. Orkney and Shetland islands.
spatagus. Leith shore.

TESTACEA.

Chiton.

C. fascicularis. Rocks on Leith shore.
ruba. Fabricius. Rocks in the island of Unst, one of the Shetlands.
lævis. Leith shore.
margenitus. Ibid.

Lepas.

L. striata. Pennant. Adhering to roots of fuci, Leith shore.
anatifera. Orkney islands.

Pinna.

P. muricata. Fished up in the sea off the island of Unst.

Dentalium.

D. entalis. Orkney and Shetland islands.

ZOOPHYTA*.

Tubipora.

T. catenularia. Orkney islands.
serpens. Orkney and Shetland islands.
fascicularis. Ibid.

Millepora.

* The animals of this order appear to have been the first that were called into existence. This assertion does not rest on the metaphysical

Millepora.

- M. truncata.* Islands of Unst and Fula.
cellulosa. Orkney and Shetland islands.
pumicosa. Leith shore, investing sertularia.

Cellepora.

- C. pumicosa.* Leith shore, attached to fuci.

Isis.

- I. hippuris.* East coast of Scotland, and Orkney islands, but is very rare.

Gorgonia.

- G. lepadifera.* I have not found this species myself, but have examined a specimen in the museum of the university of this place, which is said to have been taken up on the coast of Aberdeenshire.

Alcyonium.

- A. Schlosseyi.* Leith shore.
Cydonium. Island of Fula.
arenosum. Shaw. Leith shore.
gelatinosum. Ibid.
digitatum. Ibid.

Spongia.

- S. ventilabrum.* Fished up in the vicinity of the islands of Unst and Fula.
infundibuliformis. Island of Unst. Some specimens nearly a foot in diameter.
compressa. Fabricius. Brassay Sound, main land, Shetland.

Flustra.

- F. carbacea.* Pretty frequent at times on Leith shore, but rare in other parts.
hispidula. Pallas. Leith shore, adhering to fuci.
lineata. Orkney islands.
membranacea. Adhering to fuci on Leith shore and the Orkney islands.

consideration of the most simple animals being first created; it has been demonstrated by the observations of the illustrious Werner. This profound naturalist discovered them in the oldest transition rocks, viz. those that lie immediately on the newest primitive coral petrifications. These, therefore, according to the principles of sound geognosy, are to be viewed as the remains of the oldest, and consequently first created beings.

Tubularia:

Tubularia.

T. indivisa. Leith shore and Shetland islands.
 ramosa. Ibid.

Sertularia.

Of this extensive genus a great number of species occur on our coasts. As it may be interesting to some to know the species in this neighbourhood, I shall here enumerate those I have found on the shores of Leith:

S. rosacea.	S. halecina.	S. geniculata.
pumila.	thuya.	dichotoma.
operculata.	falcata.	spinosa.
tamarisca.	antennina.	loriculata.
abietina.	verticillata.	fastigiata?
cupressoides.	cuscuta.	avicularia.
cupressina.	filicula.	scruposa.
argentea.	uva.	ciliata.
rugosa.	lendigera.	eburnea.

volubilis. Orkney and Shetland islands.
 nigra. My friend Mr. Brown, who is now employed in exploring the natural history of New Holland, found this rare species near Aberdeen.

Pennatula.

P. phosphorea. Firth of Forth.
 mirabilis. Muller. Prestonpans bay, Firth of Forth,

Hydra.

H. squammata. Muller. This beautiful species I found on the shore of the island of Burra, and on the Holm of Cruster, in Brassy Sound, in Shetland. It was adhering to the fucus digitatus.

Edinburgh,
 July 19th, 1803.

XXVII. Letter from M. HUMBOLDT to C. DELAMBRES, one of the perpetual Secretaries of the National Institute*.

MY RESPECTED FRIEND, Lima, Nov. 25, 1802.

I HAVE just arrived from the interior of a country where in a large plain I made experiments on the small horary

* From *Annales du Museum d'Histoire Naturelle*, No. 8.

variations of the magnetic needle; and I learn, with regret, that the frigate Astigarraga, which was not to set out before the end of a fortnight, has hastened her departure for Cadiz, and will sail this very night. This is the first opportunity we have had during five months of writing to Europe from the solitude of the South Seas; and the want of time renders it impossible for me to write as I ought to the National Institute, which has given me the most affecting marks of the interest and kindness with which it honours me. A few days before my departure from Quito for Jaën and the river Amazon, I received the letter dated Pluviöse 9th, addressed to me, through you, by that illustrious body. This letter employed two years to reach me in the Cordillera of the Andes: I received it the day after a second excursion which I made to the crater of the volcano of Pinchincha to carry thither a Volta's electrometer, and to measure the diameter of it, which I found to be 752 toises, while that of Vesuvius is only 312. This reminded me that on the summit of Guaguapichincha, where I have often been, and which I love as classical ground, La Condamine and Bouguer received their first letter from the ci-devant academy; and I figure to myself that Pinchincha, *si magna licet componere parvis*, is fortunate to philosophers. It is impossible for me to express the joy with which I read that letter of the Institute, and the repeated assurances of your remembrance. How agreeable it is to know that one lives in the memory of those whose labours continually tend to favour the progress of the human mind! In the deserts of the plains of the Apure, in the thick forests of Casigniare and of the Oronoko, every where, in short, your names have been present to me; and, while reviewing the different periods of my erratic life, I stopped with pleasure on that of the years 6 and 7, when I lived among you, and when Laplace, Fourcroy, Vauquelin, Guyton, Chaptal, Jussieu, Destfontaines, Hallé, Lalande, Prony, and you in particular, loaded me with kindness in the plains of Lieusaint. Receive together the homage of my tender attachment and of my constant gratitude.

Long before I received the letter which you wrote to me in your quality of secretary to the Institute, I successively added to the Class of the Physical and Mathematical Sciences three letters, two from Santa-Fé di Bogota, accompanied with a work on a genus of cinchona; that is to say, specimens of seven kinds of bark, with coloured drawings of these vegetables; the anatomy of the flower so different by the length of its stamina, and the skeletons carefully dried.

dried. Dr. Mutis, who showed me a thousand marks of kindness, and for whose sake I went up the river in forty days, gave me a manuscript of nearly a hundred magnificent drawings representing new genera, and new species of his Flora of Bogota. I have thought that this collection, as interesting to botany as remarkable for the beauty of the colouring, could not be in better hands than in those of Jussieu, Lamarck, and Desfontaines; and I have presented it to the National Institute as a small mark of my attachment. This collection and cinchona were sent off for Carthagena of the Indies about the month of June this year; and Dr. Mutis took upon him to transmit them to Paris. A third letter for the Institute was dispatched from Quito with a geological collection of the productions of Pinchincha, Catopaxi, and Chimborazo. It is distressing to remain under a melancholy uncertainty in regard to the arrival of those objects, as well as to that of the collection of rare seeds, which three years ago I addressed to the *Jardin des Plantes* at Paris.

Want of time at present will not allow me to give you an account of my travels and occupations since our return from Rio Negro. You know that at the Havannah we received the false intelligence of the departure of captain Baudin for Buenos Ayres. Faithful to the promise which I made of joining him wherever I could, and persuaded that I should be more useful to the sciences by uniting my labours to those of the naturalists who accompanied captain Baudin, I did not hesitate a moment to sacrifice the little glory of finishing my own expedition; and I immediately freighted a small vessel to Bataban, that I might proceed to Carthagena of the Indies. This short passage was lengthened more than a month by stormy weather; the winds had ceased in the South Seas, where I expected to find captain Baudin; and I entered on the difficult route to Quito by Honca, Ibagué, the passage of the mountain of Quindin, Popayan, and Pastos. My health continued in a wonderful manner to withstand the change of temperature to which one is exposed in this route, descending every day from snowy regions of 2460 toises in height to scorching valleys where the thermometer does not fall below 26° or 24°. My companion Bonpland, whose knowledge, courage, and immense activity were of great assistance to me in my botanical researches and comparative anatomy, was afflicted for two months with a tertian fever. The season of the great rains came upon us in the most critical passage, the high plain of Pastos, and after a journey of eight months we arrived

at Quito, where we learned that captain Baudin had pursued his voyage from west to east by the Cape of Good Hope. Accustomed to misfortunes, we consoled ourselves with the idea of having made so great sacrifices for an intention of doing good; casting our eyes on our herbals, our barometric and geodesic measurements, our drawings, and our experiments on the air of the Cordillera, we did not regret our having traversed that country, a great part of which has never been visited by any naturalists. We were sensible that man ought never to depend on any thing but what is produced by his own energy. The province of Quito, the highest land in the world, and torn by the grand catastrophe of the 4th of February 1797, furnished us with a vast field for physical observations. Volcanoes so enormous, the flames of which often rise to the height of 500 toises, have never been able to produce a drop of liquid lava; they vomit up fire, sulphurous hydrogen gas, mud, and carbonated argil. Since 1797 this whole part of the world has been in agitation; we every moment experience frightful shocks, and the subterranean noise in the plains of Rio Bamba resembles that of a mountain crumbling to pieces under our feet. The atmospheric air and moistened earth (all these volcanoes are in decomposed porphyry) appear to be the grand agents of these combustions and these subterranean fermentations.

It has hitherto been believed at Quito that 2470 toises is the greatest height at which men could resist the rarity of the air. In the month of March 1802 we spent some days in the large plains which surround the volcano of Antisana at 2107 fathoms, where the oxen, when huted, often vomit up blood. On the 16th of March we found out a passage over the snow, a gentle acclivity, on which we ascended to the height of 2773 toises. The air there contained 0.008 of carbonic acid, 0.218 of oxygen, and 0.774 of azote. Reaumur's thermometer was only $\approx 15^{\circ}$; it was not at all cold, but the blood issued from our lips and eyes. The situation did not permit me to make trial of Borda's compass but in a grotto lower down at the height of 2467 toises: the intensity of the magnetic forces was greater at that height than at Quito in the ratio of 230 to 218: but it must not be forgotten that the number of oscillations often increases when the inclination decreases, and that this intensity is increased by the mass of the mountain, the porphyry of which affects the magnet. In the expedition I undertook on the 23d of June 1802 to Chimboazo, we proved that with patience it is possible to sustain a
greater

greater rarity of the air. We ascended 500 toises higher than Condamine (on Carazon), and on Chimborazo we carried our instruments to the height of 3031 toises, where we saw the barometer fall to 13 inches 11·2 lines: the thermometer was at $1\cdot3^{\circ}$ below zero. We still bled at the lips; our Indians deserted us as usual; C. Bonpland, and M. Montufar, son of the marquis de Salvaigre of Quito, were the only persons who remained. We all experienced an uneasiness, debility, and desire to vomit, which certainly arose as much from the want of oxygen in these regions as from the rarity of the air. At that immense height I found only 0·20 of oxygen. A frightful chasm prevented us from reaching the summit of Chimborazo, of which we were within 236 toises. You know that a great uncertainty still prevails in regard to the height of this colossus, which La Condamine measured only at a very great distance, assigning to it the height of nearly 3220 toises, whereas Don Juan makes it 3380 toises; nor does this difference arise from the different heights which these astronomers adopted for the signal of Carabura. I measured in the plain of Tapia a base of 1702 metres. Pardon me if I speak sometimes of toises and sometimes of metres, according to the nature of my instruments. You know that in publication every thing may be reduced to the metre and centigrade thermometer. Two geodesic operations gave me for Chimborazo 3267 toises above the level of the sea; but the calculations must be rectified by the distances of the sextant from the artificial horizon and by other circumstances. The volcano of Tunguragua has decreased a great deal since the time of La Condamine: instead of 2620 toises I found no more than 2531; and, in my opinion, this does not arise from an error in the operations, because in my measures of Cayambe, Antisana, Cotopaxi, and Iliniza, I seldom differ ten or fifteen toises from the results of La Condamine and Bouguer. The inhabitants of these unfortunate countries all say that Tunguragua has visibly decreased in height: on the other hand, I find that Cotopaxi, which has been subject to such immense explosions, is of the same height as in 1744, or rather somewhat higher. But the stony summit of Cotopaxi indicates that it is a chimney, which resists and retains its figure. The operations we made from January to July in the Andes of Quito gave to their inhabitants the dismal intelligence that the crater of Pinchincha, which La Condamine saw full of snow, burns again; and that Chimborazo, which was thought to be so peaceable and innocent, has been a volcano, and perhaps will one day be so again. We

found

found burnt rocks and pumice-stone at the height of 3031 toises. It will be unfortunate for the human race if the volcanic fire, for it may be said that the whole high land of Quito is one volcano with several summits, should force a passage through Chimborazo. It has often been said that this mountain is granite, but a single atom of it is not to be found: it is porphyry, here and there disposed in columns inclosing vitreous feld-spar, cornceerre and olivin. This stratum of porphyry is 1900 toises in thickness. On this subject I could mention a *polarizing porphyry* which we discovered at Voisaco near Pasto; a porphyry which, analogous to the serpentine I described in the *Journal de Physique*, has poles without attraction. I might mention other facts relative to the grand law of the parallelism of the strata, and of their enormous thickness near the equator: but this is too much for a letter, which perhaps will be lost; and besides, I shall recur to this subject another time: I shall only add, that besides the elephants teeth which we sent to C. Cuvier from the land of Santa-Fé, 1350 toises in height, we have preserved for him others more beautiful; some of the carnivorous elephant, and others of a species a little different from those of Africa, brought from the valley of Timana, the town of Ibarra, and from Chili. Here then we have confirmed the existence of that carnivorous monster from the river Ohio from 50° northern latitude to 35° south latitude. I spent a very agreeable time at Quito. The president of audience baron de Carondelet loaded us with kindness, and for three years I have not once had reason to complain of the agents of the Spanish government, which have every where treated me with a delicacy and distinction of which I must ever retain a grateful remembrance. How much the times and manners have changed! I have paid particular attention to the pyramids and their foundation, which I do not think at all deranged in regard to the mill-stones (*pierres molaires*). A generous individual, a friend to the sciences, and to those men who have done honour to them, such as La Condamine, Godin, and Bouguer, the marquis de Salvaegre, at Quito, thinks of reconstructing them; but this leads me too far.

After passing Assonay and Cuença, where they gave us bull-lights, we pursued our way by the Oxa to complete our labours on cinchona. We then spent a month in the province of Jaén de Bracamorros and among the Pongos of the river Amazon, the banks of which are ornamented with the *andina* and *tuganviltea* of Jussieu. It appeared to me of importance to fix the longitude of Tomependa and Chucungat,

cungat, where Condamine's chart begins, and to connect these points with the coast. La Condamine was able to determine the longitude only of the mouth of the Napa: time-keepers were not then in existence, so that the longitude of these countries requires a great many changes. My chronometer by Louis Berthoud does wonders; as I see by making observations from time to time on the first satellite, and comparing point for point my differences of meridian with those found during the expedition of M. Fidalgo, who by order of the king performed trigonometrical operations from Cumana to Carthagena.

From the river Amazon we crossed the Andes at the mines of Hualgayoc, which produce a million of piastres per annum, and where the mine of gray argentiferous copper is found at the height of 2065 toises. We descended by Casamasca (where in the palace of Atahualpa I delineated the arches of the Peruvian vaults) to Truxilla, proceeding thence by the deserts of the coast of the South Sea to Lima, where for one-half of the year the heavens are obscured by thick vapours. I hastened to Lima, that I might observe there the transit of Mercury on the 9th of November 1802.

Our collections of plants, and the drawings which I made in regard to the anatomy of genera, agreeably to the ideas communicated to me by Jussieu in conversations in the Society of Natural History, have greatly increased by the riches which we found in the province of Quito, at Loxa, at the river Amazon, and in the Cordilleras of Peru. We found a great many of the plants seen by Joseph de Jussieu; such as the *Urochloa affinis*, the *quillajae*, and others. We have a new species of *jussiaea* which is charming, *colletia*, several *passiflores*, and the *loranthus* in a tree of sixty feet of height. We are particularly rich in palms and gramineous plants, on which C. Bonpland has made a very extensive work. We have at present 3784 very complete descriptions in Latin, and nearly a third more of plants in herbals which for want of time we have not been able to describe. Of every vegetable we can indicate the rock where it resides, and the height in toises at which it grows; so that in our manuscripts will be found very correct materials for the geography of plants. To do still better, I and Bonpland have often described the same plant separately. But two-thirds and more of the descriptions belong to the assiduity of Bonpland alone, whose zeal and devotion for the sciences cannot be too much admired. Jussieu, Desfontaines, and Lamarck, have in him formed a pupil
who

who will do great things. We compared our herbals with those of Dr. Mutis, and we consulted a great many books in the immense library of that great man. We are convinced that we have a great many new genera and species, but much time and labour will be required to determine what is really new. We shall bring with us also a siliceous substance analogous to the *tabascher* of the East Indies, which M. Macé has analysed. It exists in the knots of a gigantic gramineous plant which is confounded with the bambou, but which in its flower differs from the *bambusa* of Schreiber. I do not know whether Fourcroy has received the milk of the *vegetable cow*, a tree so called by the Indians. It is a milk which when treated with nitric acid gave me a caout-chouc of a balsamic odour, but which, instead of being caustic and hurtful like all vegetable milks, is nourishing, and agreeable to drink. We discovered it in the road to Oronoko, in a plantation, where the negroes drink a great deal of it. I have sent also to Fourcroy by the way of Guadaloupe, as well as to sir Joseph Banks by Trinidad, our *dapiché*, or white oxygenated caout-chouc, which exudes from the roots of a tree in the forests of Pimichin in the most remote corner of the world towards the sources of the Rio Negro.

I shall not go to the Philippines. I shall proceed to Europe by Acapulco, Mexico, and the Havannah; and I hope to have the pleasure of seeing you at Paris in September or October 1803.

Health and respect,

HUMBOLDT.

P. S. I shall be at Mexico in February, and at the Havannah in June.

XXVIII. *Account of the Marine Spencer for the Preservation of Lives in Cases of Shipwreck or other Accidents at Sea.*
By KNIGHT SPENCER, Esq. of Bread-street, Cheapside.

FOR this invention, which seems likely to be of great utility, the Royal Humane Society were pleased to award to Mr. Knight their honorary silver medallion on the 25th of March 1803.

Explanation.

A (Plate IV.) is a girdle of a diameter to fit the body, six inches broad, composed of about 800 old tavern corks, strung upon a strong twine, well lashed together with lay-cord,

cord, covered with canvas, and painted in oil, so as to make it water-proof.

BB are tapes or cords about two feet long fastened to the back of the girdle with loops at the ends.

C is another tape or cord about three feet long, in the middle of which a few corks are strung, covered with canvas, and painted as above.

D is a pin of hard wood three inches long and half an inch diameter, fastened to the front of the girdle by a tape or cord about two inches long.

E the same.

When the marine spencer is to be used, slide it from the feet close up under the arms; bring the tapes or cords BB one over each shoulder, and fasten them by the loops to the pin D; bring the tape or cord C between the legs, and fasten it to the pin E.

A person thus equipped, though unacquainted with swimming, may safely trust himself to the waves; for he will float head and shoulders above water in any storm, and by paddling with his hands may easily gain the shore.

N. B. A marine spencer constructed as above, and covered with strong canvas unpainted, will have nearly the same buoyancy, though more liable to damage from the effects of sea water.

XXIX. On the Electric Fluid*.

THE hypothesis of the electric phenomena being caused by *two* fluids is now nearly given up for the more simple one of a *single* fluid. Cavallo in his Treatise on Electricity, 4th edit. vol. i. p. 106, 107, says: "When a body does not show any electrical appearances, it is then supposed to contain its natural quantity of electric fluid (but whether that quantity bears any proportion to the quantity of matter in general or not, is uncertain, and therefore that body is said to be in its *natural* or *non-electrified* state): but if a body shows any electrical appearances, it is then said to be electrified, and it is supposed that it has either acquired an additional quantity of electric fluid, or that it has lost some of its natural share. A body having received an additional quantity of electric fluid is said to be *overcharged*, or *positively electrified*; and a body that has lost part of its natural quantity of electric fluid is said to be *undercharged*, or *nega-*

* Communicated by a Friend to Physical Inquiries.

tively electrified." This is, it is believed, the theory now generally adopted by electricians, and is probably true as far as it goes: but the writer of this paper is of opinion that the electrical effects do not only depend on an *additional* quantity in one case, and a *loss* of the fluid in the other, but on the state of *condensation* (including in this term *compression*) and *rarefaction* (including *dilatation*) of the fluid, the *condensed* state being what is usually called the *positive* or *plus*, and the *rarefied* the *negative* or *minus* state. If the electric fluid be, as is commonly thought, elastic, there is little doubt but that, when an additional quantity is thrown on to a body, it suffers *compression*; and, on the contrary, when a portion is taken away, that the remaining fluid is *dilated*. With non-elastic substances a state of *condensation*, or rather *density*, may exist without *compression*; and, on the contrary, *rarefaction* without *dilatation*, if the particles of such matter are of different sizes; for suppose a box were filled with wooden balls of an inch diameter, there would be many vacancies between, which might be filled up with smaller balls, and the bulk not increased: here would be *density* without *compression*: take the small balls away, and there will be *rarefaction* without *dilatation*. This case we can hardly suppose to exist amongst elastic substances, though certainly possible, if each of the particles were at their greatest state of *compression* or *dilatation*, and were of different magnitudes.

In the Philosophical Magazine, vol. xii. p. 186, an optical experiment was suggested which might perhaps give some insight into the two electric states: hitherto there do not seem any *decisive* experiments which determine the *state* or the *course* of the electric fluid. In order to illustrate the hypothesis here advanced, let the conductor which is connected to the cushion, commonly called the negative conductor, and the other, or positive conductor, be compared to the receiver of a common (rarefying) air pump and the receiver of a condensing machine (or condensing air pump, as it might be called), both united, as they sometimes are, on one frame: here, whilst the air in one receiver is rarefied, in the other it becomes condensed. It is, according to the opinion now brought forward, the *equilibrium* of *condensation* and *compression*, not simply of *quantity*, which produces the shock, the spark, and other electrical effects.

Would not the terms *supplying conductor* and *receiving conductor* be preferable to *positive* and *negative conductor*? Perhaps electrical *supplier* and *receiver* alone would be sufficient when speaking of the machine.

XXX. *Proceedings of Learned Societies, Societies of the Arts, &c.*

BATAVIAN SOCIETY OF THE SCIENCES AT HAARLEM.

THIS society held its fifty-first yearly general meeting on the 21st of May, and, after examining the answers sent in to prize questions since the last meeting, resolved to continue the following to which no answers had been received :

1st, What light has the new chemistry thrown on our knowledge of the nature of the human body ?

2d, How far have physicians, in consequence of the light thrown on the nature of the human body by the new chemistry, become better acquainted than before with the nature and causes of certain diseases ? and what consequences, more or less confirmed by experience and useful to the practice of medicine, can be thence deduced ?

3d, How far has the new chemistry made physicians fully acquainted with the action of certain medicines which have either been long used or lately recommended ? and what advantages arise from this knowledge in the treatment of certain diseases ?

As some celebrated philosophers, in their application of the fundamental principles of the new chemistry to the knowledge of the human body, to diseases and to the cure of them, have indulged too much in hypothesis not founded on experience ; and as great injury must thereby arise to the practice of medicine, to which the new chemistry seems to promise so much advantage, provided Lavoisier's rule of admitting nothing in it which is not founded on experience be observed ; the society requires that, in answering these three questions, a distinction will be made between what is fully proved and what rests on weak grounds, and that the uncertainty of the latter only will be briefly pointed out.

4th, How far are the causes of the corruption of stagnant water known ? And from what is already known and can be proved on the subject, what are the best and least hurtful means which can be employed to preserve stagnant water from corruption ?

The answers to these questions are to be sent in before the 1st of November, 1804.

The following questions are again proposed to be answered before the 1st of January, 1804 :

A natural history and description of whales, to serve as the means of tracing out the places where these animals are to be found ; together with the safest and best methods either

either already known or practised, or which can be brought into practice, to kill them with the greatest speed and safety.

To be answered before the 1st of November, 1804 :

I. How far can the meteorological observations made in the Netherlands serve for acquiring a knowledge of the nature of the winds of these countries? which are the most prevalent winds? what is their regular or general succession? from what previous circumstances can the inhabitants on some occasions, and with any degree of certainty, foretel changes of the winds? and what influence have they in general or sometimes on changes of the weather?

II. An accurate nomenclature of the mammalia, birds, and amphibia, not formerly introduced, which are found in the Netherlands, with their synonyms and the characteristic marks of the species and genera arranged according to the Linnæan system, with an indication of one or more of the best figures of each animal.

The following is proposed in consequence of the fund established by the late director, N. W. Kops :

III. As it is of great importance to have the principal truths of it briefly and clearly exposed, the society requires that, from the great number of writings on the action of the Voltaic pile, whether given in journals or other periodical works, an essay may be composed containing an account of what has been learned in regard to the Voltaic pile and the experiments made with it.

The following questions are still proposed to be answered under the usual conditions :

For an unlimited period.

I. What have we been taught by experience in regard to the utility of some animals which appear to be noxious, and especially in the Netherlands? and what precautions are to be employed in order to extirpate them?

II. What indigenous plants, hitherto not tried, can be employed with advantage in the materia medica in the room of exotic plants?

III. What indigenous vegetables, never yet used, can be employed as wholesome and cheap food? and what foreign vegetables, not used in this country, can be introduced for the same purpose?

IV. What indigenous plants, not yet used, are capable, according to well established proofs, of furnishing good dye-stuffs, which can be prepared and employed with advantage? and what exotics, fit for the same purpose, can be introduced and cultivated on waste land with advantage?

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The answers, written in the usual manner in Dutch, French, Latin, or German (but not in German characters), accompanied with a sealed billet containing the author's name, must be transmitted (post paid) to M. Van Marum, secretary to the society.

The prize for each of the above questions is a gold medal, with the usual impression of the society, and the name of the author and the date on the edge, or thirty ducats, at the option of those to whom the gold medal is adjudged.

CHALCOGRAPHIC SOCIETY, LONDON.

It must be highly gratifying to the lovers of the fine arts, to those who know their powerful influence on the public mind, to be informed that a society of engravers has been formed under the immediate patronage of his royal highness the prince of Wales. To alleviate the misfortunes and miseries resulting from sickness and the decays of nature, is the immediate object of the institution; and we have no doubt but the funds arising from the subscriptions of artists, aided by the support which it merits, and will no doubt obtain, not only from the lovers of the arts, but from those who entertain liberal and enlarged views of the true interests of their country, will prove equal to the support of those who, in spite of their exertions, may need such aid.

To the public this institution promises the most solid advantages; advantages impossible to be commanded by any other means. Talent, energy, and power, with the peculiar acquirements and excellencies of each artist in each line, united and concentrated in single productions, must give them a degree of excellence which could never be obtained from insulated talents, however splendid, and must increase in proportion the commercial interests of the country.

The institution had its first anniversary dinner on Wednesday, the 6th of July, at the Crown and Anchor, in the Strand. It was numerously and respectably attended, and the afternoon was spent amidst that social enjoyment which arises from a mutual interchange of sentiments enlivened by genius, and polished and corrected by taste. Great harmony prevailed during the whole evening, and the company separated highly gratified with the result of their meeting, and animated by the hope of that success to which, by their laudable exertions, they are so well entitled.

The following is a list of the officers and committee for the present year:

Francis Bartolozzi, R. A. engraver to the king, president.

Peltro William Tomkins, esq. historical engraver to the queen, vice-president.
 Anker Smith, esq. A. R. A. treasurer.
 Abraham Raimbach, esq.
 Charles Warren, esq.
 James Parker, esq.
 Cosmo Armstrong, esq.
 Robert Mitchell Meadows, esq.
 Thomas Medland, esq.
 James Fitler, esq. A. R. A. naval engraver to the king.
 Thomas Milton, esq. honorary secretary.
 Wilson Lowry, esq. } auditors.
 John Samuel Agar, esq. }
 Philip Hammersley Leaths, of the Middle Temple, honorary consulting member.

FRENCH NATIONAL INSTITUTE.

Notice of the Labours of the Mathematical and Physical Class of the Sciences since the last Public Sitting.

The name of tautochrones is given to curves in which the oscillations of a heavy body are always of the same duration, whatever may be their extent. Tautochrones have been rendered celebrated by the labours of the greatest geometers, who have successively endeavoured to overcome the difficulties exhibited by the different hypotheses which may be formed in regard to the laws of gravity and resistance. But though their formulæ had all the generality possible, they sought only for plane tautochrones, while for each hypothesis there exist an infinite number which are of a double curvature.

An examination of these new tautochrones, and their relation with plane tautochrones, form the object of a memoir by C. Biot. From a consideration of the formulæ the author has deduced some theorems remarkable for their simplicity. But whatever may be their elegance or novelty, we shall not enter into any detail respecting a matter so abstract, that Bossut in his *Essay on the History of the Mathematics* thought it his duty to give an exculpation of those geometers who have employed their powers and their genius on these problems, which are merely theoretical. Nothing can be added to the solidity of the reasons which he adduces in favour of these speculations, which on the first view appear barren, but which in the end may be applied to useful purposes. But this may be rendered more sensible by a striking example. When the ancient geometers were endeavouring with so much care to disco-

ver all the properties of the conic sections, and when Apollonius made them the subject of a profound treatise, the books of which have been long regretted, and which have not been handed down to us; might not the same reproach of the loss of time in meditations which might have been better employed, have been addressed to them? Who could then foresee the numerous applications which have been made of these curves to several branches of the mathematics? and who could suspect that the ellipsis is the figure of all the planetary orbits?

Of the most useful elementary problems, none has received more solutions than that the object of which is to correct the apparent distances of the moon from the sun and stars, in order to deduce from it the longitude at sea. This problem is not indeed very difficult; but it is of daily use, and those who have occasion to employ it are not always well versed in calculation: on this account, the easiest approximations sufficiently exact for practice have been substituted for the rigorous methods. The subject was thought to be exhausted, and yet very simple considerations, which never before occurred to any one, have furnished C. Legendre with an entirely new solution. His formula possesses a remarkable symmetry, which serves to engrave it on the memory. Nothing was wanting but a little brevity in the calculation; and the author has found means to give it this merit by including in two tables several terms, the suppression of which shortens the operation a third. Other known formulæ possessed the latter advantage; but the new solution has, above all others, the merit of elegant symmetry; which ought to be considered of great importance, since it contributes to facilitate the operation.

If the phænomena of the tides were subjected only to the combined action of the sun and moon, they might be predicted with the same precision as the celestial phænomena. With a few data obtained by observation, one might announce before-hand, both the exact moment and the precise elevation of the waves. The action of the winds, which will doubtless always escape our calculation, may serve indeed to account for the principal and periodical causes of the tides: but it at least modifies their effects; it can increase or diminish, accelerate or retard them; and if the sun and moon shall happen to be so placed as to produce the strongest tide, and if the wind conspire also to raise the waters, they may then produce extraordinary inundations, of which it is of importance to be forewarned

in order that the necessary precautions may be taken. It is on this account that for some time the *Connoissance des Temps* has announced for all the new and full moons the force of the tides, abstracting from every local and accidental circumstance. The tides of Ventose and Germinal last were announced as about to be the highest in the course of the year. They attracted the attention of observers and of the curious. If the expectation of the latter was not fully answered, the former had reason to be more satisfied. These tides, indeed, were the highest of any remembered: but the atmosphere was calm; and consequently none of those accidents, the possibility of which only was foreseen, could take place. C. Rochon communicated to the class what he observed at Brest, and C. Septfontaines has transmitted to us what he saw at Calais. Their notices induced C. Laplace to read a memoir; at the conclusion of which the class, sensible of the necessity of a series of observations made at different ports, and according to an uniform method, appointed a commission charged with drawing up instructions proper for serving as a guide to observers.

The report of the commission has been printed, to be distributed in the ports. The ministers have promised to give their orders; and the series of observations destined to make known what part of the phenomena of the tides depends on periodical and general causes, and what depends on local or accidental causes, will soon be begun.

The new planets discovered by Piazzzi and Olbers continue to engage the attention of astronomers. Notwithstanding the smallness of the arc which they have passed through in our sight, and notwithstanding the considerable perturbations which they experience from Jupiter, we have already obtained the elements of their orbits with sufficient precision to find again these bodies in the place indicated by calculation, when they become visible, after having been several months lost in the rays of the sun. The greatest difficulty arises from their extreme smallness, which sometimes causes us to doubt whether we have them in the field of the telescope. This is true in regard to Pallas in particular, which appears sometimes like a star of the 10th and 11th, or even the 12th magnitude, while Ceres appears of the 7th or 8th. But as there is something too arbitrary in this distribution of the stars according to the order of their magnitudes, it will be better to say with Messier, that Pallas is the smallest object that can be distinguished with an excellent telescope.

An extraordinary circumstance has given to this imperceptible star for a moment a more sensible diameter and a stronger light. On the 28th of May, the weather being very fine, C. Messier was surprised to find in it a light double to that which it had before; and yet, according to calculation, the distances of the sun and moon being nearly the same, the brightness of the planet ought not to have changed. The cause of this appearance was soon discovered. The small planet in its course met with a star, to which it appeared to be so close that the least interval could not be observed between them. Forty-two minutes after, a separation took place, and, according to the known motion of the planet, the interval must have been 15". The position of the small star may be determined at leisure; and from the repeated observations which may be made of it, there will result for the moment of the observation of Messier a determination of the place of the planet more exact than any of those which could have been procured in a direct manner. Those observations known under the name of *appulses* are exceedingly rare. However numerous the small stars may appear, the intervals which they leave between them are sufficiently large for the planets to make the tour of the heavens without concealing one of them, or at least any of those which can be observed. The moon, however, ought to eclipse some of them every day: but their faint light becomes extinct on the approach of a stronger light; and the observation of these eclipses is too difficult and too uncertain to induce astronomers to attend to them: they pay no attention but to stars of the 4th or 5th magnitude, and below.

The arc of the meridian employed by the French astronomers for determining the fundamental unity of the metric system was the greatest ever measured. C. Mechain, during his residence in Spain, remarked that it could still be extended two degrees by forming two triangles, which touching the coast of Spain between Barcelona and Tortosa should terminate at the islands of Majorca and Ivica. The difficulty was to measure the angles, and perceive in a telescope not half a metre in length signals at the distance of two hundred miles. These observations could succeed only under the most favourable and consequently the rarest circumstances; they could not be attempted but in the middle of winter, and then they could have been attended only with half success. C. Mechain found himself obliged to abandon a project highly interesting, the plan of which was already drawn up. The reciprocal dispositions

tions of France and Spain were not then sufficiently amicable to allow us to flatter ourselves with the hope of obtaining that aid and concert indispensably necessary for operations so difficult; but these dispositions having happily changed, the French government gave orders for the continuation of our meridian to the Balearic islands. C. Mechain is already at Barcelona with instruments the best suited to the difficulty of the observations, which will be begun as soon as he has concerted the proper measures with the Spanish commissioners. This new enterprise promises two advantages. The first is, that it will add two degrees to the arc already measured, which will be sufficient to indemnify us for the time and labour it may cost. Another advantage, still more important in the eyes of some persons, is, that we shall have a whole arc equally divided into two in the parallel of 45° , and from which, without any supposition on the figure of the earth, we may deduce the whole extent of the meridian.

The fame of these operations, of which France set the example, has more than once excited the emulation of neighbouring nations. Hence, after the measurements performed by the French in Peru, at the polar circle, in France itself, and at the Cape of Good Hope, we have seen degrees measured at Rome, Turin, Vienna, Hungary, Pennsylvania, and Milan. The Swedes have lately repeated and extended with instruments made in France, and with all the means furnished by the present state of the sciences and the arts, the operations performed in 1736 at the polar circle. The details of the new measurement have not yet been published: but we learn by letters from M. Melanderhielm, perpetual secretary of the Academy of Sciences at Stockholm, and the projector of the new operation, that the conclusions deduced from it do not accord with what resulted from the first. The latter gave a degree which was considerably different from all the rest, and supposed so great a flattening, that it excited some suspicions in regard to the exactness of the measures. The new one reconciles the whole. This degree, compared with that of France, gives for the flattening nearly the same quantity as the degree of France compared with that of Peru. This result would be so satisfactory that we scarcely dare to give credit to it. There were some doubts in regard to the correctness of the operations performed in 1736; but the error which it would be necessary to acknowledge in it far exceeds the limits in which it was supposed to be included.— Until the publication of the labour of the Swedes has produced

duced complete conviction, we have reason to think that the irregularities of our globe are not so great as has hitherto been believed, and that the curve of the meridian, abstracting some local circumstances, deviates much less from the regular elliptical figure.

It is acknowledged by the ablest naval officers that the port of Brest in time of war cannot be supplied with provisions and stores by sea, and the burthensome method of employing carts can be resorted to only on the most urgent occasions. The counsellor of state Bruix has already proved in a printed memoir the indispensable necessity of an internal communication between Brest and the Loire. Boats of the burthen of ten tons at most, and a canal, would be sufficient for the continual wants of the navy. C. Rochon, who has been long occupied with projects of internal navigation proposed to the states of Brittany, has given more extent to his ideas in a memoir which he read to the class. He shows in what manner a highly useful communication might be established between Nantes, L'Orient, and Brest, by rendering navigable the rivers Erdre, Isac, Ourt, Blavet, and Châteaulin.

We shall take this opportunity to say a few words respecting some experiments lately made by C. Rochon with a telescope, of which he gave a description, with an account of its uses, in a memoir printed in the year 9.

It is well known that rock crystal has the property of double refraction, and of producing two images. This property C. Rochon has found the ingenious means of converting to advantage. A prism of this crystal placed in the interior part of a telescope causes two images of the observed object to appear, and these images approach or recede from each other according as the prism is brought nearer to, or removed from, the eye. When the images are brought into contact, a scale engraved on the outside of the telescope indicates to the observer how many times the diameter of the observed object is contained in the distance. Hence, when the distance is known the diameter can be determined, and the diameter when known will give a sufficiently correct idea of the distance. If you observe a ship at sea, which you wish to come up with or avoid, bring in contact the two images; if you are approaching the vessel the two images will encroach on each other: but, on the other hand, if the two vessels are receding from each other, the images will be soon separated. It is easy to distinguish the rate of the observed vessel, and by these means you may know very nearly the dimensions of her masts: bring

into contact, and end to end, the two images of the main mast, and you will know how many lengths of that mast you are distant from the vessel. At land, you observe the images of a corps of the enemy, and place these images in such a manner that the feet of the one may stand on the heads of the other; and if you estimate the mean height of a soldier at five feet ten inches, the telescope will show how many times that quantity is contained in the distance you are from the enemy.—This short explanation sufficiently proves of what utility this instrument may be, and which would even be interesting were it only an object of mere curiosity. Experiments on this subject were repeated at Saint-Cloud on Tuesday, the 31st of May, before the First Consul, who ordered several telescopes of this kind to be constructed. This discovery may be of great use also to astronomy. C. Rochon has already employed it to measure the diameters of Mars, Jupiter, and Saturn. At first he was not able to apply it to the sun and moon, the diameter of which is about 30', because the angle of refraction is only 20'; but Rochon and Torelli de Nancy, by cutting the crystal in an ingenious manner, were able to double and even to triple the angle of refraction. There is no planet, therefore, the diameter of which cannot be measured in this manner, provided it be sufficiently luminous; for it is evident that the two images are necessarily weaker and fainter than one image would be. There is no inconvenience of this kind in regard to the moon and sun, which have always too much light; and one of these prisms is going to be adapted to the best telescope in the national observatory.

XXXI. *Intelligence and Miscellaneous Articles.*

NEW FULMINATING POWDER.

PROFESSOR PROUST, in a letter to A. I. C. Delametherie, says: "I have made in the course of my lectures an experiment which had almost been attended with serious consequences to my auditors. It is not exaggeration, perhaps, to say, that a mixture of oxygenated muriate* with arsenic takes fire with the rapidity of lightning. I am accustomed to compare the duration of different kinds of

* The *Journal de Physique*, from which we have taken this notice, does not say what oxygenated muriate was employed.

powder by burning them in cannons of equal diameter, which are cased in cork, so that they may be afterwards placed in water and immersed almost entirely lengthwise. I was desirous of burning in the same manner an arsenical mixture along with an equal quantity of the same powder, that I might compare their flames and duration : but scarcely had I time to retire a little, after having set fire to the two cannons, when the first made an explosion and burst, breaking the glass vessel, and throwing the water in all directions like radii from a common centre. Though the mouth of the tube was not confined, so that the escape of the flame was free, the cannon split from the top to the bottom, and unrolled itself like a card. This powder is so violent in its effects that in my opinion it would be dangerous to try to make any application of it. If two long trains, one of gunpowder and the other of the above mixture, be formed on a table, and if they be made to coincide at one extremity in order to set fire to them at the same time, you will be astonished to see the one disappear like lightning, while the other seems to burn exceedingly slow.

PREPARATION OF A FULMINATING OXALATE OF SILVER.

Extracted from a Letter of Brugnatelli.

Take 100 grains of lapis infernalis in powder, and having put them into a beer glass pour over them first an ounce of alcohol, and then as much concentrated nitrous acid. The mixture becomes heated, enters into ebullition, and there is visibly formed ether, which is changed into a gaseous fluid. The matter gradually becomes milky and opaque, and is filled with small very white flakes : when the whole gray powder of the lapis infernalis has assumed this form, and when the liquor has acquired consistence, you must immediately add distilled water to suspend the ebullition, and to prevent the matter from being re-dissolved, so that nothing may be found but the solution of silver. Then collect the white precipitate on a filter, and suffer it to dry. This precipitate is fulminating silver : a little more than half the weight of the lapis infernalis employed is obtained. The detonating force of this preparation even in a much smaller quantity far surpasses that of fulminating mercury prepared according to the process of Mr. Howard. It detonates in a terrible manner when scarcely touched with a glass tube the extremity of which has been dipped in concentrated sulphuric acid, or even that of the shops. A grain of this fulminating silver put upon a burning coal made so loud a report that it stunned the by-standers. The same effect

effect was produced by putting a little of the same preparation on an electric pile, with a piece of paper interposed, and making a spark pass through the middle of it by means of a metallic plate: the paper will be either perforated or torn.

NEW METHOD OF PREPARING FULMINATING MERCURY.

By Brugnatelli.

The process described by Mr. Howard for obtaining fulminating mercury, which I have several times prepared in my laboratory, suggested to me the idea of preparing this singular production by making ether by nitric acid on mercurial oxides without the application of heat, which frequently produces so rapid and so spontaneous an effervescence that it occasions the loss of a great part of the materials. My process is as follows:

On two gros of the oxidulous sulphate of mercury called *turbith mineral* pour an ounce of pure alcohol, and add at two different times ten gros of concentrated and rutilating nitrous acid. The mixture will immediately enter into effervescence, and the alcohol becomes etherized and is reduced to vapour. These vapours are at first rare and light, but they gradually become more copious and dense: they pass into the receiver, whence they issue in clouds and occupy the lower parts of the apartment, diffusing themselves in the air to a great distance. I never saw any effervescence produce so many vapours. The mercurial mass loses its yellow colour and becomes gray. When the matter has been suffered to cool, collect the concrete part on a filter, and wash and dry it. This substance fulminates like Howard's mercury, and possesses all the other properties of that preparation.

By treating in the same manner the other mercurial oxides, I was able to convert them all into fulminating mercury.

The quantity of white vapours disengaged during this process, and their singular gravity, which far surpasses that of air, induced me to make researches respecting their nature. For this purpose I proceeded in the following manner:—I put into a glass tubulated retort two gros of *turbith mineral*, adapted to it a double-necked retort, and to the latter a second large tubulated receiver. Having luted the joinings, I poured over the mercurial powder in the retort an ounce of alcohol, and then a gros of concentrated rutilating nitrous acid, closing exactly the tubulature. An effervescence took place, and the white vapours soon filled the lower part of the receivers. When the effervescence had subsided,

subsided, and the apparatus had cooled, there was found in the retort a mercurial mass of a very white colour and highly fulminating. The receivers contained an ethereous oily liquor, and crystals of salt, which were carefully collected, were observed on their sides. They were exceedingly soluble in water. On examination I found them to be nitrate of mercury.

The vapours disengaged during this operation are therefore composed of ether by nitric acid and nitrous gas holding in solution nitrate of mercury. The latter circumstance, on which the great gravity of these vapours depends, is truly worthy of attention. I was not able to ascertain whether there was a concomitant formation of ammonia, as Berthollet supposes.

DISCOVERY OF A DETONATING OXYGENATED MURIATE OF LEAD. *By the same.*

I obtained an oxygenated muriate of lead by saturating with oxygenated muriatic acid a solution of lead in nitric acid. This solution, which was transparent and colourless, acquired by the addition of oxygen gas a yellow colour. I reduced it to two-thirds by slow evaporation, and left it at rest. By cooling, it gave a quantity of very small brilliant cubes, having a sweet and cool taste. These crystals attract humidity in humid air, and become dry in dry air. Sulphuric acid decomposes them with a disengagement of simple muriatic acid gas: when struck with a small bit of phosphorus they detonate violently, and are fused on burning coals.

If simple muriatic acid gas be made to pass into a solution of muriate of lead impregnated with a little oxygenated muriatic acid gas, the metal is precipitated under the form of very small crystals of muriate of lead: on evaporating the liquid there are obtained small needles of the same salt. Crystallization then furnishes the two salts above mentioned. Simple muriate of lead neither detonates with phosphorus, nor fuses on burning coals.

NEW ANIMALS.

A letter from Paris says: "A couple of living quadrupeds, entirely unknown to naturalists, have been sent home by captain Baudin, in the *Naturaliste* lately arrived. Professor Geoffroi calls them *fasciomes*. They come from the western coast of New Holland; their fur may be of some utility; and their flesh, in the opinion of captain Hamelin and his crew, affords excellent food. The *fasciomes*

Lomes are interesting to naturalists in particular on account of the singularity of their organization. They resemble the marmot in the form of the head; the number, the nature, and arrangement of their teeth; and by the conformation of their fore-feet, which they employ for burrowing in the earth: but they differ by the existence of a bag under the belly of the female, and by the whole apparatus of the organs of generation, which are like those of the *sarique* of Buffon. The hind-feet also are formed like those of that animal, the thumb being separated from the other toes, and destitute of claws. The tail is so short that it remains concealed among the hair: the latter is brown, tufted, and very long.

“The *fascolomes* in the menagerie are still young, and larger than rabbits. They are remarkably gentle. They may be touched, and carried from one place to another, without showing the least fear, anger, or discontent. Their gait is heavy and embarrassed: they live under the earth, sleep in the day-time, and in the night go in quest of food: in general they have little activity and energy: they scratch themselves like apes. They are fed with bread, milk, roots, and all sorts of herbs.”

GALVANISM.

A letter from Turin, dated 25th June, contains the following particulars:—“The experiments made by the Galvanic Committee of Turin, and by several members of the Academy of Sciences, have greatly contributed to the rapid progress made by this part of the physical sciences: but what ought to distinguish the success of the learned philosophers of which this committee is composed, is the advantage with which they have applied Galvanism to the animal œconomy, and the well-arranged series of experiments they have made to determine its influence on the different diseases with which man may be afflicted. Of the various trials made with great success by Rossi, Vassalli-Eandi, and Giulio, we shall mention that only of Rossi with Galvanic piles of a new composition. Animated by the most ardent desire of rendering Galvanism useful to suffering humanity, Rossi constructed disks with the cancerous tumours extirpated from a man in the hospital of St. John. He varied this apparatus by employing these new disks sometimes without moistening them, and sometimes moistening them in water mixed with a tenth of its volume of oxygenated muriatic acid. He compared the results produced by these two different piles with those obtained by means of the common pile, and by
a fourth

a fourth and fifth pile composed one of disks of flesh suffered to putrefy in the sun for fifteen days, and the other formed of disks of the same substance moistened in oxygenated muriatic acid mixed with nine parts of distilled water. The results of these experiments, by demonstrating more and more that the Galvanic fluid, drawn from the different substances which compose the pile, has the power of carrying with it, in its circulation, different matters analogous to the respective bodies through which it passes, have induced Rossi, Vassalli-Eandi, and Giulio, to conclude,

1stly, That Galvanism, though arising from electricity, which, as we may say, is its basis, is not simple electricity, but electricity so modified, that its effects are in no manner similar to those of electricity properly so called.

2dly, That the oxygenated muriatic acid combined with distilled water, in proportions always determined by the different cases in which it is employed, may be used with the greatest advantage in the cure of various maladies.

The latter discovery made by C. Rossi has been applied by him with the completest success. He has employed oxygenated muriatic acid externally in the manner above mentioned for the cure of very extensive gangrenous ulcers. The effects of this new remedy have been exceedingly great, both in the hospital of Moncalieri, where several individuals, treated without success by the common means for several months, were entirely cured by this method; and in the hospital of St. John, where the effects were so speedy, that in the course of twenty-four hours gangrenous ulcers of the legs were reduced to the state of simple ulcers.

ELECTRICITY.

Vassalli-Eandi has confirmed, by a series of experiments publicly repeated before Lelievre the celebrated mineralogist, member of the National Institute of France, what he had proved in the year 1790,

1stly, That metals and their oxides, thrown on his electrometer, bring thither a contrary kind of electricity: the metal positive electricity, and its oxide negative.

2dly, That the electric fluid does not affect the fluid of the Voltaic pile, the action of which is not altered by the union of positive electricity to the negative of the pile, nor by another combination of electric and Galvanic conductors. It is from these and other experiments of the same kind that he has deduced the theory of Galvanism and its effects, which he explained in the last sitting of his public experiments at the Athenæum of Turin.

EXPEDITION OF CAPTAIN BAUDIN.

Copy of a Letter addressed to C. Gregoire, Member of the French Senate.

Straits of Basle, King's Island,
Nov. 27th, 1802.

You have certainly heard the result of the expedition of captain Baudin since our departure from France. You must have seen that, after staying forty days at the Isle of France, we proceeded to the coast of New Holland, which we explored from Cape Lewin to the Bay of Seals. All this sandy coast, nearly destitute of fresh water, is almost uninhabited. The few inhabitants who reside on it are still as savage as in the time of Dampierre. Being nearer to the state of nature than any other people, they are almost as fierce, and possess no art except that of sharpening sticks to defend themselves from their enemies, or to procure those provisions with which they can be supplied either by hunting or fishing.

On quitting this coast we proceeded to Timor, one of the islands to the south of the Moluccas, where we found a mild and lively people in a state of demi-civilization. The inhabitants of the coast have become hospitable towards strangers by their commerce with the Dutch; but those who reside in the interior, to whom an European visage is still unknown, behave with cruelty towards those who venture to penetrate into their country.

From Timor we directed our course to Van Diemen's Land. This island is inhabited by people of a different race from those of New Holland. The latter have long hair like the Asiatics, though their skin is as black as that of the African negroes, while the former have woolly curled hair like the inhabitants of Congo. The difference between them is shown by other characters also: the former, habituated to see European vessels from time to time on their coasts, are less savage than the other tribes of these countries.

From Van Diemen's Land we proceeded to Port Jackson. This infant colony is the first the inhabitants of which have no cause of complaint against the Europeans. Here they are treated with every kind of attention, but they have always rejected civilization: though they have lived for fifteen years with the English they have not yet adopted any of their customs. Clothes are still to them a superfluity: they sometimes, though seldom, wear any thing to defend them from the cold, but never to conceal their nudity. Their language alone has undergone some changes.

The English, in the course of the fifteen years they have been settled here, have carried cultivation to a very advanced state. The antient forests have disappeared, and nothing is seen but fields of wheat, which are remarkably productive. We have found here towns and villages where every European article, and even superfluities, are to be had in abundance. The population amounts to nearly 8000, without any slaves. I have sent you a specimen of the wool of the sheep of this country. They came originally from Peru, Paraguai, the Cape of Good Hope, and Bengal. They have already improved in a singular manner, and still promise more. Those of Bengal, which were clothed only with hair, have already produced young ones covered with rich fleeces. A residence of five months enabled me to traverse the whole country. We have just left it in order to explore those parts of New Holland which we have not yet visited. The commodore has sent the *Naturaliste* back to France laden with the collections we have made. I have left that vessel to go on board the *Geographe*, and to supply the place of my friend Depuch, who returns to France on account of his health.

BAILLY, Mineralogist.

FIRE-BALL.

On the 4th instant (July) a ball of fire struck the White Bull, public-house, kept by John Hubbard, at East Norton. The chimney was thrown down by it, the roof in part torn off, the windows shattered to atoms, and the dairy, pantry, &c. converted into a heap of rubbish. It appeared like a luminous ball of considerable magnitude; and, on coming in contact with the house, exploded with a great noise and a very oppressive sulphureous smell. Some fragments of this ball were found near the spot, and subjected to chemical analysis by a gentleman in the neighbourhood, who found them to consist of the same ingredients as those stones of similar origin analysed by Mr. Howard and other chemists, and nearly in the same proportions. The surface of these stones is of a dark colour, and varnished as if by fusion. From some indentures on the surface it appears probable that the ball was soft when it descended; and it was obviously in a state of ignition, as the grass, &c. is burnt up where the fragments fell. Its motion while in the air was very rapid, and apparently parallel to the horizon.

SINGULAR

SINGULAR CAVERN.

A singular cavern has lately been discovered two leagues from Nizza, in the district of Falcion. It has a narrow entrance, and the interior divisions, which have not yet been sufficiently explored, contain natural temples supported by columns formed by crystallizations. Some of these columns are so large that 400 persons cannot encompass them. On account of the great reflection, very little light is required to illuminate this cavern.

ASTRONOMY.

The astronomers Delalande junior and Burckhardt observe with great assiduity the planet discovered by Dr. Olbers on the 28th of March 1802. Its longitude on the 1st of July at 11^h 45' was 9 signs 7° 14' 25", and its latitude 46° 23' 18". Burckhardt has thence deduced its revolution to be 1682 days, or four years seven months and twelve days; which is a day less than what was found some months ago, as may be seen in my *Bibliographie Astronomique* just published; but at present there is scarcely an uncertainty of a few hours. He is employed in calculating the derangements it must experience from the attraction of Jupiter, and which are very complex; but he has presented to the Institute a learned memoir which leads to this research.

JEROME DELALANDE.

Table of the geocentric motions of the two new planets for the month of August:

	A. R. of Pallas.	Declin. North.	A. R. Ceres Ferd.	Declin. South.
Aug. 4	17 ^h 59 ^m 52 ^s	19° 3'	18 ^h 15 ^m 52 ^s	29° 48'
	7 17 58 44	18 31	18 14 32	29 51
	10 17 57 48	17 59	18 13 20	29 55
	13 17 56 52	17 27		
	16 17 56 20	16 53		
	19 17 55 56	16 19		
	22 17 55 44	15 44		
	25 17 53 40	15 9		
	28 17 53 48	14 33		

The calculations of Ceres after the 10th have not yet come to hand.

XXXII. *Materials towards a more intimate Knowledge of Native Molybdena, containing a very advantageous Method of extracting Molybdic Acid from that Sulphuret.*
By C. F. BUCHOLZ*.

AFTER the repeated labours of a great many chemists, such as Quist, Scheele, Bergman, Heyer, Ilsemann, Pelletier, Richter, &c. on sulphuret of molybdena, both in regard to a better method of extracting molybdic acid from that mineral, and in regard to the phænomena it exhibits with other bodies, one might believe that no further researches remain to be made on this subject. But by reflecting properly on what has been done, we shall soon be convinced of the contrary. As my principal object in this memoir is to make known a more advantageous method of preparing molybdic acid, it will be requisite, to justify the necessity of such a method, to recapitulate the old and new processes, in order to show their defects, and to compare them afterwards with mine.

The first process for extracting molybdic acid from molybdena consists in separating the sulphur from the molybdena by a strong heat, and converting the remaining metal into acid by continuing to heat it in the air. The vapours of the molybdic acid are received in a proper vessel, where they are condensed under the form of yellowish white scales, which are alone considered to be molybdic acid. As these scales are formed very slowly and in small quantity, and as the volatility of the molybdic acid in a moderately strong heat on the one hand, and on the other the property of the same acid of fusing in a very strong heat, and of penetrating, during its fusion, the matter of the crucible, render it impossible to obtain in this manner a certain quantity of acid, this method cannot in any point of view be considered as advantageous or even practicable: we shall therefore proceed to examine the second method.

The second process, which like the first belongs to Scheele, consists in distilling off at different times nitric acid of a moderate strength from molybdena until the sulphur and the metal are both converted into acid. This operation requires from 20 to 24 parts of nitric acid for one part of sulphuret of molybdena, and it leaves with the molybdic acid the iron and earths which the purest molybdena

* From Scheerer's *Allgemeinen Journal der Chemie* 1802, no. 5. p. 485.

always contains in greater or less quantity. This process, which was considered as the only one proper for obtaining pure molybdic acid, besides its not answering the proposed end, is too expensive to be followed, and therefore another more æconomical and more proper for obtaining an acid free from foreign mixture is desirable to be substituted in its stead.

The third process consists in distilling the sulphurated molybdena with arsenic acid, and in expelling by volatilization the oxide and sulphuret of arsenic which are formed. This method also is so expensive and so dangerous that we shall not enter into any further discussion of it.

The fourth process, which belongs also to Scheele, is that which furnishes the acid in the speediest manner; but it does not furnish it pure. It consists in causing the molybdena to deflagrate with nitrate of potash in proportions respecting which authors vary. Scheele* took four parts of nitrate to one of sulphuret of molybdena; Lampadius † recommends six parts of nitrate to one of molybdena, and Fourcroy ‡ three parts of nitrate to one of molybdena.

The sulphur and the metal are here converted into acids, and in that state combine with the potash. A portion of the nitrate remains undecomposed, and there is formed at the same time deoxygenated nitrate: the remaining mass is dissolved in warm water; and the molybdate of potash, still mixed with its other salts, is decomposed (or this may be done after it has been separated by crystallization) by means of the nitric, muriatic, or sulphuric acids. The molybdic acid, when the solution is not too dilute, separates under the concrete form: this precipitate is not pure molybdic acid, but, as Scheele before announced, molybdic acid holding potash, or, according to Fourcroy, acidulous molybdate of that alkali.

This acidulous molybdate is much more soluble in water than pure molybdic acid, which for its solution requires 500 parts of that liquid, whereas acidulous molybdate dissolves in three or four parts of water. The latter fuses in the fire sooner than the acid, and more readily corrodes the crucible. After cooling, it is of a beautiful yellow colour: the acid, on the contrary, exhibits a radiant mass of a whitish gray colour, having a slight metallic brilliancy. The acidulous molybdate does not suffer itself to be sublimated; whereas the pure acid is vaporized by heat, and deposits it-

* *Opuscula Physica et Chemica*, tom. i. p. 202.

† *Handbuch der Chemischen Analyse*, p. 325.

‡ *Système des Connoissances Chimiques*.

self, on cold bodies presented to it, under the form of needles and yellow scales. It appears that acidulous molybdate has often been employed for pure molybdic acid. Thus Lampadius gives reason to believe that the acid which he prepared by the above process was not pure acid but acidulous molybdate. He recommends decomposing the mass which remains after deflagration by muriatic acid in excess. It will be seen by the following experiments that the molybdic acid cannot be entirely freed from the potash. To remove this obstacle to the preparation of pure molybdic acid, Scheele recommends dissolving the precipitate obtained in the smallest quantity of water possible, and boiling it for some minutes with a new quantity of nitric acid.

When the alkali has been thus separated, the molybdic acid is supposed to deposit itself in very small crystals. It appears to me confirmed that in this manner there is separated a little pure acid; but the very large part of the acid which remains dissolved with the acid added in excess is obtained separately with very great difficulty; and when enough of acid is not added, a mixture of pure acid and acid holding alkali is obtained. As this fourth process appeared to me far preferable to the rest, being much shorter and more economical, I wished to try whether it was not possible still to improve it, and to render it common in the preparation of the molybdic acid.

As the proportion of nitre, even in the process of Fourcroy, appeared to me still too great, I was desirous to examine whether something might not be saved in this point of view.

Experiment I.

I heated to redness in a Hessian crucible six gros of saltpetre, and projected on it, in parts, pure sulphurated molybdena pulverized. After I had added two gros and twenty grains of this mineral, the deflagration was still very brisk. I maintained the matter in fusion for some minutes longer, after which I dissolved the mass in water. The solution was complete, a few grains of oxide of iron and of silex excepted. On proceeding to the crystallization of the salts contained in this mass, I obtained some small crystals of undecomposed nitre.

Experiment II.

According to the indication furnished by the preceding experiment, I took two pounds three quarters of purified nitre, which I brought to red fusion, and gradually projected on it a pound of pulverized sulphuret of molybdena.

When the whole quantity of molybdena was introduced, a very brisk deflagration took place. I dissolved the saline mass in distilled water. The whole dissolved except about ten gros, which were oxide of iron and silex. The ley furnished by crystallization a little undecomposed saltpetre, nitrite of potash, and gaso-nitrous sulphates and molybdates of potash, or holding nitrous gas.

These experiments prove that $2\frac{3}{4}$ parts of nitre are sufficient to deprive of its sulphur one part of sulphurated molybdena, and to oxygenate the metal. This quantity might even be diminished, were it not to be apprehended that the small portion of undecomposed sulphur which is found in the mass towards the end of the operation would with difficulty be attacked by too small a quantity of nitre.

To find the means of separating the molybdic acid from the acidulous molybdate of potash thus obtained, I made the following experiments.

Experiment III.

To a portion of the above solution I added sulphuric acid till nothing more was precipitated and the mixture had acquired a strongly acid taste, after which it was digested cold for two hours. The precipitate obtained was washed several times with distilled water, and it was heated with twenty parts of the same water. The whole of it readily dissolved.

To this solution I added nitric acid until there was a considerable excess. No precipitate was formed. I evaporated the liquor to dryness. The residuum contained no crystallized part, had no taste of saltpetre, and did not deflagrate when thrown on burning coals. This residuum, which was yellow, easily fused in an ignited crucible without being evaporated by a strong heat. After cooling, it was found to have assumed a yellower colour. This experiment evidently proves that acidulous molybdate of potash is not decomposed either by sulphuric acid assisted by a digesting heat, or by nitric acid; for the precipitate obtained was exceedingly soluble, and was not volatile at a white heat.

As I had evaporated the solution of molybdate conjointly with nitric acid, it was possible that the nitrate of potash formed in the first instance might afterwards be decomposed by the molybdic acid separated. I therefore resolved to repeat the same operation with nitric acid.

Experiment IV.

I decomposed a part of the above solution of acidulous molybdate,

molybdate, adding an excess of nitric acid. A large portion of the precipitate was redissolved. After some hours digestion I tried the nature of the precipitate, after having collected and washed it on a filter. On putting a little of the precipitate, still moist, into a Hessian crucible, and exposing it to heat, it resolved itself into a transparent liquor. When the whole moisture was dissipated, and the crucible had begun to be red, the matter entered into fusion; but only a very weak volatilization was observed by an increase of heat. The fused mass was of a yellowish gray colour. A great part of it had passed through the crucible. It resulted from these phenomena, compared with those which the free acid exhibited with water and in the fire, that the precipitate obtained in this experiment was a mixture of free molybdic acid and acidulous molybdate of potash.

After this imperfect success I resolved to try the muriatic acid to decompose acidulous molybdate of potash.

Experiment V.

I decomposed the above-mentioned solution by means of muriatic acid. An abundant precipitate was formed. I added a strong excess of acid, which dissolved a remarkable quantity of the precipitate. I digested the mixture for some hours in a warm place, which completed the solution of almost the whole it. After cooling, there was formed in the liquor a considerable number of very small crystals. I tried these crystals, and found that they were almost entirely soluble in three or four parts of warm water; that they readily fused in an ignited crucible without emitting much vapour; that they penetrated the matter of the crucible; and that after cooling they had a grayish yellow colour. I thence concluded that the precipitate obtained by the muriatic acid contained scarcely any free acid, but was almost entirely composed of acidulous molybdate. I then evaporated to one-half, in a moderate heat, the solution which still contained molybdic acid. During the evaporation, but in particular after cooling, there was separated a quantity of small yellowish white crystals: there, however, remained in the solution a still greater quantity of molybdic acid, as was indicated by its acid taste and the trials to which it was subjected. The crystals obtained exhibited the same phenomena with water, and in the fire, as pure molybdic acid.

These experiments did not furnish a method of preparing, with safety and advantage, pure molybdic acid, and of obtaining the whole quantity contained in the sulphurated

molybdena; or there was formed acidulous molybdate of potash which was not decomposed; or only a little molybdic acid was separated; or there remained in the liquid too large a quantity of acid, which could be separated only incompletely and with difficulty. In this unfavourable state of things I thought of several means for decomposing the molybdate by the help of double affinities, but I was not able by any of the known means to separate the acid completely from the alkali. I therefore resolved to recur to the old method, which consists in oxygenating the sulphuret by nitric acid, taking care to correct the process by separating the greater part of the sulphur by calcination. By this preliminary separation of the sulphur a great quantity of the acid is saved, and the labour is considerably shortened. I consequently proceeded to calcination in the following manner.

Experiment VI.

Five ounces and a half of sulphuret of molybdena in fine powder, and which were perfectly pure, a few small particles of quartz and oxide of iron excepted, were introduced into a large Hessian crucible, which, for the greater convenience, was placed obliquely in a furnace and surrounded by charcoal. When the matter was red it was stirred incessantly, and in turns, by me and my friend M. Haberle, to whom I was indebted for the molybdena employed in these different experiments. During the first hour, and before the greater part of the sulphur was dissipated, the matter retained its lightness: it then united into a mass, and its black colour passed successively to gray, reddish gray, and then to whitish gray.

When the whole sulphur was driven off, which required two hours, the mass by an increase of heat coagulated more and more, and even began to fuse at the bottom of the crucible, and it appeared by the vapour which rose that the molybdena was volatilizing. Having maintained a moderate fire for half an hour, I took the crucible from the fire and examined the matter. It had a whitish gray colour, exhibited here and there splendour and a crystalline form, and weighed a little more than four ounces; which approaches nearer to the 0.23 of sulphur which Lampadius said he found in molybdena than the proportion of Kirwan, which is 0.55. The sensible metallic taste of the calcined matter, its brilliant and crystalline aspect, and the experiments made by Ilsemann*

* Crell's *Chemische Annalen* 1787, vol. i. p. 410.

and Heyer*, in which calcined molybdena exhibited the phenomena of an acid, made me suspect that this molybdena might be molybdic acid like that sublimated. In this case the nitric acid might have been spared; which would have rendered the process still shorter and more economical.

To clear up my doubts in this respect I made the following preliminary experiments.

Experiment VII.

I reduced to fine powder some grains of calcined molybdena, poured over it a gros of water, and heated it above a lamp. I then instilled into it some drops of a solution of carbonate of soda: on each new instillation a strong effervescence took place, and a great part of the matter was dissolved. For the moment this result was sufficient to enable me to conclude from it the acid nature of the calcined molybdena. The solution was filtered, and decomposed by nitric acid. There was formed a precipitate of molybdic acid, which placed it beyond a doubt that there was formed a combination of this acid with soda.

Experiment VIII.

I pulverized some grains of calcined molybdena, and boiled them for some minutes with an ounce of water. The taste of the liquor, which was sensibly acid and metallic, as well as the red colour it communicated to turnsole paper, again proved the nature of the acid of calcined molybdena.

It gave me great satisfaction to have found so short and so economical a method of obtaining molybdic acid. I cannot help wondering, as the experiments of Scheele, Heyer, and Ilsemann, have been so long known, that chemists have not followed this method indicated to us by the nature of calcined molybdena, and to which they had so nearly approached. In order to ascertain more fully whether calcined molybdena is pure molybdic acid, and to determine the means of separating from it the heterogeneous substances with which it might be still combined or mixed, I made the following experiments.

Experiment IX.

As the molybdic acid has the property of combining easily with a portion of potash, and of retaining that base, if not entirely, at least in a great part, against the action of all acids, this salt could not be of any use for purifying that

* Crell's *Chemische Annalen*, vol. ii. p. 26 and 125.

acid. The alkaline earths, and earths which with the molybdic acid form salts difficult of solution, could not answer my purpose to separate the acid from the insoluble substances with which they are mixed. It remained to try soda and ammonia, which I found to be both equally proper under circumstances peculiar to each of them, for accomplishing the proposed end.

Experiment X.

I boiled two gros of calcined molybdena with eight ounces of water, and instilled into the solution a ley of carbonate of soda until no more effervescence took place. An excess of alkali was added, and the matter was left to boil for half an hour: it was then filtered, and the concrete matter which remained on the filter was edulcorated. The different liquors were evaporated to two ounces, and nitric acid to excess was instilled into the warm liquid. During the instillation of the nitric acid a crystalline white powder was deposited, and after cooling there were formed in the liquor crystals of a certain size. A new quantity was obtained by evaporation. The different salts were washed and dried. The residuum on the filter, after being washed and dried, weighed thirty grains, and had the reddish colour of oxide of iron.

Experiment XI.

The preceding experiment was repeated, with this difference: the liquor, after the effervescence had ceased, was boiled for half an hour longer. The matter, after being reduced to two ounces, and filtered, was suffered to cool before the nitric acid was added. The addition of this acid caused to be precipitated a larger quantity of the white powder, which had a more crystalline aspect, and which after edulcoration and desiccation had the splendour of mother-of-pearl. As I did not add so much acid in this experiment as in the former, the liquor was less charged with molybdic acid. The residuum, when washed and dried, weighed also thirty grains.

As I suspected that this residuum might contain a portion of molybdic acid less soluble than the preceding, I tried to extract this acid by boiling the residuum with nitric acid. With this view I made the following experiment.

Experiment XII.

The thirty grains of the residuum of the two last experiments were boiled with an ounce of nitric acid of the specific gravity 1.250 and half an ounce of water, until the whole

whole moisture was dissipated. During the evaporation I observed no disengagement of nitrous gas, from which I could conclude that there was in the residuum a little molybdena not yet acidified. I then boiled the residuum for half an hour with a ley of carbonate of soda, after which there was added to the solution nitric acid to decompose it; but I observed no precipitation of molybdic acid, though an excess of nitric acid had been added. The solution had only a metallic acid taste. On examining the residuum more closely, it was found to be composed of alumine, oxide of iron, and a few fragments of silix, which, if I except the oxide of iron contained in the sulphurated molybdena, arose in part from the molybdena and partly from the crucible.

Experiment XIII.

The white precipitates of experiments X. and XI. were put into an ignited crucible. By an increase of heat they first entered into fusion, and were then volatilized and condensed, partly under the pulverulent form and partly under the crystalline. By continuing the fusion the matter penetrated through the crucible. The fused matter was very liquid, and on cooling it formed itself into a crystalline radiant mass of a whitish gray colour having a slight metallic splendour. In other respects it exhibited the same phenomena as pure molybdic acid.

This last method furnishes a very short and æconomical method of obtaining molybdic acid. There can be no doubt that if the molybdena had been entirely free from matrix the calcined mass would have entirely dissolved, except an atom of iron, from which sulphuret of molybdena is never free.

I was desirous also to try the separation of the molybdic acid by the help of ammonia. With this view I made the following experiments.

Experiment XIV.

I digested for twelve hours, in a moderate heat, a gros of the calcined mineral in fine powder with half an ounce of concentrated liquid ammonia, shaking the matter from time to time; after which the liquor was filtered. It had still a strong odour of ammonia. The half of it was decomposed by nitric acid, which precipitated from it molybdic acid in abundance. The other half was evaporated in a small porcelain capsule, and then heated to a slight degree of incandescence. The ammonia was entirely dissipated, and left a grayish blue residuum, which entered into fusion
in

in a stronger heat, and which, after cooling, exhibited a radiant whitish gray mass perfectly similar to fused pure molybdic acid. The residuum collected after the digestion of the calcined molybdena with the ammonia weighed twenty grains: I subjected it to the following experiment.

Experiment XV.

I boiled the residuum above mentioned with two gros of nitric acid to perfect evaporation, and digested the new residuum with two gros of liquid ammonia. The residuum was then filtered, washed, and dried. It weighed sixteen grains. The liquid was evaporated, and after incandescence I obtained four grains of molybdic acid. The residuum of this experiment, like those of Nos. X. XI. and XII. contained oxide of iron, alumine, and quartz sand.

After these experiments I will not hesitate to prefer the method of separation by ammonia to that by soda, especially in operating with pure soda. For it may happen that the molybdena will enter into vitreous fusion with the foreign bodies contained in its ore, and then ammonia would not be capable of separating the molybdic acid from that combination. However, this inconvenience may be prevented by not making the fire too strong towards the end of the calcination. When the method by ammonia is properly considered, it will be readily perceived that it would be impossible to devise one more expeditious and less expensive.

The molybdena may be deprived of its sulphur without the aid of the nitric acid or of nitrate of potash. The molybdic acid formed is purified from the oxide of iron with which it is naturally united, and from the quartz and argil with which they are accidentally mixed, by digesting it with ammonia; which can be done only with difficulty by means of nitric acid. Molybdate of ammonia is decomposed by a heat of incandescence, which separates from it the alkali in the state of purity and without being decomposed. It may therefore be collected without great loss in a distilling apparatus, and the acid, almost without any decrease, will be found in the retort. So many advantages united are certainly not to be found in any other method, not even in that of separation by soda.

By following these different indications we are led to the following process, which is the most advantageous for preparing molybdic acid.

New Method of preparing pure Molybdic Acid.

Take the purest sulphuret of molybdena that can be found,
and

and reduce it to powder; introduce it into an inclined retort, and torrefy it, to separate the sulphur, and to oxygenate the metal, in a heat which must be immediately raised to a very high degree. When the molybdena has lost its brilliancy, and its black colour has changed to gray, lessen the heat, and continue to expel the last portions of the sulphur, and to complete the oxygenation of the metal at a more moderate heat, continually stirring the matter with an iron spatula. By these means the matter will be prevented from entering into fusion with the oxide of iron and a small quantity of alumine with which they are mixed, and from forming a mass which the ammonia can afterwards decompose only with difficulty, and nothing will remain adhering to the crucible. The mineral is then to be reduced to a very fine powder, and it must be digested several times with a sufficient quantity of liquid ammonia to extract the whole molybdic acid. The different liquids are then to be united, and being poured into a glass retort are distilled to dryness. Molybdate of ammonia will remain in the retort. The heat is increased to incandescence, in order to expel the ammonia. If the retort be too large, the matter must be removed into a smaller.

This process would not answer where it is required to analyse ore of molybdena. In this case it would be necessary to oxygenate the sulphur and the metal by the help of nitric acid. This method will contribute in all probability to ameliorate and render easier the analysis of sulphuret of molybdena.

Addition to the preceding Experiments.

I had undertaken four times, and with the same success, the separation of the molybdic acid by the help of ammonia in the proportions of experiment XIV, when in treating in this manner a pretty large quantity of calcined molybdena I observed a surprising phenomenon which hitherto I had not remarked. I separated the acid from two ounces of molybdena by the help of ammonia. The filtered liquor was left at rest for some days, to see whether any thing would be deposited; which actually was the case. Some flakes which exhibited the same phenomena as oxide of iron were deposited. I again placed the liquor on one side, to see whether a new separation would take place. I had, however, evaporated and decomposed a quantity of the solution of molybdate of ammonia equal to a sixteenth part of the whole liquid. The residuum exhibited the same phenomena

phænomena exactly as that obtained in experiment XIV, and was pure molybdic acid. Numerous occupations prevented me from undertaking new experiments with the solution of molybdate of ammonia until six weeks after. Having evaporated the solution to dryness, I put the saline mass into a new Hessian crucible in order to expel the ammonia by incandescence. To my great astonishment I observed that the mass deprived of ammonia, which under other circumstances fuses so readily, and which, after being some time in fusion, penetrates into the substance of the crucible, could not this time be brought into fusion even in the strongest white heat, and was only softened, emitting vapours of molybdic acid. On examining the cooled mass, I found at its surface several irregular depressions. It had an argentine splendour inclining to gray. On its fracture it exhibited a cupreous blue colour of steel with a metallic tint; its texture was scaly and very compact; it was difficult to be broken and pulverized. Its specific gravity was 5050. By pouring over it moderately concentrated sulphuric acid the mass acquired a considerable heat, emitting nitrous gas in abundance, and transforming itself into white oxide.

After all these phænomena, it can be considered only as metallic molybdena, or oxide of molybdena very near to the metallic state. I can explain this in no other manner than by admitting, that in consequence of the long contact of the ammonia with the molybdic acid the latter was decomposed. I dare not, however, assert that this was actually the case, and that some other cause which I did not observe may not have concurred to produce the same effect. However this may be, it is remarkable that under the circumstances above mentioned the molybdic acid, the reduction of which is so difficult, should be reduced so easily and by so weak a fire; and that the metallic molybdena, which is so little fusible, should be fused, or at least softened into a homogeneous mass of a scaly texture. The want of molybdena has hitherto prevented me from making the experiments necessary to ascertain the cause of this singular effect.

XXXIII. *Of the general Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, and the Circumstances by which the former are influenced.*

[Continued from p. 33.]

Of the Standard of Proof:

§ 14. **T**HE standard of comparison which, in conformity to the principles already spoken of in § 3 has been established in this country, is called *proof spirit*; and it appears that some such standard was in use long before we were in the possession of any means of correctly appreciating its strength. This, indeed, has never yet been done; and it is from the want of correctly defining the strength of this standard that a great part of the uncertainty at present existing, with regard to the relative strengths and values of spirituous liquors in general, has originated.

§ 15. When a spirituous compound of any kind contains about a certain quantity of alcohol (which quantity, however, varies considerably in different liquors, and even in liquors of the same description, but differing in their purity), it will afford, on shaking it in a phial, a crown of small bubbles of a peculiar appearance, which gradually go off, or subside without breaking; and which has generally been called by the French the *chapelet*, and by us the *proof*. Liquors of any kind which were capable of exhibiting this phenomenon have, for upwards of two centuries past, been denominated of proof strength.

Upon the average, perhaps, an experienced person may by this means guess within about 6 or 8 per cent. of the real strength of such liquors as are capable of being examined in this way, if in a tolerable state of purity.

The following curious account of the processes for proving spirituous liquors, which is extracted from Mr. Highmore's Treatise on the Excise Laws, published in 1796, vol. ii. p. 283, may, perhaps, serve to give a better idea of the degree of reliance which ought to be placed on them, and of the necessity which there is for a total revision of this important subject:

“Proof spirits, or common saleable goods, are spirits of any kind of a determinate strength, being the same with those of good brandy, and the malt and sugar spirits of the distillery, as they are usually sold, containing equal quantities of rectified spirit and water.”

“The best proof spirit is that distilled from French wine; but

but for common use may be employed the spirit drawn from melasses."

"The common method of judging of the due strength is by striking the bottom of the sample phial, filled half way with common malt spirit, with the palm of the hand; the bubbles raised on the surface will go off again in a strong manner, without breaking or swelling; and this is the method constantly used by the traders: it is said to be fallacious, and easily open to deception; for if a little treacle, syrup, &c. be added to a quantity of highly rectified spirit of wine, it will give a brandy proof to that spirit. The true strength may, however, always be known by carefully burning away a measured quantity of brandy, &c.; since if it leaves one half water, it is right; if more or less, it is too strong or too weak."

"Perfect proof is that crown of bubbles before mentioned, of a certain size, arising as a head upon a small quantity of a well qualified spirit shaken in a slender phial."

"Proof more than perfect is that in which the bubbles raised by shaking the spirit are larger than those on the common or perfect proof, and go off more suddenly; that is, according as the spirit is higher, or approaches more to the nature of rectified spirit, or, as it is usually called, spirit of wine."

"Proof less than perfect is that wherein the bubbles are smaller, and go off quicker and fainter than in perfect proof; the spirit, in this case, being mixed with more than its own quantity of phlegm, or being too poor for sale."

"The most exact of all methods of determining the strength of any spirit is by distillation, rectifying it up to an alcohol, or totally inflammable spirit; but this, though liable to no error, is too tedious to come into common use. And, upon the whole, the best method of all others seems to be that of *deflagration*; namely, by setting it on fire: if, after it will no longer burn, the remainder is half as much as the quantity measured out for the trial was, then the spirit tried is found to consist of half water, and half totally inflammable spirit; that is, exactly *perfect proof*: and according as the remainder is more or less than half the original quantity, it is so much below or so much above proof, or the due strength of brandy."

"In commerce, with regard to spirits, it would certainly be a much better method to abolish such uncertain proofs, and to make all the goods of the strength of what is called spirits of wine; that is, a totally inflammable spirit, whose purity is much greater, whose strength may always be found
with

with exactness, and whose bulk, stowage, carriage, and incumbrance, would be only half in regard to that of brandy, or proof spirit; and it might at all times, as occasion called for it, be mixed into a great variety of extemporaneous liquors, and the exact degree of strength would be always precisely known."

"This operation, indeed, in the common way, proves so tedious and expensive, and, after all, so short of expectation, and so generally unsatisfactory, that it is not to be expected that the common distillers, till they have fallen into a better manner of working, should come into the proposal. But if, instead of the common way of rectifying by the hot still, they would try the using a large *balneum Mariæ*, made of a large rectangular boiler, and a set of tall conical vessels, they will find that little fire and little attendance, and consequently very little expense, will, in this manner, furnish them with spirits reduced at once to this standard, and greatly superior, in all respects, to the common ones of the same strength. In this case there would be no need of any addition of salts; but the distiller may work more perfectly and more expeditiously without them, and thus preserve the fine essential vinosity of the spirit, which in the common way of working they constantly lose."

"The advantage of this method would be yet greater to apothecaries, to the makers of compound cordial waters, who want only a pure spirit of such a strength, and suffer greatly in the fineness and perfection of their commodities by the spirit they are obliged to use having in it a fulsome and nauseous oil of its own, which will always mix itself with their compositions, and the oils of the aromatics, &c. which they add to it. If spirits were brought to this standard for the market, there would be no possibility of deceit; and no further examination need be made of it by the buyer than its burning perfectly dry in a spoon."

So much for the general state of science with respect to matters of this nature.

§ 16. The only trace which we are able to find of any attempt to define by parliamentary authority what any denomination of strength of a spirituous liquor really signifies, is by a loose passage in the act 2 Geo. III. c. 5, which is as follows: "And for the purposes of this act it is hereby enacted, by the authority aforesaid, that each gallon of brandy or spirits, of the strength of one to six under hydrometer-proof, shall be taken and reckoned at seven pounds and thirteen ounces the gallon."

The reader will in a moment perceive the striking inaccuracy

curacy of the above definition. The weight of the averdu-
pois pound, and the length of the inch and consequent solid
content of the wine-gallon, neither of which was at the time
of making the statute appreciated with sufficient accuracy,
we shall for the present take to be so, and assume that the
former is equal to 7000 grains troy; and that the general
relation between the measures and weights of this country
is such, that the cubic inch of distilled water, at 60° of Fah-
renheit's thermometer, would weigh, in air at the same tem-
perature when the barometer stands at 29 $\frac{1}{2}$, 252 $\frac{1}{2}$ of the same
grains. [*Vide* vol. xv. p. 278.] There still remain, however,
two very important questions which require to be answered
before we can ascertain the real strength of proof spirit.

1stly, What is meant by *one to six* under proof? Is it in-
tended to signify that compound which would be produced
by the addition of one part of water to six of proof spirit by
measure—or that of which a given quantity by measure shall
contain and consequently be equal in value to six-sevenths
of *that measure* of proof spirit? These are by no means
convertible terms, as will be easily perceived by considering
what is before mentioned in § 10. We must take one sup-
position or the other; and we shall, for the reasons which are
stated hereafter in § 24, adopt the latter.

2dly, What is the temperature at which it is intended that
this spirit should weigh seven pounds thirteen ounces? A
compound of this strength would vary in its weight upwards
of two ounces per gallon in different temperatures in this
climate, and still more in some other countries; and every
liquor of which a gallon at any possible temperature would
be of that weight, would answer equally to the description
of the statute. A compound apparently 12 per cent. under
proof at 35°, would, if judged of by this criterion, appear to
be over proof at 70°; and vice versâ.

§ 17. The omission to fix the temperature, therefore, at
which the compound mentioned in the statute is to weigh
seven pounds thirteen ounces per gallon, coupled with the
ambiguity of describing a mixture differing from proof strength
rather than proof spirit itself, and from which the strength
of the latter can only be determined by a calculation in which
different data may be assumed, are the leading causes of all
the uncertainty relative to this subject. Something must be
guessed at with respect to each; and the following is the
mode in which the authors of this work have hitherto pro-
ceeded in the appreciation of the strength of proof spirit.

Assuming that the avoirdupois pound and the content of
the wine-gallon of 231 cubic inches bear such a relation to
each

each other as is already described in the last section, the specific gravity of a liquor weighing seven pounds thirteen ounces avoirdupois, or 54687.5 grains per wine-gallon, will be 937.59. Now sir Charles Blagden tells us, that, although no temperature is mentioned in the statute, yet the understanding of the trade is, that it means that the diluted spirit thus spoken of should be considered as being at 55°; whilst others contend that the temperature should be rated higher, for the following reason. It seems that, at the time of making the fundamental experiment on which the statute was founded, the spirit which is there described was produced by lowering either alcohol, or some other kind of rectified spirit, to the strength there spoken of with water, and immediately weighing it; under which circumstances it was then found to weigh 7 pounds 13 ounces per gallon, and to be one to six under proof. Now, if this was really the case, the mixture could not probably be lower than 60°. We will however suppose that the truth lies somewhere between them, and that the temperature was in fact 57½°. Now we find, from Mr. Gilpin's tables, that a spirit which is of the specific gravity of 937.59 at 57½° is similar to one composed of 100 parts of water by weight, and 86½ of his alcohol of the specific gravity of 825. at 60°, and of which it contains a quantity which would be by measure, when at 60°, equal to .5278 of the bulk of the compound at 57½°. But proof spirit is, by the directions of the act, according to the construction of it which we adopt, to contain 7-6ths of this proportion, or .6157 of its quantity of such alcohol by measure when at 60°; that is, it will be equal to a compound which will be found by the same tables to have exactly the specific gravity of 920 at 60°. This appreciation, therefore, of proof spirit has been adopted by the authors of this tract in all the instruments which they have made for many years, and appears now to have become that of the trade in general. The wine gallon at 60° of spirit of this strength weighs 7 pounds 10 ounces and 10.47 drachms avoirdupois; and the same measure at 55°, 7 pounds 10 ounces and 15.59 drachms.

The commissioners of the customs have, however, (according to the statement of Mr. Ramsden, in his "Account of Experiments to determine the Specific Gravities of Fluids, thereby to obtain the Strength of Spirituous Liquors," printed in 1792,) been in the habit of considering proof spirit as weighing 7 pounds 12 ounces per gallon, at 55°, which would, according to the foregoing estimation of the relation between this weight and measure, give no less than

928 for its specific gravity at 60°. In fact, the real standard of proof spirit has never yet been tolerably defined, and it is therefore nugatory to talk about it. The language of the legislature, *hydrometer proof*, perhaps, conveys a more accurate idea of it than any other; for it has hitherto been just what the hydrometer-maker chose to consider it.

§ 18. The real strength of proof spirit, which was already so indefinitely described by the act of 2 Geo. III. c. 5. was, however, rendered still more doubtful by a clause which was inserted into a subsequent act (27 Geo. III. c. xxxi. § 17) enacting and declaring, "That, until the fifth day of April one thousand seven hundred and eighty-eight, all spirits shall be deemed and taken to be of the degree of strength at which the said hydrometers called Clarke's hydrometers shall, upon trial by any officer or officers of excise, denote any such spirits to be." The meditated investigation of this business, however, did not take place; and this clause was, after being once or twice continued in contemplation of such a measure, at length made perpetual by 41 Geo. III. c. xxvii. § 8.

Now, if the instrument mentioned in the act had agreed with itself, it seems as if the proof as denoted by this hydrometer would have become the legal standard; the subsequent act being, of course, paramount to the antecedent one with respect to those points in which they might be found to clash with each other. Unfortunately, however, it had long been, in all respects, the worst instrument which had been in use in the spirit trade for these purposes, its indications differing every where with each other. Mr. Clarke's one to ten, for example, (by which is meant 10 per cent. over proof,) is only between 8 and 9 per cent. over his own proof, and some points actually differ in like manner to the enormous extent of 7 or 8 per cent. from the truth; and, to complete the confusion, there were always two of those instruments made, one for import and the other for export spirit, each of which differed in every graduation from the other. How this came to be the case we are not sufficiently informed: we are told, however, by the pamphlet delivered with these instruments, that "the export hydrometer is used in trying British spirits in the different divisions of distillery, and by distillers in general. The import is used at the port of London and all the out-ports in trying imported foreign spirits, and by brandy merchants in general; and is a small matter lighter than the export, and in favour of the importer. This notice is thought necessary, as some gentlemen have tried instruments with one
of

of a different kind, not knowing there was any difference in Clarke's hydrometer!!!”

§ 19. Vague and indefinite as the term *proof* has hitherto been in England, it has always been still more so in Ireland, where they have, for about forty years past, used an hydrometer for the purposes of the revenue which has no correction whatever for temperature. The Irish proof, therefore, is altogether incapable of being defined with any tolerable accuracy. It appears, however, to be upon the average somewhere about 9 per cent. over proof by our standard, or to have a specific gravity of about 909 at 60°. It was probably established at first of that strength, as being the highest which a foreign spirituous liquor could possess, without paying the augmentation duty on its subsequent importation into this country.

§ 20. It appears, from what has been before said, that the first step towards the permanent arrangement of this important business must necessarily be to fix and ascertain the real strength of proof spirit by an act of parliament, which shall define its specific gravity at a given temperature. If it were enacted, for example, “that every spirituous liquor whose specific gravity, when free from adulteration, should, at the temperature of 60° of Fahrenheit's thermometer, be to that of distilled water at the same temperature as 920 to 1000 (or in any other ratio which might be thought proper), should be deemed and taken to be proof spirit; and that every spirituous liquor of different strength should be estimated by the quantity of such proof spirit at the said temperature of 60°, which, according to the tables contained in the schedule to such act annexed, (annexing a set of tables for that purpose, calculated from those of Mr. Gilpin, in the manner hereafter mentioned in § 32, &c.) would produce or be producible from every such other spirit by the addition of water to the stronger of the two till they were reduced to the same degree of strength,” all the present uncertainty respecting this matter would instantly vanish. The hydrometer would still continue to be the most convenient instrument; and its accuracy would be ensured by the facility with which its errors would be detected, as any man who could use a pair of scales would then be able to examine the truth of its indications.

[To be continued.]

XXXIV. *Of the Variations of the Temperature of the Summer and Winter Seasons that take place in different Years.* By RICHARD KIRWAN, Esq. LL. D. F. R. S. and P. R. I. A.*

TO reason with precision on this subject, we must at first abstract from all sublunary physical causes, and indicate the temperature appropriated to different latitudes from mere astronomical considerations.

Halley has ingeniously resolved this problem so far as the mere ratios of heat in the different seasons are concerned. (Phil. Trans. Abr. ii. p. 165; and Lambert in his Pyrometric, § 596.

Halley, calculating the ratios of heat communicated by the sun to the earth (which he considers merely as a planet, abstracting from all distinction of land and water,) in the different seasons in the northern hemisphere, reduces these seasons to three, the equinoxes, the summer solstice, and the winter solstice; and attending only to the sines of incidence of the sun's rays, and the duration of their action, he sets the heat communicated at latitude 0, on the days of the vernal and autumnal equinoxes, at 20,000; and on the tropical days in the same latitude at 18,341: and then adds the ratios which the heat in every 10th degree of north latitude bears to these at the same periods. Lambert adds the ratios of lat. 49° and $66^{\circ} 33'$, stating the equatorial heat on the equinoxial day at 999.

But to express these ratios in thermometrical measures we must endeavour to find the greatest heat of the equinoxial day, taking a mean of the heat of the morning at two o'clock, and the evening under the equator, or very near it; and this I find to be 88° or 89° of Fahr. (see Ulloa, Mem. Philosoph. p. 61,) in the northern hemisphere, on the 20th of March, on the ocean; to which, indeed, we must confine ourselves in this inquiry, and particularly the Atlantic, for no uniformity can be expected on land.

It is uncertain what thermometer of Reaumur Ulloa employed, whether the true or the false; and hence I place the heat at 88° of Fahr.

This correspondence being found, the thermometrical degrees corresponding with all the other ratios are easily found by the rule of proportion, and the degrees thus found

* From his paper entitled "Of the Variations of the Atmosphere. 1801."

I call the *mathematical temperature*. But in most cases this temperature is far from agreeing with the temperature really observed, and which I therefore call the *real temperature*; this I take at a *mean*, and not at its *maximum*, which I could not always discover, and is more fugitive and contingent. These temperatures I exhibit in two separate tables, the first indicating those of the vernal equinox and of the northern tropic or midsummer, and the second those of the autumnal equinox and the southern tropic or midwinter, over the Atlantic or standard ocean in our hemisphere.

Table the First.

Latitude.	Vernal Equinox.		Midsummer.	
	Mathem.	Real Mean.	Mathem.	Real Mean.
0	88°	84°	80·7°	83°
10	86·5	82	89·3	84·3
20	82·69	77	95·64	80·5
30	76·21	69·5	99·66	73·5
40	67·4	60	101·41	70·5
49	61	51	101·7	62 Lambert
50	56·5	50·5	101·86	61
60	44	40	100·2	56
66° 33'	39·81	34	101·21	55 Lambert
70	30·99	32	101·41	54
80	15·28	27	108·56	51
90	9·6	109·93
	Lambert			

In this table we see, 1. That during the vernal equinox the heat differs but little from the mean heat really observed in all latitudes, and perhaps still less from the maximum of real heat; yet, except in latitude 80°, it is always higher, both from the quantity of rays lost in passing through the air, and from the quantity reflected by water and the frequent interposition of clouds, &c.

2. We see that the astronomical heat constantly increases with the height of the latitudes, as the duration of the solar rays more than compensates for their obliquity when the sun is in the northern tropic; but the real heat decreases as the latitudes increase, because this theoretic compensation does not take place from the interposition of clouds and the access of cooler winds, and the increased reflection from the surface of the water.

The different temperatures of different summers are ultimately resolvable into the different direction of the winds during those seasons and the different electrical states of the atmosphere, the south or south-east producing not only clouds which intercept the sun's rays, but also copious rains or hail, which, descending from great heights and occasioning a copious evaporation, cool the air to a great degree. The north and north-east, on the contrary, unless immediately succeeding great rains (for then they increase the evaporation), disperse the clouds, and, proceeding from countries then somewhat heated, allow the sun's rays their natural calefactive effect. But why winds from opposite points should prevail in different years cannot be known until the contemporaneous states of the atmosphere between the northern tropic and the equator are known. It is possible that frequent hurricanes and tornadoes, during which a quantity of air may be destroyed and converted into water, may demand an annual supply from the north, and thus occasion our north and east winds; and the absence of these phenomena may occasion an influx from the south, if the north and east are summoned to a different quarter, by similar causes.

Table the Second.

Latitude.	Autumnal Equinox.		Midwinter.	
	Mathem.	Real Mean.	Mathem.	Real Mean.
0°	88°	84°	80·7°	83°
10	86·5	84·6	69·66	78·5
20	82·69	80	58	72·5
30	76·21	73·5	44·54	64·5
40	67·41	70·5	30·55	54
49	61	59	18·23	45 Lambert
50	56·56	58·5	16·71	44
60	44	48	4·73	34
66·33	39·81	42	1·32	30 Lambert
70	30·99	39	0	27
80	15·28	33·5	22
90	9·6

On this table we may remark, 1. That though the mathematical temperature of the autumnal equinox be exactly the same as that of the vernal, yet the real is much higher, as the northern hemisphere being cooled during the winter is slowly heated, and being heated during the summer is slowly cooled.

2. That

2. That in consequence of this circumstance the real temperature of the autumnal equinox approaches much nearer to the astronomical than does that of the vernal, until we arrive at latitude 70° and the higher latitudes.

3. In all latitudes above the equator a cold approaching to the astronomical is scarce ever felt at sea in winter: to what can this be attributed but to the equatorial effluence? For other causes, viz. evaporation and the frequent intervention of clouds, or at least haze, intercept the sun's rays, and consequently should cool the air even below the astronomical ratio which supposes the incidence of all the rays: in latitudes above 20° the difference is enormous.

4. At the distance of some hundred miles from the coasts of the Atlantic, in latitudes above 40° , the cold is much more moderate than the mathematical ratios indicate in most years; owing to the above-mentioned cause, and to the reign of westerly and southerly winds, which convey their heat to a considerable distance before they are cooled.

But in latitudes between 55° and 36° a degree of cold far superior to the astronomical has often been observed, and particularly of late, and in countries not very distant, or even bordering on the Atlantic. These extraordinary seasons may be attributed partly to the absence of the superior effluence, or its refrigeration in communicating with air replete with vapours, and partly to the prevalence of east-north-east winds which proceed from the interior and coldest parts of our continent; and hence the cold of the year 1776, so well described by Van Swinden, seems rather to have followed the order of the longitudes than of the latitudes. Wargentin, secretary to the Royal Academy of Stockholm, informs him that the cold observed that winter in Stockholm was not at all extraordinary, and expresses his surprise that it should have been so rigorous in Germany, France, and England. (Mem. Paris, 1776, p. 129.) Nay, it appears that the north-east wind which raged so furiously in Holland and at Montmorenci, latitude 49° , on the 27th (see Van Swinden, p. 40), had not been at Petersburg on the 18th, nor any day after; for a perfect calm reigned on that day, and the high winds of the remainder of the month were from the north-west. (Act. Petropol. p. 352.) It is therefore probable that this wind proceeded obliquely from the eastern and southern parts of Russia, and may have been derived from latitude 55° and longitude 40° , and originated on the Atlantic, latitude 40° ; and hence the utmost rigour of this cold was sooner perceived in the south of France, as Thoulouse, Marseilles, &c. than in the

more northern latitudes, as may be seen in Van Swinden's general table, for it reached these towns on the 18th or 19th of January. In the more northern latitudes it was felt only on the 27th; it is true its date at St. Jean de Luz, latitude 43° , is January 28th in the general table; but this is a mistake, as may be seen p. 181, for January 19th is there said to be the true date, and, p. 179, it is said that the 18th or 19th of January are the days on which the greatest cold was observed in all places south of the Garonne; which fully confirms my former statement, that the wind which produced this cold originated in the south-west, and thence was gradually propagated northwards and eastwards. All the minuter modifications of this cold, in places not very distant from each other, may be ascribed either to recent falls of snow, the proximity to which must affect more or less the thermometers, the greater or lesser abundance of vapours in the atmosphere, and other circumstances too tedious and minute for insertion in this general view.

Snow falling from some height in the atmosphere is generally for some time surrounded with an atmosphere much colder than the air some feet above it, as Mr. Wilson observed, though it did not occur to him that the cold was communicated to the air by the snow; for he thought it highly remarkable that a thermometer hung 24 feet above the snow was four degrees less cold than one suspended $2\frac{1}{2}$ feet above it. (Phil. Trans. 1780, p. 462.) Yet Mr. Boyle has long since noticed a similar fact (Boyle Abridg. i. p. 629), as related to him by some navigators; and Foster expressly mentions, that being to the leeward of an icy mountain, probably many feet distant, the thermometer sunk four degrees, and rose to its former height when he had passed that mountain (Observat. p. 73); but when there is not a recent fall of snow, the air several feet above the surface of the earth is generally colder (when no great evaporation takes place) than that nearer to its surface. Thus during the intense cold of January 1776, there having been no fall of snow since the 24th, Van Swinden found the degree of cold on the morning of the 27th to be $8^{\circ} 25'$, while Camper, in the same street, whose thermometer was some feet nearer to the earth, found it only $6^{\circ} 5'$. (See Van Swinden, sur le Froid de l'Année 1776, p. 24, 25, 28, and 176.)

XXXV. *Account and Description of a Stone which fell from the Clouds in the Commune of Sales, near Ville-Franche, in the Department of the Rhone. From a Memoir of M. DE DREE, read in the National Institute April 11, 1803*.*

WE have more than once entertained our readers with phænomena analogous to that which is to be the subject of the present memoir; and since the attention of philosophers has been directed to these singular facts, one might say that they have been more frequent. We cannot, and indeed ought not, to explain them until their existence has been fully proved, and until all the circumstances by which they are characterized are well known. The account which we are about to transcribe is extracted from a pretty long memoir on these phænomena read some time ago before the National Institute, by M. de Dree, brother-in-law of Dolomieu, a very enlightened amateur of mineralogy, and possessor of one of the richest collections of this kind in France. Among the facts which he collected, the one about to be mentioned has a striking character of authenticity; and it acquires a particular degree of interest from a comparison which proved to us that we observed the same meteor, without doubting, any more than others who saw it either at Geneva or in the western part of Swisserland as far as Berne, that it was one of those stones the origin of which it is so difficult to determine.

“ In the month of February 1802 I was at Lyons with Dr. Pétetin, president of the Medical Society of that city, member of several learned societies, and author of a work entitled *Nouveau Mécanisme de l'Electricité*: we were conversing on scientific subjects, when he asked me whether I had heard any thing of a meteor which appeared at Lyons some years ago; and on my answering in the negative, he related the fact, of which he was a witness, in nearly the following manner:

“ About four years ago (said he) during the evening twilight in the month of March, the weather being serene, and not at all cloudy, there passed over Lyons, nearly in a direction from east to west, a luminous ball, which, as it attracted attention by the strong light it emitted in its passage, was almost generally observed. He added, that he learned a few days after that this luminous globe had been

* From *Bibliothèque Britannique*, vol. xxii. no. 4, April 1803.

seen by some travellers on Mount-Cenis; and he was informed at the same time that it had fallen in the environs of Ville-Franche under the form of an incandescient stone, a small fragment of which was sent to him.

“ He assured me also, that a comparison he then made of the periods at which the meteor had been observed on Mount-Cenis, at Lyons, and at Ville-Franche, positively announced that it was the same ball which had traversed that line and shown itself in these three points.

“ I expressed to Dr. Pétetin a desire of seeing the fragment of this stone which he had in his possession; and the doctor, judging, no doubt, from the anxiety I showed to obtain information respecting this phænomenon, how much I was interested in it, was so kind as to offer me this fragment in case he should find it.

“ I was the more desirous, indeed, to see the specimen of this mineral mass, as I had it in my power to compare it with analogous specimens, one of which fell near Wold Cottage, in Yorkshire, on the 13th of December 1795, and another near Benares, in Bengal, on the 19th of September 1798, a fragment which I brought with me a few years ago from London, where I received it from count de Bournou, F. R. S., a very celebrated mineralogist.

“ Some time after Dr. Pétetin sent me the fragment in question, and I was much surprised to find that it had a perfect similarity to specimens of those which fell at Benares and at Wold Cottage; a similarity manifest not only in regard to the genus of stones but to the mineralogical species which enter into their composition, and also in regard to the effects resulting from their movement in the atmospheric fluid.

“ As the details which Dr. Pétetin was so kind as to transmit to me along with this valuable present gave me reason to hope that I should be able to discover the exact spot where this globe fell, and the circumstances attending its fall, I made researches in the neighbourhood of Ville-Franche, which were not fruitless; for by successively acquiring more satisfactory information I was at length directed towards the commune of Sales, at about the distance of a league and a half to the north-west of Ville-Franche, in the department of the Rhone, where I learned that most of the inhabitants had been witnesses of, and much frightened by, the arrival of this luminous body, which had fallen in a vineyard within three hundred paces of the village, and near the house of a vintager named Pierre Crepier.

“ I addressed myself to two of the inhabitants best acquainted

quainted with the fact, and who appeared most capable of relating all the circumstances of it with simplicity, and free from that air of the marvellous from which minds not directed by a knowledge of the principles of philosophy can scarcely be preserved. I proceeded with them towards the house of Crepier, and it was there on the spot where the stone buried itself in the earth that I received every information respecting it, and even obtained the last specimen of this stone which Crepier had remaining.

“ The following are all the circumstances I collected in regard to this singular phænomenon, omitting the useless reasoning in regard to its authenticity :

“ On the 12th of March 1798, about six in the evening, the weather being calm and serene, a luminous globe of an extraordinary appearance attracted towards the east the eyes of the inhabitants of the commune of Sales and of the neighbouring villages, as they were returning from their labour; and its rapid approach and horrid humming noise, like that produced by an irregular and hollow body traversing the atmosphere with rapidity, threw all the inhabitants of that commune into the greatest terror, especially when they saw it pass over their heads at a very little elevation. According to their report this ball left behind it a long train of light, and emitted, with an almost continual crackling noise, small blue sparks of fire similar to small stars.

“ Its fall was then observed by three workmen who were not more than fifty paces from it. One of them, named Montillard, a young man who was nearest to it, was struck with terror, and dropped his coat and a billet of wood which he was carrying, in order that he might escape as fast as he could. The other two, named Chardon and Lalous, were no less frightened, and fled to Sales, where a general alarm prevailed. These three witnesses agree in stating that this body moved with astonishing rapidity, and that after its fall they still heard a kind of hissing noise proceeding from the place where it buried itself.

“ In regard to Crepier, he was at home; where he was so much frightened with the hissing of the body in the air, and the noise of its fall, which took place within less than twenty paces of his habitation, that at first he shut himself up with his family in the cellar, and then in his bed-chamber; where fear prevailing over curiosity, he spent the night without daring to go out to examine what had happened.

“ Next morning he was called out by Chardon and Lalous, who had carried with them M. Blondel, adjunct of the commune of Sales, and several other persons, and they all

all repaired together to the place where the luminous body had been seen to bury itself. There, at the bottom of a hollow, eighteen inches in depth, that is to say, of the whole thickness of the vegetable earth, they found a large, black, irregular, ovoid mass, according to their expression like a calf's head. It was entirely covered with a blackish crust; it was no longer warm, and had the smell of gunpowder. They observed also that it was split in several places, so that Chardon by thrusting his bill into one of the fissures made it fall to pieces. I was not able to learn properly whether this fissure was lined by a crust similar to that on the surface; they only thought they remembered that it was partly black. This mass, having been transported to Crepier's house, their first care was to examine the nature of so unexpected an object, and what it contained. The stone therefore was weighed, and immediately broken; but finding only a stone, from avarice, which did not fail to succeed their emotions of fear, they proceeded to a sentiment of indifference for this mass, while the phænomenon was imputed to the most whimsical and supernatural causes, according to the kind of impression which had been communicated to the spectators.

“ The weight of this stone was about twenty pounds.

“ The noise of this event was soon spread; and the commissioner of the executive power to the administration of Ville-Franche being informed of it, he sent to request the stone, with information respecting its fall. A fragment of it, weighing about seven pounds, was brought to him, a part of which, with an account of the phænomenon, he transmitted to one of the members of the conventional assembly. I do not know what attention was paid to it, and what effect it produced, at a time when every mind was absorbed in politics.

“ M. Place, a merchant of Ville-Franche, who was at Sales at the same time I was there, assured me that he was a witness, as well as many inhabitants of Ville-Franche, to the passage of this luminous globe over the town; that he heard its humming noise; that its elevation could not exceed 500 toises; and that its direction was from east by south to west by north.

“ I must add, that the simplicity of most of the reports made to me, their perfect agreement in all the important points, and the great number of persons who saw this phænomenon, which took place at that time of day most favourable for its being generally observed, leave no doubt with me in regard to the veracity of the account which

I have

I have here given, and of the certainty of the fact in question.

“ Some time ago, when conversing on the subject of this phenomenon with professor Pictet, he recollected that at the same period he and a number of the inhabitants of Geneva and the neighbouring towns, as far as Berne, had observed a luminous body which suddenly appeared in the southern regions, proceeding rapidly from east to west. This phenomenon at that time was considered to be a meteor; but he is so fully persuaded that this body is the same which fell at Sales, that he has given me permission to quote his testimony*.

“ To these circumstances I shall join a description of the characters of this mineral substance, in regard to the point of view in which they ought to be considered by a mineralogist; and I shall give the result of the analysis of it made by Vauquelin, member of the Institute.

“ Its colour in the inside is an ash-gray formed by a mixture of whitish parts interspersed with black metallic points.

“ Its texture is gravelly, and the substances, of which I am about to speak, are disseminated in it, as octaëdral iron and sulphuret of iron are in the amphibolic steatites of Corsica, &c.

“ Humidity does not communicate to it an argillaceous odour.

“ The following are the substances found disseminated throughout the mass of this stone. The first is malleable iron in grains which readily rust, and which vary in size from an almost imperceptible point to a line in diameter, and even to the weight of twenty-four grains.

“ These grains of iron are malleable, soluble, and susceptible of attraction by the magnet. They, however, differ from forged iron in having a whiter aspect and less ductility; in being a little oxidated, and containing nickel, as is found by analysis. Those placed in the centre of the mass are in the same state as those found at the surface.

* I have a perfect recollection of the appearance of this meteor. Its light was exceedingly bright, and its motion so rapid, that it was seen only for a few seconds, during which it diffused throughout the whole town an alarming light, though it passed at the distance of more than twenty leagues to the south. Its direction, according to estimation, was precisely towards the quarter in which it fell. My learned colleague, professor Prevost, embraced this opportunity of presenting to the Physical Society established at Geneva a very interesting memoir on the *bolides* or fire-balls, in which he collected considerations respecting the supposed magnitude, distance, and velocity of these meteors, which will excite less surprise at present than they did at that period.—*Note of professor Pictet.*

“ The

“ The second substance is lamellated pyrites (sulphuret of iron) of a white colour inclining a little to that of nickel: it is sometimes dispersed throughout the mass in grains, and sometimes it lines the fissures interspersed in the stone. This pyrites experiences only a very slight, partial, and transient effervescence in acids: it is not susceptible of attraction by the magnet; by the blowpipe it readily gives a frit interspersed with small globules like other pyrites. That in a thin stratum on the sides of the fissures has a gray and less brilliant colour. It contains less sulphur, and approaches near to malleable iron in grains.

“ The third, which is very rare, is under the form of spherical or irregular globules of a dark gray colour, brittle, and having a smooth compact fracture. It produces no effervescence in acids; is not susceptible of attraction by the magnet; is refractory to the blowpipe, and assumes in it only a red and black tint. When scratched with a graver this substance assumes a metallic colour like trap.

“ Besides the gray globules I found also other globular and irregular bodies of an olive-green ground inclining sometimes to yellow, the fracture of which has a fat shining aspect like the steatites of Briançon, and which are not very hard. As several of these bodies lose their distinguishing character, approaching more or less to the appearance of paste; and besides, as they are in very small quantity, I did not think it proper to include them among the number of the contained substances.

“ The surface of this stone is a vitrified black crust, slightly puffed up, which strikes fire with steel; of a quarter of a line in thickness at most, and in the surface of which are observed a few grains of iron and some gray globules, which being more refractory than the rest of the paste have resisted the effects of the heat.

“ It is to these grains and these globules, which in all probability have been detached from the mass by the fusion of the neighbouring parts or by the intensity of the heat, that we ought to ascribe those blue sparks which escaped from the mass during its course.

“ It is also to be remarked, that in a fragment of this stone the vitrification has been effected in a part of an interior fissure lined with pyrites, with this difference, that this vitrification is much more puffed up.

“ The interior of the stone even in the parts nearest to the vitrified crust does not seem to have undergone any change by the action of the fire, only its granulated tissue has that lax aspect observed in all stones which with this kind of
aggregation.

aggregation have undergone a certain degree of heat; an aspect which may be better distinguished than described. In regard to the globular bodies, they cannot be the result of a kind of fusion, since they have none of the characters of glass; and being the most refractory of all those substances of which the stone is composed, they could not pass to the state of vitrification near pyrites, which, though the least alterable substance, has nevertheless retained its lamel-
lated texture.

“ The paste or earthy part even in small fragments ex-
periences in nitric acid a partial solution without losing its cohesion, and being before susceptible of attraction by the magnet is not so afterwards; which announces that the iron only has been attacked by the acid. It however still retains some metallic grains, which are pyrites. Treated by the blowpipe it gives black frit, assumes the metallic aspect, and continues to be attractable.

“ I shall not give the specific gravity of this stone: as it is not homogeneous, and as its gravity depends on the greater or less quantity of metallic particles, and especially on the iron found in it, no positive character can be deduced from this property.

“ To conclude: I can assert that the earthy part of the interior of this mass, as well as each of the contained substances, is in its original state, and that the only changes which the fire has effected are the relaxation in the tissue which I have already mentioned; and perhaps the change of colour, in case it has a particular one. I have also reason to think that in certain respects, and particularly in regard to texture, it approaches some kinds of pot-stone or siliceous serpentine containing metals.

“ I shall conclude this description by transcribing the letter in which Vauquelin communicated to me the result of the analysis he made of this stone:—“ The stone which you gave me to analyse (says he) consists of

Silex	-	-	-	-	46
Oxidated iron	-	-	-	-	38
Magnesia	-	-	-	-	15
Nickel	-	-	-	-	2
Lime	-	-	-	-	2

“ This stone is directly attacked by acids. By the effect of this action sulphurated hydrogen gas is constantly developed. The liquors resulting from the solution of this
stone

stone in acids have a more intense green colour than that which, as appears, ought to arise from the iron it contains.*

“ On pulverizing this stone there were detached globules of iron, one of which weighed about 24 grains. Its colour is whiter than that of ordinary iron, and, though ductile, it is harder than bar iron. It contains sulphur and nickel. Whence it appears that the sulphurated hydrogen gas and nickel found in it arise from the iron it contains.

“ The three parts which are in excess in the result of the analysis arise from the oxygen absorbed by the iron. It ought even to have acquired more of it, and consequently there must be a loss.

“ The experiments of Mr. Howard on the same subject have been published in the *Annales de Chimie*, and you will see that my analysis has a great relation to his. You ought to place the more confidence in it, as I obtained the same results before I was made acquainted with those of Mr. Howard*.

XXXVI. *Account of a Fire-ball which fell in the Neighbourhood of Laigle: in a Letter to the French Minister of the Interior. By C. Biot, Member of the National Institute, dated July 20, 1803.*

I READ yesterday to the Institute an account of the journey I lately undertook by your desire in consequence of the meteor of Laigle. It has been ordered to be printed. An extract from this account may be interesting to you who have contributed to place this astonishing phenomenon beyond a doubt, and perhaps to the Chief Consul, who amidst so many labours still finds means to devote some moments to the sciences.

When I set out from Paris on the 26th of June, I did not proceed directly to Laigle. Had the explosion been as sudden as was announced, it must have been heard at a great distance. It was therefore agreeable to the rules of criticism, first to collect scattered testimonies, and to suffer myself to be gradually conducted by them to the place where the meteor was said to have burst; for in regard to all the circumstances of the explosion the accounts could not but agree, in whatever part they might be collected.

* In the next Number we shall give an account of the stones which fell in the neighbourhood of Ensisheim and of Agen, taken from the same source.

I went first to Alençon, fifteen leagues west-south-west of Laigle, and in going thither I learned that a globe of fire had been seen proceeding towards the north. The appearance of this globe had been followed by a violent explosion. This took place on the 26th of April 1802, at one in the afternoon. By the direction of this phenomenon, the day, and particularly the hour, I judged that this had been the commencement of the meteor of Laigle.

At Alençon nothing had been heard—in consequence, no doubt, of the usual noise which prevails in a large town; but if I received only vague reports, I acquired a very important certainty, by the mineralogical collections of the country, that nothing exists in the neighbourhood of Laigle which has any resemblance to the meteoric stones.

From Alençon I proceeded to Laigle, traversing the villages, conducted by the accounts given me by the inhabitants. All of them had heard the meteor on the day and at the hour mentioned. In this manner I reached Laigle, and proceeded to the house of our colleague Le Blond; and I was happy to find in him the intelligence of a philosopher and the kindness of a friend.

The meteor did not burst at Laigle, but at the distance of half a league from it. I saw the awful traces of this phenomenon; I traversed all the places where it had been heard; I collected and compared the accounts of the inhabitants: at last I found some of the stones themselves on the spot, and they exhibited to me physical characters which admit no doubt of the reality of their fall.

If we first consider the physical testimonies, no meteoric stones had been found in the hands of the inhabitants before the explosion of the 26th of April. The mineralogical collections, formed on the spot with the greatest care for several years, contained nothing of the kind.

The founderies, iron works, and mines, in the neighbourhood which I visited, exhibited nothing in their productions or in their scorizæ which had the least affinity to these substances. No traces of a volcano are found in the country.

All of a sudden, and only since the time of the meteor, these stones have been found on the ground and in the hands of the inhabitants, who are better acquainted with them than any other person.

These stones are found only in a certain extent, in ground foreign to the substances they contain, and in places where, on account of their size and their number, it is impossible they could have escaped notice.

The largest of these stones when broken still exhale a strong sulphureous odour from their interior parts. That of their surface has vanished, and the smallest exhale no sensible odour, so that the odour of the former seems also from its nature likely to be dissipated in the course of time.

These are so many physical proofs which attest that the meteoric stones of the neighbourhood of Laigle are foreign to the places where they have been found; that they were conveyed thither exactly at the time of the explosion, and by a cause which has modified the principles they contain.

If we now consult the moral testimonies, what do we find? Twenty hamlets, dispersed in an extent of more than two leagues square, almost all the inhabitants of which declare themselves to have been eye-witnesses of the meteor, attest that a dreadful shower of stones was projected from it. Among the number there are men, women, and children. They are simple and unlettered peasants, labourers possessed of strong natural sense and reason; respectable ecclesiastics, and young people who having been military men are free from the illusions of fear. All these persons, of professions, manners, and opinions so different, who had very little or no intercourse with each other, agree in attesting the same fact, which they had no interest to invent: they all refer it to the same day, the same hour, and the same moment, making use of the same comparisons; and this fact, so strongly and so generally attested, is only a consequence of the physical proofs previously collected; which is, that stones of a peculiar nature fell in that country immediately after the explosion of April 26.

Besides, traces which strongly attest the fall of these masses, never mentioned without terror, are still shown. The inhabitants say that they saw them descend along the roofs of the houses like hail, break the branches of the trees, and rebound after they fell on the pavement. They say they saw the earth smoke around the largest of them, and that they still burnt after they were in their hands. These accounts are given and the traces shown only in a certain extent. It is there only that meteoric stones are found on the ground. Not a fragment is found beyond that district, and there is not a single person who pretends to have seen any of them fall beyond it.

A third kind of proof results from certain physical peculiarities uniformly related by the inhabitants of the country, who are too little enlightened to have foreseen the consequences. I here allude to the successive changes observed in the hardness of the stones and in the odour exhaled; changes

changes which, according to the report of eye-witnesses, among whom we must reckon our colleague Le Blond, took place in the interval of a few days after the explosion of the meteor, and of which I myself observed very sensible traces on breaking fragments of different dimensions; and this new comparison of testimonies and facts serves only to show a new agreement between them.

All the physical and moral proofs which it has been possible to collect are therefore concentrated, and converge towards one point: and if we consider the manner in which I was led, by a comparison of the testimonies, to the place of the explosion; the number of particulars which I collected on the spot; their coincidence with those which I brought from the distance of ten leagues; the multitude of the witnesses; their moral character; the resemblance of their accounts, and perfect agreement from whatever part obtained, without its being possible to discover a single exception in that respect; it may be concluded, without the smallest doubt, that the fact to which these proofs refer actually took place, and that stones really fell in the neighbourhood of Laigle on the 26th of April 1802.

From the aggregate of the testimonies we have deduced the following description of the phænomenon:

On Tuesday, April 26, 1802, about one in the afternoon, the weather being serene, there was observed from Caen, Pont-Audemer, and the environs of Alençon, Falaise, and Verneuil, a fiery globe of a very brilliant splendour, which moved in the atmosphere with great rapidity.

Some moments after there was heard at Laigle, and in the environs of that city to the extent of more than thirty leagues in every direction, a violent explosion, which lasted five or six minutes.

At first there were three or four reports like those of a cannon, followed by a kind of discharge which resembled a firing of musketry; after which there was heard a dreadful rumbling like the beating of a drum. The air was calm and the sky serene, except a few clouds, such as are frequently observed.

This noise proceeded from a small cloud which had a rectangular form, the largest side being in a direction from east to west. It appeared motionless all the time that the phenomenon lasted. But the vapour of which it was composed was projected momentarily from the different sides by the effect of the successive explosions. This cloud was about half a league to the north-north-east of the town of Laigle: it was at a great elevation in the atmosphere, for

the inhabitants of two hamlets a league distant from each other saw it at the same time above their heads. In the whole canton over which this cloud hovered, a hissing noise like that of a stone discharged from a sling was heard, and a multitude of mineral masses exactly similar to those distinguished by the name of *meteoric stones* were seen to fall at the same time.

The district in which the stones fell forms an elliptical extent of about two leagues and a half in length and nearly one in breadth, the greatest dimension being in a direction from south-east to north-west, forming a declination of about 22° . This direction which the meteor must have followed is exactly that of the magnetic meridian; which is a remarkable result.

The largest of these stones fell at the south-east extremity of the large axis of the ellipse; the middle-sized ones fell in the centre, and the smallest at the other extremity. It thereby appears that the largest fell first, as might naturally be supposed.

The largest of all those which fell weigh $17\frac{1}{2}$ pounds. The smallest I saw weigh about two gros, which is the thousandth part of the former. The number that fell is certainly *above* two or three thousand.

In this account I have confined myself to a simple relation of facts; I have endeavoured to view them as any other person would have done, and I have employed every care to present them with exactness. I leave to the sagacity of philosophers the numerous consequences that may be deduced from them; and I shall consider myself happy if they find that I have succeeded in placing beyond a doubt the most astonishing phenomenon ever observed by man.

XXXVII. *Experiments to ascertain the Value of different Steeps in curing the Smut in Wheat, and promoting its Growth.* By Mr. B. BEVAN.

To the Editor of the Philosophical Magazine.

SIR,

Leighton, Bedfordshire,

July 19, 1803.

I TAKE the liberty of sending you a copy of a table of results in a set of experiments made principally with a view to ascertain the *value of different steeps* in curing the smut in wheat, and promoting its growth; with twelve samples of good wheat A, and twelve samples of very smutty B; each

each sort steeped in twelve different solutions of substances most easily to be procured.

Solutions in which the Wheat was steeped 24 Hours.	February 27, 1862.	Specific Gravity if Steep.	Bushels sown per Acre.	Number of smutty Ears in three Sheaves.		Bushels of good Wheat per Acre of Produce.		Cwt. of Straw per Acre.	
				A.	B.	A.	B.	A.	B.
1. Solu. of potash - - -		1.357	3.51	1	81	21.6	13.6	30.6	29.1
2. — of muriate of potash -		1.097	3.51	3	218	20.2	10.1	30.0	21.1
3. — of nitrate of potash -		1.080	3.51	7	115	23.8	14.3	36.9	31.9
4. — of soda - - -		1.056	3.51	9	159	20.2	11.7	35.6	26.7
5. — of muriate of soda -		1.089	3.51	0	290	24.0	14.5	41.5	33.3
6. — of sulphate of soda -		1.047	3.51	12	241	21.6	12.3	38.5	27.8
7. — of muriate of ammonia		1.026	3.51	1	150	19.8	17.6	35.1	30.2
8. — of common soot - - -		1.025	3.51	0	123	20.8	11.4	34.8	25.3
9. — of lime saturated - - -		1.003	3.51	0	2	21.9	12.4	38.7	25.9
10. — of nitric acid - - -		1.016	3.51	none	grew	-	-	-	-
11. — of muriatic acid - - -		1.011	3.51	0	136	20.7	16.1	35.7	31.1
12. — of sulphuric acid - - -		1.050	3.51	0	0	20.4	17.8	35.4	37.1
13. Dry in its natural state -		-	3.51	6	323	20.3	14.7	35.7	31.0
14. Washed in common water -		-	3.51	none sown	107	-	18.3	-	38.5

The wheat was sown in rows in Leighton field on a sandy soil mixed with little or no calcareous matter, and is but indifferent land for bearing wheat.

Neither of the samples that were steeped in solution of nitric acid came up, except one or two single corns; and which, whether by having more room, or receiving but a less degree of stimulus, grew extremely luxurious. I tried the same steeps with barley, and found the same effect from the nitric acid, as not a single one came up.

The very powerful effect of this solution will induce me to try it again in different degrees of strength; and should the result be important, I shall make it public.

I am, sir,

Your most humble servant,

B. BEVAN.

XXXVIII. *New Method of preparing corrosive Sublimate (hyperoxidated Muriate of Mercury) in the humid Way.*
By M. L. VON SCHMIDT PHISELDECK*.

IT is well known how much apothecaries desirous of preparing their own medicines are indebted to Mr. Westrumb for having furnished them with a method of preparing corrosive sublimate without being exposed to the dangerous vapour it emits during the sublimation. For some time past I have employed myself, merely from scientific views, in preparing corrosive sublimate according to this method. But however much I may be sensible of the advantages of this process, I cannot help regretting the loss sustained in the nitric and muriatic acids, which in general cost so much trouble and expense before they can be obtained pure. I reflected a long time on the means of avoiding this loss, and at length discovered a process much more œconomical than that of the chemist Hameln. The question was, to dissolve the mercury in the cheapest concentrated acid (this acid, without doubt, was the sulphuric acid), and to present to the oxide of mercury the muriatic acid without having separated it from its alkaline base. I resolved then to prepare a solution of mercury in sulphuric acid, and to decompose the sulphate of mercury by muriate of soda. I then hoped that I could easily separate the two salts that were formed by crystallization, as the sulphate of soda for its solution took only eight parts of cold water, whereas corrosive sublimate takes 162; but I found that after the first

* From *Journal de Chimie et de Physique*, par J. B. Van Mons, Pluviôse 15, an. 11.

crystallization the two salts mixed, and that no other means of separating them remained but by alcohol. I shall pass over in silence the operations, which were attended with more or less success in this point of view, and describe only the process which I definitively adopted.

I introduced into a tubulated retort two ounces of mercury and three ounces three gros of concentrated sulphuric acid; I then adapted to the retort a receiver, without luting it, and made a pretty strong fire. During the solution there was disengaged a very considerable quantity of sulphureous gas. When nothing remained in the retort but a white mass, I added a solution of $5\frac{1}{2}$ ounces of marine salt in six ounces of water, and exposed the mixture to strong ebullition for half an hour. A complete solution took place. I filtered the liquid while still in a state of ebullition, put it into a retort, and distilled it to dryness. On the remaining mass I poured 16 ounces of alcohol, and caused it to digest for some hours. I then decanted the liquid from off the residuum, filtered it again warm, and put it once more into the retort after I had washed it, taking care not to spread any of it in the neck of the retort, and distilled it to dryness. I must here remark, that the distilled liquid, which at first had the colour of Malaga wine, assumed, after the solution was concentrated, the colour of water de Rabel; and the saline mass, after the complete evaporation of the alcohol, was exceedingly white. Lime water made no change in the colour of this liquid.

I poured over the mass in the retort 12 ounces of water; I boiled it to solution, and, having filtered the liquor, exposed it to crystallize. Very beautiful crystals in the form of elongated prisms were deposited. I then poured over the residuum eight ounces of new spirit of wine, and again obtained a considerable quantity of corrosive sublimate. The distilled liquid, after being rectified on half an ounce of potash, was perfectly pure.

By employing this method, corrosive sublimate, in my opinion, will cost one-half less than by Westrumb's process. Sulphuric acid costs only one-third of what the nitric acid does; and there is no comparison between the price of pure muriatic acid and that of marine salt. I therefore flatter myself that this method will meet with a favourable reception.

XXXIX. *Observations on the Plant called St. John's Wort.* By C. BAUNACH*;

ST. JOHN'S WORT is a very common plant, which grows in great abundance in the fields, in the woods, and in uncultivated places. Botanists have described its distinguishing characters under the name of *hypericum perforatum*: it is employed in medicine as an excellent vulnerary and balsamic remedy; but unfortunately its juice is of little utility, since it is unknown to the greater part of dyers, in whose art, however, it may be applied with success. St. John's wort is a resinous plant, the flowers and summits of which, filled with seeds, contain a juice soluble in water, in alcohol, and vinegar: it diffuses throughout the first two liquids a red colour like that of blood, and in the latter a most splendid and beautiful crimson: when combined with acids or metallic solutions it presents a beautiful yellow colour; which proves that it contains two colouring matters, one more soluble than the other, that is the red.

To dye cloth, wool, silk, and cotton, yellow, it is sufficient to immerse them in water properly impregnated with the juice of this plant and a certain quantity of mordant. The salt best for being used as a mordant with this colour is sulphate of alumine, combined with a proper proportion of potash (carbonate of potash), in which the stuffs are suffered to remain some time; for it is on the length of the time, the quantity of the mordant, and the heat employed, that the fixity of the colour, and the shades resulting from it, depend. When little mordant is used, the dye is of a yellow colour; by increasing the mordant it inclines to green; and by adding solution of tin in nitro-muriatic acid it assumes rose, cherry, and crimson shades, all very beautiful. The alum, generally employed for all extractive dyes, does not succeed well in the process here alluded to: the addition of potash is essentially necessary, because it decomposes this salt, precipitates its earth, dissolves a considerable portion of it; and it is this alkaline salt with an earthy base which in this operation becomes the true mordant, especially as the colouring principle resides in a matter almost purely resinous.

The juice of St. John's wort united to the mordant here mentioned gives to paper a beautiful yellow colour; and as it produces the same effect on skins, leather-dressers may employ it with advantage for dyeing white sheep- and other skins.

* From the *Annales de Chimie*, No. 137.

The plant in question contains a great deal of tannin. A solution of common glue in water, and several experiments made in this respect, have proved it to me in a convincing manner.

Having poured into the juice of St. John's wort a little solution of the sulphate of iron, there was formed a precipitate of a blackish brown colour which had the property of absorbing oxygen, of becoming at length insoluble in water, and of assuming the characters of a concrete resin.

St. John's wort contains no essential oil. Having subjected a certain quantity of this plant to distillation with water, the product of this liquid had a strong and agreeable odour, in which I could discover no trace of volatile oil.

The juice of St. John's wort does not dissolve in fat nor in volatile oils, but it combines very well with resins. For this purpose, having the juice from the plant, it was poured into flat dishes to be desiccated. This operation may be performed by placing the dishes in an oven some time after the bread has been taken out: it is then reduced to powder, in which state it may be united to turpentine. This solution is easily effected in a copper mortar which has been well heated. The resin saturated in this manner may be mixed with fat and volatile oils. If combined with olive oil it forms a medicine known in pharmacy under the name of *oil of hypericum*, which when prepared in this manner has decisive properties, and may be employed with success. When incorporated with linseed oil it produces a beautiful red varnish, which may be used with advantage for furniture.

It is certain that the juice of St. John's wort is a resinous-extractive substance, in which the resin is considerably predominant. The phenomena of its solution in water, alcohol, and resin, and in particular its inflammability, sufficiently prove it. The last property is so great, that when exposed on a burning coal it burns like incense, and emits very little smoke. It has the property of absorbing the oxygen of the atmosphere; it is no way altered in the air; its taste is somewhat bitter, and weakly astringent.

XI. *Account of a new Kind of American Crocodile.* By
E. GROFFROY*.

THE captain-general Leclerc, being informed by some officers of his staff who had served in Egypt that the croco-

* From *Annales du Museum d'Histoire Naturelle*, No. 7.

dile of Saint Domingo had a great resemblance to that of the Nile, thought it a matter of importance to furnish naturalists with the means of confirming this circumstance: he therefore was desirous of making a sacrifice to us of two crocodiles which had been presented to him. The arrival of the younger, which was brought over alive, was announced at the time in the public journals; but it died just when about to be landed at Havre. The second reached us in Nivose: it was much larger; it had been properly prepared at Saint Domingo, and served as an original for delineating the figure which accompanies this memoir. (See Plate V.)

The fact mentioned by the officers attached to general Leclerc's staff, was not yet known: on the contrary, naturalists were persuaded that America contained only one kind of crocodile*, the principal characters of which were an obtuse muzzle, a cavity in the upper jaw for receiving the fourth lower tooth, and the hind-feet half-webbed. They were therefore much surprised to see arrive from Saint Domingo a crocodile similar to those of the old continent, having, like them, the muzzle oblong, an indentation in the side of the upper jaw to afford a passage to the fourth lower tooth, and the hind-feet entirely webbed. Our first suspicion on receiving this animal was, that the identity of species was proved, and that thus the real crocodile existed in the warm countries of both hemispheres.

This, however, was a result so contrary to one of the finest laws established by Buffon, a law of the greatest importance in zoology as well as in the history of the revolutions of the globe, that I did not think proper to admit this first idea without a more accurate examination.

This law, founded on an observation Buffon had made, that no species of the torrid zone had been primitively placed in both continents, had either never been contradicted, or had been so only by objections the weak foundation of which had been soon discovered.

I therefore compared the crocodile of Saint Domingo with that which I had brought from Egypt, and it gave me pleasure to find that there was a difference between these animals sufficient to make them be considered as two distinct species; for I do not think there is any reason for observing, in opposition, that their differences ought to be ascribed to age. They are both nearly of the same size;

* See the excellent Dissertation of my colleague Cuvier, read in the National Institute, and which he published in Wiedemann's Annals of Zoölogy and Zoötony.

and I was even enabled to confirm, that age in the crocodile gives rise to other differences than those of which I am about to speak. I can mention, by way of proof, the two individuals for which we are indebted to the enlightened zeal of general Leclerc: though different in age and size, they still appeared to me to be perfectly similar.

The crocodile of Saint Domingo resembles that of the Nile in regard to all those characters which serve to distinguish the latter from the caiman: it, however, has the jaws narrower and longer; the breadth of them is to the length as three to six. In the crocodile of the Nile the ratio is that of four to six. The body of the crocodile of Saint Domingo is also proportionably longer, and the tail consists of three bands more, twenty in the one, and seventeen in the other. The first two of the lower teeth are so long that they pierce the upper jaw from one side to the other; whereas they are smaller in that of the Nile, and form for themselves only two small cavities in which they are received. The fourth tooth of the lower jaw of the former can scarcely be distinguished from the two neighbouring ones, while in the other crocodile these fourth teeth are much larger. The plates which cover the back are much fewer in number, and more unequally distributed in the crocodile of Saint Domingo; the ridges of each are only really prominent in the exterior row, all those of the middle are almost entirely effaced: on the other hand, in the crocodile of the Nile every plate and ridge has the same form, the same prominence, and the same respective arrangement. In a word, all the scales, even those which cover the extremities, are perfectly square in the crocodile of Saint Domingo, and round or hexagonal in that of the Nile.

All these differences appear to me to furnish so many inductions proper for making us believe that the crocodile of Saint Domingo forms a species distinct from that of the Nile. But if this fact cannot be established at present in an incontestable manner, there is at least no reason to consider the law established by Buffon as invalidated by the discovery at Saint Domingo of a crocodile with elongated jaws. To give a decisive opinion in regard to this question, it would be necessary to have a more accurate knowledge of the changes which crocodiles may undergo at the different ages; that is to say, whether they are not subject to local influences which produce accidental variations, and to obtain some information respecting their habits.

XLI. *Preparation of a new Luting proper to be used in all chemical Operations.* By C. PAYSSE, Professor of Chemistry*.

IT is generally admitted that the rapid progress which chemistry has made during the last twenty years, is in part owing to the different kinds of apparatus invented by the immortal Lavoisier, and the precautions employed in the art of luting. In this point of view lutings have been of essential service to chemists, since by facilitating the condensation of many æriform products they have afforded us the means of determining their nature, and appreciating their volume as well as their gravity. This truth did not escape the sagacity of the celebrated chemist Chaptal, who in his Elements of Chemistry says: "On the art of luting an apparatus properly the whole success of an operation depends."

Among the substances most used for this purpose, are reckoned the fat luting paste of almonds or of linseed, the oil of which is extracted, and mixed with strong glue, and that of the white of eggs, and new cheese united to lime. These different kinds of luting are attended with inconveniences which render them improper for being employed under all circumstances. Fat luting for example, composed of dry clay and oil, combined with an oxide of lead, cannot be applied but on parts which receive a weak impression from the heat; for they liquefy at a low temperature, soon run, and consequently become unfit for the proposed end: that of linseed and almonds, mixed with glue or gelatine, is often too porous, easy to be destroyed by acids, and by ammonia, when in a gaseous state: those prepared with the white of eggs, and cheese, mixed with lime, are only attended with the inconveniency of becoming too soon solid, and immediately after mixture; so that it is exceedingly difficult to apply them.

The necessity I was under, in preparing oxygenated muriatic acid on a large scale, to find a luting which to the advantage of being cheap should unite that of being soon prepared, and of resisting the destructive action of the vapours of that acid, and that strong action of heat which the luted part is often obliged to sustain; of being easily applied, and in an uniform manner, without too speedily becoming hard, induced me to make some researches, which furnished me with the most satisfactory result,

* From *Annales de Chimie*, No. 137.

After making a great number of mixtures with different substances, I thought it my duty to adopt the following, which gave me a homogeneous compound, which drying as slowly as could be desired, acquired very great hardness and became very compact, so as to have all the properties I wished for:

Take the whites of eggs with their yolks, carbonate of lime in powder, or lime strongly slaked in the air, equal in weight to about one half that of the eggs; and having put the whole on a piece of linen, apply it in the usual manner.

This lating, the composition of which is simple, possesses when dry a certain degree of elasticity. I have formed of it vessels impermeable to water, and susceptible of being polished by the lathe. In a word, this mixture resembles that substance called sea scum, of which tobacco pipes are made.

XLII. *Account of the Travels of M. A. DE HUMBOLDT in South America, extracted from some of his Letters*.*

M. HUMBOLDT's brother, who is now at Rome, received from him lately three letters: one dated Quito, June 3, 1802; another, Cuenca, July 13, 1802; and the third, Lima, the capital of Peru, November 25, 1802. They announce that M. Humboldt will soon return, and that he expected to land in the month of August or September at Cadiz, or Coruana; but the last of his letters in particular is the most interesting. In the following extract from it care has been taken to insert every thing worthy of attention in the other two:

MY DEAR BROTHER, Lima, Nov. 25, 1802.

You must have learned by my preceding letters that I had reached Quito, at which we arrived by traversing the snow of Quindian and Tolima; for as the cordillera of the Andes forms three separate branches, and as we were at Santa Fé de Bogota, on that which is the most eastern, it was necessary to cross the highest to approach the coasts of the South Sea. Oxen are the only animals which can be employed in this passage for transporting baggage. Travellers in general are carried by men called *targeros*. They have a chair, in which the traveller is seated, tied to their back; they travel about four hours journey every day, and in five or six weeks earn only fourteen piastres. We preferred

* From *Magazin Encyclopédique*, Thermidor, an. 11.

travelling

travelling on foot; and the weather being exceedingly fine, we spent only seventeen days in these solitudes, which exhibit no trace of their having ever been inhabited: we slept in huts constructed of the leaves of the heliconia, which travellers carry with them on purpose. On the western side of the Andes there are marshes in which we sunk up to the knees. The weather had changed, and during the last days of our journey there fell such torrents of rain that our boots rotted on our legs; and we arrived at Carthago with our legs naked and covered with bruises, but enriched with a beautiful collection of new plants, of which I have a great number of drawings.

From Carthago we went to Popayan by Buga, crossing the beautiful valley of the river Cauca, and having always at our sides the mountain of Choca, and the platina mines which it contains.

During the month of November 1801 we remained at Popayan, and went to visit the basaltic mountains of Juliusuito; the mouths of the volcano of Puracé, which with a horrid noise throw out vapours of hydro-sulphurous water; and the porphyritic granites of Pisché, which form columns of from five to seven planes, similar to those which I remember to have seen in the Fuganean mountains of Italy, and which are described by Strange.

The greatest difficulty still remained; which was, to go from Popayan to Quito. It was necessary to cross the Paramos from Pasto, and even in the rainy season, which had already commenced. The name of Paramo is given in the Andes to every place at the height of 1700 or 2000 toises, where vegetation ceases, and where a cold which penetrates to the bones is experienced. To avoid the heats of the valley of Patia, where people in the course of one night are seized with fevers which continue three or four months, and which are known under the name of *calcuturas de Patia* (fevers of Patia), we passed the summit of the cordillera by horrid precipices, in order to proceed from Popayan to Almager, and thence to Pasto, situated at the bottom of a terrible volcano.

Nothing can be more frightful than the entrance and outlet of this valley, in which we spent the Christmas holidays, and where the inhabitants received us with the utmost hospitality. They were covered with thick forests, situated among marshes where the mules sunk half up to the backs, and we passed ravines so deep and so narrow that we thought we were entering the galleries of a mine. The roads therefore are paved with the bones of mules which have perished here

here of cold and fatigue. The whole province of Pasto, comprehending the environs of Guachucal and Tuqueres, is a cold plain, almost above that point at which vegetation can take place, and surrounded by volcanoes and soufrieres which continually throw up clouds of smoke. The unfortunate inhabitants of these deserts have no other food but potatoes; and when these fail, as they did last year, they go into the mountains to eat the trunk of a small tree called *achupalla* (*pourretia pitcarnia*). As this tree however is the food also of the bears of the Andes, the latter often dispute with them the only nourishment which these elevated regions afford. To the north of the volcano of Pasto I discovered in the small Indian village of Voisaco, at the height of 1370 toises above the level of the sea, a red porphyry with an argillaceous base inclosing vitreous feldspar and hornstone, which has all the properties of the serpentine of the Fichtel-Gebirge. This porphyry has very evident poles, and shows no attractive force. After having been wet day and night for two months, and exposed to the danger of drowning by a very sudden rise of the waters, accompanied with earthquakes, we arrived on the 6th of January 1802 at Quito, where the marquis of Salvaalegre was so kind as to prepare for us a house, which after so many fatigues afforded us all the conveniences that we could have wished for at Paris, or at London.

Quito is a beautiful town, but the sky is dismal and cloudy. The neighbouring mountains exhibit little verdure, and the cold is very considerable. The great earthquake of February 4th 1797, which agitated the whole province, and destroyed in a moment from thirty-five to forty thousand people, has also been fatal to the survivors. It has so changed the temperature of the air, that the thermometer generally stands at from 4° to 10° of Reaumur, and rarely ascends to 16° or 17° ; while Bouguer always observed it at 15° or 16° . Since that catastrophe there have been continual earthquakes: and what shocks! It is probable that the whole elevated part is only one volcano. What are called the mountains of Cotopaxi and Pinchincha are only small summits, the craters of which form different apertures all terminating in the same hollow. The earthquake of 1797 unfortunately proved this hypothesis, for the earth everywhere opened and vomited up sulphur, water, &c. Notwithstanding these horrors and dangers with which nature has surrounded the inhabitants of Quito, they are cheerful, lively, and agreeable. Their town breathes nothing but pleasure; and no where does there appear so decided a taste for amuse-

ments. It is in this manner that man is accustomed to sleep soundly on the brink of a precipice.

We resided nearly eight months in the province of Quito; that is to say, from the beginning of January to the month of August, and employed that time in visiting the different volcanoes. We examined in succession the summits of Pinchincha, Cotopaxi, Antisana, and Ilniça, spending from a fortnight to three weeks on each of them; and always returning in the intervals to Quito, which we left on the 9th of June 1802 to proceed to the environs of Chimborazo, which is situated in the southern part of the province.

I twice ascended, viz. on the 26th and 28th of May 1802, to the edge of the crater of Pinchincha, a mountain which overlooks the town of Quito. Before, no person, as far as I know, except Condamine, ever saw it; and Condamine himself arrived there only after five or six days of fruitless researches, and without instruments; and, on account of the excessive cold, could remain on it only twelve or fifteen minutes. I succeeded in carrying thither my instruments; I made important measurements, and collected some of the air to analyse it. The first time I ascended I was accompanied only by an Indian. As La Condamine approached the crater at the lower part of its edge covered with snow, I made my first attempt by following his traces; but we were in danger of perishing. The Indian fell into a fissure up to the breast; and we observed with horror that we had walked on a bridge of frozen snow, for at the distance of some paces from us there were holes through which daylight appeared. We then found ourselves on arches which adhere to the very crater. Alarmed, but not discouraged, I changed my project. From the circumference of the crater there arise, projecting themselves as I may say over the abyss, three peaks or rocks not covered with snow; because the vapours exhaled from the mouth of the volcano continually dissolve it. I climbed up one of these rocks, and found at its summit a stone, which being supported at one end only, and hollow below, projected over the precipice in the form of a balcony. Here I stationed myself to make experiments. But this stone was only about twelve feet in length and six in breadth, and was strongly agitated by frequent shocks of an earthquake, of which I counted eighteen in less than thirty minutes. That we might examine the bottom of the crater better, we lay down on our bellies; and I do not think that the imagination can conceive any thing more gloomy and frightful than what we then saw. The mouth of the volcano forms a circular hole of nearly a league

league in circumference, the edges of which, cut perpendicularly, are covered with snow at the top. The inside is very black, but the gulph is so immense, that the summits of several mountains placed there can be distinguished. These summits seemed to be 300 toises below us: you may judge then where their bases must be. I have no doubt that the bottom of the crater is on a level with the town of Quito. La Condamine found this crater extinct, and even covered with snow; but we had melancholy news to carry to the inhabitants of Quito, that the volcano in their neighbourhood was now burning. We were convinced of this beyond all doubt by the most evident signs. When we approached the mouth of it we were almost suffocated by sulphureous vapours. We even saw blue flames moving about here and there, and every two or three minutes we experienced strong shocks of an earthquake, with which the edges of the crater were agitated, and of which nothing was perceived at the distance of 100 toises. I suppose that the great catastrophe of February 7th 1797 kindled up the flames also of Pinchincha. After visiting this mountain alone I returned two days after, accompanied by my friend Bonpland, and Charles de Montufar the son of the marquis de Selvaalegre. We were furnished with more instruments than the preceding time, and measured the diameter of the crater and the height of the mountain. We found the former to be 754 toises*, and the latter 2177. In the interval of two days which took place between our excursions to Pinchincha we had a very violent earthquake at Quito. The Indians ascribed it to some powder which I must have thrown into the volcano.

During our journey to the volcano of Antisana, the weather was so favourable that we ascended to the height of 2773 toises. The barometer fell in that elevated region to 14 inches 7 lines; and in consequence of the rarity of the air the blood flowed from our lips, gums, and even eyes. We experienced extreme weakness, and one of the persons who accompanied us fainted. It was before thought impossible to ascend higher than the summit called Corazon, which is 2470 toises in height, and which La Condamine reached. On analysing the air brought from the highest point to which we ascended, it gave 0.008 of carbonic acid for 0.218 of oxygen gas.

We paid a visit also to the volcano of Cotopaxi, but it was impossible for us to reach the mouth of the crater. It is not

* The crater of Vesuvius is only 312 toises in diameter.

true that this mountain has become lower since the earthquake of 1797.

On the 9th of June 1802 we left Quito to proceed to the southern part of the province, where we wished to examine and measure Chimborazo and Tunguragua; and to take a plan of the whole country convulsed by the grand catastrophe of 1797. We succeeded in approaching to within about 250 toises of the summit of the immense colossus of Chimborazo. A ridge of volcanic rocks, destitute of snow, facilitated our ascent. We ascended to the height of 3031 toises, and felt ourselves incommoded in the same manner as we had been on the summit of Antisana. Two or three days even after our return to the plain we were still subject to an indisposition, which we could ascribe only to the effect of the air in these elevated regions, which by analysis gave us 20 hundreds of oxygen. The Indians by whom we were accompanied deserted us before we reached that height, saying that we intended to kill them. Bonpland, Charles Montufar, and one of my domestics, who carried a part of my instruments, were the only persons who remained with me: nevertheless, we could have continued our journey to the top had we not been prevented by a fissure too deep to be crossed. We therefore thought proper to descend. Being ill secured from the cold of these elevated regions, we suffered very much; and I in particular had the misfortune to lacerate my foot by a fall a few days before, which subjected me to great pain in a road where we every moment struck our toes against sharp stones, and were obliged to examine the ground at every step. La Condamine found the height of Chimborazo to be nearly 3217 toises. Trigonometrical measurement, which I made at two different times, gave 3267 toises, and I have reason to place some confidence in my operations. All this enormous colossus, as well as all the high mountains of the Andes, consists not of granite, but of porphyry, from the base to the summit; and the porphyry is 1900 toises in thickness. The short stay which we made at that enormous height was dismal and melancholy: we were enveloped by a thick fog, which only suffered us from time to time to have a glimpse of the horrid abysses by which we were surrounded. No living creature, not even the condour, which on Antisana continually hovered over our heads, was to be seen. Small kinds of moss were the only organized beings which reminded us that we were still in the neighbourhood of the earth.

It is almost probable that Chimborazo, like Pinchincha and

and Antisana, is of a volcanic nature. The ridge on which we ascended consists of burnt and scorified rock, mixed with pumice stone. It resembles all the currents of lava in this country; and continues beyond that point where I was obliged to set bounds to my researches, towards the summit of the mountain. It is possible that this summit may be the crater of an extinguished volcano; and this is even probable. The idea of this possibility, however, makes one shudder—and with reason; for, if the volcano should be rekindled, this colossus would destroy the whole province.

The mountain of Tunguragua sunk down at the period of the earthquake of 1797. Bouguer makes its height to be 2620 toises; I found it to be only 2531: it has lost therefore nearly 100 toises of its height. The inhabitants of the neighbouring country assert that they have seen its summit crumble down before their eyes.

During our stay at Riobamba, where we spent some weeks with the brother of Charles Montufar, who is corregidor there, we by chance made a very curious discovery. The state of the province of Quito before the conquest of the inca Tupayupagi * is absolutely unknown. But the king of the Indians, Leandro Zapla, who resides at Lican, and whose mind is highly cultivated, has in his possession manuscripts written by one of his ancestors in the sixteenth century, which contain the history of that period. These manuscripts are written in the language of Paraguay, which formerly was the general language of Quito; but in the course of time it gave place to that of the Incas, or the Anehma, and is now lost. Fortunately, another of Zapla's ancestors amused himself in translating these manuscripts into Spanish. We made extracts from these valuable documents, and particularly in regard to the memorable period of the eruption of the mountain called *Nevalo del Atlas*, which must have been the highest in the universe, superior even to Chimborazo, and which the Indians called *Capa-Uren*, 'the chief of mountains.' Ouania Abomatha, the last independent *cochocando* (king of the country), reigned at that time at Lican. The priests informed him that this catastrophe was a sinister presage of his destruction. "The face of the universe," said they to him, "is changing: other gods will expel ours. Let us not oppose what has been ordained by fate." The Peruvians indeed introduced into the country the worship of the sun. The eruption of the mountain continued seven years, and Zapla's manuscript asserts that the shower of ashes at Lican was so abundant that continual night pre-

* Quito was conquered by the Peruvians in 1470.

ailed during that period. When the quantity of volcanic matters which are found in the plain of Tapia around the enormous mountain which then crumbled to pieces, is considered, and when we reflect that Cotopaxi has often involved Quito in darkness for fifteen or eighteen hours, we may believe that the exaggeration of this account is not very great. This manuscript, the traditions which I collected at Parime, and the hieroglyphics I saw in the desert of Casiquiare, where at present no vestiges of mankind remain, added to the ideas offered by Clavigero respecting the emigration of the Mexicans towards the southern part of America, have given rise to some conjectures on the origin of these people, which I purpose to explain as soon as I can find leisure.

I have applied also with great assiduity to the study of the American languages, and I have seen how much what La Condamine says of their poverty is false. The Carib language is rich, beautiful, energetic, and polished: it is in no want of expressions for abstract ideas. It speaks of posterity, eternity, existence, &c.; and the numerical signs are sufficient to express all the possible combinations of figures. I applied in particular to the Inca language: it is generally spoken in company; and is so rich in delicate and varied phrases, that the young men, in order to say soft things to the ladies, when they have exhausted all the resources of the Castilian, begin to speak Inca. These two languages, and others equally rich, are sufficient to prove that America formerly possessed a greater degree of culture than the Spaniards found there in 1492. But I have collected still further proofs, not only at Mexico and in Peru, but even at the court of the king of Bogota, a country the history of which is absolutely unknown in Europe, and whose mythology even and fabulous traditions are highly interesting. The priests were acquainted with the art of drawing a meridian line, and observing the moment of the solstice. They reduced the lunar year to a solar by intercalations; and I have in my possession a heptagon stone, found near Santa Fé, which they employed for calculating these intercalary days. But what is still more, even at Eravato, in the interior of Parime, the savages believe that the moon is inhabited by men; and know by tradition from their ancestors, that it derives its light from the sun.

From Riobamba I proceeded by the famous paramo of Assuay towards Cuença, after having visited the large sulphur mines of Tirrau. It was this mountain of sulphur which the negroes who revolted in 1797, after the earthquake, attempted to set on fire. This no doubt was the

most desperate project ever attempted, for they hoped by these means to form a volcano which would swallow up the whole province of Alaussy. At the height of the paramo of Assuay, an elevation of 2300 toises, are the magnificent ruins of the Inca's highway. It conducted almost to Cuzco, was entirely constructed of cut stone, and very straight, and resembled the most beautiful of the Roman roads. In the same neighbourhood are found also the ruins of the palace of the inca Tupayupangi, of which La Condamine gave a description in the Memoirs of the Academy of Berlin. In the quarry which furnished the stones there are still seen several half cut. I do not know whether Condamine spoke also of the so-called Inca's billiard-table. The Indians name this place in the Quichua language, *Inca-chungana*, the Inca's game. But I much doubt whether it was ever destined for that purpose. It is a seat cut out in the rock, with ornaments in the arabesc form, in which it is believed that the ball ran. There is nothing more elegant in our gardens in the English style; and every thing proves the good taste of the inca, for the seat is so situated as to command a delightful view. In a wood not far from this place is found a round spot of yellow iron in freestone: the Peruvians have ornamented it with figures, supposing it to be the image of the sun. I made a drawing of it.

We remained only ten days at Cuença, and proceeded thence to Lima through the province of Jaen, where we spent a month in the neighbourhood of the river of the Amazons. We arrived at Lima on the 23d of October 1802.

In the month of December I purpose proceeding from this place to Acapulco, and thence to Mexico, that in the month of May 1803 I may reach the Havanna, where I shall embark without delay for Spain. I have given up, as you may see, the idea of returning by the Philippines. I should have crossed an immense tract of the ocean without seeing any thing but Manilla and the Cape; or, if I had attempted to proceed to the East Indies, I should have wanted the necessaries for that voyage, and which it was impossible for me to procure here.

We have had forty or fifty young crocodiles, on the respiration of which I have made very curious experiments. Other animals diminish the volume of the air in which they live, but the crocodile increases it. A crocodile immersed in 1000 parts of atmospheric air, which contain 274 of oxygen gas, 15 of carbonic acid gas, and 711 of azot, increases this mass in one hour and forty-three minutes 124 parts;

and these 1124 parts contain then, as I found by exact analysis, 106.8 of oxygen, 79 of carbonic acid, and 938.2 of azotic gas, mixed with other unknown gaseous substances. The crocodile then in one hour and three quarters produces 64 parts of carbonic acid, and absorbs 167.2 of oxygen; but as 46 parts are found in the 64 parts of carbonic acid, it appropriates to itself only 121 parts of oxygen; which is very little, considering the colour of its blood. It produces 227 parts of azote, or other gaseous substances, on which the acidifiable bases exercise no action.

I made these experiments in the town of Munpox with lime water and nitrous gas prepared with great care. The crocodile is so sensible to carbonic acid gas and to its own exhalations, that it dies when put into air corrupted by one of its own species. It however can live two or three hours without breathing at all. I made these experiments on crocodiles seven or eight inches in length: notwithstanding this smallness of size, they are capable of cutting off a finger with their teeth, and they have the courage to attack a dog. These experiments are troublesome, and require great circumspection. We have made very minute descriptions of the caïman or crocodile of South America; but as the descriptions of that of Egypt which I saw before my departure from Europe were not equally circumstantial, I cannot venture to determine whether they are of the same species. The Institute of Egypt must undoubtedly have obtained details which will remove all doubt in regard to this point. This much however is certain, that there are three different species of crocodile in the tropical regions of the new continent, to which the inhabitants give the names of *bava*, *caïman*, and *crocodile*. No naturalist has yet sufficiently distinguished these species. These monsters, as at New Barcelona, are sometimes of so peaceable a nature that people bathe before them; and sometimes, as at New Guiana, they are so mischievous and ferocious, that during the time we were there they devoured an Indian on the quay in the middle of the street. At Oratuen we saw an Indian girl, eighteen years of age, whom a crocodile seized by the arm. She had the courage to put her other hand into her pocket to pull out her knife, with which she gave the monster so many wounds in the eyes that he let her go, but cut off the arm near the shoulder. This girl's presence of mind was as astonishing as the skill displayed by the Indians in speedily curing so dangerous a wound: one might have said that the arm was amputated and dressed at Paris.

Near Santa-Fé there are found in the Campo de Gigante,
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at the height of 1370 toises, an immense number of fossil elephants' bones, both of the African species and of the carnivorous kind discovered near the Ohio. We caused several to be dug up, and have sent some specimens of them to the National Institute. I much doubt whether any of these bones were ever before found at such a great height: since that time I have received two from a place of the Andes situated about two degrees of latitude from Quito and Chili, so that I can prove the existence and destruction of these gigantic elephants from the Ohio to the country of the Patagonians. I shall bring with me a fine collection of these bones for M. Cuvier. About fifteen years ago the entire petrified skeleton of a crocodile was discovered in a calcareous rock in the valley of the Magdalen: it was broken through ignorance, and it was impossible for me to procure the head, which existed not long ago.

XLIII. *The Art of moulding Carving in Wood.* By LENORMAND, Professor of Natural Philosophy in the Central School of the Department of Tarn*.

INGENIOUS or curious men are often thwarted in the execution of their projects by the difficulty of finding in the places where they reside workmen capable of assisting them in the articles for which they may have occasion. Small towns in particular furnish only indifferent workmen; and besides, they do not contain artists of every kind. Good carvers, for example, reside only in large towns; and these even are not very common. I had seen plasterers supply the want of good modellers by incrusting in their decorations plaster moulded on excellent models. I therefore conceived that it might be possible to mould carving in wood, to be afterwards applied to cabinet-makers' work. This idea I did not at first carry into execution; but two or three years after, having occasion for some pieces of carving, I invented a new art †, as will be seen by what follows. Necessity rendered me industrious, and I at length accomplished my object.

Wishing to obtain a case for a pendulum clock I had constructed, I drew a plan of it; and presented it to an excellent cabinet-maker in the small town in which I resided.

* From *Bibliothèque Physico-Economique*, June 1803.

† This art is not new; but the experiments of the author may furnish useful hints to artists.—EDIT.

He would undertake only the plain work, and referred me for the execution of the carving to Toulouse or Bourdeaux. I was sensible how difficult it would be to get the carving of the different pieces executed at a distance, and particularly within the required time; and how expensive it would be to transport such a case, which might also be damaged by the way. I told him that I would myself undertake the carving of the laurel and oak foliage which I had placed in the plan, provided he would undertake the remaining part. Fearing, however, that my carving would not correspond to his work, and might tend to degrade it, he was unwilling to undertake any thing till I had shown him a specimen of my labour—a proposal to which I consented.

I was well aware that very hard wood, such as box, might be moulded by putting it under a press in copper moulds, after having subjected it to certain preparations: but for this purpose very expensive moulds, an excellent press, &c. are required, which occasions considerable expense, and by this method bas-reliefs only can be executed. But the art I am about to describe requires only cheap materials with very little practice, and affords the means of making not only figures in relief, but even the most difficult objects of sculpture.

In the town where I resided I found one of those Italians who employ themselves in moulding plaster figures. I caused him to make such moulds as I had occasion for, and which were copies from the best masters. I succeeded perfectly in moulding my garlands in walnut-tree wood; and I showed them to my cabinet-maker, who took me for an able sculptor. He constructed the case, applied to it the foliage I had made, and neither he nor any person who saw it had the least suspicion of the method I had employed. All believed that the ornaments had proceeded from the chisel of an able carver. Since that time I have moulded for my friends bas-reliefs, trophies, &c. with wood of every kind. I shall now describe my

Process.

I made very clear glue with five parts of Flanders glue and one part of fish glue or isinglass. I dissolved these two kinds of glue separately in a large quantity of water, and mixed them together after they had been strained through a piece of fine linen to separate the filth and heterogeneous parts which could not be dissolved. The quantity of water cannot be fixed, because all kinds of glue are not homogeneous, so that some require more and some less. The
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 proper

proper degree of liquidity may be known by suffering the mixed glue to become perfectly cold: it must then form a jelly, or rather a commencement of jelly. If it happens that it is still liquid when cold, a little of the water must be evaporated by exposing the vessel in which it is contained to heat. On the other hand, if it has too much consistence, a little warm water must be added. In a word, the proper degree will be ascertained by a few trials.

The glue thus prepared is to be heated till you can scarcely endure your finger in it: by this operation a little water is evaporated, and the glue acquires more consistence. Then take fine raspings of wood or sawdust, sifted through a fine hair-sieve, and form it into a paste, which must be put into moulds of plaster or sulphur after they have been well rubbed over with linseed- or nut-oil, in the same manner as when plaster is to be moulded. Care must be taken to press the paste in the mould with your hand, in order that it may acquire all the forms of the mould: then cover it with an oiled board, and, placing over it a weight, suffer it in that manner to dry. The desiccation may be hastened and rendered more complete by a stove. When the impression is dry remove the rough parts, and if any inequalities remain behind they must be smoothed; after which the impression may be affixed with glue to the article for which it is intended. Then cover it with a few strata of spirit of wine varnish, as is done in general in regard to carved work, or with wax in the encaustic manner. It requires much attention to discover that such ornaments are not carved in the usual manner. Gilding may be applied to them with great facility. This operation is exceedingly easy; nothing is necessary but moulds; and with a little art the ornaments may be infinitely varied.

I tried also to mould figures, and completely succeeded. These, however, require more care. I first make a paste, similar to the former, with very fine sawdust, and place a stratum, of about two lines in thickness, on every part of the mould; after which it is left to dry almost entirely. In the mean time I prepare a coarse paste with coarse sawdust which has not been made to pass through a fine but a coarse sieve, and instead of Flanders glue I employ common glue, which is less expensive, adding to it a sixth of fish glue. I first put together two parts of the mould, after introducing into the joints a slight stratum of the fine paste, which I make very clear and apply with a small brush. I fill up the vacancy between the two pieces with coarse paste. I then apply the third piece as I did the second, and so on
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until the whole are adjusted, always filling up the vacancies with coarse paste. I suffer the whole to dry in the mould, and obtain a figure in relief of solid wood executed with all the delicacy of plaster figures. Care must be taken to remove with a sharp knife, or small file, the prominences formed by the joinings. If the figure be not suffered to dry too much, these prominences may be easily removed with the point of a sharp penknife. It will be necessary to learn the art of determining the proper degree of desiccation; for if the figure be taken from the mould before it is properly dried it will become warped, and if it be too dry it cannot be corrected but with a file, which is tedious and laborious, whereas if the proper moment be seized the paste may be cut like wax; especially if the sawdust has been fine which is necessary for the exterior strata. The figures may then be completely dried in a stove, by which means they will acquire a degree of desiccation and solidity hardly to be conceived. Figures thus moulded may be bronzed or varnished: they will then be unalterable by the effects of moisture or dryness.

I have already said that Flanders and not common glue ought to be employed for the exterior strata, because this glue is almost colourless*; whereas the other, being dark-coloured, gives too obscure a tint even to walnut-tree wood. Being desirous to try whether my moulded figures would be unalterable by the effects of moisture or dryness, I made the following experiments:

Experiment I.

I exposed in a large bell-glass filled with atmospheric air two figures, one of which was varnished and the other not. I placed under the bell Saussure's hygrometer and a capsule filled with water, after having moistened the sides of the bell. The air was soon saturated with water, and the hygrometer marked 100 degrees. I observed no alteration whatever in the varnished figure, and the other exhibited no other sensible alteration than a commencement of solution in the glue, so that on applying my finger to its surface it was found to be somewhat viscid; in a word, the figure was not in the least warped.

Experiment II.

I then introduced my two figures and the hygrometer into another very dry bell, under which I had placed a cap-

* When this cannot be had, a glue fit for the purpose may be made by boiling shreds of parchment in common water till dissolved.—EDIT.

sule filled with calcined potash. The moisture of the air by which the figures were surrounded was soon absorbed, and the hygrometer indicated zero. In order to ascertain whether the whole moisture imbibed by the unvarnished figure was entirely dissipated, I left every thing *in statu quo* for four hours, the hygrometer still indicating zero. I then took out the two figures, neither of which had experienced the least alteration.

Experiment III.

I repeated the first experiment with a view to cause the two figures to absorb as much moisture as possible; and when the hygrometer marked 100° I took them from the bell, and suddenly introduced them into a stove the heat of which was 50° of Reaumur. The unvarnished one became dry without cracking, and the other showed a little softening in the varnish. This effect I ascribed to the imperfect desiccation before the experiment, for the softening was more considerable than is generally the case when a varnished body is exposed to heat.

These experiments appeared to me sufficient to induce me to conclude, that sculpture in moulded wood, according to the process here described, is unalterable by moisture or drought, for in our climates the thermometer never rises to 50°. Such sculptured figures have the solidity of wood, and are even preferable to it; for a slight blow given to wood, if cut across the fibres, will detach some of the parts; whereas figures formed of artificial wood, if I may be allowed the expression, are homogeneous in all their parts, and are not so easily broken.

Besides the advantages which this invention on the first view exhibits, it offers others which may be of great utility to our arts and manufactures.

1st, In the large manufactories of mirrors the ornaments in general are in a very bad taste and miserably executed, because the carvers are very ill paid. If this new method be adopted, sculptors would pay more attention to their first work: they would mould their ornaments in plaster or in sulphur, then take a multitude of copies with the greatest facility, and these ornaments would add to the value of our furniture.

2d, Inlayers would make much more elegant works by employing pastes of different coloured woods, which might be managed with greater ease than the thin pieces of coloured boards which they employ. I am now engaged with
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some experiments on this subject. My intention is to make small tablets to imitate mosaic. I shall communicate the result to the public as soon as my experiments are terminated.

XLIV. *Evidence of the precise Date of the Cow-Pock in America.*

To Mr. Tilloch.

SIR,
FINDING that the honour of the first instances of the vaccine inoculation in America is not bestowed upon the gentleman to whom it is due, and that there have been even some little contradiction and mistakes among the English practitioners with regard to which of them first introduced the new practice into that country by furnishing matter, I trust the following statement of facts may establish the historical truth in question.

In the winter of the year 1799 Dr. John Chichester, a practitioner of the first distinction in Charleston, South Carolina, and to whom I have been pupil, received vaccine matter from his learned friend and former teacher Dr. Pearson, accompanying the first publications written on the cow-pock by Dr. Jenner and himself. With this matter several persons were inoculated, but the disease was produced in one case only. This was a mulatto boy named Robert, about seven or eight years old, the property of Thomas Tunno, esq. merchant. The small-pox matter was subsequently inserted, in the most careful manner, without effect. It was some time after the occurrence of the above case, before those which have been published as the first instances in America really happened.

It may be proper to notice that my late worthy master Dr. Chichester was not supported by the approbation of his brethren in his introduction of the vaccine inoculation in America, notwithstanding the high authorities above mentioned who first proposed it to the public.

I remain, sir, your humble servant,

NATH. H. RHODES.

P. S. Since my arrival in London I have seen doctor Waterhouse's latest Treatise on the Variola Vaccina, 8vo. Cambridge 1802: hence I am enabled to fix the precise date of *his first* inoculation from his own words, viz.:—"I commenced the experiment July 8, 1800, on my own children,

children, four of whom, with three of my domestics, passed regularly through the distemper; and they soon after went into the licensed small-pox hospital in this neighbourhood, and all seven of them were inoculated by Dr. Aspinwall with the matter of the small-pox, without the least trait of infection." Page 5.

The error concerning the inoculation of the cow-pock in America would not have happened if Dr. Chichester's account had not failed in getting to Europe; nor would the first introduction have been imputed to the Vaccine Institution, as was supposed from a passage in Dr. Lettsom's book on the cow-pock, viz. "The vaccine matter which first succeeded with professor Waterhouse was transmitted from England in a bottle with a glass stopper*." Page 24.

London,
July 24, 1803.

XIV. *Account of the Life and Labours of the late Mr. RAMSDEN, in a Letter from Professor PIAZZI, of Palermo, to M. DE LALANDE†.*

WHEN I had the pleasure of seeing you lately at London, you admired, as I did, the genius and works of the celebrated Ramsden, which has induced me to address to you such circumstances as I have been able to collect respecting the life and labours of this incomparable artist. No one has contributed more to the progress of astronomy than you have done by your zeal, and by your works on the principles and calculations of that science; and Mr. Ramsden is certainly the first for inventing and constructing instruments: but as he is not so well known in France, perhaps, as he deserves to be, my letter may serve to give your countrymen a just idea of his merit.

Jesse Ramsden was born at Halifax, in Yorkshire, on the 6th of October 1730. At an early period he conceived a strong desire of devoting himself to literature, and especially to history and antiquities: the mathematics and chemistry engaged his attention also in their turn: but his father was anxious that he should pursue some occupation which might be useful to him; and as he was a clothier, young Ramsden applied to the same employment till he had

* This mode of transmitting matter was peculiar for a time to the Vaccine Institution, now at No. 44, Broad-street, Golden-square.

† From the *Journal des Sçavans*, Nov. 1783.

attained to the age of twenty-one. He then went to London, to seek for some occupation more suited to his genius. Besides other things, he applied to engraving under Burton* : and a fortunate circumstance conducted him to that object for which nature seemed to have destined him, which was to be the reviver and father of the instrumental part of astronomy. Mathematical instruments were often brought to him to be engraven: the more he examined them the more he was sensible of their defects, and a secret instinct made him desirous of constructing better ones. He therefore resolved to make an attempt in this line: he soon acquired the use of the file, and made himself acquainted with the method of turning brass, and even of grinding glasses. In the year 1763 he constructed instruments for Sisson, Dollond, Nairne, Adams, and other mathematical instrument makers. He then established a shop on his own account, in the Hay-market, about the year 1768; from which he removed to Piccadilly in 1775. Having formed a design of examining all the astronomical instruments, he resolved to correct those which being founded on good principles were defective only in the construction, and to set aside those which were wrong in both these respects.

Hadley's sextant, which is so much employed in the British navy, appeared to him one of the most useful, but it was then very imperfect; the essential parts were not of sufficient strength; the centre was subject to too much friction; and the index could be moved several minutes without any change being produced in the position of the mirror; the divisions in general were very coarse; and Mr. Ramsden found that the abbé de la Caille was right, when he estimated at five minutes the error which might take place in the observed distances of the moon and stars, and which

* Mr. Burton was a thermometer and barometer maker, and divider of instruments. Instruments at this period were divided by means of a plate applied to them, and the divisions were in this manner marked off. Mr. Burton was one of the best workmen of his time, and worked for Short, Bird, and other eminent artists. Mr. Ramsden bound himself apprentice to Mr. Burton for four years; and after his time was expired entered into partnership with Mr. Fairbone, who lived afterwards in New-street, Shoe-lane. This partnership, however, did not long continue. Mr. Ramsden opened a shop on his own account in the Strand, and, having married miss Dollond, became possessed of a part of Mr. Dollond's patent for achromatic telescopes. Mr. Ramsden in the course of a few years removed to the Hay-market, and then to Piccadilly, where he died in the year 1800. Mr. Ramsden had seven children; but none of them are now alive, except one son, captain John Ramsden of the East India company's service, and late commander of the *Dorchester*. —

might occasion in the longitude an error of fifty nautical leagues. Mr. Ramsden therefore changed the construction in regard to the centre, and made these instruments so correct as to give never more than half a minute of uncertainty. At present he warrants sextants of fifteen inches radius to within six seconds. Since the time when he improved these instruments he has constructed 983; and several of them having been carried to India and America, the error has been found to correspond with what he determined it to be before their departure. He has since made sextants from fifteen inches to an inch and a half radius, and in the latter the minutes can be clearly distinguished; but in general he prefers those of ten inches, as being easier managed and susceptible of the same exactness.

The invention of a dividing machine having now become necessary, he employed himself in constructing one, which he did with the greatest success. The dividing machines before used were far from being exact. Graham and Bird employed beam compasses. The latter kept his method a secret till it was purchased from him by the board of longitude, in order to be published. Mr. Ramsden had already discovered a method of his own, which in exactness surpassed that of Bird. For large works he still uses the beam compasses; but as it is necessary in the greater number of common instruments to save time, he has employed himself for ten years in improving his dividing machine, in which ease and expedition are united. You have seen that admirable machine with which a sextant could be divided in the course of twenty minutes, and which is sufficient to give an idea of the inventive genius and superior talents of Mr. Ramsden. It was your friend Dr. Shepherd who made this excellent machine known to the board of longitude, who gave the inventor a premium of 600*l.* sterling, and caused an engraving to be made of it in 1777; but the edition was accidentally burned, and you are right in wishing to cause it to be engraved at Paris. The machine is still in the hands of Mr. Ramsden, and he has undertaken to divide sextants for three shillings. The board of longitude has often given greater premiums for objects of less utility; but the greatest men do not always obtain the greatest rewards. Newton, indeed, got a place in the mint; but he was not indebted for it to his merit alone.

Mr. Ramsden has constructed an instrument also for dividing straight lines, a description of which has been printed; and I am sorry that you have no longer at Paris that invented by M. Megnic, in order that they might be compared.

While

While Mr. Ramsden was employed on his dividing machine, he improved at the same time other instruments. The theodolite before consisted merely of a telescope, turning on a circle divided at every three minutes, by means of a vernier; but in the hands of Mr. Ramsden it has become a new and perfect instrument, which serves for measuring heights and distances as well as for taking angles. I saw in his possession the largest and most wonderful of all theodolites, employed by general Roy for measuring the triangles which at present join those of France, and by which there cannot be an error of a second, though it is only eighteen inches radius. It is furnished with two telescopes, which each turn on a horizontal axis, and by which the angles between objects more or less elevated are reduced to the horizon, and measured. General Roy has lately measured the angle between the pole star and the sides of his triangles, in order to have the convergency of the meridians such as it is in our oblate spheroid. These operations have already shown that the difference between the meridians of the observatories of Paris and Greenwich is $9' 20''$.

The barometer destined for measuring the heights of mountains has been much improved by Mr. Ramsden. His method of marking at the bottom the line of the level, and of looking at the top to the contact of the index with the summit of the mercury, renders it possible to distinguish the hundredth part of a line, and to measure heights within a foot. He showed M. de Luc that it is the summit of the column, and not the part which touches the glass, that ought to be observed; and he caused to be engraved a table, which accompanies his barometers, and which, without calculation, gives the heights of places according to the height of the barometer, and even for different degrees of heat. He has simplified also, in the most ingenious manner, the apparatus for carrying and supporting this portable barometer.

Various other philosophical machines have been made by Mr. Ramsden, and always with new improvements: such as an electric machine; a manometer for measuring the density of the air; an instrument for measuring inaccessible distances, and which renders it unnecessary to measure a base; assaying balances which turn with a ten-thousandth part of the weight; levels exceedingly sensible; the optic rectangle, prismatic eye-glasses where much fewer rays are lost than by the reflection of an inclined mirror, when it is necessary to look on one side; the dynameter, with which he measures the magnifying power of a telescope, by applying before the eye-glass a small scale divided into hundredths

of a line to measure the pencil or image of the object-glass. This was the original invention, but it was afterwards much improved.

The pyrometer, destined to measure the dilatation of bodies by heat, afforded exercise also for the talents of Mr. Ramsden; and with the happiest success, as may be seen in the *Philosophical Transactions* for 1785. Mr. Ramsden, on examining the pyrometer then in use, had observed the radical defect of that instrument, in which the bodies subjected to experiment were not sufficiently separated. But with his microscopic pyrometer he found means to compare the natural state of a body with the same body exposed to any degree of heat or of cold, and by a micrometer adapted to the microscope he measured these variations with an exactness before unknown, and which furnished the measurement of a base with a precision ten times greater than in any of those ever before measured. On this occasion, as on all others, Mr. Ramsden showed a natural sagacity in discovering the essential faults of every instrument, and in inventing the most simple and most exact methods of correcting them.

Optics are no less indebted to him. He found means to correct the aberration of sphericity and refrangibility in compound eye-glasses applied to all astronomical instruments, and in a new and perfect manner. Opticians had imagined that this could be accomplished by making the image of the object-glass fall between the two eye-glasses; which was attended with this great inconvenience, that the eye-glass could not be touched without deranging the line of collimation, and the value of the parts of the micrometer. To remedy this inconvenience Mr. Ramsden set out from a very simple experiment, namely, that the edges of an image observed through a prism are less coloured according as the image is nearer the prism; and, in consequence of this truth, he sought for the means of placing the two eye-glasses between the image of the object-glass and the eye, without failing to correct the two aberrations, which he did by changing the radii of the curves, and placing the glasses in a manner altogether different from that commonly employed.

He invented also a reflecting object-glass micrometer, a description of which may be seen in the *Transactions* of the Royal Society of London for 1779. In his paper on this subject he points out the defects and inconveniences of that of Bouguer, first invented in 1748, in which the different positions of the eye, in regard to the pencil of light, cause the two images to appear sometimes to touch each other, sometimes to be separated, and sometimes alternately by a

sort of oscillation. He found also that the aberration of the rays, which renders the object badly defined, increased the inconvenience of that instrument. He thought it would therefore be necessary to abandon the principle of refraction, and to substitute that of reflection. This instrument, as simple as ingenious, contains no more mirrors or glasses than what are necessary for the telescope; and the separation of the two images depends only on the inclination of the mirrors, and not on the focus.

He however employed himself in improving the refracting micrometer, and conceived the happy idea of placing this micrometer not towards the object-glass, but exactly in the conjugate focus of the first eye-glass. This micrometer is composed of two plano-convex lenses, which can be moved and form two images, as in the object-glass micrometers, but with this difference, that the rays before they fall on the plano-convex lenses pass through a lens convex on both sides, at a certain distance towards the object-glass. By these means the contrary refraction of the two plano-convex lenses, and the convex lens, corrects the error which takes place in object-glass micrometers, where the image depends only on the focus of the two plano-convex lenses. The image being already considerably magnified before it falls on the refracting micrometer, the imperfection of the glasses can occasion only an insensible error in the measurement of angles. It is true, indeed, that by this position the field of the micrometer will be smaller than what it would be were the micrometer near the object-glass. Mr. Ramsden devised means also for making the images to be uniformly illuminated in every part of the field. With this micrometer the diameter of the planets may be measured in every direction; it may be adapted to achromatic telescopes of every kind; it may be brought near to or removed from the object-glass at pleasure, to render vision distinct; and it may be taken from the tube of the eye-glasses, that the telescope may be employed without a micrometer. All these advantages have given great reputation to Ramsden's micrometers, and the astronomer who can obtain one of them is fortunate.

In consequence of these and other inventions Mr. Ramsden was elected a fellow of the Royal Society in 1786.

The objects hitherto mentioned, however, are not the most important of the works of Mr. Ramsden. The equatorial, the transit instrument, and quadrant, received in his hands new improvements. The equatorial first constructed by Sisson, and which was somewhat improved by Short, was
much

much further improved by Ramsden. He first suppressed the endless screw, which by pressing on the centre destroyed its precision. He placed the centre of gravity on the centre of the base, and caused all the movements to take place in every direction. He pointed out the means of rectifying the instrument in all its parts; and he applied to it a very ingenious small machine for measuring or correcting the effect of refraction. This invention is much anterior to that given by Mr. Dollond in the *Philosophical Transactions*. Mr. Ramsden had a patent for this kind of equatorial. The honourable Stewart Mackenzie, brother of the earl of Bute, the friend and patron of Mr. Ramsden, wrote a description of this machine, which has been printed. But Mr. Ramsden does not always strictly adhere to the description: his inventive genius rarely allows him to construct the same instrument many times in the same manner; and it often happens that he breaks to pieces instruments which have cost a great deal of labour, if they are not as correct as he wishes.

The largest equatorial instrument ever constructed is that destined for Sir George Shuckburgh, on which Mr. Ramsden has been employed for nine years. The circle of inclination is four feet in diameter, so that observations can be made nearly within a second. The telescope is placed between six pillars, which form the axis of the machine; and the whole turns around two pivots resting on supporters of masonry-work.

The transit instrument is employed in all the large observatories of Europe; but Mr. Ramsden has added to it several improvements. He invented a method of illuminating the wires, by making the light pass along the axis of the machine. The reflector is placed in the inside, and obliquely in the middle. He did not lessen the aperture of the object-glass: and as the light passes through a coloured prism, which may be moved at pleasure, the light may be increased or diminished. For adjusting this essential instrument, Mr. Ramsden invented a method which supersedes the use of a spirit-of-wine level, on which he sets no value, because it does not give that exactness which it is always his aim to obtain. He suspends a thread and plummet before the telescope placed vertically: this thread passes over two points, which are marked on two pieces fixed one above and the other below the telescope, and one of which has a small motion. The thread is absolutely detached from the telescope; and when it corresponds on the same points in the two different situations of the telescope,

the observer is certain that the axis is horizontal, as you have remarked in your Astronomy. What is newer and more ingenious in Mr. Ramsden's method is, that the thread and plummet sometimes pass over the images only of the points which are formed in the focus of a lens, because he is sometimes obliged to remove the thread to a considerable distance from the instrument and from the points; but the exactness is not lessened, and there is no parallax.

Ramsden's meridian telescopes, such as those at Blenheim, at Mannheim, at Dublin, and such as those made for the observatories of Paris and Gotha, are also remarkable for the excellence of the object-glasses. Mr. Usher, in a letter from Dublin, says that he can see in the open day stars of the fourth magnitude, and those of the third very near their conjunction with the sun. These telescopes are eight feet in length. I had the satisfaction to obtain one of five feet for my observatory of Palermo; but it is so good, and the sky is so serene, that I hope to have the same advantage in my observations.

The mural quadrant is the most important of all the astronomical instruments, and Mr. Ramsden has distinguished himself here also by the exactness of his divisions: he places the thread and plummet behind the instrument, in order that it may not be necessary to remove it when observations are made near the zenith. His method of illuminating the object-glass and at the same time the divisions, and of suspending the telescope, is also new, and consequently more perfect. In those of eight feet which he made for the observatories of Padua and Vilna, and which Dr. Maskelyne examined, the greatest error does not exceed two seconds and a half. One for Milan is in a great state of forwardness, and is of the same size.

The mural quadrant of the duke of Marlborough at Blenheim, which is six feet, is one of the instruments which you and I admired. It is fixed to four pillars which turn on two pivots, so that the instrument may be placed north and south in a minute. This instrument is as beautiful as perfect, and no one deserves more to possess it than his grace the duke of Marlborough: professed astronomers are not more zealous, assiduous, or correct. It was for this noble instrument that Mr. Ramsden invented a method of rectifying the arc of 90 degrees, respecting which an able astronomer had started some difficulties; but with a horizontal thread, and a thread and plummet forming a kind of cross which does not touch the quadrant, he showed him that there was not a second of error in 90 degrees; and that
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the difference arose from a mural quadrant of Bird, where the arc of 90 degrees contained several seconds too much, and which had not been verified by so exact a method as his.

But the quadrant is not the instrument which Mr. Ramsden values most. It is the whole circle; and he has proved that to attain to the utmost degree of precision of which observation is susceptible, we must renounce the quadrant entirely. His principal reasons are, 1st, The least variation in the centre is perceived by the two points diametrically opposite. 2d, As the circle is turned the plane is always rigorously exact; which cannot possibly be the case in the quadrant. 3d, Two measurements can always be had of the same arc; which serves for verifying the accuracy of the observation. 4th, The first point of the division can be verified every day with the greatest ease. 5th, The dilatation of the metal is uniform, and can produce no error. 6th, This instrument is a meridian telescope as well as a mural. 7th, It becomes a moveable azimuth circle by adding a horizontal circle below the axis, and then gives the refractions independently of the measure of time. You therefore approved the resolution I had formed of confining myself to this instrument, and of not quitting London till I could carry with me a circle of five feet, which Mr. Ramsden was constructing for the observatory of Palermo: as soon as mine is finished he promises to put in hand that destined for Paris. He then hopes to finish that for Dublin, which is twelve feet, and which you saw in an advanced state; but a circle of 7 or 8 feet is sufficient to give precision within half a second, as in the zenith sector, which is employed for the most rigorous observations in regard to the figure of the earth.

You remarked with the greatest satisfaction the ingenious manner in which the axis is supported, that it may have no friction on the pivots, and especially Mr. Ramsden's new invention for rendering the axis perfectly horizontal, by means of a thread and plummet, which is however without the machine; and you had the pleasure of seeing that inventive genius exercise his talents on this new problem, and solve it in the completest manner. As his talents have been exercised in a wide field, he has collected in his shops workmen in all professions which relate to the construction of mathematical and philosophical instruments, that he might have every thing made under his own inspection. The same workman is always confined to the same branch, and by these means acquires the utmost degree of correctness. But notwithstanding this perfection, which ought to

enable Mr. Ramsden to make a fortune, he sells his instruments cheaper than any other artist in the same line at London: the difference is sometimes a third. Though he has nearly 60 workmen in his employment he is not able to execute all the orders which he has received from every part of the world, and you yourself have experienced how difficult it is to obtain instruments from him.

No person can be more reasonable, more attentive to business, or more indifferent for pleasure or for riches than Mr. Ramsden: he is exceedingly frugal in his manner of life; and, unless provoked, no one can be more polite, milder, or more complaisant. I hope, sir, you will publish with pleasure this tribute of my gratitude to a man of uncommon talents, whom you esteem as much as I do, and who, in his turn, has conceived for you a real attachment.

XLVI. Some Details respecting the Voyage of the two French Corvettes, Le Geographe and Le Naturaliste, sent out under the Command of Captain BAUDIN for the Purpose of making Discoveries.

THESE two corvettes sailed from the north-west port of the Isle of France on the 25th of April 1801, and on the 27th of May discovered the land of New Holland in lat. $34^{\circ} 36'$, long. $111^{\circ} 44'$: the land they saw was that called Leeuwin's Land, which forms the south-west extremity of New Holland, and which the charts indicate as little known.

Preparations were then made for exploring the country, and the two corvettes were employed in this service from the 27th of May to the 14th of June. As these were the first geographical operations made on board these vessels, it appears that they are not very correct; and captain Baudin has sent home no account of them, nor any of the charts which he caused to be constructed. He announces that on his return from Port Jackson he proposes to revisit Leeuwin's Land, and to repeat the operations undertaken, in order to construct a chart of Bai du Geographe, which he found in that part.

Among the charts brought home by the *Naturaliste* is one of the same coast of Leeuwin's Land, executed on board the *Naturaliste* by C. Faure: it contains an extent of coast from lat. $34^{\circ} 23'$ to $32^{\circ} 13'$, but coarsely delineated and without any details.

After these operations the two corvettes were separated, and did not again meet till they arrived at the Island of Timor; but both of them sailed along the coast of the Land of Endracht, which lies to the north of Leeuwin's Land.

Captain Baudin entered the Bay of Seals on the 27th of June, and remained there till the 13th of July. Here Bernier, the astronomer, made some observations, which fix the longitude of the northern point of Barren Island at $109^{\circ} 13' 46''$. The course of the *Geographe* in this bay and on the coast to the north of it is traced out on a chart constructed by C. Boulanger, one of the engineers belonging to the expedition. This chart, however, is only a copy of a Dutch chart given to captain Baudin before his departure, and which C. Boulanger corrected by the results of astronomical observations.

Captain Baudin observes in his memoir, that his first operations in regard to this coast, and that of De Witt's and of Leeuwin's Land, were not satisfactory; and he proposes to repeat them on his return from Port Jackson. He has therefore sent home none of the charts of the early part of his voyage along these coasts. Among the charts which the *Naturaliste* has brought home there is only one small chart of a part of the coast of New Holland, where the *Geographe* landed; and which is supposed to be on the coast of De Witt's Land, or of the north-west. This chart, constructed by Rousard, an officer of the marine engineers, is merely a sketch taken at sight, and may give some idea of the observations made on board the *Geographe*; though, as captain Baudin announces, it contains only fragments of the coasts. He says he transmitted to the minister of the marine, in a letter dated Timor, October 6, 1801, some details respecting his navigation on the western coasts of New Holland from Leeuwin's Land, and that he then announced that they were by no means satisfactory. He arrived at Timor, and entered Coupang Bay on the 22d of August 1801.

The *Naturaliste* being separated from the *Geographe* on the coast of Leeuwin's Land proceeded to the Island of Rottenest, which had been agreed on as the first place of rendezvous in case of separation. The captain explored that island as well as another in the neighbourhood, and which is not marked in the charts: he called it *Isle-aux-Ours*. He examined at the same time Swan's River, on the coast of New Holland, opposite to the Isle of Rottenest: some boats were ordered to proceed up that river as far as they possibly could without danger; and a chart of it was constructed,

which is inserted in the journal of C. Hamelin, the commander of the *Naturaliste*.

The same corvette sailed along the western coast of New Holland from lat. 32° south, which is that of the Isle of Rottenest as far as the Bay of Seals, or of Dirk Hartoge; and a chart of that coast was constructed on five large sheets. This chart, however, is merely a sketch made at sight, and can serve only to give a general idea of the course. It contains none of those details exhibited in the Dutch chart of the same coast.

The *Naturaliste* having remained some time in the Bay of Seals, waiting for the *Geographic*, took that opportunity of exploring this bay, and the result is a chart very different from any before published. It deserves consideration, and may be useful to navigators who in future may touch at this bay, in the different gulphs of which they will find resources of which no idea was before entertained.

From the Bay of Seals the *Naturaliste* steered a direct course for the Island of Timor, without following the coast of Endracht's Land or that of De Witt's, and arrived in Coupang Bay on the 20th of September, having left the Bay of Seals on the 3d. Captain Baudin has added to the charts which he sent home, copies of some Dutch charts of the islands of the Indian Archipelago, which he made during his stay at Timor.

The two corvettes left Timor in company on the 13th of November 1801, and proceeded to D'Entrecasteaux's channel on the south-east coast of Van Diemen's Land, where they arrived on the 13th of January 1802. They explored every part of that channel with the greatest care; and captain Baudin announces that they found nothing in it to be rectified. "It is hardly possible," says he, "to find any work more correct, or better executed, than that of the geographers who have made us acquainted with these places for touching at; and we shall be well satisfied if we hear the navigators who succeed us give the same account of our labours, in regard to the coasts which no one ever visited before us."

Captain Baudin only observes that the land called Tasman's Land, in the chart of D'Entrecasteaux's channel, is not an island; and that it is joined to Van Diemen's Land by an isthmus of about 80 or 100 paces in breadth. It is to be observed that D'Entrecasteaux's boats did not proceed so far in this channel as those of captain Baudin; and the observation he makes in regard to Tasman's Land is correct. He observes also that the Bay of Frederick Hendrick is not in the place where it is laid down in D'Entrecasteaux's chart,
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and he gives a long dissertation on this subject. D'Entrecasteaux was not in that part, and the Bay of Frederick Hendrick was given from old charts.

After ascertaining the correctness of the chart of D'Entrecasteaux's channel, captain Baudin did not think it necessary to make a new one, as he had nothing to add to it. He confined himself to the construction of one of Frederick Hendrick's Bay, every part of which he explored.

In the chart of Van Diemen's Land, constructed in 1798 and 1799 by captain Flinders, and which was engraved at the depot in order that captain Baudin might have a sufficient number of copies, the eastern coast of Van Diemen's Land was traced out in a vague manner, as countries little known are in general. Captain Baudin was therefore charged to explore it in a complete and correct manner; and this operation he undertook on leaving the Bay of Frederick Hendrick. Among the charts he has sent home there is one of the Island Maria, the coast of which was explored and correctly traced out; another of the coast of Van Diemen's Land between Maria and Schouten's Islands; and a third of Schouten's Islands and the adjacent coast. It is seen by the last-mentioned chart that there is only one Schouten's Island, instead of the five laid down in captain Flinders' chart. The rest form a long peninsula joined to Van Diemen's Land by an isthmus.

There is a chart also of the remainder of the eastern coast of Van Diemen's Land, from Cape Pelé in lat. $40^{\circ} 8'$ to Swan's Island in lat. $40^{\circ} 42'$. This chart is the result of the course followed by C. Boulanger, who had been sent on shore to explore the country, and who was not able to return on board. Having waited some days for a boat to come and fetch him, he resolved to proceed along the coast which he supposed the corvettes would sail along; and he fortunately arrived at Swan's Island and Banks's Straits, where he found an English ship, the captain of which promised to carry him to Port Jackson; but some days after he fell in with the *Naturaliste*, which took him on board.

The two corvettes had been separated some time, and were endeavouring to fall in with each other. Both of them proceeded to Van Diemen's Land, but without meeting. They visited Dalrymple River, which is situated about the middle of that coast, and another called North River; both proceeded also to the coast of New Holland, which forms the northern coast of Bass's Strait. The *Naturaliste* explored the coast from Cape Wilson to Port Western. That port also was visited, and plans were made of both. After
this

this operation the *Naturaliste* made for Port Jackson, in hopes of finding there the *Geographe*.

Captain Baudin, after visiting different parts of Bass's Straits in hope of meeting with the *Naturaliste*, determined to explore the southern coast of New Holland, which was entirely unknown. He first proceeded to Cape Wilson, from which he took his point of departure, and directed his course west, following the coast to the distance of fifteen degrees of longitude. About the middle of his course he fell in with captain Flinders, who had left England eight months after him, and who was charged to make the same researches as captain Baudin on all the coasts of New Holland. He had cruised along the southern coast from Leeuwin's Land to the point where he fell in with captain Baudin, and two days before meeting him had discovered a large and beautiful island, to which he gave the name of Kangaroo Island. This island is situated in lat. $35^{\circ} 50'$ south, and long. $135^{\circ} 4'$, and appears to be about thirty leagues in extent from east to west. Captain Flinders had passed through the channel which separates it from the land, and had seen none of the southern part.

Captain Baudin, continuing his course, fell in with this island, which he found such as it had been described by captain Flinders: like him he passed on the north side, and did not see the southern part; but on the north side he found two gulphs which proceeded a great way inland, and which he entered, to explore the whole extent of them: he however could see only one side, because the other was filled with sand-banks and shoals, which did not permit the vessel to approach the land.

When he came out of these gulphs he continued his course towards the west as far as the Isles of St. Peter and St. Francis, which were nearly the term of the researches of D'Entrecasteaux on that coast: he then proceeded to the south-east to reach Port Jackson, where he found the *Naturaliste*:

This discovery of captain Baudin is highly important, as it completes the survey of the southern coast of New Holland, which is entirely owing to France. As yet we can form no opinion in regard to the degree of correctness with which it has been explored, because C. Baudin has sent home only a part of the chart which he constructed, and as the chart itself is only a sketch: he has added to it a chart which only indicates his course, with the soundings along that coast; and he promises to send the other part of the coast by the first opportunity.

Captain

Captain Baudin has joined to these charts twelve views well executed, and by which he has endeavoured to give an idea of the nature of the country. They relate only to Leeuwin's Land; but he promises others of the same kind in regard to every part of New Holland which he has visited.

On his departure from Port Jackson he purposed,

1st, To explore King's Island lately discovered in Basse's Straits, and situated to the north-west of Hunter's Isles.

2d, The large island called Kaugaroo Island, the southern part of which is unknown.

3d, The two large gulphs situated to the north of Kangaroo Island, and which he can examine in every part by means of the Kasuarina, a small vessel which he procured at Port Jackson, and which draws little water.

4th, The northern part of the Islands of St. Peter and St. Francis, and where the discoveries he has made unite with those of D'Entrecasteaux.

5th, Leeuwin's Land, and that of Endracht, which he has already seen, but not in a satisfactory manner.

6th, De Witt's Land, where he knows he shall experience great difficulties, but where he hopes to find also interesting objects.

7th, In the last place, the Gulph of Carpentaria, which will be the boundary of his researches.

The two vessels sailed from Port Jackson, Nov. 18, 1802, twenty-five months after their departure from France, and on the 6th of December anchored in the Bay of Sea Elephants in the eastern part of King's Island in lat. $39^{\circ} 51'$, long. $141^{\circ} 34'$.

Two days after, captain Hamelin, having received his ultimate orders, separated from the *Geographe* and the *Kasuarina* in order to proceed to France.

When on the point of sailing, an English galliot anchored near them. This vessel had been dispatched for the purpose of exploring Port Philips on the south-west coast of the Bay of Frederick Hendrick, in Van Diemen's Land, and the river in the north of the same land very near D'Entrecasteaux's channel; to construct charts of these places; and to wait at the latter for the arrival of the *Porpoise*, which was to carry thither the troops necessary for forming a settlement. They learned also by this vessel, that the *Lady Nelson* brig, which sailed from Port Jackson along with the *Investigator*, had returned thither, having lost all her anchors, and been obliged to make one of wood. They had separated from captain Flinders on the 2d of October

1802 in lat. 20° north, very near the coast. The latter had also lost three anchors, and had several times struck; as had been the case with the *Lady Nelson*, which by these means had broken her sliding keel. Captain Flinders continued his voyage to the Gulph of Carpentaria.

XLVII. *Observations on the Chemical Nature of the Humours of the Eye.* By RICHARD CUENEVIX, Esq. F.R.S. and M.R.I.A.*

THE functions of the eye, so far as they are physical, have been found subject to the common laws of optics. It cannot be expected that chemistry should clear up such obscure points of physiology as all the operations of vision appear to be; but some acquaintance with the intimate nature of the substances which produce the effects cannot fail to be a useful appendage to a knowledge of the mechanical structure of the organ.

The chemical history of the humours of the eye is not of much extent. The aqueous humour had been examined by Bertrandi; who said that its specific gravity was 975, and therefore less than that of distilled water. Fourcroy, in his *Système des Connoissances Chimiques*, tells us that it has a saltish taste; that it evaporates without leaving a residuum; but that it contains some animal matter, with some alkaline phosphate and muriate. These contradictions only prove that we have no accurate knowledge upon the subject.

The vitreous humour is not better known. Winttingham has given its specific gravity (taking water at 10000) as equal to 10024; but I am not acquainted with any experiments to investigate its chemical nature.

We are told by Chrouet, that the crystalline lens affords, by destructive distillation, fœtid oil, carbonate of ammonia, and water, leaving some carbon in the retort. But destructive distillation, although it has given us much knowledge as to animal matter in general, is too vague a method for investigating particular animal substances.

I shall now proceed to mention the experiments I have made upon all the humours. I shall first relate those which were made upon the eyes of sheep (they being the most easily procured), and shall afterwards speak of those of the human body, and other eyes. I think it right to observe, that all these eyes were as fresh as they could be obtained.

* From the *Transactions of the Royal Society for 1802.*

SHEEP'S EYES.

Aqueous Humour.

The aqueous humour is a clear transparent liquid, of the specific gravity of 10090*, at 60° of Fahrenheit. When fresh, it has very little smell or taste.

It causes very little change in the vegetable re-active colours; and this little would not, I believe, be produced immediately after death. I imagine it to be owing to a generation of ammonia, some traces of which I discovered.

When exposed to the air, at a moderate temperature, it evaporates slowly, and becomes slightly putrid.

When made to boil, a coagulum is formed, but so small as hardly to be perceptible. Evaporated to dryness a residuum remains, weighing not more than 8 per cent. of the original liquor.

Tannin causes a precipitate in the fresh aqueous humour both before and after it has been boiled, and consequently shows the presence of gelatine.

Nitrate of silver causes a precipitate, which is muriate of silver. No metallic salts, except those of silver, alter the aqueous humour.

From these and other experiments it appears that the aqueous humour is composed of water, albumen, gelatine, and a muriate, the basis of which I found to be soda.

I have omitted speaking of the action of the acids, of the alkalis, of alcohol, and of other re-agents, upon this humour. It is such as may be expected in a solution of albumen, of gelatine, and of muriate of soda.

Crystalline Humour.

To follow the order of their situation, the next of the humours is the crystalline.

This differs very materially from the others.

Its specific gravity is 11000.

When fresh, it is neither acid nor alkaline. It putrefies very rapidly. It is nearly all soluble in cold water, but is partly coagulated by heat. Tannin gives a very abundant precipitate; but I could not perceive any traces of muriatic acid when I had obtained the crystalline quite free from the other humours. It is composed, therefore, of a smaller proportion of water than the others, but of a much larger proportion of albumen and gelatine.

* All these specific gravities are mean proportionals of several experiments. The eyes of the same species of animal do not differ much in the specific gravity of their humours.

Vitreous Humour.

I pressed the vitreous humour through a rag, in order to free it from its capsules; and in that state, by all the experiments I could make upon it, I could not perceive any difference between it and the aqueous humour, either in its specific gravity (which I found to be 10090, like that of the other,) or in its chemical nature.

M. Fourcroy mentions a phosphate as contained in these humours; but I could not perceive any precipitation by muriate or nitrate of lime; nor did the alkalis denote the presence of any earth, notwithstanding M. Fourcroy's assertion of that fact.

HUMAN EYE.

I could not procure a sufficient quantity of these, fresh enough to multiply my experiments upon them. However, by the assistance of Mr. Carpué, surgeon to his majesty's forces, I fully convinced myself that the humours of the human eye, chemically considered, did not contain any thing different from the respective humours of the eyes I had examined. The aqueous and vitreous humours contained water, albumen, gelatine, and muriate of soda; and the crystalline humour contained only water, albumen, and gelatine. The specific gravity of the aqueous and vitreous humours I found to be 10053, while that of the crystalline was 10790.

EYES OF OXEN.

I found the eyes of oxen to contain the same substances as the respective humours of other eyes. The specific gravity of the aqueous and vitreous humours is 10088, and that of the crystalline 10765.

What is particularly worthy of notice is, that the difference which appears to exist between the specific gravity of the aqueous or vitreous humour and that of the crystalline, is much greater in the human eye than in that of sheep, and less in the eye of the ox. Hence it would appear that the difference between the density of the aqueous and vitreous humour and that of the crystalline, is in the inverse ratio of the diameter of the eye, taken from the cornea to the optic nerve. Should further experiments show this to be a universal law in nature, it will not be possible to deny that it is in some degree designed for the purpose of promoting distinct vision.

In taking the specific gravity of the aqueous and vitreous humours, no particular precaution is necessary, except that they ought to be as fresh as possible. But the crystal-

line humour is not of an uniform density throughout; it is therefore essential that attention be given to preserve that humour entire for this operation. I found the weight of a very fresh crystalline of an ox to be 30 grains; and its specific gravity was, as I before stated, 10765. I then pared away all the external part, in every direction, till there remained but 6 grains of the centre; and the specific gravity of these 6 grains I found to be 11940. From this it would seem that the density increases gradually from the circumference to the centre.

It is not surprising that the crystalline humour should be subject to disorders, it being wholly composed of animal matter of the most perishable kind. Fourcroy says that it is sometimes found osseous in advanced age. Albumen is coagulated by many methods; and, if we suppose that the same changes can take place in the living eye as in the dead animal matter of the chemists, it will be easy to account for the formation of the cataract; a disorder which cannot be cured but by the removal of the opaque lens. If a sufficient number of observations were made respecting the frequency of the cataract in gouty habits, some important conclusions might be drawn as to the influence of phosphoric acid in causing the disorder, by the common effect of acids in coagulating albumen.

XLVIII. On a Change observed in the Colour of Prussian Blue by coming in Contact with Iron. By THOMAS GILL, Esq.

To Mr. Tilloch.

SIR,

HAVING frequently discoursed with you on the various colours producible from iron, I make no apology for troubling you with this letter. Happening this morning to be grinding some Chinese blue colour with parchment, size, and water, I noticed that a knife I employed in mixing it became of a greenish tinge: upon this I resolved to prosecute the experiment, when the following singular result was obtained:—After spreading a little of the colour upon the blade of the knife with a camel's hair pencil, I diluted and took off enough of the colour to form a tint upon paper, and as soon as that was laid on proceeded to take off a second, a third, &c. portion, without adding more colour to the knife, until I had obtained thirty-six different tints, each varying

varying in colour from the original blue. The changes were to a greenish blue, a green, an olive green, a yellowish green, a yellow, and so on to a buff colour, where I ceased from further prosecuting the search.

I then tried the same experiment with a cake of Newman's Prussian blue; and, repeating the process quicker, I obtained from the same original quantity of colour first laid on the knife no less than eighty-six different tints, each varying in colour as in the former experiment.

Indigo treated in a similar manner did not change its tint in the least degree under the process.

This discovery may lead to very important consequences in the theory of colour-making; and it furnishes some curious facts, namely, that the Chinese are acquainted with the art of making a colour similar to the Prussian blue; and that indigo should be preferred to the Prussian blue when we wish our colours to be durable, and not subject to change on coming in contact with iron: it should also serve as a caution never to employ the pallet knife or iron in any form in treating Prussian blue.

It would appear from the result of this experiment, either that the Prussic acid, one of the constituent principles of Prussian blue, is not saturated in that combination with iron, but is still able to exercise an action on that metal in its metallic state, or else that the acid itself becomes decomposed, giving up its oxygen to the iron, and producing the ochrey tint which changes gradually the blue to green.

We are possessed of one tinge only of green colour produced from iron, namely, Prussian green, with the process for obtaining which I am unacquainted.

Allow me before concluding to mention another fact with which some of your readers may not be acquainted. The superiority of the Chinese colours over those of Europe is owing chiefly to their being ground much finer, and being mixed with size instead of gum, which prevents their having any gloss.

XLIX. *On the Marine Spencer, invented by* KNIGHT
SPENCER, *Esq.*

IN our last Number we gave a short description of this invention. To show its great utility, we need only to mention a few of the many possible cases in which it may be employed

ployed with advantage. It may be proper to premise, that being made of an article at all times to be had in abundance, and of no present value whatever, and being very simple in its construction, it may be made by old people in poor-houses, by Greenwich pensioners, &c. who could not be better employed, and therefore may be afforded at a price within the reach of every foremast man.

It would be a desirable appendage to the life-boat in cases when the whole crew could not be taken in at once. Any number of persons furnished with these might be floated ashore attached to the boat with small cords. In cases of shipwreck where no life-boats are at hand, doubtless many lives might be saved by the marine spencer; and we may presume that seamen will act with more effect in time of danger when they know they have the certain means on board of getting safe to shore when every exertion is found in vain to save the vessel.

In cases of persons falling overboard, any one not acquainted with swimming, if furnished with a marine spencer, might safely leap after them and keep them from sinking until a boat could be launched.

In cases of fire at sea, or in harbours, the most beneficial effects may be expected from its use; and had the *Queen Charlotte*, which was burnt at sea, been furnished with only one hundred of the marine spencers, many hundreds of lives might have been saved, as in moderate weather one will keep three or four people from drowning.

A corner in every seaman's locker in the royal navy could not be better employed than in containing one of these; and if the men were occasionally exercised in the use of them, so as to render them familiar, the most important consequences might attend it.

Most certainly the life-boat is the best means yet discovered of preserving lives in cases of shipwreck; but as the smaller description of merchant vessels could not afford the expense of one, these marine spencers are certainly the next best means of preserving the lives of the crews in such cases.

L. *List of interesting Curiosities collected by Mr. CLARKE during his Travels in the East.*

To the Editor of the Philosophical Magazine.

SIR,

As various inaccurate accounts have appeared concerning the acquisitions to literature and science which have been

brought home by Mr. Clarke, of Jesus college, Cambridge, from his extensive travels, I take the liberty of transmitting to you a list, though brief, that may however be relied on; and am, sir,

Yours, &c.

From Patmos.

1. The works of Plato, most beautifully written upon vellum, in folio. The scholia in minute capitals. The colyphon proves that it was written by John the calligraph for Arethas, deacon of Patræ, for 13 Byzantine nummi, in the 14th year of the indiction and the 6404th of the world (A. C. 896), in the reign of Leo, son of Basilus.

2. Lexicon of St. Cyril of Alexandria.

3. Greek poetry, accompanied by antient Greek musical notes.

4. Ditto, ditto.

5. The works of Gregory of Nazianzum.

From Naxos.

Copies of the Gospels, in capitals, of very antient date.

From Mount Athos.

1. The orations of Demosthenes.

2. The works of ten Athenian orators, some of which not hitherto known.

From Constantinople.

1. The works of Dionysius the areopagite, with a curious and learned commentary, written on vellum, in folio.

2. Complete copy of the Gospels, written in the eighth century.

3.

4. } Various copies of the Gospels, and of the Epistles
5. } and Acts of the Apostles of different dates.

6.

7. The works of Philip the hermit.

8. The dialogues of Theodore the Syracusan.

9. A work on the Greek Grammar.

10. } The writings of commentators on the Gospels;

11. } and the works of the earliest fathers of the
12. } church.

13. Very antient copy of the Evangelistarium of the Greek church.

14. Ditto ditto.

15. A work of Philes on animals.

The Plato which professor Porson calls a *monument of literature* was, on its arrival, supposed to be the oldest Greek manuscript extant with an express date, as the MS.

six years older, noticed by Montfaucon in his *Paleographia*, p. 42, has disappeared; and that of Euclid, mentioned by Dorville on Chariton, p. 49, 50, was supposed to have been lost. The latter, however, which was written by the same hand as the Plato, the year before it, has been since found, and purchased, with the rest of Dorville's collection, by Dr. Raine and Mr. Banks. Professor Porson, who, in copying the scholia on Plato, has thence discovered passages from Greek plays and from poets that were lost, will, it is hoped, allow the world to profit by the fruits of his industry and unrivalled erudition.

To the above MSS. in the Greek language should be added, in Hebrew, the Bible of the Karaean Jews; in Coptic, the Gospels; in Arabic, many volumes of history, poetry, &c.; in Abyssinian or Æthiopic, the Gospels, &c.; and in Persian, some unpublished works of Sadi and other writers.

The rest of the collections consist of, 1. Antique monuments from Saïs, in Egypt, the ruins of which city were discovered for the first time by Messrs. Clarke and Crips. Various other antiquities from the upper parts of this country.

2. Medals and vases from all parts of Greece.

3. Sculpture and inscriptions from the Cimmerian Bosphorus, the Crimea, the shores of the Euxine, the plain of Troy, the Greek islands, and the Grecian continent.

4. Minerals from all the countries passed through between the 69th and 29th degrees of north latitude, including many new substances.

5. Plants, seeds, &c. from the same regions. These include several new species, and a new genus. Also the herbarium of professor Pallas, which comprises all his botanical discoveries in Siberia; and of Dr. Noezen, of Sweden, abounding in arctic plants.

6. Original maps and charts not yet published. Among these are the great chart of islands and seas between Kamshatka and America, the result of Billings's voyage; the map of the countries between the Black and Caspian seas, on a very large scale; the Crimea; charts of the Russian ports; and a map of the plain of Troy, now engraving by Arrowsmith.

7. Models, implements of husbandry, customs of different nations. Animals, insects, &c.

8. A large collection of drawings from nature, for the purpose of illustrating the account of the journey, if it should ever be published.

LI. *Notices respecting New Books.**Philosophical Transactions of the Royal Society of London for the Year 1803. Part I.*

THIS valuable publication consists of the following papers:—1. The Bakerian Lecture. Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By William Hyde Woollaston, M.D. F.R.S.—2. A Chemical Analysis of some Calamines. By James Smithson, Esq. F.R.S.—3. Experiments on the Quantity of Gases absorbed by Water at different Temperatures and under different Pressures. By Mr. William Henry.—4. Experiments and Observations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold; being the Substance of a Report made to the right honourable the Lords of the Committee of the Privy Council, appointed to take into Consideration the State of the Coins of this Kingdom and the present Establishment and Constitution of His Majesty's Mint. By Charles Hatchett, Esq. F.R.S.—5. Observations on the Chemical Nature of the Humours of the Eye. By Richard Chenevix, Esq. F.R.S. and M.R.I.A.—6. Account of some Stones said to have fallen on the Earth in France; and of a Lump of native Iron said to have fallen in India. By the Right Honourable Charles Greville, F.R.S.—7. Observations on the Structure of the Tongue; illustrated by Cases in which a Portion of that Organ has been removed by Ligature. By Everard Home, Esq. F.R.S.—8. Observations of the Transit of Mercury over the Disc of the Sun; to which is added, an Investigation of the Causes which often prevent the proper Action of Mirrors. By William Herschel, L.L.D. F.R.S.—9. An Account of some Experiments and Observations on the constituent Parts of certain astringent Vegetables, and on their Operation in Tanning. By H. Davy, Esq. Professor of Chemistry in the Royal Institution.—10. Appendix to Mr. Henry's Paper on the Quantity of Gases absorbed by Water at different Temperatures and under different Pressures.—APPENDIX. Meteorological Journal kept at the Apartments of the Royal Society.

An Essay on the Law of Patents for new Inventions: to which are prefixed two Chapters on the general History of Monopolies, and on their Introduction and Progress in England to the Time of the Inter-regnum: with an Appendix, containing Copies of the Caveat, Petition, Oath,
2
and

and other Formulæ; with an arranged Catalogue of all the Patents granted from the 1st of January 1800 to the present Time. By JOHN DYER COLLIER. 1803. Longman and Rees. Royal 8vo.

It is the principal design of our periodical work to give some account of the progress of science and philosophy; and it will not appear to our readers unconnected with this object if we take notice of a publication which is the first on the subject, and the professed intention of which is to enable men of superior attainments to reap the produce of their talents. In the 21st of James the statute of monopolies was passed, by which the exclusive privilege of the sale of any important discovery for fourteen years was assigned to the ingenious inventor. To the comments on this act of parliament, and the cases that have been tried upon it, the essay to which we are now adverting is devoted; and full instructions are given to enable the candidate for the royal grant to obtain his patent, and to protect himself from the evil consequences of its infringement.

The list of patents alluded to in the title page shows how extensively the private studies of the philosopher are at this day rendered subservient to the purposes of commerce and the arts, on which the wealth and prosperity of the country so essentially depend; and this important application of the pursuits of the scientific student is in a great degree to be attributed to the reward thus assigned for his labours by the wisdom of public institution. Yet he is often disappointed of this honourable remuneration from a variety of causes, of which it is extremely material that he should be apprised: we shall therefore extract a few observations from this view of the Law of Patents, which will inspire him with caution, and tend to show how material it is to him now and then to descend from the elevated sphere of science to the lower world we inhabit, and the vulgar affairs in which it is conversant:—

“It is a melancholy reflection, that of the numerous patents which pass the great seal, and, of course, subject the grantee to considerable expense, very few become sufficiently popular and notorious to reward him for the application of his time, labour, and talents. We mention this as a caution to men of enterprise and ability, that the sanguine temperament to which they are commonly subject may not lead them to colour too highly the remote prospect. A profound observer of human nature has told us, that before we arrive at the age of forty we usually make an anticipation

too favourable of the future; and the spirit and energy which supply the inventive talent are peculiarly liable to this species of self-deception.

“ It will be seen in the catalogue, that a great variety of patents have been extended to objects connected with those branches of the trade and manufactures of this country which are of the first importance to commercial prosperity. No less than thirteen patents in a very short period have been assigned for improvements on the steam-engine, most of which would have been probably suppressed if the legal discussion on the patent of Messrs. Boulton and Watt, for an improvement on the same invention, had not completely decided the question on the competency and validity of patents for improvements, whenever those improvements can be deemed material and useful.

“ The mere inspection of the following list will show how necessary it is that patentees should be informed of the rules laid down in the Law of Patents. It will be seen how great a portion of the grants appear by their titles to be extended not to any *piece of mechanism, utensil, or manufacture*, but to a *process, method, or principle*, for which no patent can be valid. It is, however, some consolation to reflect, that what is stated as such is often the application of a method, process, or principle, in some substantial form, for which a patent can be maintained. It is now understood that where this construction can be given it will be applied in the most beneficial sense for the patentee; and that no advantage will be taken of minute verbal criticisms to render the royal grant nugatory, and to disappoint the inventor of his equitable reward.”

Useful Hints to those who are afflicted with Ruptures; on the Nature, Cure, and Consequences of the Disease; and of the empirical Practices of the present Day. By T. SHELDRAKE.

For the present work the public is indebted to the author of a very useful work entitled “ A Practical Essay on the Club-foot and other Distortions of the Legs and Feet of Children,” which has been well received. The present appears to us to be equally useful, and well deserving of the attention of such as are afflicted with ruptures, or have relations in that situation.

LII. *Proceedings of Learned Societies.*

ACADEMY OF SCIENCES AT STOCKHOLM.

IN the sitting of the 13th of April M. Melanderhjelm read an account of the expedition recommended by his Swedish majesty in the beginning of the year 1801, for the purpose of solving in a decisive manner the problem in regard to the figure of the earth, which is of so much importance to astronomy. The king for the accomplishment of this object appropriated the sum of 5000 rix-dollars, and appointed two members of the academy, Messrs. Ofverbom and Svanberg, to perform the requisite operations. These gentlemen immediately proceeded to the neighbourhood of the polar circle to prepare every thing necessary for the operations: such as observatories for the two remotest stations; signals on every convenient point in the extent of twenty-two Swedish miles; and a series of triangles as convenient as the situation of the country would admit. After making these preparations they returned again to Stockholm to receive one of Borda's circles, constructed at Paris by Lenoir under the inspection of Delambre, together with two metres, and a Peruvian toise of iron which the French National Institute presented to the Swedish Academy of Sciences. These articles, however, did not arrive before the beginning of December at Stockholm, where Messrs. Ofverbom and Svanberg made preparations for their new journey. But as the academy foresaw that two persons would not be sufficient to superintend all the individual parts and details of this operation, which required so much accuracy, Dr. Holmquist of Upsal and Mr. Palander of Abo were appointed to unite themselves to Messrs. Ofverbom and Svanberg, and to assist them in their labour. These gentlemen set out together in the beginning of the year 1802; and immediately on their arrival at Tornea prepared the iron rods which were destined for measuring the base line, and made ready for the measurement. The place on the river Tornea used as a base in 1736 was again made choice of for the same purpose. Eight weeks were employed in this operation, from the middle of February to the middle of April. The centigrade thermometer often stood at 25, 26, and even 30° under the freezing point. It was necessary to measure with the greatest accuracy all the angles belonging to the series of triangles—a labour they were obliged to defer till summer, which in that climate is exceed-

ingly short. The number of these angles amounted to about 80; and it was requisite that the measurement of each should be repeated at least thirty times, in order to render as insignificant as possible the errors in the graduation, from which no instrument is entirely free. This was not only done, but many of the angles were measured sixty and even eighty times; and at the end of August the operations were so far advanced, that observations were made of the pole-star, by which the extent of the arc of the meridian measured could be determined. The last southern point was at Malörn, a small island in the archipelago of Tornca, where the gentlemen employed arrived in the beginning of September, and continued their observations on the latitude of this place till the end of October. The observation of the zenith distance of the pole-star was repeated 400 times.

In order to avoid, in determining the absolute latitude of the first and last stations, the uncertainty which might arise from the declination of the pole-star, its zenith distance at its inferior passage of the meridian was observed 88 times. When all these operations were performed, the expedition was considered to be at an end, and the observers returned home. Messrs. Ofverbom and Svanberg, in order to gratify the curiosity of astronomers, who are interested in the result of this new measurement, immediately on their return to Stockholm undertook a calculation; according to which it appears, that in the latitude of $66^{\circ} 20' 11.83''$ the length of a degree of the meridian is 57209 toises, or 196 toises less than that given by the measurement of Maupertuis. This without doubt will be an agreeable novelty to those who are acquainted with the importance of this question, and its relation to the grand phænomena of nature. If this result be compared with that of Bouguer's measurement at the equator, we obtain $\frac{1}{313}$ th for the flattening of the earth at the pole; and the northern degree of the meridian, which hitherto has been so different from all other results deduced from the precession of the equinoxes, experiments with the pendulum, calculations of the parallax compared with the ellipsoid form of Jupiter, no longer forms any exception, but coincides nearly with that which the greatest mathematicians of the last century were obliged to admit. Messrs. Ofverbom and Svanberg, however, do not consider the above result as absolute and definitive, because improvements may still be made in their provisional calculation, which perhaps may amount to 20 toises; and this is the more possible, as one second of variation will occasion a difference of 16 toises.

All

All that we as yet think ourselves authorised to assert is, that the flattening of the earth cannot at any rate be greater than 1-300th.

LIII. *Intelligence, and Miscellaneous Articles.*

AEROSTATION.

Garnerin's 33d Ascent.

ON the 1st of July Garnerin, accompanied by Mrs. Garnerin, ascended in his balloon at Petersburg in presence of their imperial majesties, a great number of persons of distinction, and an immense concourse of spectators. This ascent, being Garnerin's 33d, took place in the garden of the first corps of cadets, where a tent had been erected for their majesties, who repaired thither at six o'clock in the evening. After they had examined the balloon, Garnerin presented to the emperor a small one filled with inflammable air, which his majesty let off himself. Other balloons of a similar kind were then presented to their majesties, and to his royal highness the grand duke Constantine, who let them off in like manner, in order to ascertain the direction of the wind. The state of the atmosphere was exceedingly favourable. At half after seven the aërial travellers entered the car. The signal was given, and the balloon ascended in a majestic manner. At eight o'clock it began to descend, and the aëronauts reached the earth in safety on the little Ohta at some distance from Petersburg.

Mr. Robertson's Balloon.

Mr. Robertson about the middle of July ascended in a balloon at Hamburgh, accompanied by his friend M. Lhoest. The following are the details published by Mr. Robertson himself:

“ On the day on which I determined to make my ascent, I resolved to rise to as great an elevation as my health and strength would permit. M. Lhoest, my friend and companion, having determined to accompany me, we took, besides provisions, as large a quantity of ballast as the car could conveniently contain. After hovering some time over the city of Hamburgh, we resolved to ascend higher; and, having thrown out some ballast, we rose to such a height that the elasticity of the air distended our balloon so much, that we were obliged to open the valve and suffer a certain quantity

quantity of gas to escape. It issued from the balloon with a loud noise. The tension of the balloon being considerably lessened, we threw out more ballast, and ascended till it was almost impossible for us to endure the great cold which we experienced. My teeth chattered; my pulse beat with violence; my veins swelled; and the blood issued from my nose. The state of my friend was different: his head seemed to be swelled, and he could not keep his hat on. We experienced also a great numbness, which inclined us to sleep. Not being able any longer to endure this state, we resolved to descend; which we did slowly, for half an hour, with our watches in our hands. About half after twelve we arrived over Badenburg near Winsen on the Luhe, where we purposed to get out; but the inhabitants, taking us for spectres, fled with the utmost consternation, carrying with them their cattle, and sought shelter in their houses, the doors of which they shut.

My friend having observed to me that this terror might be injurious to us, and induce these people to fire at us, we threw out a part of our remaining ballast, and, having again ascended, continued our voyage till two in the afternoon. We at length descended near Wichtenbeck, on the road to Zell, where commissioners Weyhe and M. Raven were so kind as to give us every assistance in their power, and caused us to be conducted with our balloon to the first post, about five leagues distant from the place of our descent. We arrived at Wichtenbeck in perfect health, after having passed over twenty-five French leagues in five hours.

By some further particulars which Mr. Robertson has since published at Hamburg, it appears that he rose to the height of 2600 toises. "When the balloon rose (says he) the thermometer was at 28 inches. At 11 o'clock the machine, which had not been entirely filled, became so dilated that the inflammable air issued with noise from the lower tube. As this aperture was not sufficient, I was obliged to open the upper valve. It remained open nearly a quarter of an hour, during which time the balloon ascended in a perpendicular direction: at intervals we threw out some ballast. The atmosphere below us was serene, but above us it was somewhat cloudy. Though we approached the sun, the heat decreased as we ascended, and we could look at that luminary without being dazzled. When the barometer was at 14 inches it appeared to become stationary. The thermometer was at $4\frac{1}{2}$ degrees below zero. The cold was not excessive; but the singing in my ears increased, and all our faculties seemed to be palsied by a general indisposition.

Having

Having taken a little wine to recruit my strength, I began some experiments, but they were not satisfactory. I proposed to my companion to ascend higher: he consented, though as much indisposed as myself. We successively threw out ballast. The mercury in the barometer fell to 12.4 inches. At that height the cold out of the car was insupportable, though the thermometer was only one degree below the freezing point. We were obliged to respire faster, and our pulse beat with extreme rapidity. We could scarcely resist the strong inclination to sleep with which we were seized. The blood rushed to our head, and M. Lhoest remarked that it had entered my eyes. My head was so swelled that I could not put on my hat. In this region, where the balloon was invisible from the earth, Mr. Robertson made the following experiments:

1st, Having let a drop of ether fall on a piece of glass, it evaporated in four seconds.

2d, He electrified by friction glass and sealing-wax. These substances gave no signs of electricity which could be communicated to other bodies. The Voltaic pile, which when the balloon was set free from the earth acted with its full force, gave only a tenth part of its electricity.

3d, The dipping needle seemed to have lost its magnetic virtue, and could not be brought to that direction which it had at the surface of the earth.

4th, He struck with a hammer oxygenated muriate of potash. The explosion occasioned a sharp noise, which, though not very strong, was insufferable to the ear. It is also to be observed, that though the aëronauts spoke very loud they could with great difficulty hear each other.

5th, At that height Mr. Robertson was not able to extract any electricity from the atmospheric electrometer and condenser.

6th, In consequence of a suggestion from professor Helmbstadt of Berlin, Mr. Robertson carried with him two birds: the rarefaction of the air killed one of them; the other was not able to fly, it lay extended on its back, but fluttered with its wings.

7th, Water began to boil by means of a moderate degree of heat maintained with quicklime.

8th, According to observations made, it appears that the clouds never rise above 2000 toises, and it was only in ascending and descending through clouds that Mr. Robertson was able to obtain positive electricity.

NATURAL HISTORY.

Captain Baudin, commander of the ships sent on a voyage of discovery round the world, by the French government, having collected at New Holland and the Moluccas a quantity of productions from the three kingdoms of nature, sufficient to load one of the ships, dispatched them to France. They arrived lately at Havre in the *Naturaliste*, commanded by captain Hamelin. One of the professors in the Museum of Natural History at Paris was commissioned to receive them, and to take the speediest measures for conveying them to the place of their destination.

This collection consists of more than 140 boxes or half barrels, containing minerals, vegetables, or animals.

The minerals are contained in 14 boxes.

The vegetables, consisting of plants dried and prepared for the herbal, occupy 12: besides these there are 3 casks filled with specimens of different kinds of wood; 2 boxes with seeds, and more than 60 half casks of living plants.

The dead animals, or the remains of them, such as madrepores, shells, insects, preserved birds, and skins of quadrupeds, fill 35 boxes.

The living animals consist of 19 individuals, and are contained in 9 cages.

All these articles, planks of wood proper for cabinet-makers, and some utensils of the Indians, were conveyed from the *Naturaliste* into two small brigs of the state destined to carry them to Paris, and to land them at the gate of the Museum of Natural History.

This cargo is one of the most valuable of the kind ever brought to Europe; but unfortunately the length of the voyage, the want of means, and the multitude of rats with which the vessel was infested, and ignorance in regard to the manner of treating live beings, caused a great many of them to perish on the passage from the South Sea to France. Many of the animals died, and several of those alive are in a languishing condition. A small number only are in good health, such as the Indian hinds and the black swans, male and female. Fortunately they are the most valuable of those embarked.

In regard to the living vegetables, they have suffered still more: of a hundred individuals sent from Port Jackson only thirty give any signs of life, 12 or 15 vegetate feebly, and only half a dozen at most are in a good state. Among the latter there are several kinds of the flax of New Zealand,

land, the most valuable plant of the whole cargo for the purposes of rural and domestic œconomy.

We are assured that captain Baudin will continue his researches around New Holland; that he is employed in collecting geographical observations and natural productions; and that he will return in the month of July next year with a more considerable cargo. As the different parts of this new collection, during the voyage, will be under the care of those who collected them, it is probable they will arrive in a good state, and that they will enable naturalists to become better acquainted with the productions of this fifth part of the world.

RUSSIAN VOYAGE OF DISCOVERY.

The two vessels destined for the new Russian voyage of discovery are the *Nadeschda* (the *Hope*), commanded by captain-lieutenant Krusenstern, the chief of the expedition; and the *Newa* commanded by captain-lieutenant Lisjansky. Both these gentlemen are naval officers of distinguished talents, and animated with the utmost zeal for promoting the objects of the undertaking. Dr. Redowsky of Moscow has been appointed physician to the expedition, with a salary of 2000 roubles, and 800 roubles for his table. As these vessels are destined also to carry an embassy to Japan, this opportunity will be embraced to send back to their native country some Japanese separated from it by an unfortunate accident. In the year 1793 the governor of the province of Sendey, in Japan, dispatched a transport laden with provisions for the maintenance of the troops in the capital. In consequence of tempestuous weather this vessel was driven from its course, and, after wandering about for four months, was at length wrecked on the island of *Nazky*, which belongs to the Russian American company. The governor received the Japanese who escaped, and who amounted to sixteen persons, with great friendship, and sent them to *Ochozk*. They were conveyed thence to *Irkuzk*, where they arrived in the month of September 1794, and afterwards were removed to *Petersburgh*. At present they amount to twelve, as four of them died and two remained voluntarily at *Irkuzk*. They have learned to speak the Russian language with tolerable facility, and four of them have embraced the christian religion. The emperor has given them their choice, either to return to their own country or to remain in Russia. Only three have resolved to return to Japan, and one even of these will accompany the embassy back to Russia. Among the presents destined for

for the emperor of Japan is an ingenious piece of workmanship carried from England to Russia by a German named Schuri, and sold to the empress Catharine II. for 15000 roubles. It represents a peacock of the natural size, which spreads out and folds together its magnificent feathers in a manner which exactly imitates nature. It is accompanied by a great number of birds and small animals, which all move with the greatest ease, and emit the sounds peculiar to them. This beautiful automaton was placed formerly in the Tauride palace, and under Paul I. was removed to the collection of rarities in the Hermitage.

VACCINE INSTITUTION.

The following new Edition of Directions for the Vaccine Inoculation has been lately issued by this Institution:

I. The limpid matter should be taken from a decidedly characterized cow-pock, which is proceeding, apparently, through its respective stages. It is most efficacious in producing the vaccina from a pock before the eleventh or twelfth day; and is most abundant, and is usually taken, about the ninth day. But it may be used at an earlier period, even as early as the fifth day, if it can be collected. However, matter from a pock later than the eleventh or twelfth days is not more liable to produce inflamed arms than that from younger pocks; and if the cow-pock be excited at all, it is as distinct as from any earlier matter. No differences in the effects of the vaccine matter inoculated appear to depend on the presence, extent, or absence of the red areola.

II. The matter is usually taken on glass, thread, or a quill, on which it should be suffered to become dry without applying heat, and when so dried it is scarcely visible. The air should be excluded by keeping the matter between two glass plates, or in a bottle filled with hydrogen gas.

III. As dried matter fails much more frequently to excite the vaccina than recent fluid matter, it will be advisable, in order to insure the effect, or for obtaining a great quantity of matter, that, instead of a single puncture or scratch (which is sufficient and preferable with recent matter), there be matter inserted in two punctured or scratched parts in each arm. The dried matter at the time of inoculation should be softened by warm but not very hot water.

IV. The inoculation must be performed in the same manner as for the small-pox.

V. If the infectious matter produce the required effect in
three,

three, four, or five days, there will be seen a red spot like a small gnat-bite; in six or seven days, a small vesicle will appear; in nine days, a circular vesicle (improperly called a pustule) will be found as large as a pea, or from about two-tenths to four-tenths of an inch in diameter, usually surrounded by a red areola. By the eleventh day the vesicle begins to scab or grow dry, and turn black in the middle, and the areola becomes more extensive. By the fifteenth day, but often later, the pock becomes a mere scab, circular, prominent, well defined, of a blackish or mahogany colour, adhering firmly; but the areola disappears. Unless it be separated by violence the scab does not fall off, in general, sooner than the twentieth day. It then leaves a cicatrix permanent for life.

VI. If the eruption or pimple excited by inoculation has not the characters and does not pass through the stages in the course above stated (V), although sometimes anomalous, this cow-pock may render the constitution unsusceptible of the small-pox, yet it cannot be depended upon. In such cases, the inoculation should be re-instituted; for, if the vaccina cannot be again excited, the unsusceptibility desired will have been produced: but if a further proof be wanted, recourse must be had to inoculation with the variolous matter.

VII. In many cases, no constitutional affection or fever can be perceived: when it occurs, it is almost always on the ninth and tenth days; but provided the pock exhibit the distinctive characters of the cow-pock, even without areola, with the usual course of its stages, the susceptibility of the small-pox will be as effectually destroyed as if there had been considerable febrile affection, and extensive areola.

VIII. If erythema, like erysipelas, extend over the arm, with swelling, pain, &c., it has always subsided in a few days of itself, only avoiding irritating applications, or at most on using sedatives.

IX. Eruptions sometimes occur, but they require no particular treatment.

X. The small-pox may break out at any period within twelve days of inoculation for the cow-pock. If they appear earlier than the sixth or seventh, the vaccina is cut off in its progress; if they appear later, the vaccina goes forward in its usual course.

XI. The medical treatment which may be required from unusual or supervening complaints, being analogous to that in the small-pox, must be accordingly.

XII. Mea-

XII. Measles, chicken-pox, hooping-cough, and other disorders, may intervene during the vaccina, without, in general, varying its progress*.

FULMINATING SILVER.

In our last we gave (from M. Van Mons's Journal) a letter from Brugnatelli, announcing *as new* a preparation of a fulminating silver. Having since compared it with a process described by the honourable Mr. Howard at the end of his interesting paper on Fulminating Mercury, published in the Philosophical Transactions, (also in this work, see vol. vii. p. 126,) we find it to be exactly the same.

ASTRONOMY.

The weather was very favourable for observing the solar eclipse on the morning of the 17th; but owing to an error in the Nautical Ephemeris, many people were disappointed of an opportunity of seeing the commencement of the phenomenon.

Geocentric motion of Pallas for September 1803.

	A. R. of Pallas.	Declin. North.
Sept. 3	17 ^h 56 ^m 47 ^s	13° 23'
6	17 57 29	12 48
9	17 58 22	12 13
12	17 59 24	11 40
15	18 0 34	11 6
18	18 1 55	10 32
21	18 3 26	10 1
24	18 5 6	9 27
27	18 6 52	8 56
30	18 8 49	8 25

* It has been found proper to require half-a-guinea for arming three lancets, or for matter on thread or glass. but each practitioner may be supplied with matter as often as wanted for his own use only, by paying one guinea annually, the expense of postage and portage being discharged by those who apply.

The Institution does not warrant any matter but that which has on the package the impression of the seal of the Institution, namely, a cow, with the motto *Feliciores inserit.*

LIV. *On the Stones said to have fallen at Ensisheim, in the Neighbourhood of Agen, and at other Places. From the Memoir of M. DE DREE*.*

BEING desirous to collect a certain number of these extraordinary facts, that I might compare them with each other, I sought for and obtained some correct ideas respecting two masses of this kind; one of which fell near Ensisheim, and the other in the environs of Agen. These stones, indeed, have been already mentioned; but as the latter was neither analysed nor described mineralogically—as the circumstances of the fall of the former are imperfectly known—and as, at the period at which Mr. Barthold professor of the central school of the Upper Rhine described and analysed it, he could not search for certain distinguishing characters of this kind of stones, which were not then known—I thought it might be of importance to give an account of them here, in order to remove all uncertainty respecting the nature of these bodies.

1. *Stone of Ensisheim.*

The stone known under this name, which fell at Ensisheim in Alsace, made a great noise, especially about the end of the fifteenth century, and is mentioned in several works. Mr. Butenschoen, professor of history in the central school of Colmar, gave some account of it, which was inserted in the *Decade Philosophique*; and to the kindness of that gentleman I am indebted for the following extracts from the chronicles of the time.

Literal Translation of a German Notice respecting the Stone of Ensisheim, which was formerly preserved along with that Stone in the Parish Church of the Place.

“On Wednesday, Nov. 7. the night before St. Martin’s day, in the year of our Lord 1492, a singular miracle happened: for between the hours of eleven and twelve a loud clap of thunder took place, with a long-continued noise, which was heard at a great distance; and a stone fell from the heavens in the Ban of Ensisheim which weighed 260 pounds; and the noise was much louder in other places than here. A child then saw it strike on a field situated on the upper Ban, towards the Rhine, and the In, near the

* See the last Number of the Phil. Mag.

canton of Gisgane, which was sown with wheat. It did no hurt, except that it made a hole there. It was afterwards transported thence; and a great many fragments were detached from it, which the land-vogt forbade. It was then deposited in the church, with intention of suspending it as a miracle; and a great many people came hither to see this stone, respecting which there were singular discourses. But the learned said they did not know what it was, for it was something supernatural that so large a stone should fall from the atmosphere; but that it was a miracle of God: because, before that time, nothing of the kind had ever been heard of, seen, or described. When this stone was found, it had entered the earth to a depth equal to the height of a man. What every body asserted was, that it had been the will of God that it should be found. And the noise of it was heard at Lucerne, at Villing, and many other places, so loud, that it was thought the houses were all overturned. And when king Maximilian was here, the Monday after St. Catharine's day of the same year, his royal excellency caused the stone which had fallen to be carried to the castle; and after conversing a long time with his lords, he said the people of Ensisheim should take it: and he gave orders that it should be suspended in the church, and that no person should be permitted to take any part of it. His excellency, however, took two fragments; one of which he kept, and the other he sent to duke Sigismund of Austria. The people talked a great deal of this stone, which was suspended in the choir, where it still is, and many came to see it."—*Arithenicus in Chronico Hirsaugiensi, in Vita Blavii Abbatis xi. ad Annum 1492. Edit. M.S. Galli, 1690, Vol. II. p. 551.*

"The same year (1492), on the 7th day of November, a stone, called a thunder-stone, of a prodigious size (for we have it from eye-witnesses that it weighed 255 pounds), fell from the heavens in the village of Suntgaw, near the town of Ensisheim, not far from Bale, a city of Germany. Its fall was so violent that it broke into two pieces. The largest is still to be seen at the door of the church of Ensisheim, suspended by an iron chain, as a proof of the truth of the fact which we announce, and to preserve the remembrance of it."—*Paulus Lang, in Chronico Cirizense, in Vol. III. Scriptor. Rer. Germ. Histor. p. 1264.*

"On the 7th of the ides of November, in the year of our Lord 1492, there arose a storm, during which the heavens appeared to be on fire. While the thunder roared, a stone of a prodigious size fell from the heavens, with a horrible crash, near the town of Ensisheim, on the lands belonging

to the emperor Frederick. Its form was that of a delta, with a triangular point. It is still shown at Ensisheim as a wonderful phænomenon."—*Joh. Linturii Appendix ad Fasciculum Temporum Wernerii Rollewinck. in Vol. I. Scriptor. Rer. Germ. Histor.* p. 580.

The same year (1492), after the festival of St. Martin, a stone weighing 300 pounds and more, hard, and of different colours, fell in Alsace, with a great noise, from a brilliant and flaming cloud. The rest of the horizon exhibited no clouds. At the same moment, the heavens being still serene, a large red cross was observed around the moon:

In a rescript of king Maximilian, dated Augsbourg, Nov. 12, 1503, that sovereign mentions this stone, which he says fell near him while at the head of his army; and he gives it as a presage of the victory which he had gained over the crown of France*.

Brant also has made this stone the subject of some poems †.

One of the fragments of this stone was suspended and preserved in the church of Ensisheim till a few years back, when it was conveyed to the library of Colmar, and there deposited. It weighs about 150 pounds at least, notwithstanding the specimens which have been detached from it; and professor Butenschoen says that this stone is beyond all doubt that spoken of in the before-mentioned chronicles.

M. Felix Desportes, præfect of the department of the Upper Rhine, having permitted several fragments to be detached from this mass, in order to be sent to me, I am indebted to that zealous protector of the sciences, and to professor Butenschoen, for the advantage of being able to give a comparative description of its characters, and for an analysis of it, which Vauquelin undertook to make.

“The different specimens of this stone which I received did not exhibit that continued black vitrified crust observed on the stone of Sales, and other stones of the same kind; but I discovered this crust in the cavities which had been sheltered from the shock and from friction; and these remains are sufficient evidences to attest that this crust had existed. It exhibits the puffed-up appearance of vitrification: the colour is only rather brown than black; which arises either from the effect of time, or from the greater quantity

* Rescriptum Maximiliani regis de cruciata, &c. die 12 Nov. 1503. in vol. Rer. Germ. nono, de pace publica, autore J. P. Datt. Ulm, 1698, p. 214.

† De fulgetro immani jam nuper, anno 1492, prope Basileam, &c. In variis Sebastiani Brant carminibus. Basileæ, 1498, 4to.

of iron diffused throughout the mass, as will be seen hereafter.

As this stone has a great resemblance to that of Sales, I shall for the sake of brevity give only a comparative description of them.

In this stone, as well as in that of Sales, there are found malleable iron, containing nickel disseminated throughout it in grains; lamellated and whitish sulphuret of iron in the form of lumps and grains; gray sulphuret of iron less sulphurated, in thin scaly strata, which line a multitude of small fissures which traverse the stone in every direction; but it is observed that the white pyrites in it form larger lumps, and that the gray pyrites is more abundant than in that of Sales.

Small amygdaloid globules might perhaps be seen in it; but as they have a more metallic fracture than those of the stone of Sales, as they are almost confounded in regard to appearance with the mass, they do not deserve particular attention. Its interior colour is darker and bluer than that of Sales, and its fracture is somewhat changeable in its colour, in consequence of the metallic splendour of small fissures, which gives it a different appearance by the magnifying glass; and on the transverse branches of these fissures it is observed, that this texture and the constituent elements are the same in both, except in regard to the grain, which is finer, and the tissue, which is more compact, than in that of Ensisheim.

On carefully examining the differences which I observed between this stone and that of Sales, and which distinguish it also from the stones of Agen and of Benares, and from others of the same kind about to be mentioned, it will be seen that they do not relate to the constituent elements, but only to their proportions; it is therefore impossible not to discover that this mineral mass is of the same nature as that of Sales and others of the like kind. This is confirmed by the following note, communicated to me by Vauquelin:

“This stone has a perfect resemblance in the number, nature, and quantity of its constituent principles, to all the stones said to have fallen from the clouds which have hitherto been subjected to chemical analysis.”

“It is certainly composed of siliceous earth, iron, nickel, sulphur, and a small quantity of lime.”

“I have ascertained, by particular trials, the presence of sulphur and nickel in the grains of malleable iron, and in the pyrites, but in different proportions. This stone there-

fore resembles, in every point, all those which have fallen from the atmosphere.”

2. Stones of Agen.

The stones known under the name of *the stones of Agen* have been mentioned in the *Bibliothèque Britannique* *. Their fall has been confirmed by numerous testimonies and authentic documents, inserted by M. Bertholon in the *Journal des Sciences utiles* †.

According to the above accounts, these stones fell on the 24th of July 1790, between nine and ten in the evening, in the communes of Juliac, Creon, and others adjacent, between Roquefort in the department of Landes, Mezin in the department of Lot and Garonne, and Eause in the department of Gerz, after the apparition of a large fire-ball which passed through the air, accompanied with a loud report.

They fell at different distances; some gently, and others with rapidity and a hissing noise.

They buried themselves more or less in the earth: several had fissures in them.

Their weight in general was between a quarter of a pound and two pounds: some of them, however, are said to have weighed from twenty to twenty-five pounds; and Mr. St. Amand saw in the museum of Bourdeaux one of these fragments about fifteen inches in length, taken from a stone which, as said in the accompanying note, crushed a cottage, made a conical hole of about five feet in depth, and killed a farmer and some cattle, at the time of the explosion of July 24, 1790, near Roquefort in Landès.

There is no stone in the place where they fell which has any resemblance to them.

Mr. Darceet was so kind as to transmit to me a fragment of one of these stones, which fell near Barbotan, a place situated within the boundaries above traced out. It was that subjected to analysis by Vauquelin.

This stone has so great a resemblance to that of Sales, even in the most minute particulars, that the mineralogical description would be the same, and therefore I refer to the latter. It has the same vitrified crust; the same substances included, and nearly in the same proportions; the same texture, hardness, and aspect; the same chemical characters.

The analysis made of it by Vauquelin gave him the same chemical substances as the other stones, and in proportions nearly the same.

* No. 154. p. 85.

† No. 23. and 24. 1790.

3. *Notice respecting a Mass of Iron which fell in the Mogul Territories. Communicated by Mr. GREVILLE to the Royal Society of London.*

All the masses of iron of unknown origin, such as those found in Siberia, America, &c. contain nickel, and have besides characters analogous to those of the stones which have fallen from the clouds. These circumstances induced Mr. Howard and Count de Bournon to conclude that these substances might have had the same origin. The following fact tends to confirm this opinion. Mr. Greville, who communicated it to the Royal Society, extracted it verbatim from the Memoirs of Jehangire emperor of the Moguls, written in Persian by himself, and translated by colonel William Kirkpatrick.

“*A. H. 1030, or the 16th of the reign.* The following is among the extraordinary occurrences of the period.

“Early on the 30th of Furverdeen of the present year*, and in the eastern quarter (of the heavens), there arose in one of the villages of the purgunnah † of Jalindher such a great and tremendous noise as had nearly, by its dreadful nature, deprived the inhabitants of the place of their senses. During this noise a luminous body (was observed) to fall from above on the earth, suggesting to the beholders the idea that the firmament was raining fire. In a short time, the noise having subsided, and the inhabitants having recovered from their alarm, a courier was dispatched (by them) to Mahomed Syeed, the aumil ‡ of the aforesaid purgunnah, to advertise him of this event. The aumil instantly mounting (his horse) proceeded to the spot (where the luminous body had fallen). Here he perceived the earth, to the extent of ten or twelve guz § in length and breadth, to be burnt to such a degree, that not the least trace of verdure, or a blade of grass, remained; nor had the heat (which had been communicated to it) yet subsided entirely.

“Mahomed Syeed hereupon directed the aforesaid space of ground to be dug up; when the deeper it was dug the greater was the heat found to be. At length a lump of iron made its

* The first of Furverdeen of this year (A.H. 1030) corresponded with Saturday the 27th of Rubbi ul Akhir; consequently the 20th of Furverdeen fell on the 26th of Jummad ul Owul, or A. D. 1620.

† A purgunnah is a territorial division of arbitrary extent. The purgunnah of Jalindher is situated in the Punjaub, and about 100 miles south-east of Lahor.

‡ Aumil is a manager or fiscal superintendent of a district.

§ A guz is rather less than a yard.

appearance, the heat of which was so violent that one might have supposed it to have been a furnace. After some time it became cold; when the aumil conveyed it to his own habitation, from whence he afterwards dispatched it, in a sealed bag, to court.

“ Here I had (this substance) weighed in my presence. Its weight was 160 tolahs*. I committed it to a skilful artisan, with orders to make of it a sabre, a knife, and a dagger. The workman (soon) reported that the substance was not *malleable*, but *shivered into pieces under the hammer* †.

“ Upon this I ordered it to be mixed with other iron. Conformably to my orders three parts of the *iron of lightning* ‡ were mixed with one part of common iron; and from the mixture were made two sabres, one knife, and one dagger.

“ By the addition of the common iron, the (new) substance acquired a fine temper; the blade (fabricated from it) proving as elastic as the most genuine blade of Ulmanny § and of the south, and bending like them without leaving any mark of the bend. I had them tried in my presence, and found them cut excellently; as well (indeed) as the best genuine sabres. One of these sabres I named *katai*, or *the cutter*; and the other, *burk-serisht*, or *the lightning natured*.

“ A poet || composed and presented to me, on this occasion, the following tetra-stich;

“ This earth has attained order and regularity through the emperor Jehangire.

“ In his time fell *raw* iron from lightning:

“ That iron was by his word-subduing authority converted into a dagger, a knife, and two sabres.”

“ The chronogram of this occurrence is contained in words which signify ‘the flame of the imperial lightning,’ and give the year (of the Hegira) 1030.

“ N. B. The foregoing translation (which is nearly literal) has been made from a manuscript that has been several years in my possession; and which, although without a date, bears marks of having been written at a remote period.

“ WILLIAM KIRKPATRICK.”

* A tolah is about 180 grains Troy weight.

† Literally, “it did not stand beneath the hammer, but fell to pieces.”

‡ This expression is equivalent to our term *thunder-bolt*.

§ The name of the place here designed is doubtful.

|| The poet is named in the original, but the name is not perfectly legible.

4. Notice respecting the Stones which fell lately in France.

In the sitting of the Institute on the 9th of May, C. Fourcroy read a letter addressed to C. Vauquelin from the town of Aigle, in the department of Calvados (the ci-devant Normandy), containing a circumstantial account of the recent fall of a considerable number of stones. The following details are extracted from it :

On April 26th, about one in the afternoon, the sky being almost serene, there was heard a rolling noise like that of thunder. It seemed to proceed from one cloud which was on the horizon, and which the inhabitants beheld with uneasiness ; when, to their great surprise and terror, explosions like the reports of a cannon, sometimes single and sometimes double, were heard, with a violent hissing : phænomena which struck a terror even into domestic animals, for the cows bellowed, and the poultry fled to a place of shelter. This noise was succeeded by the fall of a great number of stones of different sizes, weighing ten, eleven, and even seventeen pounds. The largest entered the earth to the depth of a foot. Several of them fell in the court-yard of M. Bois-de-la-ville, and one of them very near him. Many curious persons collected some of them, and C. Fourcroy laid before the Institute one of these fragments, which, when compared with the fragment of a stone that fell near Ville-Franche, presented to the Institute in the same sitting by C. Pictet, had a great resemblance to it in every point : the same colour, the same texture, the same black crust ; in a word, the fragments could not be distinguished from each other but by the size.

C. la Marck then reported that he had received from the department of Calvados several letters, making mention of a globe of fire which had been seen to pass, proceeding in a direction from west to east, with great velocity, on the same day and at the same hour at which the event alluded to took place. It was added, that this meteor had been seen at sea before it reached the continent.

If any doubts remain to our readers on the certainty of the real fall of those foreign bodies of which we have frequently given an account, we request them to peruse a work which has lately appeared under the title of *Lithologie Atmosphérique*. M. Izarn, the author, who is professor of philosophy, gives in this work a complete treatise on phænomena of this kind. It is divided into three sections. The first contains

tains a collection of the facts and opinions published in France since the year 1700 on *thunder-stones, thunder, stones fallen from the heavens, &c.* In the second is found a critical examination of the systems hitherto formed on this subject, both in regard to the reality of the fall of stones from the atmosphere, and on their origin and formation. It results from it that the phænomenon of the fall of solid bodies on the earth is, according to every appearance, as old as the world; and that the certainty of the fact is now so well proved, that it can be denied only by those who admit nothing as certain. The third section contains an essay towards a theory on the formation of stony and metallic bodies in the atmosphere. At the end, the author gives a sort of recapitulation of his whole work in the following Tables:—1st, Of the principal opinions entertained in regard to the solid substances which have fallen from the clouds. 2dly, Of the different periods of the fall of these substances on the earth.

Table of the principal Opinions entertained in regard to the solid Substances which have fallen from the Clouds.

Philosophers who have considered them as productions thrown on the earth by volcanoes or hurricanes:

Freret,	Barthold,
Gassendi,	G. A. Deluc,
Muschembroek,	Delalande.

As mineral substances fused by lightning on the spots where found:

Lemery,	Stahl,
The Academicians,	Gronberg,
Agricola,	Patrin.

As concretions in the atmosphere:

Descartes,	Sir William Hamilton,
Lesser,	Edward King,
Goyons-d'Arzas,	Eusebius Salverte.

As masses foreign to our planet:

Chladni,	Poisson,
Biot,	The Bibliothèque Britannique.

<i>Substances.</i>	<i>Places where they fell.</i>	<i>Period of their Fall.</i>	<i>Testimonies.</i>
Shower of stones	At Rome	Under Tullus Hostilius	Livy
Shower of stones	At Rome	Consuls C. Martius & M. Torquatus	J. Obssequens
Shower of iron	In Lucania	Year before the defeat of Crassus	Pliny
Shower of mercury	In Italy	-	Dion
A very large stone	Near the river Negro, Thrace	Second year of the 78th Olympiad	Pliny
Three large stones	In Thrace	Year before J. C. 452	Ch. of count Marcellin.
Shower of fire	At Quesnoy	Jan. 4th, 1717	Geoffroy le Cadet
Stone of 72 lbs.	Near Larissa, Macedonia	Jan. 1706	Paul Lucas
About 1200 stones—-one of 120 lbs.	Near Padua in Italy	In 1510	Cardan, Varcit
Another of 60 lbs.	-	-	Gassendi
A stone of 59 lbs.	On Mount Vaiser, Provence	Nov. 27th, 1627	Père la Feuillée
Shower of sand for 15 hours	In the Atlantic	April 6th, 1719	Moses
Shower of sulphur	Sodom and Gomorra	-	Sparagenberg,
Sulphureous rain	In the duchy of Mansfeld	In 1658	Olaus Wurminus
The same	Copenhagen	In 1646	Siegesber
Shower of sulphur	Brunswick	Oct. 1721	Muschembroek
Ditto of a viscid unknown matter	Ireland	In 1695	Delalande
Two large stones weighing 20 lbs.	Liponas in Bresse	Sept. 1753	Bachelay
A stony mass	Niort, Normandy	In 1750	Gurson de Boyaval
A stone of 7½ lbs.	At Luce in Le Maine	Sept. 13th, 1763	Morand
A stone	At Aire in Artois	In 1768	St. Amand, Baudin, &c.
A stone	In Le Cotentin	In 1768	Earl of Bristol
Extensive shower of stones	Environs of Agen	July 24th, 1790	Capt. Topham
About 12 stones	Stenna, Tuscany	July 1794	Leleivre and De Drée
A large stone of 56 lbs.	Wald-Cottage, Yorkshire	Dec. 13th, 1795	F. Lloyd Williams, esq.
A stone of about 20 lbs.	Salé, department of the Rhone	March 17th, 1798	B. de Born
A stone of 10 lbs.	In Portugal	Feb. 19th, 1796	Philosophical Magazine
Shower of stones	Benares, East Indies	Dec. 19th, 1798	Pallas, Chladui, &c.
Shower of stones	At Plann, near Tabor, Bohemia	July 3d, 1753	Darcet jun., Lomet, &c.
Mass of iron 70 cubic feet	America	April 5th, 1800	Burenshoen
Mass of ditto, 14 quintals	Abakank, Siberia	Very old	Acad. de Bourd.
Shower of stones	Barhoutan, near Roquefort	July 1789	De Drée
Large stone, 260 lbs.	Einsheim, Upper Rhine	Nov. 7th, 1492	Fourcroy
Two stones, 200 and 300 lbs.	Near Verona	In 1762	-
A stone of 20 lbs.	Salcs, near Ville-Franche	March 12th, 1798	-
Several ditto from 10 to 17 lbs.	Near L'Aigle, Normandy	April 26th, 1803	-

LV. *Memoir on the Stones which have fallen from the Atmosphere, and particularly near Laigle, in the Department of l'Orne, on the 26th of April last. Read by C. FOURCROY, in the public Sitting of the Class of the Mathematical and Philosophical Sciences of the Institute, June 19th, 1803.*

NATURE sometimes exhibits to us facts insulated, as we may say, and so different from any thing with which we are acquainted, that their existence is long problematical even to men who are most accustomed to observe its wonderful works, and to calculate its powers. It was in this manner that naturalists and philosophers for a long time classed among fables and popular errors the fall of solid and stony bodies on our globe.

Exact accounts, however, which have been multiplied for six or eight years past; the coincidence of meteoric circumstances which in all these accounts accompany the principal phænomenon; the analogy of the form, structure, and colour, observed in several of these stones, which have fallen at different times and in different places very distant from each other; and the difficulty of referring these stones to any of the species with which we are acquainted, induced Mr. Howard, an English chemist, to analyse these productions hitherto so little known.

By chemical examination he not only found that they were all composed of the same principles, but that there was a striking difference between them and all the other mineral substances hitherto analysed. He found that they contained in general from a fourth to two-thirds of their weight of silice, a third of iron, a sixth or seventh of magnesia, and some hundredth parts of sulphur and nickel. He found also that the general mass of these stones contains inclosed in it globules of iron alloyed with nickel and a little sulphur, and fragments of pyrites composed of sulphurated iron and nickel.

C. Vauquelin obtained the same results from three of the same stones analysed by Mr. Howard, and from two others that fell in France, one at Barbotan in 1789, and the other at Creon, in the parish of Juliac, on July 24, 1790.

The attention of philosophers was much excited by the novelty of these results, while the ability of the chemists who presented them commanded the utmost confidence. Hence, instead of rejecting the existence of the phænomenon,

non, as had before been done, the greatest philosophers were desirous that it should be carefully studied, fully confirmed, and accurately described. With this view C. Izarn composed his *Lithologie Atmosphérique*, which was soon presented to the National Institute. In this interesting work, the first ever written on the subject, are found a multitude of similar facts having all the characters of authenticity; and all the opinions hitherto expressed, both in regard to the existence and causes of the phenomenon, are clearly detailed and discussed.

At the time when we were most occupied with this new problem of natural philosophy, and while, uncertain in regard to its existence, we were discussing the authenticity of the accounts given of it by the ancients and moderns, the inhabitants of Laigle and of a vast extent of surrounding district were witnesses of the phenomenon: it appeared over their heads on the 26th of April, with circumstances capable of striking with terror and astonishment.

SECTION I.

Description and Analysis of the Stones which fell at Laigle, in the Department of l'Orne, on the 26th of April 1803.

From all the letters I received, and which I successively communicated to the Institute, of which I may mention as the most authentic those of our fellow-member Leblond, who has resided in Laigle for several years, it results,

1st, That about one in the afternoon, on the 26th of April, the air being rather cold than warm, and the sky without clouds, there was seen, at the distance of twelve or fifteen leagues west-south-west from Laigle, a luminous globe moving towards the north-west with great velocity.

2d, That nearly at the same hour there was heard at Laigle and in several of the surrounding villages a violent explosion, succeeded by two others no less extraordinary, which were followed by a rumbling noise, the more terrible as no one knew to what it could be compared or ascribed, and which continued about ten minutes.

3d, That after this noise, by which the animals were as much frightened as the inhabitants, there were seen to fall, with a hissing noise, stones very much scattered, and of different sizes, from 2 or 3 gros to 17 pounds in weight; that these stones at first exhaled a strong smell of sulphur, which was gradually dissipated; that those who picked up some

of them at the time found them very warm; and that to judge by the number collected, and by the extent of the ground on which they were found, an astonishing quantity must have fallen.

These stones in general are irregular, polygonal, often cuboid, sometimes sub-cuneiform, and exceedingly various in their diameters and weight; they are all covered with a black gravelly crust consisting of a fused matter, and filled with small agglutinated grains of iron. The greater part of them are broken at the corners, either by their shock against each other, or by falling on hard bodies. The interior parts resemble those of all the stones analysed by Messrs. Howard and Vauquelin; they are gray, a little varied in their shades, granulated, and as it were scaly, split in many points, and filled with brilliant metallic points exactly of the same aspect as those of other stones of the like kind.

In conjunction with C. Vauquelin I analysed them in the following manner, which has been already employed on similar occasions: The stone being reduced to fine powder, we poured over it muriatic acid somewhat weakened. A pretty strong effervescence was produced; an odour of sulphurated hydrogen gas was disengaged, and the liquor assumed a very evident green colour: the gas collected was not entirely sulphurated. Muriatic acid was twice in succession poured over it to deprive of its colour the insoluble part, which after being well washed was found to be pure silex, forming more than half of the whole weight of the stone. The muriatic solution with excess of acid was treated with ammonia, which precipitated from it the oxidated iron, and retained the magnesia and the nickel. The iron was completely separated by boiling the liquor, and nearly 36 per cent. of that metal, weakly oxidated, was obtained. The liquor, containing a triple muriate of ammonia, nickel, and magnesia, was mixed with a solution of potash to precipitate the magnesia, which carried with it a small portion of nickel. Nearly 9 per cent. of magnesian earth was obtained. The water charged with sulphurated hydrogen was afterwards employed to separate the oxide of nickel, of which we found about 3 per cent.

I shall omit saying any thing here of some difficulties which occurred in the details of this analysis: as I reserve these for a particular memoir, I shall content myself with announcing the result of the analysis. It gave us as the constituent materials of the stone of Laigle the following proportions nearly:

Silex

Silex	-	-	-	54
Oxidated iron	-	-	-	36
Magnesia	-	-	-	9
Nickel	-	-	-	3
Sulphur	-	-	-	2
Lime	-	-	-	1
				<hr/>
				105
				<hr/>

The 5 per cent. of increase arose from the oxidation of the metals produced by the analysis.

SECTION II.

Analysis of the Stone of Ensisheim.

The stone which fell at Ensisheim about the end of the 15th century has given rise to many accounts more or less fabulous. Almost all contemporary authors speak of it. M. Butenschoen, professor of history in the central school of Colmar, has communicated to me several interesting extracts from them: but I shall give only the principal facts of this interesting history.

We read in a manuscript chronicle, written in German, that between the hours of eleven and twelve in the forenoon, on the 7th of November 1492, there was heard in the environs of Ensisheim a terrible clap of thunder, and that a child saw fall in a field sown with wheat an enormous stone, which entered the earth to the depth of about three feet: it weighed at that time 260 pounds. Maximilian, king of the Romans, after causing some fragments to be detached from it, gave orders that it should be suspended in the parish church of Ensisheim. Since the revolution it has been transported to Colmar, and placed in the library: at present it weighs only 171 pounds.

M. Barthold, professor of chemistry in the central school of the Upper Rhine, gave in the year 8 an analysis of this stone. Besides silex, iron, sulphur, and magnesia, he announced 0.17 of alumine, and considers it as a secondary argillo-ferruginous stone, arising from the decomposition of primitive rocks, and detached from a neighbouring mountain.

The method of analysis which the professor followed did not allow him to distinguish very accurately the earths which enter into the composition of this production. He admits also alumine, which we did not find in any of our experiments;

experiments; while, on the other hand, he observed no nickel; which, indeed, it was impossible to discover by the means he employed.

C. Felix Desportes, præfect of the Upper Rhine, always disposed to favour researches useful to the sciences, sent me a fragment of the stone of Ensisheim weighing several kilogrammes, which contained on one side a portion of the black fused crust a little oxidated, and exhibited all the other properties of the other stones which have fallen from the atmosphere. There are found in it a kind of small veins of gray brilliant sulphuret of iron and nickel. We did not meet with any very apparent globules of iron.

A hundred parts of this stone, treated according to the processes already described, gave

Silex	-	-	-	56
Oxidated iron	-	-	-	30
Magnesia	-	-	-	12
Nickel	-	-	-	2.4
Sulphur	-	-	-	3.5
Lime	-	-	-	1.4

105.3

It contains then the same principles as the stone of Laigle, and differs from it only by a little less iron and nickel, and by a little more magnesia and silex: but this difference amounts only to a few hundredth parts.

On comparing the analysis of these two stones with those already made by Messrs. Howard and Vauquelin, it is impossible not to observe a striking identity in their composition.

SECTION III.

Conclusion and Reflections on the Origin of these Stones.

Here then we have nine stones all well ascertained to have fallen from the atmosphere with noise, detonation, luminous meteors; all gray, granulated, and metalliferous in the interior parts; which give the same products by analysis, containing no alumine, but a great deal of silex, a little magnesia, and a singular combination of iron, nickel, and sulphur: in a word, all perfectly similar to each other, and all different from the other known minerals of the earth.

It cannot therefore appear surprising that so striking a physical and chemical analogy should induce a belief that all these stones have the same origin, and that, as they form an order of compounds different from any thing ever yet observed

served among minerals, some philosophers should conclude that they do not belong to the fossils of our globe. Several hypotheses have consequently, for some months past, been devised to explain the formation of these singular productions.

It has long been asserted that they are nothing else but minerals elevated and projected from the earth by volcanoes. Other philosophers considered them as stones of our globe struck and fused on the outside by lightning on the spot where they were found; and lately they have been considered as earthy and metallic substances raised into the air, which being there collected and agglutinated have formed these masses, which immediately fell down by their own weight.

The manifest contradictions exhibited by these opinions, either with the principal circumstances or the fact itself, of the fall of these stones, have given rise to one less improbable, though perhaps more extraordinary. It is that of some geometricians, who consider them as volcanic productions projected from the moon beyond the sphere of its attraction, and to the confines of that of the earth.

If this opinion, on the first view, seems to be contradicted by all the ideas hitherto entertained, it is at any rate seen that it is much less susceptible of solid objections than any of the preceding hypotheses. The same may be said of that of Chladni, who with some other philosophers considers all the masses which have fallen to the earth as solid bodies detached from some other planet at the time of their formation, and which move about in infinite space till they meet with another, which becoming to them a new centre of gravity attracts them to its surface.

An analytical examination of all these hypotheses, and the little agreement between them and the aggregate of the circumstances which constantly accompany the phænomenon of the fall of stones, and which are essential to them, have induced the author of the *Lithologie Atmosphérique* to suppose that these stones are formed of the elements of those earths and metals which they exhibit by analysis; elements which he supposes to be in the gaseous state at a great height in the atmosphere, and the combination of which he ascribes to unknown circumstances that rarely occur. This opinion admits of several hypotheses, too far distant from what is yet known not to present difficulties which cannot be solved in the present state of our knowledge.

To conclude: In such situations one is obliged to choose
 3 among

among ideas each as uncommon as the other ; but it is only by rejecting what is absurd and impossible that we can adopt what at first would have appeared incredible.

LVI. *Of the general Relation between the Specific Gravities and the Strengths and Values of Spirituous Liquors, and the Circumstances by which the former are influenced.*

[Continued from p. 211.]

Of Over-Proofs and Under-Proofs, and the Modes of appreciating them.

§ 21. **T**HE first idea respecting the denomination of the relative values of spirituous liquors appears to have been that of Mr. Clarke, the inventor of the hydrometer now known by his name, which was founded on the supposed respective proportions of water which would be requisite to reduce an over-proof spirit to proof, or proof to an under-proof: the quantity of water was, however, considered as invariable, being always one gallon; whilst that of the spirit was regarded as variable, and as being so modified as to produce the required ratio between the two. Thus, a liquor which was so strong as to be supposed to require the addition of half its measure of water to reduce it to proof was called "one to two over proof," indicating that one gallon of water added to two of the spirit would make proof spirit of it; a spirit, with respect to which it was conceived that one-third of its measure of water would render it of proof strength, was called "one to three over proof," and so on. With regard to such liquors as were below the proof strength, an analogous mode of denomination was used. Thus, a liquor which was considered as being of equal strength with a mixture of one gallon of water with three of proof spirit was called "one to three," or more commonly "one in four under proof." This latter denomination became at length almost generally employed by those who used Clarke's hydrometer; so that "one to four" was regarded as signifying that a liquor was 25 per cent. over proof; and "one in four," that it was 25 per cent. under proof, without the use of these epithets themselves.

§ 22. It was impossible, however, not to feel the inconvenience of a system of denomination which was so complex and indefinite. Every series by which successive quantities, qualities, or values of any description are defined,

ought necessarily to be as nearly equi-differential as is consistent with the nature of the thing. This, however, is by no means the case with that which is here employed. The differences between the successive terms of the harmonic series one-half, one-third, one-fourth, one-fifth, one-sixth, &c., are one-sixth, one-twelfth, one-twentieth, one-thirtieth, &c.; or such that they are constantly to each other as the products of the terms between which they fall; and their inequality, therefore, becomes very soon exceedingly great.

It will perhaps, however, be more intelligible to some of our readers, if we explain this matter in other words. The first objection, then, which must naturally occur to every one with regard to this method of denominating strengths, is, that in one part of the series the difference between those of two denominations immediately succeeding each other is vastly too great, whilst at another part of it this difference is as disproportionally minute. Let us suppose, for example, that spirit of any particular kind, of proof strength, is worth 12s. per gallon. Then, what is *meant* by Clarke's "one to two," or that spirit of which 2 gallons would make 3 of proof, would be worth 12 multiplied by 3 divided by 2, or 18s. per gallon; and what is *meant* by his "one to three," or that of which 3 gallons would make 4 of proof, would be worth 12 multiplied by 4 divided by 3, or 16s. per gallon only. The difference between the strengths and values of these two kinds of spirit is enormous when regarded in a commercial point of view; and yet we have no denomination according to his system (without using more complex ratios and higher terms) for any intermediate strength. If we wished to express the strength of a spirit of the same kind which was in this respect worth 17s. per gallon, we have no name for it. Now let us look at two other terms of his series, and see what is the difference in value between his "one to nine" and "one to ten." The former of these is worth 12 multiplied by 10 divided by 9, or 13s. 4d.; and the latter, 12 multiplied by 11 divided by 10, or somewhat less than 13s. 2½d.; so that here the difference is but little more than 1½d. a gallon, instead of 2s.; and the further we proceed according to this system, the more the disproportion increases. But this is not all. There are many kinds of spirit which are above his "one to two," though we have not yet discovered any which would require an equal measure of water to reduce it to proof; and there are, on the other hand, faints and low wines which are worth preserving, which are below his

"one

“one in two:” and for all these we have no term whatever.

§ 23. The next mode of denomination of which we shall speak is that which is founded on the consideration of the proportion of water which it would be necessary to add to or subtract from a given quantity of any liquor, in order to render it of proof strength, and which proportion is estimated in hundredth parts of the quantity of the liquor in question. Thus a liquor of which 100 gallons would require the addition of 20 gallons of water to reduce it to the proof strength was said to be 20 per cent. over proof; and one of which 100 gallons would have required the *subtraction*, if that were possible, of 20 gallons of water to render it of proof strength (or, which is the same thing; of which 100 gallons might be produced by making up 20 of water to 100, by the addition of proof spirit), was said to be 20 per cent. under proof.

§ 24. This latter method of denominating the strengths of these liquors which obtained in consequence of the defects and inconveniences already stated in § 21 to have belonged to that of Mr. Clarke, was, as well as that, founded on the supposition that the quantity by measure of any compound of spirit and water would be equal to the sum of their quantities before mixture, the principle of concentration (§ 10) being, when it was first adopted, scarcely known.

If, indeed, this supposition were true, either mode of denomination would, though with different degrees of convenience, convey an idea of both the relative strengths and values of the compounds. This, however, being now known to be by no means the case, as has been already stated, it is, perhaps, rather unfortunate that the system of denomination mentioned in the last section has now acquired such a very general acceptance, that we may expect that it will not be without some difficulty that the same system, only so far changed as to render it consistent with the present state of science and truly indicative of relative strengths and values, will be received and understood.

§ 25. If the gallon of proof spirit is to be our standard of comparison, we should of course indicate its temperature, for quantities by *measure* are not the same at different degrees of heat (§ 8). We have already supposed this to be 60° (§ 20): but the gauging or measuring and proving of spirituous liquors, both by the revenue officers and dealers, are performed at various temperatures; and the quantity of the spirit itself which its measure indicates therefore varies accordingly. Now if this variation in bulk was equal

in spirituous compounds of all strengths, a gallon of any such liquor would at any degree of heat contain a certain proportion of its bulk of alcohol, and its per-centage would therefore not vary, whatever was its temperature, though a correction would be necessary with respect to the actual measure of each. We have already said, however, that this is not the case (§ 9); and it is also true, that not only the quantity of this variation is different in respect of their strengths, but the law of its progression varies still more. If the expansion of mercury be considered as uniform, as it appears to be very nearly between the freezing and boiling water points, the expansion of alcohol is progressively increasing, but in a very small degree, between 30° and 80° of Fahrenheit's thermometer; whereas water actually contracts by elevation of its temperature till it reaches 40° , after which it again begins to expand in a very increasing progression, its expansion of bulk between 70° and 80° being more than 5 times as great as that which takes place between 40° and 50° . The expansion of mixtures of these two fluids will in both respects approach nearest to that of the predominant ingredient; but the exact quantity of it, or the law of its progression in any such compound, can only be ascertained by experiment.

We must therefore apply such corrections in our process as may give us the quantity of proof spirit by weight; or, which amounts to the same thing, by measure when reduced to 60° , which is equivalent to 100 parts by measure of any spirit at any given temperature at which it may be measured and proved.

§ 26. The ultimate conclusion from the premises laid down in this chapter with respect to the mode of denominating the strengths of spirits differing from proof is, that it will be convenient that the scale should be graduated in per cents indicating the relative values of each compound with respect to the common standard of proof; or the number of parts of proof spirit by measure at 60° , which would produce or be producible from 100 parts by measure at any given temperature of any given spirituous liquor. Taking, therefore, the strength or value (for, *cæteris paribus*, they are in the same ratio) of proof spirit as denoted by the number 100, some other number between unity and 170 will represent that of every other spirituous compound.

If, for example, we mean to express a kind of spirit of such a strength, that, on 100 parts of it by measure at the existing temperature being reduced to 60° of Fahrenheit's thermometer, and then made up to 134 with water, it should

should become equal to proof, we should call it "134," or "34 per cent. over proof;" adding the quantity of the concentration, if we should wish to ascertain the necessary quantity of water for this purpose, which would in this case be about 2 parts more, or 36 parts in the whole. If we speak of a spirit of which 100 parts would be producible by the addition of water to 80 of proof spirit, we should call it "80," or "20 per cent. under proof." It appears to the authors that it would save unnecessary periphrasis, and be more convenient in several respects to use the former of these modes of expression, and omit the terms *over proof* and *under proof* altogether in the denomination of these strengths. If this system were adopted, the values of all spirituous liquors, when equal in other respects, would be in the direct ratio of their per-centages thus appreciated, and the duties might be estimated accordingly. If proof paid 5s., 120, or 20 per cent. over proof, should pay 6s.; and 80, or 20 per cent. under proof, only 4s.: and, in short, the relative values of proof spirit and of any given compound, or of any two liquors of different strengths, or the equivalent quantities of each, would in this case be to be determined in a moment.

We shall here, merely for the purpose of illustrating its advantages, give rules for the solution of two or three questions which occur hourly with respect to this subject, with an example or two to each, founded on the supposition of the establishment of that system of denomination which is recommended in this section.

I. To determine the Value per Gallon of Spirit whose Per-centage is known, when that of Proof of the same Value in other respects is given.

Practical Rule.—Multiply the value per gallon of proof by the per-centage of the liquor, and the product, pointing off two decimals on the right, gives the answer.

EXAMPLES.

1. What is the value per gallon of rum of 127 (or 27 O.P.), when proof of the same quality in other respects is at 14s.?

Ans. It is worth 168 d. = 14s.) $\times 127 \div 100 = 213.36$ pence, or 17s. 9½ d. per gallon.

2. What is the value per gallon of rum at 73 (or 27 U.P.), proof being worth 14s.?

Ans. It is worth 168 d. (= 14s.) $\times 73 \div 100 = 124.64$ pence, or 10s. 4¾ d. per gallon.

II. *To determine the Value per Gallon of any Spirituous Liquor differing in Strength from Proof, when that of any other Liquor, which is similar in all other respects except Strength, is known—the Per-Centage of each being given.*

Practical Rule.—*Multiply the value per gallon given by the per-centage of the liquor whose value is required; divide the product by the per-centage of the liquor whose value is given, and the quotient is the answer.*

EXAMPLES.

1. What is the value of brandy of 115 (or 15 O.P.) when that of 87 (or 13 U.P.) is 11s. per gallon?

Ans. It is worth 132 d. ($= 11s.$) $\times 115 \div 87 = 174\frac{1}{2}$ pence, or 14s. 6 $\frac{1}{2}$ d.

2. What is the value of brandy of 87 (or 13 U.P.) when that of 115 (or 15 O.P.) is 14s. 6 $\frac{1}{2}$ d. per gallon?

Ans. It is worth 174.5 d. ($= 14s. 6\frac{1}{2}d.$) $\times 87 \div 115 = 132$ pence, or 11s. per gallon.

III. *To determine the Quantity of Spirit of any given Strength which is equivalent in Value to a given Quantity of any other Strength.*

Practical Rule.—*Multiply the quantity given by the per-centage of the liquor; divide the product by the per-centage of the liquor whose quantity is required, and the quotient is the answer.*

EXAMPLES.

1. How much Hollands of 113 (or 13 O.P.) is equivalent to 556 gallons of 94 (or 6 U.P.)?

Ans. $556 \times 94 \div 113 = 462\frac{1}{2}$ gallons.

2. How much Hollands of 94 (or 6 U.P.) is equivalent to 462 $\frac{1}{2}$ gallons of 113 (or 13 O.P.)?

Ans. $462\frac{1}{2} \times 113 \div 94 = 556$ gallons.

§ 27. It will, perhaps, be expected from the authors of this essay that they should here enter into a considerable detail respecting the various constructions and uses of those instruments which they are known to have manufactured so extensively for several years past for these purposes. This it is by no means their intention to do in the present instance; a few observations, however, on the subject in general may not be unimportant.

It has been a very common error amongst the makers of these instruments to conceive that their stem should be very slender in proportion to their bulb; a construction which rendered a great number of weights necessary, and which

was by no means conducive to their accuracy. The surface of a slender stem is proportionally greater when compared with its solid content than that of a small one: the capillary attraction, therefore, and the weight of the liquor which adheres to it, both operate more powerfully. If we take any of those instruments whose stem is a mere wire, we shall find them, from these causes, so sluggish in their motion that they will generally stand at any point of the stem at which they are placed, within a quarter of an inch of a certain part of it, whereas those with a thicker stem regain the same point instantaneously. The thicker the stem, therefore, the better, so as it be only within such limits as to render the difference in the points at which the instrument will stand, in two liquors which differ in a very minute degree with respect to their specific gravities, sufficiently perceptible.

Let us calculate for a moment how many weights we must have to a spirit hydrometer. The stem may very conveniently be 4 inches long, and in this length we could easily graduate 40 or 50 divisions, so that a difference of one-fourth of each shall be very visible. But we want to express a difference of nearly 200 in specific gravity (or from 800 to 1000) by this instrument within about one-fourth of an unit. We shall therefore do it extremely well by the virtual lengthening of the stem to four or five times its real length by means of three or four weights. It is true, the divisions, unless they be arbitrary ones with a scale of reference, are not equal; if they be units of specific gravity, they must be graduated from several harmonic scales; but this will not affect the truth of our general deduction; and three or four weights, therefore, will be fully sufficient for the nicest spirit hydrometer, whatever scale of graduation we may use on its stem.

This kind of instrument would be far preferable, even if superior accuracy were alone the object, if simplicity were entirely out of the question, and 40 weights as easy and as little liable to error in their application as 4: but when we reflect that simplicity and facility of use are, perhaps, even of more consequence than accuracy in the result, the difference in the advantages of these modes of construction becomes enormous;

It has been a favourite opinion with some gentlemen, that glass hydrometers would be preferable to metallic ones, because they would not be liable to have their bulk altered by any contusion without being broken to pieces; whereas the latter might be so delved by rough usage as to give fal-

lacious indications of strength without its being perceived by the owner of them. The fact is, however, that although, for certain purposes, where metallic instruments cannot be used on account of their being liable to corrosion (as in the case of the mineral acids) we are obliged to employ glass, yet they are by no means capable of being rendered so accurate as those which are made of metal. The proportion of the bulb to the stem, and the consequent extent of the graduations, are, of course, within certain limits, matters of mere chance; and they are, therefore, not the subject of calculation. Every such instrument can only be graduated by direct experiment: and to those who would contend for the eligibility of such a method, where the other can be used, we can only say, that they have not sufficiently considered the subject. The metallic stems and bulbs, on the other hand, are by the tools employed for these purposes capable of being reduced so accurately to the required bulk, that the error in any point of the scale on this account will not amount to one-tenth of an unit in the specific gravity. Nor is it true that it is easily possible for the effect of such an accident to pass unnoticed by any one who ought to be entrusted with the use of an instrument at all. The least want of proper convexity in the bulb strikes the eye in an instant, even before its effect would be such as to be perceptible in the indication on the stem; and if it were not so, the simple operation of immersing the hydrometer, loaded with its proper weight in distilled or rain water at 60°, would at once detect such a circumstance, so that no probable inconvenience can ever result from such a cause.

[To be continued.]

LVII. *On the Purification of Nickel, with some Remarks on the Solutions of metallic Oxides in Ammonia, &c.* By Mr. R. PHILLIPS, Member of the Askesian and British Mineralogical Societies*.

ALTHOUGH nickel has been discovered more than a century, yet its existence as a peculiar metal has not been so well established but that several chemists, even of late years, have entertained considerable doubt on the subject. This has probably been occasioned by the difficulty in separating it from the metals with which it is usually mixed. Of these copper and cobalt, and more particularly the latter, strongly

* Communicated by the Author:

resemble it in some of its chemical habitudes. On account of its magnetic property, it has been supposed impossible to divest it of iron; and until the method lately adopted by R. Chenevix, esq. no certain means appear to have been known for the separation of the arsenic.

In order to purify this metal the following method may be adopted, which includes that above alluded to for the separation of the arsenic. Let nickel be dissolved in dilute nitric acid to complete saturation; to the filtered solution add nitrate of lead in sufficient quantity to precipitate the arsenic acid. If more should be employed than is required, the excess occasions no inconvenience. Having separated the arseniate of lead by the filter, add a small quantity of nitric acid to the solution, and immerse a bar of iron to precipitate the copper. This being done, there remain in solution the oxides of nickel, cobalt, iron, and lead, which may be precipitated by carbonate of potash. The precipitate, after sufficient washing, is to be put, while moist, into a solution of ammonia, which, dissolving the oxides of nickel and cobalt, leaves those of iron and lead to be separated by filtration.

All that is now necessary is to separate the oxides of cobalt and nickel, in order to complete the purification of the latter: but before the experiments for this purpose are related, it may not be amiss to state the properties of the ammoniacal solutions of some of the metallic oxides, although the habitudes of all of them are not immediately connected with the present inquiry. These I prepared by precipitating the oxides from their solutions in acid, and putting them, while moist, into solution of ammonia. Notwithstanding this is the most favourable state for the ammonia to act upon them, yet in every case, even after long digestion, the ammonia is in excess, since a part of it may be evaporated without causing any precipitation of the oxide. As these solutions possess different properties when the ammonia is in excess and when it is not, in the following experiments I shall call the former *solution*, and the latter *evaporated solution*. The evaporation was continued until moist turmeric paper, when held over the solutions, suffered no change.

Solution of oxide of silver in ammonia. Colourless: is not decomposed by water. *Evaporated solution*—Speedily decomposed by water: the precipitate blueish white, becoming gradually brown by exposure to light.

Solution of oxide of copper in ammonia.—Colour purple.

It is not decomposed by water.—The *evaporated solution* is immediately decomposed by it.

Solution of oxide of cobalt in ammonia.—Colour deep red: not decomposed by water. Nor is the *evaporated solution* decomposed by water, even when so diluted as to be nearly colourless.

Solution of oxide of nickel in ammonia.—Colour greenish blue: slowly decomposed by water. *Evaporated solution*—Colour pure green: decomposed immediately by water.

As silver and copper may be easily obtained from their solutions by well-known methods, the above-related experiments are more particularly applicable for the separation of cobalt and nickel; especially as the properties of their solutions differ very materially. But as the precipitation by water is often incomplete, and always inconvenient on account of the quantity necessarily employed, without examining the extent of its effects in the present instance, I tried other methods to effect the separation of these oxides, and, after some fruitless attempts, found potash answer this purpose extremely well. When a solution of it is added to the ammoniacal solutions of metallic oxides, the effects produced are as follow:

Solution of oxide of silver in ammonia.—Slowly decomposed, requiring three or four days for its completion. The precipitate is blackish brown. I have not examined whether it is fulminating. *Evaporated solution*—Immediately decomposed: the precipitate is of a lighter brown than the former.

Solution of oxide of copper in ammonia.—Slowly decomposed, and in very small quantity. *Evaporated solution*—Quickly decomposed.

Solution of oxide of cobalt in ammonia.—Very slowly and sparingly decomposed, even by large quantities of the solution of potash, and more slowly as the solution is more dilute. *Evaporated solution*, by the addition of potash, gradually changes from red to pink; then becomes scarlet, at length turns brown, and deposits brown oxide of cobalt. If a quantity of solution of potash be added to a small quantity of the evaporated solution, precipitation ensues in a few hours; but if a considerable quantity of water be added to similar quantities of the evaporated solution, and of potash, five or six days are required to complete the precipitation.

Solution of oxide of nickel in ammonia.—Immediately decomposed, and the more readily as it is more diluted. *Evaporated solution*—The same properties in a greater degree.

By

By comparing the above statements it appears proper, for the separation of the oxides in question, that the solution containing them should have the ammonia in excess, and be largely diluted before the addition of the potash. The excess of ammonia prevents, for a considerable length of time, any precipitation of the oxide of cobalt, while it produces no delay in the precipitation of the oxide of nickel. Mere dilution precipitates a part of the oxide of nickel; and at the same time that it renders the remainder more easy of precipitation, it prevents for several days any deposition of the oxide of cobalt. The following experiment will show that this method may be relied upon:

To a measure of solution of oxide of nickel in ammonia I added solution of potash as long as precipitation took place. The precipitate was washed, dried, and weighed. As it appeared probable that all the oxide of nickel might not be precipitated, I heated the solution after filtration till the ammonia had evaporated, but I did not obtain any further quantity of oxide.

To a similar measure of the same solution of oxide of nickel I added a measure of solution of oxide of cobalt in ammonia, and precipitated by potash. The precipitate appeared to be pure oxide of nickel; and, after drying, its weight did not differ 1-10th of a grain from that of the oxide of nickel obtained in the former experiment. This experiment was repeated with nearly similar results.

Soda produces the same effects as potash, and appears to act rather more readily; but whether in smaller quantity than the latter I have not tried. Carbonate of potash produces no effect. The decomposition of the ammoniacal solutions by potash appears to depend upon a combination of the two alkalies; the compound possessing, as is usual in chemical combinations, properties either partially or totally differing from those of its constituents. That this precipitation does not depend upon dilution has, I think, been already shown; and the following experiments more clearly determine that it must be ascribed to some other cause:

To one measure of solution of ammonia I added three measures of water. Moist oxide of nickel put into this dilute solution quickly coloured it. After several days digestion the solution decomposed by potash furnished nearly four grains of oxide.

One measure of solution of ammonia was mixed with three measures of dilute solution of potash. After as long digestion with moist oxide of nickel as in the former case, the
solution

solution had not acquired any colour; and on evaporating the ammonia no oxide of nickel was obtained.

As far as I have examined, the solutions of oxides in carbonate of ammonia possess the same properties as those in ammonia; for although carbonate of potash does not decompose the latter, yet the former are decomposed by potash. This is easily explained. Potash has a stronger affinity for carbonic acid than ammonia has: when, therefore, a carbonated ammoniacal solution is decomposed by it, a part of the potash combines with the carbonic acid of the carbonate of ammonia, and the remaining part decomposes what by the action of the other has become ammoniacal solution.

Considering that the supposition of a combination of the two alkalies would be strengthened if the experiments which gave rise to it could be reversed, i. e. if it were possible to precipitate with ammonia substances dissolved by potash, I dissolved in the latter silica, alumina, and several metallic oxides. On adding ammonia to these solutions, I at first thought I had succeeded in obtaining precipitates; but upon examining the ammonia employed, I found that it contained a small quantity of carbonic acid; and that when ammonia free from it was used, no precipitate was in any case obtained. Although these experiments did not succeed in supporting the above supposition, yet they are by no means fatal to it. It is probable that potash has a stronger affinity for the substances which it dissolves than it has for ammonia; and in this case, as no combination could be effected, no precipitation would ensue. In the first experiments potash and ammonia seem to possess a stronger affinity for each other than ammonia has for the oxides soluble in it.

Judging by the effects produced by water and potash, the affinity of ammonia for the metallic oxides appears to be as follows:

Oxide of cobalt,
Oxide of copper,
Oxide of silver,
Oxide of nickel.

I reduced a quantity of the oxide of nickel, obtained by the above-described process, and obtained a button of metal which exhibited the following properties:

Colour—Dull yellowish white.

Fracture—Foliated. Specific gravity 8.51.

Fragile, but capable of slight extension by hammering.

Strongly magnetic.

In one of the late French journals it is proposed to separate the oxides of cobalt and nickel by oxidizing the former by means of the hyperoxygenized muriatic acid. I have tried the method recommended without success.

LVIII. *On Machines for measuring Elasticity. By a Friend to Physical Inquiries.*

To the Editor of the Philosophical Magazine.

SIR,

A FEW years ago an eminent botanist, now abroad, being desirous of ascertaining the comparative elasticity of different woods, was wishing to have some machine made for that purpose; but whether or not any such machine has been yet made I am not acquainted. At the same time the subject was mentioned the following contrivances occurred to my mind, which, if carried into execution, might in some measure answer the purpose; and if you think them worth communicating to the public, I shall be glad to see them noticed in your magazine. They may serve for some of your readers as hints to improve on, and be the means of some much more accurate method of ascertaining the elasticity of bodies being invented.

First Contrivance.—A machine might be made which should have a flap on which balls of different sorts of wood, or other substances, might be placed, and suddenly let fall on a slab of ivory or marble: on this machine there should be an upright graduated post in order to see to what height each ball rebounded; by which, in some instances, the comparative elasticity of the body subjected to trial might be judged of.

Observations.—This method, in order to be accurate, should be tried in the exhausted receiver of an air-pump; otherwise with *light bodies*, such as cork and elder-pith, both very elastic, the experiment will not answer. The instrument for performing the common *guinea-and-feather* operation might, perhaps, be as good a contrivance as any: a wire cage might be placed under the receiver in order to protect the glass. Bodies of nearly the same gravity might be tried in the air. The standard ball might be ivory let fall on ivory, or marble let fall on marble.

Second Contrivance.—Thin slips of different sorts of wood, &c. might be bent half round a circular piece of board,

board, and suddenly let loose to fly back by their own force against a piece of wood hanging like a door on hinges*. The piece of wood should move on a graduated arch, in order to determine the degree of force with which the different slips fly back.

Observations.—How far this contrivance will answer I am not able to determine; but it appears on consideration that some judgment may be formed of the elastic quality of bodies by this method, where the substance is capable of being cut into slips, which will bend easily and recover their form again. Should either of these proposed schemes, or others more preferable, be adopted, it appears to me that the knowledge obtained by them might be of considerable use in various branches of art, as well as satisfactory to the inquiring naturalist.

London,
September 7, 1803.

LIX. *Account of two Fœtuses produced by the same Parrots which in the Year 1801 produced a young one at Rome.*
By C. L. MOROZZO, Member of the Institute of Bologna, of the Italian Society of the Sciences, of the Academy of Stockholm, and of that of Padua †.

IN the letter which I addressed to C. Lacépède in the year 1801 on the birth of a parrot at Rome (see Philosophical Magazine, No. XLVII.), I contracted an engagement with the public, as I promised to employ all my care on the eggs which these parrots might lay in the spring of the next year (1802). I shall now discharge my promise; and though my care was not crowned with success, I flatter myself that the amateurs of natural history will find in it some interesting details.

I did every thing in my power to get again into my hands these parrots, promising to the owner to take the greatest care of them possible. By these means I should have been enabled to observe more exactly their habits; but J. A. Passeri, the owner of them, who was governor of Orvieto, gave absolute orders that his son, in whose keeping they had been the preceding year, and at whose house I had made all my observations, should send them to him.

* Or it may be made to strike the end of a rod or arrow in the manner of the catapultæ. The distance to which the arrow is carried will determine the elastic force.—EDIT.

† From the *Journal de Physique*, Floréal, an. 11.

These birds had already begun their amours. No reasons were able to persuade him to recall his orders. I was therefore obliged to suffer them to depart: happily they sustained no injury by the way,

I therefore employed the only resource that remained; which was, to request him to take every possible care of them; to cause materials for building their nests to be placed within their reach; and to transmit to me a journal of every thing that might take place. I even delivered to him a small memorandum in regard to the observations which I more particularly wished him to make.

He informed me, that on the 21st of June the female deposited her first egg; that on the 25th she deposited another; and on the 29th a third. I flattered myself with the happiest success; but though the female sat on them continually, after the term of forty days, which we found the preceding years to be the term of incubation, observing that the first egg had been dragged out of the nest, and that the other two had been bruised, we thought proper to examine them; and the following is a literal translation of the procès-verbal, written in Italian, which was sent to me on the occasion:

“ We the undersigned, physician and surgeon of the town of Orvieto, declare to whom it may concern, that we saw in the house of J. A. Passeri, governor of the town, two parrots of the kind called Amazons, viz. a male and a female, and that on a requisition from the said governor we paid several visits to these birds after the female seemed ready to lay. We observed that the female on the 21st of June 1802 deposited an egg in a nest which she had constructed under a chest of drawers, and lined with linen rags which had been placed within her reach.

“ On the 25th of the same month she deposited a second, and on the 29th a third.

“ We observed that the female sat on the eggs, and that she continued to do so with great assiduity, and that during the whole time the male supplied her with food.

“ On the 18th of July we observed the first egg she had laid to be without the nest: a small round hole, suspected to have been made by the beak of the mother, was found in one side of it. On this egg being opened, nothing was found in it but a little coagulated matter in one corner, almost black, and putrid, without any appearance of an embryo.

“ On the 22d of the same month we examined one of the

the eggs which was in the nest, which was cracked, and depressed on one side, without any hole being observed as in the former. On the 24th it was remarked that the third egg had the same depression and fracture as the other.

“ The female, however, continued her incubation with great assiduity; and on the 2d of August, finding that the eggs would not hatch, and that the fœtus was dead, we determined that day, in presence of the canon Felix Albarici, and M. Bernard Piermattei, merchant of that city, to break the eggs with great caution; and when this was done we found the following results :

“ In one of the eggs we found the shell very hard, with an almost round depression in one of its sides: on the shell being broken it was observed that the pellicle by which eggs in general are enveloped was dry, and adhered to the fœtus. This pellicle being gently removed, we found the fœtus, which was completely formed, and we distinguished in it a small parrot. We observed that the head was placed between the thighs, with the beak large and long in the upper part, the tongue thick, the eyes shut, and covered by the eyelids. It had the two wings formed, as well as the thighs and legs, with claws on the toes, two of which were before and two behind. On the upper part of the head, the back, and as far as the tail, it was covered with white down; on the breast and belly there was none; on the middle of the belly was observed a carneous mass adhering to it, which resembled the placenta. We found also near the thorax a depression which corresponded to the contusion observed in the shell, and which there is reason to suppose was the cause of the death of the young.

“ In the shell of the other egg a contusion was found as in the first: the pellicle and fœtus were completely formed, though smaller than in the preceding, which gave us reason to believe that this egg had been the last that was laid. In this fœtus were observed on the right wing, and near the thorax, a depression and contusion corresponding to the fracture in the egg.

“ We remarked that the beak of the former was of an ash colour, like that of the male, and that the beak of the latter was blacker, like that of the female: we therefore supposed that the first was a male, and the second a female.

“ These two fœtuses were put into a flask with spirit of wine, closed and sealed in the presence of the above witnesses, to be kept as an authentic testimony of the prolific powers of these two parrots; which did not surprise us, as

we saw them produce young ones at Rome the preceding year, and as that produced in 1801 is lively, and now in the possession of the princess Venosa."

The witnesses who attest the above are :

Dr. Louis Bernardi,
Joseph Taruchi, surgeon,
Felix Alberici, canon,
Bernard Piermattei.

It is to be regretted that the hatching of these parrots has not been attended with the same success as it was last year.

We here see that the climate of Orvieto, which is more temperate than that of Rome, was no obstacle to the copulation or laying of these birds. What in all probability prevented the female from hatching the eggs was, that the parrots were not in separate apartments as the preceding year, and that they were deranged by the too great number of people who approached them either through curiosity or by accident, since they were placed in a chamber which served as a kitchen.

These birds are suspicious and timid, and it is probable that the female bruised the eggs in endeavouring to change their position with a view of placing them in more safety.

But as these birds, according to my calculation, are in the vigour of youth, it is to be hoped they will produce young hereafter; and that the owner, taught by the experience of this year, will take the necessary precaution of placing them in an apartment less exposed to noise and bustle, where the solitude of the place may bring them nearer to the wild state, and suffer them to complete their incubation as during the preceding year.

Turin,

September 25, 1802.

LX. *Account of a new Spring of Petroleum discovered in Italy; in a Letter to the Editors of the Annales de Chimie.* By J. POGGI*.

Paris, Dec. 22, 1802.

BEING detained in the capital by the great means it affords of applying to the study of nature, I think it my duty to communicate to you an account of an interesting phenomenon with which nature has enriched my country. It consists in a very abundant and permanent spring of *petroleum* or *naphtha*, which has appeared for some months at

* From the *Annales de Chimie*, No. 134.

Amiano, a village in the state of Parma, near Josnovo and Varese, on the confines of Liguria. Being vexed that the government of the country did not take this object into consideration, I have heard lately with satisfaction that the government of Liguria has converted it to a useful purpose. After being analysed by an able chemist, it has been applied with great advantage to the purpose of lighting the city of Genoa.

It was C. Mojon, professor of chemistry in the university of Genoa, who made the most conclusive experiments on this combustible substance. He read a report on this subject before the National Institute of Liguria on the 4th of July 1802; and it is an extract from this report, procured from the author himself, that I now have the honour of presenting to you.

Professor Mojon, having been on the spot, was enabled to see that the richness of the spring of petroleum at Amiano is so great, that, though people have continually drawn from it since its discovery, it still keeps at the same level. He has found in it the following characters:

This petroleum is exceedingly limpid, of a vinous yellow colour, or rather like that of the topase of Saxony. Its smell is strong, penetrating, and less empyreumatic than that of the common and brownish petroleum. Its specific gravity is to that of water as 83 to 100, and to that of olive oil as 91 to 100.

If a few drops of it be poured on writing paper and exposed to heat, it is entirely volatilised, and leaves no stain. If distilled in a glass retort, in a moderate heat, it passes entirely into the receiver, and leaves no residuum.

If it be mixed with concentrated sulphuric acid, it burns, becomes thick and dark-coloured. It readily dissolves amber, sulphur, and resins; and when mixed with gum copal forms a varnish, which, when spread over a piece of board, and exposed for some days to the sun, dries, and loses its odour. When poured on alcohol, it floats at the surface without dissolving, even when heated and strongly stirred. It floats also over fixed and volatile oils, with which it combines by agitation. Its combustibility is so great, that it inflames when brought near to a burning body: it seems even to attract the flame in consequence of its great volatility. Its own flame is white and lively, like that of oil of turpentine, but it emits much more smoke.

Having thus determined the characters of petroleum, professor Mojon, mindful of his commission, examined whether it might not be employed for lighting the city of Genoa.

noa. He made experiments to ascertain the intensity and degree of the light which this liquid produces in comparison of olive oil; and was able to make an exact calculation how far it would be advantageous to employ it.

He put an ounce of petroleum into a glass lamp; added a flat wick of four lines in breadth, placing it in such a manner that the bottom of the flame should be about an inch above the fluid. He then put an ounce of olive oil into another lamp of the same kind, and kindled both lamps at the same time. They gave a flame equally strong, and a light of the same intensity; but with this difference, that the former alone produced a little smoke, and burnt an hour and a half, while the lamp containing the olive oil burnt an hour and thirty-five minutes. Both of them were then dry, without leaving any residuum.

In order to try whether the smoke might not be destroyed or diminished, he burnt the petroleum in a lamp with a current of air furnished with a glass cylinder. He indeed found that the liquid, while it burned completely with a white and very bright flame, emitted no longer any smoke or bad smell. The combustion was so rapid that the flame was agitated by it. Having then introduced into the same lamp a mixture of equal parts of petroleum and olive oil, he obtained a slower combustion, with a more tranquil and uniform flame. He obtained the same result by the union of these two substances in a reverberating lamp with a wick of about an inch in breadth.

From these experiments he concluded that this petroleum might be advantageously employed for lighting the streets, observing the following precautions:

1st, That the flame should be about an inch above the oil.

2d, That the lamp should be covered and closed in such a manner as to prevent the flame from being communicated to the petroleum.

3d, To use a flat wick, to prevent smoke and render the combustion complete.

It was in consequence of this report of professor Mojon that the Ligurian government ordered petroleum to be employed for lighting the streets of Genoa. It is used at present without any mixture: the reverberators are constructed with the improvements above indicated; but care has been taken to add a kind of conical tube or chimney of tin plate, to convey off the smoke which may be disengaged. By these means the same quantity of light is obtained as with

olive oil, and at a fourth of the expense, as the petroleum costs only two Genoese sous per pound, (which is less than a penny English.)

LXI. *Report on the Discovery of a Deposit of bituminous Wood. Read in the Ligurian Institute July 1802 by C. MOJON, public Professor of Chemistry*.*

AT the period when nature opened in the state of Parma, on the north side of the Appenines, an extraordinary spring of petroleum †, it presented on the opposite side, in Liguria, at the distance of about fifteen leagues, a considerable depot of bituminous wood. The discovery of this fossil is even a little more recent, and is equally interesting to the naturalist and to the state. For a knowledge of it we are indebted to the useful labours of professor Mojon, as will be seen by the following extract from the report which he made to the National Institute of Liguria:

“Castel Nuovo is a country of Lunigiana on the confines of the Italian republic. It is in the plain of that country, half a league from the mouth of the Magra, that the mine of the substance in question has been discovered. It is situated in a soil formed of argillaceous and calcareous strata, more or less thick, and inclined in different directions throughout their whole extent. The nature of the fossil, as well as the constitution of the soil, evidently shows that these strata were formed only by great floods, which carried with them and buried whole forests ‡. The extent of this deposit, however, cannot be determined, because the pits proposed to be sunk at different distances for that purpose have not yet been constructed. Hitherto one only has been made, about 40 feet in depth, the bottom of which is inundated by a spring that issues from an excavation attempted in a lateral direction; and it is to be observed, that in this pit no disengagement has been remarked of carbonic acid gas, nor of those gases generally developed from such excavations.

* From the *Annales de Chimie*, No. 135.

† See the preceding article.

‡ Is there not reason to believe that the *Lucus sacer* of the antient *Luna* must form part of this deposit? The ruins of the town, in digging among which monuments worthy the attention of the antiquary have lately been discovered, are found very near, and at the distance of the third of a league from the mouth of the Magra.

“ This bituminous wood, which in some places appears almost uncovered, at the surface of the ground still retains its primitive figure. In the course of the search which has been made by digging, trunks of different thickness up to two feet diameter have been dug up; and among these there are some compressed, which exhibit in their transverse section an elliptical form.

“ The colour of this fossil is sometimes a perfect black, sometimes a grayish black, and sometimes a wood brown. The blackest is sometimes pretty shining, and has even the splendour of glass; the grayish black, and brown, are dull, but shine very much on being polished.

“ Its texture is entirely that of wood, as it has not been altered by the bitumen with which it is impregnated; so that in some pieces the tree to which they belonged may be determined; and in particular those which have the transverse fracture conchoid and shining, and which are perfectly black, exhibit all the characters of fir: others have a texture similar to that of oak.

“ It is not so brittle as coal, and, when sawn in a direction perpendicular to the axis of the trunks, exhibits a solid, compact, and very smooth surface. The concentric strata of the fibres of which the trunk is composed may be easily separated by introducing the point of any instrument whatever.

“ It readily kindles, and without the aid of any other combustible: it gives in burning a lively and brilliant flame. The heat it produces is more intense and durable than that of any other vegetable coal; and, when once kindled, never becomes extinct till entirely consumed, leaving very little ashes. The combustion may be interrupted and renewed at pleasure.

“ Lighter than coal, its specific gravity is to that of distilled water as 1235 to 1060: the fragments which exhibit the characters of oak are still lighter.

“ Sulphuret of iron is sometimes found in this fossil disseminated in lumps and small grains, which when long exposed to the air are decomposed, and cause the fragments to which they belong to fall in pieces.

“ It has besides the properties common to coal: but if naturalists have long entertained doubts respecting the origin of coals, in consequence of the variety of their texture, the irregularity of their form, their fragility, their strata analogous to those of schistous stones, &c., the fossil of Castel Nuovo presents no difficulty to their researches. The uniformity of the tissue; the constant direction of its fibres;

the bark of the different trunks; its knots; fracture; the colour of some kinds; the facility with which it inflames; the alkali contained in its ashes;—all these characters, very distinct, render it perfectly similar to charred wood.

“It must also be added, that it possesses this singular property, that it is susceptible of being worked in the lath with the greatest facility, and of acquiring a polish and splendour which render it superior to ebony.

“In the last place, it is a conductor of the electric fluid, and transmits it with facility.”

After this short account of the situation and nature of this fossil, I shall describe some of the experiments which the author made, in order to determine what advantages may be derived from the employment of this substance.

He burnt 12 pounds of this bituminous wood in a furnace, adapting to it a thermometer at the distance of eight inches, the temperature at the time being 10 degrees of Reaumur. Sixteen minutes after, the whole wood was completely kindled, and gave a clear brilliant flame, with less smoke than coal and a slight odour of bitumen, which not being sulphureous did not incommode those persons even who were nearest to it. The thermometer had risen to 42 degrees. Twenty-four pounds of water, which were in a copper vessel on the furnace, then began to boil. The thermometer stood at the same degree for 12 minutes, and the water continued to boil for 20. When the ebullition had ceased the thermometer fell to 26 degrees, and the water was reduced to three pounds. The fossil was entirely consumed in the course of an hour and a quarter, and left 225 grains of yellowish impalpable ashes in the form of flakes.

He then repeated the experiment under the same circumstances, and with oak charcoal. The latter kindled in twelve minutes, and in four more the 24 pounds of water contained in the copper boiler entered into ebullition. The thermometer rose to 38 degrees, at which it maintained itself for 18 minutes, and when the ebullition ceased the thermometer fell to 25 degrees. Almost the whole of the water was evaporated, and the coal, being consumed in the course of an hour and a half, left only 154 grains of ashes.

By this experiment, calculating the sum total of the heat respectively extricated during the combustion of the two substances, the author was able to remark, that the bituminous wood produced 816 degrees of heat during an hour and a quarter; while oak charcoal, in an hour and a half, produced only 784,

He also tried whether this fossil could be used with advantage in forges for forging iron. He kept at a red heat for some time the extremities of an iron rod, one in bituminous wood and the other in oak charcoal, employing equal parts. When the iron rod was taken out, he found its extremities equally ductile, malleable, and tenacious. There is every reason to think that this new combustible may even be employed in the reduction of iron ore; but this the author has not been able to ascertain, for want of materials.

Professor Mojon terminates his report with the following analysis:

This fossil wood gives almost the same products as coals, if we except the ashes, from which a little potash is extracted.

By distillation he obtained phlegm, yellowish bituminous oil, a quantity of carbonic acid gas, carbonated hydrogen gas, and an empyreumatic oil thicker than the former.

In alcohol a portion of this wood, at the end of some time, gave a blackish resinous substance.

By ebullition in distilled water it suffers to be precipitated calcareous earth and argil.

By pouring nitric acid over this fossil, it is decomposed, disengaging nitrous gas.

In the last place, the 225 grains of ashes, obtained by the above combustion of 12 pounds of this fossil, gave by lixiviation and filtration 1 grain of potash, oxide of iron, alumine, lime, and magnesia.

LXII. *Memoir on aëriform cutaneous Perspiration.* By C. TROUSSET, M. D. Professor of Natural Philosophy and Chemistry in the Central School of the Department of Isere, &c.*

PHYSICIANS at all times have endeavoured to ascertain the influence which the air has on the human body; but how can we conceive that the ancients, who were not acquainted with the gravity of that fluid, should have been able to determine its action? If we therefore except Hippocrates, who formally asserts in his works that air is digested in the lungs as the aliments are in the stomach, his contemporaries and successors have left us on this subject incoherent ideas, often ridiculous, and always erroneous, the fruits of an imagination not guided by any certain experience.

* From *Annales de Chimie*, No. 133.

If I intended to consider the influence of the air under every point of view, I should begin with respiration; a function of so much importance, that without it life could not exist, while it alone can for some time maintain and preserve it.

But the modern chemists, after an exact analysis of atmospheric air, have given a theory of respiration so ingenious, so complete, and so well founded on correct experiments, that all the attempts hitherto made to overturn it have only tended to establish it more and more.

I could therefore only repeat here what has been said before me by Lavoisier, Seguin, Crawford, Fourcroy, Chaptal, &c.; and this matter has been so well elucidated by the labours of these celebrated chemists, that it is in some measure exhausted; and their theory in this respect has been so widely diffused, that it is now adopted by all men of science. I shall therefore abstain from repeating what would be here tiresome and superfluous.

But if what takes place in the lungs is exactly known, the same light has not yet been thrown on the functions of the skin. Independently of cutaneous perspiration, observed with so much care and correctness by Sanctorius and several others, does there escape through the skin one or more aëriform fluids? and, in this case, of what nature are they? Such are the two questions which I purpose to examine in this memoir.

The antients had no idea of this aëriform cutaneous perspiration, and they make no mention of it in their works.

Count de Milly first announced, in 1777, the discovery of an elastic fluid * which escapes through the skin. He asserts that a person in a warm bath may collect half a pint of it in the course of three hours; and it results from his analysis, which indeed is very imperfect and incorrect, that it is fixed air (carbonic acid gas).

Dr. Ingenhousz announced some time after that an aëriform fluid escapes through the skin; but he believed it to be phlogisticated air (azotic gas.)

Dr. Priestley and M. Fontana repeated the experiments of the two preceding philosophers, and observed no aëriform emanation through the skin.

M. Jurine, a surgeon at Geneva, being desirous to become a candidate for the prize proposed by the Royal Society of Medicine †, repeated the experiments of Milly and

* Mem. de l'Acad. Royale des Sciences de Berlin, ann. 1777, p. 32.

† See Histoire et Mémoires de la Société de Médecine, tom. x. p. 54 et seq.

Ingenhousz, both on himself and on several individuals of different ages, employing different kinds of water, the temperature of which he varied, and asserts that he never observed the least aëriform emanation. Presuming that the water, according to its gravity, might impede the escape of the air, or that it might crisp the exhaling vessels of the skin, he continued his researches, varying the processes employed by Dr. Priestley and M. Fontana; and he thinks he has proved by experiments, the inexactness of which might be easily shown, that a small quantity of fixed air (carbonic acid gas) is continually escaping through the skin.

Fourcroy on this subject expresses himself as follows: "It is not true that elastic fluids, and particularly carbonic acid gas, escape through the skin, as some of the moderns have asserted*.

Such, a few years ago, was the state of the question which forms the subject of this article. Incorrect experiments, the contradictory results of which were contested either in the whole or in part, left philosophers in uncertainty, and seemed to call for new researches, in order to fix the opinion of philosophers on this point.

I often reflected on it, and had formed a design of employing myself with it, when in the spring of the year 8, being near one of my patients who was in the bath, I perceived that he was entirely covered with small bubbles of air. The hairs on his body were surrounded by bubbles decreasing from the base to the summit, so that a great number of them exhibited the appearance of pyramids more or less elevated. I made all these bubbles disappear; but in the course of half an hour they were succeeded by an equal number. In consequence of observing this phænomenon, I caused my patient to continue his bathing; and having collected several bell-fulls of this gas, I examined it carefully several times, and found that it was azotic gas, perfectly pure, without any mixture of carbonic acid.

I was then desirous to know whether this phænomenon was general, or whether it depended on the pathological state of the subject. I made experiments on myself, and on several other individuals, but never observed any thing of the kind.

In the beginning of the year 9 I communicated my experiments, and the result of them, to C. Fourcroy, who requested that I would continue them. Encouraged by the

* *Système des Connoissances Chimiques*, tom. ix. p. 203.

approbation of so celebrated a professor, I resolved to do so. It was not sufficient that I was certain of the exactness of my experiments; it was necessary also that I should convince philosophers of it.

Having collected with great care, in the spring of the year 9, a certain quantity of this gas, I filled with it a small bell 10 lines in diameter and 8 inches in height. A taper was immediately extinguished in it eleven times in succession.

It traversed lime-water without rendering it turbid, and without decreasing in volume.

It experienced no change from ammoniacal gas.

It produced no alteration in blue vegetable colours.

Phosphorus which remained immersed in it for more than a month had not decreased in volume.

I thought these experiments more than sufficient to convince me that the subject in question was azotic gas.

I again transmitted the result to C. Fourcroy in the month of Fructidor, year 9; but it is probable that the hurry of business prevented him from returning me an answer.

Having reflected a good deal since that period on the importance of this discovery, I did not cease to attend to it; but my ideas are entirely changed in regard to the consequences which may be deduced from it.

In letters written to Fourcroy, and particularly in the last, I considered my discovery as a particular fact depending on a pathological state; but at present I am obliged to consider it as a general phænomenon belonging to the human species.

1st, Because it is probable that the gas which passed so abundantly through the skin of count de Milly was azotic gas. One will easily be convinced of it by reading his memoir with attention.

2dly, Dr. Ingenhousz, convinced from his own experiments that an aëriform fluid escapes through the skin, believed that it was phlogisticated gas (azotic gas).

3dly, The idea of Ingenhousz is confirmed by my experiments.

4thly, I some time ago met with another individual who perspires abundantly in the bath. The bubbles with which he is constantly covered do not liquefy in the water; they are probably azotic gas: but I confess that I never made any exact experiment on the subject.

5thly, The experiments made by Dr. Priestley, Fontana, and Jurine, which consisted in placing open flasks under the armpits, afford no proof against what I have already ad-

vanced; for it is evident that, these flasks being filled with atmospheric air, the latter could not be displaced by the azotic gas, the specific gravity of which is less; while the case ought to have been different with carbonic acid gas, if any be constantly disengaged from the skin, as Jurine infers from his experiments.

Those which Jurine made, by placing his arm in a cover of glass, are not more conclusive; because, having been made with a view to prove that carbonic acid gas escaped, he did not employ the means proper for indicating the presence and quantity of azotic gas, of which he did not suspect the slightest disengagement. But why has not this phenomenon, which I believe to be general, been observed by Dr. Priestley, Fontana, Jurine, &c.? and why is it not remarked in all individuals placed under the same circumstances? It is probable, as I have said at the commencement of this memoir, according to M. Jurine, that the water acting by its gravity on the exhaling vessels of the skin, which in different individuals are endowed with a different energy, opposes in the greater number the escape of any gaseous substance.

I shall here terminate this memoir, to which I might have given more extent had I been inclined to treat of all the questions which naturally arise from the discovery of this phenomenon; but as I had no other object in view than to call the attention of philosophers to this subject, and being satisfied with having laid the first stone of the edifice, I leave to abler architects the glory of finishing the building.

LXIII. *Extract from the third Volume of the Analyses of M. Klaproth.*

THIS celebrated chemist, so well known for the precision of his analyses, and by the valuable discoveries with which he has enriched the province of chemistry, has published a third volume of his *Chemical Researches*, dedicated to Vauquelin. Among the number of ingenious analyses it contains, there are some particularly interesting to geologues: such as those of cryolite, sonorous porphyry, and basaltes; in which we are surpris'd to see soda form, and even in considerable quantity, one of the constituent principles of rocks and compact stones. That of the ombre earth of Cyprus is, we may say, the first with which we are acquainted, and the most correct. The rest, some extracts from which we shall
here

here present, confirm or rectify those before made by the most celebrated chemists.

Analysis of sonorous Porphyry.

Sonorous porphyry unites in it all the mineralogical characters of the other porphyries. Its substance, equally hard, is composed of silex and alumine, interspersed with some small leaves of feld-spath, and small grains, not very numerous, of amphibolite. But it differs from it by its foliaceous fracture. Besides, when broken, it has a sound almost metallic. The name of sonorous porphyry appears, then, to be perfectly suited to it.

It is found in the mountains of Bohemia, in those of Upper Lusatia, and in the country of Fulda. It never forms these continued chains, but only insulated mountains, and commonly situated in the neighbourhood of those of basaltes. It forms mountains of the hardest kind, and which present the greatest resistance to degradation. It effloresces only at the surface; and the argillaceous crust which covers it renders these stones very slippery, and of difficult ascent.

The colour of the sonorous porphyry is gray, sometimes inclining a little to green. It is always compact, rough, split in its fracture. It breaks into thick leaves, very thin fragments of which have pellucid edges. Its substance, the grain of which is fine, is very hard and smooth. When reduced to powder it is of a gray colour. Its specific gravity is 2.575.

Sonorous porphyry reduced into small fragments, and freed as much as possible from the leaves of feld-spar and grains of amphibolite disseminated throughout its mass, lost 3 per cent. of its weight by calcination. Its gray colour became lighter, but it experienced no other alteration.

When exposed to a porcelain furnace it fuses into a compact glass.

A hundred grains of sonorous porphyry reduced to powder, and treated in succession with potash, muriatic acid, barytes, and succinate of ammonia, gave

Silex	-	-	-	-	-	57.25
Alumine	-	-	-	-	-	23.50
Lime	-	-	-	-	-	2.75
Oxide of iron	-	-	-	-	-	3.25
Oxide of manganese	-	-	-	-	-	.25
Soda	-	-	-	-	-	8.10
Water	-	-	-	-	-	3.00
						<hr/>
						98.10
						<hr/>

The naturalist will be able to appreciate the discovery of soda as a constituent principle of rocks. It is seen that it is no longer necessary to recur to the decomposition of fossile or marine muriates of soda, to explain in all cases the formation of free soda, or of the carbonate of soda.

The sonorous porphyry subjected to this analysis was taken from the mountain of Donnersberg, near Milleschau, in Bohemia. This majestic cone, of 2500 feet height above the level of the sea, and from which is discovered, to the east, the magnificent plain of Prague, crowned by the high mountains of Bohemia and Silesia, and in the west the Fichtelberg, which overlooks Franconia, is entirely composed of the same rock of porphyry. If it be now considered that soda constitutes almost the twelfth part of that enormous mass, it will not appear exaggeration to advance, that the Donnersberg alone could supply all Europe with it for several centuries, if the least expensive means of extracting it could be discovered.

Analysis of the prismatic Basaltes of Hasenberg.

Dr. Kennedy has already published that he found soda in his analyses of the lava of Etna, and in basaltes: Klaproth has made a new analysis which confirms this discovery.

Basaltes exposed to the heat of a porcelain furnace, in a clay crucible, was converted into blackish brown glass, pellucid at the edges. In a crucible lined with charcoal, it was converted into a gray porous mass, abundantly interspersed with small grains of iron. The following is the result of his analysis:

Silex	-	-	-	-	44.50
Alumine	-	-	-	-	16.75
Oxide of iron	-	-	-	-	20.00
Lime	-	-	-	-	9.50
Magnesia	-	-	-	-	2.25
Oxide of manganese	-	-	-	-	0.12
Soda	-	-	-	-	2.60
Water	-	-	-	-	2.00
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					97.72
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That of the basaltes of the island of Staffa, given by Dr. Kennedy, approaches near to the preceding. The proportions are as follow:

Silex	-	-	-	-	48
Alumine	-	-	-	-	16
Oxide of iron	-	-	-	-	16
Lime	-	-	-	-	9
Soda	-	-	-	-	4
Muriatic acid	-	-	-	-	1
Water and volatile parts	-	-	-	-	5
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					99
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Dr. Kennedy makes no mention of magnesia, but he indicates muriatic acid as one of the principles of basaltes. M. Klaproth analysed it again, in order to try to discover it. He decomposed basaltes by nitric acid, and poured into the solution a solution of nitrate of silver. He observed a slight cloud, and collected a deposit which weighed 3-10ths of a grain. On this muriate of silver he found nitric acid, which reduced it to 1-20th of a grain; a quantity so small, that it does not announce the hundredth part of a grain of muriatic acid in the basaltes of Hasenberg.

Analysis of the Gold Ore of Transylvania.

It was by the analysis of these gold ores, so rich, that M. Klaproth discovered tellurium; and as this new metal constitutes the greater part of them, they have since been called the auriferous ores of Tellurium. Under this name M. Klaproth comprehends,

1st, That of paradoxal gold, or the problematic metal taken from Mariahilf, near Zalathna, in the mountains of Faczebayer in Transylvania.

A thousand parts of this ore of tellurium are composed of

Tellurium	-	-	-	925.50
Iron	-	-	-	72.00
Gold	-	-	-	2.50
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2d, The ore of graphic gold of Offenbanya, which is very rich. It contains

Tellurium	-	-	-	60
Gold	-	-	-	30
Silver	-	-	-	10
				<hr/>
				100
				<hr/>

3d, That

3d, That of Nagyac, which is more compounded. It contains

Tellurium	-	-	-	-	-	44.75
Gold	-	-	-	-	-	26.75
Lead	-	-	-	-	-	19.50
Silver	-	-	-	-	-	8.50
Sulphur	-	-	-	-	-	0.50
						100
						100

4th, The foliaceous ore of Nagyac. It contains

Lead	-	-	-	-	-	54.0
Tellurium	-	-	-	-	-	32.2
Gold	-	-	-	-	-	9.0
Silver	-	-	-	-	-	0.5
Copper	-	-	-	-	-	1.3
Sulphur	-	-	-	-	-	3.0
						100
						100

An Account of some new Properties of Tellurium.

Mercury seems only to be weakly attracted by this metal. One part of tellurium pulverised and heated in a small retort with six parts of mercury, seemed to be united into an amalgam with a crystallized surface. But M. Klaproth found that the mercury had scarcely dissolved any of the tellurium, and that the former had only covered the surface of it under the form of small scales.

The solution of tellurium in muriatic acid is clear. If water be added to the saturated solution, it first produces a precipitate, which a greater quantity of water afterwards redissolves. But if alcohol be poured in instead of water, and if the precipitate beedulcorated with alcohol, scarcely any tellurium will remain in the solution. Precipitates obtained by alcohol or by water are not pure oxides of tellurium. They always retain a little muriatic acid.

Solutions of tellurium where the acid predominates a little, are neither rendered turbid nor produce any precipitate by those of prussiate of potash. This is a remarkable property of this metal, which it however participates with gold, platina and antimony. Tincture of gall-nuts poured into a solution of it gives a flaky precipitate of an Isabella colour. Phosphorus immersed in a muriatic solution of tellurium becomes covered in the course of time with metallic leaves.

Analysis of the Tungsten Ore of Schlackenwald.

Scheele found in the calcareous *scheelin* of the ore of Bitzberg in Sweden,

Oxide of tungsten	-	-	65
Lime	-	-	31
Silex	-	-	4
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			100

Messrs. d'Ellyar found tungsten also in analysing the wolfram ore of Linnwald. At the same time they gave the following proportions for the white tungsten of Schlackenwald :

Yellow oxide of tungsten	-	68
Lime	-	30
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		98

Klaproth, in his new analysis of the grayish white *scheelin* of Schlackenwald, pellucid and crystallized in octaëdra, found them to be very different. Its specific gravity is 6.015.

A hundred parts of these crystals of *scheelin* gave :

Yellow oxide of tungsten	77.75
Lime	17.60
Silex	3.00
<hr/>	
98.35	

He extracted from 100 parts of the calcareous *scheelin* of Pengilly in Cornwall :

Yellow oxide of tungsten	75.25
Lime	18.70
Silex	1.50
Oxide of iron	1.25
Oxide of manganese	0.75
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97.45	

New Analysis of Gadolinite found at Itterby in Sweden :

Yttria	59.75
Silex	21.25
Black oxide of iron	17.50
Alumine	0.50
Water	0.50
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99.50	

Analysis of the Black Ore of Szekeremb in Transylvania.

Besides the red ore of manganese found at Szekeremb, which varies both in regard to its form and colour, and serves as matrix to the auriferous ores of tellurium worked there, there is one which exhibits the following characters :

It has a mean colour between the brown and the black of iron (ethiops). It is almost always compact, sometimes veined with red ore of manganese, or it forms alternate strata with that ore.

It has a semi-metallic state.

Its fracture is unequal, of a small grain, and foliated in every direction.

It splits into fragments irregularly angular, the edges of which are not very obtuse.

It is opaque.

When scratched the trace appears of the dark yellow colour of brass, inclining much to green, and almost entirely dull. It is of a mean hardness, and pretty smooth to the touch. Its specific gravity is 3.950. This ore contains .

Oxidulous manganese soluble in nitric acid	82
Carbonic acid - - - - -	5
Sulphur - - - - -	11
	98

New Analysis of Cryolite by M. Klaproth.

Cryolite is one of the most interesting of the new discoveries in mineralogy. It was found in Greenland, and a small quantity of it was some time ago received at Copenhagen. Professor Abilgard, who was of great service to the mineralogy of chemistry, undertook the analysis of it. He found it to be composed of fluoric acid and alumine; a very unexpected combination, no instance of which had before been found.

External Characters of Cryolite.

External form as yet undetermined; colour a bright whitish gray; fracture, when longitudinal, brilliant; when parallel to the axis, less brilliant. Both have a vitreous splendour: its fractures are foliated, and intersect each other at right angles; in the other directions they are unequal. Cryolite breaks into cubic fragments: it is semi-transparent, tender, and pretty soft to the touch; exceedingly fragile; and, according to Andrada, of the mean specific gravity of 2.9695, or according to Haüy 2.949.

Exposed to the blow-pipe on coals, it first forms itself into a white opaque round ball; but it afterwards loses its fusibility, and resembles earth strongly calcined. The name of cryolite then is not suited to this fossil because it fuses by the blow-pipe like glass, but perhaps because it has some external resemblance to it. Klaproth repeated the analysis of cryolite in the following manner, in order to determine with the greater precision the proportions of the constituent principles of this fossil: he heated to dryness in a platina crucible 100 grains of cryolite in powder with 300 grains of sulphuric acid, to effect a complete separation of the fluoric acid. The mixture at first boiled up with a disengagement of fluoric acid vapours. The residuum being dissolved in water was precipitated by evaporation under the form of a soft saline matter, which was easily redissolved by the addition of a little water.

2d. M. Klaproth precipitated from the solution, by means of caustic ammonia, the alumine, which when washed and dried weighed 46 grains, and only 24 when afterwards calcined. This earth, when dissolved by heat with diluted sulphuric acid, was entirely precipitated by the addition of potash in regular crystals of alum.

M. Klaproth neutralised, by means of acetous acid, the solution from which the alumine had been separated by ammonia: he poured into it acetite of barytes; and the liquor being filtered and evaporated, he brought the residuum to a red heat in a platina crucible. He then caused it to redissolve, and freed it by the filter from a small quantity of carbonaceous matter. By evaporation to dryness he obtained $62\frac{1}{2}$ grains of very dry carbonate of soda, which contained 36 grains of pure soda. Saturated with acetous acid, it wholly crystallized into acetite of soda.

Deducting from the 100 grains subjected to analysis the weight of the quantities of alumine and soda found in them, we shall have that of the fluoric acid and of the water of crystallization.

Cryolite then is composed of alumine, soda, and fluoric acid in the following proportions:

Soda	-	-	-	-	-	36
Alumine	-	-	-	-	-	24
Fluoric acid and water of crystallization						40
						<hr/> 100 <hr/>

Analysis of Umber Earth by M. Klaproth.

The old mineralogists gave the name of umber earth to a brown

brown earthy powder. It is composed of a brown carbonaceous earth, which may easily be known, as it is entirely converted into ashes by exposure to fire. On this account Cronstedt gave it the name of *mumia vegetalis*, and Wallerius calls it *humus umbra*. This substance has nothing in common with the real umber earth but the colour.

On the other hand, umber earth is incombustible, and, according to its constituent principles, ought to be classed among the ores of iron. It may be considered as a variety of bog iron ore. The only analysis made of it before that of Klaproth was by M. Santi, who analysed that of Castel del Piaro. The proportions he indicates are as follow :

Oxide of iron	-	-	-	53
Alumine	-	-	-	24
Silex	-	-	-	19
Magnesia	-	-	-	4
				100

But as M. Santi makes no mention of manganese, which however is one of the essential principles of umber earth, this omission must excite some doubts in regard to the correctness of the proportions which he indicates. The umber earth employed in the following analysis was procured from the island of Cyprus: it resembles externally that known in commerce under the name of fine Turkish umber, and is equally good for painting; it would therefore be superfluous to describe its exterior characters.

1st. Kept at a red heat for half an hour in a crucible, it lost 14 per cent. of its weight. But it experienced no other alteration except that its colour had become a darker brown.

2d. Exposed to a more violent heat, it enters into fusion. M. Klaproth put a fragment weighing 200 grains into a charcoal crucible, which he placed in a porcelain furnace. He took out the crucible quite safe, and containing a button well fused under a thick vitreous scoria of a hyacinth colour, and covered with points at the exterior surface. This metallic button was a little tenacious in its fracture under the hammer, and presented a granulated tissue like that of steel. It weighed 80 grains, and the vitreous scoria 47. The loss consequently amounted to 36½ per cent.

A hundred grains of umber earth well pulverized, mixed with 200 grains of concentrated sulphuric acid evaporated to dryness, were brought to a red heat in a crucible by means of a very strong fire. The red mass appeared of a brick colour: it was porous, and easy to be pulverized.

When washed with water and filtered, the liquor became brown, and contained sulphate of magnesia.

A hundred grains of amber earth reduced to fine powder, and digested with boiling muriatic acid, left a residuum which weighed 19 grains: The solution when filtered was condensed by evaporation, and mixed with a concentrated solution of tartrite of potash; but the mixture remained clear. M. Klaproth then endeavoured to obtain a precipitate by the solution of caustic soda, but did not succeed: he even did not observe any cloud, and the saturated solution assumed a reddish brown colour. He poured into it muriatic acid, and the colour of the solution was heightened: he then tried, but without success, to precipitate it by ammonia; for the mixture reappeared with the brownish red colour before indicated, and without any cloud.

M. Klaproth's object was to separate the manganese from the iron by means of these reagents; but this trial only confirmed a peculiar property of the tartareous acid, which is, that by its presence it renders iron soluble in alkaline solutions.

A hundred grains of pulverized amber earth were mixed with a solution of 200 grains of caustic soda, then desiccated to dryness, and exposed in a crucible to a moderate fire for half an hour. The mass dissolved in the water gave it a beautiful dark emerald green colour, which became of an amethyst red by the addition of muriatic acid. This acid added to saturation dissolved completely, and the liquor had acquired a red colour inclining to yellow. There were disengaged at the same time vapours of oxygenated muriatic acid. The solution, when evaporated almost to dryness, assumed at last the form of a jelly: diluted with new water, it left on the filter silex, which when well dried weighed 13 grains.

2d. M. Klaproth poured into the liquor a solution of caustic soda, which he added in excess, and the brown oxide of iron precipitated by it was washed. The remaining solution was precipitated by sulphuric acid, and neutralized by carbonate of potash: alumine was deposited, which when dried weighed 5 grains.

3d. The oxide of iron after being dried was black and brilliant, and resembled a piece of coal: it weighed 68 grains. R. dissolved in muriatic acid added only to the point of saturation, it was precipitated from it by the succinate of ammonia. The rest of the solution become colourless, mixed with the abundant water arising from the washing of the flaky deposit of iron, was again precipitated by that of caustic

caustic soda, with which Klaproth boiled it. The blackish brown flaky deposit, which was blacker after being washed and dried, weighed 20 grains: it was formed of oxide of manganese. This quantity deducted from the 68 grains of iron previously obtained, reduced the real proportion of the oxide of iron to 48 grains.

The proportions of the constituent principles of the amber earth of Cyprus then are :

Oxide of iron	-	-	48
Oxide of manganese	-	-	20
Silex	-	-	13
Alumine	-	-	5
Water	-	-	14
			100

Analysis of Spar in Tablets.

Spar in tablets* is generally found in a mixture of brown crystallized granite and blue calcareous spar, of which it forms the third substance. It is of a milky white colour. M. Karsten has described hexaëdral crystals of it. M. Stutz calls it spar in tablets, because it exhibits in its fracture long leaves a little brilliant. It is found, according to M. Stutz, at Dognaska in the canal of Temeswar, and according to M. Esner, at Oravuzza.

The analysis of it proves that it is not a kind of tremelite. Spar in tablets contains:

Silex	-	-	50
Lime	-	-	43
Water	-	-	5
			100 †

The lime in it is not carbonated, for nitric acid poured over this fossil produces neither bubbles nor loss in dissolving.

Analysis of Miemite.

Miemite takes its name from Miemo in Tuscany, where it was found by Dr. Thompson in 1791, and made known by him under the appellation of magnesian spar: it is of an asparagus green colour, crystallized in flattened triangular pyramids: it is of mean hardness and gravity, rough to the touch, &c. A hundred parts of miemite contain

* The analysis of spar in tablets is found in Klaproth's work next to that of pharraelite and scorza sand, as well as that of miemite.

† There appears here to be some mistake.

Carbonate of lime	-	-	-	-	53·00
Carbonate of magnesia	-	-	-	-	42·50
Carbonate of iron mixed with a little magnesia					3·00
					<hr/>
					98·50
					<hr/>

The proportion of the principles of miemite, which approaches very near to that of the magnesian spar of the Tyrol, ought to make them be considered as two species of the same kind.

Analysis of prismatic Magnesian Spar.

This spar, discovered at Gluuk Bronn, in the country of Gotha, in veins of cobalt, is very rare: its crystals are tetrahedral, almost rectangular, of an asparagus green colour, darker in several varieties than that of the chryso-beryl, and rarely so clear as that of the apatite of Cape Gates. They exhibit a vitreous splendour in their fracture; they break in irregular angular fragments; they are very pellucid, and of a mean hardness; they leave a snow-white trace; their specific gravity is 2·885.

Some crystals of magnesian spar, calcined in a platina crucible for half an hour, came from it entire; but exceedingly friable, and altogether opake. Zones of different colours, the interior of which was Isabella yellow, were observed in them; the second was reddish white, and the nucleus pink brown: they retained also some splendour, and had lost 45 per cent. of carbonic acid, though the whole acid was not then entirely volatilized. A hundred parts of magnesian spar contain

Lime	-	-	-	-	33·00
Magnesia	-	-	-	-	14·50
Oxide of iron					2·50
Carbonic acid	-	-	-	-	47·25
Water and loss	-	-	-	-	2·75
					<hr/>
					100
					<hr/>

Analysis of Egyptian Natrum.

We are indebted to M. Berthollet for the most correct description of the lakes of natrum, and of the deserts of Makaria in Lower Egypt. The natrum found there is not all of equal purity: from time immemorial it has been an important article of commerce.

The

The natrum of Egypt, when well freed from the earthy parts mixed with it, is composed of

Dry carbonate of soda	-	-	163
Dry sulphate of soda	-	-	104
Dry muriate of soda	-	-	75
Water	-	-	158
			<hr/>
			500
			<hr/>

Analysis of the radiated Natrum of Trona.

The natrum of Egypt is often found in very hard crystalline masses: it is indebted for its hardness to the muriate of soda, which is generally combined with it: so great is its hardness, that it has been even employed in constructing the walls of Fort Quasir.

That of Debrezin in Hungary, and of Monte Nuovo near Naples, having, on the other hand, lost its water of crystallization, is found only under the form of dust.

Radiated natrum is not subject to effloresce, though formed in the hottest climates of Africa: this phenomenon will be explained by the analysis hereafter given. The following is the description of this remarkable kind of natrum by M. Bagge, the Swedish consul at Tripoli:

“ It is found at a place called Trona, in the province of Sukena, two days journey from Fessand, at the foot of a rocky mountain, where it forms a crust of an inch in thickness at most, and sometimes of only a few lines: it is always crystallized: its fracture exhibits long crystals, agglutinated, parallel, and sometimes radiated: it resembles uncalcined plaster. Besides a large quantity of Trona, a name it has received from the place where it is found, which is carried to Negroland and Egypt, a thousand quintals of it are carried every year to Tripoli. It is not mixed with muriate of soda; and the salt mines are on the sea coast, twenty-eight miles from Trona, which is situated in the interior of the country.”

The natrum employed by M. Klaproth in his analysis forms a crystalline crust of from four to six lines in thickness, composed of vertical laminae of a foliated and radiated texture.

The natrum of Tripoli, according to analysis, contains

Water of crystallization	-	22.50
Carbonic acid	-	38.00
Pure soda	-	37.00
Sulphate of soda	-	2.00
		<hr/>
		99.50
		<hr/>

If the proportions of this natural carbonate of soda be compared with that of the recently prepared crystals of artificial carbonate of soda, which contain

Soda	-	-	-	-	22
Carbonic acid	-	-	-	-	16
Water of crystallization	-	-	-	-	62

100

a striking difference will be found in the proportion of the carbonic acid; for that of the carbonic acid is at most 73 parts in a hundred in the common carbonate, while it is nearly 103 in the natrum of Tripoli, which makes 30 more.

It is this more complete saturation of soda by the carbonic acid which gives to trona the property of not efflorescing. There are certainly some local circumstances which tend to favour it. But these must be determined by future naturalists who may visit the places where it is found.

Common carbonate of soda is not at its maximum of saturation; consequently it is susceptible of combining with a larger quantity of carbonic acid. This may be effected in the same manner as the complete saturation of the carbonate of potash. That of artificial soda completely saturated approaches, therefore, to trona, both externally in regard to the foliated form of its crystals, and the property which it acquires at the same time of presenting greater resistance to efflorescence.

[To be continued.]

LXIV. *On the Modifications of Clouds, and on the Principles of their Production, Suspension, and Destruction; being the Substance of an Essay read before the Askesian Society in the Session 1802-3.* By LUKE HOWARD, Esq.

[Continued from p. 107.]

Explanation of the Plates.

PLATE V. *a, a*, represents different appearances of the cirrus: *b*, a regular cumulus: *c*, a stratus occupying a valley at sun-set, in the midst of which is supposed a spot of higher ground, with trees, &c.

Plate VI. *a, a*, exhibits the character of the cirro-cumulus, as also its appearance in the distance. *b, b*, a light and a dark cirro-stratus; the former taken just before the commencement

mencement of wet weather, the latter in the twilight of the evening, when dew was falling: the smaller ones show its appearance in the distance. *c, c*, the mixed and the distinct cumulo-stratus; the latter in its most regular state, as sometimes seen at the approach of thunder storms and after showers.

Plate VII. A distant shower coming from behind an elevated point of land, in which are represented the superior sheet stretching in different parts to windward, and cumuli advancing towards and entering the mass, the whole of which constitutes the nimbus.

As the establishing distinctive characters for clouds has been heretofore deemed a desirable object, and it is consequently probable that the author's modifications will begin to be noted in meteorological registers as they occur, a practice which may be productive of considerable advantage to science, the following system of abbreviations may, perhaps, be found of some use in this respect. They will save room and the labour of writing, and types may be easily formed for printing them. These are advantages not to be despised, when observations are to be noted once or oftener in the day. It is only necessary that they be inserted in a column headed *Clouds*; that the modifications which appear together be placed side by side, and those which succeed to each other in the usual succession of the column, but separated by a line or space from the preceding and succeeding day's notations.

- ∖ Cirrus.
- ∪ Cumulus.
- Stratus.
- ∖∪ Cirro-cumulus.
- ∖— Cirro-stratus.
- ∪— Cumulo-stratus.
- ∖∪— Cirro-cumulo-stratus, or Nimbus.

In tracing the various appearances of clouds, we have only adverted to their connection with the different states of the atmosphere (on which, indeed, their diversity in a great measure depends), having purposely avoided mixing difficult and doubtful explanation with a simple descriptive arrangement.

Of Evaporation.

On the remote and universal origin of clouds there can be but one opinion—that the water of which they consist has been carried into the atmosphere by evaporation. It is

is on the nature of this process, the state in which the vapour subsists for a time, and the means by which the water becomes again visible, that the greatest diversity of opinion has prevailed.

The chemical philosopher, seduced by analogy, and accustomed more to the action of liquids on solids, naturally regards evaporation as a solution of water in the atmosphere, and the appearance of cloud as the first indication of its precipitation; which becoming afterwards (under favourable circumstances) more abundant, produces rain. The theory of Dr. Hutton goes a step further, assumes a certain rate of solution differing from that of the advance of temperature by which it is effected, and deduces a general explanation of clouds and rain from the precipitation which, according to his rule, should result from every mixture of different portions of saturated air. The fundamental principle of this theory has been disproved in an essay heretofore presented to the society*, and which was written under the opinion, at present generally adopted by chemists, that evaporation depends on a solvent power in the atmosphere, and follows the general rules of chemical solution.

The author has since espoused a theory of evaporation which altogether excludes the above-mentioned opinion (and consequently Dr. Hutton's also), and considers himself in a considerable degree indebted to it for the origin of the explanation he is about to offer. It will be proper, therefore, to state the fundamental propositions of this theory, with such other parts as appear immediately necessary, referring for mathematical demonstrations and detail of experiments to the work itself, which is entitled "Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Elastic Fluids by Heat. By John Dalton."—See *Memoirs of the Literary and Philosophical Society of Manchester*, vol. v. part 2.—The propositions are as follow:

"1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

"2. The force of steam from all liquids is the same at equal distances above or below the several temperatures at which they boil in the open air; and that force is the same under

* See *Phil. Mag.* vol. xiv. p. 55.

any pressure of another elastic fluid as it is *in vacuo*. Thus the force of aqueous vapour of 212° is equal to 30 inches of mercury; at 30° below, or 182° , it is of half that force; and at 40° above, or 252° , it is of double the force: so likewise the vapour from sulphuric ether which boils at 102° , then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force: and so in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to half an inch of mercury when admitted into a Torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

“ 3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.”

The following is part of the Essay on Evaporation:

“ When a liquid is exposed to the air, it becomes gradually dissipated in it; the process by which this effect is produced we call *evaporation*.

“ Many philosophers concur in the theory of chemical solution: atmospheric air, it is said, has an affinity for water; it is a menstruum in which water is soluble to a certain degree. It is allowed notwithstanding by all, that each liquid is convertible into an elastic vapour *in vacuo*, which can subsist independently in any temperature; but as the utmost forces of these vapours are inferior to the pressure of the atmosphere in ordinary temperatures, they are supposed to be incapable of existing in it in the same way as they do in a Torricellian vacuum: hence the notion of affinity is induced. According to this theory of evaporation, atmospheric air (and every other species of air for aught that appears) dissolves water, alcohol, ether, acids, and even metals. Water below 212° is chemically combined with the gases; above 212° it assumes a new form, and becomes a distinct elastic fluid, called *steam*: whether water first chemically combined with air, and then heated above 212° , is detached from the air or remains with it, the advocates of the theory have not determined. This theory has always been considered as complex, and attended with difficulties; so much that M. Pictet and others have rejected it, and adopted that which admits of distinct elastic vapours in the atmosphere at all temperatures, uncombined with either of the principal constituent gases, as being much more simple and easy of explanation than the other; though they do not remove the grand objection to it, arising from atmospheric pressure.”

“ On

“ On the Evaporation of Water below 212° .”

“ I have frequently tried the evaporation at all the temperatures below 212° : it would be tedious to enter into detail of all the experiments, but shall give the results at some remarkable points. In all the high temperatures I used the vessel above mentioned*, keeping a thermometer in it, by which I could secure a constant heat, or at least keep it oscillating within narrow limits.

“ The evaporation from water of 180° was from 18 to 22 grains per minute, according to circumstances; or about one-half of that at 212° .

“ At 164° it was about one-third of the quantity at the boiling temperature, or from 10 to 16 grains per minute.

“ At 152° it was only one-fourth of that at boiling, or from 8 to 12 grains, according to circumstances.

“ The temperature of 144° affords one-fifth of the effect at boiling; 138° gave one-sixth, &c.

“ Having previously to these experiments determined the force of aqueous vapour at all the temperatures under 212° , I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the same temperature, and was agreeably surprised to find that they exactly corresponded in every part of the thermometric scale: thus the forces of vapour at 212° , 180° , 164° , 152° , 144° , and 138° , are equal to 30, 15, 10, $7\frac{1}{2}$, 6, and 5 inches of mercury respectively; and the grains of water evaporated per minute in those temperatures were 30, 15, 10, $7\frac{1}{2}$, 6, and 5, also; or numbers proportional to these. Indeed it should be so from the established law of mechanics, that all effects are proportional to the causes producing them. The atmosphere, it should seem, obstructs the diffusion of vapour, which would otherwise be almost instantaneous, as *in vacuo*; but this obstruction is overcome in proportion to the force of the vapour. The obstruction, however, cannot arise from the weight of the atmosphere, as has till now been supposed; for then it would effectually prevent any vapour from arising under 212° : but it is caused by the *vis inertiae* of the particles of air, and is similar to that which a stream of water meets with in descending amongst pebbles.

“ The theory of evaporation being thus manifested from

* This refers to experiments on the evaporation of water at 212° ; for which see the Essay.

experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures regard must be had to the force of vapour actually existing in the atmosphere at the time. For instance, if water of 59° were the subject, the force of vapour of that temperature is 1-60th of the force at 212° , and one might expect the quantity of evaporation 1-60th also; but if it should happen, as it sometimes does in summer, that an aqueous atmosphere to that amount does already exist, the evaporation, instead of being 1-60th of that from boiling water, would be nothing at all. On the other hand, if the aqueous atmosphere were less than that, suppose one-half of it, corresponding to 39° of heat, then the effective evaporating force would be 1-120th of that from boiling water; in short, the evaporating force must be universally equal to that of the temperature of the water, diminished by that already existing in the atmosphere. In order to find the force of the aqueous atmosphere I usually take a tall cylindrical glass jar, dry on the outside, and fill it with cold spring water fresh from the well: if dew be immediately formed on the outside, I pour the water out, let it stand a while to increase in heat, dry the outside of the glass well with a linen cloth, and then pour the water in again: this operation is to be continued till dew ceases to be formed, and then the temperature of the water must be observed; and opposite to it in the table will be found the force of vapour in the atmosphere. This must be done in the open air, or at a window; because the air within is generally more humid than that without. Spring water is generally about 50° , and will mostly answer the purpose the three hottest months in the year; in other seasons an artificial cold mixture is required. The accuracy of the result obtained this way I think scarcely needs to be insisted upon. Glass, and all other hard, smooth substances I have tried, when cooled to a degree below what the surrounding aqueous vapour can support, cause it to be condensed on their surfaces into water. The degree of cold is usually from 1 to 10 below the mean heat of the 24 hours; in summer I have often observed the point as high as 58° or 59° , corresponding to half an inch of mercury in force; and once or twice have seen it at 62° : in changeable and windy weather it is liable to considerable fluctuation: but this is not the place to enlarge upon it.

“ For the purpose of observing the evaporation in atmospheric temperatures I got two light tin vessels, the one six inches in diameter and half an inch deep, the other eight inches diameter and three-fourths of an inch deep,
and

and made to be suspended from a balance. When any experiment, designed as a test of the theory, was made, a quantity of water was put into one of these (generally the six-inch one, which I preferred), the whole was weighed to a grain; then it was placed in an open window or other exposed situation for 10 or 15 minutes, and again weighed to ascertain the loss by evaporation; at the same time the temperature of the water was observed, the force of the aqueous atmosphere ascertained as above, and the strength of the current of air noticed. From a great variety of experiments made both in the winter and summer, and when the evaporating force was strong and weak, I have found the results entirely conformable with the above theory. The same quantity is evaporated with the same evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the same evaporating force, a strong wind will double the effect produced in a still atmosphere. Thus, if the aqueous atmosphere be correspondent to 40° of temperature and the air be 60° , the evaporation is the same as if the aqueous atmosphere were at 60° of temperature and the air 72° ; and in a calm air the evaporation from a vessel of six inches in diameter in such circumstances would be about $\cdot 9$ of a grain per minute, and about 1.8 grains per minute in a very strong wind; the different intermediate quantities being regulated solely by the force of the wind."

Of the Aqueous Atmosphere.

Having quoted so much of this essay as may suffice to exhibit the principles on which we shall proceed, it may be useful, before we do this, to recapitulate the following circumstances respecting the atmosphere of aqueous gas, or (for brevity) the aqueous atmosphere.

1st, It is supplied by the process of evaporation, which by this theory appears to be reduced to the immediate union of water with caloric into a binary compound, *aqueous gas*.

2dly, The supply of vapour (by which term, for the purposes of meteorology, we may denote aqueous gas), is regulated by the following circumstances:—1. Temperature of the evaporating water, being greater as this is higher, and *vice versá*. 2. Quantity of surface exposed. Since it is from the surface only of the mass that the vapour in common cases can escape, the supply is in direct proportion thereto. 3. Quantity of vapour already subsisting in the atmosphere: the evaporation being less (with equal temperature and surface) in proportion as this is greater, and *vice versá*.

3dly,

3dly, The vapour thus thrown into the atmosphere is diffusible therein by its own elasticity, which suffices for its ascent to any height in a perfect calm. Yet, as in this case the *inertia* of the particles of air considerably resists its diffusion, so in the opposite one of a brisk current, the vapour, by the same rule, must in some measure be drawn along with the mass into which it enters.

4thly, The quantity of vapour which, under equal pressure, can subsist in a given mass of air, will be greater as the common temperature is higher, and *vice versâ* *.

Aqueous vapour is the only gas contained in the atmosphere which is subject to very sensible variations in quantity. These variations arise from its attraction for caloric being inferior to that of all the others. Hence when a cold body, such as the glass of water in the experiment above quoted, is presented to the atmosphere, the other gases, composing the latter, will only be cooled by it (and that at all known temperatures); but the vapour, after being more or less cooled, will begin to be decomposed, its caloric entering the body while the water is left on the surface.

The formation of cloud is in all cases the *remote* consequence of a decomposition thus effected, except that the caloric escapes, not into a solid or liquid, but into the surrounding gases.

Of the Formation of Dew.

Dew is the *immediate* result of this decomposition. The particles of water constituting it are, singly, invisible, on account of their extreme minuteness. The approach of dew is, nevertheless, discoverable by a dark hazy appearance, verging from purple to faint red, extending from the horizon to a small distance upwards, and most conspicuous over valleys and large pieces of water.

The theory of dew seems to be simply this:—During the heat of the day a great quantity of vapour is thrown into the atmosphere from the surface of the earth and waters. When

* "The aqueous vapour atmosphere is variable in quantity according to temperature: in the torrid zone its pressure on the surface of the earth is equal to the force of .6, and from that to one inch of mercury. In these parts it rarely amounts to the pressure of .6, but I have frequently observed it above half an inch in summer: in winter it is sometimes so low as to be of no more force than .1 of an inch of mercury, or even half a tenth, in this latitude, and consequently much less where the cold is more severe. This want of equilibrium in the aqueous vapour atmosphere is a principal cause of that constant foundation of it into the temperate and frigid zones, where it becomes in part condensed in its progress by the cold, like the vapour of distillation in the worm of a refrigeratory, and supplies the earth with rain and dew." See the Essays above quoted.

the evening returns, if the vapour has not been carried off in part by currents, it will often happen that more remains diffused in the general atmosphere than the temperature of the night will permit to subsist under the full pressure of the aqueous atmosphere. A decomposition of the latter then commences, and is continued until the general temperature and aqueous pressure arrive at an equilibrium, or until the returning sun puts an end to the process. The caloric of the decomposed vapour goes to maintain the general temperature; while the water is separated in drops, which, minute as they are, arrive successively at the earth in the space of a few hours. That the ordinary production of dew is by a real *descent* of water from the atmosphere, and not by decomposition of vapour on surfaces previously cooled (as in the experiment already mentioned), any one may readily be convinced by observing in what abundance it is collected by substances which are wholly unfit to carry off the requisite quantity of caloric for the latter effect.

Of the Formation of the Stratus.

The case which has been just stated, *of the decomposition of vapour by the atmosphere in which it is already diffused*, goes but a little way in explanation of the production of a cloud consisting of visible drops, and confined to a certain space in the atmosphere: much less does it enable us to account for the diversity of its situations and appearances. In attempting this we will begin with the stratus, as the most simple in structure, and the next step, as it were, in the progress of *nubification*.

When dew falls upon a surface the temperature of which is superior to that of the atmosphere, it is plain that it will not continue there, but will be evaporated again: and a body so circumstanced will continue to refund into the atmosphere the whole of the water thus *gradually* deposited on it, so long as its substance can supply the requisite temperature to the surface. Moreover, water either in mass, or diffused among sand, clay, vegetable earth, &c. will continue to be evaporated therefrom with a force proportioned to its temperature, so long as the latter continues above that point which counterbalances the pressure of the aqueous atmosphere.

From these causes it happens, that after the earth has been superficially dried by a continuance of sunshine, and heated, together with the lakes and rivers, to a considerable depth, there is an almost continual emission of vapour into the atmosphere by night.

This nocturnal evaporation is usually most powerful in the autumn, about the time that the temperature of the nights undergoes a considerable and sometimes pretty sudden depression attended with a calm.

In this state of things the vapour arising from the heated earth is condensed *in the act of diffusing itself* (if we may be allowed the expression): the cold particles of water thus formed, in *descending* meet the ascending stream of vapour, and condense a portion on their surfaces: if they touch the earth they are again evaporated, which is not necessarily the case if they alight on the herbage. In this way an aggregate of visible drops is sooner or later formed; and as from the temperature thus communicated to the air next the earth the vapour has still further and further to rise in order to be condensed, the cloud will be propagated upward in proportion.

Hence the stratus most usually makes its appearance in the evening succeeding a clear warm day, and in that quiescent state of the atmosphere which attends a succession of them. Hence also the frequency of it during the penetration of the autumnal rains into the earth; while in spring, when the latter is *acquiring* temperature together with the atmosphere, it is rarely seen.

Of the Formation of the Cumulus.

When the sun's rays traverse a clear space of atmosphere, it is well known that they communicate no sensible increase of temperature thereto. It is by the contact, and what may be termed the *radiation*, of opaque substances exposed to the light, that caloric is thrown into the atmosphere.

This effect is first produced on the air adjacent to the earth's surface, and proceeds upward, more or less rapidly, according to the season and other attendant circumstances. In the morning, therefore, evaporation usually prevails again; and the vapour which continues to be thrown into air, now increasing in temperature, is no longer condensed: on the contrary, it exerts its elastic force on that which the nocturnal temperature had not been able to decompose, and which consequently remained universally diffused. The latter, in rising *through the atmosphere* to give place to the supply from below, must necessarily change its climate, quit the lower air of equal temperature, and arrive among more elevated and colder air, the pressure from above still continuing unabated. The consequence is a partial decompo-

* A plentiful dew may often be found on the grass after a stratus.

sition, extending through the portion thus thrown up, and, in short, a recommencement in the superior region, of the same process which in the vicinity of the earth furnished the dew of the night. In this case, however, the particles of water cannot arrive at the earth, as they are necessarily evaporated again in their descent.

It appears that this second evaporation takes place precisely at that elevation where the temperature derived from the action of the sun's rays upon the earth, and decreasing upward, becomes just sufficient to counterbalance the pressure of the superior vapour.

Here is formed a sort of boundary between the region of cloud and the region of permanent vapour, which for the present purpose, and until we are furnished with a nomenclature for the whole science of meteorology, may be denominated the evaporating plane, or, more simply, the vapour plane*.

Immediately above the vapour plane, then, the formation of the cumulus commences (as soon as a sufficient quantity of vapour has been thrown up) by the mixture of descending minute drops of water with vapour newly formed, and just diffusing itself, as in the case of the stratus before described.

A continuance of this process might be expected to produce a uniform sheet of cloud, in short a stratus, only differing in situation from the true one. Instead of which we see the first-formed small masses become so many centres, towards which all the water afterwards precipitated appears to be attracted from the space surrounding them; and this attraction becomes more powerful as the cloud increases in magnitude, insomuch that the small clouds previously formed disappear when a large one approaches them in its increase, and seem to vanish instead of joining it. This is probably owing to the small drops composing them having passed in a loose manner, and successively, into the large one, and consequently so as not to be traced in their passage.

Are the distinct masses into which the drops form themselves, in this case, due to the attraction of aggregation alone, or is the operation of any other cause to be admitted?

A rigid mathematician would perhaps answer the latter clause in the negative; and with such a conclusion we should have great reason to remain satisfied, as cutting short

* We use the word *plane* here in the same sense in which we apply it to a surface of water. Strictly speaking, it is a portion of a sphere.

much of the inquiry that is to follow, were it not that it leaves us quite in the dark, both as to the cause of the variety so readily observable in clouds, and their long suspension, not to insist on several facts contained in the former part of this paper, which would then remain unaccounted for.

The operation of one simple principle would produce an effect at all times *uniform*, and varying only in degree. We should then see no diversity in clouds but in their magnitude, and the same attraction that could bring minute drops of water together through a considerable space of atmosphere in a few minutes, ought not to end there, but to effect their perfect union into larger, and finally into rain.

In admitting the constant operation of electricity, which is sometimes so manifestly accumulated in clouds, upon their forms and arrangement, we shall not much overstep the limits of experimental inquiry, since it has been ascertained by several eminent philosophers, that “ clouds, as well as rain, snow, and hail, that fall from them, are almost always electrified *.”

An insulated conductor formed of solid matter retains the charge given to it so much the longer, as it is more nearly spherical, and free from points and projecting parts. The particles of water, when charged, appear to make an effort to separate from each other, or, in other words, become mutually repulsive. Moreover, when a small conducting substance is brought within the reach of a large one similarly electrified, the latter, instead of repelling, will throw the small one into an opposite state, and then attract it. From these and other well-known facts in electricity it would not be difficult to demonstrate, that an assemblage of particles of water floating in the atmosphere, and similarly electrified, ought to arrange themselves in a spherical aggregate, into which all the surrounding particles of water should be attracted (within a certain distance) at the same time that the drops composing such aggregate should be absolutely prevented from uniting with each other *during the equilibrium of their electricity*.

To apply this reasoning to the formation of the cumulus, we may, in the first place, admit that the commencement of distinct aggregation, in the descending particles of water, is due to their mutual attraction, by virtue of which small bodies, floating in any medium, tend to coalesce. The masses thus formed, however, often increase more rapidly than could be expected from the effect of simple attraction

* Cavallo. Complete Treatise on Electricity, vol. i. p. 74.

exercised at great distances; and when the cloud has arrived at a considerable size, its protuberances are seen to form, and successively sink down into the mass, in a manner which forces one to suppose a shower of invisible drops rushing upon it from all parts.

In unsettled weather the rapid formation of large cumuli has been observed to clear the sky of a considerable hazy whiteness, which on the other hand has been found to ensue upon their *dispersion**.

On these considerations we are obliged to admit as a co-operating cause of the increase of this cloud, that sort of attraction which large insulated conducting masses exercise, when charged, on the smaller ones which lie within their influence. Instead of a *spherical* aggregate, however, we have only a sort of hemisphere, because that part of the cloud which presents itself towards the earth can receive no addition from beneath, there being no condensed water. On the contrary, the mass must be continually suffering a diminution there by the tendency of the cloud to subside, and of the vapour plane to rise during the increase of the diurnal temperature. It is this evaporation that cuts off all the cumuli visible at one time in the same plane, and it is reasonable to conclude that much of the vapour thus produced is again condensed without quitting the cloud, as its course would naturally be mostly upward. Thus the drops of which a cumulus consists may become larger the longer it is suspended, and the electricity stronger from the comparative diminution of surface.

Such is probably the manner in which this curious structure is raised, while the base is continually escaping from beneath it. That we may not however be accused of building such a castle in the air by attempting further conjecture, we may leave the present modification, after recapitulating some of its circumstances which appear to be accounted for.

The cumulus appears only in the day time, because the direct action of the sun's rays upon the earth can alone put the atmosphere into that state of inequality of temperature which has been described. It evaporates in the evening

* That clouds are not always evaporated when they disappear, but sometimes dispersed so as to become invisible as distinct aggregates, is a fact pretty well ascertained by observations. This happens sometimes by the approach of other clouds; at others, the evaporation of part of a cumulus is followed by the dispersion of the remainder. The criterion used was the speedy production of transparency in the one case, and of hazy turbidness in the other.

from the cessation of this inequality, the superior atmosphere having become warmer, the inferior colder: attended with a decrease of the superficial evaporation. It begins to form some hours after sun-rise, because the vapour requires that space of time to become elevated by the gradual accession from below. When a stratus covers the ground at sun-rise, however, we often see it collect into cumuli upon the evaporation of that part of it which is immediately contiguous to the earth: and this effect ought to happen; for the cloud is then insulated, the *vapour plane* is established, and every thing in the same state, except in point of elevation, as in the ordinary mode of production of the cumulus.

Lastly, the cumulus, however dense it becomes, does not afford rain, because it consists of drops *similarly* electrified and repelling each other, and is moreover continually evaporating from the plane of its base. The change of form which comes on before it falls in rain, and which indicates a disturbance of its electrical state, will be noticed hereafter.

[To be continued.]

LXV. *Account of a Spaniard who can endure, without being incommoded, the greatest Degrees of Heat.* By J. C. DELAMETHERIE*.

A NATIVE of Toledo, in Spain, about 23 years of age, who arrived lately at Paris, has made different experiments to show that he is capable of enduring the greatest degrees of heat without being incommoded. We shall here give an extract of those made at the school of medicine before several of the professors, about three hundred of the pupils, and several other persons.

Care was taken to examine him before, and it was found that his state exhibited nothing different from that of a man in good health. His pulse beat about 75 or 78 times a minute.

EXP. I. A vessel containing oil heated to 85° of Réaumur being prepared, he opened his hand and applied the palm of it several times to the oil; he then washed his hands and face in the oil, and applied the soles of his feet to it. At the end of the experiment the heat of the oil was still from 76 to 78 degrees.

EXP. II. A bar of iron from 18 to 20 inches in length

* From the *Journal de Physique*, Messidor, an. 11.

and $2\frac{1}{2}$ inches in breadth was brought to a cherry red heat at one of its extremities, and placed on bricks. The Spaniard placed the sole of one of his feet on the red part: a portion of the oil which still adhered to it immediately inflamed. He applied the sole of the other foot in the like manner, and this he repeated several times.

EXP. III. The flat part of a large iron spatula 18 inches in length was brought to a cherry red heat. The Spaniard thrust out his tongue, applied it to the red part of the spatula, and repeated the same thing several times. Three glasses of pure water were then brought, into one of which a few drops of sulphuric acid were put, and into another a pretty large quantity of marine salt: the third contained only water. The Spaniard was made to drink these three glassfulls, and was able to distinguish perfectly the savour of them.

EXP. IV. He took a lighted candle, and drew the flame of it several times over the posterior part of his leg from the heel to the ham. He was examined after these trials, and no part of his skin appeared to be in the least altered. The sole of his foot seemed to be smokey—which ought to be ascribed to the carbon of the oil—but his pulse beat from 130 to 140 times in a minute. It appears that since that time he placed himself in an oven heated to 70 degrees, and *remained in it some minutes.*

Dr. Blagden, a fellow of the Royal Society of London, supported a still greater degree of heat. He heated an apartment till Fahrenheit's thermometer rose to 260 degrees, entered it with his clothes on, and remained in it 8 minutes. At last he was much oppressed. Several other persons entered it also. His pulse when he quitted the apartment beat 144 times in a minute.

In another experiment he entered, undressed, into the same apartment heated to 220 degrees of Fahrenheit, and remained in it 12 minutes without being incommoded.

In a third experiment, the chamber being heated to 250 degrees of Fahrenheit, he entered it along with several other persons, and remained in it several minutes without being incommoded.

Some eggs and a beefsteak were placed in the same apartment on a pewter dish; in twenty minutes the eggs were entirely hard, and in forty-seven the beefsteak was not only baked but almost dry.

LXVI. *Letter from Captain BAUDIN, now employed on a Voyage of Discovery, to C. JUSSIEU*.*

On board *Le Geographe*, Port Jackson,
New Holland, Nov. 11, 1802.

THE return of the *Naturaliste* to France, under the command of captain Hamelin, will enable you to judge how well our time has been employed in what relates to natural history. I have entrusted him with the care of conveying to their destination all the articles we have hitherto collected, being persuaded that he will discharge this duty with that zeal and attention of which he has given me so many proofs. I recommend him to you on that score.

By my letter to the minister of the marine, containing several extracts of my Journal, you will see that for two years I have done every thing in my power to increase our collections of every kind.

The premature death of C. Riedlé and Maugé, whose memory I must ever respect, has reduced me to the necessity of undertaking myself the departments of both these gentlemen, in which they acquitted themselves with a zeal I can never hope to equal.

I shall not entertain you at present with an account of all the events which have occurred since our departure; I shall only observe, that I never performed a voyage attended with so many hardships. More than once has my health suffered; but if I shall be so fortunate as to terminate this expedition agreeably to the intentions of government, and the expectations of the French nation, I shall have little left to wish for, and my difficulties will soon be forgotten. I have the greater hopes of succeeding, as Lewin's Land, with those of Concord and De Witt, Entrecasteaux's Channel, the island of Maria and its environs, the eastern coast of the large island of Diemen, Basse's and Banks's straits, and the whole south-west coast of New Holland from Cape Wilson to the islands of St. Peter and St. Francis, have been explored in a manner sufficient to ensure the safety of navigation. Much, however, remains to be done for the topography of the country, which no doubt will be long unknown, on account of the natural difficulties presented by the extent of coast which we have explored.

To supply the place of *Le Naturaliste* I resolved to purchase a small vessel of thirty tons, which I have named the *Casuarina*, because the greater part of it is constructed of

* From *Annales du Muséum d'Histoire Naturelle*, No. 10.

wood distinguished by that appellation. This small vessel will in future accompany me, and will be of the greatest utility. Had I been in possession of it sooner, some places to which I could not penetrate would not have remained unexplored; as it draws little water, it can any where approach the coast.

Another consideration no less important, which made me resolve to send home the *Naturaliste* was, the embarrassment occasioned by transporting our collections, which the casualties of the sea and the length of the voyage might have rendered fruitless to government and to the sciences, had I subjected them to the new risks to which we are about to be exposed: as the number is very considerable, and as they are of some value, I am convinced that government will approve of my conduct.

Many of the great number of birds which I have sent to you, and which I received from the inhabitants of Port Jackson, are in a bad state: they will not give you a very high idea of their skill in the art of preparing them, but you will doubtless be indemnified for this loss by those which we prepared ourselves. The quadrupeds, insects, living plants, hortus siccus, seeds, shells, madrepores, &c. are in the best state; and I have no doubt that these articles will be delivered to you in the same condition by the care of captain Hamelin.

If the live plants reach the place of their destination, you will have the most curious and the most beautiful productions of the country, and you will regret that you did not botanise on the soil which gave them birth. The whole country, at the time I am now writing, is covered with the most beautiful flowers. For variety I know no place but the Cape of Good Hope which can be compared to it. Though the greater part of our live plants were collected between the 33d and 42d degree of south latitude, I think it proper to observe, that I much fear whether they can be naturalised in France so speedily as you may wish. The temperature of Van Diemen's Land is not so cold as the latitude where it is situated seems to indicate. That of New Holland is still less so. At the commencement of winter, when we were to the south of Van Diemen's Land, the thermometer was only once at five degrees. At that time a strong south-west wind prevailed, accompanied with hail. At Port Jackson, in the middle of winter, we had it during one night very near zero; in the day time it generally kept at 6 or 8 degrees, and in the night between 4 and 5, and it rarely fell to 3. It appears to me, therefore, that an
orangerie

orangerie will suit them best during our winter in France, which is much more severe, and cannot be compared with that which we have experienced. Here the orange- and lemon-trees stand in the open air. They have thriven exceedingly well, and produce as fine fruit as in Portugal.

The seeds which I send you were in part given to me by the natives of the country; others I collected myself in the interior parts. I have proceeded beyond the most distant parts known to the English; but an almost impenetrable chain of mountains of the first order, known under the name of the *Blue Mountains*, the direction of which, inclining southward, seems to extend as far as Cape Wilson, and which, towards the north, terminates at Port Stephens, did not allow me to go further than 75 or 80 miles, reckoning from Port Jackson. If credit can be given to what is stated by the natives and some English adventurers, a large river of brackish water traverses these mountains, at the extremity of which there is a settlement of white men. (This is the name given by the natives to the Europeans.) Since my return I have often conversed on this subject with governor King, whose conduct I cannot sufficiently praise; but he declared to me that he placed no belief in these reports, which were invented by some deserters, who would never have returned had they met with a settlement of Europeans beyond these mountains.

On leaving Port Jackson, I intend to proceed through Basse's Straits in order to explore an island of considerable extent lately discovered by some English fishermen, and which has been called King's Island. When I have terminated the geographical part of my labour, I shall proceed to Kangaroo Island, on the south-west coast of New Holland, the southern part of which neither I nor captain Flinders was able to examine. I shall then direct my course to the islands of St. Peter and St. Francis, to examine them again, and to ascertain the direction of the continent in that part with which I am unacquainted. Proceeding then from the point where general d'Entrecasteaux stopped, and which we have already seen, I shall proceed directly to Lewin's Land, to finish the survey of the large bay distinguished by the name of the Geographe. As it appeared to me of importance to determine the position of Rosemary Isles discovered by Dampier, and which I have already sought for in vain by the longitude and latitude assigned to them in our charts, I shall make a new attempt to discover them, that I may then proceed to De Witt's Land, the chart of which is not sufficiently correct to en-
sure

sure the safety of navigation. The reasons which prevented me from undertaking this labour the first time I sailed along that coast, are known to you by the letter which I addressed to the minister of the marine a short time before I left Timor. The northern coast of New Holland and the Gulf of Carpentaria will terminate our labours; but I am afraid that so extensive an undertaking will require more time than the provisions we have laid in here will admit.

I have seen not without admiration the immense establishments formed here by the English during the twelve years they have been settled at Port Jackson. Though they began with great means, and expended a great deal of money, it is still difficult to conceive how they could so soon attain to that state of opulence and splendour in which they are at present. Nature indeed has done every thing for them in the beauty and safety of the port where their establishment is situated; but the quality of the surrounding land obliged them to penetrate to the interior of the country, till they found a soil proper for the different kinds of cultivation, which produce an abundant supply for their subsistence, and for the consumption of the European vessels which visit that coast for the whale fishery and for other purposes. Besides brigs, sloops, and middle-sized vessels, built in the new colony, and belonging to different individuals, we found on our arrival in this port nine large vessels from England and two Americans. Some of them are to return by China; the rest will be employed in fishing for spermaceti whales. The advantage arising from this kind of speculation will add a considerable increase to the British shipping, if the fishery continues to be successful. It is carried on in general on the coasts and in the environs of New Zealand.

The present population of Port Jackson, and other places occupied by the English, amounts to 6000 persons, employed chiefly in agriculture. All the fruit-trees of Europe have become naturalized, but they have not all been attended with equal success. Among this number are the apple-, cherry-, and almond-tree. All kinds of pulse, without exception, thrive, are well tasted, and abundant in the proper season. The vine, which during the first years afforded great hopes, has degenerated so much that it is doubted whether it will maintain itself. The cause of this unexpected degeneration is not well known: it is, however, ascribed to the scorching drought of the north-east wind, the effects of which are pernicious.

The natives settled in the neighbourhood of Port Jackson have retired to the interior part of the country in proportion

tion as the English have penetrated into it. They are, however, often met with in the town and villages, and on the highways, but never in considerable numbers. They have lost very little of their primitive habits; it is only remarked that they have made more progress in the English language than the English have done in theirs. They are of no use, and little to be feared. I am strongly inclined to believe that they are of a different origin from those of Van Diemen's Land.

As the English government has omitted nothing that could tend to the prosperity of the establishment, it has not suffered in its infancy. A stock of cows, sheep, and goats, was sent hither at its expense; and these animals have multiplied so much, that at the enumeration made of them in the month of August last, there were reckoned to be 800 bulls, 3600 cows, 6000 sheep, 1800 goats, and more than 10,000 hogs. The horses brought from the Cape of Good Hope and Bengal are of all the quadrupeds those which have thriven the least, though the cause is still unknown.

I shall not enlarge further on these details in this letter, because I have sent you a copy of that which I have addressed to the minister of the marine. You will find there a particular account of the articles sent home in *Le Naturaliste*, and which you are authorized to receive.

I beg to be remembered to you, and shall use my utmost endeavours to complete a new collection as numerous as that sent to you by *Le Naturaliste*.

LXVII. *Statement of the Improvement and Progress of the Breed of fine-woolled Sheep in New South Wales; presented by Captain M^cARTHUR at the Right Honourable Lord HOBART'S Office, 26th July, 1803.*

THE samples of wool brought from New South Wales having excited the particular attention of the merchants and principal English manufacturers, captain M^cArthur considers it his duty respectfully to represent to his majesty's ministers, that he has found, from an experience of many years, the climate of New South Wales is peculiarly adapted to the increase of fine-woolled sheep; and that, from the unlimited extent of luxuriant pastures with which that country abounds, millions of those valuable animals may be raised in a few years, with but little other expense than the hire of a few shepherds.

The

The specimens of wool that captain M^cArthur has with him, have been inspected by the best judges of wool in this kingdom; and they are of opinion that it possesses a softness superior to many of the wools of Spain, and that it certainly is equal in every valuable property to the very best that is to be obtained from thence.

The sheep producing this fine wool are of the Spanish kind, sent originally from Holland to the Cape of Good Hope, and taken from thence to Port Jackson.

Captain M^cArthur, being persuaded that the propagation of those animals would be of the utmost consequence to this country, procured in 1797 three rams and five ewes; and he has since had the satisfaction to see them rapidly increase, their fleeces augment in weight, and the wool very visibly improve in quality. When captain M^cArthur left Port Jackson in 1801, the heaviest fleece that had then been shorn weighed only three pounds and a half: but he has received reports of 1802, from which he learns that the fleeces of his sheep were increased to five pounds each, and that the wool is finer and softer than the wool of the preceding year. The fleece of one of the sheep originally imported from the Cape of Good Hope has been valued here at four shillings and sixpence per pound, and a fleece of the same kind bred in New South Wales is estimated at six shillings per pound.

Being once in possession of this valuable breed, and having ascertained that they improved in that climate, he became anxious to extend them as much as possible; he therefore crossed all the mixed bred ewes, of which his flocks were composed, with Spanish rams. The lambs produced from this cross were much improved; but, when they were again crossed, the change far exceeded his most sanguine expectations. In four crosses, he is of opinion, no distinction will be perceptible between the pure and the mixed breed. As a proof of the extraordinary and rapid improvement of his flocks, captain M^cArthur has exhibited the fleece of a coarse-woolled ewe that has been valued at ninepence a pound; and the fleece of her lamb, begotten by a Spanish ram, which is allowed to be worth three shillings a pound.

Captain M^cArthur has now about four thousand sheep, amongst which there are no rams but of the Spanish breed. He calculates that they will, with proper care, double themselves every two years and a half; and that in twenty years they will be so increased as to produce as much fine wool as is now imported from Spain and other countries at an
annual

annual expense of 1,800,000 l. sterling. To make the principle perfectly plain upon which captain M^cArthur founds this expectation, he begs to state, that half his flock has been raised from thirty ewes, purchased in 1793 out of a ship from India, and from about eight or ten Spanish and Irish sheep purchased since. The other half of his flock were obtained in 1801, by purchases from an officer who had raised them in the same time, and from about the same number of ewes that captain M^cArthur commenced with. This statement proves that the sheep have hitherto multiplied more rapidly than it is calculated they will do in future; but this is attributed to the first ewes being of a more prolific kind than the Spanish sheep are found to be; for, since captain M^cArthur has directed his attention to that breed, he has observed the ewes do not so often produce double lambs.

As a further confirmation of the principle of increase that captain M^cArthur has endeavoured to establish, and which he is positive time will prove to be correct, he would refer to the general returns transmitted from New South Wales. In 1796 (since when not one hundred sheep have been imported) 1531 were returned as the public and private stock of the colony. In 1801, 6757 were returned; and although between those periods all the males have been killed as soon as they became fit, yet there is a surplus over the calculation of 633.

Captain M^cArthur is so convinced of the practicability of supplying this country with any quantity of fine wool it may require, that he is earnestly solicitous to prosecute this, as it appears to him an important object; and on his return to New South Wales to devote his whole attention to accelerate its complete attainment. All the risk attendant on the undertaking he will cheerfully bear: he will require no pecuniary aid: and all the encouragement he humbly solicits for is the protection of government, permission to occupy a sufficient tract of unoccupied lands to feed his flocks, and the indulgence of selecting from amongst the convicts such men for shepherds as may, from their previous occupations, know something of the business.

London,
July 26, 1803.

(Signed) JOHN M^cARTHUR.

LXVIII. *Proceedings of Learned and other Societies.*

SOCIETY OF THE FRIENDS OF THE SCIENCES AT WARSAW.

IN their public sitting of May 5, this Society proposed the following

Prize Questions :

I. The formation of saltpetre was formerly much more considerable than at present in several provinces of the adjacent Poland; so that it formed an important article of commerce. The Society, therefore, requires an answer to the following question:—1st, Are there in these provinces any kinds of earth from which saltpetre can be obtained by lixiviation without any further preparation; and where are these earths found? 2d, The Society requires a chemical examination of the mould or upper strata of earth in those parts of the Ukraine and Podolia which formerly produced, or still produce, the greatest quantity of saltpetre, in regard to the formation of this salt at the place of observation. 3d, A comparison of the Polish saltpetre, as sold in commerce, in regard to purity, with that manufactured in other countries, and particularly that of Bengal. 4th, An accurate description of the process by which saltpetre is obtained and refined in the different manufactories of the Ukraine and Podolia. 5th, Whether the new chemistry affords any means of improving these processes, so that saltpetre may be obtained at a cheaper rate and in a greater state of purity. 6th, A comparison of the expenses of manufacturing saltpetre according to the new French, the German, and the Polish method. 7th, A comparison of the East India saltpetre with the saltpetre made in the different manufactories of Europe, both before the year 1790 and at the present time. 8th, Plan for carrying on trade with this production in the most advantageous manner, so as to make it pay for the expenses of manufacturing it.

II. It is well known that a considerable and advantageous trade was formerly carried on, and particularly at Venice, with the Polish cochineal or *ezerwice* (*coccus Polonicus tinctorius*). The Society therefore requires, 1st, A complete natural history of *ezerwice*, with an account of the difference between the *real* Polish cochineal and the *false*, which is so often sold for the real *ezerwice*; and how this

deception can be detected. 2d, What is the essential difference between the American cochineal and the Polish *czerwiec*? 3d, For what reasons has the American cochineal been preferred in modern times to the Polish? and by what means can *czerwiec* be again introduced into commerce? 4th, Can barren sandy land, useful for no other æconomical purpose, be employed in the regular cultivation of *czerwiec*? 5th, In what manner can *czerwiec* be raised at the least expense, and in the greatest abundance? 7th, Whether *czerwiec* possesses any medicinal properties?

III. As the plague formerly occasioned great devastation in Poland, the Society requires, 1st, A complete history of the plague in Poland, from the most authentic sources. The author must examine, 2d, Whether this epidemia did not sometimes originate in Poland, or whether it was brought to these provinces from foreign countries? 3d, In both these cases, what were the regular symptoms by which it was known; and could the place where the disease chiefly originated be determined from these symptoms? 4th, Whether the disease by passing from one province to another became more destructive; or whether its malignant nature was thereby moderated? 5th, What means of preventing the infection were employed at different periods in this country with success; and what mode of curing the disease has been found most effectual? 6th, Whether the total extirpation of the plague be possible; and in what manner this is to be accomplished?

The papers, written in the Polish, Latin, German, or French languages, must be transmitted, post paid, to the secretary of the Society of the Friends of the Sciences, before the end of August 1804.

The prize for the best essay on each of these subjects is a gold medal of the value of forty ducats.

One of these prizes only is paid from the funds of the Society. The other two are proposed by two worthy members, actuated by a laudable zeal for diffusing useful knowledge.

ACADEMY OF SCIENCES AT BERLIN.

The following papers were read in this society during the first half of the year 1803:

Jan. 6th. A dissertation on the nature, causes, and cure of mania; by Dr. Hufeland.

Jan. 13th. Professor Bode read an extract from his Journal of Astronomical Observations; with a description of a new sextant by M. Klengert, of Breslau.

Jan.

Jan. 20th. M. Ancillon : continuation and conclusion of his Philosophical and Moral Thoughts.

Jan. 27th, being a public sitting, M. Merian delivered an introductory oration. M. du Verdy read a sketch of a plan of equestrian exercises in imitation of the tournaments of the antient chivalry. M. Erman : Anecdotes of the reign of the elector Frederic William the Great, from a manuscript journal of Th. Sig. de Buch, *maréchal du voyage* of the great elector : first memoir. M. Klaproth : On meteoric stones and metallic masses. M. Klein : On diseases of the mind and mental debility in a juridical point of view.

Feb. 3d. M. Erman read a dissertation of baron de Chambrier, ambassador at the court of Sardinia, on Casimir, margrave of Brandenburg-Bareith : second memoir.

Feb. 10th. Dissertation on propping fruit-trees, with observations by M. Treff.

Feb. 17th. Professor Burja : On the relation which exists between music and declamation : first memoir.

Feb. 24th. M. Klein : A continuation of his dissertation on diseases of the mind and mental debility.

March 3d. M. Trembley : Memoir on the philosophy of the poets.

March 10th. M. Klaproth : Continuation of his treatise on meteoric stones and metallic masses.

March 17th. M. Bernoulli : On the atmospheres of the celestial bodies, by M. D. Melanderhjelm, perpetual secretary of the Academy of Sciences at Stockholm; and a memoir, transmitted by M. La Grange, entitled Researches on several Points of Analysis relating to preceding Memoirs.

March 24th. M. Teller : A psychological dissertation on imitation in style.

March 31st. M. Denina : A continuation of observations on the common origin of the Slavonic, Latin, Celtic, and German languages.

April 21st. M. Gerhard : Observations and conjectures on simple earths. Part i.

April 28th. M. Trembley : On the methods of approximation : second memoir.

May 5th. M. Biester delivered an oration on character.

May 12th. M. Erman : Anecdotes of the reign of the elector Frederic William the Great : second memoir.

June 9th. Professor Fischer : On measuring heights by the barometer.

June 16th. M. Bastide : On the *alopcurus pratensis*. Professor Fischer : A continuation of his dissertation.

June 23d. M. Bastide: A dissertation of professor Buttmann on the philosophical meaning of the Grecian deities, and particularly Apollo and Diana.

June 30th. M. Bastide: A new commentary on *Montaigne*.

The Physical Class has proposed the following questions as the subject of prizes for the year 1805:

I. Is Mariott's law a general law for all elastic fluids, or only for atmospheric air?

II. What is the nature of that disease called inflammation of the spleen, which prevails so much among cattle? whence does it arise? and what is the method of cure?

The prize for each of the above questions is fifty ducats.

III. As the lungs consist of a cartilaginous air-pipe and cellular tissue, to which lymphatic vessels, bronchial arteries, veins and nerves, proceed—and as the pulmonary artery and veins convey the whole mass of the blood through the lungs, answers to the following queries are required:—In what manner, and where, does the cartilaginous air-pipe terminate? Does it proceed into the cellular tissue of the lungs, or has it determinate boundaries? Does it remain cartilaginous in its minutest divisions, and, as such, does it terminate in the surrounding cellular tissue? Do the bronchial vessels belong merely to this cartilaginous pipe, or to the cellular tissue and the lungs also?—that is to say, Do the bronchial vessels convey nourishment to the air-pipe alone, or to the cellular tissue also? Where does the pulmonary artery of the lungs end? Does it convey the blood by help of the cellular tissue merely through the lungs, and transmit it immediately to the veins of the lungs, or does it expire a fluid into the cellular tissue of the lungs, which in breathing flows out through the wind-pipe?—or, Does the pulmonary artery separate a moisture from the exterior surface of the lungs? Where do the pulmonary veins arise? Do they arise alone from the arteries, or, as absorbing vessels, do they take their origin in part also from the bronchia, the cellular tissue of the lungs, and from the exterior surface of the lungs? How do the nerves of the eight pair and the intercostal nerves terminate? Do those of the eight pair terminate alone in the bronchia, or do they run into the cellular tissue of the lungs? Are the eight pair connected with the branches which the intercostal nerve sends out to the finest vessels in the lungs? The prize is a golden medal of the value of 50 ducats, or 50 ducats in money. The answers must be grounded on microscopical observations.

LXIX. *Intelligence and Miscellaneous Articles.*

AËROSTATION.

Hamburgh, Aug. 12.

PROFESSOR ROBERTSON yesterday made his second ascent, which, like the former, was exceedingly brilliant. At a quarter past twelve he stepped into the car, with his friend M. Lhoert, carrying with him barometers, thermometers, a speaking trumpet, &c. At 35 minutes after twelve, the balloon being at the height of 600 toises, M. Robertson launched a parachute, which fell very slowly. At three quarters after twelve the balloon was at the height of about 1200 toises. During this ascent Mr. Robertson made several new experiments, which had been suggested to him by some of our philosophical men.

Aug. 13.

Professor Robertson and his friend returned hither at four in the afternoon. Their second aërial voyage has been as successful as the first. As the wind carried them towards the sea, and the sky was so obscured by thick clouds that they could not see to a great distance, they did not rise to so great a height as the first time. The aëronauts descended near the village of Rehout, in Holstein, having traversed about eight miles in the course of an hour. Mr. Robertson, during this voyage, made some new experiments, an account of which he has promised to publish.

Aug. 14.

By observations made during his last ascent, Mr. Robertson has found that barometric calculation does not show with precision the real heights in the atmosphere. He weighed different bodies by means of a spring balance, and found a great difference between their gravities in the elevated regions of the atmosphere compared with those at the surface of the earth. He ascertained that the magnetic virtue decreases as the square of the distances. He observed that sounds may be conveyed upwards to the height of 200 toises, while downwards they can be conveyed only half that distance. The solar rays collected in the focus of a lens lose one-third of their intensity. Mr. Robertson is preparing to continue his tour to Petersburg.

Aug. 15.

It was 42 minutes past twelve when Mr. Robertson descended his balloon from the earth when he made his last ascent. The barometer being at 21 inches 12 lines, and the thermometer

thermometer at 21 degrees, he launched two parachutes of different sizes, and loaded with equal weights, in order to calculate the resistance of the air. The second, which was launched a hundred toises higher than the first, fell with much greater velocity, but did not unfold itself till it had passed over a double space. At 51 minutes after twelve he passed between two large clouds, which seemed to open to afford a passage to the balloon. The form of these masses of vapour is oblong. They resemble rags suspended above the earth. Their upper parts do not form in their aggregate a smooth surface, as appears to those who look at them from the earth; on the contrary, they resemble long pyramids. This effect ought to be ascribed to caloric, which, if we may use the expression, converts these masses into Montgolfiers, the elevation of which is proportioned to the density of the atmosphere. They appeared to Mr. Robertson to plunge towards the earth, in consequence of an optic effect resulting from the apparent immobility of the balloon, which, however, was rising at the rate of 50 feet per second. When the thermometer indicated one degree above freezing, and the barometer stood at 15 inches, Mr. Robertson set at liberty two pigeons, which descended with the rapidity of lightning, without moving their wings, and in a plane slightly inclined. When the barometer stood at 14 inches he let off a third pigeon, which, having fluttered about for a moment with difficulty, perched on the net-work, and would not quit it. Two butterflies let go at the same time tried to use their wings, but in vain, as the air was too rare: they never quitted the car, and fluttered, but in a very feeble manner. Tinder exposed to a convex glass of six inches focus did not catch fire till the end of some minutes. The rays refracted by the prism no longer exhibited lively and distinct, but weak and confused, colours. Weights attached to a spring balance had lost one half of their gravity. The magnetic needle began again to put itself in motion. By means of a very ingenious instrument invented by M. Hez, mechanician, he inclosed four inches of the surrounding air along with mercury, and marked exactly the point where the air and the mercury were united. When he reached the earth, he found that the latter filled the whole tube within a tenth. This important experiment seems to prove, that in the upper regions there exists nothing but vapours, and no atmospheric air. If this conjecture of Mr. Robertson be verified, there will be no reason why people may not ascend to a much greater height than that of 3670 toises, to which we know some have ascended: but for this purpose

a balloon of 40 or 50 feet in diameter would be necessary; otherwise the loss of gas experienced by the balloon might make it descend with a velocity which would endanger the lives of the aëronauts. Mr. Robertson experienced this during his ascent before the last, when he was in danger of being killed.

ASTRONOMY.

The eclipse of the sun which took place on the morning of Aug. 17th was observed at Paris, the weather being very fine, by all the astronomers, Delambre, Messier, Bouvard, Lalande nephew, Burekhardt, and myself. I saw the commencement at 5^h 59^m 18^s, at the college of France; and M. Messier observed the end at 7^h 46^m 8^s. In consequence of the rule I have formed to calculate such observations, the same day I found the conjunction at 6^h 30^m 24^s true time, reduced to the meridian of the observatory. This will serve as a term of comparison for finding the longitude of all those countries where it may have been observed. It was annular in Egypt.

DELANDE.

DREADFUL ACCIDENT.

On Thursday the 8th of September a steam engine employed to assist in clearing the works from water at the tide-mills now erecting in the marsh between Greenwich and Woolwich, was blown up by the force of the contained steam. The explosion was as sudden and dreadful as that of a powder-mill, and was accompanied with a similar noise, which was heard at a great distance from the place. The engine was on Mr. Trevethick's plan, worked by the expansive force of steam only, without employing condensation as in the engines in common use. It was literally blown to pieces; and we are sorry to state, that by the accident three people were killed on the spot, and three others, all that were there at the time, so much hurt that two of them are not expected to recover. It was a fortunate circumstance that the accident happened at a time when the other workmen were at dinner, or a much greater number might have lost their lives.

Steam-engines on Mr. Trevethick's plan require a boiler of immense strength; for they work with a power sometimes equal to 60 pounds on the square inch, while common engines, even Mr. Watts's, seldom work with more than 5 pounds. We are happy to state, however, that the present accident arose, not from the impossibility of making a boiler strong enough, but from a culpable mismanagement of a boy

boy appointed to attend the engine. Impatient to finish his work, he had put a piece of timber between the top of the safety valve and a beam above it, so that it could not rise to allow steam to escape when produced in greater quantity than required. He even went away to fish in the river. In the mean time the engine was stopped by another workman, who knew not what the boy had done, and in a short time the mischief we have stated followed. The boy had returned, and was in the very act of removing the piece of wood he had so imprudently put over the valve when the explosion took place. He was the least hurt of all who were near the spot.

This accident ought to serve as a warning to engineers to construct their safety valves in such a manner that common workmen cannot stop them at their pleasure; which may be easily done.

From the way in which part of the boiler was bent, which was constructed of cast iron nearly an inch in thickness, it is thought the steam must have acquired an expansive force equal to 500 pounds on the square inch before it gave way—a force much beyond any that can ever be required. But though this shows that engines on Mr. Trevethick's plan may, with proper precautions, be worked with as much safety as those on the common principle, such an accident as the one we have stated cannot fail to intimidate some people from adopting them. It is therefore with much pleasure we state that a boiler on a new construction, calculated to bear a much higher degree of expansive force than can ever in any case be required, has been lately invented by a very able engineer, Mr. Woolf. It consists of a combination of cylindrical tubes, which unite the double advantage of exposing a much larger surface to the action of the fire than the common boiler, while they possess a much greater degree of strength. This invention appears to us so important, that we shall take an early opportunity of laying a description of it before the public.

METHOD OF PREVENTING IRON AND STEEL FROM
RUSTING*.

C. Conté has found out a method of preventing the oxidation of iron and steel, or, to speak in language more generally understood, to prevent iron and steel from rusting. This method consists in mixing with fat oil varnish a half, at least, or rather four-fifths, of well rectified spirit of tur-

* From the *Magasin Encyclopédique*.

entine. This varnish is applied slightly, and in an equal manner, by means of a sponge; after which the article is suffered to dry in a place sheltered from dust. Articles varnished in this manner, it is said, will retain their metallic brilliancy, and never contract any spots of rust. This varnish may be applied also to copper, of which it preserves the polish, and brightens the colour. It may be employed in particular, with advantage, for preserving from alteration philosophical instruments, which, in the course of experiments, are brought into contact with water, and by those means are liable to lose their splendour, and to become tarnished.

A SHOWER OF MUD.

The *Journal de Physique* for Germinal contains a letter from De Fortis to the editor, in which he gives an account of a shower of mud which fell in the evening of the 27th of March near Udina. "The wind (says the author) had blown with violence from the east for three days. The extent of country which was abundantly besprinkled by this strange rain was twelve miles in diameter from the borders of the sea to the bottom of the Alps of Carnia. I do not know whether the partisans of the opinion which makes lava come to us from the moon, can derive any arguments in their favour from the mud which has covered the plains of Friouli; but, for my part, I first imagined that the wind, being charged in Sicily or near Naples with clouds of volcanic dust, had deposited them at the bottom of the Carnian mountains, which prevented the clouds from going further. But having then observed, through a very powerful magnifying glass, a specimen of the sediment in question which a friend sent me from Udina, I convinced myself that it had not the least resemblance to that *detritus* which is raised by volcanoes to the superior regions of the atmosphere. It appears to me more natural to suppose that a storm, or perhaps water-spouts at sea, having sucked up some of the muddy water which the rivers by their inundation leave on the plains, raised them to the upper regions, where they were carried away by the winds. It is in consequence of similar circumstances, very natural and common, that worms, tadpoles, and small fishes have often been seen to fall from the clouds with rain, without any person conceiving the idea of making them come from an aerial race or from another globe."

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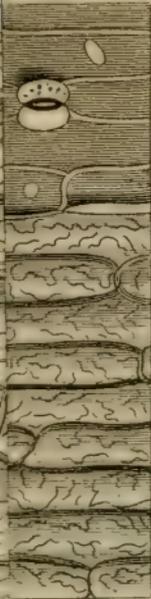
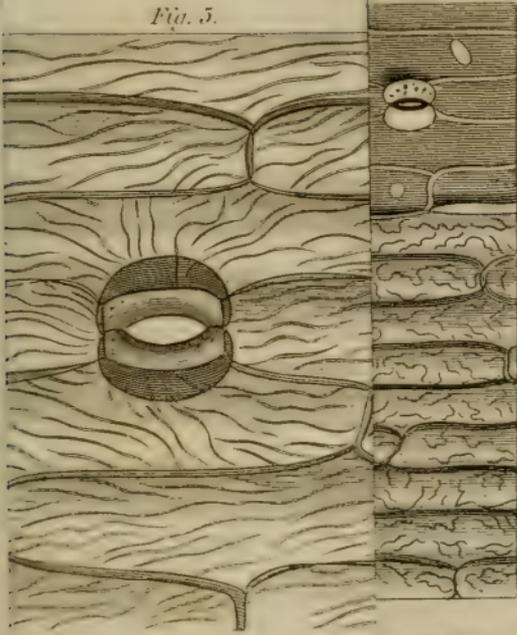
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Fig. 5.



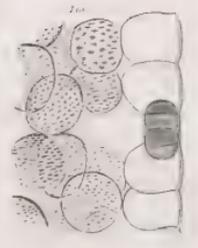


Fig. 2

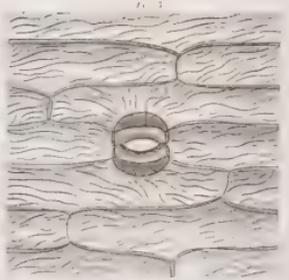


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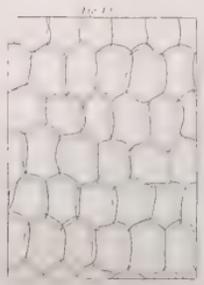


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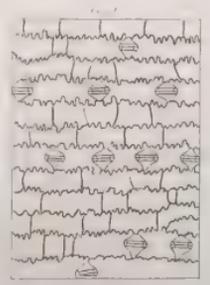


Fig. 6

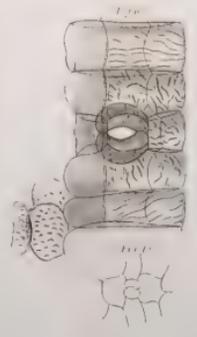


Fig. 7

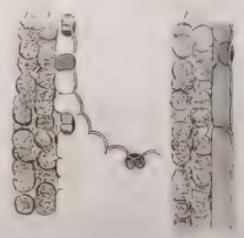
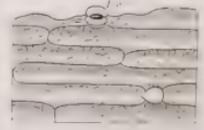
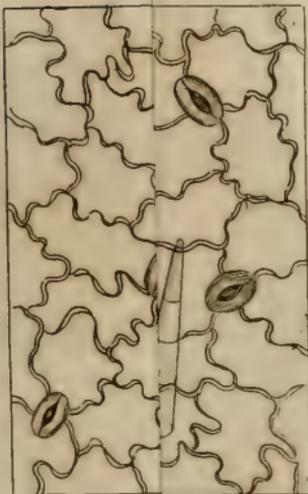


Fig. 9



Fig. 10

Pl. II. Vol. XVI.



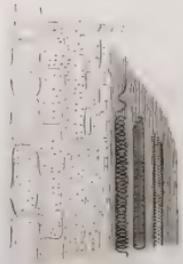
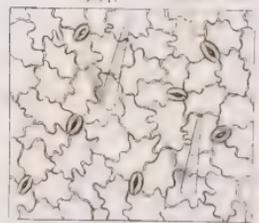
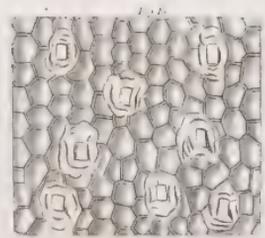
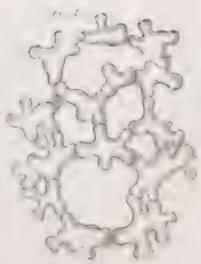


Fig. 2.

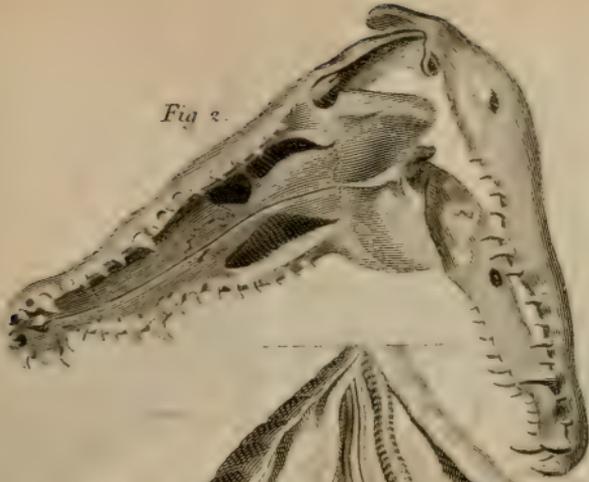
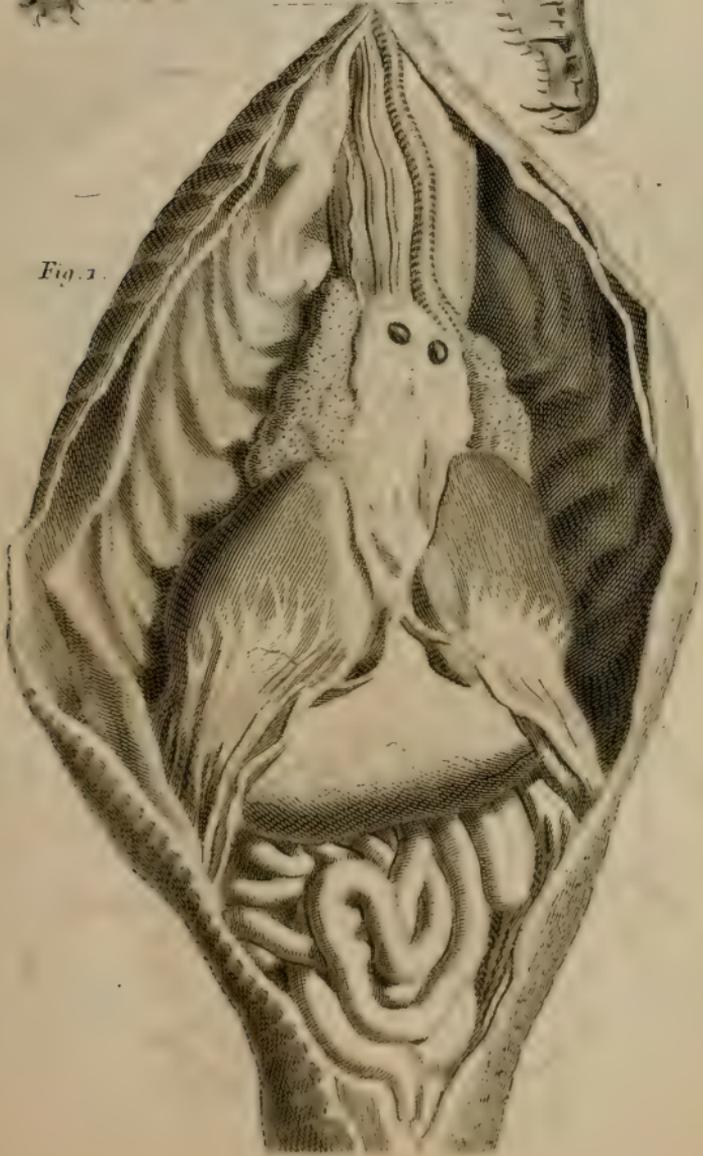
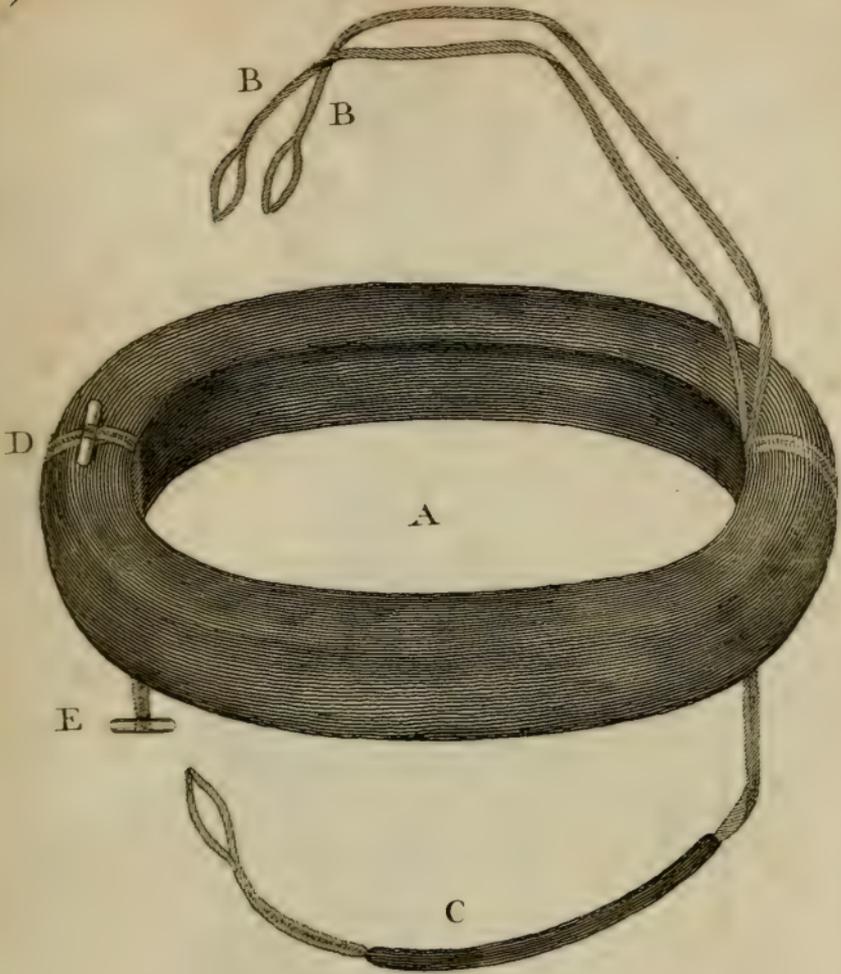


Fig. 1.



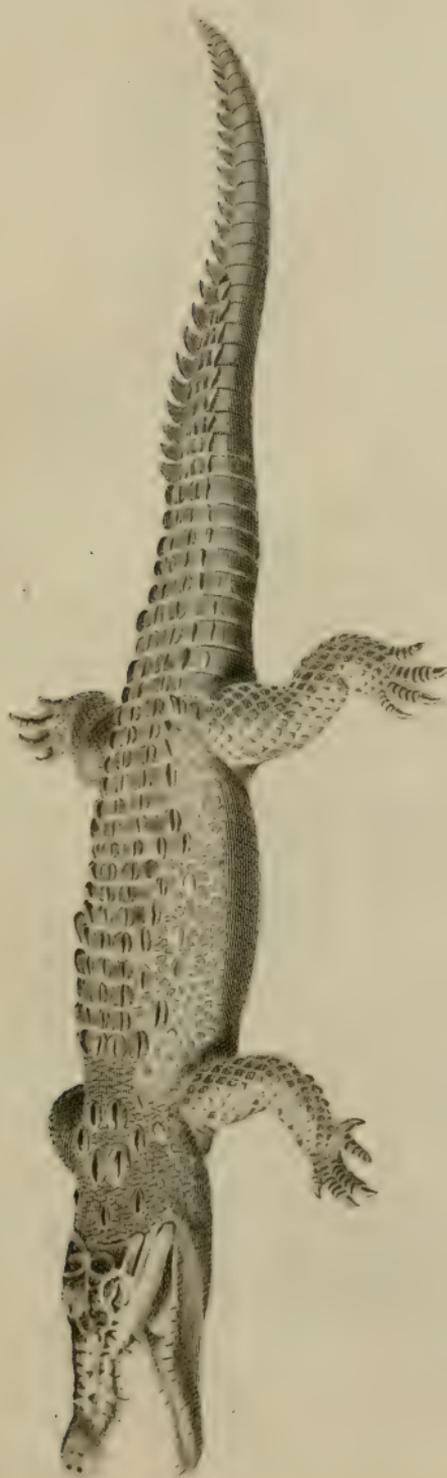




MARINE SPENCER.

*For the preservation of Lives.
in cases of Shipwreck or other
Accidents at Sea.*





Crocodile of S^t. Domingo.

