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Carnót.

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

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THE

PHILOSOPHICAL MAGAZINE.

I. *On the two Systems of Musical Temperament recommended by Earl Stanhope,—Mr. Hawkes's System, &c.* By Mr. John Farey.

To Mr. Tilloch.

SIR,

Having bestowed some pains to illustrate the System of Musical Temperament described by Earl Stanhope in your xxvth volume, as applicable to keyed Instruments, by the help of a *Monochord*, whose divisions are according to geometric *mean proportionals*, I beg now to present to your readers, the notes of the other System, described by his Lordship in the same Essay, to be effected by making three successive tempered *Fifths*, and two successive major *Thirds*, in different parts of the scale, *beat equally quick* respectively.

The table accompanying this, is divided into 10 columns, entitled at the bottom, as has usually been done.

Column 4 contains the number of complete vibrations made by a musical string or other sounding body in one second of time, when the intervals are agreeable to Earl Stanhope's *Monochord System*; whose logarithms, lengths of strings, and other particulars for comparison herewith, will be found vol. xxvii. p. 195 *, and vol. xxviii. p. 141.

* I beg here to correct an unfortunate error in the length of string which I have in this page assigned to Lord Stanhope's 6th, owing to my having taken out the number answering to the logarithm ·8100300 (instead of ·8010300) viz. ·6456987 instead of ·6324554; for which correction I wish to acknowledge my obligation to Mr. J. Barraud, a gentleman engaged in these inquiries, who has verified the numbers in this column, except in some of their last places, independent of the logarithms in the preceding column.

 Columns

Columns 5, 6, 7, 8, and 9, are intended to explain the *Equal-beating* System of his Lordship (see vol. xxv. pp. 301, 302; xxvii. p. 203; and xxviii. p. 150): the 5th contains the complete vibrations (which Earl S. would call *Beats*) made in one second of time; wherein 240 is assumed as the *pitch* of C on the Tenor Cliff line (a ledger line below the Treble, or the same above the Bass stave in music), on the authorities quoted in the article *Concert pitch*, in vol. ix. part I. of *Dr. Rees's New Cyclopædia*, lately published.

I am aware that Earl Stanhope, (vol. xxv. p. 303,) refers, in his Tuning Table, to the Octave below my C instead of above it, at least for tuning of some of his notes, but I have preferred this Octave, and added to my calculation, vol. xxvii. p. 203; extending the same to the *equally beating* THIRDS in this his Lordship's System.

If the Third E♭A in his Lordship's Table be tempered sharp 1·066 commas, and the Third ♭Ac, also sharp ·843 parts of a comma, both of these will *beat* 10·00 times per second; and his three successive Fifths GD, DA and Ae, if tempered flat ·4721, ·3163 and ·2116 parts of a comma respectively, will each be found to *beat* 3·158 times per second nearly. The half of the number of *Vibrations* in columns 4 or 5, or of *Beats* in this Octave, will answer to the first Bass Octave, and twice these numbers to the first Treble Octave respectively; and the half or double of these again, will express the next descending or ascending Octave respectively, and so on, throughout the whole scale.

Columns 6 and 7 contain the logarithms and lengths of strings in this System, for comparison with the notes in his Lordship's *Monochord* System, vol. xxvii. p. 195; as column 8 is intended, to compare with vol. xxviii. p. 141; in which column, I have preserved the terms f and m, the same, and thrown all the differences between this and the monochord system, into the term Σ, as the same are expressed in column 9: from whence it appears, that half the notes differ more than 1¼ *Schismas* from each other respectively, in these two Stanhopian Systems.

A Table

A Table shewing the Relations which the several Notes in an Octave, above the Tenor Cliff C, bear to the Key Note, when tuned according to the EQUAL-BEATING *System of Earl Stanhope; with the Notes of Mr. Hawkes's System, for Comparison therewith.*

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
12	c	VIII	480·00	480·00	·6989700	·5000000	612·0000 Σ + 12 f + 53 m		612 Σ + 12 f + 53 m
11	B	VII	450·00	450·00	·7269987	·5333333	555·0000 Σ + 11 f + 48 m		555 Σ + 11 f + 48 m
10	♭B	7th	426·67	426·67	·7501225	·5625	508·0000 Σ + 10 f + 44 m		512$\frac{2}{5}$ Σ + 10 f + 44$\frac{2}{5}$ m
9	A	VI	401·66	401·05	·7770106	·5984261	453·3248 Σ + 9 f + 39$\frac{1}{3}$ m	− 1·3419 × Σ	455$\frac{2}{5}$ Σ + 9 f + 39$\frac{2}{5}$ m
8	♭A	6th	379·47	380·02	·8004063	·6315480	405·7725 Σ + 8 f + 35 m	+ 1·2725 × Σ	400$\frac{3}{5}$ Σ + 8 f + 34$\frac{3}{5}$ m
7	G	V	360·00	360·00	·8239087	·6666666	358·0000 Σ + 7 f + 31 m		355$\frac{4}{5}$ Σ + 7 f + 30$\frac{4}{5}$ m
6	♭G	IV	337·31	337·79	·8515589	·7104915	301·7725 Σ + 6 f + 26 m	+ 1·2725 × Σ	298$\frac{4}{5}$ Σ + 6 f + 25$\frac{4}{5}$ m
5	F	4th	320·00	320·00	·8750613	·75	254·0000 Σ + 5 f + 22 m		256$\frac{1}{5}$ Σ + 5 f + 22$\frac{1}{5}$ m
4	E	III	300·00	300·00	·9030900	·8	197·0000 Σ + 4 f + 17 m		199$\frac{1}{5}$ Σ + 4 f + 17$\frac{1}{5}$ m
3	♭E	3d	284·60	285·01	·9253450	·8420638	151·7725 Σ + 3 f + 13 m	+ 1·2725 × Σ	154$\frac{2}{5}$ Σ + 3 f + 13$\frac{2}{5}$ m
2	D	II	268·88	268·42	·9513944	·8941170	98·9726 Σ + 2 f + 8$\frac{2}{3}$ m	− 1·5274 × Σ	99$\frac{3}{5}$ Σ + 2 f + 8$\frac{3}{5}$ m
1	♭D	2d	252·98	253·35	·9764976	·9473220	47·7725 Σ + f + 4 m	+ 1·2725 × Σ	42$\frac{3}{5}$ Σ + f + $\frac{3}{5}$ m
	C	Key	240·00	240·00	1·0000000	1·0000000			
Intervals in half notes.	Letters or finger-key.	Marks.	Vibrations in 1″ by Earl Stanhope's Monochord System.	Vibrations in 1″	Common Logarithms.	Lengths of Strings.	New Notation.	Differences from his Monochord System.	Mr. Hawkes's System, in New Notation.—See vol. xxvi. p. 173.
				The *Equal-Beating* System of Earl Stanhope, expressed in					

Since it appears, (vol. xxviii. p. 304,) that Mr. *William Hawkes* asserts the superiority of his System (vol. xxvi. p. 171,) over that of Earl Stanhope, as being "the best principle for tempering our present scale" with 12 notes in an octave, I have devoted a column of my present Table to his System, and whereby a comparison of the same may be made, with either of Earl Stanhope's Systems; and where the differences in *schismas* and *minutes*, between any of the respective notes in each, may be obtained by a very easy subtraction.

From such a comparison it will appear, that Earl Stanhope's notes some of them differ materially from Mr. Hawkes's corresponding ones, and that both of them differ much, comparatively, from the *Equal Temperament*, a mode of tuning, which Earl Stanhope says, (vol. xxv. p. 291,) was approved by one half of the most eminent musicians in England, whom his Lordship particularly consulted on the subject: although Mr. H. and his Lordship so cordially agree, (vol. xxviii. p. 305,) and (vol. xxv. p. 305,) in condemning the same.

In my last, I omitted to mention, respecting the *Isotonic* system in column 4, (vol. xxix. p. 347,) that if each note therein, all of which contain fractional parts of the very small interval m, be increased to the next *whole* number, as $48\frac{7}{12}$ to 49 m, $46\frac{1}{6}$ to 47 m, &c., these trifling alterations will reduce it to my Equal Temperament, mentioned vol. xxviii. p. 65, and the same can then be *practically tuned*, by the help of *perfect conchords* only! We may therefore now hope, that "Equal Temperament," which so many have commended, and others condemned, probably without having ever heard any music performed in it, will be brought to the test of experience, and practice sufficiently extensive, to get over the prejudice which will naturally arise, on the hearing of any new system whatever.

Many, I know, have imagined, that the system which was aimed at by the professional Tuners before Earl Stanhope wrote, was an Equal Temperament, to be effected by the judgment of the ear, and Mr. Hawkes thinks that their practical results agree very nearly with his system: both of these opinions I have much reason to doubt, and cannot

refrain

refrain from again endeavouring to call the attention of some gentleman, possessed of good Instruments and the necessary knowledge and experience in making experiments and calculations in harmonics, and requesting him to employ the best professional Tuners to tune his instruments, without any intimation to the Tuners, of his intentions or object; and before such instruments are put out of tune by use or standing, to ascertain exactly, by the *beats* of the different conchords, by a monochord; and by other methods also for further satisfaction, the exact value of every interval in an Octave, using single strings only: these experiments varied and repeated, on Organs and Piano-Fortes, tuned by as many good tuners as possible, would, by their results, enable us to say, how far any one system whatever has been adopted, or can be accomplished, by the method of tuning in use, and within what limits the different tuners, or the same persons at different times, do in practice fix each note. Such an one would doubtless perform the most valuable piece of service to the practical musician, and perhaps enable him to profit from the labours of theorists in harmonics, by enabling him with more certainty and facility, to accomplish the "tuning," with which by practice his auditors are become acquainted, and wherewith most of them are satisfied*, if the same did not lead to an amelioration of the system. With such information before them, the musical public would perhaps be enabled to judge, of the pretensions of the many musical quacks, who are almost every year bringing forth some new and fanciful system of temperament, (of which an almost inexhaustible fund yet lay behind,) and crying up the same with a confidence, equalled only by that with which rival empirics condemn them: until at length the *Science of Harmonics*, and the valuable discoveries of *Dr. Robert Smith* on the nature of *imperfect*

* Let it always be recollected, that performances on *perfect Instruments* or by voices, are free from defects *in harmony* if skill and good ears but direct them, and that the present inquiry is limited to the use of Instruments with 12 strings or pipes in an octave, where, or even with double that number of fixed sounds, temperaments, or errors in harmony are *impossible to be avoided.*

 consonances,

consonances, and of *Mr. Maxwell* on the system of *perfect consonancy*, are in danger of falling into utter contempt.

I beg here to mention, respecting the new notation for musical intervals, which I have explained vol. xxviii. p. 140, that the Octave, happening to contain just 12 of the *lesser fractions* f, and one of these to fall near each note of the equal temperament; in almost all calculations respecting *Douzeaves*, the temperaments or results, are free of f, and *two* only of the *three* independent or *prime* terms, of which every *accurate* notation must consist, are in general found at last; while the smallness of the most *minute*, m, it being less than the $\frac{1}{127}$th part of the *Schisma*, Σ, which is itself but a very trifle more than $\frac{1}{11}$th part of a *Comma*, c, (or $\frac{1}{11}\Sigma + \frac{1}{11}$ m) render it allowable in most practical cases to neglect m, and to consider the Σ s as *elevenths* of a comma, in the results; although I would advise the previous calculations to be always carried on strictly, in Σ, f and m, especially, as the number of f s will generally point out, to what finger-key or number of half notes, any step in the process answers.

I am, sir, your obedient servant,

John Farey.

12, Upper Crown-Street, Westminster,
February 1, 1808.

II. *Essay upon Machines in General. By M.* Carnot, *Member of the French Institute, &c. &c.**

Preface.

Although the theory to be discussed be applicable to every subject which concerns the communication of motion, I have given to this work the title of Essay upon Machines in General;—in the first place, because it is principally machines I purpose to treat of, as being the most important branch

* For a Translation of Carnot's "Reflections on the Theory of the Infinitesimal Calculus," see Phil. Mag. vol. viii. p. 222, and 335; and vol. ix. p. 39.

branch of mechanics; and in the second place, because I do not mean to treat of any machine in particular, but solely of the properties which are common to all.

This theory is founded upon three principal definitions: the first regards certain movements which I call *geometrical*, because they may be determined by the principle of geometry alone, and are absolutely independent of the rules of dynamics. I have not thought that we could easily pass over them without leaving some obscurity in the elucidation of the principal propositions, as I have particularly shown with respect to the principle of Descartes.

By the second of my definitions, I endeavour to fix the signification of the terms *force soliciting* and *force resisting*: we cannot, in my opinion, perspicuously compare causes with effects in machinery without a marked distinction between these different forces; and this is the distinction upon which I think something vague and indeterminate has been always left.

Lastly, my third definition is that by which I give the name of *moment of activity* of a power, to a quantity in which a power is mentioned which is really in activity or in movement, and where we also take account of each of the instants employed by this force, *i. e.* of the time during which it acts. Whatever it be, we cannot refuse to allow that this quantity, under whatever denomination we designate it, is not to be continually met with in the analysis of machines in movement.

With the assistance of these definitions, I arrive at propositions which are very simple: I deduce all of them from one same fundamental equation, which, containing a certain indeterminate quantity, to which we may attribute different arbitrary values, will give successively in each particular case, all the determinate equations required for the solution of the problem.

This equation, which possesses the greatest simplicity, generally extends to all imaginable cases of equilibrium and movement, whether the movement changes hastily, or varies by insensible degrees: it is even applied to all bodies, whether hard, or endowed with a certain degree of elasticity;

and

and if I am not deceived, it is sufficient of itself, and independently of every other mechanical principle, to resolve all the particular cases to be met with.

I easily draw from this equation a general principle of equilibrium and movement in machines properly so called, and from the latter naturally flow other principles more or less general, several of which are already known and very celebrated, but which have been hitherto either inexactly or vaguely explained, rather than rigorously demonstrated.

Without departing from general principles, I have united in a scholium, and as clearly as possible, the most useful remarks for practice, and which, from their importance, appeared to me to merit a particular development. Every person repeats, that in machines in movement, we always lose in time or in velocity what we gain in power; but after perusing the best elements of mechanics, which seem to be the true place where the proofs and explanation of this principle should be found,—Is its extent or even its true signification easy to seize? Has its generality, with most readers, that irresistible evidence which should characterize mathematical truths? If they exhibit this striking conviction, ought we not to see mechanics instructed in these works, incessantly renounce their chimerical projects? Would they not cease to believe, in spite of every thing that has been taught them, that there is something of magic in machines? The proofs given them of the contrary only extend to simple machines: now they do not think these capable of any great effect, and they cannot be brought to believe that it must be the same in every case imaginable; they only speak of that where there are solely two forces in the system, and they are contented with an analogy: this is the reason why these mechanicians always hope that their sagacity will make them discover some unknown resource, some machine which is not comprehended within the ordinary rules; they think themselves so much the more certain of meeting with it, the further they remove from every thing which seems to have any relation with machines in use, because they imagine that the theory established with respect to the latter, cannot be extended to constructions which do not

seem

seem to have any connection with them. It is in vain to tell them that every machine may be reduced to the lever: this assertion is too vague and too wire-drawn to be admitted without a profound examination; they cannot persuade themselves that machines which appear to have nothing in common with those denominated simple ones, are subject to the same law, nor that we can pronounce upon the inutility of a secret which has not been communicated to any person: thence it happens that the most absurb ideas, and the furthest removed from the simplicity so advantageous to machines, are those which furnish them the most hopes.

The method of rooting out this error is certainly to attack it in its very source, by showing that not only in all the machines known, but also in all possible machines, it is an invariable law—that *we always lose in time or in velocity what we gain in power*,—and to explain clearly what this law signifies; but to this effect we must raise ourselves to the greatest generality possible, and not stop at any particular machine, or resort to any analogy. In the last place, there must be a general demonstration, deduced immediately and geometrically from the first axioms in mechanics: this is what I have attempted in this Essay. I have strongly insisted upon this fundamental point, and I do not know if I have succeeded in placing it in a sufficiently clear light; but on attacking error we are compelled to substitute truth in its place;—I have shown what is the true end of machinery: if it be unreasonable to expect prodigies from them beyond all probability, we shall still find there is plenty of utility in them for exercising the most lively imagination.

The reflexions I propose upon this law lead me to say a word of perpetual motion: and I have shown not only that every machine abandoned to itself must infallibly stop, but I assign the very instant when this must happen.

There will also be found among these reflections one of the most interesting properties of machines, which I think has not yet been remarked; it is, that in order to make them produce the greatest possible effect, it must necessarily happen that there be no percussion, *i. e.* that the movement

ment should always change by imperceptible degrees; which occasions, among other things, some remarks upon hydraulic machines.

Finally, I terminate this production by some reflections upon the fundamental laws of the communication of movement, which, if they be not agreeable to every body, have at least the merit of brevity.

I repeat that this Essay has merely for its object machinery in general; each machine has its peculiar properties: here we have only to do with those which are common to all; these properties, although sufficiently numerous, are in some measure all comprehended in one very simple law: it is this law I purpose to explain, to demonstrate, and develop, always regarding machines under the most general and direct point of view.

Introduction.

I. There is no want of excellent treatises upon machinery: the properties peculiar to those in frequent use, and particularly to those called simple, have been inquired after and expounded with all possible sagacity. In my opinion, however, too little attention has been bestowed in the development of those properties which are common to machinery in general, and which for this reason no more belong to the cords of a machine than to the lever, the vice, or any other machine, whether simple or compound.

It is not, however, because geometricians have neglected to ascend to the general principles of equilibrium or movement; but it is only, as it were, *en passant* that they have spoken of their application to the theory of machines properly so called: and perhaps there is none of these principles to be found which unites to a rigorous demonstration a sufficient generality, to make it answer solely and independently for the solution of the various questions which may be proposed, as well upon the equilibrium as upon the movement of machines, *i. e.* for reducing every question to a business of geometry and calculation;—this is the true object of mechanics.

II. Among

II. Among the principles more or less general which have been hitherto proposed, we shall only mention two very celebrated ones, and upon which we shall have some observations to offer.

The first is that which assigns for the general law of equilibrium in weighing machines, that the centre of gravity of the system is then at the lowest possible point; but although this antient principle be very simple and general, it does not seem that all the attention it deserves has been paid to it: it is certainly, first, because it is subject to some expressions, like all these where a *maximum* and *minimum* is mentioned: second, because it has no relation except to a particular species of force, which is gravity: thirdly and lastly, because it appears difficult to give a general and rigorous demonstration of it. But first, we shall show that by a small change in the display of this principle, we may make of it a very precise, geometrical, and true proposition, without any exception whatever. Secondly, although it has no relation except to gravity, yet it is easy to apply it to all imaginable cases: for this purpose it is only requisite to substitute a weight in the place of each of the powers which are of a different genus; this is very easy by means of a line passing upon a return pulley, in such a manner that there now remains no other defect to this principle than that of being indirect. Thirdly and lastly, although we cannot demonstrate it rigorously without ascending to the first principles of mechanism, it is, however, easy to account for it so as to remove every doubt, if we had even no other proofs, as we shall show when we come to the exact demonstration which we shall endeavour to give of it in the course of this Essay.

Let us imagine therefore a machine to which there are no other forces except weights applied; I suppose it, besides, to be of any arbitrary form, but that no movement has been given to it; this being done, whatever be the disposition of the bodies of the system, it is clear that if there be equilibrium, the sum of the resistances of the fixed points or any obstacles, estimated in the vertical direction, contrary to the gravity, will be equal to the total weight of the system;

but if a movement is given, a part of the gravity will be employed to produce it, and it is only with the surplus that the fixed points will be charged; thus, in this case the sum of the vertical resistances of the fixed points will be less at the first instant than the total weight of the system: thus from these two forces combined (the gravity of the system and the vertical charge of the fixed points) there will result from it a single force equal to their difference, and which will push the system from top to bottom as if it were free: thus the centre of gravity will descend necessarily with a velocity equal to this difference divided by the total mass of the system. Again, if the centre of gravity of the system does not descend, there will necessarily be an equilibrium.

In general therefore—*For ascertaining that several weights applied to any given machine should make a mutual equilibrium, it is sufficient to prove that if we abandon this machine to itself, the centre of gravity of the system will not descend.*

III. The immediate consequence of this principle, which is true without exception, is, that if the centre of gravity of the system is at the lowest possible point, there will necessarily be an equilibrium; for, according to this proposition, it is sufficient, in order to prove it, to show that the centre of gravity will not descend: Now, how could it descend, when upon this hypothesis it is at the lowest point possible?

IV. In order to give another application of this principle, I suppose that it is required to find the general law of equilibrium between two weights, A and B, applied to a given machine: I say then, that in consequence of the preceding principle, there will be an equilibrium between these two weights A and B, if by supposing that one of the two has to bear it, and the machine has to take a small movement, it would happen that one of these bodies would ascend while the other descended; and that at the same time these weights were in the reciprocal rates of their estimated velocities in the vertical direction: in fact, if we suppose that A then descends with the vertical velocity V, while the velocity of B, also estimated in the vertical direction would be

u, we shall have by hypothesis, $A : B :: u : V$, or $AV = Bu$, therefore $\frac{AV - Bu}{A + B} = o$. This being done, since the bodies are supposed to be in motion, the one from top to bottom, and the other *vice versâ*, it is evident that the first member of this equation is the vertical velocity of the centre of gravity of the system: thus this centre of gravity will not descend, and therefore by the preceding position there must be an equilibrium.

[To be continued.]

III. *Additional Memoir upon living and fossil Elephants. By* M. Cuvier.

[Concluded from vol. xxix. p. 254.]

Article VII.

Comparison of the Crania of the Elephant of India and that of Africa—External Characters taken from the Ears—Parts of the Cranium susceptible of Variation in one and the same Species.

I had the good fortune to be the first to remark, in 1795, the distinctive characters presented by the crania of the two elephants, and which are so much the more interesting, as they may be applied to living, or entire individuals, without being obliged to examine their jaws*. I was able to recognise them at first only by the comparison of a cranium of each species; I have now verified these observations by inspecting seven real crania, (five of which are Indian, and two African,) and several drawings.

When these crania are separated from their lower jaws and placed upon the grinders, and upon the edges of the alveoli of the tusks, the zygomatical arcades are nearly horizontal in both species.

If we next view them laterally, what is very striking is, that

* Plate II. was long ago engraved from my own drawings. I gave a proof impression of it several years ago to M. Wiedeman of Brunswick, who copied it into his Archives de Zootomie, tome ii. cah. I. pl. I.—The Author.

that the summit of the head is almost round in the African elephant, and that it rises in the Indian elephant into a kind of double pyramid.

This summit answers to the occipital arcade of man and other animals, and is so high in the elephant merely for the purpose of giving to the occipital face of the cranium a sufficient extent for a cervical ligament and occipital muscles, proportionate to the weight of the enormous mass they have to support.

This difference in the form of the summits proceeds from the difference in the inclination of the frontal line, which retreats much further in the African elephant, where it forms with the occipital line an angle of 115°, than in the Indian elephant, where it makes an angle of 90° only.

From this come the principal differences of the profile, such as, 1st, The proportion of the vertical height of the head at the distance from the end of the bones of the nose to the occipital condyles, which are nearly equal in the African elephant, (being as 33 to 32,) and the first of which is nearly one-fourth larger in the Indian elephant (being as 24 to 19). 2d, The proportion of the distance from the edges of the alveoli of the tusks at the summit to a line which is perpendicular to it and goes from the end of the bones of the nose to the anterior edge of the occipital hollow. The first of these lines is almost double that of the other in the Indian elephant (being as 26 to 14). It is little less than one-fourth larger in the African elephant (being as 21 to 16).

Besides these in the proportions, there are also differences in the contour: 1st, The front of the Indian elephant is hollowed into a sinking and concave curve; that of the African elephant is on the contrary a little convex. 2d, The sub-orbitary hole is larger in the Indian elephant. In the African, it resembles a channel rather than a simple hole. 3dly, The temporal hollow is rounder in the African elephant; and the apophysis, which distinguishes it from the orbit, is thicker than in that of India, in which this hollow has an oval contour.

When

When observed by their front view, these crania also present very remarkable differences.

1st, The greatest length of this front, taken from the summit to the edge of the alveolus, is at its greatest breadth, taken between the post-orbitary apophyses of the frontal bone, as 5 to 3 in the Indian elephant, and as 3 to 2 in the African elephant.

2d, The aperture of the nose is nearly in the middle of the face in the Indian elephant; it is one-fifth further removed from the edge of the alveolus than from the summit of the head in the African elephant.

When seen from above, these crania differ, particularly by their zygomatical arcades; they are more salient in the African than in the Indian elephant.

When we look at them behind, we are struck with new characters:

1st, The height of the wings of the sphenoidal bones, forms in the Indian elephant more than three-fourths of that of the occipital surface, while in the African elephant it scarcely forms one half.

2d, In the African elephant the posterior extremity of the zygomatical arcades is nearly on a level with the occipital condyles; in the Indian elephant it is much lower.

3d, The occiput is terminated in the upper part in the African elephant, by a semi-elliptic curve, and its base is formed by two lines in a very open angle. In the Indian elephant, the sides are in convex arcs, and the upper part of the arc is slightly concave.

The grinders are placed in both species upon two lines which converge before; they differ only by their laminæ, as we have said above.

Most of the characters we have described, contributing to the general configuration of the head, are sensible externally; there is one still more prominent, and which may distinguish the two species at the first glance. I think I was the first to remark it: it consists in the size of the ears.

The Indian elephant has middling-sized ears:—they are so large as to cover the whole shoulders in the African elephant.

 I made

I made myself certain of the first point: 1st, In three elephants which I saw alive; and I dissected two of them: two were from Ceylon, and the third from Bengal. 2d, In two other individuals which I saw in a state of preservation. 3d, In all the figures well known to belong to the Indian species, particularly those of Buffon, Blair, and Camper. 4th, In the figure of a fœtus elephant from Ceylon, described by Zimmermann, in a quarto volume upon the subject *.

Upon the second point, I have the following proofs: 1st, The elephant from Congo, dissected by Duverney. We may see its figure in the *Mémoire pour servir á l'Hist. des Anim.* par. iii.; and I am sure that the ear is not exaggerated, for it is still preserved in the Museum, and I have seen and examined it.

2d, An ear preserved in the king of Denmark's cabinet, and taken from an elephant killed at the Cape of Good Hope by captain Magnus Jacobi, in 1675. It is three feet and a half long, and two feet and a half broad †.

3d, A young African elephant in our Museum; its ears, although shrivelled up by being dried, are still as large as its head.

4th, An embryo elephant from Africa, in our Museum.

5th, All the well-known figures of the African elephants.

From these characters we may be assured from what species those figures have been drawn the origin of which is unknown, or such as are to be seen on antient monuments.

Thus that of Gessner ‡, copied by Aldrovandus §, is an African elephant. That of Valentine ||, copied by Labat ¶, and altered by Kolbe **, is equally so.

On the contrary, those of Jonston ††, which are very good, and which have served as a model to those of Hartenfels ‡‡, from which Ludolph §§ afterwards borrowed

* Erlang, 1783, in 4to. † Oliger Jacobæus, Mus. reg. Dan. 1697, fol. p. 3. ‡ Quad. p. 377. § Quad. lib. i. p. 465. || Amphithéâtr. Zoot. tab. i. f. 3. ¶ Afr. Occ. iii. p. 271. ** Relation du Cap. trad. Fr. in 12mo, tome iii. p. 11. †† Quadr. tab. vii. viii. et ix. ‡‡ Elephantograph. curious. passim. §§ Æthiop. lib. i. cap. 9.

his;

his; that of Neuhof*, the tusks of which are too high; that of Edwards †, the head of which is too round, because it is taken from a young subject, to which it was necesssary to add tusks, are all Indian elephants.

The two figures in Buffon ‡, copied by Schreber §, and by Alessandri ||, are the two sexes of the Indian species.

Mayer gives a tolerable figure of a male *dauntelah*, (vorstell. allerh. thiere, i. pl. lxix.;) but the skeleton (ib. lxx.) is copied from Blair, without any correction.

The elephant fœtus preserved in the East India Company's house at Amsterdam, and represented by Seba, (tome i. pl. cxi.) is also of the Indian species.

The limits between the Indian and African species was already distinctly enough traced with respect to the various parts of the head, and without having occasion to resort to the other characters, which we shall point out by and bye, and which are supplied by the number of the nails, and the forms of various bones of the limbs; but before being able to apply with certainty the osteological characters of the cranium to the fossil elephant, we must determine what are the variable parts of one individual from the other, in one and the same species. I have therefore subjected my Indian crania to a comparison with each other, and I did the same with my African ones.

The latter presented me with scarce any appreciable difference.

As to the former, I found some with respect to the occiput and the alveoli of the tusks.

The occiput is more swelled in every direction in the former than in the latter, without regard to the length of the tusks.

The alveoli of the tusks of the *dauntelah* are a little more oblique in front; those of the *mookna* are a little straighter towards the bottom.

The latter are a little smaller, but by no means so much so in the proportion of the tusks themselves. What is

* Ambass. Orient. Descr. Gen. de la Chine, p. 94. † Av. 221, f. 1.
‡ Hist. Nat. xi. Pl. i. et Suppl. § Quad. ii. tab. 78. || Quad. i. Pl. ii.

 deficient

deficient in the size of the tusks is compensated by a greater thickness in the osseous substance of the alveolus. The reason is, that the alveolus, serving as a base and a socket for the muscles of the proboscis, could not shrink as well as the tusks, without the proboscis losing the strength and thickness which is necessary for it.

Lastly, There is a little variety in the length of the alveoli; and, what is very remarkable, even without any reference with that of the tusks. Our large *mookna* skeleton has them longer than our two *dauntelahs*, although its tusks are the smallest of all. To conclude,—this increase in length does not exceed an inch.

It could not be considerable without the organisation of the proboscis being essentially changed, because the muscles of its lower part are inserted under the lower edge of the alveoli of the tusks, and those in the upper part are in the front, above the bones of the nose. The base of the proboscis has therefore necessarily for its vertical diameter the distance between these two points; and if the alveoli are prolonged beyond a certain measure, the proboscis would assume a monstrous size.

It is very important to notice this article, because it furnishes the most distinctive character of the fossil elephant.

If we compare together the small number of figures of elephants' skulls found in the works of naturalists, I do not think any stronger differences will be found than those I have mentioned.

The table annexed to the succeeding article expresses these differences by numbers.

A celebrated author has supposed a difference between the crania of males and females, which we have not mentioned, but he has been deceived by simple external appearances.

Our small *mookna* from Ceylon had, at the root of the proboscis, a very perceptible protuberance, which the female had not. M. Faujas, imagining that this protuberance belonged to the osseous parts, has represented these two heads in Pl. xii. of his *Essais de Geologie*. "In order to avoid," he says, p. 238, "falling into an error, when we find

find the heads of a male and female fossil elephant, we must not mistake them for two different species."

Dissection has shown us, however, that this protuberance was only produced by two cartilages peculiar to elephants, which cover the entrance of the canals of the proboscis into the osseous nostrils.

These cartilages were a little more swelled in this individual than in the others.

It is not even a character common to all males. The *dauntelah* of Bengal had it not.

The same learned geologist has given to his figures much larger tusks than these two individuals had in reality, "In order," he says, p. 269, "to make those understand, who never saw an elephant, the manner in which these animals carry their tusks." It was not necessary, however, to give large tusks to the female, which never has any in the Indian species.

Article VIII.

Examination of the Cranium of the fossil Elephant.

The cranium was very cellular; the osseous laminæ composing it were too thin to be preserved in the fossil state: they are therefore found in innumerable fragments; but three only are mentioned as being in a state of good preservation, and the most entire of the whole wants a part of the occiput.

They belong to the Petersburgh Academy *; the best was found upon the banks of the river Indigirska, in the most eastern and coldest part of Siberia, by the learned and intrepid Messerschmidt of Dantzick, who gave a drawing of it to his countryman Breynius. The latter had it engraved at the end of a memoir he inserted in the Philosophical Transactions †, and to this day it is the only public document we have upon this part of the skeleton of the fossil elephant.

I have copied the figure of Breynius in Pl. ii. fig. 1, besides the African and Indian crania; and I have reduced the three to the same size nearly, in order to facilitate their

* Pallas, Nov. Com. Petrop. xiii.

† Vol. xl. n. 446, pl. i. and ii.

 comparison.

comparison. The first glance shows that the fossil elephant resembles, in its cranium as well as in its teeth, the Indian species rather than any other.

Unfortunately the drawing is not correct enough for an exact comparison, and it is not made upon a well-determined projection. The part of the alveoli, that of the condylon for the lower jaw, and the anterior edge of the temporal hollow, and of the orbit, are seen a little obliquely behind, while the occiput and the grinders are in a rigorous profile.

We see distinctly enough, however, a striking difference in proportion in the extreme length of the alveoli of the tusks. It is treble what it would be in an Indian or African cranium of the same dimensions; and the triturating surface of the grinders prolonged, in place of meeting the alveolary edge, would intersect the tube of the alveolus at one-third of its length.

This difference is so much the more important as it agrees with the form of the lower jaw, as we see below; and, as we have already said, it would of necessity produce another conformation in the proboscis of the fossil elephant; for where the sockets of the muscles of the proboscis were the same, *i. e.* the upper part of the nose and the lower edge of the alveoli of the tusks; in this case the base of that organ was three times larger in proportion than in our living elephants; or rather the sockets of the muscles were different, and *à fortiori* its total structure was different.

If we could trust entirely to drawings, we should also find, 1st, That the zygomatic arcade is differently figured; 2d, That the post-orbitary apophysis of the frontal bone is longer, more pointed, and more crooked; 3d, That the tubercle of the lacrymal bone is much larger and more salient.

As to the absolute size of the fossil cranium, compared with our living crania, we may form an idea of it from Plate iv. fig. 9, 10, 11, where I have represented the three crania in front, and upon the same scale.

We may form a still more correct idea of their size from the following table, in which I have collected the dimensions of all the crania with which I am acquainted.

Table

	Cranium of the Petersburg Academy measured from the Drawing.	Cranium of Messerschmidt, Phil. Trans. vol. xl. pl. I.	Cranium of the great Indian Skeleton with short Teeth.	Cranium ditto with long Teeth.	Separate ditto ditto Variety with long Teeth.	Separate ditto Variety with short Teeth.	Cranium ditto African Skeleton.	Separate ditto of the African Skeleton.
From the summit to the edge of the alveoli	1·18	1·178	0·885	0·806	0·713	0·64	0·731	0·59
— to the end of the bones of the nose	0·6	....	0·437	0·433	0·344	0·374	0·296	0·255
— to the occipital condyloids	....	0·663	0·49	0·49	0·442	0·366	0·438	0·395
From the latter to the alveolary edges ..	0·93	....	0·805	0·755	0·703	0·676	0·822	0·626
Distance of the condyloids	....	....	0·65	0·614	0·52	0·512	0·551	0·551
Greatest size of the cranium	....	0·868	0·673	0·654	0·515	0·463	0·532	0·463
Distance of the two apophyses behind the orbit	....	....	0·51	0·455	0·413	0·36	0·480	0·405

But in order to infer from any one of these crania the dimensions of the animal to which it belonged, it is not necessary to refer to its first dimensions, into which the excessive length of the alveoli of the tusks enters; these only should be taken into consideration which are really homologous.

Now, by comparing them with those of the cranium of our Indian skeletons of the *mookna* and *comerea,* we find that the fossil animal must have been nearly twelve feet high. A comparison with the skeletons of the Indian *dauntelah* and *meyhée* would give a little more to the fossil.

As soon as I was acquainted with this drawing of Messerschmidt, and added to the differences it presented those I had myself observed in the lower jaw and in some isolated teeth, I no longer supposed that the fossil elephants were of a different species from those of India.

This idea, which I first announced in a memoir to the Institute, opened quite new views to me upon the theory of the earth; a hasty glance at other fossil bones induced me to presume every thing I have since discovered, and determined me to devote myself to the assiduous researches and tedious labours in which I have been occupied these ten years past.

I ought therefore to acknowledge, that it is to this drawing, buried as it were in the Philosophical Transactions for seventy years, that the public are indebted for all those works upon which I set so high a value.

I must not dissemble, however, that the characters it presented me required to be confirmed by some other specimen, in order that they might not be considered as the same species, and in spite of their agreement with those of the lower jaw, I was happy to find a drawing of another cranium.

I applied to the Petersburgh Academy; and this illustrious body, to which I now belong, complied with my wishes, with a generosity worthy of a Society to which science is so much indebted.

The Academy ordered a superb coloured drawing of the natural size to be transmitted to me of another fossil cranium from Siberia, in their collection also. It was accompanied with

with a drawing of the lower jaw of a cranium of a rhinoceros, in two positions. In these drawings I found a confirmation of what I had conceived from seeing Messerschmidt.

The cranium, which served for the model, is not so complete. The grinders, and a part of their alveoli, are wanting, as well as the middle part of the zygomatical arcade. Nothing characteristic is however missing: there is the same length and the same direction of the alveoli; the same size of the lacrymal tubercle, and the same general form: every thing in fact convinces us that the fossil skulls partake of the same characters.

I have carefully engraved this fine drawing in my Plate viii. fig. 2.

The parallelism of the grinders is a difference which may be established independently of the drawings of Messerschmidt or of the Petersburgh Academy.

M. Jæger assures me of the same fact, with reference to a portion of a cranium in the Stutgard cabinet, and which may be found in my Plate iv. fig. 4: another piece, drawn by Peter Camper, shows the same character *. I have copied his figure, Plate iv. fig. 3, and I have placed beside it fig. 1 and 2, those of Indian and African crania, seen from below, in order to show the more remarkable convergency of their front teeth.

We have in our Museum a portion of the occiput and of the temporal bone of a fossil elephant, brought from Siberia by the astronomer Delisle, which afforded me an opportunity of comparing these parts more closely than the others, of which I had drawings only; but I found some very trifling differences; I have given a back view of it in fig. 7, and a lateral one in fig. 8, of Plate iv. This specimen belonged to an elephant ten feet high.

* Mem. de Haarlem, tom. xxiii. Pl. D.

IV. *Ob-*

IV. *Observations upon the Employment of* M. Guyton-Morveau's *Fumigations for preventing contagious Infection. By* M. A. Hedouin, *Physician**.

The prison of Mont Saint Michel, from its situation, stood more in need that any other of some chemical agent to correct the numerous local vices in point of salubrity.

Putrid and malignant fevers, so familiar among individuals crowded together in the same place, using food of slender succulency, and overwhelmed with grief and sorrows, raged with unceasing fury in the prison of Mont Saint Michel.

Juniper berries and incense were frequently burned in the cells; but instead of changing the atmosphere and neutralizing the vapours exhaled from the bodies of the sick prisoners, these means only disguised the smell. From the moment M. Morveau's method was resorted to, the number of these fevers, and of course the mortality, sensibly diminished.

In order to be convinced of these truths, it is sufficient, in my opinion, to inspect the necrology of this prison, and to compare the mortality of various years, having an eye to the increase and diminution of the number of prisoners.

Necrology of the Prison of Mont Saint Michel, from the Year 1802 *to the Year* 1807.

Years.	Number of Prisoners.	Number of Deaths.	Observations.
1802	from 96 to 100	24	The oldest had not attained his 70th year; all of them died of putrid fevers, with few exceptions.
1803	100	11	The oldest not 50.
1804†	from 96 to 100	21	The putrid fever carried off 17: two prisoners who survived it lost all their toes.
1805	120	9	Two only died this year of putrid fevers.
1806 and 1807	140	6	No putrid fevers during these fifteen months, being the whole of 1806 and three months of 1807.

* *Ann. de Chimie*, tom. lxii. p. 113.

† In this year the regular use of fumigations was ordered, and the prison was furnished with two sets of M. Dumontier's apparatus.

It

It was in the year 1801 that I succeeded M. Romilly, who fell a victim to a putrid fever, which he caught while administering relief to the prisoners; and I was astonished to find more than one half of these unfortunates attacked with this species of fever.

The cells of the prisoners had a fetid smell, of so pungent and tenacious a nature that my clothes retained it four-and-twenty hours after their exposure to the free air. In these circumstances I hastened to employ M. Guyton's process. A mixture of muriate of soda, black oxide of manganese, and sulphuric acid, was put into proper vessels, which I ordered to be taken into the cells several times. This process was repeated the following day by M. Hidou.

These experiments were attended with no accident whatever, and produced a very sensible diminution of the fetid smell with which the air of the prison was impregnated; and we had the satisfaction very soon of seeing the epidemy of putrid fevers also diminished.

In order to obviate the dangers which this mode of cleansing the atmosphere presents, particularly where there are no other apartments to remove the patients into, M. Costaz, prefect of the department, furnished us with the large apparatus of M. Dumontier, since which period these machines are carried through the hospital several times daily.

Since the commencement of 1804, the number of prisoners has increased one-third; the structure of the cells has not been changed, the same misery pervades every corner of them, nor has the amelioration in the food of the miserable inhabitants arising from their labour, been sufficient to change the nature of the diseases with which they are infected; and yet putrid fevers have almost entirely disappeared.

I am certainly of opinion that M. Guyton's process was the means of extinguishing the putrid fever in the prison. I am the more inclined to believe in its efficacy from the circumstance of this fever being epidemical in several neighbouring districts, which occasioned an order by the prefect to fumigate the churches; and from the year 1804 to this

period,

period, there have been only four cases of putrid fever in the prison; a striking difference, when we consider the mortality from epidemies in the preceding years.

V. *Letter from* M. GUYTON DE MORVEAU *upon the Effects of Fumigations in Epidemies of Cattle, and for the Destruction of putrid Miasmata* *.

14th of April, 1807.

THE communication of M. Hedouin has induced me to think that it would be interesting to publish an account of the advantages resulting from the employment of my anti-contagious process on other occasions. I am extremely sorry to observe, however, that in spite of the solicitation of the government, this method is not generally known or practised; for in the *Journal de Paris* of the 17th instant, we find that a contagious disease appeared in the prisons of Dreux, and that all the judges of the tribunal of criminal justice had been seized with it and died.

The following two facts I have received from undoubted authority:

1. About the end of last autumn, the rot made its appearance in some parishes in the department of Loire and Cher. Madame de P., the proprietor of two flocks of Merino sheep, ordered fumigations of oxy-muratic acid to be made in the folds and stables: at first in open vessels, by pouring sulphuric acid upon the mixture of sea-salt and oxide of manganese; and afterwards, by means of the large apparatus. The success of this induced her to intimate throughout the parish, that she would lend the apparatus to those who had diseased sheep. A farmer in the same commune had already lost several sheep, and he applied the preservative. He opened the apparatus twice a-day, for three minutes at each time, according to the instructions he had received. The rot became mild; one half of the flock were not affected with it, and he did not lose a single sheep.

* *Ann. de Chimie*, tom. lxii. p. 119.

Two

Two other farmers of the same commune made a similarly successful experiment. Their diseased flocks were allowed to pasture along with the healthy, the latter having been previously fumigated in the folds, and no contagion was communicated. The apparatus has of course become of general use in the department.

2. The second fact was communicated to me by the directors of the hospitals at Besançon.

Several hundred weights of meat had been left neglected for some time in the cellar of the public hospital: it diffused so infected a smell that it was impossible to enter the place to carry it away, and a pitch-fork was used for that purpose. They afterwards introduced into it, with the same precautions, a flask of anti-contagious gas, the flap of which was opened and the door of the cellar was closed. When this flask was withdrawn a few hours afterwards, there existed no smell whatever, except that which was diffused by the oxy-muriatic. Its emanations having been very strong, the window was opened, in order to procure a current of fresh air. The cellar was then so completely purified that fresh meat was put into it and completely preserved.

The stench occasioned by the carcase of a dead rat was also destroyed in a few minutes, by the use of the same apparatus.

The hospitals of the department have been since provided with the regular fumigating apparatus; and glass bottles, containing a supply of sulphuric acid, nitre, and sea-salt, are placed in the halls, with which the fumigations are repeated every evening. The benefits resulting from these precautions have been felt in an astonishing degree. Besançon is always crowded with prisoners of war and wounded soldiers, among whom no contagion whatever has made its appearance since the adoption of this salutary process.

VI. *Experiments upon the liquid Sulphur of Lampadius. By Messrs.* Vauquelin *and* Robiquet *.

Process.

Lampadius, who was the first to observe this particular fluid, which he called *liquid sulphur*, obtained it by the distillation of pyritous turfs, and pyrites mixed with a certain quantity of charcoal or saw-dust. Messrs. Clement and Desormes employed the charcoal and sulphur in order to form completely the liquid sulphur, which M. Lampadius considered as a combination of sulphur and hydrogen; but these chemists announced the process as being very difficult in its execution, since they had only succeeded once out of twenty times. Nevertheless, three successive experiments have furnished us with results equally satisfactory, and we have not remarked that they have any other essential precautions than that of cooling the flasks adapted to the apparatus, in order to avoid the volatilization of this singular substance. We therefore took, as Messrs. Clement and Desormes directed, finely pulverized charcoal; we employed it in a very dry state, and introduced it into a porcelain tube, to which was adapted, by one of its apertures, a small retort containing sulphur; at the opposite extremity we placed a large tube with a simple curvature, which was inserted into a flask three-fourths filled with water; care was taken to make a hollow in that part of the tube which entered into the water, in order that it might serve as a tube of safety for the porcelain tube; this first flask, which ought to have three tubulures, has a straight tube at one of them, and at the third, a tube communicating with a second flask surrounded by snow or pounded ice: to this last we adapted a tube, crooked so as to collect the gases. Things being thus arranged, the porcelain tube is strongly heated in a reverberating furnace; when it is red-hot the sulphur volatilizes; after a certain time, there passes into the first flask a liquid of a citron yellow colour, having the appearance of an oil, collecting into globules upon the surface of

* *Ann. de Chimie* tom. lxi. p. 146.

the

the water, and falling to the bottom of the liquor when they have acquired a certain volume. If we pass too great a quantity of sulphur, a portion joins this oil and gives it more colour and density; the other is condensed in the first glass tube, and is fixed the instant it comes in contact with the water. If the first flask is so near the furnace as to make the temperature exceed 20 or 25 degrees of Reaumur, the liquid sulphur boils, is volatilized, and passes into the second flask, when the cold water completely condenses it. We remark, that when we heat the charcoal alone, there is continually liberated carbonated hydrogen gas mixed with carbonic acid; that as soon as the sulphur passes, sulphuretted hydrogen gas is disengaged in very great quantities, and that as soon as the liquid sulphur begins to be formed, little or no gas is liberated.

Physical properties.—The liquid sulphur obtained in the first operation is of a citron yellow colour, which seems to be merely accidental, and owing to a superabundant portion of sulphur, since by a new distillation we obtain it perfectly colourless, very transparent, and of great fluidity; there remains in the vessel where it has been rectified a portion of sulphur, which unites in a mass or in small regular crystals if the distillation has not been carried too far. The density of liquid sulphur is considerably more than that of distilled water; it has a very strong, fetid, sulphurous, pungent, and garlic-like smell. It has an extremely sharp, pungent, and very cold taste. The astonishing facility with which this fluid decomposes the light announces its great combustibility.

Chemical properties.—When exposed to the air in any vessel, it vaporizes very speedily, and without leaving any residue, when it is very pure: if we bring it within a few inches of an ignited body it takes fire very rapidly, yields a white flame, which latterly becomes purple; it diffuses a suffocating smell of sulphurous acid, and deposits upon the sides of surrounding bodies a yellow dust in every respect resembling sulphur.

The water in which the liquid sulphur has been received assumes a milky appearance in a few hours; and the vessels

are

are spotted with some deep black specks. This water has the same smell with the liquid sulphur, although much weaker, it possesses the property of precipitating several metallic solutions, and particularly those of lead in an orange yellow, that of oxigenated murcury in white, that of tin in brick-coloured yellow; it does not redden turnsole.

Concentrated sulphuric acid does not seem to have any very remarkable action upon this substance; latterly, however, it dissolves a certain quantity, and acquires a fetid smell.

The nitric acid seems to make it undergo more alteration; the liquid sulphur at first occupies the upper part of the liquid, but upon agitation it is divided into globules, which unite with great difficulty. Upon placing this mixture in a convenient apparatus, so as to make the gas pass through lime-water at a heat of 15 or 18 degrees, an elastic fluid is liberated, which does not disturb the lime-water, and which inflames with the same colour as oxide of carbon. But the combustion takes place instantly, and sulphurous acid is extricated after a very pungent smell: the heat must be extremely well managed, otherwise the sulphur would pass into the lime-water.

The nitric acid employed in this operation contains no trace of sulphuric acid; when we put it into oxy-muriatic acid gas it acquires a citron yellow colour, and in a few seconds that of the gas disappears; and if we place it in contact with the atmospheric air, it diffuses a very fetid and arsenical-like smoke in great abundance. It has the property of taking fire upon being brought near an ignited body this gas when well washed also inflames, and after combustion exhales a smell of sulphurous acid, stronger or weaker in proportion as it has been more or less washed.

We poured upon the liquid sulphur a mixture of the sulphuric and nitric acids, but there was no inflammation, and the action even seemed to be confined to a simple solution, at least by diluting it with a certain quantity of water the whole became perfectly clear. Some experiments seem to show that the weak acids have more action upon this substance then the concentrated ones.

Pure caustic potash acts very feebly upon liquid sulphur: after some time, however, it is coloured, and acquires the property of precipitating several metallic solutions in colours peculiar to this combination.

Ammonia seems to dissolve it a little more easily, it assumes a deep yellow colour, and also precipitates the metallic solutions: caustic barytes dissolves it also in a great proportion, and assumes an orange colour; it precipitates the metallic solutions in the same manner.

Alcohol seems to dissolve it in any proportion, the solution is abundantly precipitated by water, and the sulphur unites in small globules, which fall to the bottom of the liquid.

We see from these first efforts, that nothing whatever indicates the presence of carbon in liquid sulphur, and they authorize us to think that it is only hydrogenated sulphur, if not to suppose that sulphur itself is a compound body. The action of the oxygenated acids upon this substance appears very remarkable, and seems to indicate a state of sulphur analogous to that of the charcoal and the azot in the gaseous oxides of carbon and azot.

Note by M. Vauquelin.—The above experiments were made at my request, by M. Robiquet. They are still far from demonstrating the nature of the elements of liquid sulphur: we should have continued them if M. Berthollet jun. had not announced to the Institute that he was engaged in a similar investigation. I should not now have published these facts, if I had not also informed the Institute that I was of the same opinion with M. Berthollet, having been busy in examining the composition of liquid sulphur, and that I had even given M. Biot a considerable quantity of it in order that he might expose light to its action, and determine if possible, by its refrangent power, the portion of hydrogen it contained.

VII. *Letter from the Right Honourable Earl* STANHOPE, *relative to Dr.* CALLCOTT'S *Pamphlet on the Stanhope Temperament.*

To Mr. Tilloch.

SIR, Stratford Place, Feb. 3, 1808.

I HAVE just read the paper "*On the Stanhope and other Temperaments of the Musical Scale,*" which is written by Mr. JOHN FAREY, and published in the last number of your Philosophical Magazine. The inaccuracy of Mr. Farey's statement obliges me to inform you correctly what the facts were.

Some months ago I received from Dr. Callcott a printed copy of a pamphlet, entitled, "*Plain Statement of Earl Stanhope's Temperament, by Dr. Callcott.*" He begged me to look it over, and to correct it, previous to its publication. But, at that time, I was extremely busy about my nautical and other important pursuits, which made it very inconvenient to me to comply with his request. I received from the Doctor a letter, dated June 10th, 1807, of which the following are extracts, viz.

"Curiosity has been so strongly excited by the Stanhope Temperament, that I thought it my duty even to record my own errors on the subject; and therefore have printed 500 copies of the pamphlet, *not to sell,* but merely to distribute hereafter, as evidence how incorrect, imperfect, and incomplete my notions were, compared with the improvements now making by the superior mathematical and experimental researches of Earl Stanhope."

"I have ordered the advertisement of publication to be inserted in all the papers for the 26th of June, and I pledge myself to print it exactly conformable to your lordship's corrections, which real copy will be sufficiently distinguished from the spurious one, by the sole insertion of the four pages of music at the end.

"Immediately on the receipt of your corrected proof, I shall commence my Letter to the duke of Cumberland, to whom of course I have not yet applied; and I have also considered

considered that the duke of Cambridge ought to have a separate and particular address on the subject; and therefore another pamphlet, containing a narrative of my original dislike and subsequent recantation, will be (I am sure) interesting to his royal highness and all the world."

Finding by that letter that Dr. Callcott had already ordered "*the advertisement of publication to be inserted in all the papers for the 26th of June,*" I resolved to take the trouble to correct that printed copy; and, when I had corrected it, I returned it to Dr. Callcott. I did this merely out of good nature, and in order to prevent an imperfect statement of *my temperament* from being published, when it was in my power to make it more correct. Dr. Callcott did himself send, by Mr. Fergusson, that corrected copy of the "*Plain Statement,*" to Mr. McMillan the printer, with directions from him (Dr. Callcott) to print the same. He also sent, by the same person, to the same printer, the manuscript of his letter to the duke of Cumberland.

These *facts* are a sufficient refutation of the idle reports which have reached Mr. Farey upon this subject.

As Mr. Farey has obliged me, much against my wish, to take up my pen again on his account; I must freely confess that I can perceive no merit in his calculations.

I have expressed the length of the wire which yields the sound of the *perfect quint* G, in the correct, precise, and plain language of *two thirds* of the length of the wire which yields the sound of the key-note C. This does not, however, satisfy Mr. Farey. He is not contented till he has told us, that *two thirds* may be expressed by ·6666666 *ad infinitum*.

He is not even content with this; for he introduces, moreover, what he terms a "*new notation;*" and he expresses "*the relation which the note* G *bears to the fundamental note* C, *according to the Stanhope Temperament,*" as follows, namely,

by 358 Σ (or *schismas*).
+ 7 f (or *lesser fractions*).
+ 31 m (or *the most minute*).

This he no doubt considers as greatly advancing the cause of science, and as rendering the study of music both more pleasant and intelligible!!!

What would any man think of a mathematician who should express the number 8691 in the following manner?

8000
+ 600
+ 3 score
+ 2 dozen
+ 7.

The difference between a man of *real* science, and one who has the ambition to be thought so, is very great. The first seeks to render difficult subjects, perspicuous and clear. The other, on the contrary, envelops even the most simple ideas in the mysterious garb of *hard words* and *scientific jargon*. If Mr. Farey be of the first of those two classes, I should recommend to him to simplify and amend his tables.

I am, sir, your most obedient servant,

STANHOPE.

VIII. *Upon the Decomposition of the Acetate of Barytes by means of Soda. By M.* DARCET*.

IN a late number of the Annales de Chimie †, M. Porperes says, when speaking of the formation of the acetous acid in bad digestions, that in order to ascertain the presence of this acid, "he saturated it with pure soda, and afterwards decomposed the acetate of soda by barytes;" and he adds, "that having set the soda free, he dissolved it in alcohol, which, by seizing upon the water of solution, operated the precipitation of the acetate of barytes which was formed."

The result of this experiment is necessarily inaccurate, as the following details will prove.

I suppose that we have a solution of barytes saturated hot: if we pour it into acetate of soda, there is immediately precipitated an infinity of small brilliant and iridated laminæ.

* *Ann. de Chimie*, tom. lxi. p. 247.

† See Phil. Mag. vol. xxvii. p. 352.

If we separate them from the liquor after its complete cooling, if we wash them in the smallest quantity of water possible, and dry them speedily by squeezing them between several sheets of blotting paper, we shall have nothing but pure crystals of barytes without mixture of acetate. I ascertained this in the following manner:

1st, I exposed a part of these crystals to the air: in a few days, the distilled water with which I washed the carbonate obtained, gave no more precipitate by the addition of the sulphuric acid, the carbonates, or alkaline sulphates. The whole mass of crystals had therefore been converted into carbonate; which would not have taken place if they had contained acetate of barytes.

2d, I dissolved two or three grammes of these same crystals in distilled water; the solution blued turnsole paper which was reddened by an acid: there was an excess of alkali therefore.

I added some drops of sulphuric acid to this solution, and there was formed a solution of sulphate of barytes. I tried the liquor again with the reagent paper, and I still found an excess of alkali. I added by degrees sulphuric acid, until there was a slight excess of acid in the liquor: I filtered it, and found no more barytes, but a little free sulphuric acid; this would not have happened if the crystals had contained acetate of barytes: for upon this supposition, at the moment when the excess of acid began to become sensible to the reagent paper, there must have been only a very small quantity of acetate of barytes decomposed, and acetous acid set at liberty. The filtered liquor should therefore have contained a slight excess of acetous acid, and more acetate of barytes not decomposed; which is contrary to the result of experiments.

3d, The mother water of the crystals employed in the preceding experiments, should contain nothing except the little pure barytes which the cooled liquor could hold in solution, besides the whole of the acetate of soda which had been employed. What also demonstrates the analysis of these mother waters, is, to pour alcohol into them as M. Porperes points out. The brilliant laminæ which are

 deposited

deposited are nothing else than crystals of barytes: when tried as I have pointed out above, they only give very pure carbonate of barytes, and not an atom of acetate. If we also try these mother waters with the sulphuric acid, or with the alkaline carbonates, we immediately ascertain that they contain but little barytes, and plenty of acetous acid: this becomes still more perceptible if we evaporate them to dryness, and redissolve the residue in distilled water: for this solution no longer contains an atom of barytes, but merely acetate of soda; the little barytes in it being carbonated during the evaporation.

Hence it follows, that barytes does not decompose the acetate of soda, and that, on the contrary, if we try the inverse experiment it will succeed. In fact, we shall decompose the whole of the acetate of barytes by adding a sufficiency of pure soda for saturating the whole acetous acid.

It is not my object to invalidate the conclusion in M. Porperes's memoir: they appear to be just, and conformable to what is already known. I criticize one of the proofs only they have furnished, and I profit by this occasion to mention that M. Anfrye and myself have already published, in the *Annales de Chimie*, a memoir upon the Affinities of Barytes; wherein we have proved that in the classification of the alkalis, barytes should not be placed before potash and soda, except with respect to the sulphuric and carbonic acids; that in every other circumstance, potash and soda have affinities superior to that of barytes. How does it happen, then, that notwithstanding the facts so clearly demonstrated in our memoir, various authors have continued barytes in its old order of affinities? In my opinion, the results published of experiments ought to be adopted, or refuted by repeating them and showing their errors.

I shall conclude this note by calling to my assistance one of the processes, the excellence of which has been demonstrated in our operations upon barytes on a large scale;—it is naturally inferred from the facts above laid down.

The decomposition of the muriate, the nitrate, and the acetate of barytes, by potash and soda, is so complete and easy, that it is certainly the simplest way of procuring in a

laboratory

laboratory the barytes we require. For this purpose we strongly calcine, in close vessels, 100 parts of sulphate of barytes well mixed with 20 parts of charcoal in powder. After an hour's calcination the crucible is allowed to cool; the residue is separated from it; it is then diluted in water, and a sufficient quantity of nitric acid, muriatic acid, or acetous acid, is added to it: the mixture is then slightly heated, which liberates a great quantity of sulphuretted hydrogen and carbonic acid, of which we must be upon our guard. When the effervescence ceases, and the reagent paper announces in the liquor a slight excess of acid, we filter and evaporate it, in order to decompose the sulphuretted hydrogen, and to precipitate the sulphur which was held in solution*. The residue is re-dissolved in the least possible quantity of water, and we add to this solution a saturated solution of caustic potash. There is precipitated, even at the moment of mixture, a great quantity of crystals of barytes; the whole is then allowed to settle at the lowest possible temperature for an hour or two: the mother water is then decanted, the crystals are washed with a little distilled water, and dried by pressing them between several sheets of blotting paper, and they are dissolved in the necessary quantity of boiling water: the filtered liquor deposits the barytes upon cooling, which by this process is far purer and cheaper than that obtained by decomposing the nitrate of barytes, by exposing it alone to a high temperature.

It must be observed, that we ought to prefer the employment of the muriatic or acetous acid to that of the nitric acid; in the first place, because the first two acids form with barytes, salts more soluble than the nitrate, and the washing is easier in this case: secondly, because in the solution, the nitric acid, on being decomposed, oxygenates a part of the sulphuret of barytes, and there is then a portion of acid lost, and a part of the barytes absorbed by the sulphuric acid which is formed.

As to the caustic potash required in this operation, it is

* We may more easily obtain the same end by pouring into the liquor some drops of nitrate of copper or lead, by allowing the metallic sulphuret to subside, and by filtering over again, &c.

essential that it should be prepared with carbonate of potash free from sulphate; we may render it caustic by following the process published by M. Descroizilles in the *Annales de Chimie* *. I have frequently pursued the methods he points out, and I have done so with inereasing advantages.

Observations by one of the Editors of the Annales de Chimie *upon the foregoing Article.*

I have examined the liquor of the flask transmitted by M. Darcet along with his paper. It was more than half filled with small white crystalline laminæ; the liquor greened strongly paper coloured with the petals of mallows. I poured sulphuric acid into it in a slight excess; there was formed an abundant precipitate of sulphate of barytes; at no time was there the least smell of acetic acid. After having filtered the liquor in which the precipitate was formed, I evaporated it in a gentle fire in a platina crucible, and it left no traceof neutral salt. There does not remain a doubt, therefore, that the acetate of barytes is radically decomposed by soda.—L. B. G.

IX. *Upon the Preparation of pure Barytes.* *By M.* Robiquet †.

In a note inserted in a late number of the *Annales de Chimie* ‡, upon the decomposition of the acetate of barytes, by means of soda, M. Darcet points out as a more œconomical and more certain process of procuring pure barytes, to decompose any barytic salt, and principally the muriate, by a caustic alkali: I do not think the preference he gives to this process over the generally employed one, namely, the decomposition of the nitrate by heat, is well founded. On considering the matter in an œconomical point of view, we see that in both cases we must first obtain a soluble salt of barytes: that in the first we cannot employ the liquors so much

* See Phil. Mag. vol. xxviii.

† From *Annales de Chimie*, tom. lxii. p. 61.

‡ See the preceding article.

much concentrated that no barytes remains in solution; that whatever precaution we may take in the preparation of the caustic alkali by lime, if not during the filtrations, there will always be a portion of it carbonated; consequently, the same quantity will be wanting in the quantity of barytes we ought to obtain: that, besides this, during its precipitation, as we are obliged to shake the liquor, it carbonates a certain quantity of it: that by washing also we experience a real loss; and lastly, that by a new solution in boiling water it carbonates still more of it: it is visible that all these subtractions united leave but a small quantity, while by the decomposition of the nitrate we absolutely obtain the whole quantity of barytes it contains, and which amounts to very near the half of the weight of dry salt; and that besides, this operation is neither difficult nor expensive, when we know how to manage it properly.

Now the following are the precautions necessary to ensure success:

Fill near two thirds of the crucible with dry and pulverized nitrate; place the crucible with its lid in a common furnace, in a gentle heat, so as merely to melt the salt in its water of crystallization; increase the fire progressively and with caution, on account of the swelling up which latterly takes place: when the mass, which should be then of a cherry red, lets no more bubbles escape, cover the crucible with an inch or two of charcoal; adapt to the furnace its dome provided with a pipe of gun metal: heat it thus for a quarter of an hour; take the crucible from the fire; break it, and pack up the barytes as quickly as possible.

I also treated by this process seven pounds of nitrate, which I had divided into three common crucibles and placed in one furnace: I produced the complete decomposition in two hours, and I obtained three pounds six ounces of perfectly pure barytes. But it must be observed, that if the barytes is kept too long in the fire after the decomposition of the nitrate, it is carbonated considerably, and it is afterwards completely impossible, whatever heat we employ, to deprive it entirely of carbonic acid. This is the whole difficulty attending my operation. I am therefore of opinion that it is

 really

really more œconomical to extract barytes from the nitrate by means of fire than to follow M. Darcet's process; for, even supposing that the quantity of barytes was equal in both cases, what I have demonstrated could not exist—the price of the potash I was obliged to employ would have cost me almost double the expense. As to the purity of the article, as we are obliged to manage the washings carefully, I do not see that the process of M. Darcet merits the preference in this respect; for it is very probable that the barytes thus obtained should retain a little of the salt contained in the mother water; and, on the contrary, that extracted from the nitrate is extremely pure, if before decomposing it we take the precaution of calcining it slightly and redissolve it, in order to separate a portion of the iron proceeding from the sulphate employed.

X. *Observations upon the Combination of the fixed Oils with the Oxides of Lead and the Alkalis. By M.* Fremy, *Apothecary at Versailles* *.

Scheele was the first who observed that the water which serves as an intermedium, when we treat the fat oils or the fats by litharge, retains in solution a substance to which he has given the name of *sweet principle of oils*, because it has, in fact, a very decided saccharine taste. But, according to the observation of this illustrious chemist, this water holding also in solution a certain quantity of oxide of lead, may we not think that the taste, which suggested to him the name of the sweet principle, proceeds from the property possessed by this metal of communicating a saccharine taste to most of its combinations? Where experience has demonstrated the contrary, would it not be interesting to inquire how this principle came to be formed? What are its properties? In what state the oil exists after having abandoned the principles which should have given rise to it? If this subtraction is absolutely indispensable for forming the combi-

* From *Annales de Chimie*, tom. lxii. p. 25.

nation

nation of the oil with the oxide of lead? And upon the experiments which these inquiries would produce, might we not establish the theory of one of the most important operations of pharmacy, and the relations which its results must have with the alkaline soaps?

Such are the considerations which led me to the following experiments:

I placed in a tubulated bell glass equal parts of olive oil, litharge, and water: I adapted to the orifice of the bell glass a tube inserted into lime water, with a bladder at the orifice to prevent the access of the external air; this bladder was so disposed that I could move a spatula in the interior of the bell glass, in order to keep the matter from sticking to the bottom of the vessel. The mixture having been brought to the boiling point, I saw the oxide of lead successively pass from red to yellow, and from yellow to white. During the time the experiment lasted carbonic acid was almost always liberated. I allowed the apparatus to cool, in order to examine successively the results of this experiment.

The water which had served as the intermedium had a strong metallic taste. When placed in contact with yeast at the necessary temperature, I was never able to produce fermentation*. It precipitated evidently by the sulphuric acid and the hydrogenated sulphurets †. I passed sulphuretted hydrogen into it, until no more precipitate was formed; and I filtrated it in order to separate the sulphuret of lead.

The filtered liquor had also a very strong saccharine taste; it was evaporated to the consistence of a syrup; the acetate of lead at this moment did not demonstrate the presence of sulphuretted hydrogen. My attempts to ferment it were equally fruitless as before the separation of the oxide of lead: exposed to the air, it strongly attracts its humidity; when

* I was for a moment led into an error, because I had inadvertently used yeast, which still contained some alcohol, not having been properly washed.

† I was convinced by various experiments that it was of no importance whether in the solution of the oxide of lead the fats or oils were rancid or not, although Scheele thinks they should be rancid. In fact, it will be seen in the course of this paper that this circumstance is completely foreign to the solution.

 projected

projected upon live coals, it burns like the oils: on boiling it with the red, yellow, and white oxides of lead, it only dissolves the yellow one: when distilled several times with the nitric acid, it produces in it the formation of the oxalic acid; when distilled in a retort in the open fire, a part ascends in distillation, as observed by Scheele: on increasing the fire, it gives empyreumatic oil as a result, acetic acid, carbonic acid, carbonated hydrogen gas, and a slight spongy charcoal, which does not contain any oxide of lead.

From what I have described, it is strongly to be presumed that oil, when combined with the white oxide of lead, is no longer in the same state as it was before this combination.

In order to separate it from this oxide I made use of the acetic acid, because the solubility of the acetate of lead afforded me an easy method of separating it from the oil, the properties of which I was about to examine.

This oil has the consistence of fat, having also the same rancid taste: it is insoluble in water, and soluble in alcohol, and is precipitated by water in the same way as the volatile oils, and like these last is volatilized in part with the oil in distillation *.

The slightest ebullition is sufficient for combining it perfectly with the white oxide of lead, and gives it a strong emplastic consistence, which does not take place with litharge and massicot.

The yellow and white oxides of lead cannot be combined with the common oils; I ascertained this fact by an ebullition much stronger than if I had employed litharge.

It results, therefore, from these experiments, that when we treat the fat oils with litharge, the oxygen of the latter carries off their carbon, and previously their hydrogen, in order to form water and carbonic acid.

That this subtraction, rendering oxygen more abundant in the oils, gives rise to that saccharine substance which Scheele calls the sweet volatile principle of oils.

That this sweet principle differs from the mucoso-saccha-

* All the fat oils are dissolved in alcohol; those, however, which have been treated with litharge are much more strongly characterized with this property.

rine

rine by the property it possesses of dissolving the yellow oxide of lead; that its saccharine taste is independent of the presence of the oxide; that it differs from sugar by its volatility, and by the impossibility to ferment it.

That oil deprived of the principles which have given rise to the sweet principle, and of the quantity of hydrogen and carbon which constitutes it a fixed oil, acquires several of the properties of the volatile oils.

Finally, that this last state of oil is the only one which can be combined with the white oxide of lead.

From the knowledge I have acquired of the theory of this combination of the oils, I did not think it right to neglect to ascertain to what extent the opinion of several chemists is founded, who consider the plasters as true metallic soaps. The analogy between the plasters and the soaps can only be verified by observing in their respective combinations a resemblance in the phænomena, or at least in the results.

I mixed some pure soap-makers' ley with olive oil; I exposed this mixture to the air under a bell glass. Eight days afterwards there was only a slight absorption; the soap had still a strong alkaline taste, and the oil of this soap was not entirely dissolved in alcohol: but at the end of six weeks the absorption of the oxygen was complete; the soap was very white, of a good consistence; the alkaline taste was but feeble; diluted sulphuric acid liberated carbonic acid from it; the oil proceeding from this decomposition had the same consistency with that from the plasters, was dissolved cold in alcohol with the greatest facility, and was precipitated from it by water.

I made soap in the same way as the soap-makers; I examined with the greatest care the liquor remaining after the operation was finished, but I could discover no trace of sweet principle.

As the absence of this principle in the alkaline soap-making probably depends only upon a greater or less subtraction of carbon or hydrogen, and the action of oxygen upon oil, and the state of the oil, are absolutely the same in the making of plasters as in soap-making, I think the plas-

ters

ters should be considered relatively to the soaps as the insoluble metallic salts are relative to the alkaline salts.

I am convinced that the defect in the consistency of the soaps of potash by no means depends upon the state of the oil, but rather upon the kind of combination; for I never obtained any thing but a soft soap on treating by potash some oil proceeding from a very dry soap of soda.

XI. *Description of the Mountain Barometer, invented by Sir* HENRY C. ENGLEFIELD, *Bart. F.R.S. and made by Mr.* THOMAS JONES, *of Mount Street, Berkley Square.*

To Mr. Tilloch.

SIR,

THE various advantages which are likely to be derived from taking altitudes of every description, in a short time, with very little trouble, and at a small expense, gives me every reason to suppose the curious and enlightened mind will be pleased with a description of a new portable mountain barometer, contrived, and most peculiarly adapted, for that useful purpose, by a gentleman well known in the philosophical world. Its great simplicity in use, as well as portability, renders it superior to any barometer yet made.

The celebrated experiment devised by Pascal (says the French National Institute, in their Transactions of 1805,) and which proved, that a column of mercury decreased in proportion as the barometer was carried to a greater height, after having proved the gravity of the air, must have made the mercury be considered as a scale capable of measuring the height to which it is carried. But this scale being very small, in comparison of the heights which it ought to measure, it was soon perceived, that it would be necessary to improve the construction of the barometer, so far as to render sensible and appreciable the smallest changes in the height of the mercury. The necessity of avoiding or of calculating the continued variations which the barometer experiences, even without changing its place, presented another obstacle, much more formidable, and which seemed

to

to take away all hope of approaching the truth, or coming near it: these difficulties, however, philosophers have been able to surmount; so that barometric measures, properly employed, may vie in exactness with the trigonometrical measures, to which they are superior, on account of their facility and generality of the method.

On this subject it is not necessary that I should add anything further, the inventor of the instrument, of which a drawing is herewith sent, having satisfactorily and correctly detailed every thing connected with it, in the paper, of which, by his desire, I send you a copy subjoined.

The paper alluded to appeared in a respectable periodical work * nearly two years ago, but it has since been revised and received some considerable improvements from the author, which renders its republication desirable.

I flatter myself, that the section which I have likewise sent, of the principal and most essential improvement, will not be unacceptable to the gentleman, nor the person who may wish to make such an instrument. *a, a, a, a,* (Plate I) represents the cistern, made of box-wood;—*c, c, c,* the cover, made of brass, which screws on, and is prevented from being unscrewed (by idle curiosity,) by four small screws, *t.*—*e e* represents the stem of the cistern, into which the glass tube, *n n,* is firmly glued;—R R represents the mahogany tube, in which is inserted the stem of the cistern, where it is secured by the screws, *t t,* passing into it.

I am, sir, your very humble servant,

THOMAS JONES.

An expeditious Method of determining Altitudes, of every Denomination, with a new portable Mountain Barometer; with a Description of the Instrument.

The mensuration of heights by the barometer has been, by the labours of M. De Luc, sir George Shuckburgh, general Roy, and several other scientific men, brought to such perfection, and affords so much an easier mode of ascertaining the elevations of the different parts of the surface

* Nicholson's Journal.

of

of the earth, to a considerable precision, than any other known process, that it might have been supposed that, in the course of thirty years, which have elapsed since this branch of science has been perfected, a very great number of observations would have been made, and the heights of almost the whole surface of our own country ascertained by the numerous travellers who continually traverse it. The contrary is however the case; and the small number of observations of this kind may be attributed to several causes.

The instruments are of considerable expense, and, from their complicated construction, easily liable to be out of order in the course of a long journey.

The observations themselves, though each not taking up any very long time, yet, when multiplied on every hill and valley, as they ought to be, for the purpose of obtaining a just idea of the face of the country surveyed, in the aggregate consume much of the traveller's time; and the constant unpacking and re-packing the instrument, becomes a greater labour than our natural indolence easily submits to.

It has moreover been generally supposed, that two instruments and two observers making simultaneous observations at the upper and lower stations of the height to be measured, are indispensably necessary. This, of course, would put it out of the power of a solitary traveller to make any observations at all.

Whether from these, or other causes, the fact is, that whoever reads the numerous Tours, Surveys, and Reports of the different parts of our island, published within these last twenty years, and many of them professedly with a view to science, either of agriculture, mineralogy, or geology, will be perpetually disappointed, by meeting with mere guesses at the elevations of the tracts of country described; though a knowledge of those elevations is almost indispensable to the geologist, mineralogist, and military surveyor; highly useful to the scientific agriculturist, and very interesting to every one, who, from mere motives of enlarged and enlightened curiosity, reads books of travels, or employs his own leisure in traversing the countries described by other voyagers.

I cannot,

I cannot, therefore, but hope, by simplifying the barometer, and thereby rendering the instrument much less expensive, and its use at the same time more easy, and showing that very considerable accuracy may be attained by a single observer, this most useful branch of science may be cultivated, to so great an extent, that, in the course of a few years, we may have almost as perfect an idea of the relative heights of the different parts of England, as we now have of their horizontal distance.

A barometer, nearly similar to that which I am now about to describe, was constructed, several years since, by Dr. Hugh Hamilton, and is by him described at large in the fifth volume of the Transactions of the Irish Academy. I saw the instrument, in his hands, nearly seventeen years ago, and was much pleased with its performance. I do not know, however, that any more were then made. I have lately constructed the barometer, whose description I shall now give, which is still more simple than Dr. Hamilton's, and much cheaper, and which, in many trials I have made of it, appears to unite solidity, lightness, and ease of observation, to as great a degree as can be wished.

The barometer tube is about 33½ inches in length; its bore is a tenth of an inch in diameter, and its external diameter is three-tenths of an inch. This sized bore is fully sufficient to allow the free motion of the mercury *. The cistern is of box-wood, turned truly cylindrical, and is one inch in its external diameter, and an inch in depth; a short stem projects from its top (the instrument being in a position for making an observation), for the purpose of giving a firmer hold to the tube: this stem is perforated with a hole sufficiently large to admit the tube, which is glued to it in the usual mode. The tube projects into the cistern

* Several barometers have been lately constructed by Mr. Jones, with the lower part of the tube only a twentieth of an inch in bore, something on the principle of the marine barometers. This alteration was made because in some few instances when very carelessly used, air had got into the tube. In the form now adopted, this defect seems completely remedied, and the motion of the mercury, though full free enough for accuracy, is rendered so equable, that observations in a carriage are much easier. I therefore, on the whole, prefer this construction.

 exactly

exactly to half its depth. The bottom of the cistern is closed by a strong lid of box, which screws on the cistern, and, pressing against a leather glued to the inside of the lid, renders the whole perfectly impervious to the mercury in every position. The tube being filled and boiled in the common way, and the instrument held inverted in a perpendicular position, mercury is poured into the cistern till it is filled within two-tenths of an inch of the top. The lid is then firmly screwed on, and secured from being opened, (by idle curiosity,) by a small screw passing through its side. The essential part of the instrument is now finished. The end of the tube, in the cistern, can never be uncovered by the mercury in any possible position, and, of course, no air can ever enter into it; and, as the areas of the cistern and tube are as the square of the diameters, the diameter of the bore of the tube being ·1, its external diameter ·3, and the diameter of the cistern 1·0, the area of the cistern is 100—9 91, and there being two-tenths of an inch left empty in the cistern, the mercury must fall 182-tenths, or 18 inches and two-tenths, before the cistern is quite full; a space adequate to the measure of greater heights than any known mountain on the earth, much more to that of any height in this country. It will not easily be believed, by those who have not seen it, that the air will act on a cistern thus completely closed, and of which the wood, in its thinnest part, is above a quarter of an inch in thickness; but the fact is, that even when the pores of the box wood are closed by thick varnish (except in that part which touches the mahogany tube,) in order to prevent the wood from being affected by damp, the mercury, on turning up the barometer, takes its level almost instantaneously, certainly in less than half a minute; and that, when the instrument is suspended by the side of the best mountain barometer, of Ramsden's construction, with an open cistern, no difference whatever can be perceived in their sensibility to the variations of the atmosphere. It is obvious that the variations of altitude in this instrument, its dimensions being as above stated, will be one ninety-first part less than in a barometer furnished with an apparatus for bringing the surface of the mercury in the cistern to a fixed level:

level: this defect might be remedied by dividing the scale accordingly; but it is much more convenient to divide the scale to real inches, and make the necessary allowance in the result.

The tube and cistern being thus prepared, are mounted in a mahogany tube or frame of the size of a common walking-stick. The stem of the cistern goes into the mahogany tube, and is there secured by a piece of brass tube, which fits to the cistern and mahogany frame to which it is screwed; or the stem may be on the outside, cut into a male screw, and so be screwed into the mahogany tube. The tube is secured in the mahogany case by passing through perforated corks in the usual way.

For the observation of the height of the mercury, two opposite slits are cut in the mahogany tube, reaching from about 32 to 20 inches for the long scales; and 32 to 25 inches for the short ones; which are sufficiently long for any purpose in this country. The front slit has its sides bevelled, and is, exteriorly, about three-fourths of an inch wide; on one side is fixed a brass scale, divided as usual to inches, tenths, and twentieths. On this scale a nonius slides, moveable by a small knob, which reads off, as in other barometers, to 500th of an inch. To this nonius a small portion of brass tube is attached, which embraces the barometer tube, and its lower edge is, in observation, made a tangent to the convex surface of the mercury, as in other well-constructed barometers; and the very narrow slit behind gives abundant light for observation.

On the bevelled side of the front slit, opposite the scale, a thermometer is placed for taking the heat of the instrument; and there is room for the scale of correction, placed on Ramsden's attached thermometers, as well as Fahrenheit's scale. This thermometer is so contrived as to take out of its place, and answers the purpose of the attached and detached thermometer.

A thin brass tube, with slits in it, turns half round, on two pins, in the usual manner, and covers the apertures above described in the mahogany tube when the barometer is not in use.

The mahogany tube is made rather tapering, and with a ferrule at the end opposite the cistern. This ferrule unscrews, and shows a steel ring, by which the barometer may be suspended when convenient.

Along the mahogany tube is a scale of feet, carefully divided to inches; the feet being accurately laid down by small dots, on the heads of brass pins, sunk into the wood. A scale of this kind is always convenient, and may often be of great use.

To those travellers whose pursuits may lead them to the measures of the higher class of mountains, I would venture to recommend a barometer constructed with a tube of two feet long only; so that the whole instrument should not much exceed 25 inches in length. This barometer would not of course be useful until the mercury fell below 24 inches, which would be at a height of about 6000 feet in the atmosphere: but its great portability into regions where from both the difficulties of the path, and the rarity of the air breathed, every ounce of incumbrance becomes a serious evil; and moreover, the great security to the instrument itself arising from its shortness, would, I am persuaded, render it well worth while to carry such instruments where great altitudes are to be measured; and it is to be remembered that the instrument loses no part of its accuracy when it once comes into action, by being thus shortened.

Having thus described the instrument, a few practical remarks on the manner of using it may not be superfluous.

When I am about to make an observation, about five minutes before I arrive at the place I take out the thermometer, holding it by the upper end at nearly arm's length from my body, and, if the sun shines, in the shade of my person. It very soon takes the temperature of the air, and is not sensibly affected by the heat of the hand. The heat being observed and written down, the barometer is turned up; the brass tube half turned; and the instrument held between the finger and thumb of the left hand aboue the slit, so as to let it hang freely in a perpendicular position. Few persons, if any, have sufficient steadiness of hand to prevent little vibrations in the mercury in this position: the hand, therefore, should be either rested against any fixed body,

body, or, if no such occurs, by kneeling on one knee. The cistern should be let down so as to touch the ground, the left hand holding the barometer in a vertical position, which a little practice will render very easy. The index must then be moved by the knob till its under surface, as before stated, is tangent to the mercury. A few light taps should be given to the tube, to ascertain that the mercury has fallen as low as it can. The height being then read off and registered, together with that of the attached thermometer, the brass tube is turned back, so as to cover the slits; the instrument gently inverted, and the whole is finished. All this may be done in two minutes.

It may not be improper here to add, that I have found by experience that it is not necessary to quit the chaise in order to make observations with this barometer; it is only requisite for the horses to stand still. The thermometer, if held at arm's length out of the chaise window, will give the temperature exactly, before the order is given to stop the carriage; and the delay to the traveller will not much exceed a minute, as the observation may be read off and written down while the carriage is again going on.

The most convenient mode for deducing the heights from the barometrical observations is, certainly, by the common logarithmic tables; and it is unnecessary here to detail the method, which may be found in numerous books. It is, however, necessary for this method to carry the tables of logarithms, which is sometimes inconvenient. The engraved table formed by Mr. Ramsden is on a single narrow sheet, and extremely portable, besides being very easy in its use; but it may be lost or mislaid when wanted. Several ingenious formulæ have been devised, which may either be engraven on the instrument itself, or committed to memory. Of the former, sir George Shuckburgh has given a very concise one, in his second paper on the measurement of heights by the barometer, in the 68th volume of the Philosophical Transactions; and Mr. Professor Leslie has invented a very elegant one of the latter sort; but these, though very simple in form, require a considerable number of figures in the operation, and are, on that account, inconvenient. For the purpose, therefore, of computing on the spot, and very near to the truth, any observations made on a journey, and that, almost without the necessity of writing at all, I have caused the following short table to be engraven on the scale of the barometer. It expresses the value of the difference of the tenth of an inch in the height of the mercury at the temperature of freezing, in English feet.

Inches.	Feet.	Inches.	Feet.	Inches.	Feet.	Inches.	Feet.
20·05	130	22·25	117	25·05	104	28·35	92
·20	129	·45	116	·30	103	·65	91
·35	128	·65	115	·55	102	·95	90
·50	127	·85	114	·80	101	29·27	89
·66	126	23·05	113	26·05	100	·61	88
·82	125	·25	112	·30	99	·95	87
21·00	124	·45	111	·57	98	30·30	86
·18	123	·65	110	·85	97	·65	85
·35	122	·87	109	27·15	96	31·00	84
·53	121	24·10	108	·45	95	·37	83
·70	120	·32	107	·75	94	·75	82
·87	119	·55	106	28·05	93	32·10	81
22·05	118	·80	105				

The method of using it is as follows:—1st, Add the two observed heights of the barometer, and halve the sum to obtain the mean height. 2d, Subtract the lesser height from the greater, the remainder is of course the difference of height in tenths, &c. of an inch. 3d, Enter the table with the mean height, and take out the feet answering to it, making a proportion, if the mean height does not exactly answer to a foot. (This proportion may be made by head.) Multiply the number thus obtained by the tenths, &c. of an inch of difference of height. The result will be nearly the number of feet, answering to the difference of height between the two barometers at the temperature of freezing. When the lower barometer stands between 29 and 30 inches, and the elevation does not exceed 1500 feet, this rule will give the height within one foot of the result from the logarithmic method. When the elevation is about 3000 feet, the error will be nearly three feet, and at heights greater than 3000 feet, the error increases in a higher ratio. It is always in defect. In this country, however, such elevations

do

do not exist; and in those parts where a knowledge of the comparative heights of the different hills is the most generally useful, they seldom exceed 1000 feet; at all events, such observations as relate to great elevations may be always recomputed by more rigorous methods at leisure.

The correction of the heights thus obtained, for the temperature of the air above freezing, is by sir George Shuckburgh supposed to be as the height of the thermometer, and to be 2·44 thousandth of the approximate height for each degree of Fahrenheit, additive when the temperature is above freezing, and subtractive when below freezing. General Roy's observations and experiments lead to a supposition that the correction is not exactly as the height of the thermometer, and that at about the temperature of 50 degrees it amounts to 2·5 — thousandths, and is less, both much above and much below that temperature. For the purpose of immediate computation, I take the correction at 2·5, which, though certainly rather too great, will in general be productive of very small error, and affords a rule which is easily remembered and quickly applied. It is this:

For every four degrees that the mean temperature of the two detached thermometers exceeds 32 degrees, add one hundredth of the approximate height, as before obtained, to it; for every 40 degrees one tenth, and so for any greater or lesser number of degrees.

I have not hitherto mentioned the correction, which in fact ought to be the first in order, viz. that for the difference of temperature of the two barometers themselves: but this correction is in general so small, as to be safely neglected. Should it, however, be thought necessary to apply it in this approximate method of computing heights, the rule deduced from sir George Shuckburgh's method is as follows, and it wants no table, though he has given one for it.

When the barometer stands at 20 inches, the expansion of the mercury for one degree of Fahrenheit is two-thousandth of an inch, when it stands at 30 inches it is three-thousandths, and for the intermediate inches it increases ex-

 actly

actly as the height of the mercury; that is, at 21 inches it is ·0021, at 22 inches ·0022, and so on; so that the height in inches is the number of ten thousandths of expansion for one degree of Fahrenheit. This is very easily remembered. The expansion for any other number of degrees is in proportion to the degrees themselves; that is, for two degrees it is twice as much, for ten degrees ten times as much, and so on. Take therefore the difference of height of the *attached thermometers* at the two stations, and multiply the expansion for one degree at the coldest barometer (which will almost always be the one at the highest station) by the number of degrees of difference between the heat of the two barometers, and add the quantity to the observed height of the coldest barometer, and it is corrected for the expansion of the mercury by the heat of the instrument.

An example will make the whole clear.

	Inches.		Therm. attached.
Observation at bottom	29·400	-	50°
Observation at top -	25·190	-	46
		Difference	4
Expansion for 1° at 25 inches		-	·0025
Multiply by difference	-	-	4
			·0100

One hundredth of an inch is therefore to be added to the observed height of the upper barometer 25·190, so the corrected height is 25·200. It is, however, to be observed, that the application of this correction is of doubtful accuracy in practice; as it is by no means certain that the attached thermometer, be it placed where it may in the mounting, will give the real heat of the column of mercury in the barometer, and therefore I had at first said that it might on the whole, in general practice, be neglected. If much accuracy is wished, and time permits, the surest way is to leave the barometer in the shade so long as for the whole instrument

to

to acquire the temperature of the air, and then to make this correction according to the rule given above, from the difference of the two *detached thermometers*; and this barometer, from the lightness of its mounting, will have the advantage of taking the temperature of the air sooner than those formerly made with solid wooden cases.

It may not be improper to give an example of the method already detailed.

Observation at bottom	29·400	Therm. in air	45
——— at top -	25.200	Therm. in air	41
2		54·600		2		86
Mean -	27·300	Mean heat -	43
		Standard -	32
Difference -	42 tenths		
Value of a tenth by the table	95·5 feet.	Difference	11

1910
3820

Approximate height - 4011·0 feet
Do. by sir G. Shuckburgh 4016·0

Error - 5 feet

Correction for Temperature.

For 8° = 2 hundredths - - 80 feet
For 3° = 3 four hundredths - 30

Correction + 110
Ditto by sir G. Shuckburgh - 107·4

Error + 2·6

Approximate height by me	4011	By Sir G. S.	4016
Correction for temperature	110		107·4
Result -	4121		4123·4

EXAMPLE

EXAMPLE SECOND.

Observation at bottom	30·017	Therm. in air	60°
——— at top -	29·534	Therm. in air	57
2		59·551	2		117
Mean -	29·775	Mean - -	58·5
Difference	4·83	Standard -	32
Value of a tenth by the table.	87·5	Difference -	26·5
	350·0		
	70·00		
	2·625		
Approximate height -	422·625		
Ditto by G. Shuckburgh	422·9		
Error -	00 3		

Correction for Temperature.

For 24°	=	6 hundredths	- -	25·3
2	=	2 four hundredths	-	2·0
½	=	1 eight hundredth	-	0·5
		Correction	+	27·8
		Ditto by sir G. S.		27·2
		Error	+	0·6

Approximate height by me	422·6	By sir G. S.	422·9
Correction for temperature +	27·8		27·2
	450·4		450·1

These two examples show how near the truth the method here recommended will come, even in considerable heights.

It has been already observed, that in observations made with the barometer I have described, a small correction is necessary on account of the rise of the mercury in the cistern, as the barometer falls. Altitudes being in all cases measured

measured by the differences of the heights of the mercury at the two stations, and these differences being evidently always too small in this barometer, the correction is obviously always additive. As in constructing different barometers, the interior and exterior diameters of the tube will not always be exactly similar, though the cisterns may be turned always alike; this error, and of course the correction for it, should be in each instrument deduced from a comparison with a barometer of known accuracy at different heights. It will probably vary in different instruments from a ninetieth to a seventieth. Indeed, if it were always taken at an eightieth, in instruments constructed as above directed, the possible error could only amount to about one foot on a thousand; a quantity of very little importance.

It now remains to say a few words on the necessity of two barometers for the mensuration of heights, and the probable error to be incurred by using a single one. There is no doubt, that when very great accuracy is required, two barometers ought to be used; but even with every precaution, altitudes cannot be taken by barometers sufficiently near for the purpose of carrying water, either by pipes or canals; and for the purpose of the geologist, military surveyor, or agriculturist, it is of very little importance whether a mountain is 1000 or 1010 feet high, though it is of the highest utility that he should know whether it is 800 or 1000. I have, during the course of many years, been in the habit of taking observations of altitudes by a single barometer, and have had many opportunities of repeating my observations on the same hills when the barometer has been at different heights, and either falling or rising during the time of observation; and more than once I have observed heights which had been trigonometrically taken by the best instruments; and I can safely say that the difference between these observations has seldom amounted to so much as two feet on a hundred. The mode I use is this:—At setting out, I take the height of the mercury, and note the time of observation; I likewise note the time of the second observation, and on returning to the first station, observe again,

again, and note the time. If the barometer has altered in the interval, a simple proportion corrects either of the three observations, and reduces the height to what would have been observed had the mercury been stationary. It is true that this method supposes the motion of the mercury to have been uniform during the interval of observation; but except in very variable weather, which does not often occur, particularly in summer, when the greater number of these observations will naturally be made, this supposition is liable to but small error. It is also true, that a traveller has often no opportunity of making a second observation at the spot he sets out from. Even in this case, a near approximation may often be made by observing, for example, at a stream on each side of the hill to be measured. If also he observes the barometer repeatedly in the morning before he sets out, and sees its tendency, and does the same at every halt, during the day, he will have data whereon to found a nearly accurate correction. But if all this should be out of his power, even under the most unfavourable circumstances, barometrical observations will give a much more accurate idea of the outline of a country than any we now possess; and it should be ever remembered, that observations though defective, if carefully made, and faithfully recorded, are valuable, and if repeated by different travellers the errors will, in most cases, compensate each other, and from the whole very accurate conclusions may be drawn.

I have entered into a greater detail than would be necessary for a greater part of your readers, in the hope of being intelligible to those who are less acquainted with the subject, and who may wish to employ any instrument-maker for the construction of barometers similar to that which I have described.

In justice to a very ingenious young artist, permit me to add, that I have employed in making those which I have, Mr. Thomas Jones, of No. 124, Mount-street, Berkeley-square (pupil of the late Mr. Ramsden), and who makes them complete at the price of three guineas and a half, with a short scale reading from 25 to 31 inches; and four guineas for those with a long scale reading from 20 to 31 inches.

I am, sir, your humble servant,

H. C. ENGLEFIELD.

P. S. On comparing several barometers made by Mr. Thomas Jones, since this description was first written, I find that in some of them the mercury does not take its true height on turning up the instrument, quite so quick as in the two which he first constructed for me. This difference is owing to the greater closeness of fibre in some pieces of box wood than in others, but it does not affect the accuracy of the instrument. In order to give a quicker action to these barometers I advised Mr. Jones to bore a small hole or two in these cisterns, and insert a pin of open grained wood into them. This answered perfectly well; but a curious circumstance occurred: when deal or willow wood pins were inserted, the mercury, when shaken for some time, passed through the pores of these woods in the form of a fine black powder, and it was necessary to substitute ashen pins to confine it in the cistern. It may not be superfluous to say, that the weight of this barometer is less than a pound and a half. The weight of Ramsden's last improved barometer is $4\frac{1}{2}$ pounds, and that of his earliest about $6\frac{3}{4}$ pounds. I subjoin a few observations by which the accuracy of this barometer may be fairly estimated.

On Richmond Hill.

1806.		Barom.	Therm.	Results.
Jan. 1,	Hill top - -	29·540	44	
	Thames' side	29·686		
				Feet.
		·146	- - - -	133
Jan. 2,	Hill top - -	29·708	38	
	Thames' side	29·860		
		·152	- - - -	134
Jan. 31,	Hill top - -	29·301	36	
	Thames' side	29·453	37	
		·152	- - - -	137

Feb.

		Barom.	Therm.	Results.
Feb. 23,	Hill top - - - - -	29·758	51	
	Thames' side - - -	29·912		
		·154	- - - -	Feet. 139
Feb. 24,	Hill top - - - - -	30·180	53	
	Thames' side - - -	30·334	54	
		·154	- - - -	140

On the Signal Hill at Brighthelmston.

In 1796 with a barometer of Ramsden's.

Signal House above High water mark - -	410
In 1806, with my barometer - - - - -	403
	416
	418

Devil's Dyke, near Brighthelmston.

In 1788, with a barometer of Ramsden's - -	697
In 1806, with my barometer - - - - - -	695

Lord Abercorn's Lodge, at Stanmore, above the Stream in Edgware Town.

	Feet.
Jan. 3, - - - - - -	306
Jan. 7, - - - - - -	306

The observations from which this height was deduced, were made in the chaise, both on Jan. 3 and 7.

XII. *On* E. V.'s *Article "On the Means of gaining Power in Mechanics."*

To Mr. Tilloch.

SIR,

A CORRESPONDENNT asserts in your Magazine for last month*, that he has a machine with which, by the application of 2lb. descending through three feet, he raises 20lb. through two feet. He wishes to have some person's opinion of it through the same medium. You possibly may

* Phil. Mag. vol. xxix. p. 351.

have

have many; and on so extraordinary an assertion, I could not forbear giving mine.

As the gentleman has neither given a description nor drawing of his machine, it is impossible to give any other opinion of it than a general one, and that must be against it: for it is not very likely that the gentleman should have discovered any latent property in the mechanical powers; since they have been tried in so many different ways and forms, and by so many ingenious persons, that, had there been such latent power, surely some of them would have discovered it. But they have hitherto found, that these powers act according to certain immutable laws, beyond which, not one jot can they be forced. The mechanical powers, by a little consideration, may all be reduced to the effect of a lever, or a combination of levers; and I can, with safety, affirm that the lever possesses no such power as that which he attributes to his machine.

I should limit this last expression a little, for I do not attempt to deny what he so positively asserts, but this I can with equal confidence assert; that though his machine may perform as above related, it is impossible that it should continue to do so; that is, that it should continue to produce a rotary motion, with that power with which it set out. It must sooner or later come to a state of rest, and will then require as much external power to restore the machine to its former state, as it had apparently gained power beyond the laws of mechanics, by its first effort. Then where is the advantage of the machine?

If the gentleman would be so obliging as to favour us with a drawing and description of his machine, we may judge of the fact much better; and if it convinces the world of its great power, and overturns my argument, it will very much improve my knowledge in mechanics, and I shall very gladly subscribe myself

his much obliged humble servant,

T. SWANWICK.

Commercial Academy,
Derby, February 17, 1808.

XIII. *Ex-*

XIII. *Extract of a Memoir upon the Products which result from the Action of the Metallic Muriates, the Oxy-muriatic Acid, and the Acetic Acid, upon Alcohol. By M.* THENARD *.

M. THENARD demonstrates in this memoir, that the metallic muriates form with alcohol only a very small quantity of ether; that this ether, which is at first dissolved in a great quantity of alcohol, may be separated from it by a gentle heat in the form of gas, particularly by means of warm water, which seizes upon the alcoholic part, and puts the etherated part at liberty to a certain point; that this ethereated gas has a very great analogy to that obtained with the muriatic acid and alcohol; that in both cases it has the same smell and taste, the same solubility in water, the same manner of burning with a green flame diffusing vapours of the muriatic acid, although before the combustion no re-agent indicated the presence of the gases; in short, that they only differ from each other by the etherated muriatic gas not liquefying, except at a heat of 12·5 of the centigrade thermometer, while the other becomes liquid at + 16·5. This difference being very slight, M. Thenard thinks we can no longer hesitate to acknowledge the nature and mode of formation to be the same in both: thus in the metallic muriates, it is only the excess of acid which acts upon the alcohol, &c. that for this reason alcohol cannot be converted into ether except by a great quantity of metallic muriate, and that this conversion is the easier, the greater excess of acid the muriate contains, and the more it is soluble in alcohol: the muriate of tin therefore will succeed better in this operation than any other. In all cases the oxide of the muriate is not de-oxidated, and a portion only of this oxide is precipitated.

Proceeding to consider the action of the oxy-muriatic acid upon alcohol, he shows that in the re-action of these two bodies upon each other, which is very brisk, almost all the oxy-muriatic acid is decomposed, and that much water is produced, plenty of muriatic acid, undecomposed alcohol,

* From *Ann. de Chimie*, tom. lxi. p. 308.

a great

a great quantity of oily matter thicker than water, having a cool taste like mint, and a peculiar smell completely different from that of ether: further, a small quantity of carbonic acid, a matter easily charred, and probably acetous acid, but no ether:—that the oxy-muriatic ether of Scheele is merely muriatic ether properly so called, when made of a mixture of alcohol, muriatic acid, and black oxide of manganese; or of muriatic ether and sulphuric ether, when made with the black oxide of manganese, sea-salt, alcohol, and sulphuric acid:—that Pelletier's is also of this nature, since he made it by using the foregoing mixture; and that the oxy-muriatic ether said to be obtained by passing the oxy-muriatic acid through alcohol, is nothing else than a solution in alcohol of a greater or less quantity of oily matter. We may even separate the oil from the latter by means of water, and we re-form it all at once by dissolving this oil in a determinate quantity of alcohol.

The novelty in this part of the author's labours does not consist in this formation of oily matter, water, acetous acid, &c.; for Scheele, in his *Memoires de Chimie*, speaks of the oily matter; and M. Berthollet, in the *Memoires de l'Academie* for 1785, speaks of this matter, and besides of water, acetous acid, &c., as produced in this operation. M. Thenard's claim to novelty consists in his having proved* that the oxy-muriatic acid could not with alcohol form ether; and he has explained why Scheele and so many other chemists happened to obtain it.

In the last place, being anxious to examine the formation of the acetic ether, M. Thenard mixed together 120 grammes of highly concentrated alcohol, and 120 parts of acetic acid, of an acidity determined by the quantity of potash required by this acid for its saturation; he distilled this mixture, cohobated it twelve times, and thus sensibly decomposed the whole of the alcohol employed, and 66·16 grammes of acetic acid, representing 32 grammes of dry acid,

* M. Berthollet, in the *Memoires de l'Academie* for 1785, has even announced that the muriatic acid and alcohol produce but very little ether; and we may perceive that he is inclined to regard this small quantity of ether as foreign to the re-action of these two bodies.

acid, or as it exists in the acetite of potash well melted. About 120 grammes only of acetic ether were formed, however, although no gas was liberated; and the operation when terminated presented a loss of seven grammes only: from this M. Thenard is led to think that a portion of the oxygen of the acetic acid is combined with a portion of the hydrogen of the alcohol, while the other principles of the acid and those of the alcohol unite to constitute ether. Otherwise, if no water was formed, it would be necessary, in order to account for this operation, to admit that the best rectified alcohol contains nearly one-fifth of its weight of water, which is scarcely probable. This ether has a pleasant smell of ether and acetic acid, and yet it neither reddens turnsole tincture nor turnsole paper; it has a taste peculiar to itself, and very different from that of alcohol. Neither its specific gravity nor its tenuity, has been as yet exactly taken; all we know is, that it is lighter than water, as it floats above it, and more turbid than alcohol. Water seems to dissolve more of it than of the sulphuric ether. It burns with a yellowish white flame, producing an acid, which is probably the acetic. Finally, in a sealed flask, it does not seem to alter upon standing for a length of time; at least M. Thenard had a six months experience on this point.

XIV. *Upon a peculiar Property in Camphorated Water.* *By M.* Cadet*.

Three years ago a surgeon in Madrid announced that the carbonic acid favoured the solution of camphor in water, and that the water had very remarkable medicinal properties in diseases of the bladder. Leaving it to physicians to judge of this matter, I was merely desirous of ascertaining the chemical fact. I made a solution of camphor in distilled water, and another in water saturated with carbonic acid by Mr. Paul's method, in order to estimate the quantity of camphor dissolved. I weighed the camphor before and after the solution, and I found that the distilled water had absorbed 16 grains of it per pint, and the carbonic acid only

* From *Ann. de Chimie*, tom. lxii. p. 132.

15 grains.

15 grains. As I had been obliged to filter the liquors and to dry the filters, I thought that the camphor not dissolved must have lost its weight by evaporation, and that the balance did not give me the just quantity absorbed by the water: I therefore sought for a reagent, which evinced to me the presence of camphor in the water.

I found that potash precipitated the camphorated water, while soda or ammonia did not affect it; but the potash must be pure and caustic. If it contains carbonic acid, it does not precipitate the camphor; and if after having precipitated it we expose the vessel to the air, the liquor absorbing carbonic acid resumes its transparency.

Here, therefore, is a new method of distinguishing potash from soda. Camphorated water is in this respect a more certain reagent than the nitro-muriate of platina, and more easily procured. But the metallic salt is more convenient, as it precipitates the carbonate of potash.

On trying by caustic potash camphorated water charged with carbonic acid, I obtained no precipitate, except by putting in a great excess of alkali: this precipitate did not seem to be greater than that obtained in distilled water. I think, therefore, that carbonic acid does not sensibly favour the solution of camphor in water; but it at least results from these experiments, that the water does not merely charge itself with the aroma of the camphor, as some chemists think, and that this concrete volatile oil is dissolved in a proportion sufficient for the purposes to which it is applied. When the camphor is reduced very small by trituration with some drops of alcohol, water takes up more of it than 16 grains per pint, and some chemists have dissolved even 30 grains.

XV. *Letter from* Gavin Lowe, *Esq., on the Comet of* 1807.

To Mr. Tilloch.

SIR,

The comet that made its appearance about the latter end of last September, and continued visible during the three succeeding months, has no doubt been carefully and assiduously observed by the astronomers, not only in this coun-

try,

try, but by many others in different parts of the world; so that the elements of its orbit will be ascertained with great precision.

I had an opportunity of observing it fourteen times between the 4th of October and the 12th of November, but none afterwards. The right ascensions and declinations were corrected for refraction, and from them the geocentric longitudes and latitudes were deduced. With these data I computed the elements of the orbit according to the rules laid down in sir Henry Englefield's excellent Treatise upon Comets, and hope that, though not quite accurate, they will not be found to err much.

The drawing (see Fig.) represents the comet's orbit simply applied to, but not projected on the plane of the ecliptic. The outer circle ♈, ♋, ♎, ♑, is drawn at pleasure with any radius.—A B C D is the earth's, and E V F part of the comet's orbit: X S V its axis: ☋ ☊ the line of nodes: V the perihelion point, and S V the perihelion distance.

The elements of the orbit are nearly as follows: The perihelion distance = 0·64802; the distance of the earth from the sun S A or S B being = 1·00000.

The time of the comet's passing the perihelion at V was September 18th, 22 hours 10 minutes M. T.

The longitude of V on the orbit was 28° 41′ in Scorpio.

The longitude of the ascending node 26° 36′ in Sagittarius.

The comet passed the ascending node September 29th, $18^h\ 48^m$.

The longitude of the axis S X as seen from the sun, was in 13° 11′ of Gemini; and its elevation or north latitude 24° 43′.—The inclination of the orbit 63° 15′:—this is easily conceived, by supposing the visible part of the orbit from *n* to F to revolve upon the line of nodes ☋ *n* till any point in the orbit, as F, is elevated 63¼° above the plane of the ecliptic.

The comet was seen here soon after it passed V; the earth at that time was nearly at A, moving from thence towards B; while at the same time the comet moved from V towards F; and consequently its motion was direct.

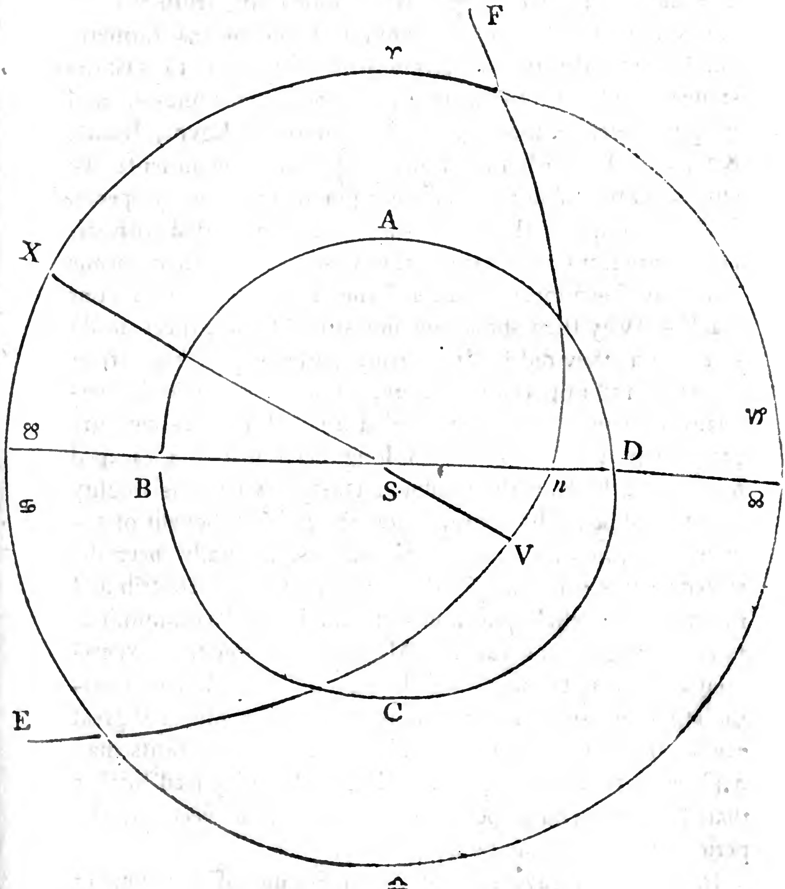
Owing

Owing to the oblique position of the axis, it was impossible to see the comet in the inferior part of its orbit E V, on its approach to the perihelion.

I am, sir, your most obedient servant,

GAVIN LOWE.

Islington,
Feb. 15, 1808.



XVI. *A Second Letter from* E. V. *on the Means of gaining Power in Mechanics.*

To Mr. Tilloch.

SIR,

MUCH as the moderns are reckoned to surpass the antients in mathematical knowledge, and notwithstanding the experience and improvements in mechanics, during the later and more enlightened ages, it is a mortifying truth that we are even at this day totally ignorant of the means formerly employed, and very extensively in use, to move to vast distances, and raise to great height, prodigious masses and weights, such as the celebrated columns of Egypt, Rome, &c.; and that with all our advantages over the antients, we still remain unable to equal their practice in these respects. It does not appear that our predecessors were gifted with superior intellect or strength to the present race; their means must have been mechanical, and therefore must be within our reach.—Why then should we not attempt and expect to do as much as they did? It is truly surprising that an art of so useful and important a nature, so nearly allied to the mechanical powers in constant varied use and progressive improvement among us, has so long been lost and escaped discovery! Perhaps the time is not distant when this mighty secret shall again be common for the general benefit of society. My humble endeavours have occasionally been directed to this end; and with this view I have contributed my mite, and shall continue so to do by the communication, through your valuable Magazine, of relative experiments and results which may be novel, in the hope of stimulating the exertions of the more scientific;—for, as "great events oft owe their rise to trivial cause," my hints may haply furnish abler heads with ideas which may lead further than I can pretend to penetrate; and thus, between us, the period of success may be accelerated.

In my last, I gave account of an engine of my contrivance, which gained considerable power:—this engine has since been so improved, that with the same moving power, the same velocity, &c., it raises a weight of thirty pounds instead

instead of twenty, and there is consequently reason to believe that it may yet be made far more powerful.

Aware of the reception which this extraordinary fact, so contrary to established opinions, is likely to meet, I have sought, and been so fortunate as to find, additional support in further experiments, and the construction of another engine materially different from the first, which, however, produces similar, and indeed greater effects.

More of this shall be the subject of my next paper; and in the mean time, those of your numerous readers, who may think with me, that the means of gaining power cannot be limited to the two which I have thus announced, have a wide field opened for the exercise of their genius in this research. I have the honour to be, sir,

your very humble servant,

Bracknell,
Feb. 23, 1808. E. V.

XVII. *On the Use of Sulphur as a Vermifuge.* By JOSEPH HUME, *Esq., of Long-Acre, London.*

To Mr. Tilloch.

SIR,

HAVING been favoured with some additional information respecting the efficacy of *sulphur*, and the proper way of applying it to vegetables, I now fulfil the conditional promise I made in my last letter *. The method is truly simple; for nothing more is required than to sprinkle sublimed sulphur, or, as it is commonly called, flowers of brimstone, over the leaves of the tree or plant, wherever the effects of worms or insects prevail, or may be expected to come. This may be so easily accomplished, that it seems superfluous to point out any particular plan or apparatus. The sulphur may be tied up in a piece of muslin or linen, and with this the leaves and young shoots should be dusted; or it may be thrown on by means of a common swandown puff, or even a dredging-box. However, if this practice become general, no doubt some convenient instrument,

* Phil. Mag. vol. xxix. p. 353.

 probably

probably a pair of bellows, constructed on purpose, will be contrived, so as to prevent unnecessary loss of the sulphur.

By the same friendly communication I have received fresh assurances, not only of the powerful influence of sulphur as a vermifuge against the whole tribe of worms and other insects, which infest, and prey upon, vegetables; but I also find, that in other respects this substance is even congenial to the health of those trees and plants on which it is sprinkled—that peach-trees in particular were remarkably improved by the sulphur; they absorbed it, and, it may be said, were even fond of it; for it was evidently absorbed, and must have entered into the vegetable system. It was likewise noticed, that the verdure and other healthful appearances were perceptibly increased; for the quantity of new shoots and leaves formed subsequently to the operation, and having no sulphur on their surfaces, served as a kind of comparative index, and pointed out distinctly the accumulation of health.

Upon the whole, it may be observed, that, independently of its deleterious effects on the vermin, the question respecting these sanative powers of such an insoluble substance as sulphur, seems to be one of the utmost importance; and, I should think, must be highly interesting to the physiologist, and, indeed, to all men of science.

I remain, sir,

your obliged and obedient servant,

Jos. Hume.

Long-Acre,
Feb. 24, 1808.

XVIII. *Experiments for investigating the Cause of the coloured concentric Rings, discovered by Sir* Isaac Newton, *between two Object-glasses laid upon one another. By* William Herschel, *LLD. F.R.S.**

The account given by Sir I. Newton, of the coloured arcs and rings which he discovered by laying two prisms or ob-ject-

* From Philosophical Transactions for 1807, Part II.

ject-glasses upon each other, is highly interesting. He very justly remarks, that these phænomena are "of difficult consideration," but that "they may conduce to further discoveries for completing the theory of light, especially as to the constitution of the parts of natural bodies on which their colours or transparency depend *."

With regard to the explanation of the appearance of these coloured rings, which is given by Sir I. Newton, I must confess that it has never been satisfactory to me. He accounts for the production of the rings, by ascribing to the rays of light certain fits of easy reflection and easy transmission alternately returning and taking place with each ray at certain stated intervals †. But this, without mentioning particular objections, seems to be an hypothesis which cannot be easily reconciled with the minuteness and extreme velocity of the particles of which these rays, according to the Newtonian theory, are composed.

The great beauty of the coloured rings, and the pleasing appearances arising from the different degrees of pressure of the two surfaces of the glasses against each other when they are formed, and especially the importance of the subject, have often excited my desire of inquiring further into the cause of such interesting phænomena; and with a view to examine them properly I obtained, in the year 1792, the two object-glasses of Huygens, in the possession of the Royal Society, one of 122 the other of 170 feet focal length, and began a series of experiments with them, which, though many times interrupted by astronomical pursuits, has often been taken up again, and has lately been carried to a very considerable extent. The conclusions that may be drawn from them, though they may not perfectly account for all the phænomena of the rings, are yet sufficiently well supported, and of such a nature as to point out several modifications of light that have been totally overlooked, and others that have never been properly discriminated. It will, therefore, be the aim of this paper to arrange and distinguish the various modifications of light in a clear and perspicuous

* Newton's Optics, 4th ed. p. 169. † Ibid. p. 256.

order,

order, and afterwards to give my sentiments upon the cause of the formation of the concentric rings. The avowed intricacy of the subject*, however, requires, in the first place, a minute detail of experiments, and afterwards a very gradual development of the consequences to be deduced from them.

As the word modification will frequently be used, it may not be amiss to say, that when applied to light, it is intended to stand for a general expression of all the changes that are made in its colours, direction, or motion: thus, by the modification of reflection, light is thrown back; by that of refraction, it is bent from its former course; by the modification of dispersion, it is divided into colours, and so of the rest.

I. *Of different Methods to make one Set of concentric Rings visible.*

In the beginning of my experiments I followed the Newtonian example, and having laid the two object-glasses of Huygens upon one another I soon perceived the concentric rings. It is almost needless to say that I found all the Newtonian observations of these rings completely verified; but as his experiments seemed to be too much confined for drawing general conclusions, I endeavoured to extend them: and by way of rendering the methods I point out very clear, I have given one easy particular instance of each, with the addition of a generalization of it, as follows:

First Method. On a table placed before a window I laid down a slip of glass the sides of which were perfectly plain, parallel, and highly polished. Upon this I laid a double convex lens of 26 inches focal length, and found that this arrangement gave me a set of beautiful concentric rings.

I viewed them with a double convex eye lens of $2\frac{1}{2}$ inches focus mounted upon an adjustable stand, by which simple apparatus I could examine them with great ease; and as it was not material to my present purpose by what obliquity of incidence of light I saw the rings, I received the rays

* Newton's Optics, 4th ed. p. 288; end of Obs. 12.

from

from the window most conveniently when they fell upon the lens in an angle of about 30 degrees from the perpendicular, the eye being placed on the opposite side at an equal angle of elevation to receive the reflected rays.

Generalization. Instead of a plain slip of glass, the plain side of a plano-concave, or plano-convex lens of any focal length whatsoever may be used: and when the convex side of any lens is laid upon it, whatever may be the figure of the other surface, whether plain, concave, or convex, and whatever may be its focal length, a set of concentric rings will always be obtained. I have seen rings with lenses of all varieties of focus, from 170 feet down to one quarter of an inch. Even a common watch-glass laid upon the same plain surface will give them.

To ensure success, it is necessary that the glasses should be perfectly well cleaned from any adhering dust or soil, especially about the point of contact; and in laying them upon each other a little pressure should be used, accompanied at first with a little side motion, after which they must be left at rest.

If the surface of the incumbent lens, especially when it is of a very short focal length, is free from all imperfection and highly polished, the adjustment of the focus of the above-mentioned eye-glass, which I always use for viewing the rings, is rather troublesome, in which case a small spot of ink made upon the lens will serve as an object for a sufficient adjustment to find the rings.

Second Method. Instead of the slip of glass, I laid down a well polished plain metalline mirror; and placing upon it the same 26-inch double convex lens, I saw again a complete set of concentric rings.

It is singular that, in this case, the rings reflected from a bright metalline surface will appear fainter than when the same lens is laid on a surface of glass reflecting but little light; this may however be accounted for by the brilliancy of the metalline ground on which these faint rings are seen, the contrast of which will offuscate their feeble appearance.

Generalization. On the same metalline surface every variety of lenses may be laid, whatever be the figure of their

uppe

upper surface, whether plain, concave, or convex, and whatever be their focal lengths, provided the lowest surface remains convex, and concentric rings will always be obtained; but for the reason mentioned in the preceding paragraph, very small lenses should not be used till the experimentalist has been familiarized with the method of seeing these rings, after which lenses of two inches focus, and gradually less, may be tried.

Third Method. Hitherto we have only used a plain surface upon which many sorts of glasses have been placed; in order therefore to obtain a still greater variety, I laid down a plano-convex lens of 15 inches focal length, and upon the convex surface of it I placed the 26-inch double convex lens, which produced a complete set of rings.

Fourth Method. The same lens placed upon a convex metalline mirror of about 15 inches focal length gave also a complete set of rings.

Generalization. These two cases admit of a much greater variety than the first and second methods; for here the incumbent glass may have not only one, but both its surfaces of any figure whatsoever; whether plain, concave, or convex; provided the radius of concavity, when concave lenses are laid upon the convex surface of glass or metal, is greater than that of the convexity on which they are laid.

The figure of the lowest surface of the subjacent substance, when it is glass, may also be plain, concave, or convex; and the curvature of its upper surface, as well as of the mirror, may be such as to give them any focal length, provided the radius of their convexities is less than that of the concavity of an incumbent lens; in all which cases complete sets of concentric rings will be obtained.

Fifth Method. Into the concavity of a double concave glass of 8 inches focal length I placed a 7-inch double convex lens, and saw a very beautiful set of rings.

Sixth Method. Upon a 7-feet concave metalline mirror I placed the double convex 26-inch lens, and had a very fine set of rings.

Generalization. With these two last methods, whatever may be the radius of the concavity of the subjacent surface, provided

provided it be greater than that of the convexity of the incumbent glass; and whatever may be the figure of the upper surface of the lenses that are placed upon the former, there will be produced concentric rings. The figure of the lowest surface of the subjacent glass may also be varied at pleasure, and still concentric rings will be obtained.

II. *Of seeing Rings by Transmission.*

The great variety of the different combinations of these differently figured glasses and mirrors will still admit of further addition, by using a different way of viewing the rings. Hitherto, the arrangement of the apparatus has been such as to make them visible only by reflection, which is evident, because all the experiments that have been pointed out may be made by the light of a candle placed so that the angle of incidence and of reflection towards the eye of the observer may be equal. But Sir I. Newton has given us also an observation where he saw these rings by transmission, in consequence of which I have again multiplied and varied the method of producing them that way, as follows:

First Method. On a slip of plain glass highly polished on both sides place the same double convex lens of 26 inches, which had already been used when the rings were seen by reflection. Take them both up together and hold them against the light of a window, in which position the concentric rings will be seen with great ease by transmitted light. But as the use of an eye-glass will not be convenient in this situation, it will be necessary to put on a pair of spectacles with glasses of 5, 6, or 7 inches focus, to magnify the rings in order to see them more readily.

Second Method. It would be easy to construct an apparatus for viewing the rings by transmission fitted with a proper eye-glass; but other methods of effecting the same purpose are preferable. Thus, if the two glasses that are to give the rings be laid upon a hollow stand, a candle placed at a proper angle and distance under them will show the rings conveniently by transmitted light, while the observer and the apparatus remain in the same situation as if they were to be seen by reflection.

Third

Third Method. A still more eligible way is to use daylight received upon a plain metalline mirror reflecting it upwards to the glasses placed over it, as practised in the construction of the common double microscope: but I forbear entering into a further detail of this last and most useful way of seeing rings by transmission, as I shall soon have occasion to say more on the same subject.

Generalization. Every combination of glasses that has been explained in the first, third, and fifth methods of seeing rings by reflection will also give them by transmission, when exposed to the light in any of the three ways that have now been pointed out. When these are added to the former, it will be allowed that we have an extensive variety of arrangements for every desirable purpose of making experiments upon rings, as far as single sets of them are concerned.

III. *Of Shadows.*

When two or more sets of rings are to be seen, it will require some artificial means, not only to examine them critically, but even to perceive them; and here the shadow of some slender opaque body will be of eminent service. To cast shadows of a proper size and upon places where they are wanted, a pointed penknife may be used as follows:

When a plain slip of glass or convex lens is laid down, and the point of a penknife is brought over either of them, it will cast two shadows, one of which may be seen on the first surface of the glass or lens, and the other on the lowest.

When two slips of glass are laid upon each other, or a convex lens upon one slip, so that both are in contact, the penknife will give three shadows; but if the convex lens should be of a very short focus, or the slips of glass a little separated, four of them may be perceived; for in that case there will be one formed on the lowest surface of the incumbent glass or lens; but in my distinction of shadows this will not be noticed. Of the three shadows thus formed the second will be darker than the first, but the third will be faint. When a piece of looking-glass is substituted for the lowest slip the third shadow will be the strongest.

Three slips of glass in contact, or two slips with a lens upon them, or also a looking-glass, a slip and a lens put together,

together, will give four shadows, one from each upper surface and one from the bottom of the lowest of them.

In all these cases a metalline mirror may be laid under the same arrangement without adding to the number of shadows, its effect being only to render them more intense and distinct.

The shadows may be distinguished by the following method: When the point of the penknife is made to touch the surface of the uppermost glass or lens, it will touch the point of its own shadow, which may thus at any time be easily ascertained: and this in all cases I call the first shadow; that which is next to it, the second; after which follows the third, and so on.

In receding from the point, the shadows will mix together, and thus become more intense; but which, or how many of them are united together, may always be known by the points of the shadows.

When a shadow is to be thrown upon any required place, hold the penknife nearly half an inch above the glasses, and advance its edge foremost gradually towards the incident light. The front should be held a little downwards to keep the light from the underside of the penknife, and the shadows to be used should be obtained from a narrow part of it.

With this preparatory information it will be easy to point out the use that is to be made of the shadows when they are wanted.

IV. *Of two Sets of Rings.*

I shall now proceed to describe a somewhat more complicated way of observation, by which two complete sets of concentric rings may be seen at once. The new or additional set will furnish us with an opportunity of examining rings in situations where they have never been seen before, which will be of eminent service for investigating the cause of their origin, and with the assistance of the shadows to be formed, as has been explained, we shall not find it difficult to see them in these situations.

First Method. Upon a well polished piece of good looking-glass lay down a double convex lens of about 20 inches focus. When the eye-glass has been adjusted as usual for seeing one set of rings, make the shadow of the penknife, in the order which has been described, pass over the lens; then, as it sometimes happens in this arrangement that no rings are easily to be seen, the shadow will, in its passage over the surface, show where they are situated. When a set of them is perceived, which is generally the primary one, bring the third shadow of the penknife over it, in which situation it will be seen to the greatest advantage.

Then, if at the same time a secondary set of rings has not yet been discovered, it will certainly be perceived when the second shadow of the penknife is brought upon the primary set. As soon as it has been found out, the compound shadow, consisting of all the three shadows united, may then be thrown upon this secondary set, in order to view it at leisure and in perfection. But this compound shadow should be taken no further from the point than is necessary to cover it; nor should the third shadow touch the primary set. The two sets are so near together, that many of the rings of one set intersect some of the other.

When a sight of the secondary set has been once obtained, it will be very easy to view it alternately with the primary one by a slight motion of the penknife, so as to make the third shadow of it go from one set to the other.

Besides the use of the shadows, there is another way to make rings visible when they cannot be easily perceived, which is to take hold of the lens with both hands, to press it alternately a little more with one than with the other; a tilting motion, given to the lens in this manner, will move the two sets of rings from side to side; and as it is well known that a faint object in motion may be sooner perceived than when it is at rest, both sets of rings will by these means be generally detected together.

It will also contribute much to facilitate the method of seeing two sets of rings, if we receive the light in a more oblique angle of incidence, such as 40, 50, or even 60 degrees. This will increase the distance between the centres of the primary and secondary sets, and at the same time occasion a more copious reflection of light.

Instead of a common looking-glass a convex glass mirror may

may be used, on which may be placed either a plain, a concave, or a convex surface of any lens or glass, and two sets of rings will be obtained.

In the same manner, by laying upon a concave glass mirror a convex lens, we shall also have two sets of rings.

The generalizations that have been mentioned when one set of rings was proposed to be obtained, may be easily applied with proper regulations, according to the circumstances of the case, not only to the method by glass mirrors already mentioned, but likewise to all those that follow hereafter, and need not be particularized for the future. In the choice of the surfaces to be joined, we have only to select such as will form a central contact, the focal length of the lenses and the figure of the upper surface being variable at pleasure.

Second Method. On a plain metalline mirror I laid a parallel slip of glass, and placed upon it the convex surface of a 17-inch plano-convex lens, by which means two sets of rings were produced.

Upon the same mirror the plain side of the plano-convex glass may be laid instead of the plain slip, and any plain, convex, or concave surface being placed upon the convexity of the subjacent lens, will give two sets of rings.

The plain side of a plano-concave glass may also be placed upon the same mirror, and into the concavity may be laid any lens that will make a central contact with it, by which arrangement two sets of rings will be obtained.

Third Method. Upon a small well polished slip of glass place another slip of the same size, and upon them lay a 39-inch double convex lens. This will produce two sets of rings; one of them reflected from the upper surface of the first slip of glass, and the other from that of the second.

Instead of the uppermost plain slip of glass we may place upon the lowest slip the plain side of a plano-oonvex or plano-concave lens, and the same variety which has been explained in the third method, by using any incumbent lens that will make a central contact, either with the convexity or concavity of the subjacent glass, will always produce two sets of rings.

Fourth Method. A more refined but rather more difficult way of seeing two sets of rings, is to lay a plain slip of glass on a piece of black paper, and when a convex lens is placed upon the slip, there may be perceived, but not without particular attention, not only the first set, which has already been pointed out as reflected from the first surface of the slip, but also a faint secondary set from the lowest surface of the same slip of glass.

It will be less difficult to see two sets of rings by a reflection from both surfaces of the same glass, if we use, for instance, a double concave of 8 inches focus with a double convex of $7\frac{1}{2}$ inches placed upon it. For, as it is well known that glass will reflect more light from the furthest surface when air rather than a denser medium is in contact with it, the hollow space of the 8-inch concave will give a pretty strong reflection of the secondary set.

Fifth Method. The use that is intended to be made of two sets of rings requires that one of them should be dependent upon the other: this is a circumstance that will be explained hereafter; but the following instance, where two independent sets of rings are given, will partly anticipate the subject. When a double convex lens of 50 inches is laid down with a slip of glass placed upon it, and another double convex one of 26 inches is then placed upon the slip, we get two sets of rings of different sizes; the large rings are from the 50-inch glass, the small rings from the 26-inch one. They are to be seen with great ease, because they are each of them primary. By tilting the incumbent lens or the slip of glass these two sets of rings may be made to cross each other in any direction; the small set may be laid upon the large one, or either of them may be separately removed towards any part of the glass. This will be sufficient to show that they have no connection with each other. The phænomena of the motions, and of the various colours and sizes assumed by these rings, when different pressures and tiltings of the glasses are used, will afford some entertainment. With the assistance of the shadow of the penknife the secondary set belonging to the rings from the 26-inch lens will be added to the other two sets;

sets; but in tilting the glasses this set will never leave its primary one, while that from the 50-inch lens may be made to go any where across the other two.

V. *Of three Sets of Rings.*

To see three sets of concentric rings at once is attended with some difficulty; but by the assistance of the methods of tilting the glasses and making use of the multiplied shadows of a penknife, we may see them very well, when there is a sufficient illumination of bright daylight.

First Method. A 26-inch double convex lens placed upon three slips of plain glass, will give three sets of rings. The slips of glass should be nearly 2-tenths of an inch thick, otherwise the different sets will not be sufficiently separated. When all the glasses are in full contact, the first and second sets may be seen with a little pressure and a small motion, and, if circumstances are favourable, the third, which is the faintest, will also appear. If it cannot be seen, some of the compound shadows of the penknife must be thrown upon it; for in this case there will be five shadows visible, several of which will fall together and give different intensity to their mixture.

Second Method. When a single slip of glass, with a 34-inch lens upon it, is placed upon a piece of good looking-glass, three sets of rings may be seen: the first and third sets are pretty bright, and will be perceived by only pressing the lens a little upon the slip of glass; after which it will be easy to find the second set with the assistance of the proper shadow. In this case four shadows will be seen; and when the third shadow is upon the first set, the fourth will be over the second set and render it visible.

Third Method. When two slips of glass are laid upon a plain metalline mirror, then a 26-inch lens placed upon the slips will produce three sets of rings; but it is not very easy to perceive them. By a tilting motion the third set will generally appear like a small white circle, which at a proper distance will follow the movement of the first set. As soon as the first and third sets are in view, the third shadow of the penknife may be brought over the first set, by which means the fourth shadow will come upon the second set, and in this position of the apparatus it will become visible.

Fourth Method. On a plain metalline mirror lay one slip of glass, but with a small piece of wood at one end under it, so that it may be kept about one-tenth of an inch from the mirror, and form an inclined plane. A 26-inch lens laid upon the slip of glass will give three sets of rings. Two of them will easily be seen; and when the shadow of the penknife is held between them, the third set will also be perceived. There is but one shadow visible in this arrangement, which is the third; the first and second shadows being lost in the bright reflection from the mirror.

Fifth Method. I placed a 6¾-inch double convex upon an 8-inch double concave, and laid both together upon a plain slip of glass. This arrangement gave three sets of rings. They may be seen without the assistance of shadows, by using only pressure and tilting. The first had a black and the other two had white centres.

VI. *Of four Sets of Rings.*

The difficulty of seeing many sets of rings increases with their number, yet by a proper attention to the directions that are given, four sets of concentric rings may be seen.

First Method. Let a slip of glass, with a 26-inch lens laid upon it, be placed upon a piece of looking-glass. Under one end of the slip, a small piece of wood one-tenth of an inch thick must be put to keep it from touching the looking-glass. This arrangement will give us four sets of rings. The first, third, and fourth may easily be seen, but the second set will require some management. Of the three shadows which this apparatus gives, the second and third must be brought between the first and fourth sets of rings, in which situation the second set of rings will become visible.

Second Method. When three slips of glass are laid upon a metalline mirror, and a plano-convex lens of about 17 inches focus is placed with its convex side upon them, four sets of rings may be seen; but this experiment requires a very bright day, and very clean, highly polished slips of plain glass. Nor can it be successful unless all the fore-

going

going methods of seeing multiplied sets of rings are become familiar and easy.

I have seen occasionally, not only four and five, but even six sets of concentric rings, from a very simple arrangement of glasses: they arise from reiterated internal reflections; but it will not be necessary to carry this account of seeing multiplied sets of rings to a greater length.

VII. *Of the Size of the Rings.*

The diameter of the concentric rings depends upon the radius of the curvature of the surfaces between which they are formed. Curvatures of a short radius, *cæteris paribus*, give smaller rings than those of a longer; but sir Isaac Newton having already treated on this part of the subject at large, it will not be necessary to enter further into it.

I should however remark, that when two curves are concerned, it is the application of them to each other that will determine the size of the rings, so that large ones may be produced from curvatures of a very short radius. A double convex lens of $2\frac{1}{4}$ inches focus, for instance, when it is laid upon a double concave which is but little more in focal length, gives rings that are larger than those from a lens of 26 inches laid upon a plain slip of glass.

VIII. *Of Contact.*

The size of the rings is considerably affected by pressure. They grow larger when the two surfaces that form them are pressed closer together, and diminish when the pressure is gradually removed. The smallest ring of a set may be increased by this means to double and treble its former diameter; but as the common or natural pressure of glasses laid upon any flat or curved surface is occasioned by their weight, the variations of pressure will not be very considerable when they are left to assume their own distance or contact. To produce that situation, however, which is generally called contact, it will always be necessary to give a little motion backwards and forwards to the incumbent lens or glass, accompanied with some moderate pressure, after which it may be left to settle properly by its own weight.

IX. *Of*

IX. *Of measuring Rings.*

It may be supposed from what has been said concerning the kind of contact which is required for glasses to produce rings, that an attempt to take absolute measures must be liable to great inaccuracy. This was fully proved to me when I wanted to ascertain, in the year 1792, whether a lens laid upon a metalline surface would give rings of an equal diameter with those it gave when placed on glass. The measures differed so much that I was at first deceived; but on proper consideration it appeared that the Huygenian object glass, of 122 feet focus, which I used for the experiment, could not so easily be brought to the same contact on metal as on glass; nor can we ever be well assured that an equal distance between the two surfaces in both cases has been actually obtained. The colour of the central point, as will be shown hereafter, may serve as a direction; but even that cannot be easily made equal in both cases. By taking a sufficient number of measures of any given ring of a set, when a glass of a sufficient focal length is used, we may however determine its diameter to about the 25th or 30th part of its dimension.

Relative measures for ascertaining the proportion of the different rings in the same set to each other, may be more accurately taken; for in that case the contact with them all will remain the same, if we do not disturb the glasses during the time of measuring.

X. *Of the Number of Rings.*

When there is a sufficient illumination, many concentric rings in every set will be perceived; in the primary set we see generally 8, 9, or 10, very conveniently. By holding the eye in the most favourable situation I have often counted near 20, and the number of them is generally lost when they grow too narrow and minute to be perceived, so that we can never be said fairly to have counted them to their full extent. In the second set I have seen as many as in the first, and they are full as bright. The third set, when it is seen by a metalline mirror under two slips, will be brighter than the

second,

second, and almost as bright as the first: I have easily counted 7, 8, and 9 rings.

XI. *Of the Effect of Pressure on the Colour of the Rings.*

When a double convex object glass of 14 or 15 feet focus is laid on a plain slip of glass, the first colours that make their faintest appearance will be red surrounded by green; the smallest pressure will turn the centre into green surrounded by red: an additional pressure will give a red centre again, and so on till there have been so many successive alterations as to give us six or seven times a red centre, after which the greatest pressure will only produce a very large black one surrounded by white.

When the rings are seen by transmission, the colours are in the same manner subject to a gradual alternate change occasioned by pressure; but when that is carried to its full extent, the centre of the rings will be a large white spot surrounded by black.

The succession and addition of the other prismatic colours after the first or second change, in both cases is extremely beautiful; but as the experiment may be so easily made, a description, which certainly would fall short of an actual view of these phænomena, will not be necessary.

When the rings are produced by curves of a very short radius, and the incumbent lens is in full contact with the slip of glass, they will be alternately black and white; but by lessening the contact, I have seen, even with a double convex lens of no more than two-tenths of an inch focus, the centre of the rings white, red, green, yellow, and black, at pleasure. In this case I used an eye-glass of one inch focus; but as it requires much practice to manage such small glasses, the experiment may be more conveniently made by placing a double convex lens of two-inch focus on a plain slip of glass, and viewing the rings by an eye-glass of $2\frac{1}{2}$ inches; then having first brought the lens into full contact, the rings will be only black and white, but by gently lifting up or tilting the lens, the centre of the rings will assume various colours at pleasure.

 XII. *Of*

XII. *Of diluting and concentrating the Colours.*

Lifting up or tilting a lens being subject to great uncertainty, a surer way of acting upon the colours of the rings is by dilution and concentration. After having seen that very small lenses give only black and white when in full contact, we may gradually take others of a longer focus. With a double convex lens of four inches the outward rings will begin to assume a faint red colour. With 5, 6, and 7, this appearance will increase; and proceeding with lenses of a larger focus, when we come to about 16, 18, or 20 inches, green rings will gradually make their appearance.

This and other colours come on much sooner if the centre of the lens is not kept in a black contact, which in these experiments must be attended to.

A lens of 26 inches not only shows black, white, red, and green rings, but the central black begins already to be diluted so as to incline to violet, indigo, or blue. With one of 34, the white about the dark centre begins to be diluted, and shows a kind of gray inclining to yellow. With 42 and 48, yellow rings begin to become visible. With 55 and 59, blue rings show themselves very plainly. With a focal length of 9 and 11 feet, orange may be distinguished from the yellow, and indigo from the blue. With 14 feet, some violet becomes visible. When the 122 feet Huygenian glass is laid on a plain slip, and well settled upon it, the central colour is then sufficiently diluted to show that the dark spot, which in small lenses, when concentrated, had the appearance of black, is now drawn out into violet, indigo, and blue, with little admixture of green; and that the white ring, which used to be about the central spot, is turned partly green with a surrounding yellow, orange, and red-coloured space or ring; by which means we seem to have a fair analysis of our former compound black and white centre.

One of my slips of glass, which is probably a little concave, gave the rings still larger when the 122 feet glass was firmly pressed against it. I used a little side motion at the same time, and brought the glasses into such contact that they adhered sufficiently to be lifted up together. With this adhesion I perceived a colour surrounding a dark centre

centre which I have never seen in any prismatic spectrum. It is a kind of light brown, resembling the colour of a certain sort of Spanish snuff. The 170 feet object glass showed the same colour also very clearly.

XIII. *Of the Order of the Colours.*

The arrangement of the colours in each compound ring or alternation, seen by reflection, is, that the most refrangible rays are nearest the centre; and the same order takes place when seen by transmission. We have already shown that when a full dilution of the colours was obtained their arrangement was violet, indigo, blue, green, yellow, orange and red; and the same order will hold good when the colours are gradually concentrated again; for though some of them should vanish before others, those that remain will always be found to agree with the same arrangement.

If the rings should chance to be red and green alternately, a doubt might arise which of them is nearest the centre; but by the method of dilution, a little pressure, or some small increase of the focal length of the incumbent lens, there will be introduced an orange tint between them, which will immediately ascertain the order of the colours.

In the second set of rings the same order is still preserved as in the first; and the same arrangement takes place in the third set as well as in the fourth. In all of them the most refrangible rays produce the smallest rings.

XIV. *Of the alternate Colour and Size of the Rings belonging to the primary and dependent Sets.*

When two sets of rings are seen at once, and the colour of the centre of the primary set is black, that of the secondary will be white; if the former is white, the latter will be black. The same alternation will take place if the colour of the centre of the primary set should be red or orange; for then the centre of the secondary one will be green; or if the former happens to be green, the latter will be red or orange. At the same time there will be a similar alternation in the size of rings; for the white rings in one set will be of the diameter of the black in the other; or the orange rings of the former will be of equal magnitude with the green of the latter.

When three sets of rings are to be seen, the second and third sets will be alike in colour and size, but alternate in both particulars with the primary set.

The same thing will happen when four sets are visible; for all the sets that are formed from the primary one will resemble each other, but will be alternate in the colour and dimensions of their rings with those of the primary set.

[To be continued.]

XIX. *Report of Surgical Cases in the City and Finsbury Dispensaries for September* 1807. *By* JOHN TAUNTON, *Esq.*

In the month of September there were admitted on the books of the City and Finsbury Dispensaries 237 surgical patients.

Cured or relieved - -	205
Died - - - -	4
Under cure - - -	28
	237

Since which time there have been admitted 963.

One of the fatal cases was that of Mrs. M. S. æt. 67, who, soon after the birth of her ninth and last child, was seized with femoral hernia: the tumour was small, but painful, attended with sickness and inclination to vomit; these symptoms did not last long, as the hernia receded without assistance: she had another attack in about six weeks, which was also of short duration; the hernia soon became irreducible, and she had been exposed to similar attacks for the last sixteen years of her life, for which time it had never been completely returned: these were occasionally longer and more severe; the symptoms frequently continued for two or three days, but were removed without any operation or medicine, except some Daffy's elixir.

On the 22d of August she was seized with the usual symptoms, viz. pain over the whole belly, particularly in the region of the stomach, with a sensation of heat, vomiting, cold sweats, intermitting pulse, and suppression of stools for four days: the tumour was now much larger than before, being nearly the size of a child's head at birth: at this period she had a motion of a small quantity of feculent matter,

matter, after which the bowels were confined for three days, when she was taken with diarrhœa, an acute pain in the bowels, cold sweats, pulse variable, vomiting of feculent matter; but the hiccup was not very distressing:—these symptoms continued for nine days, being sixteen from the attack, when she sunk under the disease.

The medicines given during this illness were, opium, calomel, extract. colocynth. cum calomel. enema communis, and the enema cum nicotiana. She could not be prevailed on to submit to the operation.

On dissection, the intestines were distended with flatus, the colon was much enlarged and inflamed, part of the sygmoid flexion was contained in the hernial sac, but not the whole circumference of the intestine, which left the canal pervious. The adhesions were firm around the mouth of the sac, from which the intestine could not be separated, and the coats were much thickened by inflammation which approached to gangrene. The omentum was much inflamed and adhered to the hernial sac; and that part of the omentum within the sac was much enlarged and more firm, which, together with the adhesions, had rendered this an irreducible hernia.

John Taunton,
Surgeon to the City and Finsbury Dispensaries, Lecturer on Anatomy, Surgery, Physiology, &c.

Greville-street, Hatton-garden,
February 23, 1808.

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On the 5th of February, a letter from Mr. Knight, to the president, was read, on the inconvertibility of the bark of trees into the alburnum. The author, as usual, detailed the effects of a great many experiments made to confirm this opinion, which he announced in the conclusion of a letter read before the Society last season, (see Philosophical Magazine, No. 109, vol. xxviii, p. 43.) One of the most obvious reasons assigned for the truth of this opinion was, that many trees having barks very dissimilar, have wood very similar; and that, had the alburnum been formed of the bark, the wood must consequently have been as different as the bark was. On the other hand, many trees with bark very similar, have wood very different. These facts, in conjunction with some minute experiments, Mr. Knight concluded as decisive that the bark is *not* transmuted, as Malpighi supposed, into alburnum; but that each performs its peculiar function in the œconomy of vegetation.

February 11 and 18. Several mathematical papers on the properties of a circle, &c., were presented to the Society by the astronomer royal, but were not of a nature to be read. A curious paper on *cranites,* or the idiotism of the inhabitants of alpine regions, who are affected with *goitres,* or swellings about the head, was read. From the author's researches among the people of Switzerland, and his investigation of the supposed connection between this species of mental imbecility and goitres, (which are generally ascribed to the effect of using snow-water on the glands,) he was led to conclude that *cranites* exists frequently where there are no *goitres,* and that many families suffer the latter complaint without experiencing the former.

February 25. A. Marsden, esq., in the chair. A geological paper on the whin-dykes in the north of Ireland, in a letter to Mr. Davy, was laid before the Society.

A continuation of Mr. Home's experiments on the functions of the spleen was read, in which this able naturalist operated on asses with extract and tincture of rhubarb, as related in his former experiments on dogs. The spleen and the colon were found impregnated with the rhubarb, when none was found in the liver. Several curious experiments were made to ascertain the quantity of serum and of rhubarb found in the blood in the *vena cava,* the left auricle of the heart, and other members: but the results are not satisfactorily established.

A letter from Mr. Murdoch, to the president, was read, containing an account of the origin, progress, and present state of gas-lights. It appears that so long ago as 1739 Dr. Clayton, in the Philosophical Transactions, discovered the inflammability of gas procured from coals: but this knowledge was never adapted to practice till about sixteen years ago, when the author, at a foundry in Cornwall, first proposed

proposed the application of this gas-light to œconomical purposes, and actually carried it into effect. In 1798 he also established it on a larger scale at Messrs. Bolton and Watts, Soho, Birmingham. But at Messrs. Phillips and Co's cotton factory it has been carried to the highest degree of perfection, and on the most extensive scale, of any place in the kingdom. From a statement of the relative expense of candles and gas, it appears that for the light which that manufactory required, 2000*l.* a year would be expended in candles; whereas the wear and tear and expenses of gas-lights do not much exceed 600*l.* a year, amounting to about one-third the expense of candles.

IMPERIAL ACADEMY OF SCIENCES AT ST. PETERSBURGH.

This Academy has published the 13th volume of their Memoirs. It contains, among other interesting articles, a description of the celebrated silver mine of Zuseoff, on Mount Athol, in Siberia, with an account of the total produce of that mine from 1747 to 1793 inclusive.

UNIVERSITY OF WILNA.

This body has announced the following as the subject of a prize question: "What are the chief diseases of plants, and what analogy exists between them and those of animals?"

The prize is 100 ducats; and the memoirs, written in Latin, French, or Polish, must be sent before the first of September 1808 to the rector of the University, under cover to Messrs. Reiser or Karner, bankers in Wilna. The prize will be adjudged in the month of January 1809.

XXI. *Intelligence and Miscellaneous Articles.*

NEW PROCESS FOR PREPARING CALOMEL.

An important improvement in the preparation of that essential article of the pharmacopœia, *calomel*, has been recently introduced by Messrs. Luke Howard and company, chemists. It consists in a peculiar mode of conducting the final *sublimation* by fire; by which the vapour of the calomel,

mel, instead of being suffered to concrete, as usual, into a solid cake at the upper part of the vessel, is thrown out into water, where it is instantly condensed into a white powder possessing the impalpable fineness of a precipitate. The imperfect operations of grinding and levigating are thus superseded, and the defects which have so generally been complained of in the medicine from this cause remedied. The product is *lighter* than levigated calomel, in consequence of its greater comminution, *three* parts by weight occupying the same space as *five* of the latter.

M. Douett Richardott, a French agriculturist, has long practised with success a new method of curing cattle whose stomachs are swoln from having fed upon wet forage. It consists in administering to the animal the twentieth part of a pound of gunpowder, mixed in a pint of milk, when first seized with the colic from eating grass or clover highly charged with dew. This remedy was long ago announced in the French Journals, but M. Richardott has been *the first to publish* the results of its application.

M. Allaire, a French chemist, has published a new method of scouring wool, which consists in dipping it repeatedly in a ley of quick-lime. The chalky earth forms an animal soap with the grease. By this means the wool is speedily and œconomically scoured, and without altering its quality.

LIST OF PATENTS FOR NEW INVENTIONS.

To Willis Earle, of Liverpool, in the county of Lancaster, merchant, for his invention of certain improvements in the tillage and dressing of land, and the cultivation of plants.—January 13, 1808.

To James Lee, of Plaistow, in the county of Essex, merchant, and John Perrin, of the same place, esq., for their new invented method of preparing certain kinds of hemp; by which the value and the utility of the articles are much increased. January 13.

To John Wilkinson, of Bradley Iron Works, in the liberty of Bilston, in the county of Stafford, esq., for his new

new invented method of making pig or cast metal from the ore; which, when manufactured into bar iron, will be found equal in quality to any that is imported from Russia or Sweden. January 23.

To Andrew Johansen, of Hommerton, in the parish of Hackney, in the county of Middlesex, gent., for certain improved methods of manufacturing a kind of tablet or artificial whetstone, for whetting or sharpening razors, penknives, surgeons' instruments, and other cutlery, and which he usually denominates "Cotific Tablets." January 23.

To Edward Moore Noble, of Birmingham, in the county of Warwick, surgeon, for his new method of making of carbonate of lead, commonly called white lead. Jan. 23.

To Samuel Phelps, of Cuper's Bridge, Lambeth, in the county of Surry, esq., for his improvements in manufacturing soap. January 23.

To Thomas Preston the younger, of Tooley-street, Southwark, in the county of Surrey, lead merchant, for his improved method of setting boilers for steam engines, pans for melting lead, tin, pewter, and other metals of easy fusion; and a new method of discharging the pans for melting lead, tin, pewter, &c., as before named, when full, and setting coppers and boilers of every description. January 26.

To George Savage, of Huddersfield, in the county of York, watch-maker, for his improved method of regulating or equalizing the force or power of the main-spring in watches, or other machines for measuring time. Jan. 26.

To William Stewart, of Limehouse, in the county of Middlesex, builder, for certain improvements in making bricks and tiles. January 26.

To Joseph Johnson and John Wilmot, of Birmingham, in the county of Warwick, manufacturers, for their new invented warming-pans, which are not only applicable to the purpose of airing and warming beds, but also for airing and warming rooms or carriages, or for other purposes requiring a long and protracted heat. January 28.

To William Newberry, of St. John-street, in the county of Middlesex, gent., for his improved machinery for the purpose of sawing wood, splitting or paring skins. Jan. 30.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For February 1808.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan. 27	26°	37°	41°	29·30	6	Cloudy
28	46	46	34	·25	11	Fair
29	34	42	42	·62	30	Fair
30	46	49	48	·70	17	Showery
31	49	52	49	·70	14	Cloudy
Feb. 1	48	53	48	·71	13	Cloudy
2	43	50	40	·55	30	Stormy
3	38	42	35	·90	18	Fair
4	32	41	36	30·30	16	Fair
5	41	47	37	·10	10	Fair
6	42	50	47	29·78	12	Stormy
7	48	49	44	·89	30	Cloudy
8	38	39	34	·73	0	Rain
9	32	39	32	·82	25	Cloudy
10	28	35	30	30·03	21	Fair
11	29	40	38	29·75	16	Cloudy
12	28	29	25	·50	0	Great fall of snow
13	28	30	25	·80	8	Fair
14	24	30	24	30·01	15	Fair
15	19	33	33	29·98	0	Snow
16	34	42	34	·88	4	Cloudy
17	32	39	40	30·02	11	Cloudy
18	42	43	38	·03	0	Rain
19	33	41	32	·28	21	Fair
20	30	38	30	·45	16	Fair
21	29	37	31	·50	20	Fair
22	30	39	32	·46	18	Fair
23	32	38	35	·37	10	Cloudy
24	35	37	33	·57	15	Cloudy

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

XXII. *On Blasting Rocks, and Tamping.* By John Taylor, *Esq.*

To Mr. Tilloch.

Sir,

A letter from an engineer of celebrity to Mr. Nicholson, and published in the ninth volume of his Journal, introduced to the public some time ago an account of the use of sand in blasting rocks or other hard substances: a letter from another gentleman, published in your Magazine soon after, continued the subject, and mentioned other modes of confining the gunpowder employed for this purpose; and we have been lately informed of the result of experiments made in France upon what Mr. Nicholson calls Mr. Jessop's method of blasting rocks with sand, by a notice published in the Philosophical Journal of July last.

As the process has long been known, however, in this neighbourhood, and the constant experience of the workmen in the mines and quarries agrees pretty nearly with the French report, it may not be useless to detail the instances in which it may be advantageously employed, and to point out the cases where it is not likely to be effectual; and in mines the latter are more numerous than the former.

As another substance has been lately introduced into use here, which possesses advantages occasionally over all others, it will likewise give me pleasure to make it more generally known, especially as this is an operation on the easy execution of which often depends the safety of the lives of the persons conducting it. The new process I allude to, is that of closing the hole, upon the proper charge of gunpowder, with clay wrought to rather a soft consistency, and rammed-in in sufficient quantity, which, where the resistance is not too great, possesses the following advantages. It may be applied to holes bored in all directions; may be perforated for the fuse without danger, and is peculiarly adapted for rock in which gunpowder is liable to be rapidly injured by water. Sand can only be used where the rock is bored perpendicularly or nearly so; and in mines but few holes of this description occur, the effect of a given quantity of gunpowder being certainly greater in those holes the mouths of which are lower than the end which contains the charge. This fact has frequently been shown by a charge in a detached piece of rock confined by *tamping*, as the miners call it, in the usual way, and fired with the mouth of the hole upwards. A certain quantity of tamping will in this position be blown out, and the rock remain entire. If the same hole be recharged with the same quantity of powder, and tamped so as to present an equal resistance, the rock will be torn to pieces by merely altering its position, so that the charge may lie higher than the mouth of the hole. Thus it may be inferred, that in firing artillery there is more danger of bursting guns when they are laid with their muzzles depressed, than when pointed at any degree of elevation.

Holes for blasting in mines are so frequently very wet as to preclude the use of sand where other circumstances would favour it; for though, by violently compressing tough clay into the fissures, the portion of the hole that is to contain the gunpowder may be made dry, yet to render it so throughout its whole length would be a process considerably too tedious. Here the advantage of applying clay for tamping is very great. The hole is instantly filled up with a water-tight substance, and an opening for the fuse is made through it with an iron rod, by a pressure so gentle as to hazard no explosion in the performance.

Clay is likewise more likely to be used by miners than sand, if they are acquainted generally with its effect, from its being always at hand, it being so necessary to the other operations of boring and charging holes that it is always in use where blasting is going on; and the difficulty of introducing any substance that requires the least exertion to procure or manage among workmen is well known, whatever safety or ease it may afford them.

The French engineers pretty accurately describe the cases in which sand will fail in its effect; that is, either when the hole is bored to a small depth, so that the quantity of gunpowder required to break the rock occupies such a length of it as not to afford space for a sufficient column of sand, or or where, from the density of the rock, or its being confined as in the shafts and levels of mines by the numerous points of contact, very great force is required to produce its fracture.

I am inclined to believe that though in these cases both sand and clay must yield to the usual mode of tamping, yet that clay will commonly produce the fracture where sand will fail to do so. About ten years ago an experiment was made in Cornwall upon a loose rock on the surface, and sand was blown out without any effect having been produced: an equal quantity of gunpowder, confined by a small quantity of tamping, broke the rock; which proved that the resistance was far inferior to that of the common mode. And from many late trials made with clay, in holes moderately deep, in Wheal Friendship copper mine and the tunnel of the Tavistock canal, I have found clay to answer nearly as well as tamping; though certainly in what the workmen call shallow holes, and in very hard rock, neither that nor sand can be depended upon. In quarries and open works where deep holes can and always ought to be had, where the ground is for the most part dry, and the rock is seldom very hard, both will be found eminently useful, and will contribute much to the safety of the workmen.

Mr. Farey mentions, in his letter in your Magazine, his having seen coal-ashes employed as tamping. The fact is, that most friable substances are proper for this purpose, those being the best that combine with friability a certain degree of tenacity: the miners esteem soft yellow copper ore, or the common galena, in the highest degree, and next perhaps is broken tile; but what is commonly used is the soft schistus rock which is attendant on most metallic veins.

These substances are beaten into the hole upon the charge of gunpowder, while a small taper iron rod called the nail, kept in during the process, forms, on being withdrawn, the vent for the rush filled with gunpowder, which constitutes the fuse. It is by the occasional attrition of this iron nail against siliceous particles, either in the act of ramming or striking it out, that dreadful accidents sometimes occur.

 The

The use of copper nails would prevent this; but though often tried, the miners, from their being not quite so convenient, will not continue the use of them.

The plan of adding to the resistance of tamping by loading the aperture of the hole, as proposed by M. Gillet Laumont, is in continual use among the Cornish miners, but in a more simple and effectual way than he suggests; and it is certain that some rock could not be blasted without this assistance.

M. Pictet, it is said in the same article of the Philosophical Journal for July, has conceived that a more effective explosion for the purposes of mining might be obtained by leaving a partial vacuity, or by the chamber not being completely filled by the gunpowder. Now this idea is directly in opposition to the received opinion of persons conversant with these operations; but not having made any accurate experiments on the subject, I shall only state a case that appears to be in point. We are frequently obliged in very wet ground to use gunpowder in cartridges of thin tinned plate; and as these cannot be made to fill up the diameter of the chamber completely, we obtain, what he conceives is desirable, a less concentrated explosion; but so far is it from being beneficial, that every miner will use every other means before he employs gunpowder in this way, because it is universally found to require a much greater quantity for a given effect, and much more, I conceive, than can be attributed to the small degree of resistance offered by the material of which the cartridge is formed.

The most promising scheme for facilitating the operation of blasting rock that I have heard of, has been suggested very lately to me by a friend whose mechanical skill and ingenuity are well known, and who has lately applied with surprising dexterity that powerful agent gunpowder at great depths under water.

A sort of powder is made of double or treble the strength of common cannon powder, and if this be applied to the uses of mining, he thinks that holes of half the usual capacity might contain sufficient charges in equal lengths. If so, it is certain that such holes, being considerably less in diameter

diameter than those now made, might be bored with much more ease and rapidity; and those only who know the labour and tediousness of the operation, besides the great expense of tools in hard siliceous rocks, can justly appreciate the value of such an improvement. I have not yet been furnished with the means of making any trial of this plan, but I hope before long to have it in my power.

I have troubled you at some length on this subject; but as it is one with which is connected the safety of several thousand men in this kingdom alone, and when it is considered, as I have heard from good authority, that the annual expense of gunpowder for the mines in Cornwall and Devon amounts to more than 30,000*l*., it becomes an object worthy discussion, if means therefrom should arise of lessening the risk to the workmen or of cost to the mines, particularly in the present distressing state to which they are reduced from the very low price at which the metals are now sold.

I am, sir, your most obedient servant,

JOHN TAYLOR, Engineer.

Holwell House, Devon,
Dec. 1, 1807.

XXIII. *Extract of a Memoir upon the Muriatic Ether, as read at the French Institute, 17th of February,* 1807. *By M.* THENARD *.

AFTER having examined why the muriatic ether has hitherto remained almost unknown among chemists, although frequently the object of chemical experiments, the author gives the method of obtaining it. For this purpose, as the above ether is habitually in the state of gas, the following apparatus must be employed:

We put in a retort barely capable of containing the mixture, an equal part in volume of highly concentrated muriatic acid and alcohol at 36°; we shake them well, in order to bring all their molecules in contact. This being done, we throw into the retort seven or eight grains of sand, in order to avoid the boiling up, which, without this precaution,

* *Ann. de Chimie*, tom. lxi. p. 290.

might take place in the course of the operation; we then place it in the naked fire upon a common furnace, by means of a grating of iron wire, and adopt Welter's tube to it, which enters into a flask with three necks, double in capacity to the retort employed, and half filled with water at the temperature of 20 or 25°*, so that the tube penetrates a considerable way into the water: afterwards we introduce into the second neck a straight tube of safety; and into the third we introduce a crooked one, which is fixed into an earthen vessel under flasks full of water, at the same degree of heat as the former, and supported by a knob screwed into the middle of it. When the apparatus is thus arranged, the retort must be gradually heated; and in about 20 or 25 minutes we see bubbles arise from the lower part of the liquid, and particularly from the surface of the grains of sand. These bubbles soon increase, and abundance of etherized gas is obtained; acid, alcohol, and water, pass over at the same time, but they remain in the first flask. From 500 grammes of air and an equal volume of alcohol, we may obtain twenty litres and upwards of etherized gas perfectly pure. But we shall extract much more from it, if, when the extrication of the gas begins to slacken, we add a fresh quantity to the residue, namely, the strongly acid liquor which remains in the retort, and the volume of which is then nearly equivalent to two fifths of the liquor from which it comes. M. Thenard even thinks, that if, by means of a straight tube going to the bottom of the retort and of a proper length, we could pour from time to time warm alcohol into the latter, the etherized gas would be formed in still greater abundance; for we should conceive that there is every moment more alcohol volatilized than muriatic acid, and that we should thus re-establish between these two bodies the primitive proportions, which are more proper than any other for the success of the operation. In all cases the management of the fire is of the greatest importance: if it be too weak, no etherized gas is produced; if too strong, but very little is produced. Besides, we do not etherize the alcohol sensibly by charging it with muriatic acid gas, nor do we obtain ether more sensibly

* The centigrade thermometer is the one intended.—Edit.

sibly by bringing together the acid and the alcohol in vapours into a tube about the temperature of about 80°. It is therefore by preserving a just medium in the application of the fire that we succeed completely. All this proceeds from too small or too great an elasticity in the alcohol, and the muriatic acid prevents their reaction upon each other. One precaution we must also take, is to use the same water for collecting the gas, and to employ the least quantity possible, because it dissolves it in a remarkable degree.

This gas is absolutely colourless; the smell of it is strongly etherized, and the taste sensibly saccharine. It has no kind of action either upon turnsole tincture, syrup of violets, or lime-water. Its specific gravity compared to that of air is 2·219 to + 18° of the centigrade thermometer, and at 0^{m} 75 of pressure at the same temperature, and at the same pressure water dissolves its own volume of it. At this same degree of pressure also, but at + 11° of temperature, the etherized gas becomes liquid. We may procure a great quantity of it in this state, by using an apparatus similar to that we have just described; simply, in place of fixing the last tube under a flask full of water, we must plunge it to the bottom of a long, straight, well dried probe, and surrounded with ice, which we must renew in proportion as it melts. It is in this probe that the etherized gas alone arrives and is entirely liquefied; for, when once the vessels contain no more air, we may without the least danger suppress its communication with the atmosphere.

When thus liquefied, this ether is of a remarkable limpidity, as in the state of gas it is without colour and without action upon turnsole tincture and syrup of violets; as well as the etherized gas, it is very soluble in alcohol, from which we may in a great measure separate it by water: like this gas, it has also a very decided smell, and a very distinct taste, which has something analogous to that of sugar, and which is particularly remarkable in water which is saturated with it, which may perhaps be employed successfully in medicine. When poured upon the hand, it suddenly evaporates and produces a considerable cold, leaving a small whitish residue. At + 5° of temperature (centigrade thermometer) it weighs 874, water weighing 1000. Thus, although it is far more volatile than the sulphuric ether, and, *a fortiori*, than alcohol, not only is it thicker than the former, but even than the latter of these two bodies. Lastly, it does not congeal at a temperature of — 29° (centigrade thermometer).

Hitherto we have seen nothing in this ether which does not perfectly agree with that presented by other matters; it is nothing else than a substance curious from its novelty, and particularly from the facility with which it is gasified and liquefied. When we reflect upon it a little more, it appears one of the most singular and extraordinary compounds we can produce. It does not in the least redden turnsole tincture; the strongest alkalis have no action upon it; the solution of silver does not meddle with it at all; and all this, whether we employ it in the gaseous or liquid state, or dissolved in water: if we set fire to it, there is suddenly developed such a quantity of muriatic acid, that this acid precipitates in a mass the concentrated nitrate of silver, suffocates those who respire it, and even appears in the form of vapours in the surrounding air.

Is the muriatic acid formed in this inflammation, as we are induced to think, or is it only set at liberty? This is the question which the author of the memoir afterwards endeavours to resolve.

If the muriatic acid be formed in the combustion of the etherized gas, the radical of this acid must exist in the gas; and this radical necessarily comes from the alcohol, or from the muriatic acid decomposed by the alcohol, or, what is not very probable, although not impossible, from both. In the first case, by distilling a mixture of alcohol and muriatic acid, we ought to find after the distillation all the muriatic acid employed, besides that which appeared in the combustion of the gas formed; in the second case, on the contrary, a great quantity of acid ought to disappear in this distillation; but by keeping an account of that which is developed in the combustion of the gas formed, this quantity of acid precisely, and no more, ought to reappear entirely. In the third case, from this distillation a loss of acid should

also

also result; but this loss should be more than compensated by the quantity of acid which the combustion of the gas formed ought to produce. Now, on performing this distillation upon 450·937 grammes of muriatic acid of a specific gravity of 11·349, at 5° temperature (centigrade thermometer), and upon a volume of highly rectified alcohol equal to that of the acid, there are formed 23 litres of etherized gas at the temperature of 21° of the centigrade thermometer, and at the pressure of 0^m 745, and there disappear 122·288 grammes of acid.

The first hypothesis is consequently false, since it is demonstrated that, even should the radical of the muriatic acid exist in the etherized gas, this radical would proceed not merely from the alcohol, but rather either from the muriatic acid alone, or from the muriatic acid and alcohol.

Let us inquire if it proceeds from the muriatic acid alone, as supposed in the second hypothesis: but here there are two ways of considering the phænomenon: either the muriatic acid must have been decomposed by the alcohol, so that its radical, without its other principle, is to be found in the etherized gas; or this decomposition will have been such, that all the principles of the muriatic acid will be found in the etherized gas, not united, and not forming muriatic acid, but combined with the principles of the alcohol, and in the same state in which hydrogen, oxygen, carbon, and azot exist in animal or vegetable matters. Now if the radical of the muriatic acid exists alone in the other principle, or without a portion of the other principle of the muriatic acid in the etherized gas, we ought, by decomposing this gas in a red-hot tube, and deprived of the contact of the air, to obtain no acid at all, or else less of it than has disappeared in the experiment which produced it: and if this gas contains not only the radical of the muriatic acid, but also all the constituent principles of that acid; as those principles, whatever they are, have a strong tendency to combine, we should conceive that by destroying the etherized gas by fire, without the contact of the air, we shall probably obtain the whole quantity of muriatic acid which has disappeared in the experiment by which we procured it. It was therefore

therefore of the greatest importance to produce this decomposition in close vessels. The operation was performed on 900 grammes of concentrated muriatic acid, and upon an equal volume of well rectified alcohol. Between the red-hot glass tube, in which the gas was decomposed, and the retort in which it was produced, there was a large tubulated flask containing water at about 15° or 16°, in order to catch the acid, the alcohol and the water, which might be volatilized along with this gas; the glass tube, besides, communicated with two other flasks, one containing potash and the other water, in order to absorb all the acid which might re-appear in the operation: finally, the gases were collected by means of another tube. To ensure the success of the operation, the glass tube should be well luted, and the fire also well managed to prevent the tube from melting. Although in this experiment there ought to have been produced nearly 30 litres of etherized gas, and nearly 250 grammes of acid ought to have disappeared in the first place, yet all the acid except four grammes re-appeared in the red-hot tube, and came to be dissolved in the two flasks of the apparatus. Thus, of all the suppositions hitherto formed of the muriatic acid being a compound body, one only is admissible, which infers that the elements of the muriatic acid exist in the etherized gas combined with the elements of the alcohol, in the same manner as the elements of water, carbonic acid, ammonia, &c., exist in vegetable and animal substances.

Now if we suppose the muriatic acid to be a simple body, we must then necessarily regard etherized gas as formed of muriatic acid and alcohol, or of a body coming from the decomposition of alcohol; for alcohol is perhaps decomposed when we distil it with the muriatic acid. In all cases, the question is thus brought to a choice of the two hypotheses. Let us now try their merits as well as we can.

One, namely that which we have last mentioned, presents us with phænomena of difficult explanation. In fact, it must be supposed that alcohol, or the body which represents it, acts upon the muriatic acid with much more energy than the strongest alkali, since this alkali cannot

take

take it off from it, and since, as will be subsequently demonstrated, the muriate of potash contains less acid than the etherized gas; and how can we conceive, on the other hand, that the nitrate of silver, which takes up all the muriatic acid from the muriate of potash, cannot take it up from the etherized gas, which contains more than the muriate of potash does?

In the other hypothesis, on the contrary, every thing is naturally explained. We see the reason why the etherized gas does not redden turnsole tincture, and the alkalis do not alter it; why the nitrate of silver produces no precipitate from it; and why, on taking fire, so large a quantity of muriatic acid is produced, that it appears in the air in the form of vapours:—in short, we can reconcile every thing with the phænomena of other bodies.

M. Thenard, however, is far from admitting the one and rejecting the other in a decided manner. Both deserve to be investigated; and whatever happens, the results must be very important.

*Note upon the Discovery of the Muriatic Ether. By M. Thenard**.

When I read the foregoing memoir to the Institute, the whole of the members, among whom were Messrs. Berthollet, Chaptal, Devoux, Fourcroy, Guyton, Vauquelin, Gay-Lussac, &c., regarded the results it contained as being extremely novel, and were struck with the consequences which might be drawn from it. M. Proust, who was then in Paris, and before whom I repeated the experiment of testing the etherized gas by the tincture of turnsole and the nitrate of silver, before and after the combustion of the gas, &c., evinced the same surprise with the French chemists. A few days afterwards, however, M. Gay-Lussac, on perusing the German Journal of Gehlen, accidentally discovered in a note that Gehlen himself had made experiments upon the muriatic ether, and published them in his Journal for 1804. M. Gay-Lussac did me the favour to

* *Ann. de Chimie*, tom. lxi. p. 303.

translate

translate M. Gehlen's memoir; and as it has a great similarity to mine, I shall give the following extract:

M. Gehlen made muriatic ether with the smoking muriate of tin and alcohol, by employing an equal part in weight of both. He also made it in Basse's method (a chemist of Hameln), by a mixture of sea-salt, concentrated sulphuric acid and alcohol, from which it was never thought that any thing else than sulphuric ether could be extracted. He obtained none with the muriatic acid alone. In short, M. Gehlen recognised most of the properties which I did in the muriatic ether. Thus he saw that this ether is most frequently in the state of gas, that it liquefies at about + 10° of Reaumur, that it is slightly soluble in water, that it has a saccharine taste, that it does not redden turnsole tincture, that it does not precipitate the nitrate of silver, and that when we burn it a great quantity of muriatic acid is developed. M. Gehlen made no experiment, however, to prove from whence this muriatic acid came, or to ascertain the quantity produced by the etherized gas; neither did he attempt to establish the theory of this etherification. It is in this respect only that my labours differ from his. We differ a little also, but not remarkably, as to the process I employed for making the muriatic ether, and by means of which I obtained in an instant probably more ether than by any other, and an ether purer also than M. Gehlen's, since his weighed 845 only, and mine 874, a greater specific gravity in the present instance being a proof of greater purity.

Having no doubt, from the above, that the muriatic ether had been made in Germany, and that its property of producing muriatic acid when burning was also known; and convinced as I was on the other hand, that in France and Spain the fact was unknown, I endeavoured to ascertain if the English chemists knew any thing of the matter. For this purpose I applied to M. Riffault, the superintendant of the gunpowder manufactories, and who was at that moment translating the third edition of Thomson's System of Chemistry, a work full of erudition, and begun long after

M. Gehlen's

M. Gehlen's memoir appeared. M. Riffault read to me every thing respecting the muriatic ether, but I found nothing about Gehlen, or the singular properties he relates of the muriatic ether. Mr. Thomson only speaks of the process of Basse, which consists in mixing melted sea-salt, alcohol, and sulphuric acid; and which, with the exception of the fusion of the salt, has been pointed out by several chemists. I think myself therefore entitled to conclude, that in Great Britain, as well as in France and Spain, the muriatic ether was unknown, and that, being ignorant of M. Gehlen's labours, I have at least the merit of publishing it. How often does it happen that a discovery is made in one country, which had been known in another long before! and this happens because all learned men do not speak one language, and their works are not always translated. This is the case with M. Gehlen's discovery*.

XXIV. *Memoirs of the late* Erasmus Darwin, *M.D.*

[Continued from vol. xxix. p. 339.]

DARWINIANA.

In Doctor Darwin's First Class, Ord. I. Gen. 1, mentioning *arterial hæmorrhage*, he suggests the breathing an air with less of oxygen. In the *hæmoptoe of arterial blood*, he proposes, besides the reduced air, making the patient sick by whirling round in a chair suspended by a rope; actual vomiting, a practice we believe first introduced by the famous Dr. Willis; bathing in cold water, or sudden immersion of some of the limbs, or sprinkling the whole body with cold water. For the *hæmoptoe narium*, bleeding of the nose, he advises plunging the head into cold water with powdered salt hastily dissolved in it: lint strewed with wheat flour

* Since writing the above, M. Boullay, a respectable apothecary in Paris, informed me that he made the ether in question with the muriatic acid and alcohol long ago, although he never gave publicity to his experiments, because he did not think they were so complete as they ought to have been. I am proud to have an opportunity of doing justice to M. Boullay.

Note by M. Thenard.

put

put up the nostrils; or a solution of steel in brandy applied to the vessel by means of lint.

Arriving at Gen. 2, mentioning *sudor calidus*, warm sweat, he has the following curious observations on the use of calico and flannel:

"If an excess of perspiration is induced by warm or stimulant clothing, as by wearing flannel in contact with the skin in the summer months, a perpetual febricula is excited, both by the preventing the access of cool air to the skin, and by perpetually goading it by the numerous and hard points of the ends of the wool; which, when applied to the tender skins of young children, frequently produce the red gum, as it is called; and in grown people, either an erysipelas, or a miliary eruption, attended with fever.

"Shirts made of cotton or calico stimulate the skin too much by the points of the fibres, though less than flannel; whence cotton handkerchiefs make the nose sore by frequent use. The fibres of cotton are, I suppose, ten times shorter than those of flax, and the number of points in consequence twenty times the number; and though the manufacturers singe their calicoes on a red-hot iron cylinder, yet I have more than once seen an erysipelas induced or increased by the stimulus of calico, as well as of flannel.

"The increase of perspiration by heat, either of clothes or of fire, contributes much to emaciate the body; as is well known to jockeys, who, when they are a stone or two too heavy for riding, find the quickest way to lessen their weight is by sweating themselves between blankets in a warm room: but this likewise is a practice by no means to be recommended, as it weakens the system by the excess of so general a stimulus, brings on a premature old age, and shortens the span of life; as may be further deduced from the quick maturity and shortness of the lives of the inhabitants of Hindostan and other tropical climates.

"M. Buffon made a curious experiment to show this circumstance. He took a numerous brood of the butterflies of silkworms, some hundreds of which left their eggs on the same day and hour; these he divided into two parcels; and placing one parcel in the south window, and the other in the

the north window of his house, he observed, that those in the colder situation lived many days longer than those in the warmer one. From these observations it appears, that the wearing of flannel clothing next the skin, which is now so much in fashion, however useful it may be in the winter to those who have cold extremities, bad digestions, or habitual coughs, must greatly debilitate them, if worn in the warm months, producing fevers, eruptions, and premature old age."

He makes the following ingenious and unfortunately too true observations respecting the *diarrhœa infantum*, gripes in infants:

"Milk is found curdled in the stomachs of all animals, old as well as young, and even of carnivorous ones, as of hawks (Spallanzani). And it is the gastric juice of the calf which is employed to curdle milk in the process of making cheese. Milk is the natural food for children, and must curdle in their stomachs previous to digestion; and as this curdling of the milk destroys a part of the acid juices of the stomach, there is no reason for discontinuing the use of it, though it is occasionally ejected in a curdled state. A child of a week old, which had been taken from the breast of its dying mother, and had by some uncommon error been suffered to take no food but water gruel, became sick and griped in twenty-four hours, and was convulsed on the second day, and died on the third! When all young quadrupeds, as well as children, have this natural food of milk prepared for them, the analogy is so strong in favour of its salubrity, that a person should have powerful testimony indeed of its disagreeing, before he advises the discontinuance of the use of it to young children in health, and much more so in sickness. The farmers lose many of their calves, which are brought up by gruel, or gruel and old milk; and among the poor children of Derby, who are thus fed, hundreds are starved into the scrophula, and either perish, or live in a state of wretched debility.

"When young children are brought up without a breast, they should for the first two months have no food but new milk; since the addition of any kind of bread or flour is liable to ferment, and produce too much acidity, as appears by the consequent diarrhœa with green dejections and gripes; the colour is owing to a mixture of acid with the natural quantity of bile, and the pain to its stimulus. And they should never be fed as they lie upon their backs, as in that posture they are necessitated to swallow all that is put into their mouths; but when they are fed as they are sitting up, or raised up, when they have had enough they can permit the rest to run out of their mouths. This circumstance is of great importance to the health of those children who are reared by the spoon, since, if too much food is given them, indigestion, and gripes, and diarrhœa, are the consequence; and if too little, they become emaciated; and of this exact quantity their own palates judge the best."

His observations are no less curious respecting *perspiratio fœtida*, fetid perspiration:

"The uses of the perspirable matter are to keep the skin soft and pliant, for the purposes of its easier flexibility during the activity of our limbs in locomotion, and for the preservation of the accuracy of the sense of touch, which is diffused under the whole surface of it to guard us against the injuries of external bodies; in the same manner as the secretion of tears is designed to preserve the cornea of the eye moist, and in consequence transparent: yet has this cutaneous mucus been believed by many to be an excrement; and I know not how many fanciful theories have been built on its supposed obstruction. Such as the origin of catarrhs, coughs, inflammations, erysipelas, and herpes.

"To all these it may be sufficient to answer, that the antient Grecians oiled themselves all over; that some nations have painted themselves all over, as the Picts of this island; that the Hottentots smear themselves all over with grease. And lastly, that many of our own heads at this day are covered with the flour of wheat and the fat of hogs, according to the tyranny of a filthy and wasteful fashion, and all this without inconvenience. To this must be added the strict analogy between the use of the perspirable matter and the mucous fluids, which are poured for similar purposes upon all the internal membranes of the body; and besides its

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being in its natural state inodorous; which is not so with the other excretions of fæces, or of urine.

"In some constitutions the perspirable matter of the lungs acquires a disagreeable odour; in others the axilla, and in others the feet, emit disgustful effluvia; like the secretions of those glands, which have been called odoriferæ; as those which contain the castor in the beaver, and those within the rectum of dogs, the mucus of which has been supposed to guard them against the great costiveness which they are liable to in hot summers; and which has been thought to occasion canine madness; but which, like their white excrement, is more probably owing to the deficient secretion of bile. Whether these odoriferous particles attend the perspirable matter in consequence of the increased action of the capillary glands, and can properly be called excrementitious; that is, whether any thing is eliminated, which could be hurtful if retained; or whether they may only contain some of the essential oil of the animal; like the smell which adheres to one's hand on stroking the hides of some dogs; or like the effluvia, which is left upon the ground, from the feet of men and other creatures; and is perceptible by the nicer organs of the dogs, which hunt them, may admit of doubt.

"M. M. Wash the parts twice a day with soap and water; with lime water; cover the feet with oiled silk socks, which must be washed night and morning. Cover them with charcoal recently made red-hot, and beaten into fine powder and sifted, as soon as cold, and kept well corked in a bottle, to be washed off and renewed twice a day. Internally rhubarb grains vi. or viii. every night, so as to procure a stool or two extraordinary every day, and thus by increasing one evacuation to decrease another."

He gives a cure for the *tape-worm.*

"The tape-worm is cured by an amalgama of tin and quicksilver, such as is used on the back of looking-glasses; an ounce should be taken every two hours, till a pound is taken; and then a brisk cathartic, of Glauber's salt two ounces, and common salts one ounce, dissolved in two wine-pints of water, half a pint to be taken every hour till

 it

it purges. The worm extends from the stomach to the anus, and the amalgama tears it from the intestine by mechanical pressure, acting upon it the whole way. Electric shocks through the duodenum greatly assist the operation. Large doses of tin in powder. Iron filings in large doses. The powder of fern-root seems to be of no use, as recommended by M. Noufflier."

He makes the following judicious observations on the cure of the *ascarides*, or thread-worm:

"Ascarides are said to be weakened by twenty grains of cinnabar and five of rhubarb taken every night, but not to be cured by this process. As these worms are found only in the rectum, variety of clysters have been recommended. I was informed of a case, where solutions of mercurial ointment were used as a clyster every night for a month without success. Clysters of Harrowgate water are recommended, either of the natural, or of the factitious, as described below, which might have a greater proportion of liver of sulphur in it. As the cold air soon destroys them, after they are voided, could clysters of iced water be used with advantage? or of spirit of wine and water? or of ether and water? Might not a piece of candle, about an inch long, or two such pieces, smeared with mercurial ointment, and introduced into the anus at night, or twice a day, be effectual by compressing their nidus, as well as by the poison of the mercury?

"The clysters should be large in quantity, that they may pass high in the rectum, as two drams of tobacco boiled a minute in a pint of water. Or, perhaps, what might be still more efficacious and less inconvenient, the smoke of tobacco injected by a proper apparatus every night, or alternate nights, for six or eight weeks. This was long since recommended, I think, by Mr. Turner of Liverpool; and the reason it has not succeeded, I believe to have been owing to the imperfections of the joints of the common apparatus for injecting the smoke of tobacco, so that it did not pass into the intestine, though it was supposed to do so, as I once observed. The smoke should be received from the apparatus into a large bladder; and it may then be certainly in-

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jected like the common clyster with sufficient force; otherwise oiled leathers should be nicely put round the joints of the machine, and a wet cloth round the injecting pipe, to prevent the return of the smoke by the sides of it. Clysters of carbonated hydrogen gas, or of other factitious airs, might be tried.

"Harrowgate water taken into the stomach, so as to induce six or seven stools every morning, for four or six weeks, is perhaps the most efficacious method in common use. A factitious Harrowgate water may be made probably of greater efficacy than the natural, by dissolving one ounce of marine salt, (called bay salt,) and half an ounce of magnesia Glauber's salt, (called Epsom salt, or bitter purging salt,) in twenty-eight ounces of water. A quarter or half a pint of this is to be taken every hour or two hours in the morning, till it operates, with a tea-spoonful of a solution of liver of sulphur, which is to be made by putting an ounce of hepar sulphuris into half a pint of water."

[To be continued.]

XXV. *Experiments for investigating the Cause of the coloured concentric Rings, discovered by Sir* Isaac Newton, *between two Object-glasses laid upon one another. By* William Herschel, *LLD. F.R.S.*

[Continued from p. 90.]

XV. *Of the sudden Change of the Size and Colour of the Rings in different Sets.*

When two sets of rings are viewed which are dependent upon each other, the colour of their centres and of all the rings in each set, may be made to undergo a sudden change by the approach of the shadow of the point of a penknife or other opake slender body. To view this phænomenon properly, let a 16-inch double convex lens be laid upon a piece of looking-glass, and when the contact between them has been made to give the primary set with a black centre, that of the secondary will be white. To keep the lens in this

 contact,

contact, a pretty heavy plate of lead with a circular hole in it of nearly the diameter of the lens should be laid upon it. The margin of the hole must be tapering, that no obstruction may be made to either the incident or reflected light. When this is properly arranged, bring the third shadow of the penknife upon the primary set, which is that towards the light. The real colours of this and the secondary set will then be seen to the greatest advantage. When the third shadow is advanced till it covers the second set, the second shadow will at the same time fall upon the first set, and the colour of the centres, and of all the rings in both sets, will undergo a sudden transformation from black to white and white to black.

The alternation of the colour is accompanied with a change of size, for as the white rings before the change were of a different diameter from the black ones, these latter, having now assumed a black colour, will be of a different size from the former black ones.

When the weight is taken from the lens the black contact will be changed into some other. In the present experiment it happened that the primary set got an orange-coloured centre, and the secondary a green one. The same way of proceeding with the direction of the shadow being then pursued, the orange centre was instantly changed to a green one, while at the same moment the green centre was turned into orange. With a different contact I have had the primary set with a blue centre and the secondary with a deep yellow one; and by bringing the second and third shadows alternately over the primary set, the blue centre was changed to a yellow, and the yellow centre to a blue one; and all the rings of both sets had their share in the transformation of colour and size.

If there are three sets of rings, and the primary set has a black centre, the other two will have a white one; and when the lowest shadow is made to fall on the third set, the central colour of all the three sets will be suddenly changed, the first from black to white, the other two from white to black.

A full explanation of these changes, which at first sight

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have the appearance of a magical delusion, will be found in a future article.

XVI. *Of the Course of the Rays by which different Sets of Rings are seen.*

In order to determine the course of the rays, which give the rings both by reflection and by transmission, we should begin from the place whence the light proceeds that forms them. In figure 1, (Pl. VI.) we have a plano-convex lens laid upon three slips of glass, under which a metalline mirror is placed. An incident ray, 1, 2, is transmitted through the first and second surface of the lens, and comes to the point of contact at 3. Here the rings are formed, and are both reflected and transmitted: they are reflected from the upper surface of the first slip, and pass from 3 to the eye at 4: they are also transmitted through the first slip of glass from 3 to 5; and at 5 they are again both reflected and transmitted; reflected from 5 to 6, and transmitted from 5 to 7; from 7 they are reflected to 8, and transmitted to 9; and lastly they are reflected from 9 to 10. And thus four complete sets of rings will be seen at 4, 6, 8, and 10.

The most convenient way of viewing the same rings by transmission, is that which has been mentioned in the second article of this paper, when light is conveyed upwards by reflection. In figure 2, consisting of the same arrangement of glasses as before, the light by which the rings are to be seen comes either from 1, 2, or 3, or from all these places together, and being reflected at 4, 5, and 6, rises up by transmission to the point of contact at 7, where the rings are formed. Here they are both transmitted up to the eye at 8, and reflected down to 9; from 9 they are reflected up to 10, and transmitted down to 11; from 11 they are reflected to 12, and transmitted to 13; and lastly, from 13 they are reflected to 14; so that again four sets of rings will be seen at 8, 10, 12, and 14.

This being a theoretical way of conceiving how the rays of light may produce the effects, it will be required to show by experiments that this is the actual progress of the rays, and that all the sets of rings we perceive are really reflected or transmitted in the manner that has been pointed out; but as we have so many reflections and transmissions before us, it will be necessary to confine these expressions to one particular signification when they are applied to a set of rings.

When the centre of the rings is seen at the point of contact, it is a primary set; and I call it reflected, when the rays which come to that point and form the rings undergo an immediate reflection. But I call it transmitted, when the rays after having formed the rings about the point of contact are immediately transmitted.

Thus in figure 3 and 4 the rays *a b c, d e f,* give reflected sets of rings; and the rays *g h i, k l m,* in figure 5 and 6, give transmitted sets.

In this denomination, no account is taken of the course of the rays before they come to *a, d, g, k;* nor of what becomes of them after their arrival at *c, f, i, m:* they may either come to those places or go from them by one or more transmissions or reflections, as the case may require; but our donomination will relate only to their course immediately after the formation of the rings between the glasses.

The secondary and other dependent sets will also be called reflected or transmitted by the same definition: and as a set of these rings formed originally by reflection may come to the eye by one or more subsequent transmissions; or being formed by transmission, may at last be seen by a reflection from some interposed surface, these subsequent transmissions or reflections are to be regarded only as convenient ways to get a good sight of them.

With this definition in view, and with the assistance of a principle which has already been proved by experiments, we may explain some very intricate phænomena; and the satisfactory manner of accounting for them will establish the truth of the theory relating to the course of rays that has been described.

The principle to which I refer is, that when the pressure is such as to give a black centre to a set of rings seen by reflection,

reflection, the centre of the same set, with the same pressure of the glasses, seen by transmission, will be white *.

I have only mentioned black and white; but any other alternate colours, which the rings or centres of the two sets may assume, are included in the same predicament.

XVII. *Why two connected Sets of Rings are of alternate Colours.*

It has already been shown, when two sets of rings are seen, that their colours are alternate, and that the approach of the shadow of a penknife will cause a sudden change of them to take place. I shall now prove that this is a very obvious consequence of the course of rays that has been proposed. Let figure 7 and 8 represent the arrangement given in a preceding article, where a 16-inch lens was laid upon a looking-glass, and gave two sets of rings with centres of different colours: but let figure 7 give them by one set of rays, and figure 8 by another. Then, if the incident rays come in the direction which is represented in figure 7, it is evident that we see the primary set with its centre at 2 by reflection, and the secondary one at 4 by transmission. Hence it follows, in consequence of the admitted principle, that if the contact is such as to give us the primary set with a black centre, the secondary set must have a white one; and thus the reason of the alternation is explained.

But if the rays come as represented in figure 8, we see the primary set by transmission, and the secondary one by reflection; therefore, with an equal pressure of the glasses, the primary centre must now be white, and the secondary one black.

Without being well acquainted with this double course of rays, we shall be liable to frequent mistakes in our estimation of the colour of the centres of two sets of rings; for by a certain position of the light, or of the eye, we may see one set by one light and the other set by the other.

XVIII. *Of the Cause of the sudden Change of the Colours.*

Having thus accounted for the alternation of the central

* See Article XI. of this Paper.

colours, we may easily conceive that the interposition of the penknife must have an instantaneous effect upon them. When it stops the rays of figure 7, which will happen when its second shadow falls upon the primary set, the rings will then be seen by the rays 1, 2, 3, 4, and 1, 2, 3, 5, 6, of figure 8. When it stops the rays of figure 8, which must happen when the third shadow falls upon the primary set, we then see both sets by the rays 1, 2, 3, and 1, 2, 4, 5, of figure 7. When the penknife is quite removed both sets of rays will come to the point of contact, and in some respects interfere with each other; but the strongest of the two, which is generally the direct light of figure 7, will prevail. This affords a complete explanation of all the observed phænomena: by the rays of figure 7 the centres will be black and white; by those of figure 8 they will be white and black; and by both we shall not see the first set so well as when the third shadow being upon it has taken away the rays of figure 8: indeed we can hardly see the secondary set at all, till the shadow of the penknife has covered either the rays of figure 7 or of figure 8.

As soon as we are a little practised in the management of the rays, by knowing their course, we may change the colour so gradually as to have half the centre white while the other half shall still remain black; and the same may be done with green and orange, or blue and yellow centres. The rings of both sets will also participate in the gradual change; and thus what has been said of the course of rays in the 16th article will again be confirmed.

XIX. *Of the Place where the different Sets of Rings are to be seen.*

By an application of the same course of the rays, we may now also determine the situation of the place where the different sets of rings are seen: for according to what has been said in the foregoing article, the situation of the primary set should be between the lens and the surface of the looking-glass: and the place of the secondary one at the metalline coating of the lowest surface. To try whether this be actually as represented, let us substitute a metalline mirror with

with a slip of glass laid upon it in the room of the piece of looking-glass; and let there be interposed a short bit of wood, one-tenth of an inch thick, between the slip of glass and the mirror, so as to keep up that end of the slip which is towards the light. This arrangement is represented in figure 9, where both sets of rays are delineated. Then if we interpose a narrow tapering strip of card, discoloured with japan ink, between the slip of glass and the mirror, so as to cover it at 7, we do not only still perceive the primary set, but see it better than before: which proves that, being situated above the slip of glass, the card below cannot cover it. If on the contrary we insert the strip of card far enough, that it may at the same time cover the mirror both at 4 and at 7, we shall lose the secondary set; which proves that its situation was on the face of the mirror.

When several sets of rings are to be perceived by the same eye-glass, and they are placed at different distances, a particular adjustment of it will be required for each set, in order to see it well defined. This will be very sensible when we attempt to see three or four sets, each of them situated lower than the preceding; for without a previous adjustment to the distance of the set intended to be viewed we shall be seldom successful: and this is therefore a corroborating proof of the situation that has been assigned to different sets of rings.

XX. *Of the Connection between different Sets of Rings.*

It will now be easy to explain in what manner different sets of rings are connected, and why they have been called primary and dependent. When the incident rays come to the point of contact and form a set of rings, I call it a primary one: when this is formed some of the same rays are continued by transmission or reflection, but modified so as to convey an image of the primary set with opposite colours forward through any number of successive transmissions or reflections: whenever this image comes to the eye, a set of rings will again be seen, which is a dependent one. Many proofs of the dependency of second, third, and fourth

sets

sets of rings upon their primary one may be given;—I shall only mention a few.

When two sets of rings are seen by a lens placed upon a looking-glass, the centre of the secondary set will always remain in the same plane with the incident and reflected rays passing through the centre of the primary one. If the point of contact, by tilting is changed, the secondary set will follow the motion of the primary set; and if the looking-glass is turned about, the secondary will be made to describe a circle upon that part of the looking-glass which surrounds the primary one as a centre. If there is a defect in the centre or in the rings of the primary set, there will be exactly the same defect in the secondary one; and if the rays that cause the primary set are eclipsed, both sets will be lost together. If the colour of the primary one is changed, that of the secondary will also undergo its alternate change, and the same thing will hold good of all the dependent rings when three or four sets of them are seen that have the same primary one.

The dependency of all the sets on their primary one may also be perceived when we change the obliquity of the incident light; for the centres of the rings will recede from one another when that is increased and draw together when we lessen it, which may go so far that by an incidence nearly perpendicular we shall bring the dependent sets of rings almost under the primary one.

XXI. *To account for the Appearance of several Sets of Rings with the same coloured Centres.*

It has often happened that the colour of the centres of different sets was not what the theory of the alternation of the central colours would have induced me to expect: I have seen two, three, and even four sets of rings, all of which had a white centre. We are however now sufficiently prepared to account for every appearance relating to the colour of rings and their centres.

Let an arrangement of glasses be as in figure 9. When this is laid down so as to receive an illumination of day-light,

light, which should not be strong, nor should it be very oblique, the reflection from the mirror will then exceed that from the surface of glass; therefore the primary set will be seen by the rays 6, 7, coming to the mirror at 7, and going through the point of contact in the direction 7, 2, 3; which proves it to be a set that is seen by transmission, and it will therefore have a white centre. The rays 1, 2, 4, passing through the point of contact, will also form a transmitted set with a white centre, which will be seen when the reflection from 4 to 5 conveys it to the eye. But these two sets have no connection with each other; and as primary sets are independent of all other sets, I have only to prove that this secondary set belongs not to the primary one which is seen, but to another invisible one. This may be done as follows;

Introduce the black strip of card that has been mentioned before, till it covers the mirror at 7; this will take away the strong reflection of light which overpowers the feeble illumination of the rays 1, 2, 3; and the real hitherto eclipsed primary set belonging to the secondary one with a white centre, will instantly make its appearance with a black one. We may alternately withdraw and introduce again the strip of card, and the centre of the primary set will be as often changed from one colour to its opposite, but the secondary set, not being dependent on the rays 6, 7, will not be in the least affected by the change.

If the contact should have been such as to give both sets with orange centres, the introduction of the strip of card will prove that the set which is primary to the other has really a green centre.

Another way of destroying the illusion is to expose the same arrangement to a brighter light, and at the same time to increase the obliquity of the angle of incidence: this will give a sufficient reflection from the surface of the glass to be no longer subject to the former deceptive appearance; for now the centre of the primary set will be black, as it ought to be.

XXII. *Of*

XXII. *Of the reflecting Surfaces.*

The rays of light that form rings between glasses must undergo certain modifications by some of the surfaces through which they pass, or from which they are reflected; and to find out the nature of these modifications, it will be necessary to examine which surfaces are efficient. As we see rings by reflection and also by transmission, I shall begin with the most simple, and show experimentally the situation of the surface that reflects, not only the primary, but also the secondary sets of rings.

Upon a slip of glass, the lowest surface of which was deprived of its polish by emery, I laid an object-glass of 21 feet focal length, and saw a very complete set of rings. I then put the same glass upon a plain metalline mirror, and saw likewise a set of them. They were consequently not reflected from the lowest surface of the subjacent glass or metal.

It will easily be understood, that were we to lay the same object-glass upon a slip of glass emeried on both sides, or upon an unpolished metal, no rings would be seen. It is therefore neither from the first surface of the incumbent object-glass, nor from its lowest, that they are reflected; for if they could be formed without the modification of reflection from the upper surface of a subjacent glass or metal, they would still be seen when laid on rough surfaces; and consequently, the efficient reflecting surface, by which we see primary sets of rings, is that which is immediately under the point of contact.

To see a secondary set of rings by reflection, is only an inversion of the method of seeing a primary one. For instance, when a lens is laid upon a looking-glass, the course of the rays represented in figure 8, will show that the rays 1, 2, 3, 5, 6, by which a secondary set is seen, are reflected about the point of contact at 3, and that the lowest surface of the incumbent lens is therefore the efficient reflecting one; and thus it is proved, that in either case of seeing reflected rings, one of the surfaces that are joined at the point of contact contributes to their formation by a certain modification of reflection.

XXIII. *Of*

XXIII. *Of the transmitting Surfaces.*

It would seem to be almost self-evident, that when a set of rings is seen by transmission, the light which occasions them must come through all the four surfaces of the two glasses which are employed; and yet it may be shown that this is not necessary. We may, for instance, convey light into the body of the subjacent glass through its first surface, and let it be reflected within the glass at a proper angle, so that it may come up through the point of contact, and reach the eye, having been transmitted through no more than three surfaces. To prove this I used a small box, blackened on the inside, and covered with a piece of black pasteboard, which had a hole of about half an inch in the middle. Over this hole I laid a slip of glass with a 56-inch lens upon it; and viewed a set of rings given by this arrangement very obliquely, that the reflection from the slip of glass might be copious. Then guarding the point of contact between the lens and the slip of glass from the direct incident light, I saw the rings, after the colour of their centre had been changed, by means of an internal reflection from the lowest surface of the slip of glass; by which it rose up through the point of contact, and formed the primary set of rings, without having been transmitted through the lowest surface of the subjacent glass. The number of transmitted surfaces is therefore by this experiment reduced to three; but I shall soon have an opportunity of showing that so many are not required for the purpose of forming the rings.

XXIV. *Of the Action of the first Surface.*

We have already shown that two sets of rings may be seen by using a lens laid upon a slip of glass; in which case, therefore, whether we see the rings by reflection or by transmission, no more than four surfaces can be essential to their formation. In the following experiments for investigating the action of these surfaces I have preferred metalline reflection, when glass was not required, that the apparatus might be more simple.

Upon a plain metalline mirror I laid a double convex lens, having

having a strong emery scratch on its upper surface. When I saw the rings through the scratch, they appeared to have a black mark across them. By tilting the lens, I brought the centre of the rings upon the projection of the scratch, so that the incident light was obliged to come through the scratch to the rings, and the black mark was again visible upon them, but much stronger than before. In neither of the situations were the rings disfigured. The stronger mark was owing to the interception of the incident light; but when the rings had received their full illumination the mark was weaker, because in the latter case the rings themselves were probably complete, but in the former deficient.

I placed a lens that had a very scabrous polish on one side, but was highly polished on the other, upon a metalline mirror. The defective side being uppermost, I did not find that its scabrousness had any distorting effect upon the rings.

I splintered off the edge of a plain slip of glass; it broke as it usually does with a waving striated, curved slope coming to an edge. The splintered part was placed upon a convex metalline mirror of 2-inch focus, as in figure 10. The irregularity of the striated surface through which the incident ray 1, 2, was made to pass had very little effect upon the form of the rings; the striæ appearing only like fine dark lines, with hardly any visible distortion; but when, by tilting, the returning ray, 2, 3, was also brought over the striated surface, the rings were much disfigured. This experiment therefore seems to prove that a very regular refraction of light by the first surface is not necessary; for though the rings were much disfigured when the returning light came through the splintered defect, this is no more than what must happen to the appearance of every object which is seen through a distorting medium.

I laid the convex side of a plano-convex lens 2·8-inch focus with a diameter of 1·5 upon a plain mirror, and when I saw a set of rings I tilted the lens so as to bring the point of contact to the very edge of the lens, both towards the light and from the light, which, on account of the large diameter of the lens, gave a great variety in the angle of incidence to the rays which formed the rings; but no diffe-

rence

rence in their size or appearance could be perceived. This seems to prove that no modification of the first surface in which the angle of incidence is concerned, such as refraction and dispersion, has any share in the production of the rings, and that it acts merely by the intromission of light; and though even this is not without being influenced by a change of the angle, it can only produce a small difference in the brightness of the rings.

A more forcible argument, that leads to the same conclusion, is as follows: Laying down three 54-inch double convex lenses, I placed upon the first the plain side of a plano-convex lens of $\frac{5}{8}$ inch focus; upon the second, a plain slip of glass; and upon the third, the plain side of a plano-concave lens also $\frac{5}{8}$ inch focus. I had before tried the same experiment with glasses of a greater focal length, but selected these to strengthen the argument. Then, as nothing could be more different than the refraction of the upper surfaces of these glasses, I examined the three sets of rings that were formed by these three combinations, and found them so perfectly alike that it was not possible to perceive any difference in their size and colour. This shows that the first surface of the incumbent glasses merely acts as an inlet to the rays that afterwards form the rings.

To confirm the idea that the mere admission of light would be sufficient, I used a slip of glass polished on one side but roughened with emery on the other: this being laid upon a 21-feet object-glass, I saw a set of rings through the rough surface; and though they appeared hazy, they were otherwise complete in figure and colour. The slip of glass when laid in the same manner upon the letters of a book made them appear equally hazy; so that the rings were probably as sharply formed as the letters.

Having now already great reason to believe that no modification, that can be given by the first surface to the incident rays of light, is essential to the formation of the rings, I made the following decisive experiment:

Upon a small piece of looking-glass I laid half a double convex lens of 16-inches focus, with the fracture exposed to the light, as represented in figure 11. Under the edge of the

the perfect part of the lens was put a small lump of wax, soft enough to allow a gentle pressure to bring the point of contact towards the fractured edge, and to keep it there. In this arrangement it has already been shown that there are two different ways of seeing two sets of rings: by the rays 1, 2, 3, we see a primary set; and by 1, 2, 4, 5, the secondary set belonging to it: by the rays 6, 7, 2, 3, we see a different primary set; and by 6, 7, 2, 4, 5, we see its secondary one. That this theory is well founded has already been proved; but if we should have a doubt remaining, the interposition of any small opaque object upon the looking-glass near the fracture will instantly stop the latter two sets of rings, and show the alternate colours of the two sets that will then be seen by the rays 1, 2, 3, and 1, 2, 4, 5. Remove in the next place the stop from the looking-glass, and bring the second shadow of the penknife over the primary set, and there will then only remain the two sets of rings formed by incident rays which come from 6, and which have never passed through the upper surface of the lens. Now, as both sets of rings in this case are completely formed by rays transmitted upwards from the coated part of the looking-glass without passing through the first surface of the incumbent lens, the proof that the modifying power of that surface is not required to the formation of the rings is established.

It can hardly be supposed that the first surface of the lens should have any concern in the formation of the rings when the rays are reflected from the looking-glass towards the eye; but the same experiment, that has proved that this surface was not required to be used with incident rays, will show that we may do without it when they are on their return. We need only invert the fractured lens, as in figure 12, when either the rays 1, 2, 4, 5, or 6, 7, 2, 4, 5, will convey the image of the rings after their formation to the eye without passing through any part of the lens.

[To be continued.]

XXVI. *Ob-*

XXVI. *Observations upon Sulphurous Mineral Waters.* *By M.* Westrumb*.

M. Westrumb has been employed in making researches upon several kinds of sulphurous waters, and latterly upon those of Eilsen in the county of Schaumbourg.

One of the most interesting facts he has discovered is, that all sulphurous waters contain a greater or less quantity of hydro-sulphuret of lime.

In order to ascertain it, he boiled the mineral water excluded from the contact of the air, in order to expel from it the sulphuretted hydrogen gas and the carbonic acid.

He afterwards poured into the residue—

1st, Sulphuric acid, which liberated sulphuretted hydrogen gas from it; and sulphat of lime was precipitated.

2d, Smoking nitric acid, which separated sulphur from it.

3d, Oxalic acid, which liberated sulphuretted hydrogen gas from it; and oxalate of lime was formed.

4th, The water evaporated with the contact of the air, precipitated sulphat of lime; and the sulphuretted hydrogen gas was liberated.

In order to determine rigorously the quantity of the sulphuretted hydrogen gas and of the carbonic acid, M. Westrumb proceeds in the following manner:—He introduces sulphurous water into a matrass to a certain point, putting a mark upon the level of the liquid; he adapts a curved tube to it, entering into a long cylinder, which is at one time filled with lime-water, and another time with acetate of lead, with excess of acetic acid.

The apparatus being thus arranged and well luted, he boils the water, and continues the ebullition until there is no more gas liberated.

In the first experiment it is the carbonat of lime which is precipitated, 20 grains of which correspond to ten cubic inches of carbonic acid; and in the second case it is oxidated hydro-sulphuret of lead, 19 grains of which indicate ten cubic inches of sulphuretted hydrogen gas.

* From M. Gehlen's new Journal of Chemistry, vol. v.

Another observation equally remarkable concerns the sulphuretted hydrogen gas.

M. Gimbernat, a Spanish chemist, has asserted, that the hot springs of Aix-la-Chapelle contain sulphuretted azotic gas.—M. Schaub attempted to extract it from the sulphurous waters of Nenndorf in Hesse. The following characters have been attributed to this gas:—1st, A smell similar to the sulphuretted hydrogen gas; 2d, Being indecomposable by the carbonic acid gas; 3d, Not being inflammable; 4th, Being improper for supporting an inflamed body; 5th, Being indecomposable by the nitrous gas; 6th, Being decomposed by the concentrated nitric acid, which separates sulphur from it; 7th, Decomposing the metallic solutions, and forming sulphurets; 8th, Having a great affinity for water, from which it cannot be separated, except by long ebullition.

But M. Westrumb has found that when we wash sulphuretted hydrogen gas with lime-water, or when we pass a current of this gas through slaked lime, it acquires all the above properties.

Whether the sulphuretted hydrogen gas be obtained from sulphurous waters, or be prepared in any other way in use, the same phænomena take place.

On bringing back the lime-water by an acid, sulphuretted hydrogen gas is liberated, which is inflammable, and which possesses its ordinary properties.

Sulphuretted azotic gas is therefore a product of the operation.

M. Westrumb does not yet know if this new gas be produced by the action of quick-lime upon sulphuretted hydrogen gas, or if this substance does not contain sulphuretted azote.

Lastly, a third observation not less interesting is the presence of carbon and carbonated substances in the sulphurous mineral waters.

M. Westrumb has discovered a new principle in them—a *fetid resin of sulphur*. (Stinkendes schwefelharz.)

In order to obtain it, we must evaporate the sulphurous water excluded from the contact of the air, and afterwards take up up the residue by alcohol, which dissolves this resin more than the earthy muriates. By the evaporation of the alcoholic liquid, the substance at first looks like a yellowish fat, it is successively coloured brown, and becomes resinous.

By repeated solutions in alcohol, and by evaporations, it is decomposed into sulphur, and into a resin of a blackish-brown.

It has a garlic smell, which becomes very strong, and similar to assafœtida, when we pour water into the alcoholic solution.

Its solution acts like an acid.

Its resin is dissolved in ammonia, and communicates a yellow colour to it; the liquor acts like that of Beguin. With lime-water we obtain a hydro-sulphuret. All these solutions act upon the metallic combinations like sulphuretted hydrogen.

As sulphurous mineral waters have their origin in beds of pit-coal, we might perhaps find the source of this bituminous principle in pit-coal itself.

Around the baths of Eilsen, like those of St. Amand, there is accumulated a kind of crust, which gradually becomes of a dark colour, and latterly black.

By analysis, there have been extracted from it sulphuretted fetid resin, hydro-sulphuret of lime, sulphur, lime, alumine, magnesia, charcoal, and sand, with some fibrous substances, a little sulphuretted hydrogen gas, and carbonic acid gas.

Whatever be the origin of the bituminous principle in sulphurous waters, M. Westrumb succeeded in producing charcoal and fetid resin, by employing sulphur perfectly pure.

For this purpose he digested sulphur precipitated by an acid in alcohol. By distilling the alcoholic liquor, there is separated yellow crystalline sulphur, or a yellowish gray powder: the fetid resin is then completely formed in the liquor floating above, and possessing all the above properties.

We might attribute its formation to the presence of the alcohol, and *à fortiori*, because, after its separation from the

 residue

residue of the evaporated sulphurous water, the penetrating smell is manifested, when it is taken up by the alcohol. But several observations lead M. Westrumb to think that the alcohol does not contribute to the formation, and that it rather derives its origin from the sulphur itself.

Letter of M. Roloff, *of Magdebourg, upon the foregoing Subject**.

I have recently recognised in an unexpected manner the sulphuretted fetid resin of M. Westrumb.

M. Michaelis, after having precipitated the golden sulphat of the hydrogenated sulphuret of antimoniated potash, evaporated the liquor floating above containing the sulphat of potash.

When the ley began to concentrate, a vapour was developed, which very much embarrassed the artist who was stirring the mass. There was at the same time manifested an insupportable smell, analogous to that of burnt assafœtida.

The saline mass evaporated to dryness had a gray colour, and the remarkable smell we have mentioned.

It was put in digestion with alcohol, which acquired the taste and smell of garlic.

The alcoholic liquor evaporated spontaneously, yielded a gray gluey mass, possessing the same smell and taste.

I am desirous that this experiment should be made public, not knowing if M. Westrumb is acquainted with the formation of a large quantity of fetid resin which may be easily procured by this process.

As the smell is manifested before putting in the alcohol, we may conclude, with M. Westrumb, that the alcohol does not contribute to its formation.

* From Gehlen's new Journal of Chemistry.

XXVII. *On*

XXVII. *On the Preparation of Calomel.* *By Mr.* Joseph Jewel.

In our last number, page 93, we mentioned that Messrs. Howard and Co. had introduced an important improvement in the preparation of this essential article of the pharmacopœia. It is the discovery of Mr. Joseph Jewel, one of the partners, who gives the following specification of his invention, for which he has taken out a patent:

"Calomel, or mercurius dulcis, as usually prepared, is at first a hard crystalline substance, and requires to be pounded and triturated with water, either in a mortar or on a slab with a muller, or in a mill. After having been ground or triturated for a considerable time, more water is added, and the whole well stirred up. The finer particles, which remain suspended for a short time, being poured off with the water into another vessel, and left to subside, the water is then decanted, and the fine powder dried for use. The coarser particles are again submitted to the operation of grinding and washing over, until the whole be finished. Now the nature of my invention is to produce the effect of the grinding or trituration above described, in a more perfect manner, during the last sublimation of the calomel; which I do as follows:

"I take calomel, or mercurius dulcis, broken into small pieces, and put into an earthen crucible of the form of a long barrel, so as to fill about one half thereof. I place the crucible on its side in a furnace provided with an opening, through which the mouth of the crucible projects about an inch. I then join to the mouth of the crucible an earthenware receiver, having an opening at its side to receive the open end of the crucible. This receiver is about half filled with water. I lute the joint with a mixture of sand and pipe-clay. The receiver has a cover, which cover has a side continued upwards for containing water, with a chimney or tube in it, to allow the escape of steam from the water below. I then apply a fire around the crucible, sufficient to raise the calomel in vapour, and force it through the mouth of the crucible into the receiver; where, by the water,

 while

while cold, or assisted by the steam when it becomes hot, it is instantly condensed into an impalpable powder, possessing all the qualities of calomel in its most perfect state. The calomel, when thus prepared, is purer, whiter, and more attenuated, than that obtained by grinding. It is proper to wash the product over with water, before it is dried, to rid it of any coarser particles which may form about the mouth of the crucible.

XXVIII. *On the Contraction which takes place in Mercury at low Temperatures by Abstraction of Heat;—and on the Ratio of Contraction between Mercury, Alcohol, Water, and Silver. By* JOHN BIDDLE, *Esq. of Birmingham.*

Birmingham, February, 1808.

To Mr. Tilloch.

SIR,

A LETTER from M. Tardy de la Brossy, dated Joyeuse, (Ardeche) October 13th, 1805, addressed to Professor Pictet, of Geneva, has appeared in your Magazine [vol. xxiv. p. 322]. It contains observations on some experiments which I had the pleasure of showing to the Philosophical Society here on the specific gravity of mercury in its frozen state, which experiments were communicated to the public through the medium of Mr. Nicholson's Journal for April 1805. The observations of M. Tardy de la Brossy have induced me to look over the original papers, containing the results of those experiments, with some attention; and, with deference to the opinions of that gentleman (though I fear some inaccuracy exists), I must, in defence of my experiments generally, and the deductions made from them, request you to communicate a few experiments and observations through the channel of your Magazine.

M. Tardy de la Brossy, after avowing the object of his communication to be "the extension of truth, and the removal of error," proceeds to describe the result of some of my former experiments, and to make his observations on my calculations from them, to which I would refer; but if he he had added experiment to his calculations, he would have been convinced that the principles on which I proceeded are just, and that one source of difference in our opinions arises from the *partial* application of the mode of reasoning which he uses. For, admitting with him the specific gravity of the alcohol employed to be ·810 nearly, or ·8141 where water is 1·000, and that 1000 grains of mercury would exhibit a loss of weight in alcohol of the temperature stated, of 59·8, when weighed by the hydrostatic balance, yet it does not appear to me necessary to suppose, *à priori,* so long as each of these substances remains in a fluid state, that the ratio of their densities should differ when uniformly subjected to the lower degrees of heat. As, however, my former experiments were not made with a view to discover the contraction of the volume of the alcohol, I made no observation relating to it, and now think it right to investigate the subject by experiment.

A. I first distilled mercury as before, with great care, using only that 30 per cent. of the whole which first came over in the distillation, esteeming it the most pure. I found, by the hydrostatic balance, the specific gravity of this to be 13·613, as 1000 grains lost in distilled water 73·4 at the temperature 50 of Fahrenheit's scale.

B. I took alcohol from the same parcel which I had used in my former experiments, and filling a light glass bottle formed with a long narrow neck for the purpose, it was found to weigh ·8141, when water weighed 1·000 at the temperature of 48° nearly.

C. Having obtained a mass of very pure silver, procured from luna cornea, 1000 gr. lost in distilled water 97·8, the specific gravity of which was thereby found to be 10·225; but by hammering it into a form convenient for my purpose the specific gravity increased to 10·362, the loss of weight being 96·5 at 50° of temperature. At the following temperatures the variations of loss of weight are expressed in the second column, the consequent specific gravities in the third, according to the usual mode of calculation, and in the fourth is shown the loss of weight in alcohol:—parts of a series of observations from experiments, that are given more at large in the table containing a general comparison of the several experiments.

1	2	3	4
Degrees of heat according to Fahrenheit's scale.	Loss of weight of 1000 gr. of silver in distilled water.	Specific gravity of silver when water is 1·000.	Loss of weight of 1000 gr. of silver weighed in alcohol.
200	94	10·638	
185	94·4	10·593	
175	94·6	10·570	
158	95·2	10·504	
150	95·3	10·493	
130	95·5	10·471	75·4
102	95·9	10·428	76·2
80	96·3	10·384	77·6
66	96·4	10·373	78·5
55	96·4	10·373	
50	96·5	10·362	78·6
34	96·5	10·362	79·4
21	—	—	80
9	—	—	80·5
0	—	—	81
20	—	—	81·6
32	—	—	82·5
54	—	—	83
56	—	—	83·1

D. By the following experiments I then proceeded to ascertain the specific gravity of mercury and silver at various temperatures, from 100° above zero to more than 40° below it, with a wish to obtain a ratio of contraction between mercury and silver, and between each of these and alcohol. For this purpose two hydrostatic balances were so placed that the metals suspended from them might fall into a glass vessel containing alcohol.

I then attached 1000 grains of silver to one balance, by a wire 12 inches long, weighing only $\frac{7}{10}$ of a grain; and to the other 1000 grains of mercury, in a small glass bucket weighing $101\frac{2}{10}$ grains, suspended by a wire nearly 4 inches long, weighing $\frac{2}{10}$ grain.

Then also suspending in the alcohol a good thermometer, marked

marked for low temperatures, and having provided a quantity of muriate of lime and snow, I reduced the temperature of the alcohol and metals, as in my former experiments the subject of M. Tardy de la Brossy's observations, and obtained the results expressed in the following table.

1	2	3	4	5	6	7
Specific gravity of silver if the alcohol does not increase in density.	Loss of weight of 1000 gr. of silver weighed in alcohol by the hydrostatic balance.	Temperatures according to Fahrenheit's scale.	Loss of weight of 1000 gr. of mercury and the glass bucket.	Loss of weight of the glass bucket alone weighing 101·9 gr.	Loss of weight of mercury alone.	Specific gravity of mercury if alcohol does not increase in density.
10·680	76·2	100	82·6	24·3	58·3	13·963
10·518	77·4	60	85·0	25·3	59·7	13·636
10·362	78·56	50	85·3	25·5	59·8	13·613
10·359	78 3	44	85·5	25·6	59·9	15·589
10·208	79·7	20	86·6	26	60·6	13·433
10·290	79·9	6 or 7	86·7	26·1	60·6	13·433
10·037	81·1	0	86·9	26·2	60·7	13·411

In an examination of the 1st and 2d columns of this table relating to silver, it being previously known that 10·362 is the specific gravity of silver, and that ·8141 is the specific gravity of alcohol at 50° of Fahrenheit, where water is 1·000, it appears, following the general mode of calculation in which the loss of weight is made the divisor of the quantity weighed, and the quotient expresses the specific gravity, that as at 100 degrees of heat the divisor for the quantity of metal is less, and at zero greater, that at zero the specific gravity is less, and at 100 degrees above it the specific gravity is greater:—A conclusion opposing a general law, which therefore we cannot admit.

To account for this apparent inaccuracy of result, we must direct our attention to the alcohol employed, and see if, in the comparison of the *fluid* alcohol and the *solid* silver, this appearance of contradiction to an established law does not arise from the contraction of the fluid. Of this it will be difficult to bring visible proof to the extent required; for if I put into a glass tube alcohol, of which the weight is known,

known, and expose it to different temperatures from 100 to 50 degrees, it will contract; but whilst the fluid contracts, the glass which contains it contracts also, and shows only the difference of the contraction between the fluid and glass:—however, as we know that silver by heating cannot become specifically heavier, or by cooling specifically lighter, in these experiments which are evidently a comparison of densities, the alcohol must become more dense by deprivation of heat.

The third column of this table contains the temperatures.

In the 4th column the 1000 gr. of mercury and the glass bucket containing it appear together to have varied at the two extremes in loss of weight 4·3.

By the 5th column it is shown that the glass bucket alone varied at the two extremes in loss of weight 1·9.

By the 6th column it appears that 2·4 only can be stated to belong indispensably to a variation of the respective densities of the mercury and alcohol in the changes of temperature from 100° to zero. The other variations, I presume, arise from some inaccuracy, such as making the observations when the fluids were at different temperatures.

By the 7th column are shown the different specific gravities of the mercury, by calculation from the loss of weight, supposing the density of the alcohol the same throughout: from this it appears that the density of mercury is greatest at the highest temperature, and least at the lowest; but as this cannot be actually the case, it is obvious that the alcohol increases in density; and this appears also from the results of the 2d column relating to silver, at C.

For the specific gravities of alcohol at various temperatures, I must refer to the calculations at L, made from the following experiments, and which are contained also in a general table of results.

E. I procured the bladder of a rabbit, and washed it well with alcohol. It weighed 7 grains in air; and having annealed a wire about 9 inches long, the weight of which was $\frac{6}{10}$ths of a grain in air, I found that the bladder and wire, when sunk in alcohol to a certain mark on the wire, weighed 2·5 grains only, at the temperature 45°.

4 This

This bladder was filled with mercury, not quite pure, then suspended by the wire; and when one end of the wire was attached to the balance, it was found to contain 5276 grains; weighed in alcohol, it lost 321·5, from which deducting the weight of the bladder and wire in alcohol, 2·5, it appears that the loss of the 5276 grains of mercury, when the thermometer is suspended in the alcohol

at 45° of temperature, is 319

65	the mercury loses		315	less by	4
93	—	—	312	—	7
100	—	—	311	—	8
112	—	—	309	—	10
118	—	—	308	—	11
125	—	—	307	—	12
130	—	—	306·5	—	12·5
135	—	—	306	—	13 { The alcohol now in agitation by the heat could make no higher observation.
130	—	—	306·5	—	12·5
125	—	—	307	—	12
123	—	—	307·5	—	11·5
120	—	—	308	—	11
118	—	—	308·5	—	10·5
117	—	—	309	—	10
113	—	—	309·5	—	9·5
111	—	—	310	—	9
108	—	—	310·5	—	8·5
104	—	—	314	—	8
100	—	—	311·5	—	7·5
97	—	—	312	—	7
93	—	—	312·5	—	6·5
89	—	—	313	—	6
85	—	—	313·5	—	5·5
83	—	—	314	—	5
80	—	—	315	—	4
78	—	—	316	—	3
76	—	—	317	—	2
70	—	—	317·5	—	1·5
68	—	—	318	—	1

at

at 65	the mercury loses	318·5	less by	0·5
62	— —	319	—	0
50	— —	320	more by	1
45	— —	320·5	—	1·5
40	— —	321	—	2
36	— —	321·5	—	2·5
34	— —	322	—	3
27	— —	323·5	—	4·5
14	— —	324	—	5
5	— —	324·5	—	5·5
0	— —	325·5	—	6·5
5	below zero	326·5	—	7·5
18	— —	327·5	—	8·5
21	— —	328	—	9

We may observe in this experiment that some difference is occasioned by the direction in which the heat passes, whether from without to the alcohol, and thence to the mercury, as in the first part of these observations from 45° to 135°; or from within from the mercury to the alcohol, as from 135° to zero and 21° below: and it appears that the temperature was not the same in these two fluids when the observations were made; for when I weighed the same mercury with a hole in the bladder to introduce the bulb of a thermometer, that I might observe the variation of loss of weight when the alcohol and mercury were at the same temperature exactly, it showed the mean of these variations to be correct. So that in comparing 5276 parts of mercury with alcohol, there is a variation in the contraction of these two fluids expressed by 22 of loss of weight in passing through 156 degrees of temperature. In comparing 1000 parts of mercury with alcohol, it follows that there is a variation of loss of weight in 156 degrees of temperature expressed by 4·17, which shows ·0267 is the loss of alcohol in each degree greater than that of mercury.

If these two fluids were to contract by deprivation of heat in the ratio of their specific gravities, it is obvious that the loss of weight shown by the hydrostatic balance would be the same through all the changes of temperature, the changes being the same in both fluids at the time of observation.

It

It is therefore of material consequence to this inquiry to know in what degree equal bulks of mercury and alcohol increase together, without the hydrostatic balance denoting the alteration by any change of loss of weight; for this purpose we must look to D, and forward to the experiment G, and the calculations from them at L and I, where we shall find a rule for discovering the specific gravity of alcohol.

F. To find the proportion of contraction between mercury and water, I took 3024 grains of mercury in a bladder, as before; and weighing it in distilled water by the hydrostatic balance, the mercury and water heated to the temperature

200	the mercury lost	217·4	These 3 observations were unsatisfactory, the water having risen in vapour and condensed on the balance.
195	—	217·6	
190	—	217·8	
185	—	218	
180	—	218·2	
175	—	218·4	
170	—	218·6	
165	—	218·8	
160	—	219·1	
155	—	219·4	
150	—	219·7	
145	—	220	
135	—	220·3	
130	—	220·4	
125	—	220·5	
120	—	220·6	
110	—	220·8	
98	—	221·1	
88	—	221·3	
78	—	221·5	
68	—	221·7	
58	—	221·9	
50	—	222·1	

Thus, at 200° of temperature mercury would appear of greater specific gravity than when at 50°, following the general mode of calculation; but if we suppose the water is more

 contracted

contracted than mercury by abstraction of heat, and attribute to that the difference of loss of weight, then, in comparing 3024 of mercury with water, there is a variation in the contraction of these two fluids expressed by 4·7 loss of weight in passing through 150 degrees of temperature. In comparing 1000 of mercury with water, a variation of loss in 150° is expressed by 1·5542.

G. The following experiment was made before many of my philosophical friends with mercury, described before, of the specific gravity of 13·613, suspended by a very fine wire 10 inches long, weighing only $\frac{6}{10}$ of a grain;—the barometer standing at 29·8, the thermometer at 35.

1000 grains of mercury with 750 grains of alcohol, as before described, were put into a thin glass vessel, made for the purpose, round at the bottom, and increasing in diameter gradually to near the top, so that the mercury might be easily suspended in the alcohol by a wire introduced into it whilst fluid. These were placed in the centre of a mixture of 4 pounds of snow and 4 pounds of muriate of lime, at 12 o'clock at noon: at 5 minutes past a thermometer placed in the frigorific mixture fell to 52° below zero, then to 54 and to 60. The mercury in the tube of the thermometer appeared frozen: it was withdrawn, and when exposed to the air a few seconds, suddenly fell to 140° on the scale, in consequence of the mercury in the tube again becoming fluid, and occupying the vacuum which had been occasioned by the contraction of the mercury in the bulb after that which was in the tube had become solid: it was then immediately returned to its place in the mixture of snow and muriate of lime; it had remained at 140° for several minutes when taken out and exposed to the air, so that the mercury still in the tube was again made fluid; it instantaneously sunk into the bulb much below 270°, the lowest point on the scale. During this time the mercury and alcohol, very much reduced in temperature, were removed from the above mixture, and placed in a second mixture of 3 pounds of snow and 3 pounds of muriate of lime: the whole was then placed in the first mixture; and at the moment of the crystallization of the mercury, the wire, already partly attached, was by raising

raising it gently, drawn from the side to the centre of the surface of the mercury. When it was fixed and the whole was solid, it became necessary to withdraw the glass, and expose it a moment to the air of the room, until that part of the mercury attached to the glass was softened; then, by keeping one hand drawing gently at the wire, the whole of the mercury was suspended, and, with the alcohol, immediately returned to its place in the cooling mixture; the mercury was now suspended from the hydrostatic balance by the wire fixed in it, and weighed with great accuracy;—and the following observations were made from the time the second quantities of muriate of lime and snow were mixed.

In 5 minutes the mercury was crystallizing.
10 ——— became nearly solid.
30 ——— quite solid—withdrawn.
40 ——— having been replaced.
50 ——— suspended in the alcohol, and weighed by the hydrostatic balance with great care, lost —— 60·8
60 minutes the mercury and alcohol having been a little withdrawn, the mercury lost 60
85 minutes the mercury lost 59·9 it was now so nearly fluid, there was splendour on the surface.
130 minutes the mercury lost 60·8
145 —— —— 60·3
150 —— —— 60·1
160 —— —— 60·3
170 —— —— 60·1 having been withdrawn, and the temperature increased.
175 —— —— 60·7
180 —— —— 61

As I could not decrease the heat so as to indicate a greater loss than 61, and my own body during these three hours having suffered an unusual and partial deprivation of heat, I withdrew the mercury and alcohol, not doubting, if it could have answered any purpose, by these means to have been able to have kept the mercury solid some hours longer, the surrounding substances having lost so much heat.

We see from the observations made in this experiment, that

59·9 is the least diminution in the weight of mercury when weighed in a solid state in alcohol, and that it is at this moment when their specific gravities are furthest from each other; for we may observe that at almost the next degree of temperature to that in which the mercury loses its fluidity, whilst the alcohol preserves its fluid form, the mercury appears to become of less specific gravity, as it loses 60; which gives, according to calculation at L, only 14; but the fact appears to me to be, that alcohol proceeds in the ratio of a fluid by decrease of temperature, and that mercury, having obtained a solid form, follows the ratio of contraction of a solid; therefore their densities approach each other. I should have ascribed this greater loss of weight to the particles of the mercury at the moment of crystallization occupying, in consequence of their new arrangement, more space than at the moment before it became solid, as with some of the metals is known to be the case, had I not carefully observed the passage of this metal in other experiments, where I had an opportunity of seeing the contraction proceed very distinctly; and had not the mercury, also proceeding to still lower temperatures, lost from 60, 60·1, 60·3, 60·7, 60·8, and 61, long after the whole had become solid, and was suspended by the wire: thus, 61 is the greatest diminution of weight by the abstraction of heat which I could obtain, and I am of opinion that the space described by these changes of loss of weight denotes a range of many degrees of temperature: how much greater the density may then be than that stated at L, I cannot presume to say; but as it takes, with some probability, a proportion of contraction approaching to that of silver, it certainly appears improbable that it should reach the specific gravity which my former calculation from a single observation led me to attribute to it. I am therefore inclined to believe there was some inaccuracy in weighing the silver by the hydrostatic balance, when by my former experiments I gave 88·1, the quantity of loss of 1000 gr. of silver:—from many observations in this laborious train of inquiry, it seems hardly possible that the former could have been correct; for if the alcohol and silver continued to contract in the same proportion from 56° below zero, as they do from 64° above zero to that temperature, it would appear that an abstraction of heat must necessarily have

taken

taken place equal to 120° more, or to 176° below zero, which I can hardly suppose, though the mercurial thermometer used in these experiments fell lower than 270° below zero. If my former statement be erroneous, it certainly is not attributable to the mode of calculation, as apprehended by M. Tardy de la Brossy. As it does not appear that he had made any experiment on the subject, he could not have anticipated this inaccuracy in the weight of silver: and as he says that only a few grains of the increased gravity of the mercury was attributable to the alcohol, he could not be aware of the great increase of the specific gravity of alcohol shown at I, by the same mode of calculation used in my essay, the subject of his animadversions.

It is still, however, obvious, that mercury would not be of the specific gravity which I attributed to it, unless it had followed the same rate of contraction after it became solid as it did whilst fluid: the evidence of the increased loss of weight is so much against me, that I cannot defend that experiment; and I would now be understood to carry my observations on the specific gravity of mercury, with accuracy, to the point of congelation only; or to state that near 56° below zero its specific gravity is 14·465, as by calculation at L. Then, if we attribute to mercury in a solid state nearly double the contraction of silver as at H, or ·00100 in each degree, as it is near to its point of fluidity, we shall arrive only at the specific gravity 14·485, as is shown at M.

To ascertain whether the silver at C, with which I proposed to compare the mercury, had expanded or contracted by the deprivation of heat to which it had been exposed,

H—I took an ingot of silver, 16 inches long, 2 inches wide, and half an inch thick, weighing nearly 100 ounces; and provided an instrument for the purpose of measuring accurately the contraction and expansion of the silver, by fixing in a piece of well baked wood two centre pins, exactly 15 inches from each other, one of them very fine, for the purpose of striking an arch of a circle on the surface of the silver, when the other was fixed in a perforation made in the silver by the centre itself. Then, on the surface of the bar of silver, heated to a pale red, an arch was described with

 this

this instrument; and when again reduced to the temperature of 50 degrees, another arch was drawn :—the contraction of the bar of silver between the two points appeared to equal $\frac{1}{85}$th part of its dimensions.

The silver was then heated to 200° of temperature of Fahrenheit's scale, by boiling it in water, and gradually cooling; an arch of a circle was then struck on the surface of the silver: reducing it to 150° of temperature, another arch was struck with the same unvaried centre pins, which showed evident contraction in the bar of silver: again, at 100° of temperature another arch was described; and a fourth, at 50°, showed that the silver had contracted in the deprivation of 150° of temperature $\frac{1}{340}$th part of its length.

Then, similar parallelopipedons being to each other as the cubes of their homologous sides, the increased specific gravity is shown by multiplying the specific gravity at 50°, namely, 10·362, by the cube of 341; and dividing it by the cube of 340, which equals 10·4537; ·0917, therefore, is the difference in 150° of heat; or ·0006114 is the contraction of silver in each degree; which, from the way this experiment is performed, I call its *visible contraction*. From it the specific gravities at the several temperatures below are calculated :—

At 200°	silver is of the	specific gravity	10·2702
150	——	——	10·3908
135	——	——	10·3100
117	——	——	10·3210
100	——	——	10·3314
50	——	——	10·3620
0	——	——	10·3925
50 below zero		——	10·4231
52	——	——	10·4243
56	——	——	10·4268

I. Then, if 10·4268 be the specific gravity of silver at 56° below zero, and weighed in alcohol at that temperature, by calculation from its loss of weight at C, it will appear to be 9·796, by taking ·8141 as the supposed density of alcohol, dividing the quantity 1000 by the loss 83·1, multiplying the quotient 12·033 by ·8141, and dividing by 1·000, the

the specific gravity of water, the result of this being 9·796; What then is the *specific gravity of the alcohol*—if not 8141?

It is found by this rule:—

Divide 1000, the quantity weighed, by the loss of weight, and by the quotient divide the density of silver, according to its visible contraction; the quotient will be the specific gravity of the alcohol, or fluid in which it is weighed.

Thus, at the temperature 56° below zero, the loss of weight is 83·1; by which, if 1000 (the quantity weighed) be divided, the quotient is 12·033; by which 10·4268 (the density of silver according to its visible contraction) is divided; and the quotient is ·8665, the specific gravity of the alcohol at 56° below zero. Thus, ·0524 is the sum of variation of the specific gravity of alcohol in 106°, or 00049434 is the variation in each degree of temperature: from it the specific gravities at these several temperatures are calculated.

At 135°	the specific gravity	of alcohol is	·77208
117	———	———	·78098
100	———	———	·78938
50	———	———	·81410
zero	———	———	·83882
50 below zero		———	·86353
52	———	———	·86482
56	———	———	·86650

K. Then taking the specific gravity of the alcohol at any certain temperature, and the loss expressed at C; What will be *the specific gravity of the silver* according to C?

It is found by this rule:—

Divide 1000 (the quantity weighed) by the loss of weight, and multiply the quotient by the specific gravity of the alcohol or fluid in which it is weighed.

Thus, at 56° below zero the loss of weight is 83·1, according to C; by which if 1000 be divided, the quotient is 12·0337; which, multiplied by ·8665, the density of alcohol, as at I, in that temperature the product is 10·427, the specific gravity of silver at 56° below zero.

Then, having ascertained the specific gravity of silver and

 of

of alcohol at several points of the scale of temperature by calculations at I, from experiments at H, the calculation of *the specific gravity of mercury* is thus performed :—

L.—Divide the quantity by the loss, and multiply the quotient by the specific gravity of the alcohol at such temperature.

Thus, mercury at G, in the temperature of 56° below zero, appears to be of the specific gravity 14·465. For dividing 1000 by 59·9, the least loss in the solid state, and multiplying the quotient 16·694 by ·8665, the specific gravity of alcohol at that temperature, the product is 14·465.

Then if mercury in 106°, from 50° above to 65° below zero, increases in specific gravity from 13·613 to 14·465, namely ·852, which gives ·00804 each degree; this sum, multiplied into the number of degrees from 50° above zero, gives the specific gravity of mercury at that degree, if deducted when above 50 from 13·613, or added to the same when below 50°.

Thus at 135° above zero, which from 50° is 85°, if the sum ·00804 be multiplied by 85, the product is ·6834; which subtracted from 13·613, the specific gravity of mercury at 50°, gives 12·9296, the specific gravity of mercury at 135° of temperature.

At 200°	the specific gravity of mercury is		12·407
150	——	——	12·809
135	——	——	12·929
117	——	——	13·074
100	——	——	13·211
50	——	——	13·613
0	——	——	14·015
50 below zero		——	14·417
52	——	——	14·433
56	——	——	14·465

Then, to see if the alcohol in which the mercury was weighed at G obtains by the same mode of calculation a specific gravity, according with the calculations at I, from the experiments at H on silver,—observe the rule at I.

At the temperature 56° below zero, the loss of weight is 59·9 on the quantity 1000; by which if it be divided, 16·694 is is the quotient; this again dividing the specific gravity of the mercury 14·465, the quotient is at this temperature ·8665, the specific gravity of the alcohol according to G, which agrees with the calculations from the loss of weight of silver by experiments at H, and calculations at I, it there appearing that ·8665 is the specific gravity of the alcohol.

M. Supposing the mercury weighed by the hydrostatic balance at G in a solid form, during the variations in the loss of weight from 59·9 to 61, to have passed through 20° of temperature to 76° below zero, and to have increased ·001 each degree, then mercury in a solid state will have arrived at the specific gravity 14·485, and the alcohol in which it is weighed with a loss of weight of 61 will consequently be of greater specific gravity.

For if alcohol at 56° below zero was of the specific gravity ·8665, as at I, the mercury losing 59·9, as at G, and at a lower temperature the same mercury weighed in the same alcohol lost 61, it will appear that the alcohol must have increased in specific gravity; for if 59·9 be ·8665, 61 will be ·8842, the mercury remaining at 14·465, as at L: but as we have reason to state, it passes on to 14·485 at 76° below zero, then the specific gravity of the alcohol must appear to be ·8854; because if 14·485 increases ·001 each degree, ·8842 will increase ·000061; which sum, multiplied by 20, equals ·001220; this, added to ·8842, equals ·8854.

N. We observe at F, 3024 of mercury lose 222·1 at 50° of temperature; at 200° of temperature it loses 217·4; and by calculations at L, the specific gravity of the mercury so weighed was shown to be 12·407 at that degree of heat.

Then by the rule at I:—

If 3024 be divided by 217·4, the quotient is 13·909, which sum dividing 12·407, the known gravity of mercury at L, the specific gravity of the water will consequently appear to be ·8920: this deducted from 1000, the specific gravity of water at 50° of temperature, leaves ·1080, the sum of difference in 150 degrees, and

At the following temperatures water is of the specific gravity stated:

200°	——	·8920
150	——	·9306
135	——	·9419
117	——	·9537
100	——	·9661
50	——	1·0000

and as ·1080 is the sum of difference in 150°, this sum ·00072 expresses nearly the variation in each degree.

We find from the data obtained in these experiments a *ratio of contraction* between mercury, alcohol, water, and silver of equal volume.

Mercury at L is	·00804
Alcohol at I is	·0004943
Water from F above	·0007200
Silver at H is	·0006114

What will be the proportionate contraction of each, that of mercury being supposed 100?

As the increased specific gravity of mercury is to the supposed number, so is the increased specific gravity of either to the number required.

If 00804 be 100, what will { 0004943 = 6·144 Contraction of alcohol
0037200 = 8·955 —— water
0006114 = 7·604 —— silver

of equal volume, when that of mercury is 100.

If we suppose water to be 1·0000
then mercury will be 11·1666
alcohol ·6865
silver ·8491

As in the hydrostatic balance the comparison relates to *equal volumes* of the thing weighed, and the fluid in which it is weighed; What will be the ratio of contraction of *equal weights* of each of the above?

As the specific gravity of either is to the specific gravity of mercury, so is the ratio of contraction of equal volumes to the ratio of contraction of equal weights.

As ·8141 : 13613 : : 0004943 = 008265 alcohol.
As 1·000 : 13613 : : 0007200 = 009801 water.
As 10·362 : 13613 : : 0006114 = 000803 silver.
005040 mercury.

Table

Table of Results of the foregoing Experiments, with some Calculations from them.

Specific gravity of *alcohol* calculated from loss at C, and the visible contraction of silver at H as at I.	Specific gravity of *silver* calculated from loss at C, and its visible contraction at H as at K.	Specific gravity of *silver* calculated from its visible contraction at H.	Loss of weight of 1000 of *silver* weighed in alcohol at C.	Temperature of Fahrenheit's scale.	Loss of weight of 1000 of *mercury* weighed in alcohol at E and G.	Specific gravity of *mercury* calculated from the various densities of alcohol at E and G as at L.	Loss of weight in 1000 of *mercury* weighed in water at A and F.	Specific gravity of *water* calculated from loss at F, and the specific gravity of mercury at G.
		10·270		200		12·407	71·88	·8920
				175			72·26	
		10 300		150		12·809	72·65	·9306
7720	10·240	10 310		135				
			75·4	130	58	12·929	72 76	·9419
7810	10·276	10 321	76	117	58·47	13·074		
			—·4	108				
7893	10·276	10 331	—·7	100	58·3	13·214	73·10	·9661
			77	95				
			—·6	82				
			—·9	70	59·5			
			78	68				
			—·1	64				
8141	10·357	10·362	—·6	50	59·8	13·613	73·459	1·0000
			79	45	59·9			
			—·2	40				
			—·6	32	60·5			
			80	21	60·0			
			—·5	9				
8388	10·355	10·392	81	zero	60·7	14·015		
			—·4	10				
			—·7	21				
			82·2	28				
			—·3	31				
			—·5	32				
			—·5	37				
			—·6	45				
			—·7	48				
8635	10·427	10·423	—·8	50		14·417		
8645	10·428	10·424	—·9	52		14·433		
			83	54	59·9			
8665	10·427	10·427	83·1	56		14·465		
					60			
					60·1			
					60·3			
					60·7			
					60 8			
8854	·	This temperature supposed at M		76	61	14·485		

To

To recapitulate the foregoing Experiments—

Observe,

At A, mercury is of the specific gravity 13·613 at the temperature 50.

At B, alcohol is of the specific gravity ·8141 at the temperature 50.

At C, silver is of the specific gravity 10·362 at the temperature 50.

At C and D there is error shown to exist in estimating the specific gravity of bodies in the usual mode, without having regard to a fixed point of temperature, at which the medium chosen to compare other bodies with should be estimated at 1·000; and without having regard to the ratio of contraction in the body weighed, and the medium in which it is weighed.

At E, a variation of contraction between mercury and alcohol expressed by 22 in weighing 5276 of mercury.

At F, the error of common practice noted at D is confirmed, the increased loss of weight showing the contraction of water to be greater than that of mercury of equal volume; it is expressed by 4·7 in 150° on the quantity 3024.

At G, mercury in its frozen state weighed by the hydrostatic balance lost 59·9 to 61 on the quantity of 100. For the calculations from these facts, see L and the annexed table. The mercurial thermometer fell below 270° on the scale below zero.

At H, silver, going down 150 degrees of Fahrenheit's scale, contracts $\frac{1}{340}$th part of its dimensions, which is called its *visible contraction*.

The silver at	117 above zero	is	10·321
——	50	——	10·362
——	56 below zero		10·426

The increase of silver in its specific gravity is ·0006114 each degree.

At I, the error of general practice observed at D and F is confirmed. A rule is given for finding the specific gravity of the alcohol. When ·8141 at 50 above zero it appears to be ·8665 at 56 below zero.

At

At I, the increased specific gravity of alcohol each degree is ·0004934.

For the further variations of gravity see the annexed table.

At K, a rule for finding the specific gravity of silver from loss of weight at C, and the specific gravity of alcohol at I.

If silver at 50 above zero be 10·362, it will be at 56 below zero 10·427.

At L, a rule is shown for finding the specific gravity of mercury.—At 200 above zero it is found to be 12·407

50	——	13·613
56 below	——	14·465

The increase each degree is ·00804

A comparison of C and G as it relates to alcohol; and by the rule at I it appears they accord in the number ·8665, the specific gravity at 56 below zero.

At M, the contraction of mercury in its solid state, supposed to be near twice that of silver, or ·001 each degree, because near the point of fluidity.

Alcoholof the specific gravity ·8854 at 76 below zero.

Mercury in a solid form of the specific gravity 14·485 at 76 below zero.

The mode of calculating this is shown.

The increase of specific gravity of alcohol in each degree not shown by the hydrostatic balance, when mercury is weighed in it in a solid form, is ·000061.

At N, rule for finding the increase of water of equal bulk to that of mercury. It appears from F to be ·00072 each degree.

Rule for finding the *ratio of contraction* of mercury, alcohol, water, and silver, *of equal volume*; mercury being supposed 100.

A ratio of contraction also of water, mercury, alcohol, and silver; water being supposed 1·000.

Rule also given to find the ratio of contraction of alcohol, water, silver, and mercury, *of equal weight*.

XXIX. *Essay upon Machines in general.* By M. Carnot, *Member of the French Institute, &c. &c.*

[Continued from p. 15.]

V. The second principle upon which we purpose making some observations, is the celebrated law of equilibrium of Descartes. It comes to this, that two powers in equilibrium are always in reciprocal ratio to their velocity, estimated in the direction of these forces, when we suppose that one of the two comes to take it from the other in an infinitely small degree; so that a small movement arises from it.

But although this proposition be very beautiful, and we generally regard it as the fundamental principle of equilibrium in machines, it is nevertheless infinitely less general than that which has been quoted in the first place: because it is applied solely to the case where there are only two powers in the system: and besides, it is very easily deduced from what has been said upon the subject of the two weights A and B, since we evidently approximate the one case to the other by substituting, by means of pulleys, weights in place of the forces which we wish to value.

Moreover, it is to be remarked, that this principle does not express the conditions of the equilibrium between two powers so completely as that which has been quoted in the first place; for it only gives the account of the quantities of force composing equilibrium, at the place where the latter also gives, in some sort, the account of their directions:—for example, in the case of equilibrium between two weights, the principle of Descartes solely teaches that the weights should be in the reciprocal ratio to their vertical velocities; but it does not indicate, like the first, that one of these bodies should necessarily ascend, while the other descends. In order that an axle, for instance, to the wheel and cylinder of which weights are suspended by cords, should remain in equilibrium, it is not sufficient that the weight applied to the wheel be to that of the cylinder as the radius of the cylinder is to the radius of the wheel:—it must also happen that these weights tend to make the machine turn in a contrary direction to each other; *i. e.* that they are placed in different sides with respect to the axis; else their efforts, being mutual, will put the machine in motion. It is therefore evident that what renders the principle of Descartes incomplete is, that by determining the reference of the powers, as to their values or intensities, he does not express that these powers should make opposite efforts, nor in what consists this opposition of efforts: it is clear, in fact, that for an equilibrium one of the forces must resist while the other solicits: now, this is not what happens in the case of the example of the axletree;—But what is it in general that distinguishes soliciting from resisting forces? This in my opinion has not yet been determined. We shall see in this essay that the characteristic difference of these forces consists in the angle they form with the directions of their velocities, so that the one form always acute angles with their velocities, while the others form obtuse ones with theirs.

Lastly. One fault with which we may reproach the principle of Descartes, as well as all those where we are discussing the small movement which would arise in the system if the equilibrium was disturbed, is, that they do not indicate the method of determining this small movement. Now, if for this purpose we must have recourse to some new mechanical principle, the former is not sufficient; and if we can determine it by pure geometry, What is the method of doing so? This is what the principle does not say: and let us not say that the proportion indicated by the principle always takes place whatever the movement is, provided it is possible, *i. e.* compatible with the impenetrability of bodies; for this would be an error: and we shall by and by show that these movements are subjected to certain conditions, in consequence of which I think it right to give them the name of *geometrical movements.*

We may make the same remark upon all the principles upon which we propose to consider a machine in two states infinitely near each other; for, in order to determine what are those two states; *i. e.* what movement the machine should take in order to pass from the one to the other, we must either employ new mechanical principles conjunctly with that proposed, which would render the latter insuffi-

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cient; or else geometry is sufficient; and in this case it is a defect in the principle, not to make known the geometrical conditions to which this movement is subjected.

VI. The two laws mentioned are confined to the case of equilibrium. We pass easily from this case to that of the movement by M. D'Alembert's principle in dynamics. But we have found several others which are immediately applied to the case of movement; such as that of the preservation of living powers under the shock of perfectly elastic bodies; which is so much the more general, as it extends even to the case of the movement passing rapidly from one state to the other: but it would seem that people have little dreamed of the use that might be made of it in the theory of machines properly so called. It is, however, evident, that this law should have its analogy in the shock of hard bodies: and as we generally take the latter to use it as a term of comparison, this principle, transferred to hard bodies with the modification which the difference of their nature requires, cannot fail to be more useful than the preservation in question. We shall show, in fact, that we may deduce from it several capital truths with the greatest facility, and particularly the preservation of living powers in a system of hard bodies, the movement of which changes by insensible degrees; a principle of well-known utility in the theory of machines. We shall thereby see, at the same time, an intimate relation between these two preservations of living powers;--we draw from it also the principle of Descartes; and even, by generalizing it, the law of equilibrium in machines with weights above mentioned. This principle, in short, after having given to it the extension of which it is susceptible, appeared to us to contain all the laws of equilibrium and of movement: and we have not found a better for the basis of our theory.

VII. This essay will be divided into two parts: In the first we shall treat of the general principles of equilibrium and of movement in machines; and in the second we shall examine the properties of machines properly so called, without ever stopping at any particular machine.

PART

PART FIRST.

General Principles.

When one body acts upon another, it is always immediately, or by the agency of some intermediate body: This intermediate body is generally what is called a machine: the movement lost every instant by bodies applied to this machine is partly absorbed by the machine itself, and partly received by the other bodies in the system; but as it may happen that the object of the question is simply to find the reciprocal action of bodies applied to intermediate bodies, without having any occasion to know the effect of it upon the intermediate body itself, it has been thought, in order to simplify the question, to make an abstraction of the very mass of this body, preserving to it on the other hand all the other properties of matter. Hence the science of machines has become in some measure an isolated branch of mechanics, in which it is required to consider the reciprocal action of the different parts of a system of bodies; among which there are found things which, when deprived of the inertness common to all parts of matter such as exists in nature, have retained the name of machines.

IX. This abstraction may simplify in certain particular cases, where circumstances indicate those of bodies, the mass of which it is convenient to neglect, in order more easily to attain our object; but we conceive that the theory of machines in general has really become more complicated than formerly: for this theory was once contained in that of the movement of bodies, such as nature presents them to us; but at present we must consider at once two kinds of bodies, the one as they really exist, and the other as deprived in part of their natural properties. Now, it is clear, that the first of these problems is a particular case of the latter; therefore the latter is more complicated: further, although we easily succeed by similar hypotheses, in finding the laws of equilibrium and of movement in each particular machine, such as the lever, the axle, and the vice, there results an assemblage of facts, the connection of which is perceived with difficulty, and solely by a kind of analogy; which should ne-

cessarily

cessarily happen, as often as we have recourse to the particular figure of each machine, in order to demonstrate a property which is common to it with all others. These common properties being those which we have in view in this essay, it is clear that we shall only succeed in finding them by the abstraction of particular forms. Let us begin, therefore, by simplifying the state of the question, by ceasing to consider under one and the same system, bodies differing in their nature. Finally, let us restore to machines their *vis inertiæ*. It will be easy, after this, to neglect their mass in the result: we shall have the choice of doing so or not; and in setting out, the solution of the problem will be equally general, at the same time that it will be simpler.

[To be continued.]

XXX. *On Caloric, and the Heat evolved during Combustion.* *By* James Scholes, *Esq., Manchester.*

To Mr. Tilloch.

Sir,

Having been induced to pay particular attention to combustion for some time past, I have insensibly imbibed principles different from the generally received theory. I very soon began to suspect caloric as a compound substance, and six months ago had recognised two fluids of electricity for its component parts. The only demonstrative grounds I then had for my ideas was the production of light and heat, particularly the latter, and for the purpose of measuring the quantity thereof I had an apparatus constructed. But when Mr. Davy's recent experiments were noticed in your Magazine, I immediately saw them as an additional support of my peculiar principles, and prepared a lecture, which was delivered to a Society in this town on the 29th of January, laying down the whole system, as supported by facts deduced from electricity and the experiments of Mr. Davy, which I intended to publish when more matured; but on looking over the monthly publications yesterday, I found a communication in Mr. Nicholson's to a similar purport, which has induced

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me to trouble you with my communication sooner than I intended. If you deem it worthy a place in your work, I shall feel obliged by the insertion. You will see that I come to similar conclusions to Mr. Gibbes, from a very different investigation, and from different phænomena; so that there is no sameness in our productions. With respect to the merit of each (being somewhat different), I can say this, that I have candidly considered what Mr. Gibbes has advanced, but I shall not be disposed to admit without further information one of his deductions; viz. that water is a simple substance, and the base of both hydrogen and oxygen.

I am, sir, your obedient humble servant,

James Scholes.

Manchester,
March 4, 1808.

ON CALORIC, &c.

A theory to be just, ought to elucidate and exhibit the cause of every phænomena with which it is connected: if it will not do this, I shall even be inclined to believe that its principles are erroneous, or at least very imperfect. It is an undoubted fact that there are many effects which the present doctrine of caloric does not pretend to explain at all; and again, there are many others which it explains only upon principles inexplicable in themselves. And if we take a view of the doctrine as applicable to combustion (which is perhaps its *forte*), reflection cannot but produce dissatisfaction with it. The present doctrine of combustion is founded upon this basis: first, (which is undoubtedly just,) that oxygen in every case unites with the combustible; secondly, that the caloric and light is produced by a difference in the capacity for heat of oxygen and the combustible, before and after combustion. But that this second part of the doctrine is not so obvious, appears from the combustion of gunpowder *in vacuo*, and many similar facts. Where, according to the present theory, caloric ought to be absorbed instead of evolved, it is directly contrary to the system; for the products of combustion have considerably greater capacities for caloric than the combustible and supporter of combustion themselves.

Other

Other phænomena might be adduced to the same purport; but this is the strongest proof, the best known, and sufficient for my purpose. It will, I presume, be seen, that though one part of Lavoisier's doctrine is probably just, yet the other is as likely to be erroneous; and, from a retrospect of the success of various fallacious theories, from their being always established on sophistical reasoning from experimental proof, which is as liable to deceive mankind now as aforetime, we shall be justified in this conclusion; that though Lavoisier's theory appears to coincide with experimental proof, yet it may not be just, and that, failing to account for many phænomena with which it is connected, it is at best imperfect, if not materially erroneous. It indeed accounts for the emission of heat and light in common cases of combustion plausibly enough, it must be admitted;—*But where is the proof?* Plausible reasoning is often sophistical; and I cannot by any means think we have sufficient grounds for believing that the caloric and light of common combustions are produced agreeably to this doctrine. It appears to me that the evidence thereof is merely presumptive, and in many instances it is evident that the heat and light must be derived from another source. That the state of combustible, &c., and product of combustion, before and after the process, must have material influence in the quantity of heat and light given out, is not to be denied; but in rapid combustions I am disposed to think the effect is not great in proportion to the quantity generated by the process.

Since I perceived the deficiency of Lavoisier's theory, I have been induced to pay particular attention to this subject. Reflection soon taught me that oxygen and caloric must have some remarkable relation to each other, as oxygen is the only known supporter of combustion in nature:—what kind of relation this can be, does not appear so ready to determine. Yet I am inclined to believe that it will be brought to light before long, and that it will be discovered in the new field of investigation opened by the recent experiments of Mr. Davy; at least it is there I have looked for it. I just noted that there was a remarkable relation between oxygen and caloric, or rather between oxygen and the genera-

 tion

tion of caloric by combustion. According to my conception of the experiments of Mr. Davy, they show that combustibles on decombustion absorb the electric fluid: and as we know that no quantity of electric fluid is given out on combustion, it appears probable that this will lead us to the discovery of this relation between oxygen and combustion. We cannot for a moment consider the electric fluid and caloric as synonymous: we must therefore draw this conclusion; That the electric fluid is probably instrumental in producing the caloric of combustion, and that as it disappears therein, it must disappear to form something else. The combustible unites with oxygen, and we find that a large quantity of caloric appears as a large quantity of electric fluid disappears; it is therefore extremely probable that the electric fluid which disappears, forms the caloric which is generated. This is the result I infer from it; and the manner in which I conceive it is effected, I shall now lay down: viz. that there are two electrical fluids in nature; and that the peculiar relation oxygen has to combustion consists in this—that oxygen is the only substance with which one of them combines, and that the other unites with the atoms of combustibles only;—that caloric is generated during combustion by the union of these two fluids; consequently, that caloric is not a simple substance, but that its particles are composed of the two fluids of electricity—this brings combustion to be a double decomposition and combination, one of the products of which is caloric; that the electric fluids in some form or other are blended with the atoms of matter, and like the atoms of alkali and acids in salts veiling each other's properties; that on the combination of oxygen with the atoms of the combustible body, the electric fluids severally combined with each, unite, and form caloric, which is disengaged as gas is set at liberty in other decompositions. As gas lighter than atmospheric air is forced to rise up when at liberty to do so, so caloric, when disengaged, is by some power dispersed on all sides.

It appears probable from a variety of circumstances, that it is the vitreous fluid of electricity that unites with oxygen, and the resinous with combustibles. If we note, therefore, that

that gunpowder, containing one of these fluids in large quantity in potash, and the other to a requisite extent in sulphur, it will not be surprising, that gunpowder should burn *in vacuo,* nor again that it should give out so much heat and light on deflagration. These principles not only account for every phænomenon of combustion relative to gunpowder, and by applying them to combustion by acids, &c., likewise, but at the same time for the heat emitted during combustion in air. Whereas, to reconcile Lavoisier's system, we are obliged to suppose an unnatural chemical union of oxygen, different from its general properties, for which there is no other ground, than the convenience it is of, only to deceive ourselves, by regarding as a property of nature, what is in reality only a property of human imagination, and which, if persisted in, must effectually put a stop to this branch of science.

It must be obvious that these principles will in a most simple and beautiful manner account for the light and heat produced by electrical experiments and friction. To enter into particulars would exceed the limits of this paper. I shall therefore conclude with a view of the process of revification or decombustion. Suppose the body acted upon is iron; A quantity of resinous fluid unites with the metal, and the oxygen of the oxide of iron is separated in the process: it is therefore as strictly a chemical union as that effected in combustion. If we examine the process for the revification of ores, we find it peculiarly adapted to produce this decomposition. A layer of charcoal, then a layer of oxide, is alternately deposited. The heat that is first produced by setting fire thereto, carries off the oxygen of the charcoal in carbonic acid gas; the carbon thus heated has an affinity for, and combines with, oxygen. When, therefore, the oxygen of the charcoal is exhausted, it unites with, and carries off in gas, the oxygen of the ore, at the same time that the resinous electric fluid of the carbon unites with the atoms of the metal. Thus it is that fire both burns and unburns substances. The fire acts no otherwise than by placing the particles of carbon and oxide in a situation in which they have power to act on each other: one part only of the combustibility of the charcoal

coal supplies the heat requisite to this purpose, whilst the other literally undergoes no combustion at all, but is transferred unaltered from the carbon to the iron, or whatever substance is employed; that is, part of the resinous fluid which charcoal as a combustible contains, is expended in the production of heat and gas, whilst the other part unites with the iron (if such was employed) of the oxide, and renders it combustible. The metal is rendered combustible at the expense of the combustibility of the charcoal; and the reason that the iron does not undergo combustion when rendered combustible, is owing to the well-known chemical fact, that substances alter in their properties by different combinations. In this case we find that the resinous fluid (the principle of combustibility) becomes more fixed in the fire by uniting with the particles of iron, &c., than it was when united with the particles of carbon. It appears, therefore, that the resinous fluid (the principle of combustibility) is more or less fixed in the fire, according to its combination with different substances; and that it is owing to this that to burn one combustible one degree of heat is required, and another another.

XXXI. *On the Cause of the different apparent Magnitudes of the same Objects seen under different Circumstances. By* Ez. Walker, *Esq.*

To Mr. Tilloch.

SIR,

At the conclusion of a paper which was printed in the Philosophical Magazine for last October, I mentioned that the apparent magnitudes of all objects are variable, as well as those of the sun and moon.

This, however, is not a new discovery, for M. le Cat mentions it in several parts of his Physical Essay on the Senses, printed in the year 1739. This philosopher says, p. 234, " I looked through the glass of a casement, at a very remote country-seat, which appeared to me sufficiently large. I afterwards fixed my eyes on the glass itself; and it

seemed

seemed to me a great deal smaller than when I looked at it directly. Since that time I have made repeated experiments of this matter, and always found the same circumstances." In another place he says:

"I shall recount still something more extraordinary on this variation of the magnitude of the visual angle, or of the image of objects.

"Last winter I was in the country. In the night it froze hard, and there fell a little snow. On going out of my chamber in the morning, all objects appeared to me sensibly smaller than they had done the evening before.

"Since I made this discovery, and have been guarded against the rule of comparison, I plainly perceive that a very illuminated object seems smaller, and an object feebly supplied with light appears larger. The reason of this is evident. A strong light puts the whole globe of the eye on contracting itself, and a feeble one leaves it relaxed and dilated."

This author is very correct in his observations; but his explanation is founded on a false theory. The true reason is this: A strong light contracts the pupil of the eye, in which state it forms a small picture of an object upon the retina; but in a weak one the pupil is dilated, and all objects then appear larger. This property of vision will, I think, appear evident, from the following experiments.

It is difficult to enlarge the pupil of the eye to any particular dimension, but it may be contracted at pleasure, by means of perforations made either in a thin plate or a slip of paper.

Now, if an object be viewed through an aperture, about $\frac{1}{100}$ of an inch in diameter, it will appear much smaller than to the naked eye, in consequence of the aperture of the crystalline lens being contracted; but if the perforation be removed from before the eye, the object will *instantly* appear increased in magnitude: and as no change can take place in any part of the eye *instantaneously*, it is therefore evident, that the apparent magnitudes of all objects are increased by an increase in the aperture of the crystalline lens, and consequently by an enlarged pupil.

The same thing may also be proved by looking at an object though perforations of different dimensions; for it will appear smaller through a perforation of $\frac{1}{100}$ of an inch, than through one that is four times as large; and an object viewed through a perforation as large as the pupil, appears of the same magnitude as to the naked eye.

Whence it is manifest, that all terrestrial objects appear larger to the naked eye in the mornings and evenings, when the pupil is large, than at noon when the pupil is less; and for the same reason they appear larger in winter than in summer.

I am, sir, your humble servant,
E. WALKER.

Lynn,
March 18, 1808.

XXXII. *On the Identity of Silex and Oxygen. By Mr.* HUME, *of Long-Acre, London.*

To Mr. Tilloch.

SIR,

To inculcate any science with success, there is nothing so essential as a simple and perspicuous display of its first principles; and if there be any department in philosophy to which this observation is more peculiarly applicable, it is certainly the study of chemistry, than which there is, probably, none more useful to man.

The present period is, of all others, the most opportune for an improvement in chemical theory, as, from the very brilliant discoveries of Professor Davy, it is obvious that a most material revolution is now dawning upon the modern system of chemistry, and, possibly, an entirely new structure must eventually prevail. I now allude to the word *oxygen* particularly, which, in its present limited sense, stands as a solecism in language, and a mere absurdity in the nomenclature of the day, since it has been lately proved to be at once the principle of *acidity* and likewise that of *alkalescence*.

In all modern authors, the classification of simple and elemen-

elementary bodies is, I presume, too diffuse, in respect to the number of subdivisions; and many of the titles employed might, with propriety, be expunged. But, though there are many other imperfections in chemical arrangement that require reform, I mean on this occasion to confine the following observations to one article only; and shall endeavour to prove that, in this instance at least, we should revise the list of simple substances, as far as regards SILEX; which is still continued, I think, with great impropriety, to rank as a species of *earth*.

To include in any one genus both silex and the other earths, as they are now called, seems extremely improper and palpably erroneous; nor can this classification be supported by any reasonable argument whatever. The definitions given from time to time, to distinguish an earth from any other elementary body, have never been sufficiently explicit, for they do not precisely exclude the alkalis: they make a useless division under the name of *alkaline* earths; and, as *salifiable bases*, an earth, a metal, an alkali, and silex, may be said to range as four distinct species of the same genus.

All earths are declared to be salifiable bases, and this I take to be the most essential clause in every definition; for, generally speaking, the earths have a ready affinity for every acid, even from the weakest, particularly the carbonic acid, to the most powerful that exists. Indeed, as far as concerns the combination with carbonic acid, with which the earths form nearly *insoluble* compounds, this peculiar property alone might have served to distinguish an earth from an alkali. Here, however, the principal force of the definition fails, the exception to silex is decisive; there is no carbonate of silex; no nitrate, no sulphate, nor, in short, any other salt, in which the acid is saturated by this simple element: neither art nor nature ever produced a perfect neutro-saline compound, in which silex could fairly be considered as a real and independent base.

To constitute a true salt, we know, there must not be less than one acid and one base, reciprocally saturating each other; and when the number of either exceeds, and the

salt

salt is not a binary compound, we may then fairly suspect an imperfection; for, one of the elements at least is frequently in the state of mere suspension, and not in chemical union with either of the other ingredients.

There are, indeed, numberless examples of such salts, and, I am ready to allow, some in which silex is found, whether as a mere contingency or otherwise; but it never exists as a perfect base, that is, possessing the capability of saturating the whole, or any part of the acid in such compositions.

If silex be, what I have long considered it, not only dissimilar to every elementary ponderable material besides, especially in generic characters, but, also, so vastly superior in its importance and bulk, as to leave no room for comparison; surely it ought then to be instantly removed, and no longer suffered to remain in the list of earths, but should be placed in the most prominent station in the arrangement of elements. Such is its consequence, that nothing in nature is so predominant or so universally disseminated; no compound solid substance of any magnitude is exempt from it, but contains always some, if not a very large portion of this insinuating, and as I conjecture, most essential of all terrestrial matter.

All organised bodies either contain silex, or, what I shall consider as a modification, oxygen. If there be any exceptions to this conclusion, they are so few and of such trivial import, that when they do occur they should be rated as anomalies; and it may happen, that the apparent absence of silex or oxygen is rather to be attributed to our want of means, and the imperfection of science to discover it.

In a geological view of this subject, where can we turn our eyes or employ our thoughts, without meeting this grand and multifarious cement—this bond of aggregation, that fixes the solidity of all tangible nature? The very outlines of our planet are traced out with it; and all primitive matter, from the most stupendous mountain or ragged precipice to the deepest cavern, even to the centre of gravitation, we are warranted to say, is replete with silex. If we contemplate the nature, volume, and importance of this, and then recol-

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lect the insignificance of zircon, glucine, and indeed the whole of the species of earths, none of which exists without an association of silex—all comparison vanishes, there is no estimate; these are as the mere spots to the brightest of luminaries, and therefore, in all systematic classification, should be separately arranged.

Where then ought silex to be placed in the arrangement of simple elements?—should it link with any other ponderable body as a species of the same genus, or preserve a station to itself? Were I asked for an answer to such a question, I would say—that seeing nothing to which it has the slightest resemblance but oxygen-gas, of which I conceive it to be the true base, here I would not only assign its proper rank, but give it also a precedence to all other elementary matters that had resisted decomposition.

It is hardly necessary for me now to add, that I do not consider oxygen in the state of *gas* to be a simple body; for whatever is susceptible of spontaneous change should always be deemed a compound of at least two elementary substances. If one instance of this can be adduced, we may naturally infer that others will be found; and, fortunately for my present purpose, a most appropriate example has lately occurred, which confirms this conclusion: I allude to the experiments of Messrs. Allen and Pepys *, upon carbon and carbonic acid, which appear to have been conducted with uncommon precision and genius. From these gentlemen we learn, that oxygen-gas is subject to spontaneous change, or, as they very properly express it, a deterioration; and that this will happen, though the gas be of the purest kind, that obtained from oxy-muriate of potash; and even when secured in glass vessels with glass-stoppers.

Having assumed silex and the *base* of oxygen gas to be synonymous and simple bodies, I shall now proceed, as far as my humble pretensions and knowledge of this subject will permit, to substantiate this position, by offering a few only of the numberless facts, which seem to confirm this identity. It is a task, I confess, I have imposed upon myself; for, having nearly three years ago permitted my opinion to be

* Philosophical Transactions 1807.

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published, though unaccompanied with any explanation or proof, and as the work * has now arrived at its second edition, it becomes my duty to absolve its author from all responsibility; and, rather than any blame should attach to him, avow myself as the only person, who is accountable for promulgating tenets, which to many philosophers must have appeared to be visionary.

It is scarcely necessary to explain what is here meant by the word *silex*. But, that I may be clearly understood, I shall define it to be, the very *pure* part of rock-crystal, and that which constitutes by far the greatest portion of all sand, flint, gravel, and other well described rocks, stones, and minerals: a substance common in every spot of the globe, in every zone, and in every climate; and an article so obvious and familiar to the meanest capacity, that any further description would be superfluous. I shall just observe, that in rock-crystal, in quartz, and in hot-springs, silex is nearly in its pure and primitive state of perfection.

There is no subject, in which analogical reasoning is more admissible or more conducive to arrive at the truth, than the one before us: and indeed, whenever the discussion has for its object the works of nature, as in chemistry and its sister-science, geology, I do not see it possible how this mode of argument can be well avoided. It was analogy that led the penetrating mind of a Newton to some of the most brilliant of his discoveries; and it was the same faithful guide that conducted this immortal philosopher to predict some of the most important truths, which have since been so completely established by the experiments of his successors. The combustible nature of the diamond, and likewise that of water, are among these examples: they are facts that will for ever bear testimony to the great advantages that may be derived from this method of searching into the secrets of Nature's unerring works, and the laws which these obey.

Now, to apply this mode of reasoning to the present object of research, let us consider this our sublunary world under its three grand divisions. The first, is the atmosphere, which surrounds and compresses the whole of the others; and this may be called the *aëriform* division of

* "Chemical Catechism."

nature

nature. Here, it is allowed, the principal element is oxygen; but it is now in the *gaseous* state, that is, it is saturated with caloric. I have said, the *principal* element, because it is the most important of all others:—it is the matrix of fire, it is the pabulum of life; in short, such is its consequence and value to the very being of all organized matters, whether in the animal, vegetable, or mineral kingdom, that surely some more appropriate name might have been devised, than what it now bears. Though it is a digression, and remote from my plan, I shall take the liberty to hint, that merely by modifying, that is in some measure reversing, the theory which first employed the word *phlogiston,* both this word and the theory itself might with the greatest propriety be revived: and the word phlogiston, even in the theory of the present day, would more aptly suit our comprehension of all the properties of pure air, than that of *oxygen,* which implies merely the generator of vinegar or *sourness,* a derivation of all others the most puny and incomplete.

The second grand division, is the ocean, sea, or water, which we may name the *aqueous* portion of the whole. Here we again recognise our oxygen, not only as the principal ingredient in magnitude, being about four-fifths of the whole, but in all other respects claiming our first attention. In this water, the oxygen is further concentrated, having lost a part of the caloric which it possessed in the gaseous form, or in the atmospheric state; so that, in this case, we may now conceive it to be, in regard to density, midway between earth and air; and that, by an abstraction of more of its caloric, it must approach nearer to a state of solidity.

If oxygen, therefore, constitutes such a prominent and striking feature in two-thirds of the works of the Author of all creation—which, in these cases, is a truth that admits of no controversy. Why, it may be aptly demanded, should it not, also, form the most conspicuous ingredient in the other third, that is, the *solid* or real terrestrial portion of this material world? Analogy and the general complexion of all the phænomena of nature seem to answer in the affirmative, and, I think, will afford some of the most legitimate proofs, in confirmation of the doctrine I have assumed, the identity of silex and oxygen.

This theory seems to be supported by such a mass of evidence, that it is difficult to say where we should begin. Geology is, however, a source so prolific, that every spot of the globe teems with examples: There is not a rock, from the most huge and congregated lumps of matter, that render the face of nature at once awful and magnificent, to the most trifling pebble; nor is there a morsel of any mineral compound, whether it be the brilliant gem or the most unfruitful and degraded soil, where, if there be an earth, a metal, an alkali, or any other salifiable or oxidable element, the saturation is not always due either to silex alone or to some acid, that is, consequently, something containing oxygen. Such seems to be their equivalence, that when silex is absent some acid must prevail; and if neither be found in the association, then the earth, metal or alkali, whether potash or soda, puts on its obvious and peculiar character, such as taste, solubility, density, and the other generic qualities proper to each species

After silex, there is no substance so plentiful as *lime*, but this is never found pure; it is either saturated with an acid, or dwindles into a tasteless inert state of aggregation with other bodies, where it is subdued and locked up by silex; so that, there is not a vestige remaining of its primitive qualities, especially those of taste and solubility.

That lime, even when pure, is a compound, there is little room to doubt; and that carbonate of lime, or chalk, is produced at the expense, and through the means, of the degradation of silex, may probably deserve a candid and minute inquiry.

[To be continued.]

XXXIII *On the Public Utility of Medical Institutions for the Benefit of the Diseased Poor.*

To Mr. Tilloch.

SIR,

THE foundation and support of the many medical establishments for the benefit of the diseased poor, not only in the

the metropolis but throughout the kingdom, reflects the highest honour on the national character. This has long been acknowledged; and it could not fail to be highly gratifying to the public, who contribute so largely towards their support, to have a statement published annually of the receipts and disbursements of each institution, with an account of the benefits derived from them to the diseased poor. I do not think this desirable object is sufficiently attained, by the committees of a few charities circulating a report of the finances of the institutions over which they preside, among their own members or subscribers. The object in view would, I conceive, be best attained by means of some periodical work, where the reports should be recorded and referred to; by which the advantages of each charity to the *diseased poor* would be made evident; and a liberal public would in a few years be enabled to form a correct judgment as to what kind of charitable institutions was best entitled to their munificence.

The frequent, and in some instances successful, attempts to depreciate the utility of dispensaries, have led to these remarks; and when it is known that for an annual sum of 2,000*l.* upwards of 9,000 of the diseased poor are annually admitted and attended in *three* of these institutions in London, (those persons being visited at their own houses who are too ill to go to the charity,) it must be confessed that *dispensaries* are somewhat *more* than "*so many* HOT-BEDS *calculated to rear and cherish their plants* for the public service.*"

It is not my intention at present to analyse the motives which probably gave rise to the above and many similar observations, it being sufficient to record the fact; neither do I mean to advance any thing that should militate in the least against the many truly valuable public hospitals; but only to observe, that the great BENEFIT derived by each individual inhabitant of these asylums prevents its being extended to the numbers requiring aid, their establishment not being of sufficient magnitude to lodge and feed all the diseased poor: nor would it be sound policy to extend their

* The medical officers.

benefits

benefits so far; it being proved by the above statement, that 9,000 patients are attended annually in three dispensaries, a sum not more than adequate to the maintenance of 470 in an hospital.

Greville street, Hatton Garden, February 24, 1808. JOHN TAUNTON.

XXXIV. *On the constituent Principles of Potash. By* MARK TAERG, *Esq. of Beeston, near Shrewsbury.*

To Mr. Tilloch.

SIR, March 21.

IN a letter I sent you, dated September 1806*, I suggested the probability that oxygen was an ingredient in the composition of potass. This has been now confirmed by Mr. Davy: and although I was not right as to the quantity, I think I may claim some little merit. The rest, I said, was lime and though here I must have been mistaken as to the quantity, it still remains to be disproved that it does not enter into the composition of potass. May not lime be an oxide of the same metal as potass; or may not that metal united with other substances form lime? I think the subject worthy of investigation: and unless some one abler than myself takes it up, I shall trouble you with an account of the result of some experiments which I am about to make on it.

your's,

MARK TAERG.

XXXV. *On the best Means for preventing the fatal Consequences that so frequently occur from the Dresses of Females and Children taking fire.*

To Mr. Tilloch.

SIR,

NOTHING can be more distressing to human minds than the accounts so frequently given in our public prints, of women and children being burnt to death by the accident of their

* See Phil. Mag. vol. xxv. p. 358.

their clothes taking fire. Desirous of ascertaining how far persons in that horrible situation are likely to be relieved by the judgment and exertions of those who might casually be near them, I have for some time past made a point of turning the conversation to this subject, among friends, acquaintance, and strangers; and regularly inquired of the men, how they would act, supposing themselves *tête à tête* with a lady whose clothes had caught fire. I found them generally slow to reply;—that not one of them appeared at all prepared against such an event; and that their resources were few and shockingly defective. Some thought they must be guided by circumstances: forgetting that coolness and decision, which are essentially requisite in such trying predicaments, do not attend all men in the moment of alarm and danger. Others would pull off their coat to put over the flames and smother them:—but the greater number were for rolling her in a carpet. None ever mentioned any thing preferable: This last expedient appeared to be thought the best that could be devised—the *ne plus ultra.*—It had not however occurred to them that the apartment might not be furnished with a carpet. This sad experience leads me to fear, that whoever will take the trouble to repeat the experiment among his friends and acquaintance, will not obtain any more satisfactory results.

Although the insufficiency of these means must on a moment's reflection be obvious to the humblest capacity, still a kind of infatuation has made them be persevered in without further thought.—No rational plan is formed—All is in fact left to chance; the consequence of which must necessarily prove fatal in nine instances out of ten.

In circumstances of this nature, relief itself would be extreme torture, unless it be prompt: not an instant must be lost: but while chairs, tables, and other incumbrances are to be removed off the carpet; and before the sufferer can be laid down in it, or that it can be drawn over her, the flames will make rapid progress; and until she is entirely wrapped up in it, the flames are fanned and urged by the operation, which moreover takes up too much time.

Considering the multitude of valuable lives which have perished

perished in this miserable way—many of which it is probable might have been saved, had better means been employed —and that families of every rank, without exception, are equally liable to this sudden and dreadful visitation, it might naturally be expected that something more adequate, to mitigate its effects, would ere now have been made known and adopted.

In the belief that nothing of the kind has yet been done, I think it a duty incumbent on me to suggest, through your medium, what will be found an infallible method instantly to extinguish the flames which have caught a female's dress—which is as follows:—

The hands of any assistant must be passed under all the clothes, to the sufferer's shift; and then the whole clothes are to be raised up all together, and closed above her head. The flames will thus most certainly be extinguished. This may be done in five or six seconds—in the time that a person can stoop to the ground and rise up again,—and *no other method can be so ready, expeditious, and effectual.*

The sufferer will facilitate the business by folding her arms close before her. Should it happen that no person is at hand to assist the sufferer, if she has presence of mind, she may in most cases relieve herself by throwing her clothes over her head and rolling or laying upon them.

This method was always communicated to those with whom I conversed on this subject, who all expressed the greatest satisfaction at the probability, and confidence it gave them, of being thereby, better than hitherto, enabled to rescue fellow-creatures from agonizing premature death; and esteemed it a valuable addition to their small stock of expedients.

The extending this communication to your readers may be productive of the happiest consequences to them and the community at large, and materially reduce the list of human afflictions. I am, sir,

your very humble servant,

March 22, 1808. E. V.

XXXVI. *Let-*

XXXVI. *Letter from Sir* H. C. Englefield, *respecting his Mountain Barometer.*

To Mr. Tilloch.

SIR,

In a note at the bottom of page 19 of your last number, I preferred having the lower part of the tube only a twentieth of an inch in the bore, something on the principle of the marine barometer. Upon a more minute investigation, I do not find that this would answer the purpose so well as I then thought, nor is it so good as the simple tube. I therefore beg your insertion of this for the information of your numerous readers. I am, sir, &c.

H. C. Englefield.

March 21, 1808.

XXXVII. *Report of Surgical Cases in the City and Finsbury Dispensaries, for October* 1807; *with some Remarks on the Dissection of the Brain of a Person who died insane. By* John Taunton, *Esq.*

In the month of October there were admitted on the books of the City and Finsbury Dispensaries 211 surgical patients.

Cured or relieved		—	193
Died	—	—	2
Under cure		—	16
			211

Since which time there have been admitted 989.

A few days since, I was requested to examine the head of a person who had been insane for some months preceding death, which took place suddenly, ætat. about 35.

The general appearance of the body, which was robust, indicated a high degree of health and strength.

On removing the upper part of the scull, the dura mater formed a considerable projection over the posterior and superior part of the left hemisphere of the cerebrum, near the course of the longitudinal sinus. On cutting through the external

ternal lamina of that membrane, a granulated substance appeared, like a great number of small tumours adhering to the dura mater, tunica arachnoidea, pia mater, and even to the cortical substance of the brain, about two inches in length, half an inch in breadth, and nearly half an inch thick. The other part of the dura mater was of the natural appearance.

The arachnoid membrane was more opake than usual, arising partly from lymph deposited on its under surface, and partly from an effusion of a serous fluid between that membrane and the pia mater.

The cerebrum and cerebellum appeared natural, but the lateral ventricles contained about four ounces of a straw-coloured fluid; the surface of these cavities was every where covered with a layer of coagulable lymph, having that asperated appearance peculiar to recent inflammation. Many small hydatids were attached to different parts of the choroid plexus.

Is it not highly probable that the symptoms of insanity took place in consequence of the pressure produced by the tumour upon the brain? and also that the inflammation was excited by the same cause? If these positions be admitted, Would not the antiphlogistic mode of treatment, carried to an extent agreeable to the apparent stamina and strength of body, which have been rarely exceeded, have produced a more favourable termination?

Since I examined this case, I have heard from the medical gentlemen who dissected the body of Simmons, the murderer, that half an ounce of water was found in the lateral ventricles of the head. I believe it was never doubted that this unhappy person laboured under a brutal and ferocious insanity from his infancy.

XXXVIII. *Report upon a Memoir read at the French Institute, by M.* Thenard, *upon the Nitrous Ether. By Messrs.* Guyton, Vauquelin, *and* Berthollet*.

Different kinds of ether have been formed by the action of some acids upon alcohol. Volatility, inflammability, and

* *Ann. de Chimie,* tom. lxi. p. 282.

and a specific smell, give to ethers a decided character, which does not admit of their being confounded with other substances. We know but imperfectly, however, the differences which distinguish them from each other; and in particular, we have but an imperfect theory upon their production. Messrs. Fourcroy and Vauquelin have indeed thrown a great deal of light upon the production of the sulphuric ether; but their explanation cannot be extended to that of some other ethers. It was therefore important to resume the subject, in order to treat it in a general manner. This is what M. Thenard undertook;—In the first memoir presented to the Institute, he treats of the nitric ether. He will afterwards proceed to the others; and will examine why some acids have the property of producing ethers, while others are deprived of it.

M. Thenard first brings under review the processes recommended by chemists for producing the nitric ether. These are very discordant, and only have for their object the etherized liquor, which we may obtain without any analysis of the gaseous products, nor any consideration of the circumstances of the operation, unless we except the Dutch chemists, in a memoir which has particularly occupied M. Thenard's attention, at the end of his own.

M. Thenard began by distilling a mixture of equal weight of alcohol and nitric acid, both being of a determinate concentration, in an apparatus proper for separating the liquid products from the gaseous; a slight heat is sufficient, and even the action becomes so brisk that it is soon necessary to check it. He afterwards examined the residue of the retort, the liquid produce, and the gases. The residue was composed of nitrous acid, acetic acid, alcohol, water, and a little of a matter the nature of which is not determined, but which chars easily. The proportions of these substances are established by ingenious and precise means. But we are obliged to pass over the details necessary for a clear idea of the numerous operations M. Thenard's experiments require;—if we push the distillation to dryness, the viscous residue contains oxalic acid, and probably malic acid.

The liquor distilled, which has been regarded in laboratories

as

as the nitric ether, is found to be composed of water, nitrous acid, acetic acid, ether, and probably alcohol.

The gaseous product has in particular required much patience and dexterity, in order to separate it into different elements; to assign to each of these elements the properties which belong to it; and to explain the differences which result from the circumstances in which this gas is placed. It was composed of the nitrous gas, azote, oxide of azote, nitrous acid, carbonic acid, and of etherized gas, which it was particularly necessary to detach from the rest, in order to examine its properties. The author was led by these preliminary experiments to the following process, in order to separate the pure ether, and to examine it, whether in its liquid or gaseous state.

He put into a retort five hectogrammes of alcohol, and as much nitric acid. To the retort were successively adapted, by means of glass tubes, five long flasks half-filled with water saturated with muriate of soda. The last had a tube, which opened under a bell-glass, filled and destined to collect the gaseous part. All the flasks were surrounded with a mixture of pounded glass and sea-salt, which was stirred from time to time. The operation began by means of a little fire; but it soon became necessary to extinguish it, and even to cool the retort.

The liquid remaining in the retort was analogous to that in the first-mentioned experiment.

There was found upon the surface in all the flasks a yellowish liquid, and which, when collected, weighed 255 grammes. That contained in the first flask was a mixture of alcohol, ether, acetic acid, and nitrous acid; that contained in the other flasks was nitric ether, free from alcohol. In this state the nitric ether possesses a strong smell; it is specifically lighter than water, and heavier than alcohol; it is dissolved in the latter in any proportion, but it requires nearly 48 parts of water to dissolve it, and yet the latter dissolves it partly as we subsequently find. It presents in a strong degree the properties of combustible bodies. Nevertheless, this ether strongly reddens turnsole tincture; and it owes this property to a little nitrous acid and acetic acid,

which

which it retains, and which we may separate from it by means of lime.

The volatility of the ether thus prepared is such, that the tension it indicates is 0·73 metres, while that of the best sulphuric ether in the same circumstances is no more than 0·46 metres, at 21° in the centigrade thermometer, and 0·76 metres of atmospheric pressure. We see, therefore, that at this temperature and pressure it is at the limits of its existence in the liquid state.

But if we can deprive the nitric ether of its acidity by means of lime, it hastens to become acid again, whether we distil it, leave it in contact with the air, or keep it in well stopped bottles. This formation of acid also takes place when we treat ether with water, particularly if the temperature is from 25° to 30° of Reaumur. The author explains the formation of the acid, by the reciprocal action of the principles which constitute ether, and which are there feebly retained by combination.

M. Thenard afterwards proceeds to the decomposition of the nitric ether by heat, and he analyses the gases which proceed from it, founding his calculations upon the most exact data hitherto found: he obtains as a result, that 100 parts of nitric acid is composed (laying aside fractions) of

Azote	—	16
Carbon	—	39
Oxygen	—	34
Hydrogen	—	9

From this he concludes what passes in the reciprocal action of alcohol and nitric acid. The oxygen of this acid is combined with a great part of the hydrogen of the alcohol, and with a very small quantity of its carbon. From this there results, 1st, A great deal of water and gaseous oxide of azote, a little carbonic acid, and a little nitrous gas and nitrous acid; 2dly, The separation of a small quantity of azote, and the formation of a great deal of nitric ether, by the combination of a sufficiently large quantity of the two principles of the nitric acid with the de-hydrogenated and slightly decarbonized alcohol; 3dly, The formation of a little acetic acid, and a small quantity of a substance which is is easily carbonized by a combination of one part of hydrogen, carbon, and oxygen.

Supported by these results, M. Thenard discusses the processes which have been published for obtaining the nitric ether; and he shows that some of them are dangerous in the execution; and that the whole furnish but a part of the ether which may be obtained from the same quantity of ingredients, and only yield liquors more or less compound, in which the nitric ether, the name they go by, in reality forms but a part.

The Dutch chemists have made some interesting inquiries respecting the nitric ether, or rather upon the gas obtained by the action of the nitric acid upon alcohol. They have, however, resorted to an insufficient hypothesis, in order to explain the curious facts they communicate. 1st, They have regarded the gas in question as a combination of nitrous gas and ether, while it is composed of gaseous ether, nitrous gas, nitrous acid, azotic gas, gaseous oxide of azot, carbonic acid, acetic acid, and, in short, of all substances susceptible of assuming the gaseous form in the variable circumstances in which they exist. 2dly, They have supposed that ether was an identical substance; so that they have entirely neglected to analyse the nitric ether, and establish its distinguishing characters. 3dly, A consequence of this opinion is, that they have attributed to a pre-existing nitrous gas, phænomena which are owing to the decomposition of the nitric ether.

After having discussed the opinions and experiments of the Dutch chemists, M. Thenard concludes his memoir, by remarking that he has only considered the products and phænomena obtained in a given proportion, and in determinate circumstances; the effects will be different under other conditions which he purposes to introduce: but he has already convinced himself, that those he has used are most favourable to the production of the nitric ether.

M. Thenard's memoir contains a great number of new facts, and very delicate analyses. He determines the nature of a substance] very remarkable in its properties, and yet he only presents it as the beginning of a laborious work

 only

upon ethers, of which we pledge ourselves to give an account.

The Institute has approved of the present report, and adopted its conclusions.

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 3, 10, and 17, (A. Marsden, esq., vice-president, in the chair,) Dr. Richardson's geological observations on the North of Ireland were read. The Doctor confined himself to what he conceived to be peculiar facts, or rather the local features of the basaltic mountains in the counties of Antrim and Derry, dividing the strata which appear in some of the most perfect columnar basaltes into sixteen divisions, of different depths. He traced the appearance and disappearance, in several mountainous places, of those ridges called whin-dikes, and thence inferred that the whole basaltic district must have originally been one continuous mass, and that the separations or divisions, which now form extensive plains and valleys of several miles extent, must have been formed by some power in nature with which we are at present unacquainted. Dr. Richardson thinks, neither the Neptunian nor Volcanic theory sufficient to explain the varied phænomena of nature, which must be left to future ages and discoveries.

Feb. 24.—The reading of observations on the late comet, by Dr. Herschel, commenced. The Doctor defined the terms of head, nucleus, disk, coma, and tail; and directed his experiments to ascertain the real dimensions of its nucleus and disk. These he performed by comparisons with wax-balls viewed in his ten feet reflector, the results of which he applied to the apparent magnitude of the comet's disk.

SOCIETY OF ANTIQUARIES.

Mr. Lysons laid before this society an account and drawings of a mosaic pavement found by him at Frampton, Dorsetshire, in 1796. This was one of the largest mosaic pavements found in modern times; it was 30 feet long, and 20 broad,

broad, divided into several compartments, with figures of the heathen gods and other emblems. It could not be ascertained to what this mosaic had been appropriated, only that it was surrounded by a hard clay floor.

GEOLOGICAL SOCIETY.

On the 13th of November last, a society was formed in London under the above title, of which we have avoided giving any notice, till we could announce its objects and constitution.

Of its Objects.—This society is instituted for the purpose of making geologists acquainted with each other; of stimulating their zeal; of inducing them to adopt one nomenclature; of facilitating the communication of new facts; and of contributing to the advancement of geological science, more particularly as connected with the mineral history of the British Isles.

Of its Constitution.—The geological society shall consist of a patron, president, two vice-presidents, treasurer, secretary, ordinary and honorary members. The president, vice-presidents, treasurer, and secretary, shall be annually elected by ballot.

Of its Members.—I. Members shall be chosen by ballot, the election to be unanimous.—II. Any person desirous of becoming a member, may communicate his wish through the secretary to the society; and, without being proposed or recommended in any other manner, shall be balloted for at the next meeting.—III. Any member desirous of proposing an honorary member, shall communicate his wish through the secretary to the society. The person so proposed shall be balloted for at the next meeting: if elected, the name and recommendation of the member proposing shall be read and noted in the minutes; if rejected, they shall be suppressed.

Of its Meetings.—I. The society shall dine together on the first Friday of every month, from November to June inclusive, at five o'clock precisely.—II. Business shall commence at seven o'clock, and be conducted in the following order: 1. The minutes of the preceding meeting read and approved,

2. Notices of new motions presented. 3. Members proposed. 4. Members balloted for. 5. Motions on the minutes brought forward and determined. 6. Miscellaneous business. 7. Geological communications read, and presents acknowledged. At nine o'clock the president shall leave the chair.—III. All questions on which a difference of opinion may arise, shall be determined by ballot at the next ordinary meeting.—IV. If on any occasion the numbers be equal, the chairman shall have a second or casting vote.—V. At the last meeting in June, the officers of the society shall be elected; the accounts of the late treasurer shall be passed; and a subscription raised to defray incidental expenses incurred by him for the purposes of the society.—VI. A special general meeting may at any time be called by the secretary, in consequence of instructions received from the president, or a requisition signed by three ordinary members. The object must be stated to the secretary, who shall give each member intimation of it, and of the time and place of meeting, three days previously.

Of Communications.—All communications must be sent to the secretary; and, previously to their being read before the society, shall be approved by a committee composed of the officers of the society, and of three members chosen by ballot. Three to constitute a quorum. This committee shall be appointed at the June meeting, after the election of officers, and shall be trustees of all presents to the society.

Of Visitors.—I. Each member shall be at liberty to introduce a visitor; besides whom the president, or chairman for the time being, may admit any person he may think fit, with the limitation specified in the succeeding article.—II. No person resident in London can attend more than two meetings of the society without becoming a member.

List of Officers.

Patron, Right Honorable Ch. F. Greville, F.R.S. &c.—President, G.B. Greenough, esq. M.P. F.R.S.—Vice-presidents, William Babington, M.D. F.R.S.; H. Davy, esq. Sec. R.S. Prof. Chem. R.I.;—Treasurer, W. H. Pepys, esq.;—Secretary, J. Laird, M.D.

Ordinary Members.

Arthur Aikin, esq.; William Allen, esq. F.R.S. &c.; Right Honourable Sir Jos. Banks, bart. K.B. Pres. R.S. &c.; M. le Comte de Bournon, F.R.S.; J. Franck, M.D; Richard Knight, esq.; J. Parkinson, esq.; Richard Phillips, esq.; William Phillips, esq.; J. G. Ridout, esq.;

Honorary Members.

Thomas Allan, esq. Edinburgh; Robert Bevan, M. D. Monmouth; Richard Bright, esq. Bristol; Jos. Carne, esq. Penzance; M. P. Carter, M. D. Braintree; R. B. Cheston, M.D. F.R.S. Gloucester; E.D. Clarke, L.L.D. Jesus College, Cambridge; William Clayfield, esq. Bristol; Rev. J. J. Conybeare, Christ Church, Oxford; David Crawford, esq. Donegall; J. Dennis, esq. Penzance; L. W. Dillwyn, esq. F.R.S. Swansea; F. Dugard, M.D. Shrewsbury; Right Honourable John Foster; Rev. W. Gregor, Creed, Cornwall; J. Griffiths, esq. Mellecent, Ireland; Rev. J. Hailstone, Prof. Nat. Hist. Cambridge; J. Hawker, esq. Dudbridge, Gloucestershire; J. Hawkins, esq. Sussex; Rev. J. Hincks, Cork; T. C. Hope, M.D. Prof. Chem. Edinburgh; R. Jameson, Prof. Nat. Hist. Edinburgh; J. Kidd, M.D. Prof Chem. Oxford; Richard Kirwan, esq. L.L.D. F.R.S. P.R.I.A. Dublin; Robert Lovell, M.D. Bristol; J. Mac Donnell, M.D. Belfast; Sir George Mackenzie, bart. Inverness; Thomas Meade, esq. Chatley, Bath; H. Menish, M.D. Chelmsford; John Murray, esq. Lect. Chem. Edinburgh; D. Mushet, esq. Alfreston Iron-Works, Derbyshire; Rev. Benjamin Newton, Chatley, Bath; C. H. Parry, M.D. F.R.S. Bath; Sir. Christopher Pegge, F.R.S. Reg. Prof. Physic, Oxford; J. Playfair, Prof. Nat. Phil. Edinburgh; Philip Rashleigh, esq. Menabilly, Cornwall; William Rashleigh, esq.; W. Richardson, D.D. Portrush, Ireland; W. Roberts, M.D. Gloucester; Lord Webb Seymour, Edinburgh; John Taylor, esq. Holwell-House, near Tavistock; T. Thomson, M.D. Lect. Chem. Edinburgh; Rev. Jos. Townsend, Pewsey, Wiltshire; Rev. W. Turner, Newcastle; J. Ure, M.D. Glasgow; J. Vivian, jun. esq. Truro; J. Watt, jun. esq. Birmingham; J. Wilkinson, esq. Bath; J. Williams, jun. esq. Scorrier House, Cornwall.

MEDICAL

MEDICAL SOCIETY OF LONDON.

This society held its anniversary meeting on Tuesday the 8th of March. After the election of office-bearers for the ensuing year, Mr. Good, a distinguished member of this learned body, delivered an extempore oration, the subject of which was a survey of the theory of the general structure and physiology of plants, compared with those of animals; the process by which vegetable matter is converted into animal matter, so as to become the basis of animal nutriment and support; and the mode by which animal matter is afterwards reconverted into vegetable, so as to complete the circle of action, and to restore to plants the nutritive benefit which has been antecedently derived from them.

In this interesting discourse, Mr. Good took a comprehensive view of the wisdom and goodness of the Creator, which pervades the universe, and is manifested throughout the various and innumerable links of that chain by which the endless varieties of organized beings are bound together, and rendered mutually dependent on each other.

At the unanimous request of the society, Mr. Good has consented to publish this oration, in as verbal a form as his recollection may suggest to him.

WERNERIAN NATURAL HISTORY SOCIETY.

A society has been established at Edinburgh for the cultivation of the different branches of natural history. It is denominated the *Wernerian Natural History Society*, in honour of Werner.——The following gentlemen have been elected office-bearers.

President—Robert Jameson, esq. F. R. S. Prof. Nat. Hist. Edin. Vice-Presidents—Wm. Wright, M.D. F.R.S.; Rev. T. Macknight, F.R.S.; John Barclay, M.D. F.R.S.; and Thos. Thompson, M.D. F.R.S. Patrick Walker, esq. treasurer. Patrick Neil, esq. secretary. Council—nine in number—viz. the above office-bearers, with Charles Anderson, esq. F. R. C. S, and Lieut. Col. Fullerton of Bartonholm.

Sir Joseph Banks; Richard Kirwan, esq. president of the Royal

Royal Irish Academy; and Professor Werner, of Freyberg, were elected the first honorary members. The following foreign members have been elected:—viz. Professor Karsten, Berlin; Professor Klaproth, Berlin; M. Von Humboldt, and M. Von Buch, also of Berlin. M. F. Mohs, of Stiria; M. Herden, M. Meuden, and M. Friesleben, of Saxony.

At the last meeting of the Wernerian Natural History Society, Professor Jameson read a description of cotemporaneous or inclosed veins. He divided veins into two classes. The first class comprehend *true veins,* the second *cotemporaneous* or *inclosed veins.* Some veins, he remarked, excepting where the beds and strata are of very great thickness, traverse many different strata or beds; and although we do not always observe them open at the surface of the earth, they invariably open at the surface of the formation or series of formations they traverse: thus, the outgoing or opening of certain metalliferous veins that traverse clay-slate and mica-slate is sometimes covered by the second porphyry formation. Cotemporaneous or inclosed veins are in general confined to individual beds or strata, and are fairly inclosed in them, or in other words wedge out in every direction in the bed or stratum in which they are contained.

After detailing the various characters of true and cotemporaneous veins, the Professor next described the cotemporaneous veins that occur in the different great rock-formations, beginning with granite, and ending with the newest flætz trap formation. He next explained the mode of formation of these veins, When describing the cotemporaneous veins that occur in gneiss, he remarked that certain varieties of venigenous gneiss bear a striking resemblance to granite, and hence have been frequently confounded with that rock. This led him to point out the characters by which true granite veins are distinguished from veins of granitic gneiss. As connected with this part of the subject, he examined the facts on which the Huttonian theory of granite is founded; and proved by a detail of his examination of the appearances described by Dr. Hutton, Professor Playfair and others, that the supposed granite veins shooting from

from subjacent granite into superincumbent rocks, are merely veins of granitic gneiss accidentally in contact with granite.

XL. *Intelligence and Miscellaneous Articles.*

NEW BOOKS.

Professor Jameson has just published the third volume of his system of mineralogy, under the title *Elements of Geognosy*. The contents of this important work are as follows: Chap. I. Description of the surface of the earth. Chap. II. Effects of water on the surface of the earth. Chap. III. Internal structure of the earth. Chap. IV. General account of the different formations, in regard to their succession and stratification, and this illustrated by a short description of the Hartz and Saxon Erzgebirge. Chap. V. Theory of the diminution of the waters of the globe—Description of overlying formations.—An investigation of the original contents of the waters of the globe, during the different periods of the earth's formation—The division of rocks into five classes. Chap. VI. Class 1. Primitive rocks. Chap. VII. Class 2. Transition rocks. Chap. VIII. Class 3. Flætz rocks. Chap. IX. Class 4. Alluvial rocks. Chap. X. Class 5. Volcanic rocks. Chap. XI. Mineral repositories. Chap. XII. Relative age of metals, viz. 1. Molybdæna. 2. Menachine. 3. Tin. 4. Schiele. 5. Cerium. 6. Tantalum. 7. Uren. 8. Chrome. 9. Bismuth. 10. Arsenic. 11. Cobalt. 12. Nickel. 13. Silver. 14. Copper. 15. Gold. 16. Sylvan. 17. Antimony. 18. Manganese. 19. Lead. 20. Zinc. 21. Mercury. 22. Iron. 23. Platina.—General inferences. To these follow a table of 32 pages containing the relative antiquity and geognostic relations of simple minerals: also an extensive table of the most remarkable heights of mountains, hills, and lakes in different parts of the world, and a table of volcanoes. The volume is concluded with a series of notes explanatory of passages in the text, and referring to the Huttonian theory of the earth.

Mr.

Mr. John Ayrton Paris, of Caius College, Cambridge, and fellow of the Royal Medical Society, Edinburgh, has now in the press, A Compendium of Modern Chemistry, in the Latin Language;—a small work intended as the *fidus Achates* of the medical as well as chemical philosopher. It treats not only of the principal subjects in chemistry, but unfolds the processes by which the different compounds of the London and Edinburgh pharmacopœiæ are prepared, and the theories upon which such operations are founded. The analyses also of animal and vegetable bodies are as fully given as the prescribed limits of a compendium will allow.—This work will also afford easy instructions for the disciple of the Stahlean school to become the proselyte of Lavoisier.—The language in which it is written, will, it is hoped, render it no inconsiderable assistant to those desirous of writing or speaking medical Latin. And if it be remembered that no such publication has appeared in the chemical department subsequent to Boerhaave, the hope may not be presumptuous.

CULTIVATION OF INDIGO IN FRANCE.

A *proces-verbal* of the municipality of Lille, in the department of Vaucluse, has lately stated the success of a plantation of indigo, executed on a large scale and in the the open air at an estate belonging to M. Icard de Bataglini, an agriculturalist. It is stated in the *Proces-verbal*, that after an attentive examination of the indigo produced, the commissioners were of opinion that this precious plant may be naturalized in the department, and become a principal source of its riches.

MISCELLANEOUS.

New Method of taking Stains out of Linen.—Lemon-juice has generally been employed for this purpose:—but a German journal devoted to subjects of rural œconomy, and edited by M. Schnee, gives another more œconomical, but not less certain, method, by means of aqua fortis. One or two drops are sufficient for taking out a large spot of ink without damaging the linen: it is only necessary previously to moisten

moisten the spot with water, and to rinse it afterwards in water also.

Remedies against the Epidemy in Cattle.—It has been long known that nettles form an excellent food for cattle, and that they increase the milk of cows. This plant is also an excellent remedy against epidemical diseases, which are frequently the effect of bad food in the stable. In Sweden the salutary effects of nettles are particularly conspicuous, where they are administered to the horned cattle regularly every spring in great quantities.

M. Scheidlin, botanist to the court of Wirtemberg, has proved by a multitude of experiments that *Angelica* (Angelica sativa hortensis, *Linn.*) is an excellent preservative against cattle epidemies. He bruized the roots of this plant, and gave a handful to each cow, morning, noon, and night, along with its ordinary food. Cows ate it greedily, and were not attacked with the disease. He also mixed it with the water they drank.

The leaves and roots of Angelica dried and reduced into powder, and strewed over bread, or other food, have the same virtues. This root grows spontaneously in shady and humid places, and it may be planted with good effect in case of need.

Mastic for resisting the Action of Fire and Water.—The following directions are given in the foreign journals, for preparing a composition of this description:—Take half a pint of milk, mix with it an equal quantity of vinegar, so as to coagulate the milk; separate the curds from the whey, mix the latter with the whites of four or five eggs, after having beat them well up. The mixture of these two substances being complete, add quick-lime to them which has been passed through a sieve; make the whole into a thick paste to be of the consistency of putty, when you use it.

If this mastic is carefully applied to broken bodies or to fissures of any kind, and dried properly afterwards, it resists water and fire. M. Skogo, a merchant of Carlskrona, closed a large crack in the bottom of a large iron cauldon, in which

which he frequently boils pitch, and this cauldron has now served him for five years in this state without requiring any further repairs.

NAUTICAL INVENTION.

A gentleman has lately invented a very simple and ingenious method by which a vessel without any person on board can be directed with very considerable accuracy in a given course. The application of this invention to fire-ships would be of vast utility: and that this may be judged of without (for obvious reasons) giving a public description of the means by which it is effected, the model used in the first experiments, which we understand were completely satisfactory, may be seen at Mr. Thomas Jones's, mathematical, optical, and philosophical instrument maker, No. 124, Mount-street, Berkley-square.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Dumbell, of Mersey Mills, in the parish of Warrington, and county palatine of Lancaster, miller; for his invented method or methods of obtaining, producing, using, and cultivating a moving power or force; and of communicating motion to engines, pumps, and machinery, and to mechanical operations in general; and of forming and using a rotary or circular motion, or a motion to and fro *ad libitum*; and a method or methods of working and applying the same to mills, machinery, ploughs, tools, and equipage in general, and almost to an endless variety of valuable purposes, and in particular to carriages; and also a method or methods of making less disastrous and less frequent the overturning of carriages, and of improving their structure, and of guiding and governing the same:—which carriages or mechanism, when embracing his appendages and conceits, he distinguishes by the name of Ess or George's Wain. February 4.

To Samuel Brown, a lieutenant in the royal navy, and now residing in Castle-court, Budge-row, Watling-street, in the city of London, for his inventions and improvements in the rigging of ships or vessels. February 4.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For March 1808.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 25	34°	37°	30°	30·69	26	Fair
26	30	36	39	·49	20	Fair
27	40	49	39	·38	31	Fair
28	38	48	45	·35	25	Fair
29	45	52	46	·15	10	Cloudy
March 1	47	52	48	·28	11	Cloudy
2	49	53	46	·31	24	Cloudy
3	47	49	41	·35	31	Cloudy
4	33	51	35	·44	15	Fair
5	35	43	34	·42	27	Fair
6	33	45	33	·42	30	Fair
7	32	41	33	·28	38	Fair
8	32	38	32	·28	21	Fair
9	32	43	33	·32	23	Fair
10	32	41	33	·34	19	Cloudy
11	32	42	35	·32	21	Cloudy
12	33	39	37	·30	15	Cloudy
13	38	42	35	·19	25	Cloudy
14	35	41	35	·10	19	Cloudy
15	34	44	36	29·99	26	Fair
16	37	41	32	·98	25	Fair
17	32	34	29	·99	15	Cloudy
18	32	34	29	·87	15	Cloudy
19	32	37	36	·65	14	Cloudy
20	40	47	40	·55	5	Cloudy
21	36	39	37	·75	8	Cloudy
22	36	41	30	·92	16	Fair
23	31	36	31	·92	27	Cloudy
24	33	36	29	·90	25	Cloudy
25	25	36	32	·92	30	Fair
26	32	42	35	·96	15	Fair

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

XLI. *Experiments on the Influence of Time, as a Chemical Agent, in depriving an Elastic Fluid of its Elasticity. In a Letter from M.* Biot *to M.* Berthollet *.

I had lately occasion to converse with that excellent observer M. de Marty, upon the subject of several experiments, with which he has been a long time occupied, and I have received his permission to make them public.

The experiments which I shall first mention, have for their object the influence of time upon the exercise of chemical actions, when they tend to deprive an elastic fluid of its elasticity:

1. Into a glass flask, closed with a ground stopper, M. de Marty introduced a certain quantity of oxygen gas and a certain quantity of rain water boiled or not boiled. There may be any proportions of water and gas. Supposing that there is but a small quantity of water; by shaking this mixture for some minutes the water will absorb a certain quantity of gas, as may be ascertained upon opening the flask into a tub full of water. But after having shaked it and opened it several times, the water contained in the flask will soon finish by being saturated, and will absorb no more.

Things being thus disposed, close up the flask and place it out of the reach of the sun; observe at the same time the barometer and thermometer: then after two or three days of rest, shake the flask again and open it under the water, and you will see the latter ascend a little: close the flask, return it to its place and renew the shaking from time to time; you will find that it has every time absorbed a new quantity of gas. This effect will appear so much the more striking, the longer time it has been since you last opened the flask, and in this case the water of the tub will rise much more than if you had opened it within a few days.

I have witnessed these effects at M. Marty's house. He uncorked under water, in my presence, a flask which had been closed upwards of a year and a half, and which contained

* From *Ann. de Chimie,* tom. lxi. p. 271.

tained oxygen gas with a small quantity of water. When the flask was opened in the tub, the water of the latter rose in a very remarkable manner, and the absorption appeared to me *to be equal to at least half the volume of the water* contained in the flask before we opened it; the barometer and thermometer were nearly at the same height as at the moment of introduction; the temperature of the water of the tub was also the same.

It seems therefore from this experiment, that the same mass of water which at first could only absorb a certain volume of oxygen gas, absorbed a considerable volume of it after some time; whence it should seem, that in the first case the air was but feebly combined, and in some measure interposed between the particles of the water. But the prolonged action of the liquid, diminishing more and more the elasticity of the gas, and contracting it as it were by degrees, made it enter further forward into the sphere of attraction of its particles, which has rendered the water susceptible of absorbing a new quantity of gas.

2. The same thing takes place relative to hydrogen gas, which I also witnessed at M. Marty's. The absorption, in this case, was equally strong, and M. Marty finds by his experiments that the volume absorbed is more considerable, and the absorption more speedy, with this gas than with oxygen gas. He found, also, that in two years the volume of gas absorbed was not equal to the volume of the water.

3. Water already charged with oxygen is more proper for absorbing hydrogen, and *vice versa:* this is analogous to what was remarked by Messrs. Humboldt and Gay Lussac. M. Marty's experiments, however, have the advantage of having been made in close vessels.

4. The absorption is the more considerable in proportion as the volume of water is greater.

5. These effects do not take place with azotic gas: after once shaking the water for some time with this gas, it does not absorb an atom more, however long time we leave it in contact.

6. If we put water charged with azot in contact with hydrogen or oxygen gas, it absorbs these gases without abandoning

doning the azot it contains. If we think there has been a change, it is, in fact, because a little azot escapes at the commencement of the absorption of the hydrogen or oxygen; but shake the water and the gases together, the whole azot which was previously interposed in the water will enter into it and interpose itself as formerly, independently of the hydrogen or oxygen gas, which it has beside absorbed.

7. The preceding result is so correct that we may exactly analyse the atmospheric air by the simple absorbent action of water. For this purpose it is sufficient that the water has been previously impregnated with azot; it then absorbs precisely 21 hundredths of the atmospheric air put in contact with it, as a sulphuret would do. M. Marty asserts that the water thus employed in great quantities, in order that the operation may be more expeditious, is an excellent eudiometer, and that he used it as such frequently. If we have no azot at hand, we may impregnate the water with this gas by shaking it with atmospheric air, and leaving it for some time in contact with this air: by this means it absorbs all the azot it ought to contain; and the oxygen it takes at the same time does not hinder it, according to the first experiment, from absorbing in time, that of the air we mean to analyse. M. Marty also uses this absorbent property of water to ascertain if a given oxygen gas does or does not contain azot; for if it does contain it, the water saturated with azot does not absorb it completely.

XLII. *Experiments for investigating the Cause of the coloured concentric Rings, discovered by Sir* Isaac Newton, *between two Object-glasses laid upon one another. By* William Herschel, *LLD. F.R.S.*

[Concluded from p. 128.]

XXV. *Of the Action of the second Surface.*

As rings are formed when two glasses are laid upon each other, it is but reasonable to expect that the two surfaces at least which are placed together should have an immediate effect

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upon them, and so much the more, as it has been ascertained that the first surface assists only by permitting light to pass into the body of the glass. Some of the experiments that have been instituted for examining the action of the first surface will equally serve for investigating that of the second.

The lens already used with a strong emery scratch being again placed on the mirror, but with the injured side downwards, I found that the rings, when brought under the scratch, were not distorted; they had only a black mark of the same shape as the scratch across them.

The lens with a scabrous side was also placed again upon the mirror, but with the highly polished side upwards. In this position the scabrousness of the lowest surface occasioned great irregularity among the rings, which were indented and broken wherever the little polished holes that make up a scabrous surface came near them; and if by gently lifting the lens a strong contact was prevented, the colours of the rings were likewise extremely disfigured and changed.

As we have now seen that a polished defect upon the second surface will affect the figure of the rings that are under them, it will remain to be determined whether such defects do really distort them by some modification they give to the rays of light in their passage through them, or whether they only represent the rings as deformed, because we see them through a distorted medium. For although the scabrousness did not sensibly affect the figure of the rings when it was on the first surface, we may suppose the little polished holes to have a much stronger effect in distorting the appearance of the rings when they are close to them. The following experiment will entirely clear up this point.

Over the middle of a 22-inch double convex lens I drew a strong line with a diamond, and gave it a polish afterwards that it might occasion an irregular refraction. This being prepared, I laid a slip of glass upon a plain metalline mirror, and placed the lens with the polished line downwards upon the slip of glass. This arrangement has been shown to give two sets of rings. When I examined the

primary

primary set, a strong disfiguring of the rings was visible; they had the appearance of having been forced asunder, or swelled out, so as to be much broader one way than another. The rings of the secondary set had exactly the same defects, which being strongly marked, could not be mistaken. The centres of the two sets, as usual, were of opposite colours, the first being black, the second white; and all those defects that were of one colour in the first set, were of the opposite colour in the second. When, by the usual method, I changed the colours of the centres of the rings, making that of the primary white and of the secondary black, the defects in both sets were still exactly alike, and as before; except that they had also undergone the like transformation of colour, each having assumed its opposite. It remains now only to show that this experiment is decisive; for by the established course of the rays we saw the secondary set of rings when it had a white centre by the transmitted rays marked 1, 2, 4, 5, in figure 13; and when it had a black one, by the reflected rays 6, 7, 2, 4, 5, of the same figure; but in neither of these two cases did the rays come through the defective part of the lens in their return to the eye.

This experiment proves more than we might at first be aware of; for it does not only establish that the second surface, when properly combined with a third surface, has a modifying power whereby it can interrupt the regularity of the rings, but also one whereby it contributes to their formation; for, if it can give an irregular figure to them by transmitting its irregularly modified rays, it follows, that when these rays are regularly modified it will be the cause of the regular figure of the rings. Nay, it proves more; for if it modifies the figure of the rings by transmission, it modifies them no less by reflection; which may be seen by following the course of the rays 6, 7, 2, 4, 5; for as they do not pass through the defective place of the lens, they can only receive their modification from it by reflection. This opens a field of view to us that leads to the cause of all these intricate phænomena, of which in a second part of this paper I shall avail myself.

XXVI. *Of the Action of the third Surface.*

When a double convex lens is laid upon a plain metalline mirror that happens to have an emery scratch in its surface, we see it as a black line under the rings that are formed over them. This shows, that when a defect from want of polish has not a power to reflect light in an irregular manner, it cannot distort the rings that are formed upon it.

When I laid a good 21-feet object glass upon a plain slip that had some defects in its surface, the rings, in every part of the object glass that was brought over them, were always disfigured; which proves that a reflection from a defective third surface has a power of forming distorted rings, and that consequently a reflection from one that is perfect must have a power of forming rings without distortion, when it is combined with a proper second surface.

When the defective slip of glass, with a perfect lens upon it, was placed upon a metalline mirror, I saw the secondary set affected by distortions of the rings that were perfectly like those in the primary set; which proves that a polished defect in the third surface will give modifications to the rays that form the rings by transmission as well as by reflection.

XXVII. *The Colour of the reflecting and transmitting Surfaces is of no consequence.*

I laid seven 54-inch double convex lenses upon seven coloured pieces of plain glass. The colours of the glasses were those which are given by a prism, namely, violet, indigo, blue, green, yellow, orange, and red. The rings reflected from each of these glasses were in every respect alike; at least so far that I could have a black, a white, a red, an orange, a yellow, a green, or a blue centre with every one of them, according to the degree of pressure I used. The lenses being very transparent, it may be admitted that the colours of the glasses seen through them would in some degree mix with the colours of the rings; but the action of the cause that gives the rings was not in the least affected by that circumstance.

I saw the rings also by direct transmission through all the coloured glasses except a dark red, which stopped so much

much light that I could not perceive them. The colour of the glasses, in this way, coming directly to the eye, gave a strong tinge to the centres of the rings, so that instead of a pure white I had a blueish white, a greenish white, and so of the rest; but the form of the rings was no less perfect on that account.

XXVIII. *Of the Action of the fourth Surface.*

We have already seen that a set of rings may be completely formed by reflection from a third surface, without the introduction of a fourth: this, at all events, must prove that such a surface is not essential to the formation of rings; but as not only in direct transmission, but also when two sets of rings are to be seen, one of which may be formed by transmission, this fourth surface must be introduced, I have ascertained by the following experiments how far the same has any share in the formation of rings.

In direct transmission, where the light comes from below, the fourth surface will take the part which is acted by the first, when rings are seen reflected from a metalline mirror. Its office therefore will be merely to afford an entrance to the rays of light into the substance of the subjacent glass; but when that light is admitted through the first, second, and third surfaces, the fourth takes the office of a reflector, and sends it back towards the point of contact. It will not be required to examine this reflection, since the light thus turned back again is, with respect to the point of contact, in the same situation in which it was after its entrance through the first surface when it proceeded to the same point; but when two sets of rings are to be formed by rays, either coming through this point directly towards the fourth surface, or by reflection from the same point towards the place where the secondary rings are to be seen, it will then be necessary to examine whether this surface has any share in their formation, or whether these rings, being already completely formed, are only reflected by it to the eye. With a view to this, I selected a certain polished defect in the surface of a piece of coach-glass, and when

a 26-inch

a 26-inch lens was laid upon it, the rings of the set it produced were much distorted. The lens was then put upon a perfect slip of glass, and both together were laid upon the defective place of the coach-glass. The rings of the secondary set reflected by it were nevertheless as perfect as those of the primary set. It occurred to me that these rings might possibly be reflected from the lowest surface of the perfect slip of glass, especially as by lifting it up from the coach-glass I still continued to see both sets. To clear up this point, therefore, I took away the slip, and turning the defective place of the coach-glass downwards, produced a set of perfect rings between the lens and the upper surface of the coach-glass, and brought it into such a situation that a secondary set must be reflected from the defective place of the lowest surface. This being obtained, the rings of this set were again as well formed and as free from distortions as those of the primary set.

Upon a plain metalline mirror I laid down two lenses, one a plano-convex, the other a plano-concave, both of 2·9 inches focus, and having the plain side upwards. When two 21-inch double convex glasses were laid upon them, the secondary sets of both the combinations were of equal size, and perfectly like their primary sets; which proves that the refraction of the fourth surface is either not at all concerned, or at least has so little an effect in altering the size of the rings that it cannot be perceived.

The result of the foregoing experiments, relating to the action of the several surfaces, is,

I. That only two of them are essential to the formation of concentric rings.

II. That these two must be of a certain regular construction, and so as to form a central contact.

III. That the rays from one side or the other, must either pass through the point of contact, or through one of the surfaces about the same point to the other to be reflected from it.

IV. And that in all these cases a set of rings will be formed, having their common centre in the place where the two surfaces touch each other.

XXIX. *Con-*

XXIX. *Considerations that relate to the Cause of the Formation of concentric Rings.*

It is perfectly evident that the phænomena of concentric rings must have an adequate cause, either in the very nature or motion of the rays of light, or in the modifications that are given to them by the two essential surfaces that act upon them at the time of the formation of the rings.

This seems to reduce the cause we are looking for to an alternative that may be determined; for if it can be shown that a disposition of the rays of light to be alternately reflected and transmitted cannot account for the phænomena which this hypothesis is to explain, a proposition of accounting for them by modifications that may be proved, even on the very principles of Sir I. Newton to have an existence, will find a ready admittance. I propose, therefore, now to give some arguments, which will remove an obstacle to the investigation of the real cause of the formation of the concentric rings; for after the very plausible supposition of the alternate fits, which agrees so wonderfully well with a number of facts that have been related, it will hardly be attempted, if these should be set aside, to ascribe some other inherent property to the rays of light, whereby we might account for them; and thus we shall be at liberty to turn our thoughts to a cause that may be found in the modifications arising from the action of the surfaces which have been proved to be the only essential ones in the formation of rings.

XXX. *Concentric Rings cannot be formed by an alternate Reflection and Transmission of the Rays of Light.*

One of the most simple methods of obtaining a set of concentric rings is to lay a convex lens on a plain metalline mirror; but in this case we can have no transmission of rays, and therefore we cannot have an alternate reflection and transmission of them. If to get over this objection it should be said that, instead of transmission, we ought to substitute absorption; since those rays which in glass would

have

have been transmitted will be absorbed by the metal, we may admit the elusion; it ought however to have been made a part of the hypothesis.

XXXI. *Alternate Fits of easy Reflection and easy Transmission, if they exist, do not exert themselves according to various Thicknesses of thin Plates of Air.*

In the following experiment, I placed a plain well polished piece of glass 5·6 inches long, and 2·3 thick, upon a plain metalline mirror of the same length with the glass; and in order to keep the mirror and glass at a distance from each other, I laid between them, at one end, a narrow strip of such paper as we commonly put between prints. The thickness of that which I used was the 640th part of an inch; for 128 folds of them laid together would hardly make up two-tenths. Upon the glass I put a 39-inch double convex lens; and having exposed this combination to a proper light, I saw two complete sets of coloured rings.

In this arrangement, the rays which convey the secondary set of rings to the eye must pass through a thin wedge of air; and if these rays are endowed with permanent fits of easy reflection, and easy transmission, or absorption, their exertion, according to Sir I. Newton, should be repeated at every different thickness of the plate of air, which amounts to the $\frac{1}{88952}$ part of an inch, of which he says "Hæc est crassitudo aeris in primo annulo obscuro radiis ad perpendiculum incidentibus exhibito, qua parte is annulus obscurissimus est." The length of the thin wedge of air, reckoned from the line of contact, to the beginning of the interposed strip of paper, is 5·2 inches, from which we calculate that it will have the above-mentioned thickness at $\frac{1}{27}$ of an inch from the contact; and therefore at $\frac{1}{54}$, $\frac{3}{54}$, $\frac{5}{54}$, $\frac{7}{54}$, $\frac{9}{54}$, $\frac{11}{54}$, &c. we shall have the thickness of air between the mirror and glass, equal to $\frac{1}{178000}$, $\frac{3}{178000}$, $\frac{5}{178000}$, $\frac{7}{178000}$, &c. of which the same author says that they give "crassitudines aeris in omnibus annulis lucidis, qua parte illi lucidissimi sunt." Hence it follows that, according to the above hypothesis, the rings of the secondary set which extended over a space of ·14 of an inch, should suffer more than seven interruptions of shape and colour in the direction of the wedge of air.

In order to ascertain whether such an effect had any existence, I viewed the secondary set of rings upon every part of the glass-plate, by moving the convex lens from one end of it gradually to the other; and my attention being particularly directed to the 3d, 4th, and 5th rings, which were extremely distinct, I saw them retain their shape and colour all the time without the smallest alteration.

The same experiment was repeated with a piece of plain glass instead of the metalline mirror, in order to give room for the fits of easy transmission, if they existed, to exert themselves; but the result was still the same; and the constancy of the brightness and colours of the rings of the secondary set, plainly proved that the rays of light were not affected by the thickness of the plate of air through which they passed.

XXXII. *Alternate Fits of easy Reflection and easy Transmission, if they exist, do not exert themselves according to various Thicknesses of thin Plates of Glass.*

I selected a well polished plate of coach-glass 17 inches long, and about 9 broad. Its thickness at one end was 33, and at the other 31 two-hundredths of an inch; so that in its whole length it differed $\frac{1}{100}$ of an inch in thickness. By measuring many other parts of the plate I found that it was very regularly tapering from one end to the other. This plate, with a double convex lens of 55 inches laid upon it, being placed upon a small metalline mirror, and properly exposed to the light, gave me the usual two sets of rings. In the secondary set, which was the object of my attention, I counted twelve rings, and estimated the central space between them to be about $1\frac{1}{3}$ times as broad as the space taken up by the 12 rings on either side; the whole of the space taken up may therefore be reckoned equal to the breadth of 40 rings of a mean size: for the 12 rings, as usual, were gradually contracted in breadth as they receded from the centre; and, by a measure of the whole space thus taken up, I

found

found that the breadth of a ring of a mean size was about the 308th part of an inch.

Now, according to Sir I. Newton's calculation of the action of the fits of easy reflection and easy transmission in thick glass plates, an alternation from a reflecting to a transmitting fit requires a difference of $\frac{1}{137545}$ part of an inch in thickness *; and by calculation this difference took place in the glass plate that was used at every 80th part of an inch of its whole length: the 12 rings, as well as the central colour of the secondary set, should consequently have been broken by the exertion of the fits at every 80th part of an inch; and from the space over which these rings extended, which was about ·13 inch, we find that there must have been more than ten such interruptions or breaks in a set of which the 308th part was plainly to be distinguished. But when I drew the glass plate gently over the small mirror, keeping the secondary set of rings in view, I found their shape and colour always completely well formed.

This experiment was also repeated with a small plain glass instead of the metalline mirror put under the large plate. In this manner it still gave the same result, with no other difference but that only six rings could be distinctly seen in the secondary set, on account of the inferior reflection of the subjacent glass.

XXXIII. *Coloured Rings may be completely formed without the Assistance of any thin or thick Plates, either of Glass or of Air.*

The experiment I am now to relate was at first intended to be reserved for the second part of this paper, because it properly belongs to the subject of the flection of the rays of light, which is not at present under consideration; but as it particularly opposes the admission of alternate fits of easy reflection and easy transmission of these rays in their passage through plates of air or glass, by proving that their assistance in the formation of rings is not required, and also throws light upon a subject that has at different times been

* Newton's Optics, p. 277.

considered

considered by some of our most acute experimentalists, I have used it at present, though only in one of the various arrangements, in which I shall have occasion to recur to it hereafter.

Sir I. Newton placed a concave glass mirror at double its focal length from a chart, and observed that the reflection of a beam of light admitted into a dark room, when thrown upon this mirror, gave "four or five concentric irises or rings of colours like rainbows *." He accounts for them by alternate fits of easy reflection and easy transmission exerted in their passage through the glass-plate of the concave mirror †.

The duke de Chaulnes concluded from his own experiments of the same phænomena, "that these coloured rings depended upon the first surface of the mirror, and that the second surface, or that which reflects them after they had passed the first, only served to collect them and throw them upon the pasteboard, in a quantity sufficient to make them visible‡."

Mr. Brougham, after having considered what the two authors I have mentioned had done, says, "that upon the whole there appears every reason to believe that the rings are formed by the first surface out of the light which, after reflection from the second surface, is scattered, and passes on to the chart §."

My own experiment is as follows. I placed a highly polished 7-feet mirror, but of metal instead of glass, that I might not have two surfaces, at the distance of 14 feet from a white screen, and through a hole in the middle of it one-tenth of an inch in diameter I admitted a beam of the sun into my dark room, directed so as to fall perpendicularly on the mirror. In this arrangement the whole screen remained perfectly free from light, because the focus of all the rays which came to the mirror was by reflection thrown back into the hole through which they entered. When all was duly prepared, I made an assistant strew some hair-powder

* Newton's Optics, p. 265. † Ibid. p. 277.

‡ Priestley's History, &c. on the Colours of thin Plates, p. 515.

§ Philosophical Transactions for 1796, p. 216.

with

with a puff into the beam of light, while I kept my attention fixed upon the screen. As soon as the hair-powder reached the beam of light the screen was suddenly covered with the most beautiful arrangement of concentric circles displaying all the brilliant colours of the rainbow. A great variety in the size of the rings was obtained by making the assistant strew the powder into the beam at a greater distance from the mirror; for the rings contract by an increase of the distance and dilate on a nearer approach of the powder.

This experiment is so simple, and points out the general causes of the rings which are here produced in so plain a manner, that we may confidently say they arise from the flection of the rays of light on the particles of the floating powder, modified by the curvature of the reflecting surface of the mirror.

Here we have no interposed plate of glass of a given thickness between one surface and another, that might produce the colours by reflecting some rays of light and transmitting others; and if we were inclined to look upon the distance of the particles of the floating powder from the mirror as plates of air, it would not be possible to assign any certain thickness to them, since these particles may be spread in the beam of light over a considerable space, and perhaps none of them will be exactly at the same distance from the mirror.

I shall not enter into a further analysis of this experiment, as the only purpose for which it is given in this place is to show that the principle of thin or thick plates, either of air or glass, on which the rays might alternately exert their fits of easy reflection and easy transmission, must be given up, and that the fits themselves of course cannot be shown to have any exisistence.

XXXIV. *Conclusion.*

It will hardly be necessary to say, that all the theory relating to the size of the parts of natural bodies and their interstices, which Sir I. Newton has founded upon the existence of fits of easy reflection and easy transmission, exerted differently, according to the different thickness of the thin plates of which he supposes the parts of natural bodies to consist,

consist, will remain unsupported; for if the above-mentioned fits have no existence, the whole foundation on which the theory of the size of such parts is placed, will be taken away, and we shall consequently have to look out for a more firm basis on which a similar edifice may be placed. That there is such a one we cannot doubt, and what I have already said will lead us to look for it in the modifying power which the two surfaces, that have been proved to be essential to the formation of rings, exert upon the rays of light. The second part of this paper, therefore, will enter into an examination of the various modifications that light receives in its approach to, entrance into, or passage by, differently disposed surfaces or bodies; in order to discover, if possible, which of them may be the immediate cause of the coloured rings that are formed between glasses.

XLIII. *Essay upon Machines in General. By M.* Carnot, *Member of the French Institute, &c. &c.*

[Continued from p. 158.]

X. The science of machines in general is therefore reduced to the following question:

"*Being acquainted with the virtual movement of any system of bodies (that is to say, that movement which each of these bodies would take if it were free), find the real movement which will take place the instant following, on account of the reciprocal action of bodies, by considering them such as they exist in nature,* i. e. *as endowed with all the inertness common to all the particles of matter.*"

XI. Now, as this question evidently contains the whole of mechanics, we must, in order to proceed with precision, go back to the first laws which nature observes in the communication of movements. We may reduce them in general to two, which are the following:

FUNDAMENTAL LAWS OF EQUILIBRIUM, AND MOTION.

First Law.—*Action and Reaction are always equal and contrary.*

This

This law consists in this, that every body which changes its state of repose or uniform and rectilinear motion, never does so except by the influence or action of some other body, upon which it impresses, at the same time, a quantity of motion equal and directly opposite to that which it receives from it; that is to say, that the velocity it assumes the instant afterwards is the force resulting from that which this other body impresses upon it, and from that which it would have had without this last force. Every body therefore resists its change of state; and this resistance, which is called *vis inertiæ*, is always equal and directly opposite to the quantity of motion it receives, *i. e.* to the quantity of motion which combined with that which it had immediately before the change, produces, as the result, the quantity of motion which it should really have immediately afterwards. This is also expressed by saying, that in the reciprocal action of bodies, the quantity of motion lost by the one is always gained by the others, in the same time and in the same ratio.

SECOND LAW.—*When two hard bodies act upon each other, by shock or pressure,* i. e. *in virtue of their impenetrability, their relative velocity, immediately after the reciprocal action, is always null.*

In fact, we constantly observe, that if two hard bodies give a shock to each other, their velocities, immediately after the shock, estimated perpendicularly to their common surface at the point of contact, are equal, in the same way as if they were drawn by inextensible wires, or pushed by incompressible rods; their velocities, estimated in the ratio of this wire or rod, would necessarily be equal: whence it follows that their relative velocity, *i. e.* that by which they approach or recede from each other, is in every case null at the first instant.

From these two principles it is easy to draw the laws of the shock of hard bodies, and consequently to conclude the two other secondary principles, the use of which is continual in mechanics, viz.

1. That the intensity of the shock, or of the action which is

is exercised between two bodies which meet, does not depend upon their absolute *movements, but solely upon their relative movements.* 2. *That the force or quantity of movement which they exercise upon each other, by the shock, is always directed perpendicularly to their common surface at the point of contact.*

XII. Of the two fundamental laws, the *first* generally agrees with all the bodies of nature, as well as the two secondary laws which we have seen; and the *second* solely regards hard bodies; but as those which are not hard have different degrees of elasticity, we generally refer the laws of their movement to those of the hard bodies, which we take for a term of comparison, *i. e.* we regard the elastic bodies as composed of an infinity of hard corpuscles separated by small compressible rods, to which we attribute all the elastic virtue of these bodies; so that, properly speaking, we do not consider in nature any other than bodies endowed with different moving forces. We shall follow this method as the simplest: we shall therefore reduce the question to the investigation of the laws observed by hard bodies, and shall afterwards make some applications of them to cases in which bodies are endowed with different degrees of elasticity.

XIII. This essay upon machines not being a treatise upon mechanics, my object is not to explain in detail, nor to prove the fundamental laws I have related; these are truths which all the world knows, as to which they are generally agreed, and which are most strongly manifested in all the phænomena of nature. This is sufficient for my object, which is merely to draw from these laws a simple and exact method for finding the state of rest or of movement which results from them in any given system of bodies, *i. e.* to present the same laws under a form which may facilitate their application to each particular case.

XIV. Let us suppose therefore any system of hard bodies, the virtual given movement of which is changed by their reciprocal action into another which we wish to find; and in order to embrace the question in all its extent, let us suppose that the movement may either change suddenly, or vary by insensible degrees: finally, as fixed points or some obstacles may be met with, let us consider them as they really are in fact, that is to say, as ordinary bodies of themselves, making part of the system proposed, but firmly arrested in the spot where they are placed.

XV. In order to attain the solution of this problem, let us first observe, that, all the parts of the system being supposed perfectly hard, *i. e.* incompressible and inextensible, we may visibly, whatever it may be, regard it as composed of an infinity of hard corpuscles, separated from each other either by small incompressible rods, or by small inextensible wires; for when two bodies strike, push, or tend in general to approach each other without being able to do it, on account of their impenetrability, we can conceive between the two a small incompressible rod, and suppose that the movement is transmitted from the one to the other according to this rod: and in the same way, if two bodies tend to separate, we may conceive that the one is attached to the other by a small inextensible wire, according to which the movement is propagated: this being done, let us consider successively the action of each of these small corpuscles upon all those which are adjacent to it, *i. e.* let us examine two by two all these small corpuscles separated from each other by a small incompressible rod, or by a small inextensible wire, and we shall see what ought to result in the general system of all these corpuscles. Let us name for this purpose,

m' and m'' The masses of the adjacent corpuscles.

V' and V'' The velocities they ought to have the following instant.

F' The action of m'' upon m', that is to say, the force or quantity of movement which the first of these corpuscles impresses upon the other.

F'' The reaction of m' upon m''.

q' and q'' The angles formed by the directions of V' and F' and by those of V'' and F''.

This being done, the real velocity of m' being V', this velocity estimated in the direction of F' will be V' cosine q'; in the same manner the velocity of m'' estimated in the direction of F'' will be V'' cosine q''. Therefore, since by the

the second fundamental law bodies should go in company, we shall have V′ cosine $q' + V''$ cosine $q'' = 0$ (A): thus by the first fundamental law, we shall also have F′ V′ cosine q' + F″ V″ cosine $q'' = 0$ (B): for if m' and m'' are both moveable, it is clear, by this law, that we have $F'' = F''$; therefore on account of the equation (A) we shall also have the equation (B); and if one of the two, m' for instance, be fixed, or form part of an obstacle, we shall have V′ cosine $q' = 0$; therefore on account of the equation (A) we shall also have V″ cosine $q'' = 0$; therefore the equation (B) will still take place: therefore this equation (B) is true for all the corpuscles of the system taken two by two. Imagining therefore a similar equation for all these bodies taken in fact two by two, and adding together all these equations, or, what comes to the same thing, the integral equation (B), we shall have for the whole system,

s F′ V′ cosine q' + *s* F″ V″ cosine $q'' = 0$: that is to say, the sum of the products of the quantities of movement which are reciprocally impressed by the corpuscles separated by each of the small inextensible wires or incompressible rods; from these quantities, I say, each of them multiplied by the velocity of the corpuscle on which it is impressed, estimated in the direction of this force, is equal to zero.

This being done, abandoning the preceding denominations, let us name

The mass of each of the corpuscles of the system - *m*

Its virtual velocity, *i. e.* that which it would assume if it were free, - - - - - - - W

Its real velocity - - - - - - V

The velocity which it loses in such a manner that W is the result of V and of this velocity - - U

The force or quantity of movement which each of the adjacent corpuscles impresses upon *m*, and by the intermedium of which it evidently receives all the movement that is transmitted to it from the different parts of the system, - - - - - - - - F

The angle comprehended between the directions of W and V - - - - - - - - X

The angle comprehended between the directions of W and U - - - - - - - Y

The angle comprehended between the directions of V and U - - - - - - - - Z

The angle comprehended between the directions of V and F - - - - - - - - - q

We shall therefore have for the whole system s F V cosine $q = 0$, or s V F cosine $q = 0$ (C): at present we must observe that, the velocity of m before the reciprocal action being W, this velocity estimated in the direction of V will be W cosine X: therefore V — W cosine X is the velocity gained by m in the direction of V: therefore m (V — W cosine X) is the sum of the forces F which act upon m, estimated each in the direction of V: therefore m and V (V — W cosine X) is the same sum multiplied by V. Now to each molecule a similar sum answers; and further, the sum total of all these particular sums is visibly for the whole system s V F cosine q; therefore $s\ m$ V (V — W cosine X) = s F V cosine q: adding to this equation the equation (C), there comes $s\ m$ V (V — W cosine X) = 0 (D); but W resulting from V and U, it is clear that we shall have W cosine X = V + U cosine Z: substituting therefore this value of W cosine X in the equation (D), it will be reduced to $s\ m$ V U cosine Z = 0 (E); *first fundamental equation.*

XVI. Let us imagine that at the moment when the shock is about to be given, the actual movement of the system is at once destroyed, and that we make it take instead of it successively two other arbitrary movements, but equal and directly opposite to each other, *i. e.* let us make it set out successively from its actual position, with two movements, such that, in virtue of the second, each point of the system has at the first instant a velocity equal and directly opposed to that which it would have had in virtue of the first of these movements: this being done, it is clear, 1st, That the figure of the system being given, this may be done in an infinity of different ways, and by operations purely geometrical; this is the reason why I shall call these movements *geometrical movements*; i. e. that *if a system of bodies sets out from a given position with an arbitrary movement, but yet of such a nature that it is possible to make it take another in every respect equal and directly opposite, each of these movements*

movements will be named a geometrical movement *. 2dly, I say that in virtue of this geometrical movement, the adjacent corpuscles, which may be regarded as being pushed by a rod, or drawn by a wire, will not approach nor recede from each other at the first instant, *i. e.* at the first instant of this geometrical movement the relative velocity of these adjacent corpuscles will be nothing: in fact, it is clear, in the first place, that if *m* be separated from an adjacent corpuscle by an incompressible rod, it will not be able to approach it; and that if it be separated from it by an inextensible wire, it will not be able to recede from it: secondly, I say that if it be separated from it by an incompressible

* In order to distinguish by a very simple example those movements called *geometrical* from those which are not so, let us imagine two globes which push each other, but in other respects free and disengaged from every obstacle: let us impress upon these globes equal velocities, and moved in the same direction according to the line of the centres;—this movement is *geometrical*, because the bodies could even be moved in a contrary direction with the same velocity, as is evident: but let us now suppose that we impress upon these bodies movements equal, and directed in the line of the centres, but which, in place of being, as formerly, moved in the same direction, tend on the contrary to recede from each other; these movements, although possible, are not what I mean by *geometrical movements*; because if we wished to make each of these moveable powers to assume a velocity equal and contrary to that which it receives in this first movement, we should be hindered from doing so by the impenetrability of bodies.

In the same way if two bodies are attached to the extremities of an inextensible wire, and if we make the system assume an arbitrary movement, but so as that the distance of the two bodies may be constantly equal to the length of the wire, this movement will be *geometrical*, because the bodies may assume a similar movement in quite a contrary direction; but if these moveable bodies approach to each other, the movement is not *geometrical*, because they could not take a movement equal and contrary without receding from each other; which is impossible on account of the inextensibility of the wire.

In general it is evident, that whatever be the figure of the system and the number of bodies, if we can make it assume a movement so as there should result no change in the respective position of the bodies, this movement will be geometrical; but it does not follow from this that there is no other method of satisfying this condition, as we shall show from several examples.

Let us imagine an axle, to the wheel and cylinder of which are attached weights suspended by cords: if we turn the machine in such a manner that the weight attached to the wheel should descend from a height equal to its circumference, while that of the cylinder will ascend from a height equal to its circumference, this movement will be *geometrical*, because it is equally possible compressible rod, it cannot recede from it any more; for, if it receded, it is clear that in virtue of the equal and directly opposite movement, which is also possible by hypothesis, it would approach it; which could not be on account of the incompressibility of the rod: for the same reason finally it is obvious, that if it be a wire which separates *m* from the adjacent corpuscle, it will not approach, because then it would be possible to remove it by an equal and directly opposite movement: now this cannot be, on account of the inextensibility of the wire: therefore, whatever may be the geometrical movement impressed upon the

possible to make the weight attached to the cylinder descend from a height equal to its circumference, while the weight attached to the wheel would mount from an equal height to its circumference; but if while we cause the weight attached to the wheel to descend from a height equal to its circumference, we should cause the weight attached to the cylinder to ascend from a height greater than its circumference, the movement would not be *geometrical*, because the equal and contrary movement would be visibly impossible.

If several bodies be attached to the extremities of different wires united by the other extremities to one and the same knot, and if we make the system assume such a movement as that each of the bodies remains constantly removed from the knot of one and the same quantity at the length of the wire to which it is attached, this movement will be *geometrical*, even when the different bodies approach to each other; but if some of them approach the knot, the movement would not be geometrical, because, the wires being supposed to be inextensible, the equal and contrary movement would be visibly impossible.

If two bodies are attached to the extremities of a wire into which is introduced a moveable particle, it will be sufficient, in order that the movement be geometrical, that the sum of the distances from the moveable particle to each of the two other bodies is constantly equal to the length of the wire; so that if these two bodies are fixed, the moveable particle will not depart from an elliptical curve.

If a body be moved by a curved surface, for instance, in the concavity of a spherical shell, the movement will be geometrical, while the body will move in a tangent form to the surface; but if it be separated the movement will cease to be geometrical, because the equal and contrary movement is visibly impossible.

From all this it is evident, that although on giving to a system a *geometrical* movement, the different bodies of this system may be brought near to each other, yet we may say that the adjacent corpuscles, considered two by two, do not tend at the first instant either to approach or recede, as I shall prove at length in the text. Bodies therefore exercise no action upon each other in virtue of a similar movement: these movements are therefore absolutely independent of the rules of dynamics, and it is for this reason that I have called them *geometrical*.

system,

system, the relative velocity of all these adjacent corpuscles which act upon each other, taken two by two, will be nothing at the first instant. This being granted, let us call u the absolute velocity which m will have in the first instant, in virtue of this geometrical movement, and z the angle comprehended between the directions of u and U; it is clear that the corpuscles m will not tend to approach or recede from each other in virtue of the velocities u, if we suppose them animated at the same time with these velocities u and velocities U; nor will they tend more to approach or recede if animated with the mere velocities U: therefore the reciprocal action exercised among the different parts of the system will be the same, whether each molecule be animated with the single velocity U, or with the two velocities u and U: but if each molecule was animated with the single velocity U, it is plain that there would be equilibrium: thus, if it was animated at once with the two velocities U and u, or with a single velocity the result of both, U will still be the velocity lost by m; and u will be the real velocity after the reciprocal action: thus, by the same reasoning by which we had the fundamental equation (E) we shall also have $s\, m\, u\, \mathrm{U}$ cosine $z = 0$ (F); *second fundamental equation.*

It is very easy at present to resolve the problem which we propose for the preceding equation necessarily taking place, whatever be the value of u and its direction, provided the movement to which it refers be geometrical: it is clear that by successively attributing to that indeterminate different values and arbitrary directions, we shall obtain all the necessary equations among the unknown quantities, upon which depends the solution of the problem and of quantities either given or taken at pleasure.

XVII. In order to place this solution in the clearest light, it will be sufficient to give an example of it.

Let us suppose therefore that the whole system is reduced to an assemblage of bodies united to each other by inflexible rods, in such a manner that all the parts of the system should be forced always to preserve their same respective

 positions;

positions; but that there is no fixed point or any obstacle; the equation (F) gives us the solution of this problem on attributing successively to u different values and directions.

1st. As the velocities u are not subjected to any condition, unless the movement of the system in virtue of which the corpuscles m have these velocities be geometrical, it is evident that we can at first suppose all of them equal and parallel to one given line: then u being constant, or the same with respect to all the points of the system, the equation (F) will be reduced to $s\ m$ U cosine $z = 0$; which informs us that the sum of the forces lost by the reciprocal action of the bodies in the arbitrary sense of u is null, and that consequently that which remains is the same as if each body had been free; *this is a well-known principle.*

2dly. Let us now imagine that we make the whole system turn round a given axis, so that each of the points will describe a circumference round this axis, and in a plane which shall be perpendicular to it; this movement is visibly geometrical; therefore the equation (F) takes place: but then on calling R the distance from m to the axis, it is clear that we have $u = AR'$, A being the same for all the points; therefore the equation (F) is reduced to $s\ m$ R U cosine $z = 0$; that is to say, that the sum of the *momenta* of the forces lost by the reciprocal action relatively to any axis is null; *this is another well-known principle.*

3dly. We might also attribute to u other values; but this would be useless, and might lead to equations already contained in the preceding; for we know that the latter are sufficient for resolving the question, or at least for reducing it to a matter of pure geometry.

First Remark.

XVIII. The object we propose by giving a geometrical movement is to change the state of the system, without altering however the reciprocal action of the bodies which compose it, in order thereby to procure relations between these exercised and unknown forces and the arbitrary velocities which bodies assume in virtue of these different geometrical

metrical movements: but it must be remarked that there is a case where geometrical movements are not the only ones which can answer the same purpose, and where some other movements may be employed in the same way, in order to extract from the general equation (F) determinate equations: this happens when these other movements, without being absolutely geometrical, become so, nevertheless, merely on suppressing some of the small wires or rods we have supposed to be interposed between the adjacent particles of the system, at the time, I say, when these rods or wires supposed to transmit the movement from one corpuscle to another, transmitted none at all in fact; *i. e.* when the tension of some of these wires, or the pressure of some of these rods, is equal to zero; for then by suppressing these wires or rods, the tensions or pressures of which are null, we evidently change nothing at all of the reciprocal action of the bodies, and nevertheless it is possible that we may thereby render the system susceptible of some geometrical movements, which could not otherwise take place: there is nothing therefore to prevent us from regarding these rods and wires as annihilated, since they have no influence upon the state of the system; and as we consequently employ as geometrical the movements which, without being so effectively, become so nevertheless by this suppression.

Further, when two bodies are contiguous to each other, it is evidently the same thing to suppress the small rod which we have imagined to be interposed between two, to hinder them from approaching, or to suppose that these bodies are permeable to each other, *i. e.* that they may be penetrated as easily as the empty space is penetrated by all bodies; whence it evidently follows, that in general, in any system of bodies acting upon each other, immediately or by wires and rods, *i. e.* by the intermedium of any machine, if there be any wire, rod, or other part of the machine which exercises no action upon bodies applied to it, *i. e.* which may be annihilated without any change resulting in the reciprocal action of these bodies, we shall be able to treat as geometrical all the movements which, without being so effectively, would become so by this suppression,

in

in the same way as those which would become so also, regarding as freely permeable to each other, those of the bodies among which no pressure is exercised, although they are adjacent. The following, however, shows the utility of this observation:

If, when we undertake the solution of any problem, we know beforehand that a certain part of the machine does not exercise any action upon the other parts of the system, we shall be able to suppose that this part of the machine is totally annihilated, and ascertain the movement of the system according to this hypothesis, *i. e.* by treating as geometrical all the movements which would really become so by this supposition; and in the same way, if one of the given conditions of the problem is, that certain adjacent bodies do not exercise any pressure upon each other, we shall express this condition by regarding these two bodies as permeable to each other, *i. e.* by treating as geometrical the movements which would in fact become so by this supposition.

But if it happens that we are ignorant whether this pressure be real or null, we must ascertain the movement of the system, by first supposing the one or the other at pleasure: we shall suppose therefore, for example, that this pressure is real: then, if on inquiring, according to this hypothesis, the value of this pressure, we find it real and positive, we shall conclude that the hypothesis is legitimate, and the exact result; or else we shall be assured that the pressure in question is null, and that we may consequently treat as geometrical, motions which would become so in fact, if the two bodies in question were freely permeable to each other.

Further, if there was a machine in the system, a wire for example, and that we were ignorant if the tension of this wire is null or real, we might make the calculation by at first supposing that there really is tension; then, if we find for the value of this tension a real and positive quantity, we shall conclude that the supposition is legitimate, and that the result is exact; or else, we must recommence the calculation, setting out from the contrary supposition, *i. e.* supposing that the tension of the wire is equal to zero; which

which will be done by supposing the wire annihilated, *i. e.* by treating as geometrical the motions which would be so effectively if the wire in question did not exist.

From this it follows, that in order to extract in each particular case from the general equation (F) all the determinate equations which it can give, we must first make the system assume all the geometrical movements of which it is susceptible; secondly, to treat also as such all those which would become so by suppressing some machine or part of a machine, the action of which upon the rest of the system is null, or by regarding as permeable to each other, the bodies among which, although adjacent, no pressure is exercised. 3dly. In the last place, if we are in doubt whether a certain wire, rod, or any part of the machine has or has not a real action upon the other parts of the system, or that there was a real pressure between two adjacent bodies, we must first clear up this doubt, by supposing the thing in question as we have above explained it, and by treating as geometrical the movements which these suppositions shall have discovered as being capable of being taken for such.

According to this remark, it seems proper therefore to extend the name of geometrical. to all the movements, which, without being so effectively, become so on suppressing some machine or part of a machine which has no influence upon the state of the system, and on regarding also as perfectly permeable to each other, bodies in contact, without any pressure being exercised among them, *i. e.* without there being any thing except a simple juxtaposition: thus we shall presently comprehend all these movements, under the title of geometrical movements, since in fact they are equally well determined by operations purely geometrical, and are employed in the same way for extracting from the general equation (F) determinate equations, while the general and exclusive property* of these movements

* It is evident that this property belongs successively to the movements which I here call geometrical, and that it would consequently be a very false idea of them to regard them as movements simply possible, *i. e.* compatible with the impenetrability of matter: for, supposing, for instance, that all the system

ments is to change the state of the system, without altering the reciprocal action of the bodies which compose it. To leave, however, some distinction between them, we may call the first *absolute geometrical movements,* and the others *geometrical movements by supposition:* but when I speak simply of geometrical movements, without otherwise designing them, I shall imply both indifferently.

This being done,—since we have explained how we may determine, without the assistance of any mechanical principle, all the geometrical movements of which a given system is susceptible, it follows that the general problem which we proposed is entirely reduced by the general equation (F) to operations purely geometrical and analytical: we must, however, observe, that it is not sufficient to attribute to the *arbitraries u* different values, but we must also attribute to them different relations or directions; for, if we are contented to attribute different values to them without changing any thing in the relations or directions, we should obtain different equations, quite true and correct, but which would be evidently reduced to the same on multiplying them by different *constants.*

Second Remark.

XIX. As we are only speaking of hard bodies here, it is clear that among the different values which we may attribute to *u,* the velocity V is itself comprehended; *i. e.* that the real movement of the system is itself one of the geometrical movements of which it is susceptible: the first equation (E) is therefore contained in the indeterminate equation (F), and consequently we may reduce to this single equation (F) all the laws of equilibrium and of movement in hard bodies.

Now we have seen, that this equation is nothing else than the first (E), to which we have succeeded in giving

system be reduced to two adjacent globes, and pushing each other, it is clear, that if we force these bodies to separate or to move in a direction contrary to each other, this movement will not be impossible, but that at the same time bodies cannot assume it without ceasing to act upon each other. This movement, therefore, is not proper for attaining the object proposed, which is to change nothing in the reciprocal action of bodies.

more

more extension by means of the geometrical movements; but as we shall soon see (XXIV) the analogy of this equation (E) with the principle of the preservation of the moving powers in the shock of perfectly elastic bodies becomes striking by a slight transformation; and we shall see (XXVI), that in fact it is nothing else than this principle itself transferred to hard bodies, with the modification required by the different nature of these bodies: it is therefore this preservation of moving powers which will serve, as we have premised, as a basis to the whole of our theory of machines, whether at rest or in motion.

According to these remarks we shall briefly recapitulate the solution of the preceding problem, in order to show at one glance the course of the operations indicated.

[To be continued.]

XLIV. *Processes employed for finishing the Inside of the Palaces of the Native Princes in some Parts of the East Indies* *.

The principal workman employed by colonel Close in repairing the palace in the *Laul Baug*, gave me the following account of the processes used for finishing the inside of the palaces at *Seringapatam*.

At first sight, one would imagine that much gilding is used in the ornaments; but, in truth, not a grain of gold is employed. The workmen use a paper covered with false gilding. This they cut into the shape of flowers, and paste these on the walls or columns. The interstices are filled up with oil colours, which are all of European preparation.—The manner of making this false gilded paper is as follows:

Take any quantity of lead, and beat it with a hammer into leaves, as thin as possible. To twenty-four parts of these leaves add three parts of English glue, dissolved in water, and beat them together with a hammer, till they be

* From Buchannan's *Journey from Madras through the Mysore, Canara, and Malabar.*

thoroughly

thoroughly united, which requires the labour of two persons for a whole day. The mass is then cut into small cakes, and dried in the shade. These cakes can, at any time, be dissolved in water, and spread thin with a hair brush on common writing paper. The paper (when dry) must be put on a smooth plank, and rubbed with a polished stone till it acquire a complete metallic lustre. The edges of the paper are then pasted down on the board, and the metallic surface is rubbed with the palm of the hand, which is smeared with an oil called *gurna*, and then exposed to the sun. On the two following days the same operation is repeated; when the paper acquires a metallic yellow colour, which, however, more resembles the hue of brass than that of gold.

The *gurna* oil is prepared as follows:—Take three quarters of a maund (about 18 lib.) of linseed oil, half a maund (12 lib.) of the size called *chunderasu*, and a quarter of a maund (6 lib.) of *musambra*, or aloes prepared in the country. Boil the oil for two hours in a brass pot. Bruise the *musambra*; and, having put it into the oil, boil them for four hours more. Another pot having been made red-hot, the *chunderasu* is to be put into it, and will immediately melt. Take a third pot, and, having tied a cloth over its mouth, strain into it the oil and musambra: these must be kept in a gentle heat, and the chunderasu added to them gradually. The oil must be strained again; and it is then fit for use.

The *chunderasu* is prepared from the milky juice of any of the following trees: (*ficus glomerata* Roxb.), *goni* (a tree which I call *ficus gonia*), Bayla, Bayvina, Gabali, &c. It is therefore an elastic gum.

The oil used for painting consists of two parts of linseed, and one part of *chunderasu*.

In white-washing their walls, over the *chunam*, or lime plaster, the workmen of Seringapatam first give a thin coat of *suday*, or fine clay; which is mixed with size, and put on with a hair brush. They next give a coat of whitening, made of powdered *balapum* or *pot-stone;* and then finish with a coat composed of eight parts of *abracum*, or mica, one part of powdered *balapum*, and one of size.

The

The abracum is prepared from white mica, by repeated grindings, the finer particles being removed for use by washing them from the grosser parts.

The wall, when finished in this manner, shines like the scales of a fish; and when the room is lighted has a splendid appearance: but in the day-time, the wall washed with the powdered potstone alone, in my opinion, looks better than when washed with either quick-lime or mica.

XLV. *Notice upon the Analyses of the Chromate of Iron, and upon the Variety of the Epidote called Zoysite.* By *M.* Hauy*.

M. Laugier has published in a former number of these Annales†, the result of the analysis he made of the chromate of iron of Siberia, and this result was similar to that obtained by M. Vauquelin, when examining the chromate of iron discovered by M. Pontier in the department of Var. M. Klaproth lately repeated the analysis of the same substance upon a piece which came from Krieglach, in Styria; and his results having been communicated to M. Laugier, we now insert it, as presenting a confirmation of the two preceding analyses.

	KLAPROTH. Chromate of Iron of Styria.	*LAUGIER.* Chromate of Siberia.	*VAUQUELIN.* Chromate of Var.
Oxide of chrome	55·5	53	43
Oxide of iron - -	33	34	34·7
Alumine - - -	6	11	20·3
Silex - - - - -	2	1	2
Waste by roasting	2	0	0
Loss - - - -	1·5	1	0
	100	100	100

We find in a subsequent number the result of another analysis made by M. Laugier upon a grayish substance

* From *Annales du Muséum d'Histoire*, tome ix. p. 103.
† See Phil. Mag. vol. xxiv. p. 3

brought

brought from the Valais, which I recognised from its structure and physical properties to be a variety of the epidote, although it differed in its external characters from the crystals of this species hitherto observed. M. Laugier found that the respective quantities and qualities of its component principles were the same as in the epidote of Arendal, and in that of France, analysed by M. Vauquelin and M. Descostils.

The same variety also exists in Carinthia and in some of the neighbouring countries; and M. Werner has since given the name of zoysite to it in honour of baron Zoys. The distinction which M. Werner establishes between this substance and our epidote is in some measure a consequence of the nomenclature adopted by this celebrated naturalist; for he gives to the epidote the name of Pistazite (Pistachio stone) because it is generally of a more or less deep green. Now this name seems to exclude the zoysite, the colour of which is gray, brown, or brownish yellow, but never green; at least it is so in those specimens we have seen.

M. Laugier has been informed that Messrs. Klaproth and Bucholz have recently analysed the zoysite; and the following are their results, compared with those of the French chemist.

	KLAPROTH. Gray Epidote, said to be Zoysite.	*LAUGIER.* The same Substance.	*BUCHOLZ.* The same Substance.
Silex	45	37	40·25
Alumine	29	26·6	30·25
Lime	21	20	22·5
Oxide of iron	3	13	4·5
Oxide of manganese	0	0·6	0·0
Water	0	1·8	2
Loss	2	1	0·5
	100	100	100

If we compare these three results either with each other or with the others which have for their objects the epidotes of Norway and France; and if on the one hand we consider the agreement which exists between chemistry and the geometry

geometry of crystals, we shall find the most convincing proofs that the zoysite should be joined to the epidote, like the mineral of Norway, which a deceitful indication of its characters had made to be placed in a particular species under the names of *arendalite* and *akanticone.*

XLVI. *On drying Articles of Manufacture, and heating Buildings, by Steam.* By R. BUCHANNAN, *Esq., Civil Engineer, Glasgow.*

To Mr. Tilloch.

SIR,

MANY additional facts with regard to heating by steam have lately been ascertained in this neighbourhood, and its application to various processes in manufactures continues to increase. Mr. Richard Gillespie is highly pleased with its effects upon copper-plate callico-printing at his works, as also for heating his calenders. For this last purpose, and to warm his warehouse and counting-house, the steam is conveyed to a distance of above ninety-three yards.

Steam was, I believe, tried many years ago at Leeds, for drying goods, as a substitute for stoves; but for some reason, of which I am ignorant, was abandoned. Mr. Lounds, at Paisley, however, has for a considerable time used it with great success in drying fine muslins. Messrs. Leys, Mason and Co. now also use it at their bleaching works, at Aberdeen.

Some kinds of muslins have for several years been dried by being rolled round cylinders of tin plate filled with steam, but I do not here allude to that mode.

For drying of dyed yarn and pullicates, (a kind of coloured chequed cotton handkerchiefs,) a higher temperature than for fine muslin is required. I am glad, however, to have it in my power to say, that Messrs. Muir, Brown, and Co., at their dyeing and bleaching works here, have found steam to answer those purposes much better than the usual mode by stoves. Mr. Muir informs me, that, although they formerly gave out their pullicates to be bleached to some of the local bleachers in this part of the country, they never had their colours in the same perfection which they now have, and which they attribute entirely to the superior effect of the steam.

It occurs to me, that steam might be applied for warming buildings in London, in many instances, with great advantage. For instance, the bed-rooms of large inns and hotels; as also large warehouses or shops, where a number of neighbouring buildings might be warmed from one boiler, which would save much in attendance and fuel, as well as in the cost of the apparatus. It is also well adapted to the purpose of warming churches, hospitals, and other large public buildings. I am, sir, your most obedient servant,

ROBERTSON BUCHANNAN.

Glasgow,
April 2, 1808.

XLVII. *On the œconomical Uses to which the Leaves and Prunings of Vines may be applied in this Country.*

To Mr. Tilloch.

SIR,

FROM experiments which I have made, I find that, on being dried, which should be done in the shade, and infused in a tea-pot, the leaves of the vine make an excellent substitute for tea. I have also found that, on being cut small, bruised, and put into a vat, or mashing-tub, and boiling water poured on them, in the same way as is done with malt, the prunings of the vine produce a liquor of a fine vinous quality; which, on being fermented, makes a very fine beverage, either strong or weak, as you please; and, on being distilled, produces an excellent spirit of the nature of brandy.

In the course of my experiments I found that the fermented liquor from the prunings, particularly the tendrils, when allowed to pass the vinous and to run into the acetous fermentation, makes uncommonly fine vinegar. If not intended to be distilled soon after they are lopped off, or if it should not be convenient to do so at the time, they should be be dried in the shade. When intended to be used, an extract should be made with hot water, as in the common process for distilling from grain.

As this is the season when the vine puts forth its leaves, and many thousand cart-loads of the prunings, where there are not goats to eat them, are yearly thrown away as useless, your stating the above in your highly interesting and useful Magazine may be of use to many of your readers, and to the public in general. I am, sir,

your constant reader,

JAMES HALL.

London,
April 8, 1808.

XLVIII. *Observations on the Nature of the new celestial Body discovered by Dr.* OLBERS; *and of the Comet which was expected to appear last January in its Return from the Sun. By* WILLIAM HERSCHEL, *L.L.D F.R.S.* *

THE late discovery of an additional body belonging to the solar system, by Dr. Olbers, having been communicated to me the 20th of April, an event of such consequence engaged my immediate attention. In the evening of the same day I tried to discover its situation by the information I had obtained of its motion; but the brightness of the moon, which was near the full, and at no great distance from the object for which I looked, would not permit a star of even the 5th magnitude to be seen; and it was not till the 24th that a tolerable view could be obtained of that space of the heavens in which our new wanderer was pursuing its hitherto unknown path.

As soon as I found that small stars might be perceived, I made several delineations of certain telescopic constellations, the first of which was as represented in figure 1, and I fixed upon the star A, as most likely, from its expected situation and brightness, to be the one I was looking for. The stars in this figure, as well as in all the other delineations I had made,

* From Philosophical Transactions, for 1807, Part II.

made, were carefully examined with several magnifying powers, that in case any one of them should hereafter appear to have been the lately discovered object, I might not lose the opportunity of an early acquaintance with its condition. An observation of the star marked A, in particular, was made with a very distinct magnifying power of 460, and says, that it had nothing in its appearance that differed from what we see in other stars of the same size; indeed Dr. Olbers, by mentioning in the communication which I received, that with such magnifying powers as he could use it was not not to be distinguished from a fixed star*, had already prepared me to expect the newly discovered heavenly body to be a valuable addition to our increasing catalogue of asteroids.

The 25th of April I looked over my delineations of the preceding evening, and found no material difference in the situation of the stars I had marked for examination; and in addition to them new asterisms were prepared, but on account of the retarded motion of the new star, which was drawing towards a period of its retrogradation, the small change of its situation was not sufficiently marked to be readily perceived the next day when these asterisms were again examined, which it is well known can only be done with night-glasses of a very low magnifying power.

A long interruption of bad weather would not permit any regular examination of the situation of small stars; and it was only when I had obtained a more precise information from the astronomer royal, who, by means of fixed instruments, was already in possession of the place and rate of motion of the new star, that I could direct my telescope with greater accuracy by an applcaition of higher magnifying powers. My observations on the nature of this second new star discovered by Dr. Olbers are as follow.

April 24. This day, as we have already seen, the new celestial object was examined with a high power; and since a mag-

* Der neue planet zeigt sich als ein stern zwischen der 5ten und 6ten grösse und ist im fernrohr, wenigsten mit den vergrösserungen die ich anwenden kann, von einen fixstern nicht zu unterscheiden.

a magnifier of 460 would not show it to be different from the stars of an equal apparent brightness, its diameter must be extremely small, and we may reasonably expect it to be an asteroid.

May 21. With a double eye-piece magnifying only 75 times the supposed asteroid A makes a right-angled triangle with two small stars *a b*. See fig. 2.

With a very distinct magnifier of 460 there is no appearance of any planetary disk.

May 22. The new star has moved away from *a b*, and is now situated as in fig. 3. The star A of figure 1 is no longer in the place where I observed it the 24th of April, and was therefore the asteroid. I examined it now with gradually increased magnifying powers, and the air being remarkably clear, I saw it very distinctly with 460, 577, and 636. On comparing its appearance with these powers alternately to that of equal stars, among which was the 463d of Bode's catalogue of the stars in the Lion of the 7th magnitude, I could not find any difference in the visible size of their disks.

By the estimations of the distances of double stars, contained in the first and second classes of the catalogues I have given of them, it will be seen that I have always considered every star as having a visible, though spurious, disk or diameter: and in a late paper I have entered at large into the method of detecting real disks from spurious ones: it may therefore be supposed that I proceeded now with Vesta (which name I understand Dr. Olbers has given the asteroid), as I did before in the investigation of the magnitudes of Ceres, Pallas, and Juno.

The same telescopes, the same comparative views, by which the smallness of the latter three had been proved, convinced me now that I had before me a similar fourth celestial body.

The disk of the asteroid which I saw was clear, well defined, and free from nebulosity. At the first view I was inclined to believe it a real one; and the Georgian planet being conveniently situated, so that a telescope might with-

 out

out loss of time be turned alternately either to this or to the asteroid, I found that the disk of the latter, if it were real, would be about one-sixth of the former, when viewed with a magnifying power of 460. The spurious nature of the asteroidal disk, however, was soon manifested by an increase of the magnifying power, which would not proportionally increase its diameter as it increased that of the planet; and a real disk of the asteroid still remains unseen with a power of 636.

May 23. The new star has advanced, and its motion is direct; its situation with respect to the two small stars *a b*, is given in figure 4.

Its apparent disk with a magnifier of 460 is about 5 or 6-tenths of a second; but this is evidently a spurious appearance, because higher powers destroy the proportion it bears to a real disk when equally magnified. The air is not sufficiently pure this evening to use large telescopes.

May 24. With a magnifying power of 577 I compared the appearance of the Georgian planet to that of the asteroid, and with this power the diameter of the visible disk of the latter was about one 9th or 10th part of the former. The apparent disk of the small star near β Leonis, which has been mentioned before, had an equal comparative magnitude, and probably the disks of the asteroid and of the star it resembles are equally spurious.

The 20 feet reflector, with many different magnifying powers, gave still the same result; and being already convinced of the impossibility, in the present situation of the asteroid, which is above two months past the opposition, to obtain a better view of its diameter, I used this instrument chiefly to ascertain whether any nebulosity or atmosphere might be seen about it. For this purpose the valuable quantity of light collected by an aperture of 18$\frac{3}{4}$ inches directly received by an eye-glass of the front-view without a second reflection, proved of eminent use, and gave me the diameter of this asteroid entirely free from all nebulous or atmospheric appearances.

The result of these observations is, that we now are in

possession

possession of a formerly unknown species of celestial bodies, which, by their smallness and considerable deviation from the path in which the planets move, are in no danger of disturbing, or being disturbed by them; and the great success that has already attended the pursuit of the celebrated discoverers of Ceres, Pallas, Juno, and Vesta, will induce us to hope that some further light may soon be thrown upon this new and most interesting branch of astronomy.

Observations of the expected Comet.

The comet which has been seen descending to the sun, and from the motion of which it was concluded that we should probably see it again on its return from the perihelion, was expected to make its reappearance about the middle of last January, near the southern parts of the constellation of the Whale.

January 27. Towards the evening, on my return from Bath, where I had been a few days, I gave my sister Carolina the place where this comet might be looked for, and between flying clouds, the same evening about $6^h\ 49'$ she saw it just long enough to make a short sketch of its situation.

January 31. Clouds having obscured the sky till this time, I obtained a transitory view of the comet, and perceived that it was within a few degrees of the place which had been assigned to it; the unfavourable state of the atmosphere, however, would not permit the use of any instrument proper for examining it minutely.

There will be no occasion for my giving a more particular account of its place, than that it was very near the electrometer of the constellation, which in Mr. Bode's maps is called *machina electrica;* the only intention I had in looking for it, being to make a few observations upon its physical condition.

February 1. The comet had moved but very little from the place where it was last night; and as the air was pretty clear, I used a 10-feet reflector with a low power to examine it. There was no visible nucleus, nor did the light which is called the coma increase suddenly towards the centre,

tre, but was of an irregular round form, and with this low power extended to about 5, 6, or 7 minutes in diameter. When I magnified 169 times it was considerably reduced in size, which plainly indicated that a further increase of magnifying power would be of no service for discovering a nucleus. On account of cloudy weather I never had an opportunity of seeing the comet afterwards.

When I compare these observations with my former ones of 15 other telescopic comets, I find that out of the 16 which I have examined, 14 have been without any visible solid body in their centre, and that the other two had a very ill defined small central light, which might perhaps be called a nucleus, but did not deserve the name of a disk.

XLIX. *An Account of a remarkable Shower of Meteoric Stones, at Weston in America. By Mr.* Silliman, *Professor of Chemistry, and Mr.* Kingsley, *Professor of Languages, in Yale College**.

Yale College, Dec. 26, 1807.

As imperfect and erroneous accounts of the late phænomenon at Weston are finding their way into circulation, we take the liberty of enclosing for publication the result of an investigation into the circumstances and evidence of the event referred to, which we have made on the ground where it happened. That we may not interrupt our narration by repeating the observation wherever it is applicable, we may remark, once for all, that we visited and carefully examined every spot where the stones had been ascertained to have fallen, and several places where they had been only suspected without any discovery; that we obtained specimens of every stone—conversed with all the principal original witnesses; spent several days in the investigation, and were, at the time, the only persons who had explored the *whole* ground.

Benjamin Silliman.
James L. Kingsley.

* Communicated by the right hon. Charles Greville, F.R.S. &c.

The

The meteor which has so recently excited alarm in many, and astonishment in all, first made its appearance in Weston, about a quarter or half past six o'clock, A. M., on Monday the 14th instant (Dec. 1807). The morning was somewhat cloudy; the clouds were dispersed in unequal masses, being in some places thick and opaque, in others light, fleecy, and partially transparent; while spots of unclouded sky appeared here and there among them. Along the northern part of the horizon, a space of 10 or 15 degrees was perfectly clear. The day had merely dawned, and there was little or no light, except from the moon, which was just setting. Judge Wheeler, to whose intelligence and observation, apparently uninfluenced by fear or imagination, we are indebted for the substance of this part of our account, was passing through the enclosure adjoining his house, with his face to the north, and his eyes on the ground, when a sudden flash, occasioned by the transition of a luminous body across the northern margin of clear sky, illuminated every object, and caused him to look up. He immediately discovered a globe of fire, just then passing behind the first cloud, which was very dark, and obscured, although it did not entirely hide the meteor.

In this situation its appearance was distinct, and well defined, like that of the sun seen through a mist. It rose from the north, and proceeded in a direction nearly perpendicular to the horizon, but inclining, by a very small angle, to the west, and deviating a little from the plane of a great circle, but in pretty large curves, sometimes on one side of the plane, and sometimes on the other, but never making an angle with it of more than 4 or 5 degrees. It appeared about one half or two thirds the diameter of the full moon. This description of its apparent magnitude is vague, but it was impossible to ascertain what angle it subtended. Its progress was not so rapid as that of common meteors and shooting stars. When it passed behind the thinner clouds, it appeared brighter than before: and when it passed the spots of clear sky it flashed with a vivid light, yet not so intense as the lightning in a thunder-storm, but

 rather

rather like what is commonly called *heat lightning.* Its surface was apparently convex.

Where it was not too much obscured by thick clouds, a conical train of paler light was seen to attend it, waving, and in length about 10 or 12 diameters of the body. In the clear sky a brisk scintillation was observed about the body of the meteor, like that of a burning firebrand carried against the wind.

It disappeared about 15 degrees short of the zenith, and about the same number of degrees west of the meridian. It did not vanish instantaneously, but grew, pretty rapidly, fainter and fainter, as a red-hot cannon ball would do, if cooling in the dark, only with much more rapidity.

There was no peculiar smell in the atmosphere, nor were any luminous masses seen to separate from the body. The whole period between its first appearance and total extinction was estimated at about 30 seconds.

About 30 or 40 seconds after this, three loud and distinct reports, like those of a four-pounder, near at hand, were heard. They succeeded each other with as much rapidity as was consistent with distinctness, and, all together, did not occupy three seconds. Then followed a rapid succession of reports less loud, and running into each other, so as to produce a continued rumbling, like that of a cannon ball rolling over a floor, sometimes louder and at other times fainter; some compared it to the noise of a waggon, running rapidly down a long and stony hill; or to a volley of musquetry, protracted into what is called, in military language, a *running fire.* This noise continued about as long as the body was in rising, and died away, apparently in the direction from which the meteor came.

The accounts of others corresponded substantially with this. Time was differently estimated by different people, but the variation was not material. Some augmented the number of loud reports, and terror and imagination seem, in various instances, to have magnified every circumstance of the phænomenon.

The only thing which seemed of any importance beyond this

this statement, was derived from Mr. Elihu Staples, who said, that when the meteor disappeared, there were apparently three successive efforts or leaps of the fire-ball, which grew more dim at every throe, and disappeared with the last.

Such were the sensible phænomena which attended this meteor. We purposely avoid describing the appearances which it assumed in other places, leaving this task to others who have the means of performing it more accurately; while we proceed to detail the consequences which followed the explosions and apparent extinction of this luminary.

We allude to the fall of a number of masses of stone in several places, principally within the town of Weston. The places which had been well ascertained at the period of our investigation, were six. The most remote were about 9 or 10 miles distant from each other, in a line differing little from the course of the meteor. It is therefore probable that the successive masses fell in this order, the most northerly first, and the most southerly last. We think we are able to point out three principal places where stones have fallen, corresponding with the three loud cannon-like reports, and with the three leaps of the meteor observed by Mr. Staples. There were some circumstances common to all the cases. There was in every instance, immediately after the explosions had ceased, a loud whizzing or roaring noise in the air, observed at all the places, and, so far as was ascertained, at the moment of the fall. It excited in some the idea of a tornado; in others, of a large cannon-shot in rapid motion; and it filled all with astonishment and apprehension of some impending catastrophe. In every instance immediately after this was heard a sudden and abrupt noise, like that of a ponderous body striking the ground in its fall. Excepting one, the stones were more or less broken. The most important circumstances of the particular cases were as follow:

1. The most northerly fall was within the limits of Huntington, on the border of Weston, about 40 or 50 rods east of the great road from Bridgeport to Newtown, in a cross road, and contiguous to the house of Mr. Merwin Burr. Mr. Burr

Mr. Burr was standing in the road, in front of his house, when the stone fell. The noise produced by its collision with a rock of granite, on which it struck, was very loud. Mr. Burr was within 50 feet, and immediately searched for the body, but, it being still dark, he did not find it till half an hour after. By the fall, some of it was reduced to powder, and the rest of it was broken into very small fragments, which were thrown around to the distance of 20 or 30 feet. The granite rock was stained at the place of contact with a deep lead colour. The largest fragment which remained did not exceed the size of a goose-egg, and this Mr. Burr found to be still warm to his hand. There was reason to conclude from all the circumstances, that this stone must have weighed about twenty or twenty-five pounds.

Mr. Burr had a strong impression that another stone fell in an adjoining field, and it was confidently believed that a large mass had fallen into a neighbouring swamp, but neither of these had been found. It is probable that the stone whose fall has now been described, together with any other masses which may have fallen at the same time, was thrown from the meteor at the first explosion.

2. The masses projected at the second explosion seem to have fallen principally at and in the vicinity of Mr. William Prince's, in Weston, distant about five miles, in a southerly direction, from Mr. Burr's. Mr. Prince and family were still in bed, when *they heard a noise like the fall of a very heavy body, immediately after the explosions.* They formed various unsatisfactory conjectures concerning the cause— nor did even a fresh hole made through the turf in the door-yard, about twenty-five feet from the house, lead to any conception of the cause, or induce any other inquiry than why a new post-hole should have been dug where there was no use for it. So far were this family from conceiving of the possibility of such an event as stones falling from the clouds. They had indeed formed a vague conjecture that the hole might have been made by lightning, but would probably have paid no further attention to the circumstance, had they not heard, in the course of the day, that stones had fallen that morning in other parts of the town. This induced

induced them, towards evening, to search the hole in the yard, where they found a stone buried in the loose earth which had fallen in upon it. It was two feet from the surface—the hole was about twelve inches in diameter; and as the earth was soft and nearly free from stones, the mass had sustained little injury, only a few small fragments having been detached by the shock. The weight of this stone was about thirty-five pounds. From the descriptions which we have heard, it must have been a noble specimen, and men of science will not cease to deplore that so rare a treasure should have been immediately broken in pieces. All that remained unbroken of this noble mass, was a piece of twelve pounds weight, since purchased by Isaac Bronson, esq., of Greenfield, with the liberal view of presenting it to some public institution.

Six days after, another mass was discovered, half a mile north-west from Mr. Prince's. The search was induced by the confident persuasion of the neighbours that they heard it fall near the spot where it was actually found buried in the earth, weighing from seven to ten pounds. It was found by Gideon Hall and Isaac Fairchild. It was in small fragments, having fallen on a globular detached mass of gneiss rock, which it split in two, and by which it was itself shivered to pieces.

The same men informed us that they supected another stone had fallen in the vicinity, as the report had been distinctly heard and could be referred to a particular region somewhat to the east. Returning to the place after an excursion of a few hours to another part of the town, we were gratified to find the conjecture verified, by the actual discovery of a mass of thirteen pounds weight, which had fallen half a mile to the north-east of Mr. Prince's. Having fallen in a ploughed field, without coming into contact with a rock, it was broken only into two principal pieces, one of which, possessing all the characters of the stone in a remarkable degree, we purchased; for it had now become an article of sale.—It was urged that it had pleased Heaven to rain down this treasure upon them, and they would bring their thunderbolts to the best market they could. This was, it must be confessed, a wiser mode of managing the business than that which had been adopted by some others at an earlier period of these discoveries. Strongly impressed with the idea that these stones contained gold and silver, they subjected them to all the tortures of ancient alchemy, and the goldsmith's crucible, the forge, and the blacksmith's anvil, were employed in vain to elicit riches which existed only in the imagination.

Two miles south-east from Mr. Prince's, at the foot of Tashowa hill, a fifth mass fell. Its fall was distinctly heard by Mr. Ephraim Porter and his family, who live within 40 rods of the place, and in full view. They saw a smoke rise from the spot, as they did also from the hill, where they are positive that another stone struck, as they heard it distinctly. At the time of the fall, having never heard of any such thing, they supposed that lightning had struck the ground ; but after three or four days, hearing of the stones which had been found in their vicinity, they were induced to search, and the result was the discovery of a mass of stone in the road, at the place where they supposed the lightning had struck. It penetrated the ground to the depth of two feet in the deepest place ; the hole was about twenty inches in diameter, and its margin was coloured blue from the powder of the stone struck off in its fall.

It was broken into fragments of moderate size, and from the best calculations might have weighed 20 or 25 pounds.

The hole exhibited marks of much violence, the turf being very much torn, and thrown about to some distance.

It is probable that the four stones last described were all projected at the second explosion, and should one be discovered on the neighbouring hill *, we must without doubt refer it to the same avulsion.

3. Last of all, we hasten to what appears to have been the catastrophe of this wonderful phænomenon.

A mass of stone far exceeding the united weight of all

* Which has since been found, weighing thirty-six pounds and a quarter. I have seen and weighed it myself.——*G. Burr.*

which

which we have hitherto described, fell in a field belonging to Mr. Elijah Seely, and within 30 rods of his house.

A circumstance attended the fall of this which seems to have been peculiar.—Mr. Elihu Staples, a man of integrity, lives on the hill at the bottom of which this body fell, and witnessed the first appearance, progress, and explosion of the meteor. After the last explosion, a rending noise like that of a whirlwind passed along to the east of his house and immediately over his orchard, which is on the declivity of the hill. At the same instant a streak of light passed over the orchard in a large curve, and seemed to pierce the ground. A shock was felt, and a report heard like that of a heavy body falling to the earth; but no conception being entertained of the real cause, (for no one in this vicinity, with whom we conversed, appeared to have ever heard of the fall of stones from the skies,) it was supposed that lightning had struck the ground. Three or four hours after the event Mr. Seeley went into his field to look after his cattle.—He found that some of them had leaped into the adjoining enclosure, and all exhibited strong indications of terror. Passing on, he was struck with surprise at seeing a spot of ground which he knew to have been recently turfed over, all torn up, and the earth looking fresh, as if from recent violence. Coming to the place, he found a great mass of fragments of a strange-looking stone, and immediately called for his wife, who was second on the ground.

Here were exhibited the most striking proofs of violent collision.—A ridge of micaceous schistus lying nearly even with the ground, and somewhat inclining like the hill to the south-east, was shivered to pieces, to a certain extent, by the impulses of the stone, which thus received a still more oblique direction, and forced itself into the earth to the depth of three feet, tearing a hole of five feet in length and four feet and a half in breadth, and throwing large masses of turf and fragments of stone and earth to the distance of 50 and 100 feet. Had there been no meteor, no explosions, and no witnesses of the light and shock, it would have been impossible for any person contemplating the scene to doubt that

that a large and heavy body had really fallen from the skies with tremendous momentum.

This stone was all in fragments, none of which exceeded the size of a man's fist, and was rapidly dispersed by numerous visitors who carried it away at pleasure. Indeed we found it very difficult to obtain a sufficient supply of specimens of the various stones, an object which was at length accomplished principally by importunity and purchase. From the best information which we could obtain of the quantity of fragments of this last stone, compared with its specific gravity, we concluded that its weight could not have fallen much short of 200 pounds. All the stones, when first found, were friable, being easily broken between the fingers; this was especially the case where they had been buried in the moist earth, but by exposure to the air they gradually hardened. Such were the circumstances attending the fall of these singular masses. We have named living witnesses; the list of these may be augmented, but we consider the proof as sufficient to satisfy any rational mind. Further confirmation will be derived from the mineralogical description and chemical examination of these stones.

The specimens obtained from all the different places are perfectly similar. The most careless observer would instantly pronounce them portions of a common mass, and different from any of the stones commonly seen on this globe.

Of their form nothing very certain can be said, because only comparatively small fragments of the great body of the meteor have been obtained. Few of the specimens weigh one pound—most of them less than half a pound, and from that to the fraction of an ounce. Mr. Bronson's piece is the largest with which we are acquainted; we possess the next, which weighs six pounds, and is very perfect in its characteristic marks, and we have a good collection of smaller specimens, many of which are very instructive. They possess every irregular variety of form which might be supposed to arise from accidental fracture with violent force. On many of them, however, and chiefly on the large specimens,

mens, may be distinctly perceived portions of the external part of the meteor.

It is every where covered with a thin black crust, destitute of splendour, and bounded by portions of the large irregular curve which seems to have enclosed the meteoric mass. This curve is far from being uniform. It is sometimes depressed with concavities such as might be produced by pressing a soft and yielding substance. The surface of the crust feels harsh like the prepared fish skin or shagreen. It gives sparks with steel. There are certain portions of the stones covered with the black crust, which appear not to have formed a part of the outside of the meteor, but to have received this coating in the interior parts, in consequence of fissures or cracks, produced probably by the intense heat to which the body seems to have been subjected. The specific gravity of the stone is 3·6, water being 1. The colour of the mass of the stone is principally a dark ash, or more properly a leaden colour. It is interspersed with distinct masses, from the size of a pin's head to the diameter of one or two inches, which are almost white, resembling in many instances the crystals of feldtspar in some varieties of granite, and in that species of porphyry known by the name of *verd antique*.

The texture of the stone is granular and coarse, resembling some pieces of grit-stone. It cannot be broken by the fingers, but gives a rough and irregular fracture with the hammer.

On inspecting the mass, four distinct kinds of matter may be perceived by the eye.

1. The stone is thickly interspersed with black globular masses, most of them spherical, some are oblong and irregular. The largest are of the size of a pigeon-shot, but generally they are much smaller. They can be detached with any pointed iron instrument and leave a concavity in the stone. They are not attractable by the magnet, and can be broken with the hammer.

2. Masses of yellow pyrites may be observed. Some of them are of a brilliant golden colour, and are readily distinguished with the eye.

3. The whole stone is thickly interspersed with metallic points, many of them evident to the eye, and they appear numerous and distinct with a lens. Their colour is whitish, and was mistaken by the discoverers of the stone for silver. They appear to be chiefly malleable iron alloyed with nickel.

4. The lead-coloured mass which cements these things together, has been described already, and constitutes by far the greater part of the stone. After being wet and exposed to the air, the stone becomes covered with numerous reddish spots, which do not appear in a fresh fracture, and arise manifestly from the rusting of the iron.

Finally, the stone has been analysed in the laboratory of this College according to the excellent instructions of Howard, Vauquelin, and Fourcroy. The analysis was hasty, and intended only for the purpose of general information. The exact proportions, and the steps of the analysis, are reserved for more leisure, and may be given to the philosophical world hereafter. It is sufficient at present to observe that the stone appears to consist of the following ingredients:—*silex, iron, magnesia, nickel, sulphur.*

The two first constitute by far the greater part of the stone—the third is in considerable proportion, but much less than the others—the fourth is probably still less; and the sulphur exists in a small but indeterminate quantity.

Most of the iron is in a perfectly metallic state; the whole stone attracts the magnet, and this instrument takes up a large proportion of it when pulverized. Portions of metallic iron may be separated, so large that they can be readily extended under the hammer. Some of the iron is in combination with sulphur in the pyrites, and probably most of the iron is alloyed by nickel.

It remains to be observed that this account of the appearance of the stone accords very exactly with the descriptions, now become considerably numerous, of similar bodies which have fallen in other countries at various periods; and with specimens which one of us has inspected, of stones that have fallen in India, France, and Scotland. The chemical analysis also proves that their composition is the same; and

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it is well known to mineralogists and chemists that no such stones have been found among the productions of this globe. These considerations, together with the facts that are immediately to be mentioned, must, in connection with the testimony, place the credibility of the facts asserted to have recently occurred in Weston, beyond all controversy.

The falling of stones from the clouds is an event which has frequently happened in Europe, in Asia, and in South America. The accounts of such phænomena were, for a long time, rejected by philosophers, as the offspring of ignorance and superstition. Several facts of this kind, however, within a few years, have been proved by evidence so unexceptionable, as to overcome the most obstinate incredulity. It is now admitted not only that such phænomena have existed in modern times, but that the accounts of similar events in former ages are in a high degree probable. As this is the first time that stones are known to have fallen in this part of America, it may not be uninteresting to those who have paid little attention to this subject, or who still hesitate to admit that such things have happened, to see a statement of several similar events in other countries, and some of the evidence by which they are supported.

In 1492, on the 7th of November, at Ensisheim in Upper Alsace, a stone fell from the atmosphere which weighed 260 pounds. Contemporary writers agree in stating, that on this day, between 11 and 12 o'clock in the morning, a loud explosion was heard at Ensisheim, and that this stone was soon after seen to fall in a field at no great distance from the town. This stone, till within a few years, was preserved in the parish of Ensisheim.

In 1762, two stones fell at Verona, one of which weighed 200 and the other 300 pounds. Three or four hundred persons were witnesses of the event.

In 1790, on the 24th of July, a shower of stones fell near Agenin, Guienne. About nine or ten o'clock at night a meteor was seen moving through the atmosphere with very great velocity. A loud explosion was soon heard, which was followed after a short interval by a shower of stones over a considerable extent of country.

In April 1802, the same thing happened at L'Aigle. Biot, a member of the French National Institute, who visited the place to ascertain the fact, writes to this effect. Persons of all professions, manners and opinions,—ecclesiastics, soldiers and labourers, men, women, and children,—agree in referring the event to the same day, the same hour, and the same minute. They say they saw the stones descending along the roofs of the houses, break the branches of the trees, and rebound after they fell upon the pavement. They say they saw the earth smoke around the largest of them, and that the stones were still hot after they had taken them in their hands. The mineralogical collections formed on the spot with the greatest care contained nothing of the kind. On a sudden, and only since the time of the meteor, these stones have been found, and within a certain extent.

Within fifteen years past the falling of similar bodies, under similar circumstances, has happened in Portugal, Bohemia, France, Great Britain, India, and South America.

To account for the existence of these stones, various theories have been formed by philosophers. Some have supposed them to be only common stones struck with lightning and partly melted. But this theory has now no advocates. A less fanciful hypothesis is, that they are masses of matter thrown from volcanoes. But to this there are serious objections. No such bodies are found near the craters of volcanoes, or are known to be projected from them. And in many instances these bodies have fallen several hundred and even several thousand miles from any known volcano. Mr. Edward King has varied this theory, and supposes that these substances are thrown from volcanoes not in solid masses, but in the state of ashes or dust. He supposes that these ashes descending in a cloud become condensed, take fire, and produce numerous explosions. According to him, the pyritical, metallic, and argillaceous particles melt, are suddenly crystallized and consolidated, and fall in masses to the ground. This explanation evidently involves as great difficulties as those which it is intended to obviate. Some philosophers have supposed that these stones are thrown from terrestrial comets. Not to mention any other objec-

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tion to this hypothesis, it will by no means account for such a phænomenon as appeared at Sienna in 1794, when stones descended, not from a moving meteor, but from a luminous cloud. Other philosophers, ascribing to these stones an origin still more extraordinary, suppose them to be projected from the moon. Admitting that bodies can be projected beyond the sphere of the moon's attraction, they must move round the earth in one of the conic sections, and all the difficulties attending the preceding hypothesis embarrass this. The subject must be acknowledged to be involved in much obscurity, and the phænomenon, till we are possessed of more facts and better observations, must be considered inexplicable.

L. *Memoir upon the Torpidity of Monkeys and other Animals. Translated from the Italian of M.* MANGALI, *Professor of Natural History at Pavia*.*

ONE of the phænomena most worthy the attention of naturalists, is the profound sleep called lethargy, to which a great number of cold-blooded animals are subject during winter, and several warm-blooded animals also, such as monkeys, bats, dormice, hedgehogs, &c.

Several authors, both antient and modern, have treated on this subject; but as they have not made proper observations and experiments, their writings leave us in much uncertainty. The National Institute of France has therefore invited the learned of Europe to examine carefully, 1st, the phænomena presented in winter by the lethargy of animals; 2dly, the cause of this lethargy; 3dly, why it is peculiar to these animals.

Without pretending to give a complete solution of a problem, which, as it is announced, appears to me to be attended with difficulty, and particularly the latter part of it, I shall detail what I have remarked respecting several of the animals in question.

* From *Annales du Muséum d'Histoire Naturelle,* tome ix. p. 106.

I flatter myself that the results of my observations will communicate to the learned the principal phænomena which attend the lethargy of these mammiferous animals, the order pursued by their principal organic functions, and the nature of the action of a high or low temperature upon their internal œconomy.

I shall say nothing of the habits of these animals in the natural state, and in the state of slavery; I shall perhaps have occasion to speak of this at another time:—at present I mean to confine myself to an account of what I have observed in monkeys, which are the most remarkable among the animals subject to periodical lethargy.

The principal object of my observations has been to examine the various phænomena presented by monkeys during their lethargy, far different from the mortal lethargy, which may take place in all animals from the effects of cold. I have particularly studied the progress of their principal organic functions, because physiologists are generally at variance upon this subject; some pretending that these functions are entirely suspended, and others that they continue, although in a languishing manner, scarcely sufficient to evince the existence of the vital principle.

A celebrated naturalist of the last century expresses himself in the following manner:—

"When sleep commences, the respiration then becomes less; it ceases when the lethargy is complete. The eyes of the monkey are then closed, its body is bent in the form of a bow, it is immoveable and entirely cold. We may roll it about, throw it up into the air, and use it ill without giving the smallest signs of life." The same author asserts that a sharper degree of cold, in place of awakening monkeys, renders their lethargy much more profound.

I shall not quote what other authors have since written upon the subject, as it is evident they have not made the observations necessary to enable them to decide upon the cause.

On the first of December 1803, there were brought to me at Milan three male monkeys, taken in the Alps which separate

parate the territory of Chiavenne from that of the Grisons. One of the three was a little awake: I preserved it two years, in order to study its habits. The other two were in the lethargic state.

The same day I weighed the latter, in order to ascertain how much of their weight they lost in a given time of constant lethargy; the one weighed 25 Milanese ounces, and the second 22 ounces 3 deniers *. At first sight they seemed as if totally deprived of life; they were rolled up like a ball, with the nose applied to the anus, their eyes closed, the teeth locked, and they felt perfectly cold when taken into the hands.

When pinched, however, or shaken, they gave unequivocal signs of irritability; and sometimes, although rarely, I perceived a feeble dilatation and a successive sinking in the flanks, or other signs of a languishing respiration.

The two monkeys remained in the most profound lethargy until January 3d, 1804. Reaumur's thermometer, placed in the room where they were, having varied from five to nine degrees above zero, on the evening of this day the largest awoke and removed from its companion, in order to find a place where it might be more secure from the cold.

As soon as I saw that it was awake, I weighed both; and I found that the largest had lost 18 deniers of its primitive weight, and the other $17\frac{1}{2}$. At the end of 24 hours, the monkey which had awoke, again fell into a lethargy, and remained in this state until the 11th; the temperature of the room being from 5 to 8 degrees.

On the 11th in the evening, the external temperature being about 4 degrees, I exposed the same animal to the free air on the sole of my window. A short time afterwards it began to tremble and give signs of pain. I also remarked a small indication of breathing; and fearing lest a greater cold should waken it entirely, I replaced it in its bed in about an hour. In spite of my precaution its respiration became more frequent. In fact, two hours afterwards I found it almost entirely awake; but it had not removed

* The denier is the 24th part of an ounce.

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from its place, and it soon fell asleep again. Other experiments awoke it again some days afterwards, and it returned for the fourth time to its lethargic state in about 24 hours.

I have said that more than once I saw unequivocal signs of a very slow respiration. I was anxious to ascertain by experiments and repeated observations if this respiration was regularly periodical. Consequently, on the 4th of February, at nine o'clock in the evening, I placed the smallest monkey under a bell-glass, the edges of which were in some very clear lime-water. In the midst of the bell-glass was a pedestal, on which was a concave piece of wood where the monkey lay as in a nest. I took care that the water was exactly of a level within and without the bell-glass at the moment of immersion; and on the 5th of February, at nine o'clock in the morning, I found that it had risen about three lines in the inside of the bell-glass, and that a pellicle was formed at its surface. It now remained to examine the state of the air contained under the bell-glass, and the nature of the pellicle.

I tried the air with Volta's celebrated eudiometer, and I found that it had lost a part of its oxygen; some drops of nitric acid, poured upon the pellicle, produced a very brisk effervescence and liberating carbonic acid. These two experiments convinced me that during the lethargic sleep respiration is not suspended: whence it may be presumed that circulation also continues, but with a languor proportioned to that of the respiration. I have since been convinced of this by examining with a good microscope the wings of bats in the lethargic state; and I shall have occasion to speak of this in a subsequent memoir.

The smallest of the two monkeys in my experiments continuing in the most profound lethargy, I fixed my eyes upon it, and examining it with attention, I perceived a very feeble alternate depression and rising in its flanks. I took my watch, and I ascertained that these unequivocal signs of respiration were renewed at intervals of four minutes or four minutes and a half, and that there were 14 in an hour; and whereas when perfectly awake there were about 1500.

Such is the law to which these animals are subject in one

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of the principal organic functions in their natural, which I call their *preservative*, lethargy, in order to distinguish it from that produced by an excessive cold: this part is generally followed by gangrene and death, and on that account I think it should be called mortal lethargy.

If it excite surprise that I placed the monkeys in a temperature of six or nine degrees, I answer, that in general the mammiferæ subject to periodical lethargy hide themselves in holes where the temperature is mild: without this precaution they would be awakened by the pain which the cold would occasion; and if they were not able to avoid it they would be seized by the mortal lethargy, when gangrene and death would succeed.

Indeed, having frequently during the winter visited a famous grotto in this department, in which were several hundred torpid bats, I ascertained with a good Reaumur's thermometer, that the temperature of this grotto was constantly above nine degrees. We may be convinced that the temperature of the holes they dig is the same, when we reflect upon their depth, upon the care with which they close up the entrance, and upon their strewing their beds with hay. We must also observe, that their fat contributes much to preserve them from cold. It is true they inhabit places covered with snow for several months; but this snow is useful, as it hinders the frost from penetrating to them.

A moderate temperature is necessary for the continuation of the preservative lethargy: animals subject to it feel pain, and are awakened by an increased cold; they tremble, and show the most ardent desire to find a place where they can get rid of it;—of this I have been frequently convinced.

At the end of December 1799, some spiders in my apartment were awakened by the sharpness of the cold, and they turned to all corners where they might avoid it.

On the morning of the 4th of February, 1803, I found upon the sole of my window a common bat, dead. This poor animal had been torpid for some months in a hole of an adjoining wall, and had been, no doubt, awakened by the intense cold of the preceding night, which was 11 degrees. It had flown to my chamber window in the hope

hope of getting in; but its wings having been too torpid to enable it to fly further, on being disappointed, it was attacked with the mortal lethargy, and died.

To return to my observations on the monkeys:—

On the 5th of February, having again weighed them, I found the smallest weighed 21 ounces, and the second 22 ounces and 21 deniers.

The small one, from the first time I had weighed it until the 5th of February, had only awoke once, on the 4th of January, and continued awake for 24 hours. From this it results, that since the 1st of January the loss of its weight was reduced to about 9 deniers, while the largest, which had awoke several times, had lost 33 deniers in the same interval.

This difference of weight proves evidently that the fat of these animals is very useful: not only do they consume a part of it during their lethargic state, but they are also fed by it in the intervals of being awake from a lowered or increased temperature. In fact, we are perfectly certain that they pass these waking periods without food, and that this fasting causes no inconvenience.

On the same day, the 5th of February, having placed the largest of the monkeys on a pedestal, and covered it with a bell-glass, the edges of which rested on a receiver filled with lime-water, I fixed my eyes upon it for an hour, in order to have a better view of the phænomena of respiration; and I was convinced beyond a doubt, that during this time the motions of inspiration and expiration were repeated 15 times in a distinct manner, and at three intervals of four or five minutes each.

On the same day, at nine o'clock in the evening, I placed the small monkey upon the outside of my window in a bed of hay. It remained for some time immoveable, simply giving those signs of a languishing respiration which always continues during lethargy; but in an hour I perceived that its respiration became more frequent, and that it appeared rather asleep than torpid; so that the external temperature, which was three degrees and a half above zero, in place of diminishing the respiration, had considerably increased it.

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I would have left it an hour longer in the same situation; but observing that its respiration continued to increase, and perceiving upon touching it that the heat of its body was greatly increased, I withdrew it and returned it to its usual place in the room. I flattered myself that it would not waken entirely; but having visited it about ten o'clock, I not only found it awake, but saw that it had resumed its natural heat and vivacity: it leapt suddenly from its nest and hid itself among the hay as if to avoid the cold, or any other accident which might take it from its gentle lethargy.

Hence it follows, that less time is required to bring monkeys out of torpidity than to plunge them into it.

The other monkey, which I had placed under the bell-glass, assumed in its little cradle a position to preserve itself from the cold, and continued to give signs of respiration 14 or 15 times in an hour. I also observed that the water had risen in the bell-glass, and that a pellicle of carbonate of lime was formed.

On the 6th of February, the thermometer in the room being at 6° or 7°, at one o'clock P. M., the external temperature having risen to $7\frac{1}{2}°$, I resolved to expose the torpid monkey, which had been below the bell-glass, upon the sole of my window. My object was to ascertain if the action of cold, when it increased insensibly, would produce upon the animal the same effects which a sudden transition caused, although there was never any difference between the temperature of the room and that of the external atmosphere.

For two hours and a half the monkey exhibited no increased signs of life; but about six o'clock I perceived some indications of a strong respiration. The night approaching, the thermometer gradually fell, so that on the outside of the window the thermometer was at 4° only. At this moment I saw the monkey agitated by convulsive movements as if from pain: it afterwards stretched itself in its cradle, its respiration increased gradually until it appeared no longer to be torpid, but rather asleep. At seven o'clock it respired 16 times per minute, whereas while in a torpid state it respired only 15 times an hour. The heat of its body, as tried

tried by the thermometer, increased with the frequency of respiration, so that by half-past nine o'clock it was perfectly awake.

Convinced by this experiment that the action of cold, although it increases almost insensibly, occasions pain to animals in a state of torpidity, I returned the monkey to its bed of hay. I tried to make it walk upon the carpet, but it could not use its hind legs, which were torpid from having been so far from the chest.

On the 20th of February, at seven o'clock in the morning, I tried another experiment with the largest torpid monkey: I placed it outside my window in a vessel surrounded by ice and muriate of lime. This mixture produced so intense a cold in the receiver, that the thermometer I placed in it fell to 7° below zero.

This sudden transition did not excite any sudden convulsive movements in the monkey: in half an hour I observed it to give signs of pain. I observed increasing signs of respiration and expiration, which must have fatigued it much. It was eleven o'clock, however, before it was completely awake. The cold continued very sharp, and it tried to escape from it several times, by moving from side to side in great pain throughout the night.

I visited it several times, and found it always trembling: its eyes were half closed. It did not sleep, however, although I left it exposed to the same cold till nine o'clock next morning. I am convinced that a sharper cold would in a short time have plunged it into that lethargy which is followed by death, when no assistance is given to prevent it.

I have here detailed only the general outlines of my observations upon monkeys. In a subsequent memoir I shall publish my experiments upon bats, hedgehogs, &c.: and I flatter myself that their result will be an accurate knowlege of the causes which plunge animals into a torpid sleep.

LI. *The*

LI. *The reformed Sexual System of Linnæus. By* ROBERT JOHN THORNTON, *M.D., Lecturer on Botany at Guy's Hospital**.

CLASSES.

I.	MONANDRIA	one stamen.
II.	DIANDRIA	two stamina.
III.	TRIANDRIA	three stamina.
IV.	TETRANDRIA	four stamina.
V.	PENTANDRIA	five stamina.
VI.	HEXANDRIA	six stamina.
VII.	HEPTANDRIA	seven stamina.
VIII.	OCTANDRIA	eight stamina.
IX.	ENNEANDRIA	nine stamina.
X.	DECANDRIA	ten stamina.
XI.	DODECANDRIA	12 to 19 stamina.
XII.	POLYANDRIA	20 or more stamina.
XIII.	CRYPTOGAMIA	concealed stamina.

ORDERS.

I. *Orders taken from the Number of Pistilla.*

I.	*Monogynia*	one pistillum,
II.	*Digynia*	two pistilla.
III.	*Trigynia*	three pistilla.
IV.	*Tetragynia*	four pistilla.
V.	*Pentagynia*	five pistilla.
VI.	*Hexagynia*	six pistilla.
VII.	*Heptagynia*	seven pistilla.
VIII.	*Octogynia*	eight pistilla.
IX.	*Enneagynia*	nine pistilla.
X.	*Decagynia*	ten pistilla.
XI.	*Dodecagynia*	12 to 19 pistilla.
XII.	*Polygynia*	20 or more pistilla.

* Extracted from Dr. Thornton's *New Illustration of the Sexual System of Linnæus*, just published; containing a great number of picturesque botanical coloured plates of select plants, illustrative of the sexual system; the most splendid botanical work that has yet appeared in any part of the world.

II. *Orders taken from some curious Particularity in the Stamina.*

XIII.	*Didynamia* ...	four stamina, two long, two short.
XIV.	*Tetradynamia*	six stamina, four long, two short.
XV.	*Icosandria*	twenty or more stamina, inserted on the calyx or corolla.
XVI.	*Monadelphia* ..	filaments united in one body.
XVII.	*Diadelphia* ...	filaments united, forming two bodies.
XVIII.	*Polyadelphia* ..	filaments united, forming three or more bodies.
XIX.	*Syngenesia* ...	five anthers united.
XX.	*Gynandria* ...	stamina arising from the pistil.
XXI.	*Monœcia*	stamina apart from the pistil on the same plant.
XXII.	*Diœcia*	stamina apart from the pistil on different plants.
XXIII.	*Polygamia*	bisexual flowers, and unisexual.

Class CRYPTOGAMIA has the Natural Orders.
I. *Filices.* II. *Musci.* III. *Algæ.* IV. *Fungi.*

REMARKS.

I.—The Class IV. TETRANDRIA, being a numerous one, Linnæus chose to separate it into two, and an opportunity presented itself from the consideration of the differences which occur in plants having four stamina, from the *proportion* of these. DIDYNAMIA expresses this ifference; and the flowers are either *ringent* or *personate,* a *natural tribe.* But as all the *ringent* flowers are not included in the class DIDYNAMIA, some coming under class II. DIANDRIA, there can be no good reason for not making this real division of a class into *an order.* The system hence becomes more *easy* and *regular,* and in fact, *frequently, more natural.*

II.—The Class VI. HEXANDRIA, also readily separates into two parts, from the like consideration of the *proportion* in the stamina, and TETRADYNAMIA contains the *natural tribe* of *cruciform* plants.

III.—The

III.—The Class XIII. Polyandria, also readily divides into two parts, from the consideration of the *insertion* of the stamina; and one of these, the Icosandria of Linnæus, possesses *many edible fruits*; but as it is not altogether a *natural class*, therefore no one can regret seeing this part distinguished as an *order*.

IV.—In the Monadelphia of Linnæus, many of the numerical names, which had been used to characterize the classes, are employed to distinguish the orders, or subdivisions, as *Pentandria*, *Decandria*, &c., and hence arises a confusion unavoidably perplexing to the young student, and which our method, as is evident, completely removes. The same observation applies to the classes *Diadelphia*, *Polyadelphia*, *Gynandria*, *Monœcia*, *Diœcia*, where the same (may I call it so) impropriety occurs. This class in Linnæus is *not natural*, but, being made into orders, many of them then become *natural* as *orders*, as the Columniferæ.

V.—The *Papilionaceous Flowers*, as they are generally termed, form the order *Decandria* in the class Diadelphia of Linnæus; but the author, unwilling, as it would seem, to make any breach in so natural an assemblage of plants, has so far deviated from the principles of his system, as to refer to that class several genera, which strictly belong to the preceding class, being in fact *Monadelphious*. This inconvenience is entirely obviated in the present scheme, where *Monadelphia* and *Diadelphia* constitute two successive orders in our class X. *Decandria*.

VI.—Polyadelphia is a small, and, as Doctor Smith observes, "*rather an unnatural class*." Most persons are shocked to see Citrus, the orange, in this class, and not in the *Icosandria* class; for Linnæus describes it of the class XVIII. Polyadelphia, order III. *Icosandria*. Now in our *Reformed Sexual System*, it comes under class XIII. Polyandria, order *Icosandria*, in juxta-position with other edible fruits, in the subdivision Polyadelphia.

VII.—Class V. Pentandria, a very numerous class, is subdivided by Syngenesia, and so formed into two classes by Linnæus, the latter of which, however, as containing an an order *Monogamia,* is not therefore altogether a *natural class.* We obviate this by making *Syngenesia* an order, and the subdivision *Polygamia* to contain the natural tribe of *compound flowers;* whilst, under another subdivision, *Monogamia,* many plants, not having compound flowers, arrange themselves.

VIII. Against GYNANDRIA, which Doctor Smith calls "an odd and miscellaneous class," there lies the same objection, as we observed above, as against the class DIADELPHIA, the numerical names of Classes being applied to Orders. In our scheme, class II. DIANDRIA, has an order *Gynandria,* which contains the *natural tribe* of ORCHISES; and thus the mind is delighted to see a *natural assemblage* embraced in an order, if not in a class. The separation of the remainder cannot be regretted, as not possessing amongst each other the smallest affinity.

IX. MONŒCIA is a miscellaneous class, and borrows the names of its secondary divisions from most of the other classes, as *Monandria, Diandria,* &c., nay even from *Monadelphia, Syngenesia,* and *Gynandria;* for all these become, in Linnæus's *Sexual System,* orders. In our scheme, class TRIANDRIA, order *Monœcia,* contains mostly grasses: hence we retain this *natural assemblage* in the same class *at least,* if not in the same order.

X. DIŒCIA. The same remarks apply here, as in MONŒCIA.

XI. POLYGAMIA subdivides the classes *Monœcia* and *Diœcia;* therefore in the logic of science it is in reality an order.

"Pascitur in vivis livor, post fata quiescit,
Tum suis ex meritis cuique tuetur honos." LINN.

SOME apology is certainly necessary, after any endeavour to reform so celebrated and established a system as the Sexual System of the illustrious Linnæus. Many alterations in this system have been attempted. The enlightened pupil of Linnæus, Thunberg, abolished the classes XX. *Gynandria,* XXI. *Monœcia,* XXII. *Diœcia,* and

XXIII. *Po-*

XXIII. *Polygamia*. Gmelin, professor at Göttingen, to the alterations introduced by Thunberg, in publishing a new edition of Linnæus's Systema Naturæ, added the abolition of class XII. *Icosandria*; and the no less celebrated Doctor Smith, preserving the rest of the system entire, has abolished order V. *Monogamia*, in class IX. *Syngenesia*, and class XIII. *Polygamia*. "To his class Polygamia," says Doctor Smith, "many students of tropical plants *justly* objected in his lifetime, and he, as well as his son, listened to their observations." Dr. Withering, in his Arrangement of British Plants, has followed the system of Gmelin. Professor Martyn, speaking of the changes introduced by Schreber, in his new edition of Linnæus's Genera Plantarum, says, that his reduction of class XX. *Gynandria*, appears "*reasonable*," yet the singularity of the order *Diandria* surely demanded a separate place to itself. But when he comes to mention the incorporation by Gmelin of the class *Icosandria* into the *Polyandria*, he declares this change to be "*abominable*."

I am aware, that venturing to reform in such a degree the Sexual System, as I have done, will bring upon me, with some, much severe reproach. I am conscious, indeed, as well as others, that the credit of the *Sexual System* of Linnæus, as an *invention*, surpasses all power of praise, and hence has found enthusiastic admirers; and with timid hands I have ventured to take to pieces the *superstructure* he raised, and build up from the *old materials*, which I have *carefully* and *religiously* preserved, a NEW EDIFICE, suited to modern improvement and convenience; hoping, however, that those who may, hereafter, publish the works of Linnæus, will edite the Sexual System as delivered by himself, and not bring forward, in the works said to be those of Linnæus, what he never either thought or wrote. For a full defence of the *Reformed Sexual System*, vide my "Practical Botany, being a New Illustration of the Genera of Plants, with Dissections of each Genus," where this subject has been particularly considered and discussed.

In a word, as by system is only meant a plan to *facilitate* the acquirement of the knowledge of plants, the more easy this is contrived to accomplish the proposed end, the better such a system will be accounted; and I have endeavoured so to contrive this, that I hope no longer any very great obstacles can arise in the way of the student, and that this will plead my excuse with a discerning and indulgent public for venturing to step out of the beaten path, to attempt the *reformation* of a system which has conferred immortal honour upon the inventor, and received the general plaudits and admiration of the learned throughout Europe. It appeared to me more advisable to *reform* the whole, than to make any *partial amendments*; either to adopt the system as delivered to us by Linnæus, or to have the present system, as erected out of the materials of the old; a *system* which I hope may not moulder, like the other systems *, into the sand of which they were composed, but resemble the youthful Phœnix arising from the ashes of its parent; or, as a rock in the midst of the ocean, may remain until "the wreck of matter and the crush of worlds."

It is certainly a great satisfaction for me to find, that although the learned and venerable Professor Martyn has long openly disapproved of the changes made in the Sexual System by the several reformers, yet he writes to me—

Extract of a Letter to Dr. Thornton from the Reverend Mr. Martyn—

"I by no means *disapprove* of *your new* attempt to render the Sexual System, by the manner in which you have done it, an *easier medium* of attaining *a knowledge of plants*; and have been long convinced in my own mind, that we strive in vain to unite a *natural* with an *artificial arrangement*. Upon *your plan*, I see *no impropriety* in bringing the ORCHIDEÆ into the *second class*: nor can I even *object* to your *altering*, as you have done, the separated classes of Linnæus, ICOSANDRIA and POLYANDRIA. Your *method* is ably considered throughout; for along with *you* I hold our great master's system as *sacred*, and can never approve of

* Not less than fifty-two systems of Botany have been published, several of them of very considerable merit, but not *practically* good; hence most of them are now forgotten.

those

those *greater alterations*" (he might have said *mutilations*) "which some of his pupils have made,—so differently is to be estimated the conduct of persons engaged in the same object."

The rev. Doctor Milne, the learned author of "*A Botanical Dictionary*," writes to me—

Extract of a Letter to Dr. Thornton from the Reverend Dr. Colin Milne—

"Your *Reformed Scheme* of the *Linnæan System* has my *entire approbation.* It possesses all the admirable and elegant simplicity of that of Rivinus, which has always been a great favourite with me, from the steady adherence of the author to the principles of his method, and is eminently adapted for *practice.* Your remarks respecting the *Sexual System* are truly excellent; your *New Illustration* admirable."

Doctor Shaw, of the British Museum, a gentleman not less eminent as a botanist than a naturalist, declares "that he believes, had Linnæus been alive, the *Reformed Sexual System* would be that which he himself would have instantly adopted."

Similar are the flattering opinions also of several other *distinguished botanists,* who have expressed their approval of the *Reformed Sexual System.* But with extreme diffidence I submit it to the judgment of the world.

LII. *Account of the Manufactures carried on at Bangalore, and the Processes employed by the Natives in Dyeing Silk and Cotton*.*

Bangalore, or *Bangalura,* was founded by Hyder, and during the judicious government of that prince became a place of importance. Its trade was then great, and its manufactures numerous. Tippoo began its misfortunes by prohibiting the trade with the dominions of Arcot and Hyderábád, because he detested the powers governing both

* From Buchannan's *Journey through the Mysore,* &c.

countries.

countries. He then sent large quantities of goods, which he forced the merchants to take at a high rate. These oppressions had greatly injured the place; but it was still populous, and many individuals were rich, when lord Cornwallis arrived before it, with his army in great distress from want of provisions. This reduced him to the necessity of giving the assault immediately, and the town was of course plundered. The rich inhabitants had previously removed their most valuable effects into the fort; but these too fell a prey to the invaders, when that citadel also was taken by storm. After the English left the place, Tippoo encouraged the inhabitants to come back, and by promises allured them to collect together the wrecks of their fortunes, from the different places to which these had been conveyed. No sooner had he effected this, than, under pretence of their having been friendly to the English, he surrounded the place with troops, and fleeced the inhabitants, till even the women were obliged to part with their most trifling ornaments. He then kept them shut up within a hedge, which surrounded the town at the distance of a *coss*, till the advance of the army under general Harris made the guard withdraw. The inhabitants, not knowing whom to trust, immediately dispersed, and for some months the place continued deserted. The people, however, are now flocking to it from all quarters; and although there are few rich individuals, trade and manufactures increase apace; and the imports and exports are estimated already to amount to one fourth of what they were in its most flourishing state. The manufacturers and petty traders are still very distrustful and timid; but the merchants, many of whom have been at Madras, and are acquainted with British policy, seem to have the utmost confidence in the protection of our government.

The trade of the country not having been yet opened a year since the inhabitants had deserted the place, no proper estimate can be formed of the quantity of exports and imports; but it is on the increase every month, and is now about one fourth of the quantity that was exported and imported in the most flourishing time of Hyder's government. The son of the person who had then charge of the custom-

house

house states the following particulars of the trade at that period. In one year there were imported 1500 bullock-loads of cotton wool; 50 bullock-loads of cotton thread; 230 bullock-loads of raw silk; 7000 bullock-loads of salt; foreign goods from Madras 300 bullock-loads. At the same time were exported of betel-nut 4000 bullock-loads, and of pepper 400 bullock-loads.

From the quantity of the raw materials some estimate may be formed of the extent of the manufactures: 1500 bullock-loads of cotton wool, and 50 of cotton thread, make rather more than 5100 hundred weight, worth about 8160*l.*, and 230 bullock-loads of raw silk make 47,437½lbs., worth about 27,000*l.*

The cloths here being entirely for country use, and never having been exported to Europe, are made of different sizes, to adapt them to the dress of the natives; and the Hindus seldom use tailors, but wrap round their bodies the cloth, as it comes from the weaver.

1. The cloth which the women wrap round their haunches, and then throw over their heads and shoulders like a veil, is from 14 to 17 cubits long, and from 2 to 2½ cubits wide. It is called *shiray*.

2. If these cloths are for the use of girls, they are called *kirigay*; and are from 9 to 12 cubits long, and from 1¼ to 1¾ cubit broad.

3. The little jacket which the women at this place wear, is made up in pieces containing 12 jackets, and called *cupissa tan*. These are 14½ cubits long, and two cubits or two cubits and a nail broad.

4. Men wrap round them a cloth called *dotra*, which is from 10 to 12 cubits long, and from 2¼ to 2½ cubits broad.

5. The wrappers of boys, called *bucha khana*, are 6 or 7 cubits long, and 1¼ or 1¾ cubit broad.

6. Cloth for wrapping round the head and shoulders of men, like shawls, is named *shalnama*; and is 6 cubits long, and 2½ broad. Smaller ones are made for children.

7. *Paggoo*, or turban pieces, are from 30 to 60 cubits long, and ¾ of a cubit broad.

Having assembled the different kinds of weavers, I took

from

from them the following account of their various manufactures:

The *puttuegars*, or silk-weavers, make cloth of a very rich strong fabric. The patterns for the first five kinds of dresses are similar to each other, but are very much varied by the different colours employed, and the different figures woven in the cloth; for they rarely consist of plain work. Each pattern has an appropriate name, and, for the common sale, is wrought of three different degrees of fineness. If any person chooses to commission them, whatever parts of the pattern he likes may be wrought in gold thread; but, as this greatly enhances the value, such cloths are never wrought except when commissioned The fabric of the sixth kind of dress is also strong and rich; but the figures resemble those on the shawls of *Cashemire.*

The turbans are made of a thin fabric of cotton and silk.

The *puttuegars* make also, in a variety of figured patterns, the first three kinds of dresses of silk and cotton.

They also make *sada putaynshina*, or thin white muslins with silk borders. These are either plain, or dotted in the loom with silk or cotton thread; and are frequently ornamented with gold and silver. This is an elegant manufacture, and is fitted for the first five kinds of dresses.

Plain green muslin with silk borders for the first three kinds of dresses is also made by the *puttuegars*; but not of so fine a quality as that made by the *devangas*, as will be afterwards mentioned.

The same may be said of the coloured striped muslin with silk borders, called *dutari huvina*, which is used also entirely for female dresses, and is wrought of various patterns.

The *puttuegars* dye much of their own silk; and they gave me the following account of their processes:

The silk is thus prepared for dyeing, the operation being performed sometimes on the raw material, and sometimes on the thread. Take 5 *seers* ($3\frac{223}{1000}$lb.) of silk, 3 *seers* ($1\frac{934}{1000}$lb.) of *soulu* or impure soda, and $1\frac{1}{2}$ ($0\frac{967}{1000}$lb.) of quick-lime; mix the soda and lime with 4 or 5 *seers*, or about 308 cubical inches, of water, and boil them for half an an hour. One half of the boiling ley is poured into a wide-mouthed pot, and one half of the silk is immediately put into it suspended on a stick. If it be not sufficiently wet, it will not take the colour; and if it be allowed to remain any length of time, the silk is destroyed. The rest of the silk is now dipt into the remaining ley, then washed in cold water, and dried in the sun.

If a white silk be wanted, take three *seers* ($1\frac{934}{1000}$lb.) of prepared silk, 3 *seers* of *soulu* or impure soda, 1 *dudu* weight ($6\frac{471}{1000}$ drachms avoirdupois) of indigo, and 18 *seers* (about 1235 cubical inches) of water; boil them for about two hours. Then wash the boiled silk in some hot water, and dry it. In this operation much care is necessary, as by too much of the *soda* the silk is apt to be spoiled; and if it be boiled too short a time it will not be sufficiently white. The workmen judge of the time by taking up a few threads on a stick, and putting on them a drop of cold water: whenever they appear of a proper colour, the silk must be immediately washed in clean water.

To give the red dye with *lac*, take $1\frac{1}{2}$ *maund* ($38\frac{67}{100}$lb.) of *lac*, cleared from the sticks, $1\frac{1}{2}$ *seer* ($0\frac{967}{1000}$lb.) of *lodu* bark, $1\frac{1}{2}$ *seer* of *suja cara*, or soda, and two *dudus* weight ($12\frac{942}{1000}$ drachms) of turmeric. Put them into a narrow-mouthed pot, capable of holding 80 *seers* (5492 cubical inches), with 40 *seers* (2746 cubical inches) of water, and boil them four hours; then decant the liquor, which is impregnated with the dye; and having to the same materials added 20 *seers* (1373 cubical inches) more of water, boil them again for three hours; decant this liquor into the former, and then, for three hours, boil the materials a third time, with 10 *seers* ($686\frac{1}{2}$ cubical inches) of water. Decant this also into the two former, and preserve, in a covered pot, the whole liquor for eight days. At the end of this period the workman judges how much silk his materials will dye. If the *lac* has been good, it will dye 5 *seers* ($3\frac{223}{1000}$lb.); but if it be poor, it will not dye more than $3\frac{1}{2}$ *seers* ($2\frac{256}{1000}$lb). For 5 *seers* of silk take 20 *seers* ($12\frac{39}{100}$lb.) of tamarinds, and for two days infuse them in 18 *seers* (1235 cubical inches) of water. Then strain the infusion through a thick cloth, till about 5 *seers* (343 cubical inches) of clear infusion are procured. Put this into a large open pot with the silk, and warm them until they be rather too hot for the hand. Take out the silk, and pour into the warm infusion of tamarinds three quarters of the decoction of *lac*, strained through a cloth. Then return the silk, and boil it for three hours. After this, examine the silk. If it have received a proper colour, nothing more is added; but if the colour be not deep enough, the remaining decoction is strained, and added by degrees till the colour is completed. The pot must then be taken from the fire, and from time to time this silk must be examined with a stick. If the colour be blackish, some tamarind infusion must be added. If too light, it must be again boiled with some more of the decoction of *lac*: when cool, the silk must be washed in cold tank water, and dried in the shade. This is the finest red dye in use here: in some places cochineal is used; but it is much more expensive. The *lac* dye is not discharged by washing.

The *puttuegars* dye their silk of a pale orange colour, with the *capili podi*, or dust collected from the fruit of the *rotleria tinctoria*. For 5 *seers* of silk ($3\frac{223}{1000}$lb.) prepared for dyeing, take three *seers* ($1\frac{934}{1000}$lb.) of *capili* reduced to a fine powder, and sifted through a cloth; 4 *dudus* ($1\frac{619}{1000}$oz.) weight of *sesamum* oil; $1\frac{1}{4}$ *seer* ($12\frac{892}{1000}$ oz.) of powdered *soulu*, or soda; 1 *seer* ($10\frac{314}{1000}$ oz.) of *suja cara*, another kind of soda, and three *dudus* weight ($1\frac{213}{1000}$ oz.) of alum;—and put them in a pot. Then take $2\frac{1}{2}$ *seers* ($1\frac{616}{1000}$lb.) of *soulu*, and boil it in about $3\frac{1}{2}$ *seers* (240 cubical inches) of water, till it be dissolved. With this solution moisten the powders that are in the pot, and form them into a paste, which is to be divided in three equal parts. Put one of these portions in the remaining solution of *soulu*, and heat it, but not so as to boil. Then put in the silk, prepared as before, and wet it thoroughly. Take it out, and add a little water, and a second portion of the paste. This being dissolved, soak in it the silk as before. Then put in the remainder of the paste with 18 *seers* (1235 cubical inches) of water; and, replacing the silk, boil

boil it for two hours. Then cool it, and having washed it in the tank, dry it either in the shade or sun, indifferently. This is a pretty colour, fixes well, and is cheaper than that of the *lac*.

To dye their silk yellow, the *puttuegars* use turmeric. For 3 *seers* ($1\frac{934}{1000}$ lb.) of silk, take 4 *seers* ($2\frac{578}{1000}$ lb.) of turmeric, powdered and sifted: make it into a paste with water, adding 4 *dudus* weight ($1\frac{619}{1000}$ oz.) of *sesamum* oil. Divide the paste into three portions, one of which is to be put into a pot with 8 *seers* (549 cubical inches) of warm water. In this immerse the silk prepared as before, and continue the operation exactly in the same manner as with the *capili* paste. It must, however, be dried in the shade, and the colour then stands very well; which it would not do, were it dried in the sun.

The *puttuegars* give their yellow silk to the *niligaru*, who dye it with indigo. It is then washed by the *puttuegars* in the infusion of tamarinds, and afterwards is of a fine green colour; which, if it be dried in the shade, is tolerably well fixed.

The *niligaru* dye all the other colours; such as light and dark blue, sky blue, and purple. The silk is never dyed in the piece.

The red and orange-coloured silks are mostly in demand.

Some weavers called *cuttery*, who pretend to be of the *Kshatriya* cast, manufacture exactly the same kinds of goods as the *puttuegars*.

The whole of the demand for these goods, according to the account of the manufacturers, is in the country formerly belonging to Tippoo: Seringapatam, Gubi, Nagara, Chatrakal, and Chin'-ráyapattana, are the principal marts. When the goods are in much demand, it is customary for the merchant to advance one half, or even the whole, of the price of the goods which he commissions; but when the demand is small, the manufacturers borrow money from the bankers at two per cent. a month, and make goods, which they sell to the merchants of the place. They never carry them to the public market. The silk is all imported in the raw state by the merchants of this place.

The

The master weavers keep from two to five servants, who are paid by the piece. Workmen that are employed on cotton cloth with silk borders make daily about a *fanam*, or nearly 8*d*. Those who work in cloth consisting of silk entirely, make rather less, or from $\frac{10}{12}$ ($6\frac{3}{4}$ pence) to $\frac{6}{8}$ (6 pence) of a *fanam*, according to the fineness of the work. It is not usual for weavers of any kind in this country, except those of the *Wh lliaru* cast, to employ part of their time in agriculture; but many persons of casts that ought to be weavers are in fact farmers. The *cuttery* are more affluent than the *puttuegars*, and these again are more wealthy than any other kind of weavers.

Another kind of manufacture is coloured cotton cloths of a thin texture, and with silk borders. It resembles one of the manufactures of the *puttuegars*, called *dutari huvina*, but is coarser. It is entirely fitted for the different kinds of female dress; and is made of various lengths, from eight to sixteen cubits, according to the age and size of the wearers. In this way three different kinds of weavers are employed; the *shaynagaru*, the *canara devangas*, and the *teliga devangas*. These people buy the thread at the public markets. The red thread comes mostly from Advany, Balahari, and other places near the Krishna river: the various shades of blue are dyed by the *niligaru*.

The weavers themselves dye part of the red thread with the *muddi* root, which is that of two species of *morinda*; the *citrifolia* of Linnæus, and the *ternifolia* described in my manuscripts. The colour is dark, but stands washing in cold water. In boiling, it fades. The following is the process used:—The thread must be divided into parcels each weighing one *seer* ($10\frac{314}{1000}$ oz.). For each parcel take $\frac{1}{4}$ *seer* ($2\frac{578}{1000}$ oz.) of powdered *soulu*, and dissolve it in 4 *seers* ($274\frac{6}{10}$ cubical inches) of water. Put into the solution $\frac{1}{4}$ *seer* of sheep's dung, and $\frac{1}{2}$ *seer* ($5\frac{157}{1000}$ oz.) of *sesamum* oil, and with the hand mix the whole well. Wet the parcel of thread in this mixture thoroughly, and let it hang up in the house all night to dry. Next day expose it on a rock to the sun; and during the four or five following days it must be dipped nine times in a solution of $\frac{1}{8}$ *seer* ($1\frac{289}{1000}$ oz.) of of *soulu*, in one *seer* (a little more than 68 cubical inches) of water. Between each immersion it must be dried in the sun. After this, the thread remains in the house ten days; it is then taken to a tank, and well washed by beating it on a stone, as is the usual practice of this country. When it has been dried, soak each parcel in a solution of two *pagodas* weight ($1\frac{937}{1000}$ drachm) of alum in one *seer* of water, and then dry it again. Infuse one *seer* measure ($74\frac{8}{10}$ cubical inches) of powdered bark of *muddi* root, in 4 *seers* of cold water, and in this soak one parcel of thread; then throw into a large pot the whole of the parcels that have been treated in a similar manner. Next day take them to a tank, beat them as usual, so as to wash them clean, and then dry them again in fresh infusions of *muddi* powder. This must be daily repeated, till the colour is sufficiently strong; which, if the bark be from the roots of an old tree, will require six infusions; but nine infusions of bark from a young plant will be requisite.

These weavers dye cotton thread green in the following manner: They send it to the *niligaru*, who dye it *mavi*, or a kind of sky blue. The weavers then wash it, and put it into two *seers* ($137\frac{1}{4}$ cubical inches) of water, containing $\frac{1}{2}$ *seer* ($5\frac{157}{1000}$ oz.) of powdered turmeric, five *myrobalans* powdered, and the juice of ten limes. Here the thread is kept four hours, and the operation is finished. The colour is a fine green, but very perishable. It is said that the *niligaru* have the power of fixing it; but they keep their art a profound secret.

The *devangas* dye cotton cloth of a fine red colour resembling that of the pomegranate flower, and called *gulenari*. This is done with the *cossumba*, or flowers of the *carthamus tinctorius*. The same gives another red colour, called simply *cossumba*. Neither of the colours are well fixed. The demand for the *cossumba* dye being much greater than the country can supply, much of it is imported. This is always done in the form of powder, which powder is adulterated with the flowers of the *yecada*, or *asclepias gigantea*; on which account it is cheaper than the flowers produced in the neighbourhood. The powder is made by

drying

drying the flowers in the sun, and beating them in a mortar, and will not keep longer than one year; the flowers, if carefully packed in sacks and well pressed, may be preserved for five years.

The *cossumba* colour is given in the following manner:—Take 15 *sultany seers* ($9\frac{1}{10}$lb.) of pure *cossumba* powder, and put it on a cloth strainer. Clean it by pouring on water, and rubbing it with the hand, till the water runs through clear. The *cossumba* is then to be spread on a blanket, and mixed with 15 *dudus* weight ($6\frac{067}{1000}$ oz.) of *suja cara*, and an equal weight of *soulu*, both powdered. They are gathered together in the centre of the blanket, and trodden for an hour by a workman's feet. They are then put upon a cloth strainer, supported as usual by sticks at the corners; and water is poured on them until it passes through the strainer without colour. This water is divided into three portions: that which came first, that which came in the middle of the operation, and that which came last; the first being of the strongest quality. Then take 60 good limes, or 100 bad ones, cut each into two pieces, beat them in a mortar, and strain their juice, through a cloth, into the pot containing the dye of the first quality. Then put a little water to the skins, beat them again, and strain off the water into the pot containing the second quality of the dye. Then add more water to the lime-skins, and having beat them, strain it into the dye of the worst quality. The cloth to be dyed, having been well washed, is put into this last pot, and boiled for an hour and a half. It is then dried in the sun, and dipped into the second quality of dye, but not boiled. It is then dried again, and afterwards kept half an hour in the dye of the first quality. At the end of this time, should the colour not be sufficiently strong, the cloth must be boiled in the dye. It is then dried, and the operation is finished. The cloth commonly dyed is for turbans; and a turban 60 cubits long requires 15 *seers* of *cossumba*.

The only difference, in the process for dyeing the *gulenari*, is, that to the pot of the first quality, as prepared for dyeing *cossumba*, is added half a *seer* ($34\frac{1}{3}$ cubical inches) of a decoction of *tundu* flowers (*cedrella toona* Roxb. MSS.) prepared

prepared as follows:—Take 24 *dudus* weight ($9\frac{7207}{10000}$ oz.) of dried *tundu* flowers, beat them in a mortar, and boil them for half an hour in 2 *seers* ($137\frac{1}{4}$ cubical inches) of water. Then strain the decoction through a cloth for use.

The *devangas* frequently make a very dark blue, which they call black, by means of the bark of the *swamy*, or *Sweitenia febrifuga* Roxb. MSS. This colour is cheap; but its intensity leaves it on the first washing; whereas the very deep blue imparted by repeated immersions in indigo, and approaching near to black, is very high-priced, and durable. Ir is the colour most esteemed by the natives, who call it black. The *devangas* take cotton thread or cloth that has been dyed blue by the *niligaru* with indigo, and sprinkle it with a decoction of *swamy* bark. This is made by powdering the dry bark, and boiling it for an hour and a half. While the cloth or thread is sprinkled, it must be moved with the hand, so as to imbibe the colour equally in every part.

These weavers say, that they obtain advances from the merchants, and borrow money from the bankers, exactly on the same terms as the *puttuegaru*. They sell their goods to merchants, or to private customers, and never carry them to the public markets. None of them follow any other business than that of weaving, and many are in good circumstances. The *shaynagaru* are the richest. The servants are paid by the piece, and make about 20 *fanams* (13*s*. $5\frac{1}{4}$*d*.) a month.

A kind of weavers called *bily mugga* by the Mussulmans, but in fact consisting of the casts called *Shaynagaru*, *Padma-shalay*, and *Samay-shalay*, weave many kinds of white muslins.

I. *Dutary*, striped and chequered muslins, called in Bengal *durias*. They are from 28 to 32 cubits long, and from 2 to $1\frac{3}{4}$ broad; and, if commissioned, flowers of cotton, or gold thread, are frequently woven in them.

II. *Soda shilla*, or plain muslin, like the *mulmuls* of Bengal. These are from 26 to 32 cubits in length, and $1\frac{1}{2}$ to 2 cubits in breadth.

III. *Asto cumbi*, a cloth like the *cossahs* of Bengal. They have

have sometimes striped or silver borders, and are always ornamented with silver at the ends. They are used by men to wrap round their shoulders.

IV. Turbans from 30 to 100 cubits in length, and from ½ to 1 cubit in width, and ornamented with silver and gold thread at the ends.

Each kind of cloth has several patterns, and each pattern is of three degrees of fineness, which, in the technical language of European merchants in India, are marked by the letters A. B. and C.

These people say, that they receive advances from the merchants, and borrow money from the bankers, in the same manner as the *puttuegars* do. Where the cloth is made on the weaver's own account, it is sold partly to merchants and partly in the weekly markets. When a weaver receives advances, he cannot sell any cloth till his contract be fulfilled. Among the *Padma shalay* there are few servants employed; but all the males of a family live together, and work in the same house, very seldom engaging themselves to work out for hire. The *Samay shalay* keep more servants. The people of these two classes live better than those employed in agriculture. A man at fine work can gain a *fanam* (rather more than 8*d.*) a day. At coarse work a man cannot make above 3*d.* a day. The servants live in their own houses; but, although paid by the piece, they are generally in debt to their masters, and are consequently bound in the same manner as the servants of the farmers. This circumstance is applicable to journeymen weavers of every kind.

The *togotaru* are a class of weavers that make a coarse, thick, white cotton cloth with red borders, which among the poorer class of inhabitants is used as the common waist-cloths of all ages and sexes. This kind of cloth goes by the name of the manufacturers who weave it, and is also of three degrees of fineness.

The same people make *romals*, or handkerchiefs with red borders, from three to five cubits square, that are commonly used by the poor as a head-dress. The pieces are about twenty cubits long, and are divided into a greater or smaller number

number of handkerchiefs, according to their width. They are also of three degrees of fineness.

The weavers of this class are poor, and say that they cannot afford to make the cloth on their own account. They in general receive the thread from the women in the neighbourhood, and work it up into cloth for hire. For weaving a piece that is worth 8 *fanams,* or 5*s.* 4½*d.,* they get 2½ *fanams,* or 1*s.* 8*d.* This occupies a workman four or five days; so that his daily gains are from four to five pence. They never cultivate the ground.

The *whalliaru* make a coarse, white, strong cloth called *parcalla.* It serves the poorer male inhabitants, throughout the country, as a covering for the upper parts of their bodies. The pieces are from 24 to 28 cubits long, and from 1½ to 1¾ broad, and as usual of three different degrees of fineness. Weavers of this kind live scattered in the villages, and frequently hire themselves out as day-labourers to farmers, or other persons who will give them employment.

At the weekly markets the cotton wool is bought up, in small quantities, by the poor women of all casts, except the Bráhmans; for these never spin, nor do their husbands ever plough the soil. The women of all other casts spin, and at the weekly markets sell to the weavers the thread that is not wanted for family use. The thread that is brought from Balahari, and other places toward the Krishna, is much coarser than that which the women here spin.

Such is the account given me by the various weavers; but the cloth agents, who are all of a cast called *Nagarit,* say, that it is not customary to make advances for goods of an ordinary kind, unless the demand from a distance be very great. When this is the case, or when goods of an uncommonly high price are wanted, in order to enable the manufacturer to purchase the raw materials, one half of the value is advanced. The credit is for three months, and for this time there is no interest paid; but if the goods are not then delivered, monthly interest is demanded at the rate of ¾ per cent. until the contract is fulfilled. The commission here on the purchase of goods is only two per cent., and the agent is answerable for all the sums advanced to the weavers.

On confronting some of the richer *Shaynagaru* with the *Nagarit*, they acknowledged that this statement was true.

The places from whence agents are at present employed to purchase cloths are, Nagara, Chatrakal, Seringapatam, Chin'-ráya-pattana, Sira, Madhugiri, and Devund-hully. A small quantity of cotton and silk cloth for women's jackets goes to the lower Carnatic. This is the account of the *Nagarit*; but I have good reason to think, that a very large quantity of goods, especially of the silk manufacture called *combawutties*, are sent to that country, and are much in request among the women of the rich Bráhmans. The *Nagarit* say, that the merchants, who import cotton, take away silk cloths for the dress of the Bráhmans of both sexes, and also blue and red cotton stuffs; but not in a quantity sufficient to repay the whole cotton. During the former government of the Rája's family, much cloth went from this neighbourhood to Tanjore, Negapatam, and other parts of the southern Carnatic: but since to that period, this commerce has been entirely at a stop.

The Mangalore merchants send hither for every kind of cloth. The dress of that country requires cloth only eight cubits long. The pieces intended for that market have therefore a blank left in their middle. In Hyder's time there was a great exportation of cloth to Calicut: but the troubles in Malabar have put an entire stop to this branch of commerce.

[To be continued.]

LIII. *On the Means of gaining Power in Mechanics.*

To Mr. Tilloch.

SIR,

In reply to the observations and opinion of one of your correspondents in Number cxvii. of your Magazine, relative to the means of gaining mechanical power, I regret having to remark, that instead of the instruction, or satisfactory information, to be expected from an academician, he has only given us antiquated and superfluous objections and conclusions,

conclusions which are quite erroneous and ill founded:—for as to the former, I had pointedly remarked that this novelty (of gaining power) was inadmissible on the established principles of mechanics: and respecting the latter, his arguments, against his own supposition, concerning latent properties in the mechanical powers, of which I never had any idea, by no means proved the impossibility of my discovering what had escaped the superior abilities of other men; because things full as unlikely do sometimes happen. Nor is he more successful in his confident assertion, that the engine I have constructed will, at a certain period, require as much external power to restore it to its former state, as it had apparently gained power beyond the laws of mechanics by its first effort; because this engine is announced as an exception to the rules on which he forms his opinions: and the facts are, that it effects what the established maxims held out as being impossible, and that it does not require such great external power to restore it as he supposes. Hence it may properly be called a novelty. It is, however, what has long been sought, and what great numbers of well-informed geniuses in this and other countries are even now assiduously endeavouring to gain, with the established principles at their fingers' ends.

To give a drawing or particular description of this engine in *any* Magazine, or taking out a patent, would be making it too public, by putting our continental neighbours, who are now most closely confederated against us, and intent on doing us all the injury in their power, on the same footing with ourselves; and perhaps give them advantages, which, being the birth-right of my countrymen, shall, as far as rests with me, be wholly secured to them.

It will therefore suffice, for the immediate gratification of your readers particularly interested in this subject, to state in general terms, that this engine is a singular, though very simple, combination and disposition of the mechanical powers.

I take this opportunity to request the favour of an answer from some of your ingenious and obliging correspondents to the following question:

Is it possible so to dispose a moving power, (suppose a one

one pound weight,) as that, during its progress through a given space, its force shall be constantly increasing, and thereby produce an accelerated motion exceeding that of the moving power at least ten to one?

I am, sir, your very humble servant,

E. VIDAL.

Bracknell, Berks,
March 22, 1808.

LIV. *On the Identity of Silex and Oxygen.* By Mr. HUME, *of Long-Acre, London.*

[Continued from p. 171.]

To Mr. Tilloch.

SIR,

LIME, in its general attractions, and in its capacity to neutralize acids, possesses a very superior energy to clay, and, therefore, is a more decided salifiable base: hence, I shall give it the preference in any example, in which an earth, as belonging to a distinct genus, is to be contrasted with silex, whose habitudes and character are so totally dissimilar.

Nothing is so frequent in nature as an association of two or more contending elements to form one homogeneous compound, or to effect some material purpose: thus, an acid with an alkali, sulphur with a metal, and metals with earths, may be adduced as instances, in which this coexistence of opposites is, perhaps, as essential as two contrary poles to the same magnet, or the negative and positive wires of the Voltaic pile. This proximity is no where more obvious and frequent than in substances composed of silex and lime, in which the caustic pungency and other inherent distinctions of the lime are coerced into such a state of neutrality, as to evade every mode of detection, unless the purity of the compound be destroyed and the lime eliminated.

Silex is not only found alone with lime, but follows it throughout, and in nearly all its modifications; and, generally speaking, this seems the primary cause of the saturated condition of lime in all the native carbonates, such as in chalk,

chalk, marble, lime-stone, and other fossil bodies, of which carbonate of lime forms an extensive portion, so much so, as to render this class of minerals extremely important in all geological discussions.

This singular coincidence has not escaped notice, and many very respectable men have advanced opinions upon the subject. It has, indeed, been supposed, that there is a transmutation of one of these elements into the other, or a graduation of lime into silex; and it is asserted that the recent formation of flint had been perceived, near the surface, in a calcareous mountain, in which, also, animal and vegetable substances were found petrified by the silex; and that rhomboidal crystals were likewise present, passing from the state of carbonate of lime to nearly pure silex.

It is evident, such a theory as this is not tenable, but must be involved in difficulty, since it assumes a case, of all others, the most improbable; for, according to these premises, we are compelled to admit, that the diamond, the oxygen and the lime, that is to say, the real ingredients of the chalk, all contribute to the formation of silex, which is, avowedly, one of the most perfectly indecomposable of elementary bodies, and, certainly, much more so than lime. This conversion of lime into silex is, I presume, quite inconsistent with general facts, and contrary to every object in nature which contains these two ingredients among its constituent principles.

In chalk particularly, which is one of the most plentiful of nature's productions, as well as in all the other carbonates of lime, there is, usually, a very copious assortment of silex, under one shape or another; and this is either so intimately blended as to be hardly perceptible to the sight, and, often, can only be extracted by analysis; or it occurs in the form of sand, gravel, or what, in common terms, are called *flint-stones*. It is necessary to observe, that these stones are chiefly of an obtuse and rounded figure, and never pointed and angular; they are frequently found alone, and, from their appearance and other circumstances, seem to have suffered a diminution of their original bulk, by yielding up a portion, from their surfaces, to the surrounding medium in which they are imbedded.

The degradation of these flint-stones is, likewise, strongly marked by a peculiar opacity, not unlike white glass-enamel or the superior kinds of porcelain; and this forms a well-defined stratum, which covers entirely the whole surface of the stone, penetrating it to a greater or less depth. It cannot be considered as a very forced explanation, to say, that this may, probably, be the very point of contact, where this declension of silex is the commencement of a new modification; and that this terminates eventually in the perfection of a carbonate, or even of the lime itself, of which silex would then be considered as the independent progenitor.

That the blunted and nodular shape of the generality of siliceous stones, is a mark of loss in the primitive mass itself, may be explained by many examples. Thus, even in common experiments, a sharp or crystal-formed piece of any substance, capable of solution in an acid, soon loses its projecting corners, and, as the action of the acid proceeds, gradually becomes less indented, and more smooth or globular. It is rather a gratuitous conclusion, when the convex shape of pebbles, gravel, and all other siliceous stones, is ascribed to attrition: in some instances this argument may appear just, but in the more important cases it is, I conceive, extremely fallacious.

The true nature of clay, or, as it is now generally called, *alumine*, when considered as a primitive earth and simple element, seems very questionable; for, notwithstanding various methods have been employed to obtain it in a state of purity, still this doubt remains. Even, in one of the most celebrated systems of chemistry, after detailing the best mode to accomplish the end, the author adds, that the alumine "will then be *nearly* pure." One of the specific characters, peculiar to clay, tends very much to confirm this suspicion; it is that particular odour, constantly evolving from it, perceptible on all occasions, such as ploughing or stirring up land or garden-ground, and which is familiarly known by the term *earthy smell.*

It

It is true, there is also a *flinty smell,* or what the French call "odeur quartzeuse;" but this arises only when the flint is employed in the act of *collision* with steel, or against some kind of stone containing this scintillating ingredient, the silex; and, on such occasions, I have reason to believe, some new compound is the result, in which the presence of oxygen may be traced to this origin and to no other. The effect of flint upon steel is attended with this singular circumstance, that the particles which fly off are obedient to the loadstone, and consequently must be metallic; but, the metal is now deprived of its lustre and malleability, it is a compound, having imbibed a certain established dose of oxygen, at the expense of the silex, and the necessary caloric from the atmosphere.

If an ore consist chiefly of lime, silex and metal, and, if this metal be saturated with oxygen, the lime and, indeed, the whole compound be tastless and quite insoluble in water, what other inference can be drawn than this—that the silex alone is the ostensible and primary cause, both of the insipidity of the lime and the oxidized condition of the metal? Cases of this nature occur very frequently: the ore, which produces the new metal titanium, is precisely of that species; for it is composed of nearly equal parts of these three ingredients, and nothing more besides.

The quantity of silex in some ores, and in mineral substances containing acids and metallic oxides, is often very great; in others there is scarcely any: but we may occasionally trace it by its effects, and account for its absence from the condition of the ingredients left in the ore. In the following example, the quantity of silex remaining in the compound seems to be inversely as that of the oxygen, as if nearly the whole had been expended, and converted into the oxygen, which is now blended with the metals.

Thus, a specimen of wolfram, analysed by MM. Vauquelin and Hecht, contained in the hundred, 66 parts of tungstic acid, 18 of black oxide of iron, 6·25 black oxide of manganese, and only 1·5 of silex. Here, I would say, the whole of the oxygen had been generated entirely at the expense of the original silex, of which a very little or rather a mere surplus now remains in the ore. This is not only the most obvious source from whence the oxygen could have been derived, but, were there any objection, it must be also noticed that the ore itself had been originally enveloped in silex (gangue quartzeuse), so much so, that what was superfluous and extraneous adhered so closely, that it was with great difficulty these gentlemen could detach it, so as to divide it from the mere ore.

The sulphur, lime and metal, which often constitute the lead-ore, called galena, or sulphuret of lead, are accompanied by silex; and the general neutrality of the whole mixture may be ascribed to this, the common oxidizing element; for, if it be not in the ore itself, it is so very contiguous, that the matrix is, either entirely or in part, composed of this substance.

Magnesia, another of the most abundant of the earths, is never found in a pure state, but, like all others, is either blended with an acid or concealed by means of silex; and whether it be alone or accompanied by alumine, lime, or any other substance, this is invariably the state to which it is reduced. Thus, in asbestos, the magnesia, alumine and lime are sufficiently degraded to be deprived of all external peculiarities, and to shun the usual tests; for, though nearly one half of this mineral is composed of these three bodies, they are more than counterbalanced by the other element.

Potash and soda, which exist in somany situations, and are found to be more abundant than had formerly been suspected, appear to constitute one of the principal ingredients in various mineral bodies, particularly in the more huge masses of matter, in the primitive rocks and mountains, and, probably, in other substances, in which it has hitherto escaped our attention. These are to be considered, not only to be in the same predicament as the earths, but, being possessed of higher powers and more considerable energy as salifiable bases, they furnish more conclusive examples for elucidating this subject, and more openly evince the oxgyenating efficacy of silex. In all substances in which these are found, there is no appearance of an alkali; they have, till very lately, withstood all research; and even their ready solubility in water, the peculiar taste, which, it will be granted, is of the most

most horrid causticity, and other qualities, which characterize these alkalis, are totally suppressed and softened into perfect inactivity.

Here, the same agency continues its operations; for, not merely the earths, the lime, or alumine, and metals, when they occur, but a considerable portion of alkali, amounting in some instances to more than one fifth of the whole, are all reduced into one mass of tasteless inoffensive stone. The leucite, which was analysed by M. Klaproth, contains about 0·22 of potash. 0·23 of alumine, and, 54 of silex; and from this, I confess, I can draw no other inference than,—that the two salifiable bases are deprived of their primitive characters, entirely by the other ingredient.

The proximity of all the earths to silex, and the constant association of this with one or more of the former, is a circumstance too notorious to dwell upon; and, considering the public utility of your pages, it were intrusive to multiply these examples, by bringing forward every case that may be suitable. We might really confide in almost a random selection; for all scientific books are fraught with proofs and analyses, in which silex, oxidized metals, neutral salts, or saturated substances, or, in short, where some modification of oxygen is indelibly impressed.

Even substances that apparently are independent of our globe are chiefly formed of silex; and the meteoric stones, which, in a highly ignited state, have occasionally been projected from the atmosphere upon various and distant places in the world, particularly in Yorkshire, and at Benares in the East Indies, even these contain about half their weight of silex; it has also been universally remarked, that these wonderful productions are always made up of the same elements—of silex, iron, nickel, magnesia, and a small quantity of sulphur; and nearly in the same uniform proportions in all the specimens that have been analysed by other chemists since Mr. Howard, who first published the exact history of the composition.

These stones are, by many very intelligent men, supposed to have been ejected from some volcano in the moon; and though no one has positively asserted it, still the idea has been fostered, and is generally treated with respect. Indeed, I see nothing very romantic in this opinion; and, in all cases, I think, when we are at a loss to explain, we may take the liberty to conjecture. One of the most celebrated chemists of the age, and whose correct knowledge of this and every other subject of science is indisputable, thus expresses himself upon this subject: "The opinion," says M. Vauquelin, "that these stones came from the moon, however extraordinary it may appear, is still, perhaps, the least unreasonable; and if it be true that no direct proofs can be given, it is no less true that no well-founded reasoning can be opposed to it: the wiser way, therefore, is to own frankly, that we are totally ignorant of the origin of these stones, and of the causes which produce them."

[To be continued.]

LV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

The meetings of this learned body, on the 14th and 21st of April, were occupied in reading an account of a shower of meteoric stones at Weston in North America. This account we have been enabled to lay before our readers in the present Number, by the kindness of the honourable Mr. Greville. It is by far the best authenticated and precise account of so singular a phænomenon that has yet been published in any country.

LINNÆAN SOCIETY.

April 5. The right reverend the bishop of Carlisle, V. P. in the chair. A communication from Doctor Smith, the president, was read, entitled "Characters of a new Genus of Mosses called Hookeria, containing eight Species, &c." Some of these species are new, and others have heretofore ranked in the genus Hypnum; from which, however, the learned President pronounces them to be clearly distinguished by their reticulated capsules. These constitute an essential character for this new genus, all the species of which, however, accord in other characteristics. Dr. Smith has named this

this genus after Mr. W. Jackson Hooker, of Norwich, F.L.S., a young naturalist of great promise, the discoverer of *Buxbaumia aphylla*, and author of a work on the Jungermanniæ, which is about to appear.

April 19. The chair was filled this evening by the President, who read a communication of his own, on A new Genus of Liliaceous Plants, which he has called Brodæa, in honour of Mr. Brodie; whose numerous and valuable illustrations of the botany of Scotland give him a just claim to this distinction. This paper contained some admirable remarks upon the difficulty which some plants, and especially the Liliaceæ, present, of distinguishing between the calyx and corolla, and upon the doctrine of Jussieu on this subject. Dr. Smith conjectures that both these organs may be united where one of them seems to be wanted; the external surface performing the functions of the calyx, and the internal those of the corolla.

Some interesting letters from Peter Collinson to Linnæus were also read by the President, which, he stated, were intended for some future publication. One of them related a remarkable instance of hybrid fruit on an apple-tree, produced by the proximity of a tree bearing another kind. The President mentioned a similar fact which had come under his own observation near Norwich. A peach- and nectarine-tree grew close together, and bore sometimes peaches, sometimes nectarines, and sometimes a fruit partly resembling each.

WERNERIAN NATURAL HISTORY SOCIETY.

At the last meeting of the Wernerian Natural History Society, Professor Jameson read an account of a method of constructing and colouring mineralogical maps. We cannot give a satisfactory account of this paper without drawings; we shall therefore only observe, that maps executed according to this plan show distinctly the figure of the cliffs, terraces, acclivities and summits of single mountains, and also the characters of mountain-ranges and mountain-groups: and the colouring affords a true and harmonious representation of the alternation, extent, and relative position of the different rocks that appear at the surface. Professor

Jameson

Jameson at the same time read before the society a series of mineralogical queries,—of which a copy is subjoined,—drawn up with the view of directing the attention of mineralogists to the particular objects pointed out by them.

MINERALOGICAL QUERIES.

England.

1. Does the granite of Cornwall belong to the oldest or the newest granite formation; or do both formations occur in that county?

2. Is the shorl rock of Cornwall disposed in an unconformable and overlying position in regard to the older primitive rocks?—If this be its position, on what rock or rocks does it rest, and what are its other geognostic relations?

3. Does the serpentine of Cornwall belong to the first or second serpentine formation; and what are the imbedded and venigenous fossils it contains?

4. What are the characters of the different metalliferous venigenous formations in Cornwall?—Are any of them identical with those described by Werner*, Mohs†, Friesleben‡, Jameson§, and others?

5. Do the inclined slaty strata in the vicinity of Plymouth belong to the transition class of rocks?

6. Does the upper part of the mountain of Cader-Idris in Wales belong to the newest flætr trap formation?

7. Are not the mountains in Cumberland principally composed of transition rocks partially covered with the newest flætr trap formation?

8. Is not the porphyry of Cumberland a variety of clinkstone porphyry?

9. Does the gypsum of Cumberland belong to the first or second flætr gypsum formation?

Scotland.

1. Does the syenitic greenstone of Fassnet Burn, in East Lothian,

* Neue Theorie von der entstehung der gänge, von A. G. Werner, 1791.

† Beschreibung des Gruben Gebäudes Himmelsfürst, von F. Mohs. 1804.

‡ Mineralog Bemerkungen bei gelegenheit einer Reise durch den Merkwürdigsten Theil des Harzgeberges, von Friesleben. 1795.

§ Mineralogical Description of Dumfriesshire;—and Elements of Geognosy.

Lothian, belong to the transition rocks, or the newest flætr trap formation?

2. Does clay-stone occur in beds or veins in the coal-fields of the Lothians?

3. What are the geognostic characters and relations of the porphyritic rock of the Ochil hills?

4. Is Inch Keith, in the Frith of Forth, entirely composed of rocks belonging to the independent coal formation?

5. Are the geognostic relations of the porphyry slate or clink-stone porphyry of East Lothian the same as in other countries?

6. What are the geognostic relations of the clay-stone, compact felspar, and striped jasper of the Pentland hills?

7. Are the upper parts of the Lommonds in Fifeshire, of Tinto in Lanarkshire, and of the Eilden hills in Roxburghshire, composed of rocks belonging to the newest flætr trap formation?

8. What is the extent and mode of distribution of the sienite of Galloway?

9. Does the Craig of Ailsa in the Firth of Clyde, and the Bass rock in the Frith of Forth, belong to the newest flætr trap formation?

10. Does the pitchstone of Ardnamurchan belong to the newest flætr trap formation?

11. Is the granular quartz in the islands of Jura and Isla subordinate to mica slate, or does it constitute a distinct formation?

12. Are the Cullin mountains in the island of Skye composed of rocks belonging to the newest flætr trap and second porphyry formations?

13. What are the geognostic characters and relations of the Scure Eigg in the isle of Egg, one of the Hebrides?

14. Of what rock is the isle of Staffa composed; and what are its geognostic characters and relations?

15. Is the porphyry of the isle of Rasay porphyry slate?

16. What are the geognostic relations of the tremolite of Glen-Elg, in Inverness-shire?

17. Does the upper part of Ben-Nevis belong to the second

second porphyry formation:—and if this be the case, on what does the porphyry rest?

18. Does the porphyry of the Brauer near Blair in Athol belong to the second porphyry formation?

19. Does the granitic rock in the vicinity of Aberdeen belong to the granite or sienite formations?

20. Does the sandstone of the Shetland islands belong to the independent coal formation, or to any of the formations described by Werner?

21. In what species of mineral repository are the ores of Sandlodge in Shetland contained; and what are the oryctognostic and geognostic characters and relations of these ores?

22. Does the clay-stone of Papa Stour, one of the Shetlands, belong to the new flætr trap or coal formations?

23. Does the serpentine of the islands of Renst and Fetlar belong to the first or second serpentine formations?

LVI. *Intelligence and Miscellaneous Articles.*

THE MAMMOTH.

With the present Number of the Philosophical Magazine we have given a representation of the skeleton of this stupendous animal. Having, in our previous volumes, given many particulars respecting the mammoth, we shall not here repeat them, but content ourselves with the following references to the volumes in which they may be found:

Vol. xiii. p. 206; vol. xiv. p. 162. 228. 332; vol. xvi. p. 154. 170; vol. xx. p. 100; vol. xxix. p. 141.

See also Plate V. vol. xiv.

The committee to whose management the members of the association for the discovery of the interior parts of Africa have intrusted the direction of their affairs, has engaged another traveller in their service; a person now in this country, highly accomplished for such a purpose, possessed of of a strong vigorous constitution, great ardour in the pursuit of knowledge, with a temper of mind ready to submit to great privations, and prepared to accommodate himself to the various trying situations to which the prejudices of the inhabitants of that part of the world may possibly expose him. Great expectations are formed of his success.

Mr. PARKINSON is expected to publish the second volume of Organic Remains of a former World, in the beginning of June. It will contain twenty plates, with the representations of nearly two hundred fossils of the remains of zoophytes, coloured from nature; among which are several proving the existence in the former world of at least twenty species of the encrinus. It must be gratifying to the admirers of this branch of natural history, to learn that great numbers of these fossils are to be found in different parts of this island.

LECTURES.

Mr. BROOKES's Summer Course of Lectures on Anatomy, Physiology, and Surgery, will commence on Saturday, the 11th of June, at Seven o'clock in the Morning, at the Theatre of Anatomy, Blenheim-Street, Great Marlborough-Street.

In these Lectures the Structure of the Human Body will be demonstrated on recent subjects, and further illustrated by preparations, and the function of the different organs will be explained.

The surgical operations are so performed, and every part of surgery so elucidated, as may best tend to complete the operating surgeon.

The art of injecting, and of making anatomical preparations, will be taught practically.

Gentlemen zealous in the pursuit of zoology, will meet with uncommon opportunities of prosecuting their researches in comparative anatomy.

Surgeons in the army and navy may be assisted in renewing their anatomical kowledge, and every possible attention will be paid to their accommodation as well as instruction.

Anatomical conversaziones will be held weekly, when the different subjects treated of will be discussed familiarly, and the

the students' views forwarded.—To these none but pupils can be admitted.

Spacious apartments, thoroughly ventilated, and replete with every convenience, are open from five o'clock in the morning, for the purposes of dissecting and injecting, where Mr. Brookes attends to direct the students, and demonstrate the various parts as they appear on dissection.

An extensive museum, containing preparations illustrative of every part of the human body and its diseases, appertains to this Theatre, to which students will have occasional admittance.—Gentlemen inclined to support this school by contributing preternatural or morbid parts, subjects in natural history, &c. (individually of little value to the possessors) may have the pleasure of seeing them preserved, arranged, and registered, with the names of the donors.

Terms.—For a course of lectures, including the dissections, 5*l.* 5*s.* For a perpetual pupil to the lectures and dissections, 10*l.* 10*s.*

The inconveniences usually attending anatomical investigations are counteracted by an antiseptic process, the result of experiments made by Mr. Brookes on human subjects at Paris, in the year 1782; the account of which was delivered to the Royal Society, and read on the 17th of June 1784. This method has since been so far improved, that the florid colour of the muscles is preserved and even heightened. Pupils may be accommodated in the house.—Gentlemen established in practice, desirous of renewing their anatomical knowledge, may be accommodated with an apartment to dissect in privately.

Dr. Clutterbuck, Physician to the General Dispensary, Aldersgate-street, will commence his Summer Course of Lectures on the Theory and Practice of Physic and the Materia Medica, on Monday the 30th of May, at Eight in the Morning, precisely, viz., on the Theory and Practice, on Mondays, Wednesdays, and Fridays: and on the Materia Medica, on Tuesdays and Thursdays, at the same hour.

In these lectures will be given an outline of the structure and functions of the human body, as an indispensable preliminary

minary to the knowlege of the nature and treatment of its various disorders.

The lectures on the materia medica will contain the medical and pharmaceutical history of the articles in general use; with an explanation of their mode of acting, and their application to the cure of diseases.

The subjects will be illustrated by occasional reference to the practice of the Dispensary.

For further particulars, application may be made at the Dispensary, or at No. 17, St. Paul's Churchyard.

Dr. GEORGE PEARSON, Senior Physician to St. George's Hospital, will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, at his house in George-street, Hanover-square, the first week in June:—viz. the Medical Lecture at Eight, and the Chemical at Nine.—Particulars may be known in George-street, or at St. George's Hospital.

Mr. JOHN TAUNTON, Surgeon to the City and Finsbury Dispensaries, &c., will commence his Summer Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, on Saturday the 28th of May at Eight o'clock in the Evening, at his house in Greville-street, Hatton Garden, where further particulars may be known.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Francis Snowden, of Oxford-street, in the county of Middlesex, esq. engine-maker, for certain improvements in an engine for cutting hay and straw into chaff, and for cutting other articles. February 4.

To John Shorter Morris, of Pancras Place, in the parish of Pancras, in the county of Middlesex, gent., for his machine for mangling. February 4.

To Ralph Wedgwood, of Oxford-street, in the county of Middlesex, gent., for his apparatus for producing several original writings or drawings at one and the same time, which he calls a pennexpolygraph, or pen and sylographic manifold writer. February 22.

To Samuel Thomson, of Addle-street, London, calenderer; for a machine, engine, or frame for the purpose of widening or stretching to the width, leather, linen, cotton, and woollen stuffs. March 3.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For April 1808.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	35°	35°	35°	30·03	15	Cloudy
28	34	37	32	·05	30	Fair
29	33	42	33	·07	17	Fair
30	33	39	32	29·98	30	Cloudy
31	36	41	31	·91	36	Cloudy
April 1	32	42	30	·62	25	Cloudy
2	27	40	30	·87	34	Fair
3	33	48	40	·82	17	Cloudy
4	42	46	49	·60	0	Rain
5	51	54	52	·32	0	Rain
6	52	57	52	·68	12	Rain
7	53	58	50	·85	12	Cloudy
8	39	51	46	·91	16	Fair
9	38	52	45	30·25	26	Fair
10	46	55	50	·25	46	Cloudy
11	48	54	49	·23	41	Cloudy
12	47	47	39	·10	0	Storms
13	40	57	46	·26	55	Fair
14	46	58	47	·18	60	Fair
15	48	63	46	·02	62	Fair
16	39	51	35	·08	45	Fair
17	37	53	36	·12	62	Fair
18	34	46	35	29·96	50	Fair
19	40	33	32	·55	20	Snow
20	34	47	40	·55	35	Cloudy
21	41	47	35	·42	15	Stormy
22	39	47	39	·42	30	Stormy
23	40	49	40	·45	25	Stormy
24	41	49	37	·63	15	Cloudy
25	39	43	37	·83	27	Cloudy
26	38	45	37	·90	26	Cloudy

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

LVII. *Reduction of the Observation of the Transit of Mercury over the Sun, observed at the Royal Observatory, Greenwich, on the 8th of November, 1802. Communicated by* T. FIRMINGER, *Esq.*

THE particulars respecting this observation are detailed in the annual sheets published by the Royal Society; but as these sheets contain nothing more than a mere register of the astronomical observations made in the Royal Observatory at Greenwich, they are in general so little interesting, as to be found in the hands of but few astronomical or mathematical persons. It was therefore thought advisable, previous to giving a reduction of this transit, to transcribe the observation, as given in the account of the Greenwich observations of that year.

Transit of Mercury over the Sun on the 8th of November, 1802.

"I fitted the divided achromatic object-glass micrometer to the 46-inch achromatic telescope, and adapted the telescope to distinct vision, by trying it upon a large spot near the western limb of the Sun; which I had no occasion to alter during the whole period of the observation, as I continued to see Mercury perfectly distinct and well defined to the end of the transit. After having made this arrangement, I began measuring the distance of Mercury from the Sun's nearest limb, by bringing the exterior limb of Mercury to touch the interior limb of the Sun, and obtained the following set of distances.

Time per Transit Clock.			*Apparent Time.*			*Nearest Distances Limbs ☉ and ☿*				
									Reduced.	
h.	m.	s.	h.	m.	s.	Inch.	ten.	ver.	′	″
11	44	34	21	0	48	2	1	12	13	35·1
	47	18		3	32	2	1½	8	13	51·3
	48	36		4	50	2	1½	12	13	54·3
	50	11		6	24	2	2	7	14	0·7
	51	21		7	34	2	2	17	14	17·4
	53	51		10	4	2	2½	6	14	28·1
	55	1		11	14	2	2½	13	14	33·5
12	0	21		16	33	2	3	19	14	57·3
	1	21		17	32	2	3	24	15	1·2
	2	36		18	47	2	3½	2	15	3·5
	10	21		26	31	2	3½	7	15	7·3
	13	1		29	10	2	3	22	14	59·6
	14	40		30	49	2	2½	17	14	36·6
	34	6		50	12	2	1	2	13	27·5
	39	46		55	51	2	0	7	12	52·9
	40	33		56	38	2	0	2	12	49·0
	42	19		58	23	1	9½	12	12	37·5
	44	21	22	0	25	1	9	24	12	27·5

"From $12^h\ 50^m$ to $13^h\ 40^m$ per clock, or from $21^h\ 56^m$ to $22^h\ 46^m$ apparent time, I measured the horizontal diameter of Mercury, which, from a mean of 26 observations upon the scale, amounted to 11·646 of the vernier; and from a mean of 27 off the scale, it amounted to 15·255 parts of the vernier; the mean of both amounts to 11·646 parts of the vernier, or 8″·17; and the correction of the vernier is —0″·95 of its parts, or — 0″·73.

"By a mean of 21 measures the Sun's horizontal diameter was 5 inches 0½ tenths 8·253 vernier, which diminished by 0·95 the correction of adjustment leaves 5 inches 0½ tenths 7·3 vernier = 5·0646 inches, answering to 32′ 25″·4, the apparent diameter of the Sun; from which we derive the value of the scale.

"At $14^h\ 51^m\ 36^s·8$, or $23^h\ 57^m\ 21^s·4$ apparent time, I observed with the 46-inch achromatic telescope the thread of light between Mercury and the Sun's limb to break, or the

the true internal contact at the exit of Mercury. Mr. Richard Best *, who was observing near me with a five-feet achromatic telescope, made the same observation exactly at the same time. The regular circumferences of Mercury and the Sun appeared in contact about two seconds later, the small ligament joining the limbs of Mercury and the Sun being for that space of time plainly visible.

"The external contact of Mercury and the Sun was observed at $14^h\ 53^m\ 16^s{\cdot}8$ or $23^h\ 59^m\ 1^s{\cdot}1$ apparent time by myself,
14 53 13·8 23 58 58·1 by Mr. Best.

T. FIRMINGER."

Method employed in reducing the Measures of the Distances taken by the Micrometer, between the nearest Limbs of the Sun and Mercury. (See Plate VIII.)

Let O (Fig. 3.) be the centre of the Sun, A C the apparent path of Mercury, A O, B O, C O, any three observed distances, C D a perpendicular to AC, B F a perpendicular to A O, and B E a right line parallel to C O meeting A O in E. Then, because the differences of times between the observations are given, and the planet's relative motion is nearly uniform, the proportion of A C to A B, or of C O to B E, or A O to A E, is given, being as the time between the first and third obser-

* To this gentleman, who is extremely fond of astronomy, I was greatly indebted for the assistance he afforded me during the whole time of the transit: his readiness in reading off the scale of the micrometer enabled me to procure at least double the number of measures I could otherwise have done. Throughout the whole of the measures, every possible care was taken in making the exterior circular limb of Mercury to touch the nearest interior limb of the Sun; but in the middle of the observation, and at a time when these distances were most wanted, I was prevented from proceeding with so much accuracy, or acquiring so great a number as I could have wished, in consequence of a part of the micrometer coming up against one of the supporting pieces of the object end of the telescope; a circumstance I was unprepared to meet, having never used the micrometer before, or understood that such a defect existed. I mentioned this to Dr. Maskelyne when he returned, (he being in Wiltshire at the time of the observation,) who said he had often met with the same circumstance when observing the lucid parts of the Sun in a solar eclipse. This defect was soon after remedied by Mr. George Dollond, by a very simple plan I suggested to him of making the support of the object end of the telescope to shift, by connecting it to a moveable collar.

 vations

vations to the time between the first and second. Therefore A E, B E, and O E, are given. But O E is to (B O + B E) as (B O — B E) to (O F — F E), and $BF^2 = (BE + FE) \times (BE - FE)$; whence A F, B F, and the angle O A D become known, and consequently the nearest distance O D of the centres, as also A D the distance Mercury has to move from the first observation to the middle of the transit, which distance divided by the horary motion will give the time. The horary motion may be either taken from the tables, or found from the observations, being $= AB \times 3600''$, and divided by the time elapsed between the first and second observations taken in seconds, and is the method here used in determining the nearest approach of the centres, and apparent middle of the transit. But they may with equal ease be determined from the following algebraic formula which the analysis gives us. Let A, B, D, be the first, second, and third distances, and x, y, z, the elapsed times between the first and second, second and third, and first and third observations; then will the time between the first observation and the middle of the transit be

$$= \frac{y \times \overline{z + x} \times A^2 - z^2 B^2 + x^2 D}{2 \cdot \overline{y A^2 - z B^2 + x D^2}}$$

And the nearest distance of the centres will be had from this formula:

$$\frac{\overline{yA + zB + xD} \times \overline{yA - zB + xD} \times \overline{yA + zB - xD} \times \overline{zB - yA + xD}}{4xyz \times \overline{yA^2 - zB^2 + xD^2}}$$

Assuming the apparent horary motion of Mercury $= 355''\cdot375$, the nearest apparent approach of the centres comes out $= 60''\cdot137$, at $21^h\ 14^m\ 40^s\cdot5$ apparent time, as deduced from the following set of observations. Several others were calculated, which do not agree so well *, though their mean approximates to nearly the same quantity. If the horary motion be deduced from the observations, and not taken from the tables, the middle of the transit will come out about six seconds sooner.

* Owing to the defect arising from the micrometer not being properly manageable, as has been already stated. T. F.

Observations.

		Appt. nearest dist.	Appt. time.
		$''$	h. m. s.
Observations.	1st, 11th & 12th	60·215	21 14 40·3
- - -	1st, 11th & 16th	60·213	40·4
- - -	1st, 11th & 15th	60·171	40·5
- - -	1st, 11th & 14th	60·437	39·4
- - -	1st, 10th & 18th	59·648	42·8
	Mean	60·137	21 14 40·5

If $8''\cdot 8$ be taken for the mean parallax of the Sun, the horizontal parallax of Mercury from the Sun on the day of the transit is $4''\cdot 12213$, and at $21^h\ 14^m\ 40^s\cdot 5$, the parallax in longitude will be $-\ 2''\cdot 6775$, and the parallax in latitude $+\ 2''.9923$ to be applied to the apparent place: therefore the geocentric nearest distance of their centres was $62''\cdot 71$ at $21^h\ 14^m\ 10^s$ apparent time.

From the above data several theorems have been given for computing the effect of parallax in accelerating or retarding the time of the beginning or end of a transit. The following method, which is here employed, is as simple as any, and will be found sufficiently accurate.

Let O (Fig. 4.) represent the centre of the Sun, M D the relative path of Mercury, M its geocentric place at egress, and D at the nearest approach to the Sun's centre: join O D, O M, and in D O take D δ, equal to the parallax at the egress perpendicular to the path; draw $\delta\ \mu$, meeting the Sun's limb in μ and parallel to D M, and $\mu\ p$ perpendicular to D M, and take $p\ m$ in D M produced equal to the parallax at the egress in the path; then will μ be the apparent place of Mercury at the egress, and m its place seen from the Earth's centre, as is manifest from the construction. Now the egress is accelerated or retarded by the time Mercury takes to describe M m, which is $= mp + p$ M; and as the angle μ M p is $= \dfrac{\text{M O D} + \mu\text{ O D},}{2}$ $p\text{ M} = \mu p \times \cot \frac{1}{2}$ (M O D + μ O D), and M m = M p + μ p $\times$ cot $\frac{1}{2}$ (M O D + μ O D); but if m C the parallax in longitude be put $= \pi$, μ C the parallax in latitude $= \beta$, and the angle M m C or the angle which the relative path makes with the

 ecliptic

ecliptic $= \varphi$, $m\,p$ will be $= \pi \cos \varphi + \beta$ sine φ, and $\mu\,p = \beta \cos \varphi - \pi$ sine φ : consequently

$$\mathrm{M}\,m = \frac{\mathrm{s.}(\mu\,\mathrm{M}\,p - \varphi) \times \pi}{\text{sine}\ \mu\,\mathrm{M}\,p} + \frac{\cos\,(\mu\,\mathrm{M}\,p - \varphi)\,\beta}{\text{sine}\ \mu\,\mathrm{M}\,p}$$ which

will give the value of M m with the utmost accuracy. Now as in the transit of Mercury D δ must be very small, the angle μ O D may be safely supposed = M O D, and therefore μ M p = M O D; whence we have this rule for finding the effect of parallax.

Rule. The least distances of the centres divided by the Sun's semidiameter give the cosine of an angle which call w, from which subtract the inclination of the relative path of Mercury to the ecliptic, and call the difference χ. Let π be the parallax by which the apparent longitude is diminished, β that by which the apparent latitude is increased, m the horary motion of Mercury in its path in seconds and $\frac{3600''}{m} = \tau$, then will the number of seconds by which the egress is retarded be expressed

$$\text{by}\left(\frac{\text{sine}\ \chi}{\text{sine}\ \omega}.\ \pi + \frac{\cos\ \alpha}{\text{sine}\ \omega}\beta\right) \times \tau$$

In order to express this rule in numbers, we must find from the tables the relative geocentric horary motions of Mercury in latitude and longitude, and hence the angle φ. Now Lalande's tables give us at the egress the relative horary motion in longitude = $361''\cdot455$, and in latitude $51''\cdot7355$ $\varphi = 8°\ 22'\ 26''$; and the semidiameter of the Sun corrected for eradiation = $969''$: consequently $\omega = 86°\ 17'\ 22''$, $\chi = 77°\ 54'\ 56''$: and the formula expressing the number of seconds by which the egress is retarded $9\cdot93\,\pi + 2\cdot126\,\beta$: from which having given the parallaxes in latitude and longitude, the apparent egress for the centre of the Earth may be easily computed, and hence the difference of meridians of two places where the transit has been observed may be found. The parallaxes may be calculated by the common methods, or by the following formula, which will be found sufficiently accurate for that purpose. Let λ = the latitude of the place of observation, R = the *AR* of the mid-

heaven,

heaven, P = the horizontal parallax of Mercury from the Sun, L = its longitude, α = obl. ecliptic, then will the parallax in longitude = P cos λ sine L $\times$ cos $\overline{(R - P)}$ cos. α cos L sine $\overline{(R - P)}$ sine α $\times$ cos L sine λ, and the parallax in latitude = P cos α sine $\overline{\lambda - P}$ sine α cos λ sine R: whence we have in the present case the parallax in longitude at the egress or $\pi = -\ 2''\cdot972$ cos R cos λ + $2''\cdot62032$ sine R cos λ + $1\cdot1375$ sine λ, and the parallax in latitude or $\beta = 3''\cdot78$ sine $\lambda - 1''\cdot64$ sine R cos λ; of which the first must be subtracted from, and the second added to, the apparent place of Mercury.

By computing the reductions from the above formulæ, and applying them to the times of the observed transit at the different places of observation specified in the following Table, the differences of their longitudes were as they stand there determined. The Table also contains the quantity of the reduction, and the effect of parallax in longitude and latitude.

	Apparent Time at Observation.	Reduction.	Observations Reduced.	Longitude from Greenwich.	Parallax in Long.	Parallax in Lat.	Names of the Observers.
	h. m. s.	*s.*	*h. m. s.*	*h. m. s.*	*s.*	*s.*	
Paris	0 7 34	− 17·398	0 7 16·6	0 9 24·3	0·9781	3·615	Mean of all.
Gotha	0 41 15·8	− 14·3	0 41 1·5	0 43 9·2	0·6348	3·771	Baron Von Zach.
Utrecht	0 18 40·5	− 16·8	0 18 23·7	0 20 31·4	0·8933	3·735	M. Von Utenhaven Calkoen
Leyden	0 15 53	− 17·1	0 15 35·9	0 17 43·6	0·9238	3·727	M. Von Beack
Greenwich	23 58 11	− 18·7	23 57 52·3		1·1095	3·653	T. Firminger.

M. Von Zach observed the interior contact only, which must be reduced by allowing $1^m\ 30^s$ for the time between the contacts; and reduced to apparent time it gives $0^h\ 41^m\ 15^s\cdot8$ for the time at observation.

The shortest distance of the centres, combined with the distance at the moment of the egress, gives the middle of the transit at $21^h\ 14^m\ 28^s\cdot6$, and hence the time of apparent conjunction at $21^h\ 16^m\ 2^s\cdot1$: but as the aberration of the Sun is $-\ 20''\cdot2$, and that of Mercury $+\ 18''\cdot35$, the true con-

 junction

junction took place $6^m\ 35^s\cdot3$ sooner, or at $21^h\ 9^m\ 26^s\cdot8$ apparent time, when the Sun's true place from the mean equinox by Mayer's tables was $7^s\ 16^\circ\ 17'\ 13''\cdot3$, or $7^s\ 16^\circ\ 17'\ 8''\cdot5$ if we take into account the correction to be made in the tables which the Greenwich observations make $-\ 5''$ nearly. Hence the place of the node is found to be $1^s\ 15^\circ\ 58'\ 56''\cdot3$.

LVIII. *Geological Journey to Mount Ramazzo in the Appenines of Liguria; Description of this Mountain; Discovery of the true Variolite in its Bed; of Lime; of the Arragonite; and of Martial, Magnetic, Cupreous, and Arsenical Pyrites, in the Steatitic Rock; Manufacture of the Sulphate of Magnesia. By M.* FAUJAS ST. FOND*.

IN 1780, Messrs. de Saussure and Pictet visited the mountain Madona della Guardia, elevated 422 toises above the level of the sea, and of which mount Ramazzo forms a part. After having given an excellent lithological description of the first mountain, Saussure thus expresses himself†: "On ascending and descending the mountain della Guardia, we had a view to the westward of a mountain, from which we were separated by a very deep ravine, and from which we were informed that martial vitriol had been extracted; but I had no knowledge of the substance in which it was found. At the distance from which we saw this mountain, it seemed mixed with slate and ferruginous earths."

It was this mountain, (known by the name of Mount Ramazzo,) thus neglected by Saussure, which principally occupied my attention for two reasons: in the first place, because I had been told that trenches were dug at the summit of this mountain, and that a manufacture of sulphate of magnesia had been established there: secondly, because, the steatitic and serpentine rocks of this mountain being united to lime in certain points, I was anxious to visit this curiosity so seldom discovered.

* From *Annales du Muséum d'Histoire Naturelle*, tome viii. p. 313.

† *Voyage de Saussure dans les Alpes*, tome iv. p. 145.

M. Maxi-

M. Maximilian Spinola of Genoa, who is skilled in several branches of natural history, M. Viviani a botanist, and my friend M. Marozari of Vicenza, an excellent mineralogist, were anxious to accompany me. We took our departure at six o'clock in the morning from Genoa: we went in a carriage to Cornigliani, where we visited M. Durazzo's rich collection in natural history: from thence we proceeded to Sestri, where M. Alberto Anseldo conducts a manufactory of sulphate of magnesia with great spirit. This gentleman acted as our guide in the arduous excursion we were about to make: our route lay through narrow by-ways, profound ravines, and we were obliged to climb from rock to rock, which were so flinty and slippery, that they required some experience in alpine travelling to surmount them. We left our carriages at Sestri, and immediately entered the bed of the river Charavagna on foot. We ascended this torrent for about an hour. Its bed is broad, and wholly covered with blocks of serpentine and other round stones; which shows that it is subject to dreadful torrents; but there is scarcely any water in it in dry seasons. The following are the remarks I made on ascending to a lime-kiln, which I shall speak of by and by.

Notice upon the Stones in the Torrent of Charavagna.

1. Various pieces, larger or smaller, of a grayish steatitic rock, of a drier grain than that of the other steatites, of which I shall soon take notice. This rock has fissures or cracks filled with greenish crystallized epidote, similar to that of the alps at Oisan in the ci-devant Dauphiné. I am surprised how this rock escaped the attentive and vigilant eye of Saussure. It is probable, since he has not mentioned it, that the torrent of Charavagna was not then passable, or at least when he visited the mountain della Guardia.

2. Tender serpentine of a blackish green shaded with light green, and shining as if varnished, soft and even unctuous to the touch, radiating into white streaks, with a striated and undulous fracture, having a talcky appearance, and strongly obedient to the magnet.

3. Serpentine, soft, and analogous to that of No. 2, as to the

the constituent parts; but its colour is of a clearer green: its surface is much more glossy than that of the preceding, and its fracture more generally undulated: but what particularly distinguishes this beautiful specimen, which is seven inches long by five broad, is, that it is not only very attractable by the magnet, but endowed with a strong polarity throughout its whole length; and it attracts keenly at one end, while it even repels with the other.

4. Serpentine of a deep green, with some shades of a clearer green, soft to the touch, but harder than the preceding, having one of its facets striated, and something like asbestos.

5. Another serpentine somewhat hard, of a greenish black, with small spots of a greenish white near to each other, and which seem to have a tendency to the parallelopipedon form, which gives to this variety of serpentine a false aspect of black and white antique porphyry. But what renders the latter remarkable, is, that it contains in its substance, as well as upon its external facets, a multitude of very brilliant small silvery scales of metalloidal appearance (*diallage*), the brilliancy of which is the more lively, as it shines upon a black ground. This serpentine is strongly attracted by the magnet.

6. Semi-hard serpentine, attractable, of a deep greenish black, with some laminæ of metalloidal appearance like silver, and small layers more or less thin, but some of them a line in thickness, of a substance of the unctuous appearance of steatite; and its colour being olive-green, shining, and of an equal and rich tint, seems to be owing to chrome.

7. Semi-hard serpentine of a blueish-gray, with longitudinal compressed streaks, covered with a slight and transparent couch, or rather with a kind of varnish of a clear azure blue. We also discover in the fractures of this beautiful serpentine, some scales of a metalloid diallage, and of a silvery hue; it is attractable by the magnet.

8. Serpentine of a deep greenish-gray, semi-hard, feebly attractable, with small round globules, sometimes oblong, of a compact white or greenish substance, harder and more homogeneous than that of the stone which contains them, of

of a steatitic aspect, and presenting, when we look at it through the microscope, very fine lineaments which unite about the centre of each globule. Here we have a true variolite, which ought not to be confounded with an amygdaloid. The specimen I have described, and which was confounded with the other stones I have mentioned in the bed of the Charavagna, is more remarkable from the globules being distinct, a little projecting and distinct from each other, as in the variolites of Durance, occupying one-third of the size of the specimen; they are also much nearer together, and seem to touch, and they are confounded afterwards; forming at the extremity of the piece but a single couch where the globules have disappeared, and where the same substance of which they are composed no longer affects the regular form.

This stone fixed my attention, since it gave me reason to expect a variolite analogous to that of Durance, in a place where no person had met with it, or even suspected it to exist.

As the globules, however, of the true variolite belong to a substance very like feldspar and fusible like it, and as I have neither met with compact feldspar, (petro-silex of the Germans,) nor feldspar under other forms; I think the round variolite which I found, was one of those stones transported in great revolutions of the earth, and out of its proper place.

I made these reflections when advancing up the bed of the torrent; but I suddenly discovered a stony mass of a white or greenish hue, weighing more than thirty pounds, which, at first sight, awakened in my mind the idea of feldspar: it may be described as follows:

9. Compact stone, with fine paste, translucid upon the edges, soft to the touch, of a white slightly tinged with asparagus green, having the appearance of jade, breaking into scales rather lamellous than conchoidal, scratching glass strongly, and emitting some sparks when struck with steel; but it is not so hard as the jade. In the blow-pipe it bubbles up almost as soon as the fire touches it, and melts very soon into a yellowish transparent glass. On breaking this

 stone,

stone, we perceived some parts of a lively apple-green, arranged in small elongated laminæ, flat and of a silky lustre, owing to the *diallage*.

I consider this stone as a true compact feldspar, mixed with a little steatitic serpentine, and with iridescence (*diallage*): it is this mixture which contributes, perhaps, to its great fusibility. I found another piece of it weighing more than twelve pounds.

With the blow-pipe I made a comparative trial of the greenish-white globules of the variolite No. 8, which I found in the bed of the torrent, and they bubbled up and melted with the same facility as the stone I have described. Now, as the latter was of a large volume, and did not appear to come from any distance, for its angles were scarcely abraded, I presumed that it should abound in some parts of the neighbouring mountains, in the direction of the torrent which had received these fragments, and that it was perhaps found in a furrow, or mixed into the paste itself of some of the serpentines, which I thought I should find in its proper place. In fact, it was natural to think that the junction of the molecules of feldspar in globules, at the time of the formation of these mountains, might have given rise to variolites analogous to those known by the name of *variolites of Durance*; and thence I did not lose hopes of finding this kind of stone in the same rock which contributed to its formation.

10. Finally, the bed of the torrent, in proportion as I advanced, presented me with various fragments of a compact calcareous stone, hard and of a fine paste, susceptible of being polished, with some veins of calcareous spar which traversed it; I saw also some of the same spar adhering to a vein of white quartz.

These calcareous stones found in a considerable number, beside the serpentine, magnesian, and feldsparry stones I have mentioned, left me also some hope of being able to observe the points of contact of the magnesian with the calcareous rock in a country quite free of wood, and torn up by torrents, presenting great hills and deep ravines. Reflecting in this manner, I advanced a little further, and in a deep recess

recess formed by the torrent of the river, I perceived upon an eminence on the right bank of the Charavagna, a rustic habitation, and a large lime-kiln at work.

Of the Calcareous Stone proper for being converted into Lime; of its Bed beside Serpentines.

The lime-kiln used for calcining the stone, the singular situation of which I shall soon describe, is of so peculiar and unusual a construction, that it is worthy of our attention. In consequence of the extreme scarcity of wood, the saving of fuel has been their principal object: heath and broom only of a large size are used for this purpose; these give a brisk but not a durable fire, and it becomes necessary to endeavour to preserve the heat as much as possible. For this purpose, they have constructed of strong and good masonry, a kind of square tower, surmounted with a pyramid-formed capital of solid stone, which serves as a kind of roof, and forces the heat to reverberate upon the calcareous stone divided into fragments and intended to be calcined. A simple straight aperture towards the top of the vault serves for letting out the smoke, and the humidity which exhales from the calcareous stone and combustible; and it establishes a current of air necessary for keeping up the fire, which being concentrated in a great measure by the obstacles opposed to its wasting, acquires a greater and steadier intensity. If, in some peculiar circumstances, they require a greater current of air, it may be easily obtained by opening a small door placed in one of the faces of the pyramidal wall, which serves as a roof to the furnace. The lime-stone is introduced by a door made behind the furnace, and it is withdrawn by the same aperture when it is calcined: the combustibles are placed below upon a grating.

The quarry of lime-stone is not far off, and is to be seen in the kind of ravine which serves for the bed of the torrent. As my intention was to follow it in several points, so as to become well acquainted with its situation, I continued to ascend the torrent of Charavagna, by a route which becomes more rapid as we advance. The soil was every where loaded

7 with

with serpentines of various kinds similar to those I have mentioned: but there were also found considerable blocks of a mass composed of fragments of the serpentines above described, and of the same lime I have mentioned, and similar in every respect to that of which the lime is made: this mass is strongly united by a calcareous sparry cement.

A second kind of mass, also arranged in large blocks, is to be found in the neighbourhood of the first: both have been detached from the neighbouring mountains, and do not seem to have been brought from any distance: the one in question is composed only of larger or smaller fragments of different kinds of serpentine, which have entered into the composition of the neighbouring mountains; but we find no lime in it as in the first mass, and the cement which has united these serpentines has lime in it, being entirely steatitic. We soon met with these two varieties of masses again upon the edges of one of the ravines of the torrent, on the one edge beside some serpentines, and on the other adhering to lime. It seemed at first sight as if these masses served as intermedia, and were passing from one genus to another: but in examining them again, and reflecting that they must have been formed at the expense of the calcareous and steatitic rock, both of which must have had at this period the same consistency and the same hardness as at present, we could only attribute this formation to an accidental revolution, long subsequent without doubt to events of another order, which have given birth to these mountains of serpentines, and to the calcareous beds adhering to them, and which have furnished the materials for these two masses. But let us now cast a glance upon the lime in its bed, and see if it be cotemporary with the serpentine rock, or if it be subsequent.

At a certain distance from the lime-kiln, and not far from the hamlet of Serra, placed in an amphitheatre near the precipice which hangs over the torrent of Charavagna, we may observe in a very distinct manner a part of the calcareous layers in their points of contact with the magnesian rock. I give the preference to this spot over that which is nearer the lime-kiln, because we see more distinctly the

the junction of both substances; and the doubts we might raise as to the lime-stone placed against it secondarily, and afterwards against the serpentine rock, disappear entirely upon examining the facts I have mentioned.

In short, when the torrent in its various overflowings, and when after storms its waters are precipitated from precipice to precipice with a violence and impetuosity carrying every thing before it, and laying bare the calcareous and serpentine beds, so as to present the whole at full view; we then remark the gray, hard, and compact lime-stone, which is modified into white calcareous spar, and forms large strings or lineaments, which are joined and interlaced with very small layers or lineaments of steatitic serpentine. These lineaments sometimes increase, and are developed longitudinally, and like stripes of a gray or greenish colour around longitudinal or circular laminæ of calcareous spar of a white colour. In other parts adjacent, the two substances form a kind of net work. In a word, we think we can perceive in the union and in the play of these two substances, so different in their nature, the results of the movement of the fluid which held both in solution at one and the same time; and we only know the waters of the sea, and their long continuance upon these regions, at very remote periods, as having acted upon masses which constitute chains of mountains.

Every thing, therefore, inclines us to think, that in this case the lime-stone has been attached, or rather joined, to the magnesian rock, not by an after process, but in one and the same operation, when great accumulations of dissolved calcareous matters being in the neighbourhood of substances which have given birth to the serpentine rock, their molecules floated in the same fluid, which gave rise to points of meeting, contact, union, and mixture, similar to those we observe here. Nothing proves more completely that this mixture is made simultaneously than the chemical state of these two substances; for the purest of the lime-stone contains 6, 7, and even 8 per cent. of magnesian earth, while the serpentine rock has as much lime-stone in its mass.

Saussure

Saussure had remarked upon the mountain della Guardia, an alternation of calcareous and serpentine layers; which is perfectly analogous to what I have here described. But as such an operation could not be performed at a single *jet*, we cannot refrain from remarking here also that nature never takes into her account the operations of time.

Of the true Variolite (Variolites viridis verus) *in the same Rock in which it has been produced.*

As our object was to visit the quarrying of the materials used in the manufacture of sulphate of magnesia upon the highest part of Mount Ramazzo, M. Alberto Ansaldo, who guided us, informed us that we must leave the bed of the Charavagna, pass to the hamlet of Serra, and ascend over shelving precipices into a direction opposite to that of the torrent. The road, or rather the pass, was strait, rapid, and slippery; we were surrounded on all sides by serpentine rocks more or less green; some were hard, others soft: the grain of them also varied; in some places it was dry, and in others greasy and unctuous: enormous masses, placed upon still huger masses, spread spontaneously, some into irregular leaves more or less turned, and others into striated pieces imitating asbestos: the *diallage* was distinct in some fractures, and exhibited a silvery lustre; in others this was not to be seen; and in this case the heart of the stone, being of a deep blackish green, presented shades of a clearer green.

We had ascended at least six hundred feet in height from the hamlet of Serra; when being at this height, not far from a small stream of water which runs through the pass, and might serve as a point for reconnoitring, I perceived a detached piece of serpentine, the surface of which was covered with small globules of a whitish green, a little projecting, and harder than the paste of the stone. I saw with pleasure that it was a variolite not rolled nor transported from its place, but detached spontaneously by the effect of moisture, or the alternation of heat and cold, from an enormous mass of serpentine which was contiguous.

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This fine specimen is five inches long by three broad; one of its facets presents the whole characters of a fine green variolite, with small grains projecting a little, and of a green much clearer than the heart of the stone, while the opposite part is a true serpentine of a dark green, without globules or variolitic spots. We could not doubt, after looking at this specimen, that the substance which was joined in globules in order to form this variolite, was the result of an assemblage of a certain quantity of feldsparry substance, the elements of which had been mixed in the serpentine rock at the time of its formation. This kind of separation may be considered as the result of a globulous imperfect crystallization, determined by the attractive force of the feldsparry molecules, which had more affinity for each other than for the magnesian earth; and if these variolitic globules are only superficial as it were, (for the bed in which we remark them is little more than three lines thick,) it is because the substance of the feldspar was not abundant. In short, the identity of the globulous substance is absolutely the same with that which I found, separated and in voluminous pieces, in the bed of the Charavagna, and which I have mentioned in No. IX of the description of stones found in this torrent. In fact, having attacked with the blowpipe some globules of the variolite in question, they swelled upon the first attack of the fire, emitted some air bubbles, and formed a yellowish transparent glass, like the feldsparry stone above mentioned.

A variolite equally well characterized, in the neighbourhood of the rock of which it had once formed a part, gave me good hopes of meeting with some more of them. These hopes were soon realised; for after we had ascended 300 feet higher, we found beneath our feet several flat pieces, but angular, and of hard serpentine of a lighter or darker green filled with variolitic globules, the grains of which were much thicker, and penetrated into the whole mass of the serpentine. I gathered several fine specimens, some of which are six or eight inches long by an inch in thickness, and of so decided a character that we can easily distinguish with the microscope the commencement of the kind of radiated crystallization which is peculiar to each globule.

The higher we ascended, the more of these specimens did we find at the foot of the rocks of serpentine. I observed with interest the general tendency these rocks naturally have to divide into splinters, or into large flat and scaly fragments; (this I attributed to a peculiar alteration of the iron so abundant in this kind of stone;) when I suddenly perceived upon the right of the pass, a mass of serpentine in its bed more than eighteen feet high, with a base of 40 feet broad, and which seemed as it were isolated, either by natural and spontaneous decomposition of the most tender parts of the rock, or by some other cause. I perceived upon this large mass some parts much greener than others, penetrating deeply into the heart of this enormous block, which was of a very dull blackish green.

I approached very close, and I ascertained that most of the spots were produced by parts abounding in true variolite of a grass green colour, and white spots or grains, shaded from an extremely clear green. All these parts formed in variolites seemed extremely hard: I had soon some proofs of this, when I struck them with a hammer, and it was with great difficulty I obtained some good specimens.

Several of these pieces had a multitude of globules analogous and similar in every respect to the variolite of Durance: sometimes, however, the variolitic spots only entered to the depth of an inch and a half into the stone, and the rest seemed to be nothing else than a pure serpentinous rock; at other places the variolite entered deeper into the mass: in some places the granulous surface was not much larger than the palm of the hand; in others it was double this size: in short, by continuing to observe more variolites in other blocks, I was convinced that this singular stone is not in a vein in the masses of serpentines, but that it exists indifferently, sometimes in one place and sometimes in another, without order or regularity. It is the same with the arrangement of the globules: we see them huddled together upon some pieces, and as it were arranged

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in an equal and distinct manner, while they are thinly scattered upon others, or sometimes so close together that they are confounded, and form merely a large whitish spot.

I shall finish these details by observing, that I possess among my numerous specimens a remarkable piece, which will demonstrate to those acquainted with the composition of rocks, that the variolite of Mount Ramazzo took its rise in a true serpentinous rock. I shall now describe this rare and curious specimen in few words.

Its colour is the same with that of the other serpentines I have mentioned; but the rock from which I detached it with the hammer was very hard, its texture very close; but striated at the same time in a scaly manner; and its scales frequently interposed between the streaks, and intersecting them transversely, render this rock difficult to break: it is torn in some measure rather than broken, and requires heavy blows with the hammer. In this way I procured this fine specimen with a fracture a little undulous, but pure and clean, and which admits of our seeing upon the two large faces of the stone its texture, as well as upon the rock itself. We see perfectly upon a part which forms almost the half of the piece, and not only upon the faces but throughout its whole thickness, a multitude of variolitic globules of almost equal size, of a clear green, issuing every where from the streaks and scaly parts of the stone, as if they were sown in it: these globules become smaller afterwards in proportion as they approach that part of the specimen which has none of them; and this last part is then nothing but pure serpentine mixed with some irregular and thin lineaments of a white colour, not belonging to the variolite, and some of them slightly effervesce with the nitrid acid.

From the above facts we see that the true variolite exists in Mount Ramazzo, and that it is cotemporary with the serpentines in which we find it at the height of more than 1500 feet above the level of the sea. It is to be presumed we shall find it at a greater elevation, and perhaps in still greater abundance, in other parts of the Ligurian Appenines.

Of the Mine of Magnesia upon the highest Part of Mount Ramazzo.

The route becomes more and more difficult as we approach the rugged summit, where the quarries and establishment of M. Alberto Ansaldo are situated, nor do we lose sight of serpentine rocks the whole way: these are more or less compact, lamellous, streaked, glossy, unctuous, or dry and friable. We pass several ravines, and mount from precipice to precipice, until we come to about 1800 feet above the level of the sea, being the summit of Mount Ramazzo, where we find a small flat piece of ground, upon which are built some penthouses for preparing the mineral, and apparatus for stamping, roasting, leying, &c.;—in a word, for extracting the sulphate of magnesia in a simple and interesting manner.

This operation consists in carefully collecting a very pyritous steatite, which is found rather in heaps and large fragments than in regular seams: they afterwards roast it, after reducing it to pieces, in order to evaporate a little of the arsenic which is combined with it. It is in this operation of roasting that the sulphur which is combined with the iron is disposed to quit its base, and attaches itself to the magnesian earth of the serpentine, in order to form the sulphate of magnesia. They pound this roasted pyrites in a rough manner; it is then arranged in large heaps, which they water slightly: the combination with the magnesian earth is then finished by a slow fermentation which the matter undergoes: these earths are afterwards leyed, and a very abundant sulphate of magnesia is obtained, which is purified and refined in another establishment belonging to M. Alberto Ansaldo, at Sestri.

After having examined the first preparation of the ferruginous and arsenical pyrites of Mount Ramazzo, we entered the galleries of the mine, which are close to the penthouses: they are large, but the working of them is not regulated: they follow the pyrites and the pyritous steatite wherever they meet it, sometimes in a straight line and sometimes laterally, just as it presents itself. The excavations are made in in an irregular manner, and without any precautions for the safety of the workmen: the pickaxe, however, alone is used. The explosion by gunpowder would certainly bring down the roofs of the quarries, which are no where supported by props or bulwarks. The various specimens I collected here were:

1st. A greenish steatite, the surface of which, as well as its interior texture, is penetrated with a kind of pyritous varnish of a bronze colour, but so light and so efflorescent (if I may use the expression) that it seems as if the heart of the steatite, which is black, was shown through this kind of varnish. This pyritous steatite is very heavy; it blackens paper, and moves the magnetic needle strongly. It contains a small proportion of copper, but scarcely perceptible.

2d. The same steatite, still richer in pyrites, with the slight yellowish or bronze varnish I have mentioned, which seems to gild the black serpentine rock, is soft to the touch, blackening the fingers, and forcibly obedient to the magnet. We find in the same rock arsenical magnetic pyrites, very heavy, and with a metallic fracture of a grayish white.

3d. This is a rare and superb specimen, being five inches nine lines long by four inches broad: the base of it is a serpentine of a deep black, a little glossy, blackening paper, without a pyritous appearance; but very heavy, and strongly attractable by the magnet, remarkable from needles of transparent white arragonite, one crystal of which is two inches three lines long and four lines in diameter, of a hexagonal figure, but always without a pyramid. Other crystals of a still greater diameter are to be seen, sometimes in kinds of cavities in the specimen, sometimes in the mass itself of the pyritous serpentine, and seem to have been formed simultaneously with the pyritous and magnesian elements of which this rock is composed.

4th. We find a few yards from the quarries, and a little lower down, some old excavations, but not so deep: here there is a striated and silky-like pyrites, with beautiful green efflorescences of carbonated copper. This steatite is

 soft

soft to the touch, and has yellow ochrey spots in it, apparently proceeding from grains of altered cupreous and ferruginous pyrites. M. Ansaldo informed me, that this pyrites was formerly wrought for the sake of its sulphate of copper, but abandoned on account of its poverty.

LIX. *Essay upon Machines in General. By M.* Carnot, *Member of the French Institute, &c. &c.*

[Continued from p. 221.]

Problem.

XX. *The virtual movement being known of any given system of hard bodies,* (i. e. *that which it would assume if each of the bodies were free,*) *to find the real movement which it should have the following instant.*

Solution. Let us denominate each molecule of the system, - - - - - m

Its virtual given velocity, - - - W

Its real velocity sought, - - - V

The velocity it loses, in such a manner that W is the result of V and of this velocity, - - U

Let us now imagine that we make the system assume an arbitrary *geometrical movement,* and let the velocity which m will then have be - - - u

The angle formed by the directions of W and V, X

The angle formed by the directions of W and U, Y

The angle formed by the directions of V and U, Z

The angle formed by the directions of W and u, x

The angle formed by the directions of V and u, y

The angle formed by the directions of U and u, z

This being done, we shall have the equation $s\, m\, u$ U cosine $z = 0$ (F); by means of which we shall find in all cases the state of the system, by attributing successively to the indeterminates u different relations and arbitrary directions.

Definitions.

XXI. Let us imagine a system of bodies in movement in any

any given manner: let m be the mass of each of these bodies, and V its velocity; let us now suppose that we make the system assume any geometrical movement, and let u be the velocity which m will then have, (and what I shall call its geometrical velocity,) and let y be the angle comprehended between the directions of V and u; this being done, the quantity $m\ u$ V cosine y will be named the momentum of the quantity of movement mV, with respect to the geometrical velocity u; and the sum of all these quantities, namely $s\ m\ u$ V cosine y, will be called the momentum of the quantity of movement of the system with respect to the geometrical movement which we have made it assume: thus *the momentum of the quantity of movement of a system of bodies, with respect to any geometrical movement, is the sum of the products of the quantities of movement of the bodies which compose it, multiplied each by the geometrical velocity of this body, estimated in the ratio of this quantity of movement.* In such a manner that by preserving the denominations of the problem, $s\ m\ u$ W cosine x is the momentum of the quantity of movement of the system before the shock; $s\ m\ u$ V cosine y is the momentum of the quantity of movement of the same system after the shock; and $s\ m\ u$ U cosine z is the momentum of the quantity of movement lost in the shock (all these momenta being referred to the same geometrical movement). Thus, from the fundamental equation (F) we may conclude, that *in the shock of hard bodies, whether these bodies be all moveable, or some of them fixed, or, what comes to the same thing, whether the shock be immediate, or made by means of any machine without spring, the momentum of the quantity of movement lost by the general system is equal to zero.*

W being the result of V and U, it is clear that we have W cosine $x =$ cosine $y +$ U cosine z, or $m\ u$ W cosine $x = m\ u$ V cosine $y + m\ u$ U cosine z, or lastly, $s\ m\ u$ W cosine $x = s\ m\ u$ V cosine $y + s\ m\ u$ U cosine z: now we have found $s\ m\ u$ U cosine $z = 0$; therefore $s\ m\ u$ W cosine $x + s\ m\ u$ V cosine y, that is to say, *in respect to any geometrical movement, the momentum of* *the quantity of movement of the system, immediately after the shock, is equal to the momentum of the quantity of movement immediately before the shock.*

When we decompose the velocity which a body would assume if it were free, into two, one of which is the velocity it actually assumes, and the other the velocity it loses; and reciprocally if we decompose the velocity it loses, into two, one of them being that which it would have taken if it had been free, the other will be the velocity it gains: whence it visibly follows, that what we understand by the velocity gained by a body, and what we understand by its velocity lost, are two quantities equal and directly opposite: this being done, the momentum of the quantity of movement lost by m, with respect to the geometrical velocity u, being, according to the preceding definition, $m\,u$ U cosine z, the momentum of the quantity of movement gained by the same body will be $-\,m\,u$ U cosine z; for there is no difference between these two quantities, except in this, that the angle comprehended between u and the velocity gained is the supplement of that comprehended between u and U; so that one of these angles being sharp, the other will be obtuse, and its cosine equal to the cosine of the other, taken negatively.

Hence it follows, that the momentum of the quantity of movement lost by the general system, with respect to any geometrical movement, (which is null, as we have seen above,) is the same thing as the difference between the momentum of the quantity of movement lost by any part of the bodies which compose it, and the momentum of the quantity of movement gained by the other bodies of the same system: thus this difference is equal to zero, and thus the one of these two quantities is equal to the other; that is to say, *the momentum of the quantity of movement lost in the shock by any part of the bodies of the system, with respect to any geometrical movement, is equal to the momentum of the quantity of movement gained by the other bodies of the same system.*

We may, therefore, from the preceding definition, collect the three propositions contained in the following

THEOREM.

THEOREM.

XXII. *In the shock of hard bodies, whether this shock be immediate, or whether it be made by means of any machine without spring, it is clear that in respect to any geometrical movement,—*

1st. *The momenium of the quantity of movement lost by the whole system is equal to zero.*

2d. *The momentum of the quantity of movement lost by any part of the bodies of the system, is equal to the momentum of the quantity of movement gained by the other part.*

3d. *The momentum of the quantity of real movement of the general system, immediately after the shock, is equal to the momentum of the quantity of movement of the same system, immediately before the shock.*

It is clear, from the preceding definition, that these three propositions are radically the same, and are nothing else than the same fundamental equation (F) expressed in different ways.

We may also remark that these propositions bear a great relation to those we extract from the consideration of the momenta relatively to different axes; but the latter are less general, and are easily inferred from those established in XVII.

There is, therefore, as we see by the third proposition of this theorem, in every percussion or communication of movement, whether immediate, or caused by the intermedium of a machine, a quantity which is not altered by the shock: this quantity is not, as Descartes thought, the sum of the quantities of movement; nor is it the sum of the active forces, because the latter is only preserved in the case where the movement changes by insensible degrees, as we shall see lower down, and it always diminishes when there is percussion, as will be proved in the second corollary. When the system is free, the quantity of movement estimated in any ratio, is in truth the same before and after the percussion; but this preservation does not take place if there are obstacles, any more than that of the momenta of quantity of movements referred to different axes: all these quantities

tities are therefore altered by the shock, or at least are only preserved in some particular cases. But there is another quantity, which neither the various obstacles opposed to the movement, nor the machines which transmit it, nor the intensity of the different percussions can change; it is the momentum of the quantity of movement of the general system, with respect to each of the geometrical movements of which it is susceptible; and this principle contains in itself alone all the laws of equilibrium and of movement in hard bodies: we shall even see in corollary IV, that this law equally extends to other kinds of bodies, whatever be their nature and degree of elasticity.

If the shock destroyed all the movements, we should have $V = 0$: therefore the equation would be reduced to $s\, m\, W\, u$ cosine $x = 0$; which shows us that this case happens; namely, that all the movements are reciprocally destroyed by the shock, in the case where, immediately before this shock, the *momentum of the quantity of movement* of the general system is null, relatively to all the geometrical movements of which it is susceptible.

First Corollary.

XXIII. *Among all the movements of which any system of hard bodies acting upon each other is susceptible, whether by an immediate shock, or by any machines without spring, that movement which shall really take place the instant afterwards will be the geometrical movement, which is such that the sum of the products of each of the masses, by the square of the velocity which it will lose, is a* MINIMUM, i. e. *less than the sum of the products of each of these bodies, by the velocity it would have lost, if the system had taken any other geometrical movement.*

Here it must be remarked, that, by giving for the minimum the sum of the products of each mass, by the square of its velocity lost, I understand solely that the differential of this sum is null; *i. e.* that its difference from what it would be if the system had a geometrical movement infinitely little different from the first, is equal to zero: thus

thus this sum may be sometimes a *maximum*, or even neither a *maximum* nor a *minimum*; and I have only to establish $d\ s\ m\ U^2 = 0$.

Demonstration.—It is at first evident that the true movement of the system after the shock should be geometrical; for geometrical movements being those which do not alter the action which is exercised among bodies, it is clear that the first in order is the same movement as assumed by the system: it is therefore required to know, which, among all possible geometrical movements, is the one that should take place. Now, supposing that it should take another infinitely little different from that which we are seeking, the velocity of each molecule m would then have been V'; let us decompose V' into two, one of which shall be V, *i. e.* the real velocity, and the other V'': this being done, it is evident that if the bodies had not other velocities than these last V'', the movement would be still geometrical, for V'' is visibly the result of V' and of a velocity equal and directly opposite to V: now, by hypothesis, the molecules taken two by two do not tend, either in virtue of V', or in virtue of $-V$, to approach or recede, since in these two cases the movement is geometrical: thus, by supposing that the molecules m have at once the velocities V' and $-V$, or their result V'', they will neither tend to approach nor to recede; and therefore the movement will then be geometrical: thus, if we call z'' the angle comprehended between the directions of V'' and U, we shall have by means of the fundamental equation (F) $s\ m\ U\ V''$ cosine $z = 0$: on the other side, let us call U' the velocity which m would lose if its effective velocity were V', so that W would be the result of V' and U', it would necessarily follow that U' would be composed of U and of a velocity equal and directly opposite to V''; whence it evidently follows, that $U' - U$ or $d\ U = -V''$ cosine z''; therefore the equation $s\ m\ U\ V'$ cosine $z'' = 0$, found above, becomes $s\ m\ U\ d\ U = 0$ or $d\ s\ m\ U^2 = 0$.

Suppose, for example, two globes A and B striking each other obliquely, I demand their movements after the shock.

Suppose

Suppose that the velocity of A, estimated according to the line of the centres, was before the shock a, and after the shock V; that the velocity of B, also estimated according to the line of the centres, was before the shock b, and after the shock u; that the velocity of A, estimated perpendicularly to the same line, was before the shock a, and after the shock V'; finally, that the velocity of B, also estimated perpendicularly to this line of the centres, was before the shock b', and after the shock u'; this being done, by our proposition, the movement being necessarily geometrical, we must at first have $V = u$; thus the velocity lost by A, according to the line of the centres, will be $a - u$, and that lost by B, in the same direction, will be $b - u$: besides, in the direction perpendicular to the line of the centres, the velocity lost by A will be $a' - V'$, and that lost by B will be $b' - u'$; therefore $\sqrt{(a-u)^2 + (a'-V')^2}$ will be the absolute velocity lost by A, and that lost by B will be $\sqrt{(b-u)^2 + (b'-u')^2}$: therefore, according to the proposition, we should have $d\,[A\,(a-u)^2 + A\,(a'-V')^2 + B\,(b-u)^2 + B\,(b'-u')^2] = 0$, or $A\,(a-u)\,d\,u + A\,(a'-V')\,d\,V' + B\,(b-u)\,d\,u + B\,(b'-u')\,d\,u' = 0$, an equation which should generally take place, *i. e.* whatever be the values of $d\,u$, $d\,V'$, and $d\,u'$: therefore the co-efficient of each of these differentials must be equal to zero; which gives $V' = a'$, $u' = b'$, and $u = \dfrac{A\,a + B\,b}{A + B}$. Q. E. D.

It is clear that this proposition contains all the laws of the shock of hard bodies, whether this shock be immediate, or be made by means of any machine, since it assigns the character under which we recognise, among all possible movements, that which should really take place at each instant: this principle has a considerable analogy with that found by M. Maupertuis, and by him called *principe de la moindre action.* See his "Essai de Cosmologie."

Second Corollary.

XXIV. *In the shock of hard bodies, whether some of them are fixed, or all moveable, or (what comes to the same thing) whether*

whether the shock be immediate, or given by means of any machine without spring, the sum of the active forces before the shock is always equal to the sum of the active forces after the shock, plus the sum of the active forces which would take place if the velocity which remains to each moveable body were equal to that which it has lost in the shock.

That is to say, we must prove the following equation $s\,m\,W^2 = s\,m\,V^2 + s\,m\,U^2$. Now it is easily deduced from the fundamental equation (E); for W being the result of V and U, it is clear that W V and U are proportional to the three sides of a certain triangle: thus by trigonometry we have $W^2 = V^2 + U^2 + 2\,V\,U$ cosine z: therefore $s\,m\,W^2 = s\,m\,V^2 + s\,m\,U^2 + 2\,s\,m\,V\,U$ cosine Z: now by the equation (E) we have $s\,m\,V\,U$ cosine $Z = 0$; therefore the preceding equation is reduced to $s\,m\,W^2 = s\,m\,V^2 + s\,m\,U^2$. Q. E. D.

We see, therefore, as has been said (XXI), that by this transformation the analogy of the equation (E) with the preservation of the active forces becomes striking: we may also easily demonstrate the one by the other, as we shall see in XXVI.

The analogy of this same equation with the preservation of the active forces in a system of hard bodies the movement of which changes by insensible degrees, is still more evident, since it then regards a case peculiar from that we have examined; it is in fact visibly the particular case where U is infinitely small, and therefore U^2 is infinitely small of the second order; this reduces the equation to $s\,m\,W^2 = s\,m\,V^2$: but this preservation will be explained more at length in the following corollary.

Third Corollary.

XXV. *When any system of hard bodies changes its movement by insensible degrees; if, for a moment, we call* m *the mass of each of the bodies,* V *its velocity,* p *its vis motrix,* R *the angle comprehended between the directions of* V *and* p, u *the velocity which* m *would have if we made the system take any geometrical movement,* r *the angle formed by* u *and* p, y *the angle formed by* V *and* u, d t *the element of the time; we shall have these two equations:*

$$s\, m V p\, d\, t \text{ cosine } R - s\, m\, V\, d\, V = 0:$$
$$s\, m\, u\, p\, d\, t \text{ cosine } r - s\, m\, u\, d\, (V \cos y.) = 0.$$

Demonstration.—In the first place, $p\, d\, t$ cos R is visibly the velocity which the vis motrix p would have impressed upon m in the direction of V, if this body had been free: besides, d V is the velocity which it would in reality receive in the same direction; therefore $p\, d\, t$ cosine R — d V is the velocity lost by m in the direction of V, in virtue of the reciprocal action of the bodies: it is therefore this quantity that we must put for U cos. Z in the fundamental equation (E), which becomes by this substitution $s\, m\, V\, p\, d\, t$ cosine R — $s\, m\, V\, d\, V = 0'$, being the first of the two equations which we had to demonstrate.

Secondly, $p\, d\, t$ cosine r is the velocity which the vis motrix p would have impressed upon m in the direction of u, if this body had been free; besides, V cosine y being the velocity of m in the direction of u, d (V cosine y) is the quantity which this velocity estimated in the same direction augments: therefore $p\, d\, t$ cosine r — d (V cosine y) is the velocity lost by m in the direction of u, in virtue of the reciprocal action of the bodies: it is therefore this quantity which we must put for U cosine z in the second equation (F), which becomes by this substitution $s\, m\, u\, p\, d\, t$ cosine r — $s\, m\, u\, d$ (V cosine y) = 0, which is the second of the two equations we had to demonstrate.

These equations are therefore nothing else than the fundamental equations (E) and (F) applied to the case where the movement changes by insensible degrees, and therefore they contain all the laws of this movement: we may remark also, that the first of these two equations is only a particular case of the second, for the same reason that the equation (E), whence it is extracted, is contained in that (F) whence the second is extracted. But this first equation $s\, m\, V\, p\, d\, t$ cosine R — $s\, m\, V\, d\, V = 0$ deserves particular attention; because it contains the famous principle of the preservation of active forces in a system of hard bodies the movement of which changes by insensible degrees: thus:

Let

Let us first call $d\,s$ the element of the curve described by the corpuscle m during $d\,t$; this being done, we shall have $V\,d\,t = d\,s$; and therefore the preceding equation assumes this form $s\,m\,p\,d\,s$ cosine $R - s\,m\,V\,d\,V = 0$. Now let us suppose for a moment that the curve described by m is an inflexible line, that m is a moveable grain interwoven with this curve, that it traverses it freely, *i. e.* without being pressed by the re-actions of the other parts of the system, that it experiences at each point of this curve the same vis motrix as that with which it was animated in the first case; and that, finally, in this first case the initial velocity of m is K, while in the second it will be null at the first instant, and V' after an indeterminate time t: this being done, by integrating the preceding equation, in order to have the state of the system at the end of the time t, we shall have for the first case $s'\,s\,m\,p\,d\,s$ cosine $R - s'\,s\,m\,V\,d\,V = 0$, s' designating the sign of integration relative to the duration of the movement, while s is the sign of integration relative to the figure of the system: now, $s'\,s\,m\,V\,d\,V = \frac{s\,m\,V^2}{2}$: therefore the equation may be placed in this form $s'\,s\,m\,p\,d\,s$ cosine $R - s\,m\,V^2 + C = 0$; C being a constant added to complete the integral: in order to determine it, we shall observe that at the first instant we have $V = K$ and $s'\,s\,m\,p\,d\,s$ cosine $R = 0$; therefore $C = \frac{s\,m\,K^2}{2}$; therefore $2\,s'\,s\,m\,p\,d\,s$ cosine $R - s\,m\,V^2$ $s\,m\,K^2 = 0$: by the same reasons we have for the second case $2\,s'\,s\,m\,p\,d\,s$ cosine $R - s\,m\,V'^2 = 0$, without a constant, because we suppose V' as null at the first instant: taking away therefore this equation from the preceding one, reducing and transposing, we have $s\,m\,V^2 = s\,m\,K^2 + s\,m\,V'^2$; that is to say, *in any system of hard bodies the movement of which changes by insensible degrees, the sum of the active forces at the end of any given time is equal to the sum of the initial active forces, plus the sum of the active forces which would take place if each moveable particle had for its velocity that which it would have acquired by freely traversing the curve it had described, and supposing besides that it had been*

been animated at each point of this curve, with the same vis motrix which it there really experiences, and that its velocity at the first instant had been null.

It is this proposition which we call the principle of the preservation of active forces; and whence we may conclude that,

In a system of hard bodies the movement of which changes by insensible degrees, and which are not animated with any vis motrix, the sum of the active forces is a constant quantity, i. e. *the same for every instant.*

For in this case we have by hypothesis $p = 0$, which gives $V' = 0$, and therefore $s\,m\,V^2 = s\,m\,K^2$; an equation besides which is extracted immediately from that $s\,m\,p\,V\,d\,t$ cosine $R - s\,m\,V\,d\,V = 0$, found in XXIV, which, on account of $p = 0$, is reduced to $s\,m\,V\,d\,V = 0$, the integral of which completed is $\frac{1}{2}\,s\,m\,V^2 = \frac{1}{2}\,s\,m\,K^2 = 0$; whence follows the equation $s\,m\,V^2 = s\,m\,K^2$. Q. E. D.

[To be continued.]

LX. *On Chemical Nomenclature. By a Correspondent.*

To Mr. Tilloch.

SIR,

THE importance of an accurate and scientific nomenclature being now admitted by every lover of chemistry, I shall make no apology for suggesting what I consider an improvement. The metalline salts are named after a plan which is extremely defective. It proceeds upon the supposion that no more than two oxides of any metal can combine with the same acid. The salt whose base is the first of these oxides is named as if there were no oxide present: thus, the protoxide of iron and sulphuric acid form what is called sulphate of iron. On the other hand, the salt which contains the second of these oxides is known by *oxy* being prefixed, as in the oxy-sulphate of iron.

This mode of nomenclature is objectionable on several accounts.

1st. It is extremely deficient in the extent of application, as it makes not the least provision for those metals which combine with three and four proportions of oxygen; consequently there are no names for those salts which contain oxides intermediate between the minimum and the maximum. For instance, late experiments have shown that no less than *three* oxides of iron combine with the sulphuric acid. The first of these combinations is called sulphate of iron, the last is called oxy-sulphate; and for the second there is no chemical name.

2dly. It is evident that great confusion must arise from the want of some distinction between a superfluity of oxygen in the acid, and a maximum in the base. Am I to suppose that the oxy-prussiate of iron is the peroxide combined with prussic acid;—or must I conclude that it is the metal in an inferior degree of oxidation combined with oxy-prussic acid? Indeed, by pursuing the present nomenclature, we may soon expect to hear of oxy-oxy-prussiates and oxy-hyper-oxy-muriates, when there are at the same time an excess of oxygen in the acid and a maximum in the base.

3dly. When any metal has more oxides than two, the present nomenclature leaves us totally unable to distinguish the particular oxide united to the acid. Thus any one would suppose that the nitrate, sulphate and hyperoxymuriate of lead, each contained the same oxide base; yet we find these salts severally containing the prot-, deut-, and per-oxides.

In short, to these capital defects in nomenclature I ascribe the slow progress which has hitherto been made in our knowledge of the metallic salts, and I conclude that some improvement is absolutely necessary. That which I would suggest has at least the advantage of clearness and simplicity. I would carry into the nomenclature of these salts, Doctor Thomson's mode of designating the metallic oxides. For nitrate of lead, I would say nitrated protoxide of lead; and for oxy-nitrate, I would say (if such salts can be formed) nitrated deutoxide, tritoxide, or peroxide, according to the degree in which the metal may happen to be oxided. By this change every possible variety of these salts is provided with a name, clearly distinguishing the degree to which the base is oxided, and totally free from all the obscurity of the method now in use. Yours, E. B.

P. S. As Mr. Davy's late experiments hold out to us the prospect of decomposing several substances hitherto ranked with simple bodies, I would beg leave to suggest the propriety of adopting names for them as nearly as possible allied to the nomenclature in use. I would call the base of the muriatic acid, muria; that of the boracic, borax; and that of the fluoric, fluor. We should not object to these words, that they will be unmeaning in their new application, and that some of them have been used before to signify other substances. We should recollect that words are only the *signs* of things, and possess no other relation to them than that which is derived from custom: we should also remember, that the words borax and fluor, though formerly used, are no longer chemical terms, and may therefore without impropriety be applied to any new substance. Upon this principle, I have always lamented that the base of nitric acid was not called nitre, instead of azote or nitrogen; for, if the acid were named regularly after either of these, it would be the azotic or nitrogenic, and not the nitric, acid.

LXI. *Account of the Manufactures carried on at Bangalore, and the Processes employed by the Natives in Dyeing Silk and Cotton.*

[Concluded from p. 272.]

The weavers of Bangalore seem to me to be a very ingenious class of men, and, with encouragement, to be capable of making very rich, fine, elegant cloths of any kind that may be in demand: but, having been chiefly accustomed to work goods for the use of the court at Seringapatam, they must now labour under great disadvantages; for it never can be expected that the court of Mysore should equal that of Seringapatam, nor will the English officers ever demand the native goods so much as the Mussulman *sirdars* did. The manufacturers of this place can never, therefore, be expected to equal what they were in Hyder's

 reign,

reign, unless some foreign market can be found for the goods. Purnea, very desirous of the re-establishment of this city, has forwarded by me the musters of cotton and silk cloth that accompany this account, with a request that they may be presented in his name to the marquis Wellesley: and I beg leave to recommend, that the attention of the board of trade may be directed to them, with a view of forming some commercial arrangements that may assist in restoring a country which has suffered so much.

The silk manufacture seems especially favourable for a country so far from the sea and from navigable rivers; as long carriage, on such a valuable article, is of little importance. At present all the raw material is imported: but I see no reason why it might not be raised in Mysore to great advantage. Tippoo had commenced a trial; but his arbitrary measures were little calculated to ensure success. Some of the mulberry-trees, however, that remain in his gardens show how well the plant agrees with this climate. It is true, that the experiments hitherto tried below the Ghauts have not been favourable: but much resolution and patience are always required to introduce any new article of cultivation; and I suspect that the climate here, owing to its being more temperate, will be found more favourable than that of the lower Carnatic.

There is a small duty levied here on every loom; and it is judiciously diminished to those who keep many, in order to encourage men of wealth to employ their capital in that way. A man who has one loom pays, annually, $3\frac{3}{4}$ *fanams* (2*s*. $6\frac{1}{4}$*d*.); two looms pay 5 *fanams* (3*s*. $4\frac{1}{4}$*d*.); and a man who keeps more than two looms pays only for each two *fanams*, or 1*s*. 4*d*. All shopkeepers pay similar trifling duties.

There is here a set of people called *Rungaru*, who act as tailors, cloth printers, and dyers. Their printed cloths are very coarse, and the art among them is in a very imperfect state. The only two colours that they can give in printing are red and black. Their process is as follows:

The cloth that is to be printed is kept all night in a mixture

ture of sheep's dung and water. Next morning it is washed, and then bleached the whole day in the sun, having water occasionally. At night it is again put into a mixture of sheep's dung and water, to which is added a little quick-lime. Next morning it is washed again, and then put into a cold infusion of *arulay myrobalans*, (*terminalia arula*, Buch. MSS.) mixed with some gum of the *dinduga* tree, (*Andersonia Panchmoum*, Roxb. MSS.). The quantity of *myrobalans* for 12 cubits of cloth is 6 *dudus* weight ($2\frac{426}{1000}$ ounces), and of gum two *dudus* weight ($12\frac{943}{1000}$ drachms). The cloth, after being thoroughly wet in this, is taken out, and dried in the sun. It is then folded, placed on a smooth plank, and well beaten with a stick, which serves instead of mangling.

The mordant for the red dye is as follows: Dissolve in one *seer* (68 cubical inches) of hot water, 6 *dudus* weight ($2\frac{426}{1000}$ ounces) of alum, and 12 *dudus* weight ($4\frac{852}{1000}$ ounces) of *dinduga* gum. This mordant is poured into a cavity that is made in a block of timber, and covered with four folds of country blanket well moistened with the *dinduga* mucilage. The wooden blocks for printing are moistened with the mordant, by applying their surfaces to the blankets. The cloth to be printed is laid on a table covered with four folds of old cloth, and the blocks are applied, and pressed down by the hand. It is then kept for eight or ten days.

If the printer wishes to add black to the pattern, the cloth must be again printed with the following mordant. Take 5 *seers* ($3\frac{033}{1000}$ lbs.) of iron dross, and 5 *seers* of old iron, put them into a pot containing rather more than two ale quarts ($2\frac{1}{2}$ *seers*) of hot *kanji*, or decoction of rice; then add half a *seer* ($4\frac{852}{1000}$ ounces) of *sugar-jagory*, and keep it six or seven days. Next add half a *seer* of *dinduga* gum rubbed up with a little *ghee* (boiled butter), and allow it all night to dissolve: the mordant is then fit for use, and is applied in the same manner as the other. After this the cloth requires only four days to dry.

After the mordants have been dried on it, the cloth must be taken to the tank, washed very well, by beating it on a

stone

stone for an hour, and then dried. In order to give it the colour, put a piece, that has received the mordants, into a pot, with 20 *seers* (about five gallons) of water of the kind called here salt, one half *seer* of *popli* bark, and one *dudu* weight ($6\frac{471}{1000}$ drachms) of castor oil; then boil it for two hours, all the while carefully stirring the whole. The cloth is then taken out, and dried in the sun. At night it is soaked in a mixture of sheep's dung and water, next morning washed, and then bleached all day. At night it is again put into the mixture of sheep's dung and water, and next day is again bleached. The operation is then finished by starching it with *kanji*. The black is a fixed colour, but the red is perishable.

With the *patunga* wood these *Rungaru* dye cotton cloth of a red colour, which is bright, but does not stand washing. It is said that the people of Madras have the art of fixing it. The process used by the *Rungaru* is as follows: Prepare the cloth by soaking each piece in a *seer* of water, containing six *dudus* weight of powdered *myrobalans*. Then dip it into two or three *seers* (about two quarts) of a decoction of *patunga* wood, in which have been dissolved two *dudus* weight of alum. Then dry the cloth in the sun. The operation must be repeated four or five times, until the colour be deep enough. The decoction of *patunga* is made as follows: Beat two *seers* ($1\frac{213}{1000}$lbs.) of *patunga* wood, put it into a pot with 20 *seers* (about 5 gallons) of water, and boil for six hours.

The *Niligaru* are another class of dyers, of the same cast with the potmakers, and derive their name from their dyeing with the *nila* or indigo. The whole of this dye that is used here comes from the lower Carnatic, or northern Circars. In order to make a vat, the *Niligaru* take ten *seers* ($6\frac{066}{1000}$lbs.) of indigo, ground with a little water to a fine powder; put it into a pot capable of containing 50 *seers* measure (or a little more than 12 ale gallons); and add a decoction of *tagashay bija*, or seed of the *cassia tora*, which is made as follows: Take 4 *seers* measure ($\frac{113}{1000}$ Winchester gallon) of the seed, and boil it for 6 hours in four or five *seers* of water (about an ale gallon). The boiled seed, as well as the decoction, must be put into the vat; and then there must be added 10 *seers* ($6\frac{067}{1000}$lb.) of powdered *soulu*, or impure soda, 12 *seers* ($7\frac{28}{100}$lb.) of quicklime, and two *seers* of the ley of pot-ash (137 cubical inches). The whole is then stirred with a stick, and the mouth of the pot is covered up. Every evening and morning, for four days, three *seers* (206 cubical inches) more of the ley must be added; and in the last portion must be put about the size of an apple of quicklime. The vat now rests for three days; when four or five *seers* of boiling water must be added to it, and the vat is then ready for dyeing. The ley of potash is prepared as follows: Burn to ashes the branches of the *calli* (*euphorbium tirucalli*), or of the *utrayena* (*achyranthes muricata*): of these ashes put 2 *seers* ($1\frac{213}{1000}$lb.) into a pot, in the bottom of which there is a small hole. The hole is covered with a small inverted cup, and that by some rice husks or chaff. Above these are put the ashes, and on them are poured by degrees 25 *seers*, or about 6 ale gallons of water, which filters through the hole in the bottom of the pot, and forms the ley. It must be observed, that the water used by the *Niligaru* is always either that called here salt, or that which is found in places abounding with calcareous *tuffa*.

The indigo vat having been prepared, an estimate is formed of the number of *seers* weight of cotton that it will dye. For every *seer* weight of cotton thread pass a *seer* measure of water through the pot containing the ashes, and in this weak ley dip the *seer* of cotton; wash it well, and then wring out the water. The solution of indigo is then divided into five equal parts. The thread is dipped, by *seers* weight at a time, into these pots, till the colour in each is exhausted; and what does not obtain a proper colour in the first, after being dried, receives repeated dips, until the colour arrives at the required intensity. The solution of indigo is kept for a month, and every night a little lime-water is added: this enables it to give some more colour, which next day is again exhausted by dyeing some more cotton. The colour given by one dip is called *mavi*, and is a sky-blue; that which is given by five dips in a strong pot, is of an intense colour, nearly approaching to

black,

black, and is in fact called black by the natives, among whom it is in great esteem.

From the weavers the *Niligaru* receive cotton, and silk thread dyed yellow with turmeric, and return it to them of a green colour, which it obtains by a dip in a weak pot.

At Bangalore, as well as in all the neighbouring country, *goni* is a considerable article of manufacture. It is a coarse but very strong sack-cloth, from 18 to 22 cubits in length, and from ½ to ¾ of a cubit broad; and is made from the *janupa*, or *crotalaria juncea*. It is divided into three kinds, which differ in value according to their strength and to the closeness of the fabric. The same people, who are a particular cast of men, cultivate the plant, and carry on the manufacture, until the *goni* be fit for sale; the price of the hemp cannot therefore be ascertained, as it is not sold in that state. The *goni-maker* hires from some farmer as much high ground as he thinks will raise a quantity of *janupa* sufficient to employ his family to manufacture in one year. The soil ought to be red or black, like the best kinds used for the cultivation of *ragy*. It is allowed no manure; and the seed is sown broad-cast on the ground, without any previous cultivation, at the season when the rains become what the natives call *male*, that is to say, when they become heavy. After being sown, the field is ploughed twice, once lengthwise, and once across; but receives no further cultivation. At other times the *janupa* is cultivated on rice-ground in the dry season; but it must then be watered from a canal, or reservoir. It requires four months to ripen, which is known by the seeds having come to full maturity. After being cut down, it is spread out to the sun, and dried. The seed is then beaten out by striking the pods with a stick. After this the stems are tied up in large bundles, about two fathoms in circumference, and are preserved in stacks, or under sheds. The bundles are taken out as wanted, and put in the water, at which time their bands are cut, and the stems being opened out are kept down to the bottom by stones or mud. According to circumstances, they require to be kept in the water from six to eight days. They are known to be ready, when the bark separates easily from the pith. It is then taken out of the water, and a man, taking it up by handfuls, beats them on the ground, occasionally washes them until they be clean; and at the same time picks out with his hand the remainder of the pith, until nothing except the bark be left. This is then dried, and, being taken up by handfuls, is beaten with a stick to separate and clean the fibres. The hemp is then completely ready, and is spun into thread on a spindle, both by the men and women. The men alone weave it, and perform this labour in the open air with a very rude loom.

Leather is tanned here by a class of people esteemed of very low cast, and called *Madigaru*.

To dress the raw hides of sheep or goats, the *Madigaru* in the first place wash them clean, and then rub each with the fourth part of a kind of soft paste, made of 6 *dudus* weight of the milky juice of the *yecada* (*asclepias gigantea*), about 6 *dudus* weight ($2\frac{426}{1000}$ ounces) of salt (muriate of soda), and twelve *dudus* weight of *ragy sanguty*, or pudding of the *cynosurus coracanus*, with a sufficient quantity of water. This paste is rubbed on the hairy side, and the skins are then exposed for three days to the sun; after which they are washed with water, beating them well on a stone, as is usual in this country. This takes off the hair. Then powder 2 *seers* ($1\frac{213}{1000}$lb.) of *arulay myrobalans*, and put them and one skin into a pot with 3 or 4 *seers* measure of hot water, where it is to remain for three days. The skin is then to be washed and dried.

This tanned skin is dyed black as follows: Take of old iron, and of the dross of iron forges, each a handful: of plantain and lime-skins, each five or six; put them into a pot with some *ragy kanji*, or decoction of *ragy*, and let them stand for eight days. Then rub the liquor on the skins, which immediately become black.

These skins may be dyed red by the following process: Take of ungarbled *lac* 2 *dudus* weight (about 13 drachms), of *suja cara*, or fine soda, 1 *dudu* weight, and of *lodu* bark 2 *dudus* weight. Having taken the sticks from the *lac*, and powdered the soda and bark, boil them all together in a *seer* of water ($68\frac{5}{8}$ cubical inches) for $1\frac{1}{4}$ hour. Rub the skin,

after

after it has been freed from the hair as before mentioned, with this decoction; and then put it into the pot with the *myrobalans* and water for three days. This is a good colour, and for many purposes the skins are well dressed.

The hides of oxen and buffaloes are dressed as follows: For each skin take 2 *seers* ($1\frac{218}{1000}$ lb.) of quick-lime, and 5 or 6 *seers* measure (about $1\frac{1}{3}$ ale gallon) of water; and in this mixture keep the skins for eight days, and rub off the hair. Then for each skin take ten *seers*, by weight, (about 6 lb.) of the unpeeled sticks of the *tayngadu* (*cassia auriculata*), and 10 *seers* measure of water (about $2\frac{1}{4}$ ale gallons), and in this infusion keep the skins for four days. For an equal length of time, add the same quantity of *tayngadu* and water. Then wash, and dry the skins in the sun, stretching them out with pegs. This leather is very bad.

The oil-makers at Bangalore are a very considerable class of people, and are of the kind that use two bullocks in their mill, of which a drawing is given (Plate VIII *). The mortar is a block of granite. This class of people are called *Jotyphanada*, or *Jotynagarada Ganagaru.* They express the following kinds of oil: *wull'-ellu, huts'-ellu, harulu, cobri, ipay*, and *hoingay.*

The *wull'-ellu* oil is expressed from two varieties or species of *sesamum* seed, called here *surugana* and *cari ellus.* They are the same with the *wullay* and *phulagana ellus* of Seringapatam. The first gives the least oil; but for the table it is esteemed the best of any in the country: the price, however, of the two kinds is the same. The mill receives at one time about seventy *seers* measure ($2\frac{42}{100}$ Winchester bushels) of *sesamum* seed; and, in the course of grinding, ten *Cucha seers* measure of water ($2\frac{78}{100}$ ale quarts) are gradually added. The grinding continues for six hours,

* A, A, (Fig. 1 and 2) the mortar, 3 feet 6 inches outside measure from *a* to *b*. The inside cavity is 2 feet wide. The height from the ground to the top of the mortar is 6 feet 9 inches from the ground, and the block of which it is made descends into the earth 6 feet 9 inches. The pestle B, B, is 5 feet 1 inch in length. The cross handle of the pestle C, is 3 feet 7 inches long, by which, with the help of a cord, the pestle is attached to the post D, 4 feet 8 inches long, fastened into the beam E, F, which measures 12 feet from E to G, and 5 feet 6 inches from G to F.

when the farinaceous parts of the seed, and the water, form a cake; and this having been removed, the oil is found clean and pure in the bottom of the mortar, from whence it is taken by a cup. Seventy *Pucka seers* ($2\frac{42}{100}$ Winchester bushels) of *surugana*, or 65 *seers* of *cari-ellu* seed ($2\frac{256}{1000}$ Winchester bushels), give 2 *Cucha maunds* (rather more than $5\frac{1}{3}$ ale gallons) of oil. The mill requires the labour of two men and four oxen, and grinds twice a day. The oxen are fed entirely on straw, and are allowed none of the cake; which is sometimes dressed with greens and fruits into *curry*, and at others given to milch cattle.

The *huts'-ellu* is managed exactly in the same manner as the *sesamum*. The seventy *seers* measure require a little more water than the other *ellu*, and give 65 *seers* of oil (or a little more than $4\frac{1}{2}$ gallons). This also is used for the table. The cake is never used for *curry*, but is commonly given to milch cattle.

The *harulu*, or castor oil, is made indifferently from either the large or small varieties of the *ricinus*. It is the common lamp oil of the country, and is also used in medicine. The oil made by boiling is only for family use; all that is made for sale is expressed in the mill. To form the cake, seventy *seers* of the seed require only five *seers*, *Cucha* measure ($1\frac{30}{100}$ ale quarts), of water, and give 60 *seers* ($4\frac{17}{100}$ ale gallons) of oil; which, after being taken out of the mill, must be boiled for half an hour, and then strained through a cloth. The cake is used as fuel.

Cobri oil is that made from the dried kernel of the coconut, which is called *cobri*. This oil is chiefly used for anointing the hair and skin. Cakes are also fried in it, and it is sometimes used for the lamp. The mill receives 6 *maunds* weight of the *cobri* (almost 93 lb.), and 11 *Cucha seers* measure of water (a little more than 3 ale quarts). This produces three *maunds* (about $7\frac{8}{10}$ ale gallons) of oil. The natives eat the cake dressed in various ways.

The *ipay* oil, made from the fruit of the *bassia longifolia*, is used for the lamps burned before the gods, being esteemed of a better quality than that of the *ricinus*. The mill takes

70 *seers*

70 *seers* measure, and the seed requires to be moistened with 12 *Cucha seers* ($3\frac{1}{2}$ ale quarts) of tamarind water, in which 2 *seers* of tamarinds have been infused. The produce is 70 *seers* ($4\frac{365}{1000}$ ale gallons) of oil. The cake is used as soap to wash oil out of the hair of those who anoint themselves.

The *hoingay* oil, produced from the seed of the *robinia mitis*, is used for the lamp; but it consumes very quickly. It is also used externally in many diseases. Take 70 *seers*, *Pucca* measure, of the seed freed from the pods, add 4 *Cucha seers* measure of water ($1\frac{11}{100}$ ale quart), and beat them in a mortar into a paste. Then tread the paste with the feet; and, having kept it for two or three days, dry it in the sun. It is then put into the mill with one *Cucha seer* ($19\frac{6}{10}$ cubical inches) of water. It produces 40 *seers* ($2\frac{3}{4}$ ale gallons) of oil. For fuel, the cake is mixed with cow-dung.

The English weight, to which all the native weights are reduced, is the pound avoirdupois.

LXII. *Description of the Bermuda Islands, and particularly the Island of St. George. Addressed to the Directors of the French Museum of Natural History, by* M. A. F. MICHAUX, *temporary Agent of the French Imperial Administration of Woods and Forests in North America**.

I EMBARKED at Bourdeaux on the 5th of February, 1806, for the United States; my voyage having for its object to collect and transmit to the administration in the department of woods and forests, a great quantity of seeds and plants of such forest trees as might be naturalized in France, or succeed in those uncultivated districts where our own indigenous trees refuse to grow. On the 23d of March the American vessel, on board of which I was, fell in with the Leander, an English man-of-war, commanded by captain Whitby, who, suspecting our cargo to belong to French merchants, sent the ship to Halifax, in Nova Scotia. I

* From *Annales du Muséum d'Histoire Naturelle*, tome viii. p. 356.

was

was the only one of all the passengers who was ordered on board the Leander, where I remained for 43 days, during which time the cruize lasted. This disagreeable event removed me more than 600 leagues from Charlestown; but it gave me an opportunity of visiting the Bermuda Islands, where the Leander anchored on the 7th of April, to take in water. We remained there eight days, and I obtained permission from captain Whitby (who always treated me with the utmost politeness) to go on shore frequently: upon these visits I made the observations I am about to communicate*.

The number of islands composing the Archipelago of the Bermudas is so considerable, that the inhabitants say they are equal to the days of the year. The largest are only from 12 to 13 miles long. The smallest look like lime rocks just rising above the surface of the sea. The whole occupies an extent of about 35 miles in length by 20 or 25 broad. Towards the north immense strata of rocks extend from 30 to 40 miles, rendering the approach of vessels dangerous.

These islands, although much lower than the Azores, present nearly the same appearance at a distance, and resemble long and high ridges of hillocks covered with a darkish verdure. They are not surrounded by a flat and sandy beach like the Floridas, but skirted by high rocks, against which the waves are continually breaking.

The island near which the English ships of war generally anchor is called St. George's, which is also the name of the chief town. The town of Hamilton is in another island, fifteen miles off; these two are the only towns in the Bermudas. There are no houses so close together in any other place as to entitle them even to the name of villages.

St. George's island is situated at the north end of the Archipelago, and it was the only one on which I landed. It is of the second class in point of size, being nine miles long by three broad in some places, and only a quarter of a mile in others. The straights, which separate its southern shores from the islands of St. David, form the harbour, and

* M. Michaux was humanely released at Halifax, by captain Whitby, and proceeded to New York.—*French Editor.*

its

its entrance is strongly barricadoed by the projecting point of another island. It is edged round with blackish rocks, varying in height from 5 to 25 feet. When viewed at a distance, these rocks resemble a long hillock, the inequalities in which constitute so many small valleys. Upon the heights the soil is dry and sandy, and frequently the bare rock is seen : in the low grounds, on the contrary, the earth is a brown clay slightly moistened, and its vigorous vegetation announces its extreme fertility.

Three fourths of the island are covered with wood ; the rest is partly cultivated, or so barren that it is not susceptible of cultivation.

The plants peculiar to the island are few in number : and although my journeys through the island were very rapid, I think I may safely affirm that the number of species does not exceed 140 or 150. Among these plants we find several belonging to the antient continent, which do not seem to have been of a nature to occasion them to be transplanted here : these are, the *verbascum thapsus*, *anagallis arvensis*, *mercurialis annua*, *leontodon taraxacum*, *plantago major*, *urtica urens*, *gentiana nana*, *oxalis acetosella*, &c. We also find here the great cabbage palm tree, *chamærops palmeto*, and the *rhus toxicodendrum* of North America. As to other plants, I could only ascertain a small number of them ; but I collected seeds of all those which had been preserved the year before, among others a strawberry plant, the aromatic flowers of which resemble sage, and it is on this account called *sage-bush* by the inhabitants ; a beautiful species of *verbena*, and a small *medicago*, each foot of which scarcely occupies an inch of ground ; this is the most common plant in the country, forming almost the whole of the verdure every where ; the surface of the ground not being, as in Europe and the United States, covered chiefly with the grasses, of which last there are very few kinds in the Bermudas.

The *juniperus Bermudiana*, called by the inhabitants *cedar*, is the only forest tree in these islands : the whole are nearly covered with them ; and it is this tree which, when seen in clumps at a distance, gives a dull and sombre ap-

pearance

pearance to the islands. It grows throughout the whole island, in every kind of exposure; but in the valleys its vegetation is more vigorous than upon the summit of the hills, and the primordial branches attain to a great height. Its elevation does not exceed 40 or 50 feet, and its diameter is from a foot to 15 inches. Although the branches have a tendency to expand from the trunk, those of the full-grown trees touch each other; which may give an idea of the distance at which they are placed. Upon the heights, and in places which having been recently laid bare are replenished with young trees from the seeds of the old, one fourth of the young trees forms a thicket; the branches shoot out very close to the earth, and extend eight or ten feet around the parent tree.

These cedar trees are not felled at any regular season; they cut down a tree whenever they think it of a sufficient size for their purpose, leaving it to nature to fill up the vacancies made by these occasional removals; and to this improvidence may in a great measure be ascribed the high price to which the wood of these trees has risen.

It was the flourishing season when I was at the Bermudas. The female trees were discernible at the distance of 15 or 20 paces by the darker colour of their foliage: the seeds are ripe about the end of October, but they fall during winter; so that I could find only a very few upon the trees. I am very anxious to see them cultivated, and have no doubt that this tree would be a valuable acquisition, either for the island of Corsica, or for some parts of our departments of the South near the Mediterranean. They make a syrup of these seeds, said to be extremely useful in certain pulmonary complaints.

The *juniperus Bermudiana* is very much esteemed, on account of the quality of its wood*; it is fine grained, compact, and more resinous than the *juniperus Virginiana*. As in the former species the sap is only five or six lines thick in a tree of 12 or 14 inches diameter, this wood is employed

* We may judge of its colour and smell from the black-lead pencils called English crayons: the juniper tree of the Bermudas and that of Virginia are equally employed in their fabrication.

in building sloops, which has been the principal branch of the industry of the Bermudians for a long period. These vessels are remarkable for their quick sailing, and last for a long time. It is said, however, that they are more liable to go to pieces than oak vessels when they strike. Six luggers, or cutters, from 120 to 140 tons, and copper-bottomed, have been built at the Bermudas by order of the English government.

The cedar tree is the sole riches of the inhabitants; and the fortune of every individual is estimated by the number of trees he possesses. They are sold on the ground at a guinea each.

I was told there were no quadrupeds natural to the country. The only birds I saw in the woods were, the cardinal, *loxia cardinalis*, and the blue bird, *motacilla sialis*, which they say belongs to the continent of North America.

Every year, in the months of March and April, the Cachalot whales approach very near the shores, where some of the inhabitants, but particularly the men of colour, fish for them.

The shell-fish which are most common belong to the genera of *turbo*, *donax*, and *mytilla*. The latter are very abundant, and are only 5 or 6 lines long.

Agriculture, which is now entirely neglected in the Bermudas, once flourished there, as is proved by the records of the custom-house, which mention the quantity of sugar and wines annually exported from the colony. The present inhabitants employ the small number of negroes they possess in cultivating pot-herbs and maize, and in feeding poultry. They have very few cattle or horses, and I saw only about a dozen cows on the island, which were deriving a scanty maintenance from the *medicago* I have mentioned. In the country there are enclosures which might form a better sort of pasturage, but they are all planted with cedar trees. Provisions of every kind are so rare, and so dear, that the ships of war which are constantly arriving at the Bermudas can only procure potatoes and onions.

There is but one kind of stone in the island, which is found in great abundance a few feet below the surface.

Upon

Upon coming out of the quarry it is very white, and so tender that it may be reduced to powder between the fingers; when exposed to the air it becomes of a deep gray and indurates. When seen through the microscope it appeared to me as composed of a very fine sand and of shells. Two quarries are wrought near the town, in each of which eight or ten negroes or mulattoes are employed, who earn from a piaster to a piaster and a half daily. Their labour is easy: the stones when detached from the mass are sawn into pieces of about one or two feet broad by six or eight inches thick.

Neither at St. George's island nor at any other of the Bermudas are there any springs or rivulets, and experience has shown that wells cannot be dug; rain water, therefore, is alone made use of, which, from the precautions used, is not only sufficient for the inhabitants, but also for supplying the ships of war which put in there for the purpose.

About 100 yards from the sea, there are constructed upon an inclined plane two immense terraces of a triangular form, destined to receive the rain water, which flows into cisterns, around which the empty casks are rolled, and filled by means of hand pumps.

The walls of these terraces are of mason work; and although each occupies a space of 450 or 500 fathoms, they are not always sufficient for the supply of the shipping. The distance of these government cisterns from the town is about a mile. The road to them is eight or ten feet broad, and is shaded by cedar trees. Ships of war of the first and second rate cannot enter the harbour, but are anchored one or two miles off.

The town of St. George's has only 250 or 300 houses. It is intersected by a dozen of narrow streets not paved, and one of which only admits of a carriage to pass: the houses, which mostly consist of only one story above the ground, are generally coloured yellow. The whole are of stone, and covered with tiles, with a gutter round the roof to receive the rain water: this roof, which is painted white, reflects the rays of the sun so strongly as to be very injurious to the eyesight.

Several houses have small gardens, the walls of which

are

are covered with the *cactus opuntia.* The most common pot-herbs, however, are alone cultivated in them. In some I have seen the *carica papaya,* the *melia azedarach,* the *bananier,* and the *geranium roseum* and *zonale.*

We meet with very few people in the streets, and the inhabitants seem to be extremely indolent. There are only five or six shops in the town, where spices, trinkets, and cloths, are sold at a very high price. The Americans import into the place planks, maize, flour, butter, and some other provisions, for which they receive ready money. The money of the country is the heavy piaster.

They estimate the population of the Bermudas at eight or nine thousand souls. I do not know the proportion of whites to negroes, but the latter are said to be more numerous. The lower classes are accused of misleading ships in stormy weather, in order to pillage those who have the misfortune to be thrown on their shores; and the Bermudian pirates have always been proverbial for their barbarity.

These islands are said to be very healthy; which cannot be doubted from their situation.

LXIII. *Facts upon which to found a History of Cobalt and Nickel. By M.* Proust. *Extracted by M.* Chevreuil*.

The sulphuric, muriatic, and nitric acids oxidize metals in the same manner. There is a disengagement of hydrogen with the first two.

Sulphates.—Of these there are two; the one simple, and the other tripled by some neutral salt with a base of potash or ammonia.

1st. The simple sulphate has a slightly pungent taste, a little bitter, and somewhat metallic. Its crystals, not voluminous, are heaped up sections of irregular octaëdrons: they are of a gooseberry red, unalterable in the air; they lose 42 hundredth parts of water upon distillation;

* From *Annales de Chimie,* tom. lx. p. 260.

they are then red coloured and opaque. In this state they can support a red heat without being decomposed, except in those points at which they touch the retort.

2d. When we mix sulphate of potash with the foregoing, we obtain more voluminous crystals, which are rhomboidal cubes. This triple salt is less soluble than the simple; it only loses 0,26 of water upon distillation.

Carbonate.—The carbonate of potash gives from 0·40 to 0·42 of carbonate of cobalt with the simple sulphate. An excess of alkali dissolves a great part of the precipitate. Ebullition and cold water decompose this solution.

Oxide at the minimum.—100 parts of carbonate leave, after the separation of the water and carbonic acid, from 0·60 to 0·62 of greenish gray oxide. In order to have it very pure, we must charge the retorts as full as possible, and heat them gradually. Without these precautions, we obtain an oxide mixed with oxide at the *maximum*, which, in this case, gives oxygenated gas with the muriatic acid, while that which is pure does not give an atom of it.

The gray oxide is dissolved with effervescence in the nitric acid, without giving nitrous gas: when heated in the open air, it becomes black immediately. We easily discover an oxide, some parts of which are raised to the *maximum* by the application of a weak acid, which dissolves only the minor oxide. Ammonia operates the same separation as Thenard remarked.

Oxide by Precipitation.—1st. Some drops of nitrate of cobalt, poured into boiling water with a little potash in it, give a blue precipitate, which at last becomes red if the ebullition be continued: in this case a hydrate is formed!

2d. If we employ alkalized water cold, the blue precipitate is formed; but in place of making a hydrate it passes to the green, without the contact of the air being able to obscure its shade: it preserves this colour after being dried.

3d. If we boil this green precipitate, while it is fresh, in water with a little potash, it becomes a reddish gray, and does not change any more.

The weak acids, vinegar for instance, dissolve the first

precipitate

precipitate totally: applied to the other two, they separate black oxide from them. Lastly, the blue oxide does not give any gas with the muriatic acid, while the green does.

From this we must conclude, that the blue oxide is oxygenated at the expense of the air contained in cold liquids, and that the green oxide is a mixture of *blue oxide* and *black oxide*. M. Proust thinks nevertheless, that there is something more than a simple mixture; for the blue and black colours would not give this shade of grass green, which distinguishes it from every other oxide. Nothing but a true combination can yield a colour foreign to that of its components, and hinder the action of the air from elevating to the *maximum* the portion of blue oxide which forms part of the green precipitate. In order to oxidize this precipitate completely, we must dry it by means of heat, as Thenard has shown.

The reddish gray precipitate, in the third experiment, is a mixture of hydrate and black oxide.

It is only the *minimum* oxide that can be combined with the acids: the green oxide is never obtained from any solution, and cannot become the base of any saline combination.

Ammonia and Oxide of Cobalt.—The gray oxide put into a well-closed flask, along with ammonia, communicates to it a slight red colour, which does not become higher, however long the flask is kept: this oxide is therefore but very difficultly soluble in ammonia. But if the flask remain uncorked the ammonia is very quickly coloured, because it attracts carbonic acid from the air. We may operate this solution in a very short time, by placing the flask in a large bason, in which we put a salt of carbonic acid.

If we only saturate the ammonia with acid, the solution is that of the oxide in the *carbonate of ammonia*. If we continue to make the carbonic acid pass, we obtain a solution of *carbonate of cobalt in the carbonate of ammonia*. This solution, when kept in a flask full and corked, deposits crystals of metallic carbonate; it abandons a part of them by the addition of water: an excess of volatile alkali redissolves this precipitate. We may make this solution very speedily, by throwing carbonate of cobalt into carbonate of ammonia.

If we put *pure ammonia* upon carbonate of cobalt in excess, this alters the case completely. The carbonate of cobalt is divided into two parts: the one yields its acid to the ammonia and becomes hydrate, which is precipitated to the bottom of the vessel, while the portion not decomposed is dissolved into alkaline carbonate.

Here we have already two kinds of ammoniacal solutions of cobalt. There is a third discovered by Tassaert, but in general very little remarked hitherto. We obtain it by putting hydrate well washed, or blue oxide, into a flask full of ammonia and well closed. The solution is made in 24 hours. It is red like the former; but differs from them in this respect, that if we pour a drop of it into boiling water, blue oxide is immediately precipitated: when we operate with cold water we obtain green oxide. If ammonia dissolves the hydrate of cobalt, or the fresh blue oxide, more easily than the gray oxide, it is because the two former are in very minute division.

Distillation of the ammoniacal Solution.—When we distil the solution of carbonated cobalt, carbonate of ammonia passes over; the liquor in the end deposits an oxide at first of a dirty green, but which afterwards becomes black. This is a mixture of gray oxide and black oxide.

How happens this hyper-oxidation? M. Proust merely states the fact, and abstains from explanation where data are wanting.

Hydrate of Cobalt.—The crystals of sulphate, or of nitrate, thrown into a flask full of liquid potash, and immediately closed, are there decomposed: a blue precipitate is formed, which passes to the violet, afterwards to the red, by becoming hydrate.

If we boil hydrate with potash, the latter dissolves oxide, and is tinged with a fine blue colour. This solution is decomposed upon the addition of water. In the air the oxide becomes black, and is deposited.

The hydrate is dissolved cold in the carbonate of potash, and tinges it red. The oxide is not dissolved.

The hydrate of cobalt is of the colour of a dead rose leaf: the acids dissolve it with heat, and without effervescence.

The hydrate is not decomposed by ebullition, either in

pure

pure water or in alkalized water. It loses from 20 to 21 of water by heat, and is reduced to very pure gray oxide.

It keeps very badly under water: when it is in contact with the air it becomes black. The dry hydrate is better preserved, but attracts carbonic acid.

When we throw crystals of sulphate into a flask full of ammonia and immediately closed, they give a blue precipitate, which does not become red as in the potash. M. Proust asserts that the hydrate is formed, but that it is dissolved in some proportion in ammonia; so that it is the hydrate which colours the solution, and not the simple oxide.

Valuation of the Oxygen in the minor Oxide.—100 parts of gray oxide, reduced with proper precautions in a closed crucible, give $83\frac{1}{2}$ of metallic grains. The quintal of cobalt seems therefore to absorb 19 of oxygen, in order to become *minor oxide.*

Major oxide.—If we distil a nitric solution of cobalt, black crusts are deposited upon the sides of the retort, nitrous gas is disengaged, and we obtain from 125 to 126 of black oxide as the residue. Hence we may conclude, that the *maximum* of the oxidation of the cobalt exists about 25 or 26 in 100.

This oxide is not dissolved in the nitric and sulphuric acids, except by losing the portion of oxygen which constituted its *maximum.*

It gives oxygen gas with the muriatic acid.

It is insoluble in ammonia and potash.

The black oxide, heated for half an hour at the bottom of a crucible, again becomes gray oxide by losing its oxygen: we may then tinge the vitrescible matters blue.

Messrs. Proust and Thalaker found the black oxide at Pavias, in a journey to Valentia. It is also found in cobalt ores, which have been called *vitreous* or *black ores.*

The carbonate and hydrate of cobalt is changed into black oxide, by the contact of the oxy-muriatic acid.

The nitrous and sulphurous acids dissolve the black oxide, and form with it nitrate and sulphate at the *minimum.*

Muriate of Cobalt.—The gray oxide is dissolved with heat in an acid of 15°. The warm or cold solution is of a deep

 blue;

blue; it crystallizes easily; the crystals are blue; it is the de-hydrated muriate. As soon as it absorbs humidity it becomes red.

The muriatic acid at 15° yields a great deal of gas with black oxide. This solution is green while it retains the gas; but as soon as it has lost it, it becomes blue. The blue traits of the muriate of cobalt, dried upon paper, are nothing else than de-hydrated muriate. When they are green, it is because the salt still contains muriate of nickel, which tinges it yellow, and forms green with the blue.

Its Distillation.—When brought to a red heat in a luted retort, those parts only which touch the glass are decomposed: the products then are muriatic acid in vapour, mixed with oxygenated acid. The glass is tinged blue. The non-decomposed muriate is sublimed, after being melted in gray flaky flowers; these undergo a kind of condensation, which renders them insoluble in water for at least 12 hours. Latterly, they give a solution of common muriate.

Arsenite and Arseniate.—The arsenite of cobalt is prepared by pouring a solution of cobalt well diluted into a solution of arsenite of potash. We obtain a red precipitate, which preserves this colour upon drying.

Character of the Arsenite.—1st. Heated in a tube closed at one end, it is decomposed; the oxide of arsenic is sublimed, and the glass is tinged blue.

2d. The nitric acid dissolves it, and there is nitrous gas.

3d. The muriatic solution is decomposed by sulphuretted hydrogen, which precipitates orpiment.

4th. Pure potash, with the assistance of heat, sets free the blue oxide.

Arseniate.—We obtain it by using arseniate of potash in place of arsenite. The precipitate is red, like the arsenite.

Characters.—1st. Heated in the tube, it does not give any sublimate; it becomes violet, without tingeing the glass.

2d. The nitric acid dissolves it without nitrous gas.

3d. Its muriatic solution is not disturbed by the sulphuretted hydrogen until two hours after the mixture.

4th. Pure potash sets free the blue oxide, and is combined with the acid.

The

The red efflorescences which we find upon minerals containing cobalt are formed of arseniate. M. Proust found the arsenite in the heart of some pieces only.

Hydro sulphuretted Oxide. Sulphuret of Cobalt.—The gray oxide, the hydrate, and carbonate, take from water the sulphuretted hydrogen, and become hydro-sulphuretted oxide. The latter is not dissolved in ammonia; it gives water and sulphurous acid upon distillation. The remainder is sulphuret.

The oxides when heated with sulphur become sulphuret. Cobalt absorbs 40 per cent. of sulphur. The author has still some doubts upon this subject.

Facts respecting the History of Nickel.

Nitrate.—100 parts of metal dissolved in the nitric acid, and distilled until perfectly decomposed, leave from 125 to 126 of greenish gray oxide at the *minimum*. The nitric acid cannot make this oxide pass to the *maximum*.

In order to ascertain the purity of the oxide of nickel, we must dissolve it in the muriatic acid and heat it. If it contains a little oxide of cobalt, there will be an extrication of oxygenated muriatic gas: if it be pure, none will be disengaged.

The gray oxide is dissolved in all the acids, and gives the same solutions as the metals.

Nitrate at the minimum.—By distilling the nitrate of nickel with the same precautions as the nitrate of copper, we obtain, as with the latter, a nitrate with excess of base, which is insoluble in water. 100 parts of nickel give 142 of this nitrate: on deducting the 25 parts of oxygen absorbed by the metal, we have 17 parts of acid fixed upon this oxide.

100 parts of dry nitrate of nickel gave upon distillation 20 of water, and 25 of oxide: therefore 55 of acid. These proportions are not rigorously exact, because the last portions of water are a little acid.

Muriate of Nickel.—This is a granulous crystallization of an apple green, and very deliquescent.

The traces of this salt, when dried upon paper, are yellow.

This muriate loses 55 of water. What remains is a yellow de-hydrated muriate, which again becomes green in the air, by absorbing water.

The de-hydrated muriate, when fire is applied to it, does not melt: those parts only which touch the glass are decomposed: there is then an extrication of simple muriatic acid and oxygenated acid: the salt not decomposed is sublimed under the form of pearl-like flowers of a golden yellow. These flowers in two days absorb humidity, and become green. The muriatic acid dissolves them with difficulty.

100 parts of muriate of nickel gave, by means of carbonate of potash, from 61 to 62 of carbonate; which supposes from 33 to 34 of oxide.

Sulphates of Nickel.—There are two, the one simple and the other potashed. The first crystallizes in hexaëdral prisms terminated by an irregular pyramid; the second in rhomboidal prisms.

The simple sulphate loses 46 parts out of the 100 of water. The de-hydrated residue again becomes green on absorbing humidity. When strongly heated for an hour, and at a red heat, in a luted retort, it is partly reduced to the state of sulphate with an excess of base: water takes away that part which has not lost its acid.

100 parts of this sulphate gave 64 of carbonate of a clear green.

The potashed sulphate loses 24-100dths of water. The residue acts like that of the simple sulphate. The potashed sulphate only gives from 27 to 28 of carbonate for 100.

The two sulphates of nickel are transparent, of a fine emerald green; they are unalterable in the air. M. Proust thinks that the sulphate of potash is united to that of the nickel in a constant proportion.

Extraction of the Nickel on a large Scale.—Let there be an ample solution of ore first calcined, and afterwards vitriolized with the residues of ether. It is requisite to separate the nickel from iron, copper, arsenic, bismuth, and cobalt. The iron is at the *maximum:* in this state it has little affinity for the acids. We may then precipitate it to the

the state of arseniate, by means of potash, which we must add gradually. Ammonia, or a prussiate, afterwards proves if all the iron has been precipitated.

Into the filtered solution we make a current of sulphuretted hydrogen to pass; the copper, bismuth, and the whole of the arsenic are precipitated in the form of sulphurets.

When the sulphuretted hydrogen occasions no more precipitate, we reduce the liquor in order to crystallize it. The potashed sulphate of nickel, less soluble than the potashed sulphate of cobalt, is the first to crystallize. On repeating the crystallizations, we succeed in separating the two salts: as to the last portions of the salt of nickel, washing them in cold water takes off the sulphate of cobalt they contain.

All these crystallizations require a bason of fine silver, if we wish to proceed smoothly.

We ascertain that a salt of nickel is pure, when the precipitate dissolved in ammonia abandons this solvent without our finding any cobalt in it.

When we precipitate a sulphate of nickel, we must not be too sparing of the potash: without this precaution we might run the risk of precipitating sulphate with excess of base; which would alter the purity of the precipitate.

Carbonate of Nickel.—100 parts heated in a retort give from 54 to 65 of greenish gray oxide at the *minimum.* When we heat it in contact with the air, the oxide is black.

The minor oxide becomes carbonate when exposed to the air.

Hydrate of Nickel.—All the salts of nickel when thrown into boiling potash are changed into green hydrate; boiling does not alter the shade of them. Potash neither dissolves the hydrate nor the oxide of nickel.

The hydrate heated is reduced to gray oxide.

The oxide is in the state of hydrate in the saline combinations. The alkalis precipitate it in this state.

Major Oxide of Nickel.—The carbonate and hydrate rise to the *maximum,* when we put them in contact with the oxygenated muriatic acid. It is more difficult to oxidize the gray oxide.

The

The dry major oxide of nickel is black; when in a mass its fracture is vitreous.

This oxide, preserved in ammonia, gives out bubbles, returns to the state of gray oxide, and is dissolved in the alkali.

It gives a considerable quantity of oxygenated acid, with a muriatic acid at 15°. The solution is greenish yellow: crystals are formed upon cooling.

The oxides of nickel are reduced like those of cobalt. They are melted in the same way, with this difference only, that the cobalt gives a larger globule.

This metal has taken a surcharge of sulphur from 46 to 100; but the author has still his doubts on this subject.

Arsenite and Arseniate.—They are formed like those of cobalt, and are of a fine apple-green colour. The arsenite heated in the tube loses its colour with water, sets at liberty some white oxide, and passes to the olive-green. Charcoal is necessary in order to take away all the arsenic.

When heated in a platina spoon, the arsenic is speedily dissipated. An oxide at the *minimum* remains.

The arseniate heated in a gun-barrel loses its colour with water; becoming of a hyacinth and transparent appearance: but at a red heat it passes to the clear yellow, and remains unalterable.

In the spoon the arseniate becomes white, reddens without melting, or emitting the smallest arsenical fumes: we must avoid flame in order to decompose it.

Recapitulation.

M. Proust concludes from the foregoing facts, and from those he has published in other memoirs, that cobalt, nickel, and most of the other known metals, have only two degrees of oxidation distinctly marked: he has not asserted, however, that a metal can only absorb two proportions of oxygen: he only says that it is not yet time to admit all the oxides hitherto spoken of, and in which we have neither seen the quantity of oxygen ascertained, nor the combinations which they are susceptible of forming with the acids; and he adds that colour is not a sufficient character by which to distinguish them.

There are only two metals which have as yet presented to the author more than two oxidations: these are tin and lead: notwithstanding this, the quantity of oxygen in the oxide of tin (the base of *aurum musivum*) is not yet known, nor that of the oxide in the nitrate of lead which has been boiled with plates of this metal.

It seems that the different oxides of one and the same metal may be intermediately dissolved, and form true combinations. Thus the green oxide of cobalt is a combination of blue and black oxide.

May not *minium* be a combination of brown oxide and of oxide at 9 in 100, and analogous to the foregoing?

Finally: all the magnetic ores of iron and the attractable sands are mixtures or combinations of this order: if this were not the case, what could prevent the minor oxide from rising to the *maximum?* The oxide of the gun-barrel which has served to decompose the water is also in the same case; it is formed of the two oxides.

LXIV. *The mean Motions of the Sun and Moon, of the Sun's Perigee, the Moon's Perigee and Node; the Times of their several Revolutions, both in respect to the Equinox and to the fixed Stars, and in respect to each other: deduced from the New Tables of the Sun and Moon lately published by the French Board of Longitude. By* James Epps, *Esq.*

To Mr. Tilloch.

SIR,

Should you think the enclosed paper deserving a place in your Magazine, it is at your service. It contains the result of very tedious though not difficult calculations; and, as it exhibits an interesting view of the modern solar and lunar astronomy, will, I think, prove acceptable to your astronomical readers. I am, sir, yours &c.

James Epps.

No. 4, Commercial Road,
May 14, 1808.

The

	Revolutions.	*Signs.*	*Degrees.*	*Minutes.*	*Seconds.*
The sun's mean motion in respect to the equinox, in 100 years, or 36524 days -	99	11	29	46	36·75
.... in one year, or 365 days - - -	—	11	29	45	40·36835
The sun's mean diurnal motion - - - -	—	—	—	59	8·3297763
The motion of the sun's perigee in respect to the equinox, in 100 years, or 36524 days - - -	—	—	1	43	11·
.... in one year, or 365 days - - -	—	—	—	—	61·8693188
The diurnal motion of the perigee - - - -	—	—	—	—	0·16950498
Hence, the motion of the sun's mean anomaly, in 100 years, or 36524 days -	99	11	28	3	25·75
.... in one year -	—	11	29	44	38·499
The diurnal motion of the sun's mean anomaly -	—	—	—	59	8·1602713
The moon's mean motion in respect to the equinox, in 100 years, or 36524 days	1336	9	24	42	8·2
.... in one year, or 365 days - - -	13	4	9	23	4·8755
The moon's mean diurnal motion - - - -	—	—	13	10	35·0270561748
The motion of the moon's perigee in respect to the equinox, in 100 years, or 36524 days - - -	11	3	18	56	44·4
.... in one year, or 365 days - - -	—	1	10	39	45·79
The diurnal motion of the perigee - - - -	—	—	—	6	41·05695979
Hence, the motion of the moon's mean anomaly, in 100 years - -	1325	6	5	45	23·8

And

	Revolutions.	Signs.	Degrees.	Minutes.	Seconds.
And the motion of the moon's mean anomaly, in one year, or 365 days -	13	2	28	43	19·085
The diurnal motion of the mean anomaly - - -	—	—	13	3	53·97009638
The retrograde mean motion of the moon's node, in respect to the equinox, in 100 years, or 36524 days -	5	4	14	8	31·4
.... in one year, or 365 days - - -	—	—	19	19	43·360557
The mean diurnal motion of the node - -	—	—	—	3	10·63934344
The mean diurnal motion of the moon from the sun -	—	—	12	11	26·697280
The mean diurnal motion of the moon from her node	—	—	13	13	45·666400

From the above mean motions are deduced the following revolutions:

	Days.	Hours.	Minutes.	Seconds.
The sun's mean revolution in respect to the equinox, called the mean astronomical, solar, or tropical year - - - -	365	5	48	51·58732
The sun's mean revolution in respect to the fixed stars, or the mean sidereal year; (supposing the mean annual precession 50″·1)	365	6	9	11·49648
The sun's mean revolution in respect to the perigee, or the mean anomalistic year - -	365	6	13	58·074

The entire revolution of the sun's perigee, in respect to the equinox, is therefore 7645793 days, nearly, or 209 Gregorian centuries and about 33 years.

Since the progressive mean motion of the perigee in one year, in respect to the fixed stars, is only 11″·7693188, the sidereal revo-

lution

lution of the perigee will therefore be more than five times that of the tropical.

	Days.	Hours.	Minutes.	Seconds.
The time required by the sun in passing over one degree of mean longitude - - - -	—	24	20	58·1433
The time required by the sun's perigee, in performing the same, is above 58 years, or more exactly	21238	7	31	42·3205
The mean periodical revolution of the moon in respect to the equinox - - - - -	27	7	43	4·64670
The mean sidereal revolution of the moon, or the revolution in respect to the fixed stars - -	27	7	43	11·47710
The mean synodic revolution, or the revolution of the moon to the sun, called a mean lunation	29	12	44	2·850426
The mean anomalistic revolution of the moon - - - -	27	13	18	33·3391153
The mean revolution of the moon in respect to her node -	27	5	5	35·6053
The mean revolution of moon's perigee in respect to the equinox	3231	11	4	6·69936
The sidereal revolution of the moon's perigee - - -	3232	13	36	41·26640
The mean revolution of the moon's node in respect to the equinox - - - - - -	6798	4	14	51·1556
The sidereal revolution of the node - - - - - -	6793	14	28	19·8703
Time of moon's describing 1° mean motion - - - - -	—	1	49	17·17978
Time required by moon's perigee in describing 1° motion -	8	23	25	50·68531
.... by moon's anomaly -	—	1	50	13·0926
.... by moon's node -	18	21	12	42·475

Mean motion of sun's anomaly during a synodic revolution, or one lunation, 29° 6′ 19″·2611066.

Mean

	Days.	Hours.	Minutes.	Seconds.
Mean motion of moon's anomaly for the same interval, 385° 49′ 0″·8186293.				
Mean motion of the moon from her node for the same interval, 390° 40′ 13″·9587160.				
Period of 12 mean lunations -	354	12	48	34·2051
Period of 223 mean lunations, for the restitution of eclipses -	6585	7	42	35·6450

LXV. *Mineralogical Account of the Island of Corsica; contained in a Letter from M.* RAMPASSE, *formerly an Officer in the Corsican Light Infantry, to M.* FAUJAS DE ST. FOND*.

I SHALL now endeavour to gratify your desire, communicated to me on my leaving Paris, of having some details of my mineralogical inquiries in Corsica, and particularly upon the orbicular granite of that island, of which a single isolated block only has yet been recognised.

In order to make myself master of the subject, it was necessary to visit the interior of the *Pieve d'Orezza*, and I first proceeded to reconnoitre the high mountain called Santo-Pietro-de-Rostino, from which proceed the enormous masses of quartz mixed with green *diallage*, with which the bed of the rivulet of the village of *Staxzona* is encumbered. I shall not at present enter upon a detail of the reasons which should incline us to reject the improper denomination of *verde antico di arezza*, which was at first given to this stone. After this visit, I wished to direct my steps to *Liamone* by the *Pieve de Caccia*; but the extremely warm temperature which then reigned hindered me, and it was not until the end of August following that I undertook this journey.

Before entering into the details of my excursion to *Liamone*, allow me to mention a new rock which I discovered in the *Niolo*: it is of a peculiar texture and composi-

* From *Annales du Muséum d'Histoire Naturelle*, tome viii. p. 470.

tion,

tion, and I never met with it before. The following is the route I took to the place where I found this beautiful rock.

By moving in the direction which I traced out to myself when leaving *Bastia*, I not only followed some chains of mountains from N.W. to S. and from E. to W. but I also traversed several valleys, and turned considerable gulfs which separate them in various directions. When I was in the *Pieve d'Ostriconi*, where the chain commences which divides the island through its whole length to its southern extremity, I traversed the highest mountains which presented themselves to me, and among others that of Niolo, called in the language to the country *Monte-Pertusato*, because it is pierced at its summit. Its base seemed to be intersected by some detached masses, and others adhering to it, of jaspers and porphyries of great variety. I followed the valley which leads to the place called *Santa-Maria-la-Stella*. Between these two points, south-east from the former and south from the latter, and at an equal distance from both, there is a high mountain covered with wood, upon the western brow of which I discovered a block of stone, almost square, about four feet and a half long. It was sunk into the ground, and exhibited globular bodies on one of its sides, remarkable from their disposition and colour, and fixed in the stony mass: some were about an inch in diameter, while others were larger or smaller: all of them presented a peculiar character which I had never seen in any stone. Not more than six inches of this rock was exhibited above ground; and in order to ascertain its dimensions I took away the earth which surrounded it. I then found that it was two feet and some inches in thickness. I also observed that its angles were entire and acute; which made me think that it had never been removed since placed there, and particularly because the part of the slope of the mountain where it was is bare; and because, among the various blocks and masses which surround it, it is the only one which is covered with vegetable earth. I could only bring away a piece weighing about 24 pounds; the rest was too large and heavy.

When the specimen was detached and exposed to view, it seemed to me so beautiful and so extraordinary an appendage

to

to the magnificent orbicular granite of Corsica, the celebrity of which is so well known.

You may perhaps think I am exaggerating, but the following is the accurate description of the stone taken upon the spot:

The rock, the heart of which seems to be porphyroidal, has its paste composed of stony elements of a petro-siliceous nature, irregularly disposed in small grains, in points and in lineaments more or less rounded off, tying as it were with each other, and varying in colour in proportion to the various degrees of alteration the ferruginous principle, which is very abundant in this rock, has undergone: nevertheless its general aspect, when seen at a certain distance, is the reddish brown mixed with white spots shaded with red.

It is in the midst of this paste that we observe regular spheroidal bodies, from one to three inches in diameter, scattered here and there at unequal distances, and imbedded in the mass: the system of the formation of these kinds of balls can only be considered as the result of a globulous crystallization, which must have taken place rapidly; and not like geodites, which would have been formed apart, and enveloped subsequently in a porphyritical substance.

The method of crystallization in question is so far remarkable, that we can form no idea of it except by representing a circle into which a multitude of small stony bodies, oblong and compressed, of a petro-siliceous nature, very close to each other, must have been directed in radii, and as it were from end to end, from the circumference towards the centre of the circle, which gives them the appearance of divergent radii; and there has resulted from it a globulous solid, which with the hammer we may drive out from the place it occupies, leaving a hole of its own form behind it. The tendency of the crystallization has been such, that we see around the spherical bodies in question, in the paste of the stone, and round the spheres, the matter of the paste itself, which, according to the tendency it had to approach it, has formed a kind of aureolus, or zones, which surround several of the bowls, which may be more easily remarked than described:

scribed: indeed, no precise or just idea can be formed without seeing the stone itself.

The dimensions of my specimen are as follows: It is 17 inches broad, 12 high, and seven inches thick at its base: the side which I have got cut and polished presents 15 or 16 of these globules, among which we may remark several that are enchased as it were into each other.

After spending a considerable time on the spot where I found this specimen, I proceeded towards *Liamone* on the gulf of *Valinco*.

Having arrived at the village of Olmetto on this gulf, the place pointed out to me as containing the orbicular granite, I proceeded to Taravo. I dug up the *makis* covering a part of the hillock on which Stazzona is situated, and minutely examined every corner. I sounded the small lake in the neighbourhood: I visited the sea-shore: I also sounded the river, and explored it in various points by means of divers: I even followed its course upon the two banks for more than a league and a half: and finding nothing by these means, I formed the resolution of exploring 45 miles of ground beyond Stazzona.

I endeavoured to assure myself of the composition of the granites lying upon the heights surrounding the great valley of Taravo: I attacked every rock I saw, and found some specimens the composition of which resembled the granite in question.

After having pursued my researches still further, I entered the bed of the Taravo, and traversed the two banks for more than two leagues: at the moment when I was redoubling my efforts to finish my investigation, I was obliged to desist on account of the rain and snow, it being now December.

I collected the various specimens of rocks which I procured at Valinco: and after having made a comparative examination of them with the orbicular granite, I ascertained that in some of these specimens there were hornblend and feldspar, but not in the same order, nor in the same arrangement: nevertheless, I think we may infer from these specimens,

mens, that by finishing the object of the visit I paid to the two banks of the torrent, we shall perhaps succeed in discovering the primordial masses of the beautiful orbicular granite of which a small partial mass only has been hitherto dicovered: the angles of it were rounded, and it was found isolated upon the beach of Taravo, half a league from the sea, in the gulf of Valinco.

From the information I procured on this occasion, I think I proved to a certainty that the small mass of this granite, already known, comes from no place except Corsica; for you know that several naturalists have formed various conjectures upon the subject.

In the course of this tedious journey, I had also an opportunity of discovering an ore of iron, the stratum of which is half a league in length.

After having passed the river Oposata, in order to arrive at Calvy, in a plain above the village of Calenzana, to the eastward of Galeria, I found a stratum of iron ore, placed horizontally in a yellow earth, which at times disappears throughout the whole length of the ore, and the mineral of which is presented in three different views. At first it appears under the character of scaly iron, arranged in thin layers, mixed with a yellowish ochrey earth; afterwards it appears as a heavy blackish iron, compact, and almost entirely disengaged from every heterogeneous substance; and under a third aspect, in elongated spheroids from four to five inches in diameter, exfoliating at its surface, and compressed at the two sides: this gives it angles at intervals; and the sandy character and composition of it made me denominate it *arenaceous iron:* and I procured the necessary specimens for the experiments I intended to try upon it.

Having ascertained from these trials that this ore was very productive, I transmitted to the council of mines several specimens of the above stratum, begging them to publish the result of their assays.

 LXVI. *On*

LXVI. *On the Identity of Silex and Oxygen.* By *Mr.* HUME, *of Long-Acre, London.*

[Continued from p. 280.]

To Mr. Tilloch.

SIR,

AMONG these promiscuous observations, it would be unpardonable to omit *iron,* which is one of the more constant associates of silex. These two ingredients seem to be almost inseparable companions, especially in every thing of a primeval nature; for, in all original districts, mountains, rocks, and soils, and in every native compound of any consequence and extent, whatever the aspect, situation and contents may be, these two elementary bodies are sure to present themselves, and, I may add, are always united; for, though the silex may be elicited from the mineral in its simple form, the metal, on the contrary, is always *oxidized.*

So universally is this metal dispersed through the works of nature, that very few instances occur in which it is totally absent; its ubiquity is truly proverbial, and is exceeded by nothing, if we except silex or oxygen; indeed it pervades almost every solid substance, and even animal and vegetable bodies are seldom exempt from its influence, but often exhibit iron evidently as a constituent in their system. Hence, the history of iron becomes a most interesting subject to the physiologist, and, if we add its wonderful property of magnetism, it seems to be one of the most fertile for the imagination of every philosopher. As this metal is never discovered in the pure state, but is more frequently conjoined with oxygen than any other body; and as this process seems to have been effected in the immediate vicinity of silex, I see no particular or unreasonable objection, if, in all such instances, we assign the genuine cause of the oxidizement of iron, solely to this prototype of oxygen. I feel less difficulty in admitting this conclusion, when it is considered, that the more cogent examples are deducible from *originally formed* matter, from the real primordial rock, coeval with the globe itself, and made tangible, probably,

soon

soon after, or even at that very period when, the "Earth was without form and void."

The intimacy between silex and iron, and the consecutive oxidizement of the latter, need not be further urged; it occurs in such numberless cases, that whoever is at all conversant in mineralogy, and will take the trouble to search with candour, can be at no loss for evidence, sufficient to establish this singular concomitance. Thus, let us take, as an instance, that substance, familiarly known by the name of emery. Here, the iron is truly united to the silex in a very close manner, and not as a mere mixture, for the metal is oxidized and imbedded in this surplus of oxygen. "This," says M. Haüy, speaking of emery, "is a *true combination* of quartz and iron, in which the two substances contract a stronger adherence than a mere interposition of their molecules."

Though iron is considered as a pure metal and a simple substance, that is, when divested, by the usual methods, of the common impurities, to which it has a habitual affinity, particularly of these, viz. carbon, phosphorus, and silex; still, there is strong reason to believe that it has never been totally exempt from one or other of these substances. Indeed, it appears that some of these very impurities are required to render the metal more perfect, to add to its splendour, ductility, and other properties, which the arts demand. Thus, to make good steel there must be an addition of carbon as well as silex; and, if brilliancy, hardness, and a susceptibility of higher polish are to be considered as improvements, the carbon and the silex, in this case, seem to render the metal still more metallic, if such a term may be allowed.

In an analysis of four different specimens of steel, by M. Vauquelin, the result was this, taking it on an average to avoid fractions: that one hundred thousand parts of these samples of metal consist of 9817 of iron, 723 of carbon, 870 of phosphorus, and rather more than 288 of silex. That it is very difficult to deprive iron of all foreign matters, may be readily conjectured from this philosopher's labours, and the following observation confirms this truth, that iron is never pure. "The analysis of the varieties of steel," says this very accurate chemist, "is one of those parts of the

science

science the least advanced and the most difficult, especially when our object of research is the *exact* estimation of the principles which they contain :—it is thus, for example, that, in dissolving steel in dilute sulphuric acid, the hydrogen which is evolved, dissolves and *carries off* a part of the carbon, the quantity of which varies according to a multitude of circumstances."

From this and other authorities, and from a prejudice, which, I acknowledge, I have long been disposed to cherish, it may be inferred, that whatever emits smell cannot be considered as a *simple* body, and hence, the purity of hydrogen as an element must be doubted ; that species, however, which we obtain from the decomposition of water by the metals, is certainly very objectionable, if there be any truth in this observation ; for the gas is never free from a very perceptible odour, whether it has been procured by means of zinc or iron.

It is certainly not always prudent to generalize too freely upon these subjects, yet it is difficult on some occasions to avoid it entirely. The hydrogen gas, alluded to by M. Vauquelin, in these analyses, was undoubtedly impure, as it contained a certain portion of carbon from the metal, though not the whole; for, finding this mode of operating inconclusive, he at last had recourse to the sulphurous acid, with which he apparently succeeded in separating the whole.

Fluoric acid, from its peculiar effects upon the siliceous compounds, deserves a particular notice in the present inquiry, especially as its whole history remains still clouded with inconsistency and ambiguity; for, either the tables of affinity respecting its habitudes are erroneous, or the acid itself must be considered as a monstrous anomaly in the doctrine of chemical attractions. These tables begin with lime, and go on progressively with some of the earths and alkalies to silex, the very last in the enumeration, with which, by the way, it has never yet been united so as to produce a true salt. From Bergman's experiment, we learn, that he dissolved silex in fluoric acid, and that after the solution had remained undisturbed for two years, a number of crystals had formed at the bottom of the liquor in the vessel.

vessel. But, what were these crystals? They were pure silex, and had deserted this very acid, which, in all other cases, would have seized on it and dragged it into even aëriform existence. The native fluate of lime is so very generally contaminated with silex, if this expression may be allowed, that it is probable no fluoric acid exists without some of this ingredient; it may indeed owe its origin to this body, so uniformly are they associated.

But, that singular influence of fluoric acid upon silex, the corrosion of glass, is what has been chiefly noticed by most authors, for it does not appear that a direct application to the mere silex has yet been attempted, at least, with that precision which might have obtained a satisfactory result. That this acid should prefer the silex to the alkali, and in a case of single elective attraction too, is contrary to every table that has yet been published, and hence, in this example at least, it forms an exception to the general rule. But if, in similar experiments, the acid selects the silex from *lime*, a substance which is placed at the top of the list, in all arrangements, how much further does this error extend? Though in making experiments with this very curious liquid I have employed various species of glass, principally with a view to improve this method of etching, I have generally preferred *plate-glass*, on account of its form, convenience, and greater capability to endure the necessary pressure, so as to secure a number of perfect impressions. This glass is always, without exception, composed of lime, silex, alkali, and, occasionally, some other ingredients of less consequence in the present question.

It is astonishing, that in all the accounts of the decomposition of glass by fluoric acid, and even by other means of still greater energy, by electricity, little notice has been taken of the oxide of lead, and the subsequent disposition of the *whole* of the ingredients. I make no doubt, that *flint-glass* has been more frequently employed than any other, but I do not find that silex has ever put on that peculiar character of an *earth*, an alkali, or a salifiable base, and attached itself to the negative pole. On this subject, I confess, I feel extremely solicitous, as, in the late very

 splendid

splendid discoveries, which now, and probably will ever continue to, engross the attention of the scientific world, the decomposition of glass and consequent disposal of all its ingredients, form a question, to me at least, of the utmost interest; since, as far as I can judge of the phenomena, which have already been described, there appear circumstances more likely to confirm, than invalidate, my opinion of the nature of silex.

There is a remarkable similitude in the effects of oxygen and silex on the metals, particularly in that process called *vitrification*, which is, in every meaning of the word, a complete saturation. By means of these, particularly the silex, all the metals, perhaps, with no exception, from being the most opaque bodies in the universe, may be rendered quite pellucid, affording an endless variety of the most charming tints, as useful as they are elegant, since it is chiefly from metals and metallic substances that the most durable and valuable colours are obtained for staining glass and making artificial gems. The best opake colours, such as are most suitable for enamel, water, oil, crayon, and all other descriptions of painting, are derived also from the metals, combined with one or both of these substances; and though alumine and other bodies are occasionally present, they are as often absent. Even the precious stones and the less valuable pebbles, spars, and an infinite list of fossil productions, seem to derive their intrinsic value, beauty and other excellencies, entirely from the power of silex on the metals. Thus, the dull opacity of lead is as effectually changed by the *sand*, used in the composition of flint-glass, and the whole compound appears not less diaphanous, than the very same metal is, when, by means of *oxygen*, it is dissolved in nitric acid, properly diluted with water; such, however, is the inference I would draw from these premises.

The near connection between *potash* and silex, is not less manifest than in the other associations which have been already noticed; indeed, seeing with what avidity the base of potash (according to the late discoveries) clings to oxygen, I am furnished with this plea, that its original and necessary quantity had been obtained from silex; for all the potash

potash of commerce contains silex, and retains it with some degree of force, not as an adventitious ingredient, but rather as the superabundance of that primitive store, from whence it had derived that portion which is essential to its existence as potash. Now, that the constitution of potash no longer remains in doubt, and that oxygen has been proved to be as essential to the formation of potash as it is to that of sulphuric acid, I see no explanation more congenial and satisfactory than what I have here ventured to suggest, especially when it is proved that the primitive seat of potash is in *rocks* and *stones*, and in the very centre of such bodies, where the atmosphere can have had no influence; for, as far as regards its vegetable and animal existence, all is merely secondary, and, consequently, does not apply so forcibly in this theory, though, even here also, we need be at no loss for proofs.

The power which silex exercises over potash, soda, and a variety of other substances which enter into the composition of glass, is a notorious instance of its neutralizing efficacy; for no acid more completely obtunds the acrimony of alkaline bodies and disarms them of their corrosive character. The effervescence, which results when silex and the alkali enter into fusion, and form this insipid compound, is not observable till the materials are on the point of perfect combination: hence, as something is apparently evolved, neither oxygen nor any other aëriform fluid can be supposed to enter; so that the acidity, if the term may be applied, to coerce the alkaline matter, is alone due to the sand which is usually employed in the making of this beautiful and useful compound. Indeed, vitrification, in all instances, seems to be accomplished by silex or by oxygen; and the glass of lead, of antimony, of phosphorus, borax, or of any other body, is due to one, as much as the glass in common use, is to the other of these oxygenating agents.

In many very trite and familiar experiments, upon bodies containing either silex, an acid, or oxygen in some condition or other, the phenomena which succeed may be traced to the same cause. Thus, scintillation of hard bodies on collision against each other, as flint against seel; that of two siliceous stones, which emit not only light but the peculiar

culiar quartzy or rather sulphurous smell, already noticed; the effects produced by various species of phosphori; friction of two pieces of borax; the electric nature of glass; that of amber, tourmaline, and of resinous bodies; the light evolved by friction and collision of bonnet-cane and other vegetables which contain silex; and, in short, all other analogous examples may be adduced as additional illustrations on this subject.

If I were to select a case, in which silex seems to be deposited as it were, and deprived of the caloric which had suspended it in the state of gaseous oxygen, it would be that of a natural *hot-spring*, such as the Bath-waters, which are confessedly impregnated with sand or silex, not merely in suspension as an accidental material, but perfectly dissolved so as to be imperceptible to our sight. Besides these waters, all other hot springs contain silex in solution; that of Carlsbad; the Geyser, and Rykum, in Iceland; and many others, which, it is said, issue, for the most part, from granitic and other siliceous rocks. If these waters were cold the argument might fail, but while the temperature of the ambient medium can be taken into the account, I should not be willing to retract this opinion, as far as it concerns the nature of all hot-springs. It is stated from good authority, that in the kingdom of Portugal alone, there are upwards of 200 of these springs, the greater number of which, and the hottest, originate where silex is most abundant.

The presence of nitrogen in the Bath-waters, and, probably, in all other hot-springs, is a curious occurrence, and furnishes a proper theme for speculation. Whether it be the remainder of decomposed atmospheric air, which has been bereft of its oxygen, and that this is disposed of in the water, in the way I have supposed, is a question I shall not urge. The late Doctor Black analysed the hot-springs of Iceland, but the analysis, I believe, was not performed upon the spot, and, consequently, no notice could be taken of nitrogen gas. In the gallon of Geyser water, he found upwards of 31 grains of silex; and in the other, that of the Rykum spring, the proportion was 22 grains of the same ingredient in the English gallon.

The

The effect of silex in various cases is the same as an acid, and in some situations, where an acid or acid properties really exist, no other cause is present. All acids we know are not *sour*, some on the contrary, are insipid, and, therefore, it would be too much to expect silex to possess this property. It is, however, a strong support to this question to see my idea of its general acid quality corroborated by others, for it has lately been observed, (*Journal des Mines*, tome xx. p. 245.) that "*in the analysis of ores, silex acts very sensibly as an acid.*"

[To be continued.]

LXVII. *Report of Surgical Cases in the City and Finsbury Dispensaries, for November* 1807; *containing a Dissection of a Case of Hydrocephalus internus. By* John Taunton, *Esq.*

In the month of November there were admitted on the books of the City and Finsbury Dispensaries 257 surgical patients.

Cured or relieved		—	229
Died	—	—	3
Under cure		—	25
			257

Since which time there have been admitted 1007.

Some time since I was requested to examine by dissection, the head of J. W. ætat. about 9 years. It was remarked at the birth of this child, by a very intelligent surgeon, that the head was large, and that it was probable there was water contained in the brain. The child grew, and enjoyed good health till the 17th month after birth; but the head continued large. He was then seized with the hooping-cough, which was very violent, and he lost his sight for some time. It was now pronounced decidedly, to be a case of hydrocephalus internus.

On his recovering from the hooping-cough he regained his sight and *strength*, so as to enable him to walk with the hand of his nurse, or in a go-cart: his appetite was good, and

and his evacuations regular: but his head increased in size beyond the proportion it ought to have borne to the other parts of the body. He was naturally of a lively turn, and reasoned with great acuteness for his years,—observing, that it would be very difficult to regain his present time, which was lost, in point of education.

About two years before his death he wore a quicksilver girdle, and took some very stimulating snuff, from which he appeared to be relieved for some time.

About six months preceding death, he fell, and struck the back part of his head; from which he complained of great pain, which brought on violent vomiting, fits, and sometimes a loss of all sensation; as in a kind of lethargy.

About ten weeks before death, a blister was applied between the shoulders, from which he appeared to receive considerable relief; the head was shaved, and some stimulating oils rubbed in, as ol. origan, &c.: after one of these rubbings he lost his speech entirely, and remained insensible till his death.

The child was of a spare habit of body; but independent of the head, which was greatly enlarged, had a healthy appearance, and was moderately tall.

The bones of the scull were unusually thin, to which the dura mater did not adhere with the common degree of firmness; the vessels were turgid; otherwise the membrane and its sinuses were natural.

On raising the dura mater, the brain presented an uniform smooth surface, without the least convoluted appearance; the tunica arachnoidea and pia mater were healthy, the vessels lying upon the surface of the latter membrane.

The cerebrum was flaccid, and the undulation of the water could be distinctly perceived: on removing the upper part of the right hemisphere, although the incision was made more than two inches above the corpus callosum, it opened the lateral ventricle: the two laminæ forming the septum lucidum were separated from each other more than half an inch.

The lateral ventricles were greatly enlarged, and contained thirty-two ounces of a straw-coloured fluid; but there was

not

not any appearance of inflammation on the membranes lining these cavities.

On exposing the cerebellum, the right lobe appeared somewhat flaccid, in which a cist was placed containing eight ounces of a brown straw-coloured fluid, not having any communication with the fourth ventricle or other outlet.

The optic nerves were small, soft, and of a brown colour.

The other parts of the brain and nerves were natural. We perceive that the reasoning faculty was more complete than could have been expected. From such extensive derangement of parts, is it not probable that the early and slow formation of the fluid, and consequent distension of the lateral ventricles before the bones of the scull were completely ossified, or capable of affording that resistance which they are in the adult state, prevented in a great degree the effects of pressure, by allowing the bones to enlarge from the gradual pressure from within during their more membranous state? as we see a comparatively trifling pressure on the brain in the adult, when the bones are incapable of extension, whether it be in consequence of external violence or of inflammation, by which a fluid is effused into the lateral ventricles, productive in a short time of the most serious consequences.

We also perceive that nearly one half of the substance of the *cerebellum* was destroyed, by its place being occupied by a cist which contained eight ounces of a fluid; and yet the capability of associating ideas remained, which was also observed in the case of miss M. See Phil. Mag. vol. xxix. page 169.

Greville street, Hatton Garden,
May 24, 1803.

JOHN TAUNTON.

LXVIII. *Notices respecting New Books.*

MR. PARKES has for some time been engaged in revising and correcting the Chemical Catechism, in order to accommodate every part of that work to the new discoveries of Davy and others. A new edition thus amended, and with other very considerable additions, is in the press, and will be ready for delivery some time in June.

Mr.

Mr. Carmichael proposes to publish in the course of the ensuing summer, the second edition of his Essay upon the Effects of the Salts of Iron upon Cancer, with many additional cases, and several interesting practical observations upon that disease.

Mr. Carmichael has received some communications from practitioners, concerning their experience of those preparations, for which he begs to return his warmest thanks; and he at the same time takes this opportunity of earnestly requesting such of the profession as have deemed the remedy he recommended worthy of a trial, to inform him (addressed to No. 3, Gardener's Place, Dublin,) of their experience of its effects, before the end of June next, in order that he may insert their observations in his Essay, and that thus the merits of the remedy may be justly appreciated.

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 28.—This society met after the Easter recess, the president in the chair. A mathematical paper, by Doctor Young, was read, on the motion of fluids in flexible tubes, and the resistance of angular tubes to such fluids. A number of hydraulic experiments were performed; but the result was not of a nature to be stated here. This paper was merely designed as prefatory and introductory to this author's next Croonian lecture on muscular motion.

May 5.—A letter from Mr. Cadell, at Paris, to H. Davy, Esq., secretary of the Royal Society, was read. In this letter Mr. Cadell states, that the French chemists have successfully repeated Mr. Davy's experiments upon the decomposition of the fixed alkalies; and that they have found a remarkable confirmation of his discovery in the action of heated iron upon potash and soda.

This chemical result has been obtained by Messrs. Gay Lussac and Thenard. These gentlemen introduced potash into the bottom of a gun-barrel bent in the form of an S: iron filings filled the middle of it, which was strongly heated. By

By the action of the potash upon the heated iron, it is decomposed, and the metallic base partly distils over, and is partly found in a state of alloy with the iron.—The same letter states, that Mr. Berthollet jun. has read a paper to the Institute, in which he endeavours to confirm his father's analysis of ammonia.

May 12 and 19.—The president in the chair. These two evenings were occupied in reading an interesting and able paper containing the results of an analysis of numerous specimens of different calculi, by Mr. Brandé. The object of the inquiry was to ascertain the relative quantities of uric acid, and phosphats of magnesia and lime; and to determine the effects of the usual solvents, alkali and acids, for calculi in the bladder and kidneys. It appeared that out of 150 stones, 60 were found composed of phosphoric acid and animal matter, and that only 12 were found of pure uric acid; the phosphats of magnesia and of lime, with a slight portion of uric acid and animal matter, were the most common. Some of the stones had pieces of bougies, hazel-nuts, and peas for nuclei. To Mr. Brandé's experiments Mr. Home added some practical observations, tending to prove that, if alkaline solvents were used, they might dissolve the uric acid; but that the phosphoric, which is always the most plentiful, would thereby be increased, and the virulence of the disease, however mitigated for the moment, would eventually become much more dangerous. The same adverse effects were ascribed to the use of acids as solvents: so that we have yet to discover a safe and efficient remedy for calculous diseases.

May 26.—The president in the chair. The reading of a paper, by Messrs. Allen and Pepys, on the effects of respiration on the atmosphére commenced. The authors took a general view of what the principal philosophers have written on this subject, as an introduction to their observations; and expressed a hope of ascertaining with more accuracy than preceding experimenters, by means of their eudiometer, the quantity of oxygen consumed, and carbonic gas emitted, by the lungs in a given period.

UNIVERSITY

UNIVERSITY OF EDINBURGH.

We congratulate the lovers of science, as well as the public in general, on the splendid acquisition which this University has just now made by the magnificent collection of minerals bequeathed by the late Dr. Thomson of Naples.

This celebrated mineralogist, during a long residence in a country extremely fertile in the most interesting products of the mineral kingdom, has lost no opportunity of forming a most splendid collection, which, having fortunately escaped every danger, has arrived in Edinburgh untouched. Government not only indulgently remitted the duties, but allowed the whole to pass unsearched.

The liberal endowment (with which Dr. Thomson has accompanied this bequest) of 1500*l.*, the interest of which he has destined for the payment of a lecturer on mineralogy, and the support of the cabinet, we hope, will be the means of handing it down to posterity in its present high state of preservation.—It is contained in forty very large boxes, which are deposited in the museum of the University; and, we understand, proper cases are making for the reception of the specimens.

The interesting and valuable collection of the late ingenious Dr. Hutton, of this place, has also been deposited in the museum.

WERNERIAN NATURAL HISTORY SOCIETY.

At the last meeting of the Wernerian Natural History Society (14th May), Mr. P. Walker read an account of the Birds that frequent the neighbourhood of Edinburgh. He enumerated 178 species; of which 11 belonged to the genus Falco; 4 to the genus Strix; 1 to Lanius; 8 to Corvus; 1 to Oriolus; 1 to Cuculus; 1 Picus; 1 Alcedo; 1 Upupa; 1 Certhia; 2 Sturnus; 6 Turdus; 1 Ampelis; 2 Loxia; 6 Emberhiza; 8 Fringilla; 1 Muscicapa; 3 Alauda; 15 Motacilla; 4 Parus; 4 Hirundo; 1 Caprimulgus; 2 Columba; 1 Phasianus; 6 Tetrao; 3 Ardea; 6 Scolopax; 7 Tringa; 4 Charadrius; 1 Hæmatopus; 3 Rallus; 3 Fulica; 4 Podiçeps; 4 Alca; 6 Colymbus: 2 Sterna; 12 Larus; 1 Procellaria; 5 Merganser; 20 Anas; 4 Pelecanus. This account was accompanied with interesting observations on the distinctions of several of the species, their changes of plumage at different ages and times of the year, and their kind of food; and specimens of some of the dubious species were exhibited.

Mr. Jameson, at the same meeting, read the following mineralogical queries, and stated the reasons that induced him to consider the objects pointed out by them as deserving the particular attention of mineralogists.

Mineralogical Queries.

1, In what species of rock are the metalliferous veins of tyndrum situated, and what are the minerals they contain?

2. Are the leadglance veins of strontian situated in sienite, and what are their other geognostic relations?

3. Are the trap-veins that traverse the mining field at Strontian, basalt, porphyry-slate, or green-stone; or do all these different species of rock occur in that district?

4. Does the quartz rock of Scuraben and Morven in Caithness, and of Portsoy in Banffshire, occur in an unconformable and overlying position, or does it belong to the conformable primitive rocks, as clay-slate or mica-slate?

5. Does not the granular rock of Ben Nevis rather belong to the sienite than the granite formation?

6. Does the rock of the Hill of Kinnoul near Perth belong to the flœtz-trap or newest flœtz-trap formation?

7. Is the mountain of Cairnsmuir in Galloway composed of old granite?

8. What are the extent and particular geognostic relations of the black pitchstone of Eskdale-muir in Dumfries-shire?

9. Does the black pitchstone of the Cheviot Hills belong to the newest flœtz-trap formation?

10. On what formation does the porphyry-slate of Braed Hills near Edinburgh rest, and what are the venigenous and imbedded fossils it contains?

11. What are the geognostic characters and relations of the edge and flat coal beds or seams in Mid-Lothian?

12. On what formation does the Calton Hill near Edinburgh rest?

13. Does the greenstone of Corstorphin Hill belong to the independent coal formation?

14. Does the hill on which the town of Stirling is built belong to the coal formation?

15. What are the geognostic characters and relations of the veins that traverse or are included in the greenstone of the independent coal formation?

16. What are the characters of the transition greenstone of the south of Scotland?

17. What are the particular species of petrifactions that occur in the transition limestone near the Crook, on the road from Edinburgh to Moffat? P. N. Sec.

ROYAL COLLEGE OF SURGEONS, LONDON.

The Royal College of Surgeons have adjudged the *Jacksonian* prize for the best dissertation on "Diseases of the Eye and its Appendages, and the Treatment of them," to John Hyslop, Esq. Surgeon, Fenchurch-street. The same gentleman obtained the prize in 1805, for the best dissertation on "Injuries of the Head."

LXX. *Intelligence and Miscellaneous Articles.*

M. CARNOT.

With our present number we have given a head of the above celebrated French author, whose excellent Treatises on the Infinitesimal Calculus, and on Machines, have appeared in this work. Of a character so well known among men of science, it is unnecessary that we should say more than merely that he has been successively a member of the first Legislative Assembly, the National Convention, the Directory, and the National Institute of France. In 1800 he was the French minister of war. Besides the works already enumerated, M. Carnot is the author of an Eulogium on Marshal Vauban, a discourse which gained the prize of the Dijon

Dijon academy in 1784; a Collection of Fugitive Poetry; and of several Reports to the Convention, and Speeches made while President of the Directory; all of which have been printed and extensively circulated. M. Carnot was born the 13th of May, 1753.

COTTON.

M. Louis Dupoy, a colonist of St. Domingo, lately arrived in France with a variety of seeds and specimens of the cotton plant. The seeds have been distributed among the members of the agricultural society of Paris; and at a late meeting, several reports were read from members who had attempted the cultivation of this commodity in France: all these reports concur in giving a most flattering account of the success of the experiment. In Provence and Languedoc in particular, the crop of cotton was very abundant, and equalled in quality the production of the West Indies, as has been certified to the French legislature by several colonists.

TRAVELS.

M. Michaux, the author of Travels through North America, has been recently sent by the French government, a second time, to explore the forests of that vast continent. He is now actively engaged in fulfilling the object of his mission, and has transmitted to the professors of natural history in the French Institute, several specimens of seeds, with a view to the cultivation in France of the American oak and other useful trees.

PYROSOMA ATLANTICUM.

M. Peron, in his late voyage, observed this animal, not described before by naturalists, in between the 3d and 4th degrees of N. latitude. Its luminous property renders it one of the most splendid of all known zoophites. The darkness was intense when it was first discovered, the wind blew with violence, and the progress of the vessel was rapid. All at once there appeared, at some distance, a vast sheet of phosphorus floating upon the waves before the vessel. The ship having passed through this brilliant part, the crew discovered that the light was occasioned by an immense number of small animals, which swam at different depths, and assumed various forms. Those which were deepest looked like red-hot shot, and those on the surface resembled tubes of red-hot iron. Some were soon caught, and they were found to vary in size from three to seven inches. All the exterior surface was bristled with thick oblong tubercles, shining like so many diamonds, and these seemed to be the principal seat of phosphorescence. In the inside there appeared a multitude of oblong narrow glands, which possessed the phosphoric property in a high degree. The colour, when at rest, is an opal yellow mixed with green; but on the slightest motion, or spontaneous contraction, the animal instantly becomes luminous. As it loses its phosphorescence it passes successively through a number of tints, such as red, orange, green, and azure blue.

CHINESE RADISH.

Experiments lately made at Venice show that the oil of the Chinese radish is preferable to any other kind known, not only for culinary purposes, and giving light, but also as a medicine. From the experiments lately made by Dr. Oliviero, it is found to be extremely useful in rheumatic and pulmonary affections, and has been employed with much success in convulsive coughs. It is not liable to spoil by keeping, like other oils, nor is the plant injured by the strongest frosts. The seed, which is very abundant, is gathered in May and June.

LECTURES.

Dr. Satterby and Dr. Young propose to give two Courses of Medical Lectures next winter at the Middlesex Hospital. Dr. Satterby's will be Clinical Lectures, and any of the pupils of the hospital attending them will have the privilege of seeing the patients whose cases are discussed. He will be assisted in the department of morbid anatomy by Mr. Cartwright. Dr. Young's Course will be on the Elements of the Medical Sciences in general, and on the Practice of Physic in particular. It has been erroneously stated in several periodical publications, that Dr. Young had a large medical

medical work nearly ready for the press: the mistake arose from his having been for some time engaged in the preparation of these Lectures.

Mr. George Singer is now constructing a powerful Voltaic Battery; to be employed in a Course of public Lectures on the Chemical Agencies of Electricity. These Lectures will comprise the Exhibition of all the recent Discoveries; and are arranged for delivery early in the month of June, at the Scientific Institution, No. 3. Prince's-street, Cavendish-square.

Dr. George Pearson, F.R.S., and Senior Physician to St. George's Hospital, will recommence his Summer Course of Lectures on Physic and Chemistry, on Monday June 6, at No. 9, George-street, Hanover-square, at the usual Morning Hours; viz. the Therapeutics at a quarter before Eight: the Practice of Physic at half after Eight; and the Chemistry at a quarter after Nine.—Clinical Lectures are given, as usual, on the Patients of St. George's Hospital every Saturday morning, at Nine o'clock; and the Practice of Vaccination will be taught at the Institution in Broad-street, Golden-square, during the Summer Course.

LIST OF PATENTS FOR NEW INVENTIONS.

To Richard Willcox, of the parish of St. Mary, Lambeth, mechanist; for certain machinery, whereby all objects in the sea or clear water can be discovered from the surface thereof with accuracy; and for raising, suspending, and towing into harbour ships of war, and every other description of vessels that are or may be sunk at sea or near the sea-coast, channels, harbours, road-steads, or other places, and removing sunken rocks or other obstructions in rivers, harbours, and channels. March 3, 1808.

To John Cowden and John Partridge, of Francis-street, Tottenham Court Road, stove-grate manufacturers; for certain improvements in register and other stoves. March 3.

To Thomas Jefferson, of the parish of St. Saviour, Southwark, tanner and leather-dresser; Joseph Ellis, of the same parish, tanner and leather-dresser; and Alexander Galloway, of Holborn in the county of Middlesex, mechanist and engi-

 neer;

neer; for a machine for the purpose of finishing, glazing, and glossing of leather. March 7.

To Marc Isambard Brunel, of Chelsea; for certain improvements on circular saws for sawing wood in an easy and expeditious manner. March 14.

To Henry Maudslay, of Margaret-street, Cavendish-square, mechanist; for a machine for printing calicoes and other articles. March 14.

To Bryan Donkin, of Fort Place, Bermondsey, gent.; for a pen upon a new construction. March 14.

To George Nathaniel Pollard, of Queen-street, Southwark, lapidary; for certain improvements in machinery for grinding, smoothing, and polishing plate and other glass for looking-glasses, mirrors, and various other articles. March 14.

To Edward Weeks, of Llaveny Hall, in the parish of Henlau and county of Denbigh, in North Wales, gardener; for a forcing-frame on a new and improved construction, for raising and forcing of cucumbers, melons, strawberries, and other fruits and plants. March 17.

To Anthony Thomas, of Duke-street, St. James's; for a method of manufacturing hats, bonnets, and other articles of the like description. March 26.

To Benjamin Cook, of Birmingham, manufacturer; for a new method of making barrels for fowling-pieces, muskets, pistols, and other similar fire-arms, and ram rods for the same. March 26.

To John Dickson, of Edward-street, Southwark, engineer; for an improved method of constructing cocks for stopping fluids, and which cocks by one motion or operation will permit such fluids to pass in different directions. March 29.

To Charles Dibdin, of Cranford, in the county of Middlesex, gent.; for his method of facilitating the learning of music. April 9.

To Daniel Dering Matthew, of Upper Marylebone-street, in the county of Middlesex, esq.; for certain improvements in the construction of watches and chronometers. April 27.

To William Chapman, of the town and county of Newcastle-upon-Tyne, civil engineer; for his methods of conveying

veying coals and other minerals in the working of mines, or below ground, and of returning the empty vessels and carriages. April 27.

To William Bell, of Birmingham, in the county of Warwick, engineer; for his improvements in making pipes, or pumps, for conducting water and other liquids. April 30.

To Edward Coleman, Professor at the Veterinary College, in the parish of St. Pancras, in the county of Middlesex; for certain improvements in the construction and application of a horse-shoe, which will completely prevent several diseases to which the feet of horses are subject, more especially that very general disease called contraction of the hoof; and is also particularly adapted for flat convex feet, for horses of cavalry, and for hunting; and for all other purposes where the loss of a shoe is productive of great inconvenience. April 30.

METEOROLOGY.

Meteorological observations in a tabular form compress so much information in a small compass, and facilitate comparisons in such a manner as to render them highly useful. In both of these views the following tables cannot but prove acceptable to many of our readers. The estimate of rain has been given to the public for several years: Dr. Clarke's meteorological table did not commence till last June, but we understand that gentleman means steadily to pursue the same plan.

To facilitate pursuits of this kind, it is of importance that such a rain gauge should be provided as may collect all the snow as well as rain that may fall, and so arranged that the danger of bursting in a time of frost may be averted. A correspondent suggests that this object may be gained by admitting the common gauge into a hot-house kept at the temperature of 60° of Fahrenheit's thermometer, placing the large end of the cone at the top, in one of the squares now occupied by a pane of glass. Should any better method for obtaining such a desideratum present itself to any of our readers, we shall be happy in being enabled to communicate it to the public.

Quantity of Rain, which fell at the following Places in the Year 1807. *In Inches and Decimals. By the Rev.* J. Blanchard, *of Nottingham* *.

1807.	Chichester.	London.	Diss, Norfolk.	Chatsworth, Derbyshire,	Horncastle, Lincolnshire	Ferriby, Kingston-upon-Hull.	Heath, near Wakefield, Yks.	Lancaster.	Dalton, Lancashire	Kendal.	Sedberg, Yorkshire.	Nottingham.
Jan.	2·41	0·64	1·37	1·40	1·50	0·82	0 85	3·33	2·73	2·92	2·38	0·73
Feb.	2·44	1·48	1·66	1·79	2·77	2·64	2·09	3 59	4 59	5 58	4 00	1 23
Mar.	0·23	0·50	1·36	0·44	1·69	1·21	2·60	1·12	1·52	2·21	0·57	0 73
Apr.	[illegible]	1·02	0·81	0·67	2·36	1·77	1·17	3·19	3·66	2·90	1·72	0·94
May,	5·47	[illegible]	3·47	5·26	2·80	2 58	4·70	3·75	3 97	4·47	2 86	5 08
June,	0·56	1·74	1·92	2·81	2·52	1·13	2·65	1·25	2 26	2·27	4·00	3 00
July,	1·62	0·38	1·54	2·26	2·25	1·11	2·43	3·50	3 74	4·48	3 43	2·55
Aug.	3·13	1·94	1·64	1·57	1·27	3·31	2·18	}	2 92	3·49	4 58	1 40
Sept.	3·22	2·18	2·17	1·27	1·45	2·86	3 34	} 10·08	10·27	7 92	6·86	1·70
Oct.	2·48	0·94	[illegible]	3·18	1·78	1·93	1·60	}	6 08	7·09	5·15	1·70
Nov.	7·54	3·36	2·27	1·18	3·83	6·02	5 57	4·00	4 93	5 07	5·50	3·33
Dec.	0·83	0·76	1·05	2·67	0·91	1·62	0·86	3·20	3 26	4·53	2 64	0·93
Total,	29·93	18·20	20·17	24·45	25·13	[illegible]	30·04	37 01	49 93	52·93	43·69	23 32

A Meteorological Table from June to December, 1807. *By Dr.* Clarke, *of Nottingham.*

☞ The following observations on the thermometer are made at 8 A.M 2 P. M and 11 P. M and on the barometer at 2 P. M. The former instrument is placed in the open air, exposed to the west, but in a situation surrounded by buildings, which prevent any alteration of temperature from currents of air. The direction of the wind is taken from the vane of St. Peter's church; and the numbers state how often it has been observed in any particular quarter during the month.

1807.	Thermometer. Highest.	Lowest.	Mean.	Greatest variation in 24 hours	Barometer. Highest.	Lowest.	Mean.	Greatest variation in 24 hours.	Wea. Fine, or Fair.	Snowy, or Rainy	Winds. N. and NE.	E and SE	S. and SW.	N. and NW.
June,	75°	46°	57°·85	10°	30·31	29·53	29 95	·33	20	10	24	5	24	37
July,	[illegible]	52	64 00	8	30·50	29·52	29·90	·56	17	14	11	8	55	19
August,	78	53	64·98	9	30·18	29·59	29·85	·34	28	3	11	11	53	18
September,	67	40	51·93	10	30 15	29·21	29·69	·55	15	15	5	1	43	41
October,	65	40	53·29	14	30·15	29·19	29·83	·51	23	8	8	8	51	26
November,	50	26	38·93	11	30·10	28 43	29 44	·80	14	16	16	2	41	31
December.	50	24	38·14	13	30 24	29·11	29 84	·55	25	6	2	9	48	34
Avr. for 7 Mont.	—	—	52°·73	—	—	—	29·78	Totl.	142	72	77	44	315	206

* Any communications on this branch of Meteorology will be thankfully received by the Rev. J. Blanchard, Master of the Academy, Nottingham.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For May 1808.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	37°	40°	36°	29·87	0	Rain
28	40	46	38	·89	16	Cloudy
29	39	43	38	·90	31	Cloudy
30	41	50	46	·90	46	Cloudy
May 1	49	59	47	·95	51	Fair
2	46	62	59	·90	64	Fair
3	59	69	47	·84	59	Fair
4	54	73	56	·84	72	Fair
5	56	68	57	·80	47	Cloudy
6	57	70	56	·74	62	Fair
7	56	69	57	·62	57	Fair
8	58	61	50	·60	46	Cloudy
9	52	52	45	·60	0	Rain
10	51	57	49	·75	27	Stormy
11	52	58	48	30·06	15	Stormy
12	55	65	54	·20	25	Cloudy
13	56	68	55	·38	45	Fair
14	57	76	64	·19	79	Fair
15	66	79	67	·19	70	Fair
16	69	80	68	·02	92	Fair
17	61	73	55	·06	70	Fair
18	55	58	49	·15	39	Showery
19	46	59	48	·21	45	Fair
20	52	65	49	29·98	55	Fair
21	51	64	55	·80	35	Cloudy
22	60	62	54	·60	20	Showery
23	53	60	52	·72	19	Showery
24	54	65	52	·09	40	Fair
25	54	70	57	·89	52	Fair
26	57	64	57	·07	15	Showery

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

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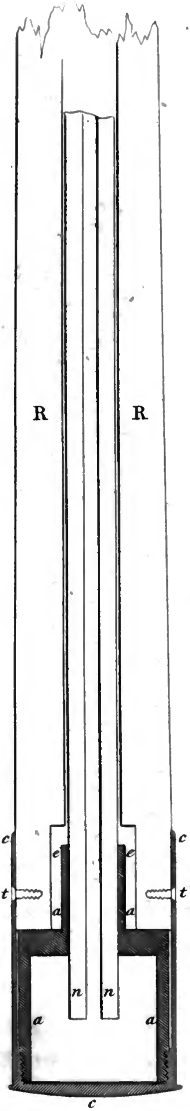
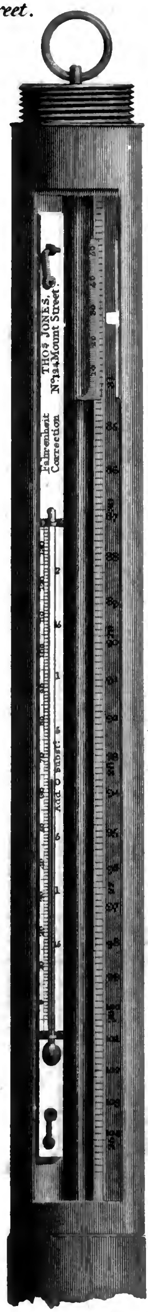
Fremy

END OF THE THIRTIETH VOLUME.

Printed by Richard Taylor and Co., Shoe Lane.

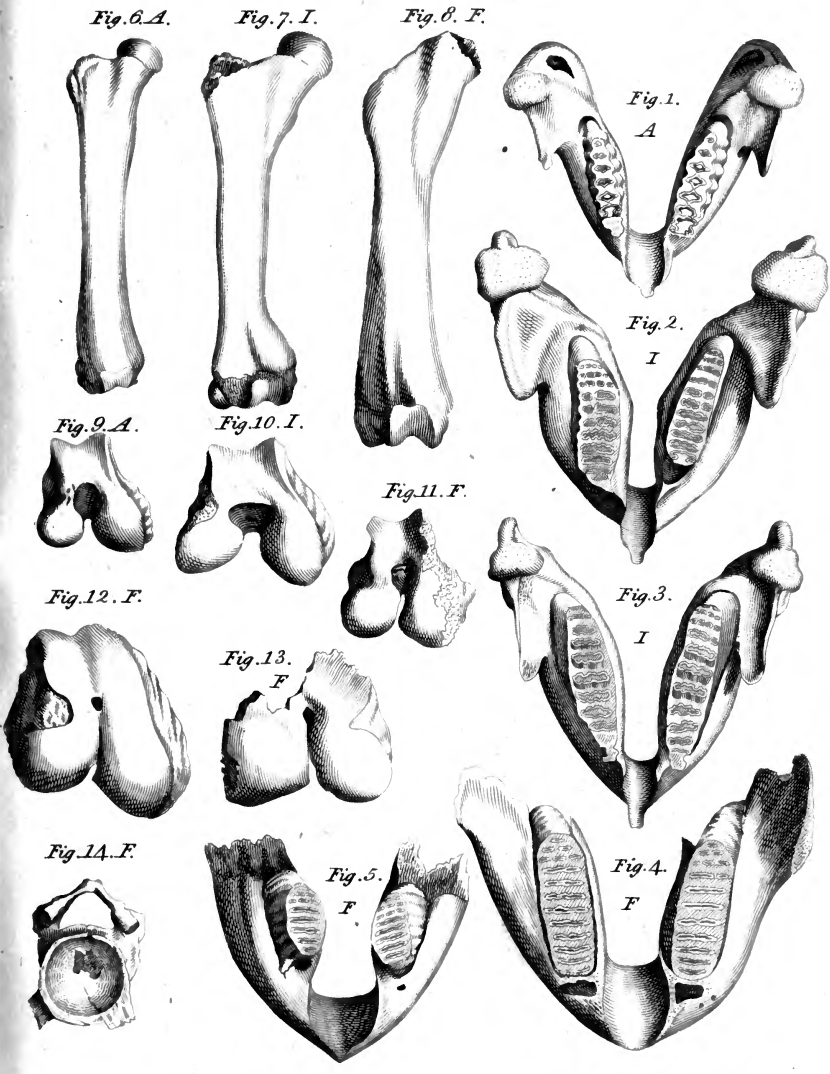
Sir H. C. Englefield's Mountain Barometer,
by Thos. Jones of Mount Street.





Lowry sculp.

ELEPHANTS.PL.V.



S.Porter.sc.

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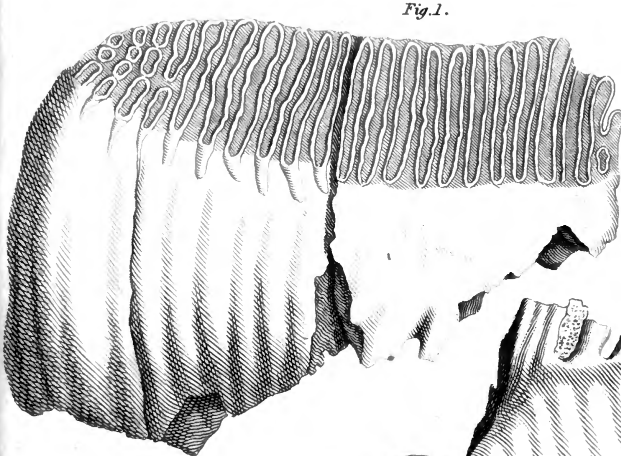
ELEPHANTS. PL. VI.

Fig. 1.



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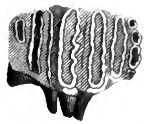

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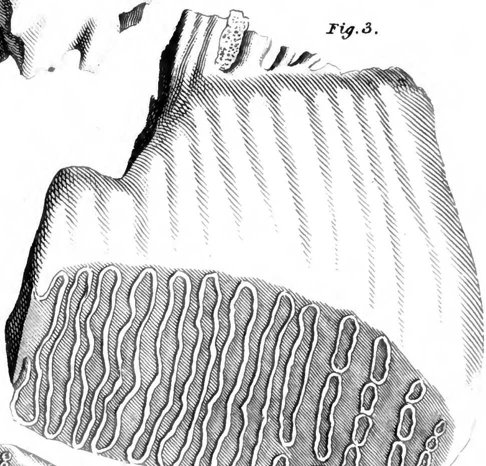

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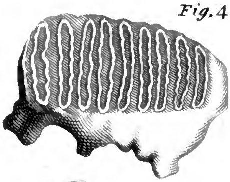
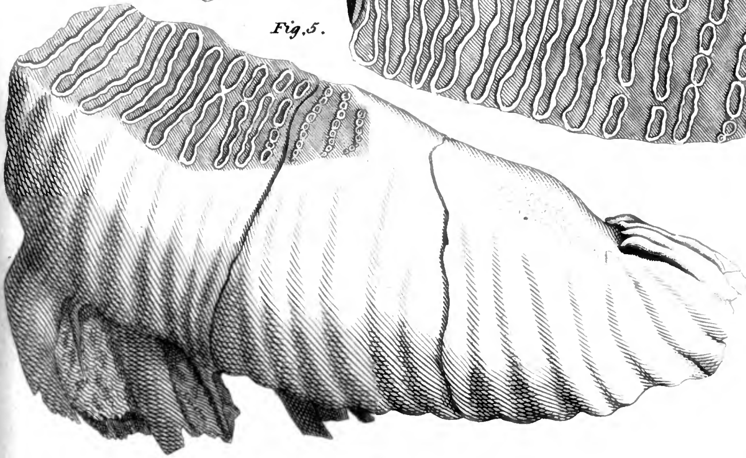


Fig. 5.



S Porter, sculp^t

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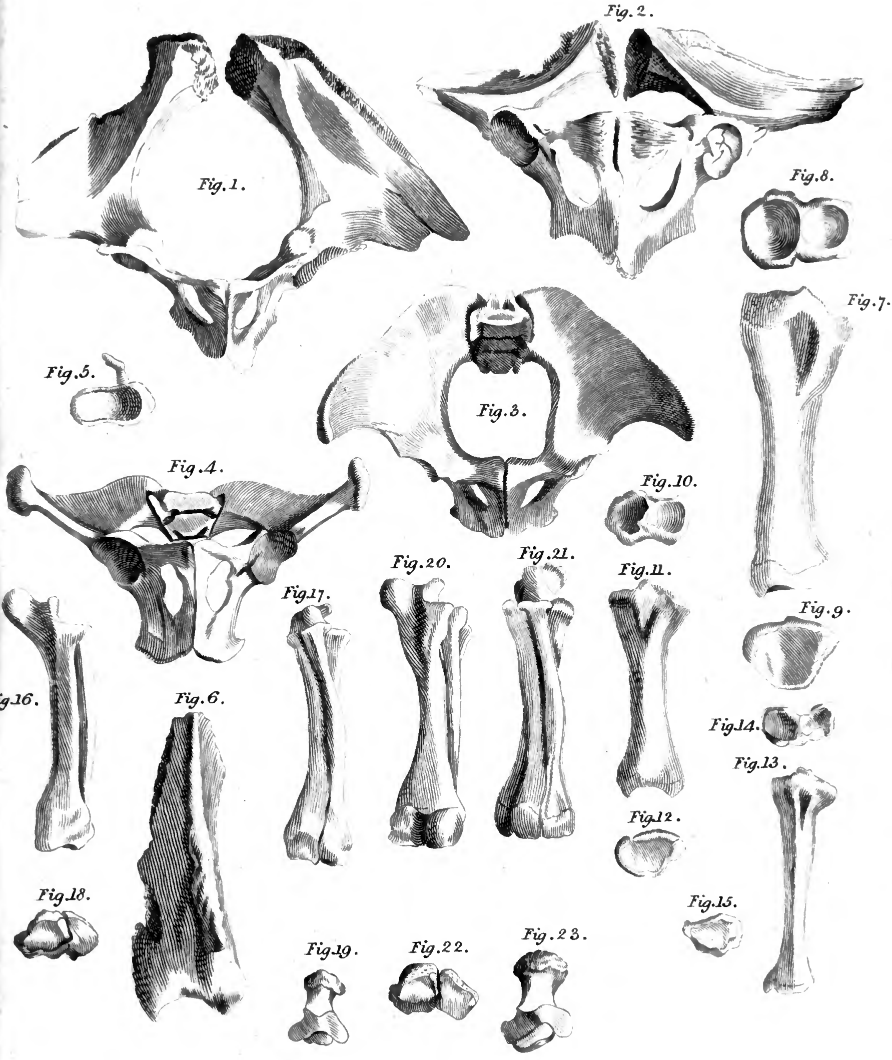

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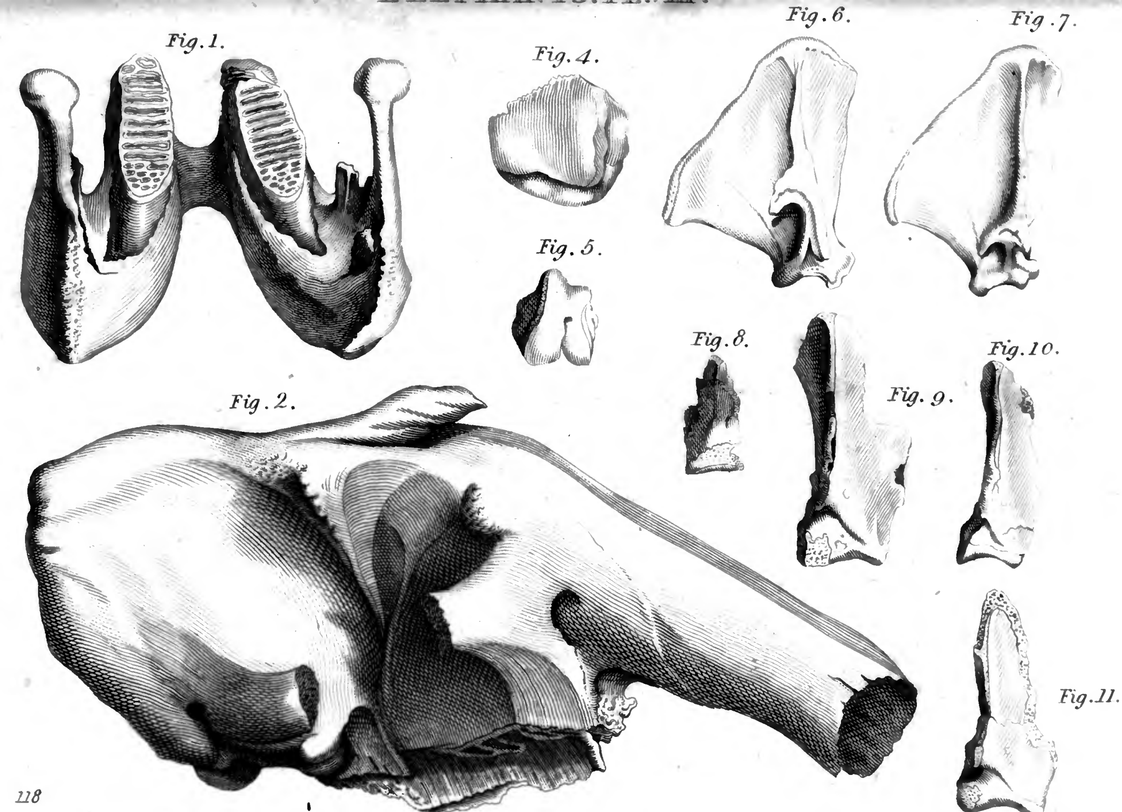

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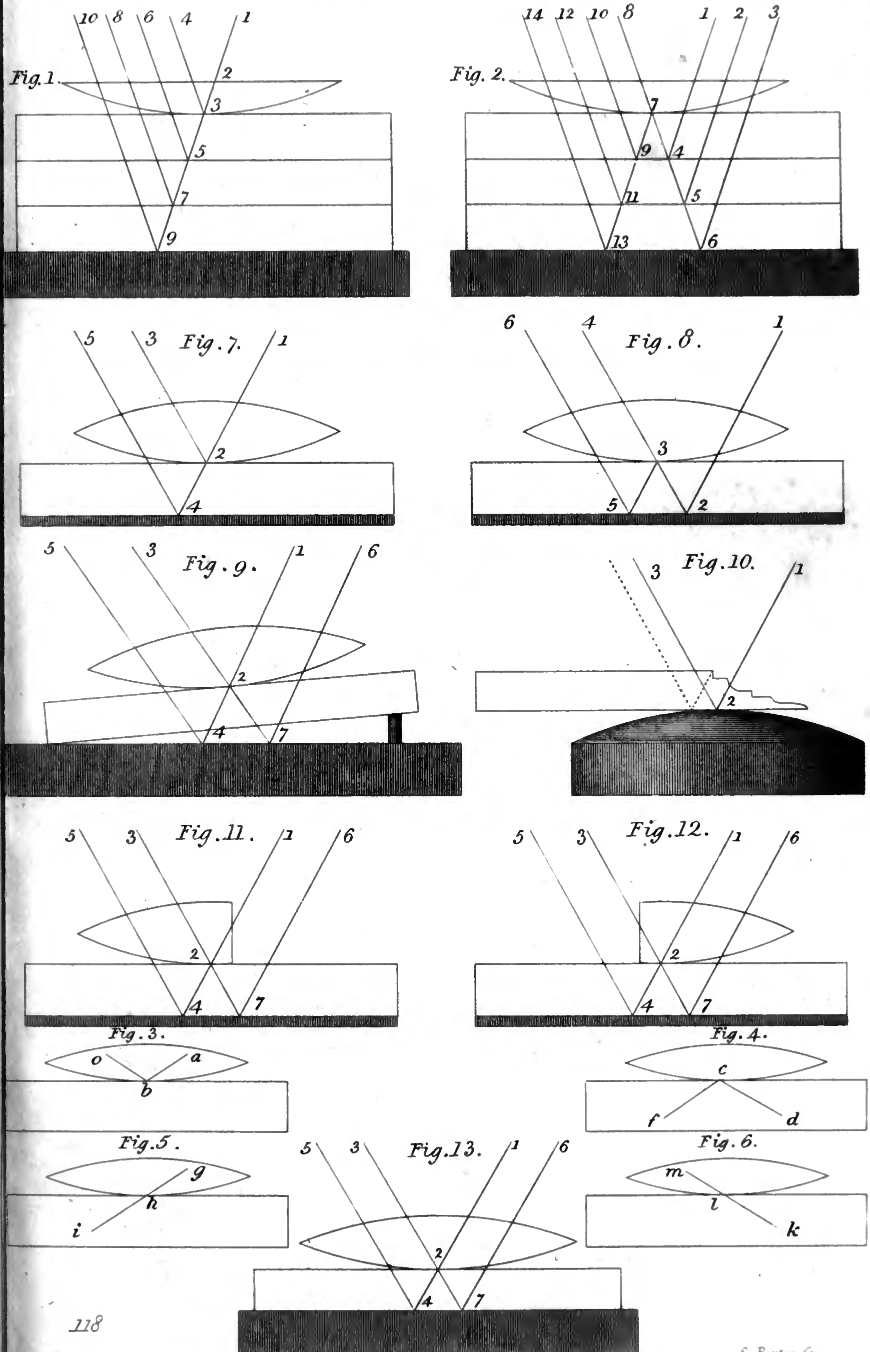
Dr. Herschel's experiments on coloured Rings.



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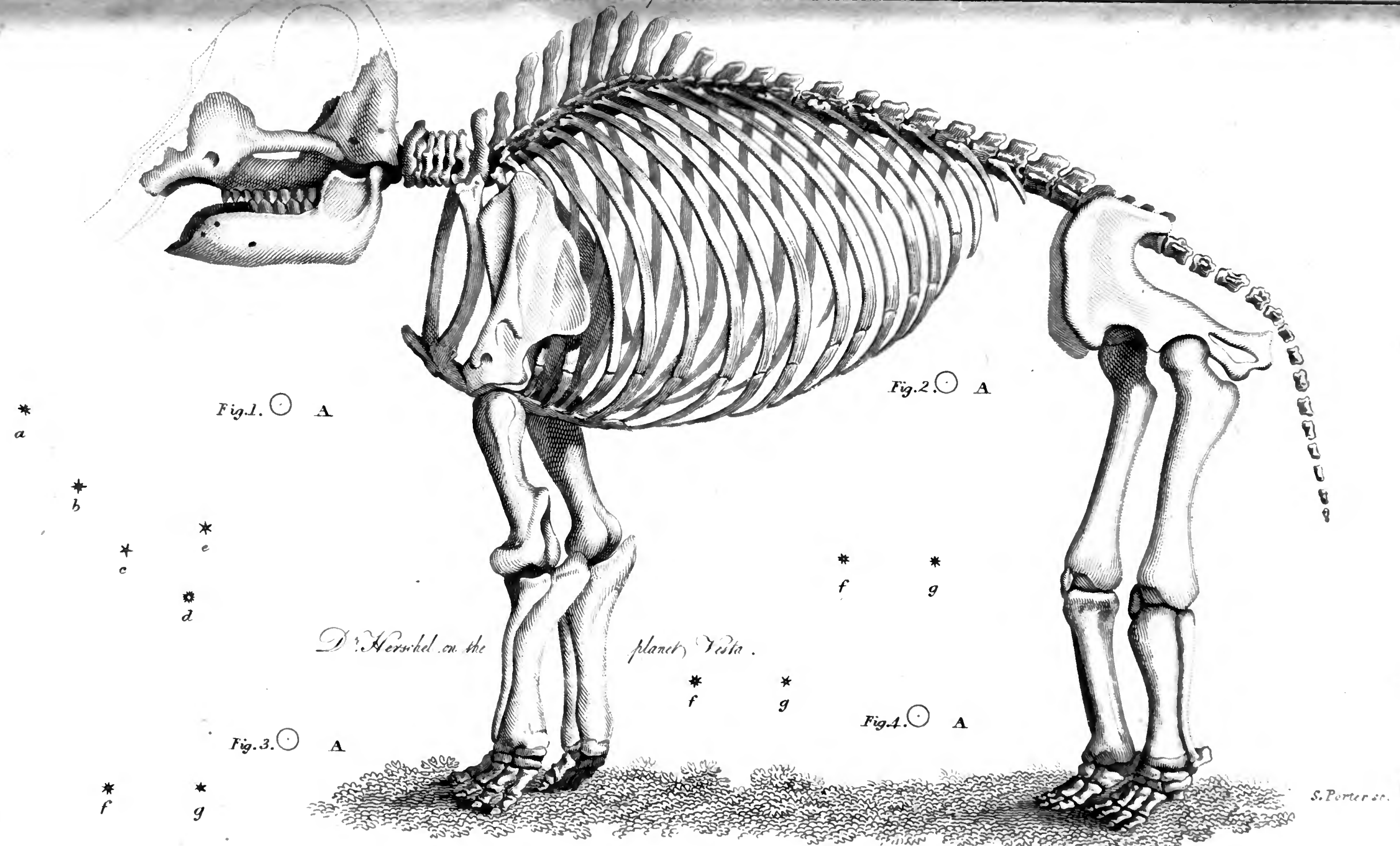
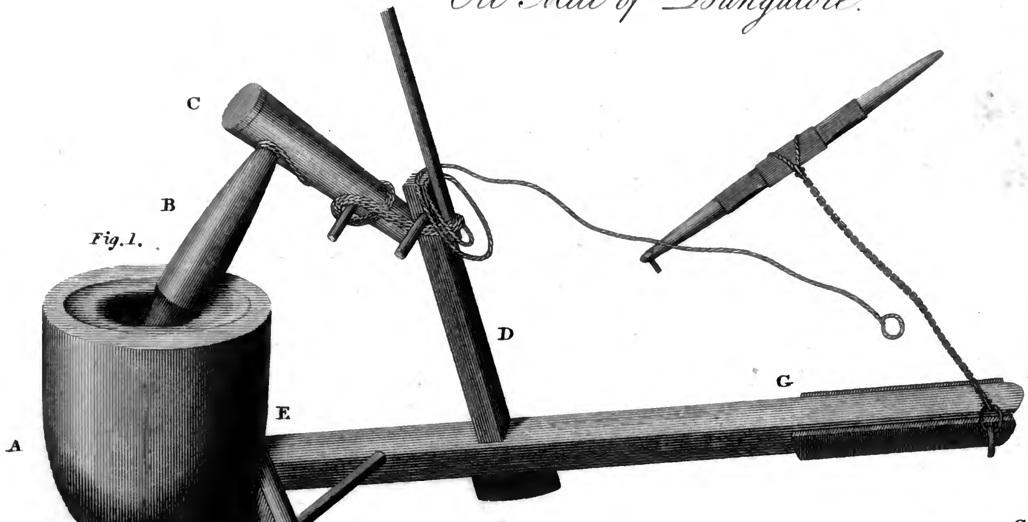


Fig.1. ⊙ A
a
b
c
d
e
Fig.2. ⊙ A
f
g
Fig.3. ⊙ A
f
g
Fig.4. ⊙ A
f
g
Dr. Herschel on the planet Vesta.
S. Porter sc.

Oil Mill of Bangalore.



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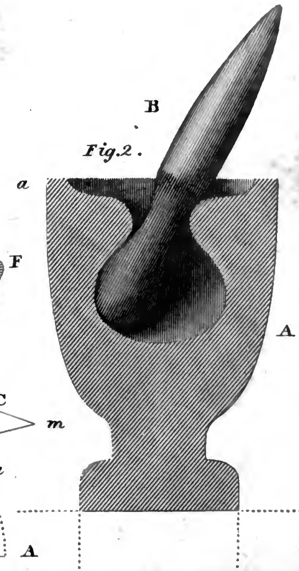

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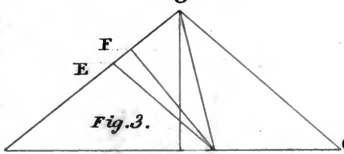

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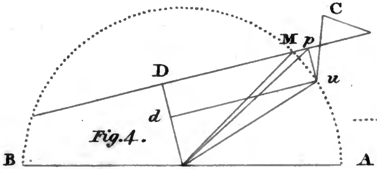

Fig. 4.

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S. Porter, sc.