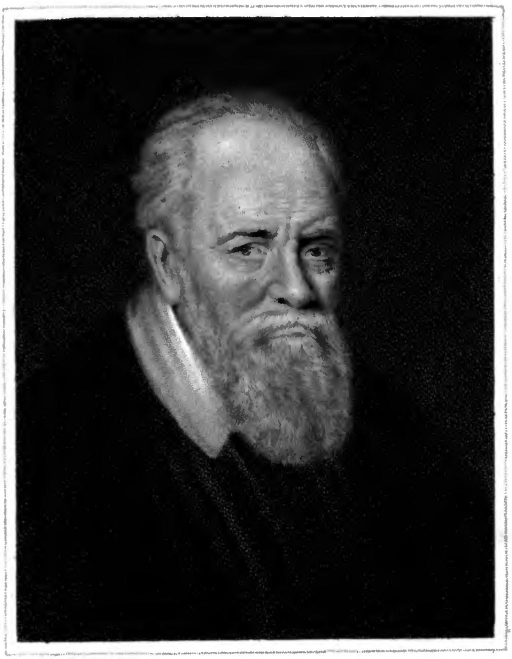

GEORGE BUCHANAN.

From the Original Picture by Titian,
in the Possession of the Earl of Buchan.
Engraved by T. Woodnoth,
at Edin.ʳ anno 1809.

Publ. And as the Act directs by Alex. Tilloch London Oct. 1809.

THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY, AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,

M.R.I.A. F.S.A. EDIN. AND PERTH, &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. 1.

VOL. XXXVI.

For JULY, AUGUST, SEPTEMBER, OCTOBER, NOVEMBER, and DECEMBER, 1810.

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OF THE

THIRTY-SIXTH VOLUME.

CONTENTS.

THE

THE

PHILOSOPHICAL MAGAZINE.

I. *A Review of the First Volume of M. J. A. De Luc's Geological Travels in the North of Europe: with Remarks on some of the Geological Points which are therein discussed. By a* Correspondent.

To Mr. Tilloch.

Sir, As a reader of your Magazine, I have been much gratified of late, by the extracts, remarks, and observations which have been given by you, or communicated by your correspondents, on subjects connected with Geology, or Geognosy, as it seems now the fashion with many to call it; and having just finished the perusal of a very useful as well as entertaining work, the first volume of M. J. A. De Luc's Geological Travels in the North of Europe, very lately published, I beg to communicate a short account of the same. The travels which are detailed in the present volume, were, it seems, undertaken, for collecting an extended body of facts in refutation of certain tenets of the late Dr. Hutton, and other geological writers, and in confirmation of the doctrines advanced by the author in his "Elementary Treatise on Geology," lately published. The route of our author commenced on the 23d of July 1804, at Berlin; he proceeded by way of Zehdenick, Furstenberg, Strelitz, Malchin, Lague, Rostock, Wismar, Travemünde, Lubeck, Eutin, Kiel, and Sleswigh, to Husum; from whence, on the 26th of August, he embarked for Harwich, to pursue a similar course of investigations in England, the details of which are to form the subject of his second volume, the publication of which will not I sincerely hope be long delayed.

The objects of M. De Luc's travels here detailed, are first stated in 27 propositions or heads: the first eleven, and the 13th, 14th, and 15th of which, relate to the question, *Whether the present rivers and streams of water, have excavated the valleys through which they flow?* a question, which some months ago exercised the pens of two of your correspondents, I recollect. M. De Luc seems to consider it as fully proved, by the numerous facts which he adduces in the volume before me, that rivers and brooks have no tendency to deepen their channels, but, on the contrary, are in every instance, with greater or less rapidity, filling up the bottoms of the valleys with the matters which they sweep away from the feet of the falling cliffs and steep banks that they undermine, in the more rapid parts of their courses: and that the whole of the matters so removed by the currents of rivers, are dropped before they reach the depths of the ocean, and are even met by and mingled with large quantities of pure sand, thrown up by the tides, from such deep parts of the ocean. The growth and operation of *peat*, in filling up some vales, is exemplified by instances at pages 131, 137, 146, 247, 334, 344, &c.; and in lessening, if not at length entirely filling up and obliterating, lakes, and natural pools of water in other instances, at pages 138, 141, 144, 145, 146, 147, 171, 185, 187, 190, 191, 233, 275, 281, 344, 347, &c.

The 12th, 16th, and 17th heads, relate to the question *Whether the bolders or blocks of granite and other stones, have emigrated on the surface of the earth?* The instances are very numerous which M. De Luc describes, through his whole route from Berlin to Husum, of blocks of granite and other primordial stones some of stupendous size, and of various species, found on the tops of gravelly eminences, in the face and at the feet of gravelly cliffs by the sea side, and on some plains, at pages 123, 127, 129, 135, 137, 156, 160, 172, 173, 178, 180, 181, 194, 195, 205, 213, 220, 223, 224, 226, 228, 234, 237, 238, 248, 251, 249, 255, 256, 257, 265, 272, 273, 274, 277, 278, 279, 285, 291, 293, 301, 302, 304, 313, 323, 329, 333, 334, 344, 367, 373, 381, 384, 386, 387, 388, &c. These instances are considered by our author as concurring in proving, the impossibility of such blocks being transported along the present or any former *surface* of the earth, to the places where they are now found, and as establishing or generalizing the supposition made by Mr. Wraxall and M. De Saussure in particular instances elsewhere, that such blocks "*seemed as if they had fallen from the sky.*"

I confess, however, sir, that I was somewhat disappointed in finding no attempt in all these details, at pointing out the exact places of the crater-like orifices, whence these

these primordial stones and gravel were projected *from beneath*, by the explosions of gases and torrents of water, that the descending masses of strata forced from the caverns previously existing under them, at the time that the dislocations of the strata and formation of mountains, hills and valleys, by the subsidences and angular motions of the strata, took place, according to the theory of M. De Luc: who, at page 64 of the present volume, speaks of "certain circular ridges of hills, which if seen from a distance by those unacquainted with their nature, might be taken for the bases or circumferences of large volcanic cones, which had fallen in;" as existing along the course of the Rhine, covered by such primordial blocks. I naturally expected, I say, that many similar spots would have been pointed out, in the vast tract of gravel and bolders in the north of Europe, which he has so well described in this volume.

At pages 123, 127, 129, 277, &c. my author speaks of flints, which had belonged to the chalk strata, as constituting part of the gravel, and considers such, as the remains of *dissolved strata of chalk*; but wherever he speaks of the undisturbed strata, below the gravel and bolders, in his route on the south side of the Baltic, they are uniformly said to be of sand, clay, and marle, and seem to me to answer either to the upper part of the Paris strata, described in your 35th volume, p. 56, &c. or to strata covering them, and answering to some that cover the chalk in England, and in the Netherlands also, according to the opinion of your correspondent, Mr. Farey, p. 131 of your same volume.

And here it may be proper to remark, that M. De Luc, in the volume before me, p. 248 and 250, speaks, on the authority of M. Von Willich, of similar strata of sand and clay in the island of Rugen in the Baltic, as being, if I rightly understand him, upper strata to the cliffs of chalk, with layers of flints and marine bodies, 200 and even 360 feet high, in the peninsulas of Wittow and Jasmund, in the north part of that island: and at page 387, on the authority of M. Hartz, he mentions, cliffs or strata of chalk and flints in Hedding in the island of Zeland, and also in the promontories of Maglebye and Mandemark in the island of Moen or Mona belonging to Denmark: all of which seem, I think, to confirm Mr. Farey's opinion above referred to, viz. that the chalk strata (instead of being dissolved) still underlay all the country, across which M. De Luc travelled: the coal and sandstone strata in the island of Bornholm (p. 387), probably answering, to those

 of

of Leige and other continental collieries, above the chalk, and not to any part of the British series of strata, of which the chalks seem nearly the uppermost.

The important question, as to whether the chalk underlays the south-eastern shore of the Baltic, might, I think, receive an answer, by a minute examination of the strata of the higher parts of the islands of Hiddensee and Ummantz on the west of Rugen and of Greifswaldiska Oe on the south-east, which are said (p. 249), on the authority of M. Von Willich, to consist of calcareous strata, which contain marine bodies, and are intermixed with strata of porcelain earth; since these would, seemingly, answer to the very characteristic limestones and potter's clays of the basin of Paris, page 44, &c. of your 35th volume.

If Mr. Farey be right, in asserting (p. 130 of the above volume), that three miles, (or 5280 yards) in thickness of strata are *known* in England, below the chalk strata, of which I have been speaking, without the intervention of granites of any kind, or of any of what M. De Luc calls primordial stones, in such strata (as I have understood to be the case), does it seem probable, that the numerous cavernous blasts and torrents, to which M. De Luc resorts, for projecting his primordial blocks towards the sky, whence they fell to the earth's surface, could have failed to have projected along with them, numerous and large specimens of the hard British limestones, sandstones, basalts, and others? but of which we read nothing in the volume of geological travels before us, except of a few flints.

From numerous passages in this volume, and in the Geology of M. De Luc lately published, it is plain, that at the time of writing them, this veteran geologist was unacquainted with the suggestions of Mr. Farey, in various parts of your late volumes, as to the *reversed action of gravity*, and the tidal currents, occasioned by a former and large satellite of this planet, whereby the extensive and vast *abruptions*, as Dr. William Richardson calls them* (or the denudations as Mr. F. calls them), which the earth's surface in so many places has sustained, are supposed to have been occasioned, as well as the transportation of its alluvia, and the excavation of its numerous valleys also, if I mistake not the drift of Mr. Farey's arguments with Mr. Carr, in your 33d and 34th volumes.

I very much wish, sir, that I could call the attention of

* See page 114 and page 258 of our 33d volume.—EDITOR.

M. De

M. De Luc to the above subjects, and that we might hear, what his long experience in studying nature, and in the consideration and discussion of geological systems, would offer, on this new hypothesis, for explaining the disruptions of the strata, the transportation of alluvial matters, and others of the vast and most mysterious operations, to which the terraqueous globe has been subjected.

But it is perhaps high time that I should mention the remaining subjects of the volume, which I undertook to explain, and as soon as possible conclude this desultory letter.

The 18th, 19th, 20th, 21st, and 22d propositions, or heads, are employed on the question, *Whether the gulfs and steep cliffs on the coasts of the ocean, were occasioned by its action?* At page 333 it is said, "that the indentations of the coasts have not been formed by the sea, but are simply the extremities of *vales*, or other original inflections of the surface, which lay below the level of the sea, when it came first to occupy its present bed;" and in numerous parts of the volume before me, facts are adduced, to show that, so far from the sea being now capable of excavating gulfs, such are almost uniformly in the process of *filling up*.

I am yet by no means satisfied, with the evidence adduced, to decide the other part of this proposition as M. De Luc has done, viz. that the *cliffs* on the sea-shore did not originate with the action of the sea. A necessary consequence of numerous valleys having opened into the sea *below its present level*, as stated above by M. De Luc, is, that separating ridges or points of hills equally numerous, projected or run out into the sea; and from having often and attentively observed, the great effect which waves propelled by a high wind, oblique to the shore in particular, have upon all projecting points, and the powerful tendency which the beach or strand *has to assume a regular line* without sudden indentations, in almost all situations, I am inclined to ascribe to the *waves* a power, of commencing and carrying on the ravages which most points of hills projecting into the sea have suffered: yet, without at all invalidating or calling in question the Mosaic chronology, which M. De Luc is so properly intent on supporting; and I am further disposed to assume, that so correct an observer and reasoner as M. De Luc, could not have overlooked these circumstances, had he not too much relied on his position, of the original marine cliffs or façades, being the mere effect of the subsidences and angular motions of the strata: and not duly considering,

 that

that the *valleys* being of the same or usual form, which descend into and much *below the level of the sea*, the separating *hills* must have been of such usual form also; and since, on following any ridge of hill on land, we scarcely ever meet with a façade or perpendicular rise in or across its height, it follows, that the numerous and almost invariably abrupt endings of hills at the sea, is the effect of their abrasion, or being worn away by the waves, and not of faults or depressions.

The 27th head discusses the question, *Whether the level of the present sea has ever changed?* Here I cannot but admire the address, with which M. De Luc has brought the numerous and, indeed, invariable instances of the horizontality of the new formed or modern alluvial lands, to bear on the question, and prove, as several others of the facts which he relates also do, that the sea has remained at its present level, or very near it, ever since the present race of men, animals, or plants have existed upon the earth; and I may add perhaps, ever since the present race, of fish also, have existed in its waters.

The remaining four heads, viz. the 23d, 24th, 25th, and 26th, relate to the question, *What is the age of our present continents?* From a chain of facts, too numerous for me here to particularize, as they run through the whole book, my author endeavours to prove, and successfully, as I think, and have hinted already, that natural appearances and the state of society concur in proving that, our continents cannot at the most, be more ancient than the Scripture chronology represents them to be. I wait with some impatience for the appearance of the 2d volume of these Travels, which is to treat of scenes somewhat more familiar to me, and am, sir, Yours, &c.

July 1, 1810. A. B.

II. *Observations on the Effects of Magnesia, in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr.* WILLIAM T. BRANDE, *F.R.S. to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society* *.

MR. HOME's inquiries into the functions of the stomach, and his discovery of liquids passing from the cardiac portion, into the circulation of the blood †, led him to consider,

* From the Philosophical Transactions for 1810, Part I.
† Phil. Trans. 1808.

sider, that the generality of calculous complaints might possibly be prevented, by introducing into the stomach, such substances as are capable of preventing the formation of uric acid, and that this mode of treatment would have many advantages over the usual method, which consists in attempting to dissolve the uric acid after it is formed.

He consulted Mr. Hatchett on the substance most likely to produce this effect, and asked if magnesia, from its insolubility in water, was not well adapted for the purpose, as it would remain in the stomach, until it should combine with any acid, or be carried along with the food towards the pylorus.

Mr. Hatchett knew of nothing more likely to produce the desired effect; and on putting this theory to the test of experiment, it was found by a very careful examination of the urine, that in several instances where there was an increased formation of uric acid, magnesia diminished it in a much greater degree than had been effected by the use, and that a very liberal one, of the alkalies in the same patient.

This circumstance led Mr. Home to wish for a more complete investigation of the subject, and he requested me to assist him in the prosecution of it. Since that time many opportunities have occurred of carrying on the inquiry during an attendance on patients labouring under calculous complaints.

It is proposed to lay the results of our joint labours before this society, with a view to establish a fact of so much importance in the treatment of those diseases.

The four following cases include the principal varieties of the disorder, which have been met with, and are therefore selected from among many others, to prevent unnecessary repetitions. In each of them the urine was occasionally carefully analysed

Case I.

A gentleman, sixty years of age, who had been in the habit of indulging in the free use of acid liquors, had repeatedly passed small calculi composed entirely of uric acid; his urine immediately after being voided, deposited at all times a considerable quantity of that substance, in the form of a red powder, and occasionally in large crystals.

Nine drachms of subcarbonate of soda, dissolved in water highly impregnated with carbonic acid, and taken in the course of the day at three doses, appeared to have no effect

effect whatever on the formation of uric acid; the red sand was deposited as usual, and the small calculi continued to form.

On account of the inefficacy of this medicine, he was advised to try the vegetable alkali, and three drachms of subcarbonate of potash, dissolved in water slightly impregnated with carbonic acid, were taken at similar intervals.

The deposit of uric acid in the urine was now somewhat diminished; but during this free use of alkalies, which, with little interruption, was persevered in for more than a year, the small calculi still continued to be voided.

The very unusual disposition to form uric acid, and the complete failure of the common alkaline medicines, rendered this case particularly favourable for the trial of magnesia, as it would afford an opportunity of comparing its effects with those of the alkalies.

Previous to giving the magnesia, the urine was examined, to ascertain the quantity of uric acid it contained: this being done, the patient was directed to take fifteen grains of magnesia three times a day, in an ounce and a half of infusion of gentian: in a week the uric acid was found, by examining the urine, to have diminished in quantity, and after the first three weeks it was only occasionally met with.

The use of magnesia has been persevered in for eight months, during which time no calculi have been voided, nor has there been any material deposit in the urine.

This patient was extremely subject to heartburn, and he likewise complained of a sense of weight and uneasiness about the region of the stomach, both of which symptoms have disappeared.

Case II.

A gentleman, about 40 years of age, had during four years occasionally voided considerable quantities of uric acid, in the form of red sand, and had once passed a small calculus.

His urine was generally more or less turbid, and after taking any thing which disagreed with his stomach, even in a slight degree, the red sand often made its appearance. He had never used the alkalies nor any other medicine, to alleviate his disorder: he was consequently desired to take a drachm and a half of subcarbonate of soda, dissolved in a pint and a half of water highly impregnated with carbonic acid, in the course of the day, and to persevere in this treatment for some time.

On

On the 30th of January 1809, he left London, and returned on the 6th of March following.

During his absence he had voided rather less uric acid than usual, but had had one severe attack, in consequence of which, twenty drops of the solution of pure potash were added to each dose of the soda water: this, however, had not the desired effect; for on the 10th of March, having taken more wine than usual on the preceding day, he was attacked with pain in the right kidney, and voided with his urine a considerable quantity of uric acid, in the form of minute red crystals. During the succeeding day, he made but little water, which deposited a copious sediment of red sand.

For the removal of this symptom, he was directed to take magnesia, in the dose of twenty grains every night and morning, in a little water; for three successive days his bowels were unusually relaxed, but afterwards became regular. He persevered in its use for six weeks without intermission: his urine was several times examined during that period, and contained no superabundant uric acid; and he has not had the slightest return of his complaint, although he has put himself under no unusual restraint in his mode of living.

Case III.

About the middle of October 1808, a gentleman, forty-three years of age, after taking violent horse exercise, was seized with pain in the right kidney and ureter. In the course of the night he passed a small uric calculus. For some months previous to this attack, he had felt occasional pain in the kidney, but had never voided either calculi or sand. His urine was now always turbid, and occasionally deposited red sand.

On the 28th of October he began the use of soda water, and for a time his urine was much improved in appearance, but the uric acid gradually returned, and at the end of December, notwithstanding the continued use of the soda water, he voided more sand, and his urine was more loaded with mucus than it had ever been before.

In consequence of these symptoms, on the 3d of January 1809, he was directed to take twenty grains of magnesia every night.

The urine was examined after the third dose, and the deposit of red sand was diminished in quantity, but it did not disappear entirely, after the magnesia had been taken for three weeks.

About

About this time (on the 26th of January) he caught cold, and his urine was again very turbid, but this was found to be wholly the effect of mucus, and the symptom soon left him.

On the 30th of January he took twenty grains of magnesia, and repeated it every night and morning, until the 1st of March, when his urine was perfectly healthy, and he left it off.

On the 1st of June he again voided a little uric acid, in the form of red crystalline sand: this attack was attended with a slight pain along the right ureter. He returned to the use of the magnesia, which he took twice a day for three weeks, in the same dose as before, and from that time to the middle of November there had been no symptoms of a return of the complaint.

CASE IV.

A gentleman, aged fifty-six, after recovering from a severe fit of the gout, voided constantly a large quantity of mucus in his urine, a symptom which he had never before noticed. There was also, occasionally, abundance of red sand, consisting principally of uric acid, but he had never voided a calculus.

His stomach was uncommonly weak, he was often affected with heartburn, and an almost constant pain in the neighbourhood of the right kidney. He had been in the habit of taking tincture of bark, and other spirituous medicines, from a belief that the pain in his right side arose from gout in the stomach.

He had already attempted to use the alkalies, which had produced such unpleasant sensations in the stomach, that he could not be prevailed upon to try them again in any form.

Under these circumstances, he readily acceded to a new plan of treatment. He was directed to omit the use of spirituous medicines, and take twenty grains of magnesia three times a day in water; but this operating too powerfully upon the bowels, the same quantity of magnesia was taken twice a day only, with an addition of five drops of laudanum to each dose.

This plan was pursued without intermission for three weeks, and he received considerable benefit, as far as concerned the state of the stomach, and pain in the region of the kidney. The urine, which was examined once a week, was also, on the whole, improved; but it occasionally deposited a very copious sediment, consisting of uric acid, with a variable proportion of mucous secretion.

After

After a further continuance of the use of the magnesia for three weeks, the urine was often much loaded with uric acid and mucus; but these appearances, which before the use of the magnesia were constant, are now only occasional, so that the disposition to form a redundant quantity of uric acid is much diminished: it is also deserving of remark, that there has not been the slightest symptom of gout from the time of the last attack, which is more than a year back, a longer interval of ease than this patient has experienced for the last six years.

He has now omitted the regular use of the magnesia; but on perceiving any unpleasant sensation in the stomach, he returns to it for a week or ten days, and then again leaves it off.

From the preceding cases it appears, that the effects of magnesia taken into the stomach, are in many respects different from those produced by the alkalies, in those patients in whom there is a disposition to form a superabundant quantity of uric acid.

With a view to ascertain their comparative effects on healthy urine, when taken under the same circumstances, the following experiments were made.

Experiment 1. *On Soda.*

Two drachms of subcarbonate of soda were taken on an empty stomach at nine o'clock in the morning, dissolved in three ounces of water, and immediately afterwards, a large cup of warm tea.

In six minutes, about one ounce of urine was voided; in twenty minutes six ounces more; and after two hours, a similar quantity.

The first portion became very turbid, within ten minutes after it had been voided, and deposited a copious sediment of the phosphates, in consequence of the action of the alkali upon the urine. It slightly restored the blue colour to litmus paper reddened with vinegar: the alkali, therefore, was not merely in sufficient quantity to saturate the uncombined acid in the urine, and consequently to throw down the phosphates; but it was in excess, and the urine was voided alkaline.

The urine voided after twenty minutes, also deposited a cloud of the phosphates; but the transparency of that voided two hours after the alkali had been taken, was not disturbed.

Here, therefore, the effect of the alkali upon the urine, was at its maximum, probably in less than a quarter of an hour

hour after it had been taken into the stomach, and in less than two hours the whole of the alkali had passed off.

Experiment 2. On Soda, with excess of Carbonic Acid.

The same quantity of soda, dissolved in eight ounces of water very highly impregnated with carbonic acid, was taken under the same circumstances as in the former experiment, and the urine was voided at nearly similar intervals.

The separation of the phosphates was less distinct, and less rapid. In two hours after the urine had been voided, there was a small deposit, composed principally of phosphate of lime; there was also a distinct pellicle on the surface, consisting of the triple phosphate of ammonia and magnesia. This appearance, produced by the escape of the carbonic acid, which had before retained the ammonia-co-magnesian phosphate in solution, and which now occasions its deposition on the surface, is by no means uncommon, even in the urine of healthy persons: in the present instance, it appears to prove, that carbonic acid passes off from the stomach, by the kidneys; for, after taking the alkalies, in water very highly impregnated with it, the pellicle is uniformly produced, and is also much more abundant and distinct than under any other circumstances.

In similar experiments with potash, the results were in all cases as similar as could be expected in researches of this nature.

Experiment 3. On Magnesia.

Magnesia was taken under circumstances similar to those of the soda in the former experiment: in the quantity of half a drachm, it produced no sensible effect upon the urine during the whole day. When taken in the dose of a drachm at nine o'clock in the morning, the urine voided at twelve o'clock became slightly turbid: at three o'clock the effect of the magnesia was at its maximum, and a distinct separation of the phosphates took place, partly in the form of a film, which when examined was found to be the triple phosphate of ammonia and magnesia, and partly in the state of a white powder, consisting almost entirely of the triple phosphate and phosphate of lime.

The effect of large doses of magnesia, in producing a white sediment in the urine, is very commonly known, and has been erroneously attributed to the magnesia passing off by the kidneys.

These experiments show that magnesia, even in very large

large doses, neither produces so rapid an effect upon the urine, nor so copious a separation of the phosphates, as the alkalies; on this its value as a remedy in calculous disorders seems materially to depend.

Experiment 4. *On Lime.*

Two ounces of lime water, taken in the morning upon an empty stomach, with a cup of milk and water, produced no effect whatever.

A pint of lime water, taken at four intervals of an hour each, produced a slight deposition of the phosphates at the end of the fifth hour. The urine voided at the third hour was not at all affected; at the fifth hour, the effect appeared at its height, but was not nearly so distinct as from small doses of soda, notwithstanding the insoluble compounds which lime might be expected to form with the acids in the urine.

The unpleasant taste of lime water, the quantity in which it requires to be taken, on account of the small proportion of the earth which is held in solution, and the uncertainty of its effect, are circumstances which render it of little use, excepting in some very rare cases, where it has been found to agree particularly well with the stomach.

The effect of carbonate of lime upon the urine was much less distinct than that of lime water: at times it produced no effect, but when taken in very large doses, a slight deposition of the phosphates was produced.

These experiments were repeated upon three different individuals, and there was always an uniformity in the results.

When the medicines were taken some hours after food being received into the stomach, their effects upon the urine were retarded, but not prevented.

The effects of many other substances upon the urine were examined into during this investigation; but they varied so much according to circumstances, that no satisfactory results were produced.

As it is found in the foregoing experiments, that the effects of soda on the urine are modified by the presence of carbonic acid, the following experiment was made, to ascertain whether any sensible effects are produced by that acid on healthy urine.

Twelve ounces of water very highly impregnated with carbonic acid, were taken upon an empty stomach at nine o'clock in the morning. At ten o'clock about eight ounces of urine were voided, which had a natural appearance,

ance, but, when compared with urine voided under common circumstances, was found to contain a superabundant quantity of carbonic acid: this gas was copiously given off when the urine was gently heated, or when it was exposed under the exhausted receiver of an air-pump.

In a patient who had a calculus of large dimensions extracted from the bladder, composed entirely of the phosphates, and whose stomach did not admit of the use of stronger acids, carbonic acid was given in water; it was found peculiarly grateful to the stomach, and upon examining the urine during its use, the phosphates were only voided in solution; but when at any time it was left off, they were voided in the form of white sand.

III. *Remarks on Mr.* Richard Walker's *proposed Alterations in the Scales of Thermometers, in our last Number.*

To Mr. Tilloch.

Sir, The reading of Mr. R. Walker's paper on thermometers in your last number, induces me to trouble you, for the purpose of pointing out to that gentleman, what I conceive to have been the reason, why various improvements and suggested reforms, in the weights, measures, and modes of estimating quantities in this country and others, have been neglected and most of them forgotten, viz. their authors' having neglected to asign *new and appropriate names and characters* to the new denominations or things, which it was their object to introduce; but transferring the *old names*, as foot, inch, ounce, pound, degree, &c. &c. to things almost as new and dissimilar, as these are from each other.

If the most precise and short compound words were fixed on, indicative of degrees of heat and degrees of cold, derived perhaps from the Greek or Latin, as being dead or standard languages universally understood, to be used as new prefixes or additions (with distinctive *characters** which could be used as abbreviations of these) to the *number* of thermometric divisions proposed, instead of using either the word *degrees* or the character ° in present use; I do not despair of seeing Mr. W.'s scale or even scales (if each have *their own names and characters*) adopted by many, since

* Perhaps the initial Greek letters of the names might answer these purposes.

negative

negative signs would thus become unnecessary, and no confusion could arise from their use; while on the other hand, every true friend of science and accuracy, will naturally adhere to the divisions *in use*, as answering their intended ends, although not in the best or most perfect manner; and set their face against imperfectly contrived changes.

It would have been desirable, in the last note at bottom of page 420, that Mr. R. Walker had pointed out a material circumstance affecting the uses of mercurial and alcoholial thermometers, viz. the very different periods of time necessary, for each to act in, or acquire and indicate the temperature of any medium under experiment.—See the Monthly Magazine, vol. xvii. p. 213.

I am, sir,

Yours, &c.

July 3, 1810. LONDINENSIS.

IV. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.* *By* HUMPHRY DAVY, *Esq. Sec. R.S. F.R.S.E. M.R.I.A.**

[Continued from vol. xxxv. p. 415.]

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

ONE of the queries that I advanced, in attempting to reason upon the singular phænomena produced by the action of potassium upon ammonia, was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists,

* From Philosophical Transactions for 1810, Part I.

chemists, on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data: the early discovery of Priestley on the passage of gases through red-hot tubes of earthen-ware, the accurate researches of Berthollet, and the experiments of Bouillon La Grange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity*. To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxygen and hydrogen with great rapidity, and in which these gases could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gases, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubic inch; it had been carefully purged of air by the air-pump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygen and hydrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gases; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, *i. e.* $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; six measures mixed with three measures of oxygen diminished to five; so that it consisted of 2·6 of hydrogen, and 3·4 of a gas having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible that at

* See Dr. Pearson's elaborate experiments, on the decomposition of water by electrical explosions. Nicholson's Journal, 4to, vol. i. page 301.

the

the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined: six measures of it, detonated with three measures of oxygen, diminished to rather less than one measure;—a result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydrogen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from those two gases alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated*. The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the

* Nicholson's Journal, August 1809, p. 258.

other in an apparatus in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases for from two to three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxygen from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though, if the asbestus was touched with unwashed hands, or the smallest particle of neutro-saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygen and hydrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese,

ganese, made red hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid was however generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearl-ash, that had been ignited by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phænomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it: figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition

 of

of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal*. The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestley upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{30}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times, in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxygen, $\frac{3}{10}$ hydrogen, and $\frac{6}{10}$ nitrogen.

Mr. Kirwan observed the fact, that when nitrous gas and sulphuretted hydrogen are kept in contact for some

* *Mem. d' Arcueil*, tom. ii. page 485.

time,

time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results, and I found, that the diminution of the volume of the gases when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

In reasoning upon this phænomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that, supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogen over mercury, the barometer being at $29{\cdot}5^{in.}$, thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at $30{\cdot}45^{in.}$, and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen, hydrogen; oxygen and sulphur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompound nitrogen, by agents which I conceived might act at the same time on oxygen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phænomena were very brilliant: as soon as the contact with the potassium was made there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygen gas.

In all trials of this kind, hydrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference that nitrogen is decompounded in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash,* which would furnish water and consequently hydrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogen, nearly equal to $\frac{1}{4}$ of the nitrogen, was produced. It cannot be said that the nitrogen is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogen, and its being separated with the potassium in the form

form of the gray pyrophoric sublimate, which I have found is always produced when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogen; but the greatest part came over densely clouded; and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenour of these inquiries cannot be considered as strengthening in any considerable degree, the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phænomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxygen of which, in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which being condensed in the upper part of the tube, would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubi-

cal

cal inch of ammonia, four of nitrogen, and eight of hydrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hydrogen, and 1·1 nitrogen. The potassium produced in the operation was sufficient to generate 3·1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. Hassenfratz *, that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which, I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hydrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions †, a loss of nitrogen, and a production of hydrogen, was perceived in a case in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the naphtha, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the naphtha: and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451 ‡, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me,

* *Journal des Mines*, *Avril* 1808, p. 275.

† Phil. Mag. vol. xxxiii. page 8. ‡ Ibid. vol. xxxiv. page 389.

against

against the mode of explaining the phænomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phænomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance; at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both, might contain metallic matter united to oxygen: and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gases,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture,

moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive-coloured matter formed on the metal; but though in this case nearly eight cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to $-15°$, there was a very feeble indication given of the addition of hydrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hydrogen was to the nitrogen in the average proportions of from 73·74 to 27·26; the proper corrections being made, and the precautions before referred to being taken*.

Assuming the common estimations of the specific gravity of ammonia, of hydrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807 would be supported by these new experiments; but as the moisture and oxygen visibly separated cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia, I resolved to investigate, more precisely than I had reason to think had been hitherto done, the specific gravities of the gases concerned in their dry state; and the very delicate balance belonging to the Royal Institution placed the means of doing this in my power.

Nitrogen, hydrogen, and ammonia, were dried by a long

* Philosophical Transactions 1809, page 459. M. Berthollet, Jun. in the second volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxygen separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and at the same time he confutes some experiments which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to over-rate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogen may be to the hydrogen as one to three; but neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

continued

continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at $30 \cdot 5^{in.}$ barometer, 51° Fahrenheit's thermometer.

For nitrogen, the 100 cubical inches .....29·8 grains.
For hydrogen, ditto .... 2·27
For ammonia ....18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of $\frac{1}{18}$ *; and if the smallest proportion be taken, the loss will be nearly $\frac{1}{13}$.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible,* that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

* 100 of ammonia at the rate of 185, will give 136·9 of hydrogen, weighing 3·1 grains, and 48·1 of nitrogen, weighing 14·33 grains; but 18·4—17·4 =1, and at the rate of 180, 133 of hydrogen weighing 3·01 and 47 of nitrogen weighing 14, and 18·4—17=1·4.

I have

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of -20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and, when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hydrogen produced.

As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases *.

If

* Even in common air, the amalgam evolves hydrogen and ammonia, nearly in these proportions, and in one experiment which I lately tried, there

If hydrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis, of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phænomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at $\frac{1}{12000}$; but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56 [page 30 preceding] be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1600}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully, these ideas, and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied

there seemed to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rapidly absorb oxygen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly at least, from the mixture of hydrogen. Whether in any case the amalgam absorbs oxygen gas, is a question for further investigation.

to

to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

[To be continued.]

V. *Description and Analysis of the Meteoric Stone which fell at Weston, in North America, the 4th December* 1807. *By* DAVID BAILIE WARDEN, *Esq. Consul-general of the United States at Paris* *.

DESCRIPTION.

THIS aerolite presents, in general, the same characters as those hitherto examined. It is enveloped with a thin, black, and uneven crust. The mass is principally composed of a granular substance, which breaks easily; it has an earthy appearance and a gray cinereous colour, which, in certain parts, passes to a whitish gray.

Those portions which possess this last tint, and which are as if glued in the mass, have a round form, so that they are distinguished by circular or oval spots which interrupt the general colour. Its specific gravity is about 3·3: the sharp parts cut glass.

In observing the fractured parts of the stone, we there perceive: 1°. Particular globules which are easily detached; little cells in which they are placed, and of which the substance is like that of the stone itself, except that its grain is more compact, and its fracture smoother.

In exposing it to a strong light, we see traces of a lamellar tissue: 2°. grains of metallic iron, which, by polish, assume a whiteness, yield to the hammer, and attract very sensibly the magnetic needle: 3°. grains of oxided iron of the colour of rust: 4°. metallic particles extremely small, of a silver white colour, which seem to be of iron; and this opinion is strengthened, when we recollect that the native iron of Kamerdorf, and that of pseudo-volcanic origin, present, in certain parts, a silver white colour.

I have not seen any mark of sulphurated iron, although I found it by the analysis, as will hereafter appear.

All the fragments of this stone have a magnetic property, but without polarity; and the iron, which is very visible in certain parts, is so disseminated in all others where it escapes the eye, that the property of which there is ques-

* From *Annales de Chimie* of March 1810.

tion,

tion, manifests itself even in the smallest particles isolated by trituration.

I found it even in the globulous bodies which are first mentioned.

Pieces of this stone weighed from six to even 100 pounds.

ANALYSIS.

Having ascertained, by preliminary essays, that this stone contained chrome, nickel, iron, manganese, lime, magnesia, silex, alumine, and sulphur; I employed the following method of separating each of these substances.

1°. 100 grains of this stone, from which the metallic iron was isolated, by means of the magnetic needle, after being pulverised, were treated with a considerable quantity of water, through which was passed a current of oxygenated muriatic gas: by this means, the sulphur being converted into sulphuric acid, by the oxygen of the oxygenated muriatic acid, sulphates and muriates were obtained.

2°. The whole was evaporated to siccity, and treated with two parts of alcoholic potash: after fusion the mass presented a fallow colour, and its dissolution in water was of a fine yellow.

3°. The portions of the mass, which remained undissolved in water, were dissolved in an excess of muriatic acid; and being evaporated to siccity, I separated the silex, which after calcination weighed 41 grains.

4°. Into the muriatic acid was poured carbonate of potash in excess, which formed an abundant precipitate after an hour of ebullition.

5°. The liquor contained sulphate and chromate of potash: after being made acid, it was precipitated by muriate of barytes in excess, and there was obtained sulphate of barytes, corresponding to 2⅓ of sulphur: and saturating afterwards the excess of acid by an alkali, I obtained chromate of barytes, corresponding to 2⅓ of chromic acid.

6°. The precipitate, No. 4, was submitted (still in a humid state) to the action of alcoholic potash, and after filtration, the liquor gave, by means of the muriate of ammonia, a grain of alumine.

7°. Ammonia was poured into the remains of the precipitate, after having dissolved it in an excess of muriatic acid. The oxides of iron and manganese were precipitated, and the lime and magnesia remained in dissolution.

8°. The precipitate was isolated, and the lime separated from the magnesia by the oxalate of ammonia, which, after calcination, weighed three grains.

 The

The magnesia was precipitated by caustic potash: it weighed, after desiccation, 16 grains.

9°. The oxides of iron and manganese were dissolved in an excess of muriatic acid, and pouring, by little and little, saturated carbonate of potash into the dissolution until red floccules were visible, and then leaving it to repose 24 hours, all the carbonate of iron precipitated, whilst that of the manganese remained in the liquor.

The carbonate of iron, after calcination, gave 24 grains of oxide: and that of manganese, deposited by ebullition, by the same operation, only $1\frac{1}{3}$.—Which makes in all:—

Silex	41
Sulphur	$2\frac{1}{3}$
Chromic acid	$2\frac{1}{3}$
Alumine	1
Magnesia..............	16
Lime	3
Oxide of iron	30
Oxide of manganese....	$1\frac{1}{3}$
Loss	3
Total......	100

Analysis of the metallic iron isolated by the magnetic needle. 1°. 100 parts of this stone gave 28 of metallic iron, which is very brittle, owing to the nickel it contains. 2°. 40 grains of this iron were dissolved in nitro-muriatic acid, and by means of ammonia in excess the oxide of iron was separated, which weighed 45 grains. The dissolution of nickel in this alkali was evaporated to siccity to expell all the ammonia. The oxide of nickel was redissolved by muriatic acid, and precipitated by the prussiate of potash, which gave one grain of the prussiate of nickel. We may infer, from these physical characters, and results of chemical analysis, that this stone is like all other meteoric stones hitherto known.

V. *Proposal for constructing, and putting in its Place, an Iron Tunnel under the River Thames. By Colonel* Lennox.

To Mr. Tilloch.

Sir, I herewith send you a plan for a tunnel under the Thames, which I hope you will not deem unworthy of a place in your valuable Magazine.

Being

Being informed that no particular plan has yet been determined on by the Thames Tunnel Company *, they are extremely welcome to adopt this if they think fit. I cannot avoid encouraging a hope, that it will be found practicable: but, should my partiality render me too sanguine in favour of it, as the idea, I believe, is new, the publication of it may lead to some other of more ingenuity, and which may be easier and safer in the execution.

Explanation of a plan for constructing a tunnel of cast-iron under the river Thames:—

Fig. 1. A A, (Plate II.) section across the river. The waving line shows the present depth of the river; *a a*, the additional depth required by the plan.

Fig. 2. B B B, three of the frames of which it is proposed that the whole tunnel shall be formed: they are to be of cast-iron, each of one piece, and to be joined together by the flanches *d d d*, which are all one foot broad and four inches thick, with the screws *e e e*, in figs. 2 and 4, of four inches diameter, with half-inch sheet-lead between:—or the joints may be secured with the cement employed by steam-engine builders.

Fig. 3. C C, section of the tunnel, showing the above three frames, in figs. 2, in perspective; each frame to be ten feet in length, eighteen feet wide inside, and twelve feet high at the sides; the top to be convex, rising two feet in the middle; to be four inches thick at the bottom and sides, and three at top. Each frame will weigh upwards of forty tons †.

Fig. 4. D D D, elevation of the same frames, which shows the screws that unite the exterior flanches, and also the iron cramps, *f f*, which embrace the two adjoining flanches at bottom; these cramps to be each twelve inches broad, six inches thick, and two feet high.

Fig. 3. *g g*, tubes of eight inches bore, with openings to receive leakage water, and to convey it to one of the ends to be pumped out.

i i i, screw-holes. The dotted line *k k* expresses the paving when the whole is completed.

* I believe the Thames Tunnel Company have settled the plan they mean to follow; but as the ideas suggested by Col. Lennox may prove useful on some future occasion, I have given it a place in this number.—Edit.

† Should the carriage of pieces of this weight from the foundry be found impracticable, the sides, and top and bottom, may be cast in separate pieces, with flanches to join them at the corners. In this case the joinings of the different parts may be so disposed that no two of the transverse joinings shall coincide, which will give additional strength to the whole as every joint may thus be supported with three solid pieces at that place in which it falls.

By the section across the river it appears that the depth of the bed at low water (being only about 30 feet) is not sufficient to admit of laying down a tunnel such as I propose upon the bottom, without obstructing the course of the stream, or interfering with the ease of navigation: therefore, the first thing necessary would be, to excavate the bed of the river entirely across to about 16 feet deeper, and from 60 to 80 feet wide; and to render it as even as possible throughout; which I conceive may be effected without extraordinary labour or difficulty. This being done as far as from *b* to *c*, fig. 1, about 800 feet or something more, I will next suppose eighty of those frames, previously formed agreeably to the plan and section, figs. 2 and 3, to be joined by screwing them strongly together, as represented in figs. 2 and 4, with half-inch sheet-lead between the flanches; which operation should be performed on the bank of the river rather below the level of low water, in a situation where the tide may have free access to it.

If then the ends of these 80 tunnel-frames so joined be (when empty) close stopped with strong oak plank, and well secured so as to render them perfectly water-tight, *a machine is formed which on the admission of the tide will float;* as may be proved by the subjoined calculation, which for greater security does not include the convexity of the top. At spring-tide, therefore, the whole may be floated to the required situation, and *by additional weights applied* sunk in its proper place. But in case of any irregularity in its descent, or unevenness of the bottom prepared to receive it, *by removing those additional weights it will again become buoyant,* when the necessary remedies may be applied and obstacles removed. When once properly placed, by turning cocks fixed in each end it will soon fill with water and be permanently bedded.

Calculation of the weight of this tunnel in round numbers:—

Cast-iron ..	20,020 cubic feet .... about	4,270 tons.
Lead	566	178
Oak	200	5
	Tons ..	4,453
Water displaced	1,850,000 cubic feet ...	5,162
This tunnel will require to sink it more than		709
Exclusive of the convexity at top estimated at		60
	Total ..	769

With

With respect to the manner of sinking this machine, I propose that two short ropes of sufficient strength, with loops at each end, should be passed over each frame, and slightly secured to keep them in their places; that when the machine is floated to its destined situation, (which should be about an hour before low water at the lowest tide,) anchors and cables shall be in readiness to secure it in its place; and that then a number of boats (suppose 160) shall attend, half on one side and half on the other, each with five tons of ballast conveniently disposed so as immediately to hook on to the ends of the short ropes before mentioned, in such a manner that one end of the tunnel shall not sink before the other, but both exactly together. These weights may be so regulated as occasion may require, should there appear any irregularity in its descent; and when it is placed as desired, and water admitted to fill it, they may be removed altogether*. The whole of this operation may, I am persuaded, be effected in two hours, that preceding and that following ebb-tide, if every previous arrangement be properly made.

This part of the tunnel is then supposed to occupy the space from *b* to *c*, fig. 1, and to be placed so as that the upper surface of it shall be nearly equal to, or rather below, the present bottom represented by the waving line. After which, by piling off the tide from low-water mark, the ends may be finished, as on dry land; which may be done either by a continuation of the same frames, or by arches of masonry or of brickwork, as may be judged best. It will then only remain to open the communication with the middle part, by removing the oak planking at each end, and pumping out the water; when, by laying a sufficient quantity of ballast so as to form a road-way clear above the lower flanches, and restoring the banks to their former state, the tunnel will be immediately ready for use

In the execution of this project a situation should be selected close to low-water mark, of nearly 300 yards in length, where it would be necessary to lay down blocks of sufficient strength to support the great weight, and upon which the whole 80 frames may be screwed together, the level of which should be at least 15 feet below that of spring tides to ensure its floating when completed.

It may be objected to this great machine, that from its

* Or perhaps the sinking may be conveniently effected by merely admitting from 800 to 1000 tons of water into the tunnel, a pump of sufficient power being properly secured in each end frame to pump out 200 or 300 tons, should it be found necessary to float the tunnel again.

vast weight and great length, the power of 67 screws at each joining would be inadequate to hold the whole perfectly together; and that in case of accident the whole must be infallibly lost, as it would then be impossible to remove it from the spot on which it would immediately sink, or even to separate the different parts of it. But as the tunnel formed in the manner proposed will *be subjected to no particular force* whatsoever at its launching, but be altogether equally borne up by the rising tide; as the weights necessary to sink it may be all so gradually applied as to ensure its regular descent, to which the form of the whole when joined as above, viz. convex at the top and rising at the ends, together with the greater thickness of the metal at the bottom, are circumstances particularly favourable; and as from the nature of the bottom it is sure to rest on a soft and uniform bed of sand, on which it cannot meet with any object to occasion any *partial bearing*,—I conceive the danger of accident is very remote, and the strength of the entire sufficiently secured: besides, trials may be made in a safe situation.

The chief difficulty appears to me to be the excavation of the bed of the river to the depth required. The best mode of effecting this, or whether it would not be better to choose another situation in which the existing depth might be found sufficient, I leave to more able and experienced engineers to determine; stating merely, that as the materials of which the tunnel is to be composed can be procured for about £44,000; allowing fifty per cent. additional for all other charges incurred in its execution, I do not conceive the expense would exceed the sum of £66,000.

I beg leave further to add, that if it should be desired to enlarge this tunnel so as to afford a foot-path in addition to the space allowed for two carriages to pass, I conceive it may safely be done by giving it six more feet in width, making altogether 24 feet between the interior flanches; and in order to afford it still greater strength, I would in this case omit the interior lateral flanches, and in the room thereof, apply plates of cast-iron of three or four inches thick, the full height of the sides, to extend from the middle of one frame to that of the next, to be fastened by a number of the same kind of screws to the sides of the two adjoining frames, with sheet-lead between and completely covering the joint inside. This would give the tunnel great additional strength without much increasing its weight, besides that it would leave nearly a foot more of free

free space inside; the increase of expense on this account would not, I suppose, much exceed twelve or fifteen thousand pounds, in addition to that before stated.

I am, sir,
Your obedient humble servant,
Bath, June 1810. W. CAULFIELD LENNOX.

VI. *Six Theorems, containing the chief Properties of all* Regular Douzeave Systems of Music; *with Twelve Corollaries thence deduced, showing others of their Relations; and Thirteen Scholia, containing the Temperaments of as many Systems, calculated thereby. With Remarks. By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR, I AM much pleased to observe, that at length a beginning has been made, at publishing tables of the Beats in 15″; made by the 72 concords in different systems of Musical Temperament, by your new correspondent Mr. C. J. Smyth, of Norwich, in your last Number, who will, I hope, persevere, and give us tables of many other systems, accompanied by such critical remarks, on their comparative merits and defects in practice, as he appears well qualified to make, either in your Magazine, or in the separate work which he has announced on the subject.

Some time ago, I had thoughts of preparing a work on Harmonics, perhaps as a kind of supplement to Dr. Robert Smith's justly celebrated work; but the prospect being now rather distant, of my being able to find leisure to complete this design, I am induced by the above paper of Mr. Smyth's, and the publication of a small work on Harmonics, by Mr. J. Marsh of Chichester, to transcribe from my papers some *Theorems*, showing the properties of *regular douzeaves*, or of such systems of twelve notes in the octave, as have all their fifths alike tempered, except, that between ♯G and ♭E, when there is a bearing fifth or quint wolf: but first I beg to make a few remarks.

In *douzeaves*, or systems of twelve notes, there are generally 16 wolves or tempered concords, differing* from the

* Wolves, taken in their general sense, are not always larger than the temperaments, but may be equal to them, as happens throughout the isotonic or equal temperament scale, and may even be less than their respective temperaments, in some cases, as in scholia 1 and 7: they are, in fact, the places in the douzeave or other defective scales, where the resulting intervals or unavoidable inequalities fall, and, as such, are very important to be known and attended to by the composers of music, to be performed in such scales.

the temperaments proper to the six several concords respectively.

In *regular douzeaves*, none of these wolves occur, in any of the six concords, upon any of the four middlemost key-notes, viz. G, D, A or E respectively.

C, F, ♭B and ♭E have no wolves in their major concords (*i e.* the III, V or VI upon them, respectively.)

B, *F, *C and *G have no wolves in their minor concords (*i. e.* 3d, 4th, or 6th.)

Whence it follows (see Dr. Smith's Harmonics, Plate XIX, p. 162, 2d edit.) that in *major Keys*, modulation can be made from C by *s, through the keys G, D, A and E without false concords or wolves; but if we proceed further by *s, B has a false III, *F has its III and VI false, *C has its III and VI false, and *G has its V, III and VI false, which last chord Mr. Smyth calls *the wolf*, by way of eminence, p. 450 of last volume.

And in major keys, modulation can also be made from C by ♭s, through F, ♭B, and ♭E, and no major wolves occur, (yet ♭E has a false 4th); but on proceeding further to ♭A (or *G) its V, III and VI are false, as above.

In *minor Keys*, modulation can be made from A by *s through E, B, *F, *C, and *G, without any minor wolf (yet *G has a false Vth), but on proceeding further to *D (or ♭E), its 4th, 3d and 6th are false.

And in minor keys, modulation can also be made from A by ♭s through D and G, without any false concords or wolves; but on continuing thus to modulate, C has a false 6th, F has its 3d and 6th false, ♭B its 3d and 6th false, and ♭E (or *D) has its 4th, 3d, and 6th false, as above.

The six following *Theorems*, express in terms, of the fractions $\frac{r}{s}$ and $\frac{t}{u}$ (either proper or improper) of the small intervals *Schisma* and *most Minute** or Σ and m, all the temperaments and wolves of the 72 concords, in any regular douzeave; and whence, such temperaments can readily be calculated for any proposed system, or the various properties and relations of its intervals can be discovered and computed: and by means of other theorems, the beats can be calculated from such temperaments, in terms of Σ and m. In the article BEATS in the "Edin-

* See vol. xxviii. p. 142, and engraved table, plate V.

burgh

burgh Encyclopædia," such a set of Theorems will shortly be given, with examples of the use of each, as will perhaps supersede the necessity of publishing them in your Magazine.

Theorem 1. In any *regular douzeave*, or system of 12 notes, where

11 major *fifths* are each tempered (flat) by .... $\frac{-r}{s}\Sigma + \frac{-t}{u}\text{m}$;

throwing the bearing, resulting fifth or wolf on ✱G (that is between ♭A and ♭E);

then, the value of this resulting Vth or wolf will be .... $\frac{11r-12s}{s}\Sigma + \frac{11t-u}{u}\text{m}$.

Theorem 2. And, 11 *minor fourths* will each be (sharp) .... $\frac{r}{s}\Sigma + \frac{t}{u}\text{m}$;

and, the resulting 4th or wolf on ♭E will be .... $\frac{12s-11r}{s}\Sigma + \frac{u-11t}{u}\text{m}$.

Theorem 3. And, 8 *major thirds* will each be (sharp) .... $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}\text{m}$;

the 4 resulting IIIds or wolves on ✱G, B, ✱F and ✱C being each ...... $\frac{8r-s}{s}\Sigma + \frac{8t}{u}\text{m}$.

Theorem 4. And, 8 *minor sixths* will each be (flat) .... $\frac{4r-11s}{s}\Sigma + \frac{4t-u}{u}\text{m}$;

the 4 resulting 6ths or wolves on C, ♭E, ♭B and F being each .... $\frac{s-8r}{s}\Sigma + \frac{-8t}{u}\text{m}$.

Theorem 5. And, 9 *minor thirds* will each be (flat) .... $\frac{3r-11s}{s}\Sigma + \frac{3t-u}{u}$m;

the 3 resulting 3ds or wolves on ♭E, ♭B and F being each .... $\frac{s-9r}{s}\Sigma + \frac{-9t}{u}$m.

Theorem 6. And, 9 *major sixths* will each be (sharp) .... $\frac{11s-3r}{s}\Sigma + \frac{u-3t}{u}$m;

the 3 resulting VIths on *F, *C and *G being each .... $\frac{9r-s}{s}\Sigma + \frac{9t}{u}$m.

Whence we may readily derive many useful Corollaries, viz.

Corollary 1. The sum of the 11 temperaments of the *fifth*, and its wolf, is a *constant quantity*, viz. 12Σ + m, or the *Diaschisma*.

For $\frac{-11r}{s}\Sigma$ is the first part of the sum of the temperaments in theorem 6, and $\frac{11r-11r-12s}{s}\Sigma$ or $\frac{-12s}{s}\Sigma$ $= -12\Sigma$ is the aggregate of this sum and the wolf: likewise $\frac{-11t}{u}$m + $\frac{11t-u}{u}$m is the aggregate of the remaining part, $= \frac{-u}{u}$m $= -$m, and therefore 12Σ + m with negative signs, indicating a *flat* temperament, is that which belongs to the fifths collectively, as I observed in volume xxix. p. 348: yet, Mr. J. Marsh in his recent "Theory of Harmonics," p. 14, says, that the same is "a *Diesis* or quarter tone," "the same degree as the 3 thirds mentioned in § 27," that is, 21Σ + 2m!! See cor. 3.

Corollary

Corollary 2. The sum of the 11 temperaments of the minor *fourth*, and its wolf, is constantly $12\Sigma + \mathrm{m}$, as observed above.

Corollary 3. The sum of the 8 temperaments and 4 wolves of the *major thirds*, is $84\Sigma + 8\mathrm{m}$, or four enharmonic Diesises.

Corollary 4. The sum of the 8 temperaments and 4 wolves of the *minor sixths*, is $-84\Sigma - 8\mathrm{m}$, as above.

Corollary 5. The sum of the 9 temperaments and 3 wolves of the *minor thirds*, is $-96\Sigma - 9\mathrm{m}$, or three Semitones minimum.

Corollary 6. The sum of the 9 temperaments and 3 wolves of the *major sixths*, is $96\Sigma + 9\mathrm{m}$, as above.

Corollary 7. The sum of the temperaments of the minor third and of the major third, is equal to the temperament of the fifth.

For $\frac{3r-11s}{s} + \frac{11s-4r}{s} = \frac{-r}{s}$, the first part of the temperament of the fifth, and so of the latter part.—(See Dr. Smith's Harmonics, cor. 6, p. 42.)

Corollary 8. The sum of the temperaments of the fifth and of the major sixth, is equal to the temperament of the major third.

For $\frac{-r}{s} + \frac{11s-3r}{s} = \frac{11s-4r}{s}\Sigma$; also $\frac{-t}{u} + \frac{u-3t}{u} = \frac{u-4t}{u}\mathrm{m}$, or $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}\mathrm{m}$, as in theorem 3. (See Dr. Smith's Harmonics, cor. 8, p. 43.)

Corollary 9. The difference between the *wolf* and the *temperament* of each of the six concords respectively, is the same, viz. $\frac{12r-12s}{s}\Sigma + \frac{12t-u}{u}\mathrm{m}$, and is what Dr. Smith, at pages 160, 223, &c. calls the *Diesis*; it is the difference between adjacent flat and sharp notes, as between ✱D and ♭E, ✱A and ♭B, &c. p. 163.

For $\frac{11r-12s}{s} - \frac{-r}{s} = \frac{12r-12s}{s}\Sigma$, and $\frac{11t-u}{u} - \frac{-t}{u} = \frac{12t-u}{u}\mathrm{m}$, in the fifths. Also, $\frac{8r-s}{s} - \frac{11s-4r}{s} = \frac{12r-12s}{s}\Sigma$, and $\frac{8t}{u} - \frac{u-4t}{u} = \frac{12t-u}{u}\mathrm{m}$, in the major third, as before, and so of all the others.

Corollary

Corollary 10. The *minor Limma* of Dr. Smith, p. 223, or the value of a *sharp* or a *flat*, is, $\frac{58s-7r}{s}\Sigma + f + \frac{5u-7t}{u}m$; or, without f*, we have $\frac{58\cdot1496s-7r}{s} + \frac{5\cdot0136u-7t}{u}m$.

As is easily deduced from my theorems above, by the process in page 158, of the Harmonics. As is also the following,

Corollary 11. The *major Limma* of Dr. Smith is $\frac{46s+5r}{s}\Sigma + f + \frac{4u+5t}{u}m$; or, without f, we have $\frac{46\cdot1496s+5r}{s}\Sigma + \frac{4\cdot0136u+5t}{u}m$.

Corollary 12. The *mean Tone* of regularly tempered systems, is $\frac{104s-2r}{s}\Sigma + 2f + \frac{9u-2t}{u}m$; or, without f we have $\frac{104\cdot2992s-2r}{s}\Sigma + \frac{9\cdot0272u-2t}{u}m$.

By multiplying these general Corollaries, I should, perhaps, exceed your limits. I must therefore content myself for the present with the following, as particular applications of the theorems and corollaries above, viz.

Scholium 1. If a douzeave be required, in which *the fifths should be perfect*, we have by theorem 1, $\frac{-r}{s} + \frac{-t}{u} = 0$; which condition will be answered, if r and t each $= 0$ and s and u each $= \pm 1$; which values substituted in the fifth wolf $\frac{11r-12s}{s}\Sigma + \frac{11t-u}{u}m$, gives $-12\Sigma - m$ as it ought to do, by cor. 1.

Also, in theorem 3, $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}m$ gives $11\Sigma + m$ or c, a major comma, as the sharp temperament of each major third in this system; also in theorem 6. $\frac{11s-3r}{s}\Sigma + \frac{u-3t}{u}m$ gives $11\Sigma + m$ or c, as the sharp tem-

* By an examination of plate V in your 28th volume, and of a more extensive table of Intervals, it appears, that the number of Σs, always exceeds the number of m's in ratios between those of 12·5 : 1 and 10·: 1 (those of $ and Є). But the major *comma* and its aliquot parts, most frequently occurring in temperaments, I have adopted its ratio of 11 : 1, and thus find $f = \cdot1496\Sigma + \cdot0136m$; from which equation, the latter ones in cor. 10, 11 and 12 are obtained.

temperament of the major sixths; which last temperaments prove the same as they must do, to fulfil the conditions of cor. 8.

Though the fifth wolf is here so large, the IIIds and VIths wolves are each Σ only.

This system approaches very near to that of Mercator, wherein the octave is divided into 53 equal parts, and where $-\cdot 035094\Sigma$ is the flat temperament of the fifth. See M. Sauveur's general table of tempered systems, Mem. de l'Acad. 1711, 16mo. p. 416.

Scholium 2. If a douzeave be required, in which *the major thirds should be perfect,* we have in theorem 3 $\frac{11s-4r}{s}=0$, or $11s=4r$, whence $\frac{r}{s}=\frac{11}{4}$; also $\frac{u-4t}{u}=0$, or $u=4t$ and $\frac{t}{u}=\frac{1}{4}$; whence it appears, that the fifth is to be flattened $\frac{11}{4}\Sigma+\frac{1}{4}\text{m}$, or $\frac{1}{4}\text{c}$; and, by substituting the above values in the wolf, the same appears to be $21\Sigma+2\text{m}$, as in cor. 3; and by the same in theorem 6, we get $\frac{11}{4}\Sigma+\frac{1}{4}\text{m}$, or $\frac{1}{4}\text{c}$, the sharp temperament of the major sixth; the same with that of the fifth; see cor. 8.

This is the *Mean Tone* system of Salinas, Zarlino, Aretinus, &c. Dr. Smith's Harmonics, p. 36, 41, &c. wherein the adjacent flat and sharp notes are distant $21\Sigma+2\text{m}$, or an enharmonic Diesis, as appears by substituting the above values of $\frac{r}{s}$ and $\frac{t}{u}$ in cor. 9.

It is also nearly the same with a division of the octave into 112 equal parts, (see M. Sauveur's table above quoted), wherein $-2\cdot 8290\Sigma$ is the flat temperament of the fifth; $\frac{1}{4}\text{c}$ above, being $-2\cdot 751966\Sigma$.

Scholium 3. If a douzeave be required, in which *the major sixths should be perfect,* we have in theorem 6, $\frac{11s-3r}{s}=0$, or $11s=3r$, whence $\frac{r}{s}=\frac{11}{3}$; also $\frac{u-3t}{u}=0$, or, $u=3t$; whence $\frac{t}{u}=\frac{1}{3}$, and $\frac{11}{3}\Sigma+\frac{1}{3}\text{m}$, or, $\frac{1}{3}\text{c}$, is here the flat temperament of the fifth: and by sub-

substituting the same in the wolf we get $32\Sigma + 3m$; as in corollary 6; likewise in theorem 3, we have $\frac{11}{3}\Sigma + \frac{1}{3}m$, or $\frac{1}{3}c$, the sharp temperament of the major third also, as is consistent with cor. 8. The IIId and Vth wolves are here each $28\frac{1}{3}\Sigma + 2\frac{2}{3}m$. (See Harmonics, p. 42.)

This system approaches very near to a division of the octave into 19 equal parts, where $-3{\cdot}6947\Sigma$ is the flat temperament of the fifth, (Dr. Smith, Harmonics, p. 158, makes it $-\frac{1}{4}c - \frac{3}{35}c = -\frac{47}{140}c$ or $-3{\cdot}6955\Sigma$); $-\frac{1}{3}c$ above, is $-3{\cdot}669288\Sigma$. See M. Sauveur's table.

The cases of equality of temperaments between the III and VI, and between the V and VI having occurred, in schol. 1 and 3, I proceed to

Scholium 4. If a douzeave be required, in which *the major fifth shall be as much tempered flat as the major sixth is sharp*; from theorems 1 and 6 we have $\frac{-r}{s} = \frac{11s-3r}{s}$, or $11s = 2r$; whence $\frac{r}{s} = \frac{11}{2}$; also $\frac{-t}{u} = \frac{u-3t}{u}$, or $u = 2t$; whence $\frac{t}{u} = \frac{1}{2}$; and $\frac{11}{2}\Sigma + \frac{1}{2}m$, or $\frac{1}{2}c$, is here the temperament of the fifth: and by substituting this value in theorem 6, we have $\frac{11}{2}\Sigma + \frac{1}{2}m$, or $\frac{1}{2}c$, for the temperament of the major sixth also. The V and VI wolves are here each $48\frac{1}{2}\Sigma + 4\frac{1}{2}m$.

Scholium 5. If a douzeave be required, in which *the major fifth shall be as much tempered flat as the major third is tempered flat*; from theorems 1 and 3, we have $\frac{-r}{s} = \frac{4r-11s}{s}$, or $5r = 11s$; whence $\frac{r}{s} = \frac{11}{5}$; also, $\frac{-t}{u} = \frac{4t-u}{u}$ or $5t = u$, whence $\frac{t}{u} = \frac{1}{5}$; and $\frac{11}{5}\Sigma + \frac{1}{5}m$, or $\frac{1}{5}c$, is here the temperament of the fifth: and by substituting this value in theorem 3, we have $\frac{11}{5}\Sigma + \frac{1}{5}m$, or $\frac{1}{5}c$, for the flat temperament of the

major

major third also: the temperaments of the major sixths being $\frac{22}{5}\Sigma + \frac{2}{5}m$, or $\frac{2}{5}c$.

This is the system of Mr. John Holden, since metamorphosed into an *irregular douzeave* in the article *Monochord*, Enc. Brit. 3d edit. vol. xii. p. 240, and into a still different one, by the Rev. Mr. Hawkes. (See your xxvith volume, p. 171, and xxxth volume, p. 5.)

It also approaches near to M. Sauveur's system of 43 equal parts in the octave, (see his general table above referred to) wherein $-2{\cdot}1177\Sigma$ is the flat temperament of the fifth; $\frac{1}{5}c$ above, being $-2{\cdot}201573\Sigma$.

One other case of this kind, viz. where the major thirds are tempered flat as much as the major sixths are tempered sharp, will be found to arise from different considerations in scholium 11.

Scholium 6. If a douzeave be required, in which *the temperament and wolf of the fifth shall be equal*, we have from theorem 1, $-r = 11r - 12s$, or $12r = 12s$, whence $\frac{r}{s} = \frac{1}{1}$; also $-t = 11t - u$, or $12t = u$, whence $\frac{t}{u} = \frac{1}{12}$, and $\Sigma + \frac{1}{12}m$, or $\frac{1}{11}c$, is the flat temperament of the fifth, in this case: and which substituted in theorem 3, either for wolf or temperament, gives $7\Sigma + \frac{2}{3}m$, or $\frac{7}{11}c$, the sharp temperament of the major third; also, in theorem 6, gives $8\Sigma + \frac{3}{4}m$, or $\frac{8}{11}c$, the sharp temperament of the major sixth.

This is the *Isotonic* or Equal Temperament System of Mersennus, &c. called by Mr. Marsh and others, though improperly, the Equal Harmony System (see scholium 10). See vol. xxix. p. 347: see also Dr. Smith's Harmonics, p. 158 and 167. In the latter page, however, the temperaments of the Vth, VIth and IIId are mistakenly said to be $\frac{1}{10}$, $\frac{7}{10}$ and $\frac{6}{10}$, instead of $\frac{1}{11}$, $\frac{8}{11}$ and $\frac{7}{11}$ of a comma, as they are above, very nearly.

Scholium 7. If a douzeave be required, *in which the several wolves shall differ from their respective temperaments, by the least known Interval or most Minute* (m): we have

have from corollary 9, $\frac{12r-12s}{s}=0$, or $12r=12s$; whence $\frac{r}{s}=\frac{1}{1}$; also, $\frac{12t-u}{u}=-1$, whence $\frac{t}{u}=\frac{0}{1}$. By which we obtain in theorem 1, $-\Sigma$ as the flat temperament of the fifths, and $-\Sigma$ —m its flat wolf: also, in theorem 3, we get 7Σ +m the sharp temperaments of the major thirds, and 7Σ their wolves; and by theorem 6, we obtain 8Σ + m the sharp temperaments of the major sixths and 8Σ their wolves.

This is my *Equal Temperament System*, whose tempered fifth, and consequently all its other intervals, can be *tuned* on an Organ *by means of perfect intervals only*, viz. 5 4ths — 3 Vths — III = V — Σ. (See vol. xxviii. p. 65); such tuning to be upwards from C as far as ♭A and downwards from *c* as far as ♭E, between which notes, the wolf Σ +m, will result. The *beats* calculated by Mr. Smyth, at page 452 of your last volume, belong in fact to this system, and not to the strict *Isotonic* above, but the difference in practice would be imperceptible between these two systems.

Scholium 8. If a douzeave be required, in which *the ratio of the temperaments of the major thirds shall be to their wolves* as $1\frac{1}{4}$ to $3\frac{1}{2}$*, that is, or as 5 to 14, we have from theorem 3. As $5:14::\frac{11s-4r}{s}:\frac{8r-s}{u}$; whence $154s-56r=40r-5s$, or $159s=96r$, and $\frac{r}{s}=\frac{53}{32}$; also, as $5:14::\frac{u-4t}{u}:\frac{8t}{u}$, whence $14u-56t=40t$, or $14u=96t$, and $\frac{t}{u}=\frac{7}{48}$: and $-\frac{53}{32}\Sigma-\frac{7}{48}$m is the flat temperament of the fifth, which substituted in the first theorem gives $\frac{199}{32}\Sigma+\frac{29}{48}$m,

* Mr. Marsh, assuming the true major third to be 48 degrees or parts, states, the tempered III to be $=49\frac{1}{4}$, and the wolf or "extended third," (as he elsewhere calls it) to be $51\frac{1}{2}$ parts: in the system which he most approves; I therefore take the excess of these above the IIId, as giving the ratio of his temperament and wolf, in order to obtain the values of his notes in my theorems. It is not however clear, that such is exactly his meaning; since, 150 being assumed as the measure of the *octave*, the values of the *major third* and of the *diesis* can be no other than 48·28921, &c. and 5·132378, &c. or very nearly in the ratios of 612Σ, 197Σ and 21Σ: and it is not possible for 48 and 6 truly to represent the major third and diesis in such octave, or in any other.

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the sharp wolf. In theorem 3, we get the sharp temperament of the major third $= 4\frac{3}{8}\Sigma + \frac{5}{12}m$, and its wolf $= 12\frac{1}{4}\Sigma + \frac{7}{6}m$. And in theorem 6, the sharp temperament of the major sixth $= 6\frac{1}{32}\Sigma + \frac{9}{16}m$, and its wolf $13\frac{29}{32}\Sigma + \frac{21}{16}m$.

This is Mr. J. Marsh's approved method of tuning a douzeave. Theory of Harmonics, page 13.

The system nearest to this of any which I remember to have met with, is that of a division of the octave into 67 equal parts, (see M. Sauveur's table above referred to), where $-1{\cdot}76455\Sigma$ is the flat temperament of the fifth, which here is $-1{\cdot}6574\Sigma$; and the same differs considerably from the other system recommended by Mr. Marsh page 18, which, perhaps after the example of Dr. Smith, he has borrowed from M. Henfling without acknowledgement. See my 10th scholium.

By Dr. Smith's Harmonics, 2d edit. p. 84, prop. XI, latter part of cor. 3, "when the bases and beats (of two tempered consonances) are the same, the temperaments have *ultimately** the inverse ratio of the major terms" of the perfect ratios of these consonances. Whence

Scholium 9. If a douzeave be required, wherein *the fifth* ($\frac{2}{3}$) *and the major third* ($\frac{4}{5}$) *to the same base shall beat equally quick*, the former flat and the latter sharp; we have from theorems 1 and 3, as $5 : 3 :: \frac{-r}{s} : \frac{11s-4r}{s}$, whence $55s-20r = -3r$, or $55s = 23r$, and $\frac{r}{s} = \frac{55}{23}$; also, $5 : 3 :: \frac{-t}{u} : \frac{u-4t}{u}$, whence $5u-20t = -3t$,

* The ultimate ratios are in these cases, very near to the exact ratios: thus in scholium 9, $\frac{5}{23}c$, or $2{\cdot}393013\Sigma$, results from the ultimate ratios; the true temperament being $2{\cdot}393693\Sigma$, as derived from the length of string l of the Vth, in the equation $4l^4 - l^3 = \frac{1}{2}$; the difference being less than $\frac{7}{10000}$th of a Σ, an interval altogether imperceptible in practice.

or $5u = 23t$, and $\frac{t}{u} = \frac{5}{23}$; and $\frac{55}{23}\Sigma + \frac{5}{23}$m or $\frac{5}{23}$c is the flat temperament of the fifth. In theorem 3, we get $18\frac{3}{23}\Sigma + \frac{17}{23}$m the sharp temperament of the major third; and in theorem 6, $20\frac{12}{23}\Sigma + 1\frac{22}{23}$m the sharp temperament of the major sixth.

This is the system *for defective scales* which Dr. Smith describes, and recommends, pages 219, 215, 189, 211, and 212: and of which Mr. Atwood has (but without acknowledgement) given the lengths of strings in his "Rectilinear Motion, &c."

A system, wherein the octave is divided into 74 equal parts, to be found in M. Sauveur's table, and where the temperament is V—2·3838Σ, differs but very little from V—2·393012Σ in this system.

Scholium 10. If a douzeave be required, wherein *the fifth* ($\frac{2}{3}$) *and the major sixth* ($\frac{3}{5}$) *shall beat equally quick*, the former flat and the latter sharp: we have from theorems 1 and 6, as $5 : 3 :: \frac{r}{s} : \frac{11s-3r}{s}$, whence $55s - 15r = 3r$, or $55s = 18r$ and $\frac{r}{s} = \frac{55}{18}$; also, as $5 : 3 :: \frac{t}{u} : \frac{u-3t}{u}$, whence $5u - 15t = 3t$ or $5u = 18t$ and $\frac{t}{u} = \frac{5}{18}$; and $3\frac{1}{18}\Sigma + \frac{5}{18}$m or $\frac{5}{18}$c is the flat temperament of the fifth: which in theorem 3, gives $-\frac{11}{9}\Sigma - \frac{1}{9}$m or $\frac{1}{9}$c, for the flat temperament of the major third: and in theorem 6, gives $\frac{11}{6}\Sigma + \frac{1}{6}$m or $\frac{1}{6}$c, the sharp temperament of the sixth.

This is the famous *System of Equal Harmony* in 3 octaves, invented by Dr. Robert Smith. See his Harmonics, pages 216, 191, 206, 212, 214, &c. And differs but little from M. Henfling's system, (Mem. de l'Acad. 1711, 16mo, p. 408), wherein the octave is divided into 50 equal parts, as Dr. Smith shows in his Harmonics, p. 157, and states its fifth to be be flattened $-\frac{1}{4}$c $-\frac{1}{37}$c or $-\frac{41}{148}$c, which is $-3{\cdot}04966\Sigma$; or more correctly it is $-3{\cdot}04811\Sigma$; $\frac{5}{18}$c being $-3{\cdot}05774\Sigma$ in the system of this scholium.

Scholium 11. If a douzeave be required, wherein *the major third* ($\frac{4}{5}$) *and the major sixth* ($\frac{3}{5}$) *shall beat equally quick*, the former sharp and the latter flat; we have from theorems 3 and 6, as $5 : 5 :: \frac{11s-4r}{s} : \frac{3r-11s}{s}$, whence $11s-4r=3r-11s$ or $22s=7r$, and $\frac{r}{s}=\frac{22}{7}$; also, as $5 : 5 :: \frac{u-4t}{u} : \frac{3t-u}{u}$, whence $u-4t=3t-u$ or $2u=7t$ and $\frac{t}{u}=\frac{2}{7}$; and $\frac{22}{7}\Sigma + \frac{2}{7}$m or $\frac{2}{7}$c is the temperament of the fifth. Which in theorem 3, gives $-\frac{11}{7}\Sigma - \frac{1}{7}$m, or $\frac{1}{7}$c, the flat temperament of the major third; and in theorem 6, gives $\frac{11}{7}\Sigma + \frac{1}{7}$m, or $\frac{1}{7}$c, the sharp temperament of the major sixth*.

This is the system which Dr. Smith barely mentions at page 220, on account of its differing so little from equal harmony, in my last scholium; wherein $\frac{5}{18}$ or $\frac{35}{126}$c is the temperament of the fifth, which here is $\frac{2}{7}$, or $\frac{36}{126}$; the difference being only the $\frac{1}{126}$-th part of a major comma or $\cdot087364\Sigma$: also $\frac{1}{9}$ or $\frac{7}{63}$, and $\frac{1}{7}$ or $\frac{9}{63}$, have a difference of $\frac{2}{63}$c, or $\cdot34946\Sigma$, but little more considerable, in the major thirds; and $\frac{1}{6}$ or $\frac{7}{42}$, and $\frac{1}{7}$ or $\frac{6}{42}$ have a difference of $\frac{1}{42}$c, or $\cdot26209\Sigma$ in the major sixths.

This system differs more from that of M. Henfling;

* Mentioned by Dr. Robison, Sup. Enc. Brit. 3d edit. ii. 662.

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(see Sauveur's table above referred to) than the last, since $-\frac{2}{7}\dot{c} = -3\cdot1451\Sigma$, and in his system $-3\cdot04811\Sigma$ is the temperament of the fifth.

Scholium 12. If a douzeave be required, wherein *the mean Tone thereof is to its major Limma as* 5 *to* 3, we have from corollaries 11 and 12, as $5 : 3 :: \frac{104\cdot2992s-2r}{s} : \frac{46\cdot1496s+5r}{s}$, whence $230\cdot7480s+25r = 312\cdot8976s - 6r$, or $82\cdot1496s = 31r$, and $\frac{r}{s} = \frac{82\cdot1496}{31}$; also as $5 : 3 :: \frac{9\cdot0272u-2t}{u} : \frac{4\cdot0136u+5t}{u}$, whence $20\cdot0680u + 25t = 27\cdot0816u - 6t$, or $7\cdot0136u = -31t$ and $\frac{t}{u} = \frac{-7\cdot0136}{31}$; and $-\frac{82\cdot1496}{31}\Sigma - \frac{7\cdot0136}{31}\text{m}$ is the flat temperament of the fifth.

This is the system of *M. Huygens.* (See Dr. Smith's Harmonics, pages 158, 168, 121, 208, 224, &c.) whose temperament of the fifth, as calculated by Dr. Smith, is $-\frac{1}{4}\text{c} + \frac{1}{110}\text{c}$ or $-\frac{53}{220}\text{c}$, $= 2\cdot6519\Sigma$; mine above being about $2\cdot6518\Sigma$.

The octave here is supposed to be divided into 31 equal parts. See M. Sauveur's table; Mr. Ambrose Warren, in 1725, pretended to the discovery of this system. See Monthly Magazine, vol. xxix. page 413.

Scholium 13. If a douzeave be required, wherein *the mean Tone thereof is to its major Limma as* 9 to 5, we have from corollaries 11 and 12, as $9 : 5 :: \frac{104\cdot2992s-2r}{s} : \frac{46\cdot1496s+5r}{s}$, whence $415\cdot3461s + 45r = 521\cdot4960s - 10r$, or $55r = 106\cdot1496s$, and $\frac{r}{s} = \frac{106\cdot1496}{55}$; also as $9 : 5 :: \frac{9\cdot0272u-2t}{u} : \frac{4\cdot0136u+5t}{u}$, whence $36\cdot1224u+45t = 45\cdot1360u - 10t$, or $9\cdot0136u = 55t$ and $\frac{t}{u} = \frac{9\cdot0136}{55}$; and $-\frac{106\cdot1496}{55}\Sigma - \frac{9\cdot0136}{55}\text{m}$ is the flat temperament of the fifth, $= -1\cdot9313\Sigma$.

This

This system answers to a division of the octave into 55 equal parts, and according to the papers of M. Sauveur in the Memoirs of the Parisian Academy, for 1707 and 1711, it was the system used by the Musicians of Paris at or previous to that time. See his general table of tempered systems above referred to.

I am, sir,

Your obedient humble servant,

Westminster, July 11, 1810. J. FAREY.

VII. *Report on the Memoirs presented to the Society of Pharmacy at Paris, in consequence of the Prizes offered in the Year* 1809. *Extracted by M.* BOUILLON LAGRANGE *from the full Report drawn up by Messrs.* NACHET, DEROSNE, *and* VALLEE.

OF nine memoirs sent to the Society, two have particularly fixed the attention of the committee. They were written in answer to the following question:

"To prepare the acetate of potash, so as to obtain it white and saturated, without employing radical vinegar, and without having recourse to fusion;—to point out which of the two, the acid or the alkali, gives it the colouring principle."

The first memoir, with the motto *Ex cognitis incognita,* is written with great precision.

The author, after having ascertained the advantage which would result from obtaining this salt in all its purity by a simple and œconomical process, begins by inquiring from whence the colouring principle arises: It cannot, he says, be owing to the alkali, when it is considered that the fusion of the acetate of potash renders it insoluble, and that the heat requisite for this fusion is not so strong as that which is necessary for the preparation of any given potash; and on the other hand, it cannot be essential to the acetic acid, when radical vinegar is capable of instantly furnishing a colourless salt. Consequently, this colouring principle must be a foreign substance contained in common vinegar, and which may be introduced into it in distillation. But this same principle is less volatile than the acetic acid, since distilled vinegar leaves a residue of it if we evaporate it a second time: it is not very soluble by itself, and it cannot be dissolved except by the addition of acetic acid, since it is precipitated, at least in part, when we saturate the latter by potash: and finally, it has been ascertained that

 it

it is of a vegeto-animal nature, either from the smell which it exhales when placed on hot coals, or by the prussiate of ammonia which upon distillation furnishes the acetate of potash prepared with distilled vinegar: a product which does not give the same salt prepared with radical vinegar: whence the author concludes that the radical principle which colours the acetate of potash is nothing but a part of the ferment of common vinegar, carried into the distillation and more or less altered by this operation.

Independently of this colouring principle, inherent in the constitution of common vinegar, the author of the memoir mentions another still more capable of making the acetate of potash look brown: this is the empyreumatic oil with which the vinegar is charged when the distillation is carried too far. He further says, that this salt may also be coloured by the oxides of iron and of manganese contained in the alkali, or by the metallic utensils used in its preparation: but this colour being merely accidental, we may avoid it entirely by using a pure potash and vessels of tin or porcelain. We must therefore adhere to the ferment and the empyreumatic oil. The following directions are given for avoiding these two colouring principles: the ferment may be separated from the acetate of potash the more easily the less of it there is in the distilled vinegar, and the latter will contain so much the less in its turn; as in common vinegar, the proportion of the ferment will be smaller with respect to that of the acid, on account of the quantity of ferment brought over in distillation being always more or less in proportion with that which exists in common vinegar. It follows therefore that it is necessary, above all, to employ common vinegar, which is at once the most acid and the least charged with ferment; and this requisite may be attained by choosing a clear vinegar, besides being very strong and completely fermented. After the choice of the vinegar, the process of distillation may also have some influence on the quantity of the ferment contained in distilled vinegar: for, since this principle is less volatile than the acetic acid, the less of it will pass over in distillation, the more slowly this process is conducted; and in this respect we may admit a slight ebullition as being the fittest degree of heat.

If the preceding rules have been well attended to, the distilled vinegar will contain so small a quantity of ferment that it will be capable of furnishing immediately an acetate of potash almost entirely colourless; but if, notwithstanding every precaution, the whiteness of the salt still

still leaves something to be desired, there remains a final method of remedying it, which consists in filtering through charcoal in powder. The action of this substance, although little known as to its theory, is nevertheless certain in its effects; since it is sufficient to boil slightly with it the solution of the acetate as above prepared, in order to obtain it perfectly white after filtration and evaporation carefully managed. As to the empyreumatic oil, there is only one way of avoiding it, which is to stop the distillation of the vinegar at the moment when this principle begins to come over, and the product gives out an empyreumatic smell: for, beyond this term, the vinegar, if still white in appearance, would not undergo any change of colour during the evaporation of the acetate; and this colour, when once produced, could not be removed, either by charcoal powder or by any other means whatever.

The second memoir presents fuller details. Its motto is taken from Boileau:

> "L'artifice agréable
> Du plus affreux objet fait un objet aimable."

The author describes in the first instance the various processes hitherto adopted in preparing the acetate of potash. He mentions as the most exact the process of M. Bouillon Lagrange, which consists in crystallizing this salt; but he regrets not having been able to put it in practice, from the difficulty of separating the crystallized acetate from the mother waters, which are very thick. In order to obtain as advantageous a result by a more practicable process, he tried double decompositions; he treated acetate of lime with the carbonate or sulphate of potash, but he did not obtain an acetate of potash less coloured than if he had directly saturated the carbonate of potash with distilled vinegar.

It would be necessary, as he observes, to employ a crystallized acetate of lime, but in this case the process would become too tedious and expensive. The decomposition of the common acetate of lead by the carbonate of soda, furnished him with a tolerably white acetate of potash: although this method unites with the facility of using it the advantage of being cheaper, the author of the memoir does not think it right to resort to it, because the smallest negligence in the operation may change a wholesome medicine into a deadly poison. Recurring to the combination described of distilled vinegar and potash, he first inquires whence arises the colour assumed by this salt during its evaporation: he is also aware that it is owing to a foreign principle contained in the distilled vinegar; but he afterwards saw that this substance was very slightly of a colouring nature by itself: he observed that the acetate of potash well saturated, is found as a consequence of its evaporation with an excess of alkali; and it is this excess of alkali which reacts on the foreign principle contained in the distilled vinegar, and colours it. In order to show more clearly this reaction of the potash, he divided into two equal portions a solution of acetate of potash: he evaporated both at the same degree of heat, maintaining constantly in the one an excess of acid, and in the other an excess of alkali: the salt produced by the liquor with excess of acid was much less coloured than that furnished by the liquor with an excess of alkali *. After having ascertained the origin of the colouring principle and the cause which develops it, the author next endeavoured to destroy it; and charcoal in his opinion is the fittest agent: with this view he filters the distilled vinegar through charcoal, he then saturates it with carbonate of potash, leaving in it an excess of acid, which he takes care to keep constantly in the liquor during its evaporation. The result is an acetate equally white with that obtained by means of fusion.

This process, although very simple, did not appear to him to be practicable, because the acetate of potash is mixed with a certain quantity of acetate of lime, to which the lime contained in the charcoal has given rise; and this salt, by altering the purity of the acetate of potash, retards its desiccation. It would, indeed, be very easy to separate it by adding a slight excess of carbonate of potash, in order to precipitate the lime; and we should afterwards put in an excess of acid: but it is easier to saturate the acid first. The following is the process as described in the memoir:

Pour into distilled vinegar a solution of carbonate of potash, until no more carbonic acid is extricated: afterwards evaporate the liquor, taking care always to keep an excess of acid in it: when it is reduced to three-fourths, allow it to cool, in order to separate from it the sulphate of potash and some impurities; decant it in order to heat it, and pour it when hot on a charcoal filter †.

* We have reason to believe, from our own experiments, that the potash still reacts, but much less on the colouring principle, even when the liquor contains an excess of acid; since by operating in this manner we always obtain an acetate of potash which is more or less coloured, whilst the same vinegar is capable of furnishing acetate of lime, magnesia, and alumine, which are very white. Soda did not appear to us to act so strongly as potash on this principle.—*Note by the Committee.*

† The acetate of lime when dried is less deliquescent than the acetate of potash, and yet it is much more difficult to produce the desiccation of it.—*Note by the Author.*

If

If the filtered liquor does not contain more free acid, add a little distilled vinegar *; then evaporate to dryness; and if we wish to obtain the acetate of potash well cleaned, we must, at the end of the evaporation, manage the fire properly, and not stir it; but in this case it is not so white as when we separate it with a silver spatula, and throw it on the edges of the basin as fast as it is formed at the surface of the liquid: this salt will also be whiter if dried by small portions.

On exposing for about 20 days to the solar rays the liquor filtered over charcoal, the author obtained a salt much whiter: hence he thinks that the same result might be obtained, by exposing to the light an acetate of potash made from distilled vinegar, without being filtered through charcoal.

He regrets that he has been unable to collect some important facts relative to the colouring matter: he remarked that it was partly precipitated after saturation; that it is a little soluble in water, and that a portion remains in solution in the liquid acetate of potash; that after having filtered distilled vinegar through very pure charcoal, like that which is produced from crystallized sugar, we no longer obtain, on saturating it with crystallized carbonate of potash, the same precipitate as before filtration.

The author of the memoir concludes, therefore,—

1. That the colouring matter of the acetate of potash belongs to a vegetable substance contained in distilled vinegar.
2. That this colouring matter is destroyed by charcoal.
3. That an excess of alkali, when we evaporate the result of the saturation of distilled vinegar by potash, may influence the whiteness of the acetate of potash.
4. That in order to obtain the earth pure white and saturated, it is sufficient to filter a concentrated solution of it over a small quantity of charcoal in powder; to keep in it afterwards to the end of the evaporation an excess of acid, by adding from time to time distilled vinegar, and to expose it for some days to the solar light.

In a note which terminates this memoir, the author says, that according to the valuable observation of Messrs. Vauquelin, Pontier and Derosne, he obtained two hectogrammes of excellent acetic ether, by rectifying over potash the first products of the distillation of 70 litres of distilled vinegar.

* A little acetic acid (radical vinegar) would be preferable; very little is necessary when care has been taken to filter the liquor in the neutral state: we must also take care that it is not acid, in consequence of the lime which is in the charcoal.

The

The committee carefully repeated the experiments detailed in the above memoirs. If we except the whitening effects on exposing the acetate to the sun, which did not succeed with them, they pronounced them to be all correct; indeed, the principal agent in the whitening process was already known. Lowitz has recommended the use of charcoal, in order to obtain an acetate of potash less coloured than by the ordinary process; but whether he has not sufficiently described the method of using it, or employed vinegar of a bad quality, ill distilled or ill saturated, over which the depurating qualities of the charcoal had no influence, several chemists have been unsuccessful.

From these considerations, and particularly from the satisfactory results obtained by the committee, they think themselves warranted in concluding, that the authors of the two memoirs would have done better by making known the principle and the causes of the colouring of this salt, at the same time that they indicated the means of preventing and removing them.

By following carefully the rules which they prescribe, and by taking all the precautions which they point out, we shall easily obtain, without having recourse to fusion, an acetate of potash very white and perfectly saturated. The society has therefore decreed a gold medal of the value of 100 francs to each of the authors of the memoirs.

The author of the memoir first mentioned is M. Bernoully of Bâle; and of the second, M. Fremy of Versailles.

VIII. *Of the Influence of Solar and Lunar Attraction on Clouds and Vapours.* *By* Salem Harris, *Esq.*

To Mr. Tilloch.

Sir, In perusing the theory of the tides as originally laid down by Kepler, and subsequently improved upon by Sir Isaac Newton, I was forcibly struck with an idea, that if the attraction of the sun and moon (more particularly the latter) is capable (as the ebb and flow of the sea appears to have proved beyond dispute) of acting with sufficient power upon that immense and ponderous mass the ocean, to raise its waters on those parts which the revolutions of the heavenly bodies alternately place in the focus of their attraction; its effects upon the clouds, the lighter and exhaling particles, or comparatively speaking the *steam* of those waters, must be still greater; and sufficient to produce, in conjunction with or opposition to the wind, those frequent and apparently uncertain changes which we hourly

hourly experience in the atmosphere. Impressed with this idea, and not without some degree of wonder to find it, as far as I could learn, unnoticed by the philosophical world, I began when at school to form a journal of the weather; noting at every observation the quarter of the wind, as well as the moon's altitude and azimuth; and had the satisfaction of finding my infantile speculation so well grounded, that I observed the weather almost invariably thick or rainy, when the wind and moon, being at or near the same quarter, were acting in conjunction; the latter drawing the clouds, as I imagine, to her nearest point of the horizon, from whence the former drives them over its surface; and that it became proportionally clearer as their relative change of situation enabled the wind to counterpoise the moon's attraction, and prevent those vapours from collecting.

In the year 1809, a voyage across the Atlantic, and a residence of some months at Havannah, enabled me to extend my observations to the northern extremity of the trade winds, as well as the climate of the torrid zone, both on sea and land. I shall therefore extract the journal of a few days in each of the situations wherein I have noticed the weather; with a slight comment on the nature of the country and the prevailing winds, or periodical change of seasons, leaving your philosophical readers to compare my statement with the idea that gave it birth.

Journal of the Weather at Wandsworth, near London.

Day of the Month.	*Time of Day.*	*Wind.*	*Moon's Azimuth.*	*Moon's Altitude or Depression.*	*Observations.*
Oct. 27, 1800.	Morn	S.S.W.	N.	50° Depr.	Very fine weather.
	Even	S.	S.E. by S.	28° Alt.	Cloudy and rainy.
28	Morn	S.S.W.	N.N.W.	14° Depr.	Fine weather.
	Even	S.S.E.	S.E. by S.	23° Alt.	Cloudy and rainy.
29	Morn	Variab. fr. S.E. to S.W.	N.W. by N.	35° Depr.	Fine weather.
	Even	S.W. by W.	S.E.	24° Alt.	Fair weather, but cloudy.
30	Morn	S.E. by E.	N.W.	26° Depr.	Very fair, but rather cloudy
	Even	Ditto.	S.E. by E.	23° Alt.	Very cloudy, but no rain.
31	Morn	S. by W.	N.W.	15° Depr.	Raining a little, but appears to be clearing off.
	Even	S.W.	E.S.E.	24° Alt.	Very fine weather; no clds.
Nov. 1	Morn	S. by E.	N.W. by W.	4° Depr.	Fine weather; a few light [clouds.
	Even	S.W.	E.	21° Alt.	Ditto ditto.
2	Morn	S.W.	W.N.W.	5° Alt.	Fine weather, a few clouds in the N.E. horizon.
	Even	S.E. by E. strong.	E. by N.	17° Alt.	Cloudy, with a little rain.

Journal of the Weather at Sea, between Madeira and the Cape Verd Islands.

Day of the Month.	*Time of Day.*	*Wind.*	*Moon's Azimuth.*	*Moon's Altitude or Depression.*	*Observations.*
March 16, 1809.	Morn	E.S.E. nearly calm.	E. by S.	25° Alt.	Very cloudy: raining to windward.
	Even	N.W. very light.	W. by N.	8° Depr.	Cloudy, but no rain.
17	Morn	N. rath. strong.	E. by S.	14° Alt.	Very fine and serene.
	Even	N. moderate.	W. by S.	19° Alt.	Ditto ditto.
18	Morn	N.W. moderate.	E. by N.	8° Alt.	Very fine and serene: a few light clouds.
	Even	N. very light.	W. by N.	6° Alt.	Fair; but rather cloudy.
19	Morn	N.N.W. nearly calm.	E. by N.	0°	Very fine and serene.
	Even	N. nearly calm.	W.	24° Alt.	Ditto ditto.
20	Morn	N. nearly calm.	E.N.E.	8° Depr.	Very fine: a few light clouds to windward.
	Even	S.W. nearly calm.	W. by N.	37° Alt.	Ditto ditto.
21	Morn	N.W. strong.	N.E by E.	15° Depr.	Cloudy and rainy.
	Even	N.W. fresh.	W. by S.	53° Alt.	Fair; but cloudy in many parts of the horizon, particularly to windward, and slight flying showers occasionally*.
22	Morn	N.W. by W. strong.	N.E. by E.	20° Depr.	Very fine: a few clouds round the horizon.
	Even	N.N.W. rath. strong.	W. by S.	58° Alt.	Very fine: a few light clouds.
23	Morn	N.N.E. moderate.	N.E.	34° Depr.	Very cloudy; with some rain.
	Even	N.E. moderate.	W.S.W.	31° Alt.	Very fine and serene: a few light clouds.
24	Morn	N. by W. moderate.	N.N.E.	42° Depr.	Very fair; but many light clouds, particularly to windward and as far as the east.
	Even	N.N.E. moderate.	W. by S.	78° Alt.	Very fine: light clouds in most parts of the horizon.
25	Morn	N.N.E. moderate.	N.E. by N.	48° Depr.	Very cloudy; and a little rain: appears now to be clearing off.
	Even	N.E. moderate.	In the	Zenith.	Very fine: a few light clouds in several parts of the horizon.

* Between the above two observations (about half past three P.M.) there was rather a heavy squall of rain with a strong breeze from the N.W.; the moon nearly in the zenith, and most parts of the horizon cloudy.

On the 16th, I was in sight of Madeira, and crossed the tropic of Cancer on the 24th, in longitude 24° 30′ west of Greenwich; the ship's course being S.S.W. The former part is consequently on the verge of the trades; as the latter is of the torrid zone. At this season of the year the winds are variable, but generally strong.

Journal of the Weather at Havannah.

Day of the Month.	*Time of Day.*	*Wind.*	*Moon's Azimuth.*	*Moon's Altitude or Depression.*	*Observations.*
Sept. 10, 1809.	Morn	N. by W.	E. by S.	21° Alt.	Very fine: a few light clouds to leeward.
	Even	N.N.E.	W.	10° Depr.	Fine: a few clouds to windwd.
11	Morn	S.E.	E.	7° Alt.	Fair; but very cloudy.
	Even	E.N.E.	W. by S.	0°	Very fine and serene.
12	Morn	S.W. very light.	E. by S.	5° Depr.	Very fine: a few clouds rising to windward.
	Even	E.S.E.	W. by S.	8° Alt.	Very fair: some clouds to do.
13	Morn	S.E. nearly calm.	E.	21° Depr.	Very fine: some clouds rising in the eastern horizon.
	Even	N.N.E.	W.S.W.	10° Alt.	Very fine: a few clouds near the moon. [At noon this day the thermometer stood at 90°.]
14	Morn	E.S.E. nearly calm.	E.	32° Depr.	Fine: some clouds rising to windward.
	Even	N.	S.W. by W.	31° Alt.	Very fine and serene.
15	Morn	S. by E. nearly calm.	E. by N.	42° Depr.	Very fine: light clouds rising to windward.
	Even	N.	S.W.	35° Alt.	Very fine and serene.
16	Morn	S.S.E.	E. by N.	64° Depr.	Very fine: some clouds to leeward.
	Even	N. strong.	S.S.W.	42° Alt.	Very fine: some clouds to windward.

Havannah is situated but a few miles south of the tropic of Cancer, and the land about it moderately high. It is sheltered by the hills of the Cavannah from all winds between N.E. and E.S.E., and these are by far the most prevalent. In the evening, however, it often shifts to the north, or even a point or two to the westward. At this season of the year the weather is intensely hot, with frequent and violent storms of rain and thunder, which usually take place between the hours of two and six in the afternoon.

Journal

Journal of the Weather between Bermuda and the Western Isles.

Day of the Month.	*Time of Day.*	*Wind.*	*Moon's Azimuth.*	*Moon's Altitude or Depression*	*Observations.*	*Height of Thermometer.*
Nov. 25, 1809.	Morn	E.S.E. very strong.	W. by N.	17° Alt.	Fine: but rather cloudy in many parts of the horizon.	67°
	Even	E.S.E. light.	E.N.E.	8° Dep.	Cloudy, with a lit. rain.	68°
26	Morn	E. by S. strong.	W.	25° Alt.	Very fine and serene.	67°
	Even	N.E. strong.	N.E. by E.	18° Dep.	Cloudy, with small rain.	69°
27	Morn	N.E. blows hard.	W. by S.	34° Alt.	Hazy weather.	66°
	Even	N.N.E. a heavy gale.	N.E.	27° Dep.	Very thick and cloudy, with small rain.	66° 30
28	Morn	E.N.E. strong.	W.S.W.	41° Alt.	Very fine: a few clouds in the horizon.	67°
	Even	E.N.E. strong.	N.E.	37° Dep.	Cloudy, with occasional showers.	69°
29	Morn	N.E. by E. very strong.	S.W. by W.	47° Alt.	Thick hazy weather, with some small rain at intervals; appears now to be clearing off.	67°
	Even	E. light.	N.N.E.	46° Dep.	Thick and cloudy, with small rain. [Between the above observations, hazy weather with occasional showers; the wind light and variable.]	68°
30	Morn	E. moderate.	S.W.	45° Alt.	Fine: a few clouds, which are clearing off.	67° 30′
	Even	N.E. by E. moderate.	N.	51° Dep.	Very fine: a few clouds to windward. [Much rain between 11 P.M. of the 29th and 1 A.M. of this day: the wind and moon being nearly in the same point (E. by N.) and the latter on the horizon.	68°
Dec. 1	Morn	N.N.E. moderate.	S.S.W.	53° Alt.	Very fair, but rather cloudy, particularly near the moon.	67°
	Even	N. by E. moderate.	N.	55° Dep.	Fair; but cloudy in most parts of the horizon, particularly from N to W.	67° 30′

In this part of the globe, more particularly during the autumnal and winter months, the wind is usually strong; often

often increasing to a gale; the heaviest usually blows from the north and north-west. These observations were made between the 35th and 37th degree of north latitude, and 44° and 39° longitude west of Greenwich: the ship's course being E. by N.

There are certainly many places, in which a particular wind almost invariably produces rain; from the intervention of a chain of hills, or even a single mountain that impedes the regular course of the clouds, when moving in a certain direction; and breaks them over the valleys below. Or, the wind may be either sufficiently strong to overpower the moon's attraction, or so light as to afford no assistance in spreading the clouds which have been collected on or below the horizon, and thus produce an effect upon the weather contrary to what might have been expected from the relative situation of the impelling powers; a circumstance which, though very material, did not strike me when I began my observations; and the velocity of the wind is consequently unnoticed in the first part of my journal. Our atmosphere may contain at times so little vapour, as to be incapable of producing rain, although the moon and wind were acting ever so much in unison; but this can always be ascertained by the state of the barometer. When also the moon's altitude or depression is so great as to place her nearly in the *zenith*, or the *nadir*, her attraction can of course avail but little, either in assisting or counteracting the effect of the wind from whatever point it may happen to blow: its power, in short, must diminish in proportion as her distance from the horizon increases.

I do not pretend to improve, much less to controvert, the theories of those many learned and scientific characters who have written upon the nature and variation of the atmosphere; for my knowledge in every branch of philosophy is very slight; but I cannot help thinking, that a little attention to the subject which I have noticed, would frequently assist an observer of the weather, in foreseeing with additional certainty an approaching change; and I offer these remarks to the public, with no other view than the possibility of their being investigated, by those who possess the knowledge and leisure requisite in philosophical studies, to the advancement of science, as well as the benefit of those professions, in which a dependence is placed upon the atmosphere. I remain, sir,

Your respectful humble servant,

Richmond Green, July 10, 1810. Salem Harris.

IX. *On*

IX. *On Crystallography. By* M. Hauy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from vol. xxxv. p. 460.]

Table of the Crystalline Forms.

I. *Substances which have a common primitive form, with the same dimensions.*

1. CUBE.

Names of the Substances.	Form of the integrant Molecule.
Borated magnesia	Cube
Muriated soda	Ditto
Amphigene	Irregular tetrahedron
Analcime	Cube
Sulphurated lead	Ditto
Sulphurated iron	Ditto
Oxidated tin	Ditto
Gray cobalt	Ditto
Calcareous scheelin	Regular tetrahedron.

2. REGULAR OCTAHEDRON.

Fluated lime	Regular tetrahedron
Muriated ammonia	Ditto
Sulphated alumine	Ditto
Spinelle	Ditto
Pleonaste	Ditto
Diamond	Ditto
Red oxidated copper	Ditto
Oxidulated iron	Ditto
Native bismuth	Ditto
Native antimony	Ditto

3. REGULAR TETRAHEDRON.

Pyritous copper	Regular tetrahedron
Gray copper	Ditto.

4. RHOMBOIDAL DODECAHEDRON.

Garnet	*Tetrahedron with isosceles triangles equal and similar*
Sulphurated zinc	Ditto.

II. *Substances the primitive forms of which only are of the same kind, with dimensions respectively peculiar to each.*

1. RHOMBOID.

* *With obtuse summits.*

Carbonated lime	Rhomboid

Tourmaline

Names of the Substances.	Form of the integrant Molecule.
Tourmaline	Irregular tetrahedron
Chabasie	Rhomboidal
Dioptase	Ditto
Sulphurated antimoniated silver	Rhomboidal.

*** With acute summits.*

Corundum	Rhomboidal
Oligistous iron	Ditto
Sulphurated iron	Ditto.

2. OCTAHEDRON.

** Pyramids with square bases.*

Alkaline fluated alumine ..	Irregular tetrahedron
Zircon	Ditto
Harmotome	Ditto
Anatase	Ditto
Molybdated lead	Ditto
Mellite	Ditto.

*** Pyramids with rectangular bases.*

Nitrated potash	Irregular tetrahedron
Carbonated lead.........	Ditto
Sulphated lead	Ditto
Oxidated zinc...........	Ditto.

**** Pyramids with rhombic bases.*

Sulphur................	Irregular tetrahedron
Red sulphurated arsenic ...	Irregular tetrahedron.
Blue carbonated copper ...	Ditto.

3. TETRAHEDRAL PRISM.

1. STRAIGHT PRISM.

** With square bases.*

Sulphated magnesia......	Isosceles-rectangle-triangular prism
Idocrase	Ditto
Meionite	Prism with square bases
Wernerite	Ditto
Mesotype	Isosceles-rectangle-triangular prism
Chromated lead	Ditto

 Oxidated

Names of the Substances.	Form of the integrant Molecule.
Oxidated uranium	Prism with square bases
Oxidated titanium	Isosceles-rectangle-triangular prism.

** *With rectangular bases.*

Cymophane	Prism with rectangular bases.
Euclase.................	Ditto
Peridot	Ditto
Prehnite	Ditto
Stilbite	Ditto
Ferruginated scheelin	Ditto

*** *With rhombic bases.*

Sulphated barytes	Scalene - rectangle - triangular prism
Sulphated strontian	Ditto
Topaz	Prism with rhombic bases
Staurotide	Isosceles-rectangle-triangular prism
Macle	Uncertain
Mica	Prism with rhombic bases.
Talc	Ditto
Arsenical iron	Ditto
Sulphurated molybdenum .	Ditto
Siliceo-calcareous titanium	Ditto.

**** *With oblique-angled parallelogram bases.*

Sulphated lime...........	Prism with oblique-angled parallelogram bases
Epidote	Prism with oblique-angled parallelogram bases
Axinite	Ditto.

2. OBLIQUE PRISM.

* *With rectangled bases.*

Borated soda	Prism with rectangled bases.

** *With rhombic bases.*

Amphibole	Prism with rhombic bases
Actinote	Ditto
Pyroxene	Oblique triangular prism
Grammatite	Prism with rhombic bases.

*** *With oblique-angled parallelogram bases.*

Feldspar	Prism with oblique angled parallelogram bases

Disthene

Names of the Substances.	Form of the integrant Molecule.
Disthene	Prism with oblique angled parallelogram bases
Sulphated copper	Ditto.

4. REGULAR HEXAHEDRAL PRISM.

Phosphated lime	Equilateral triangular prism
Telesie	Ditto
Emerald	Ditto
Nepheline	Ditto
Pycnite	Ditto
Dipyre	Ditto
Sulphurated mercury	Ditto.

5. PYRAMIDAL DODECAHEDRON.

Quartz	Irregular tetrahedron
Phosphated lime	Ditto.

III. *Forms which are found to be secondary in different species.*

1. CUBE.

Names of the Substances.	Primitive Forms.
Fluated lime	Regular octahedron
Native bismuth	Ditto.

2. REGULAR OCTAHEDRON.

Muriated soda	Cube
Sulphurated lead	Ditto
Sulphurated iron	Ditto
Gray cobalt	Ditto.

3. REGULAR HEXAHEDRAL PRISM.

Carbonated lime	Obtuse rhomboid
Corundum	Acute rhomboid
Mica	Straight prism with rhombic bases
Sulphurated antimoniated silver	Obtuse rhomboid
Phosphated lead	Pyramidal dodecahedron
Sulphurated molybdenum	Straight prism with rhombic bases.

4. RHOMBOIDAL DODECAHEDRON.

Fluated lime	Regular octahedron
Oxidulated iron	Ditto.

5. SOLID WITH 24 EQUAL AND SIMILAR TRAPEZOIDS.

Names of the Substances.	Primitive Forms.
Muriated ammonia	Regular octahedron
Garnet	Rhomboidal dodecahedron
Amphigene	Cube
Analcime	Ditto
Sulphurated iron	Ditto.

Explanation of the plan which has been adopted in the descriptions of the different species of minerals.

After having given the synonymy of the best known authors, we have successively presented the essential character of the substance, and the physical, geometrical, and chemical characters, the assemblage of which forms the specific character.

We have excluded from this character every thing connected with fugitive accidents, such as colours, when they are owing to a principle which is only interposed in the substance.

In the detail of the geometrical characters, care has been taken to indicate not only the direction of the natural joints, but also the greater or less facility of obtaining them, their difference of neatness in one and the same crystal, and those, in short, whose positions are only presumed. In addition to this, we have made known, in a note, the respective dimensions of the molecule, and all that may serve as data for applying the theoretical calculus to the laws of decrements upon which the secondary forms depend.

After the indication of the chemical characters, we have given the result of the analyses of the substance which seem to have merited most confidence.

The table of varieties, which follows the characters, is generally divided into two sections, one of which contains the descriptions of the forms, and the other refers to the accidents of lights. The forms are either determinable, *i. e.* susceptible of being described geometrically, from the number, the disposition and the mutual incidences of their faces, or indeterminable, *i. e.* produced by a confused or precipitated crystallization, so that geometry cannot describe them, and we can at most indicate the vague relations which exist between them and known objects; as when we say of a mineral that it is cylindrical, globular, granular, &c. and the last term of this kind of degradation of forms is expressed by the word *amorphous*, which designates a substance in masses of a certain volume completely irregular.

The

The description of every determinable variety presents successively the name which it bears, conformably to the principles of the method of nomenclature which has been above explained, the indication of its representative sign, that of its figure, its synonymy, according to Romé de l'Isle or other crystallographers; and lastly, the measurements of its principal angles. When the structure of the variety is complex, we add to its description explanations proper for better understanding the results of the laws upon which it depends.

The indications relative to colour and to transparency compose the second section, under the title of *Effects of light* *. It is proper to remark on this subject, that any given form may offer successively all the varieties of colour and transparency, and that, in return, every colour and every degree of transparency may be met with in every kind of form. But it is unnecessary to overload the method with all these combinations. It is sufficient, if it presents a method of indicating that which exists in any given variety, to describe this variety completely. Thus the table of the characters of telesie contains implicitly all the following combinations: *primitive limpid telesie; unitary red transparent telesie; amorphous translucid telesie.*

When the name which we have adopted for one species of mineral has been applied to different species, from a delusive resemblance, such as colour, we indicate these double applications in a particular table placed at the end of that of the varieties; and I hope I shall be applauded for the tedious task which I have entered upon, in order to clear up the confusion which arose from these communications of one and the same name to substances so ill adapted to be associated with each other.

Each article is terminated by annotations relative to the situation of the substances in the ground, to the researches which have made us acquainted with them, to its physical properties, its uses in the arts, medicine, &c. I have even thought it right to present most of these objects more in detail than has been generally done, so as to avoid the dryness of too concise indications, without however giving myself up to a multiplicity of details which would appear to be misplaced in a treatise upon mineralogy.

[To be continued.]

* We have placed the word *limpid* at the head of effects of colours, because it seemed natural to commence here by the privation of character, since it indicates that the substance is in the greatest possible state of purity.

 X. *Pro-*

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

JUNE 28, The President in the chair. The conclusion of M. De l'Isle's paper on the poison of the *bohan upas* and *antea* was read. The emetic power of this poison suggested to the author the propriety of making some experiments with other emetics, by injecting them into wounds and blood-vessels in the same manner as he did the upas. Ipecacuanha and tartar emetic were injected, and both produced very violent effects, particularly the latter; but they were not so destructive to animal life as the upas. On dissecting the bodies of the animals killed by injecting this poison, and comparing them with the effects of common emetics, he was led to conclude that the upas does not kill by any specific action on the nerves, but that, by acting on the blood only, it is so instantaneously destructive to animal life.

A paper from Mr. Good was read, describing the nature of the horny concretions which appeared all over the skin of a heifer exhibited in London last year. The head, neck, and shoulders of this animal were thickly covered with little horns of various length and thickness, some of them nearly three inches long. It appears that these horns were chiefly composed of calcareous matter, and that one-fourth of them was of an animal nature.

July 5, Dr. Wollaston read a paper on a peculiar species of urinary calculus, which he called cystic oxide, only two specimens of which he has been able to procure. The cystic oxide dissolves in solutions of all the alkalies, but not in saturated carbonate of ammonia. Dr. W. also took occasion to correct some essential errors in his paper on calculi, which appeared in the Philosophical Transactions for 1797; subsequent experience having convinced him that phosphate of lime and phosphate of magnesia rarely or never exist together in the same calculus.

A paper on muriatic acid, by Mr. Davy, was read. The object of Mr. Davy's paper was to detail some new facts respecting the muriatic acid. Finding that charcoal, though ignited to whiteness, will not burn or decompose oxymuriatic acid gas, he was led to institute experiments to determine whether oxygen could be procured from it by any means: and the results of his inquiries are, that there is no proof whatever of its containing that substance. Muriatic acid gas may be decomposed into oxymuriatic

acid

acid and hydrogen; and recomposed from these bodies. In all cases in which oxygen gas is procured from oxymuriatic acid gas, water is present: and the oxygen is furnished by the water; and hydrogen is always combined with the oxymuriatic acid gas; so that, as inflammable bodies decompose water by attracting oxygen, so oxymuriatic acid decomposes it by attracting hydrogen. Mr. Davy has detailed some experiments which render it probable that the body called hyperoxymuriatic acid is in fact the simple basis of the muriatic compounds, and that it forms oxymuriatic acid by uniting to hydrogen, and common muriatic acid gas by uniting to more hydrogen.

In attempting to decompose oxymuriatic acid gas by the combustion of phosphorus and the action of ammonia, Mr. Davy discovered a very singular compound; which, though composed of oxymuriatic acid and ammonia with a little phosphorus, is neither fusible, volatile, nor decomposable at a white heat; neither soluble in acid nor alkaline menstrua; and possessed of no taste or smell.

Mr. Davy has detailed nine modes of decomposing common salt, founded upon these new facts, and has formed nine deductions from them respecting the composition of chemical agents in general.

A paper on pus, by Dr. Pearson, was read. Previously to the author's observations and experiments, a brief historical account was given of what has been already done on the subject. The conclusions among many others are: That the pus consists essentially of three differentsubstances, viz. An opake animal oxide, seemingly already self-coagulated; matter analogous to the coagulable lymph of the blood, but in a different state of aggregation. 2. Innumerable spherical particles, seen with the microscope, separable by chemical agents from the other parts. 3. A limpid coagulable liquid, in many properties similar to the serum of blood. The saline impregnations are the same as those of serum of blood and expectorated matter, especially muriate of soda, neutralized potash, and the phosphates of lime. Various other substances are frequently found in pus, which are considered to be accidental, and depend upon different diseases.

The Society then adjourned till Thursday the 8th of November.

IMPERIAL SOCIETY OF NATURAL HISTORY OF MOSCOW.

M. Fischer, president of this society, has published the following short account of their labours for the last four years.

years. This sketch is arranged under the following heads: I. Labours and Undertakings of the Society. II. Miscellanies. III. Promotions and Rewards. IV. Necrology. V. Literary Novelties. VI. Minutes of the Society, and Report of the Presents made to the Society and to the Museum of the Imperial University. The following are the contents of the first branch of their labours.

Journey to Siberia undertaken at the expense of the Society.—This expedition set out on the 9th of February 1809, and is to last three years. It is composed of Professor Tauber, who is known from his description of the valley of Plauen in Saxony; M. James Mohr, known from his travels in Germany, France, England, and Sweden; and M. Helm, botanist and chemist, known by his description of several new plants, and by several analyses: this is his second visit to Siberia. These gentlemen are accompanied by two pupils, Messrs. Kotoroff and Leslivsky, and they are provided with every necessary, such as books, charts, instruments, and a chemical laboratory. They were to be occupied the first year with the Ouxal chain of mountains; the second, with that of the Altai; the third, with the mountains of the Daourie; and, if circumstances will permit them, they will also visit Kamschatka. The profound erudition and zeal of the above gentlemen afford reason to hope for some important discoveries. They are also accompanied by a draftsman, and by a person who is acquainted with the art of stuffing and preserving animals.

Description of the Government of Moscow.—His Imperial Majesty having given five thousand roubles to be expended in examining the immense district which goes by this name, the professors of Moscow have recently visited several parts of the country with this view. The following is an account of what has been already done: Some astronomical and trigonometrical observations have been repeated at Moscow, and in some districts of the government, such as Svenigorod berea, Mojaisk, Riotisa, by professors Goldbach and Panthner, attached to the repository for charts at St. Petersburgh. The latter has also established, at the expense of the society, barometers and thermometers at the above places, in order to obtain some useful observations.

M. Fischer undertook the natural history department: he was accompanied in his excursion by M. Drouginine, secretary to the society; and by M. Gorke, one of the pupils at the university of Moscow. From the lateness of of the season they procured but few plants or insects, but they were more fortunate in their mineralogical pursuits. Petrifactions of all kinds, several mineral springs rich in iron and carbonic acid, a good clay for earthenware, Labrador stone, garnets in granite and in gneus, granatite in gneus, and a new earthy substance, were procured by them. This new substance is of a very fine lavender blue, and is found in veins several lines thick between layers of cimolite, which in some places forms the transition to a true mountain cork. Sometimes it is found on round masses of flint, sometimes fossil shells are found in it, and pectinites which are wholly black and changed into flint. This substance contains, according to the analyses of Messrs. Helm and Muller, lime, alumine, and phosphoric acid. It forms, therefore, a new species adjoining the Apatite, and it has been designated by the name of Ratofkite, from the place where M. Fischer resides.

Mr. Davy's experiments.—M. Jacquin in a letter to M. Fischer informs him, that in concert with his friends the director Schreibers, colonel Tihursky, and M. Bremser, he repeated the recent experiments of Mr. Davy with success. They generally made use of a battery with vertical piles composed of 1300 pairs of disks, which were generally three inches in diameter, and formed together 70 square feet of surface in contact:—the experiment succeeded however with 300 pairs of disks, and it was even perceptible with 70 pairs. One of the processes adopted by the above gentlemen seems to be somewhat novel: they placed in a wine glass a small piece of alkali moistened in the air, on a small plate of platina which communicates with the hydrogen pole, and which was entirely covered with rectified petroleum. Finally, they placed on the alkali a thin plate of platina, and pressed it with a metallic rod communicating with the oxygen pole. The effects being remarked, bubbles of air were extricated as in the first experiment; sometimes there were trifling detonations; and some time afterwards they found the whole of the inferior surface of the alkali strewed with small scales having a metallic appearance like those which are seen floating in the petroleum. This preparation is very beautiful, particularly when placed in the microscope. It is not combined easily with mercury; for a globule adhering to the point of the brass wire, when plunged in mercury, was not detached, and afterwards detonated in water as before.

In the experiment last described, the place of the platina may be supplied by a flat piece of charcoal. The diamond and

and sulphur are not conductors of the electric fluid, and produce no effect. The experiment does not succeed better in vacuo than in the open air. "What is this substance (M. Fischer asks) which resembles a metal? Is it the alkali reduced, or one of its constituent parts, which being combined with oxygen represents it, as Mr. Davy seems to think? or, Is it hydruret of potash? But whence this metallic appearance?"

Miscellanies.—Their majesties the Emperor Alexander I. and the King of Prussia have examined with great interest the skeleton of the mammoth brought from the shores of the Lena by M. Adams *.

M. Tilesius, associate of the academy, well known for his talent at painting objects in natural history, has prepared 40 folio drawings of the mammoth. His observations do not seem to coincide entirely with those of Cuvier.

The meteorological observations from Moscow prove that the cold was greatest in the night between the 11th and 12th of January. Dr. Rehman froze mercury in a saucer exposed to the air. Count Bontourline observed that the mercury in three of his thermometers was frozen, and sunk into the bowl. But in a thermometer which was not frozen, he found that from six in the morning to six in the evening, on the 12th of January, the cold was at 35° of Reaumur. M. Roger, of Troitsk, observed it at 34 degrees before the mercury was frozen.

The botanist Frederick Fischer, and M. Langsdorff associate of the academy, who accompanied Krusenstern in his voyage round the world, are occupied with a work on the Ferns. They have prepared drawings of several new species.

M. Fischer, the professor and director of the academy, is collecting materials for a comparative *craniognosy.* An accurate knowledge of the cranium, as one of the chief organs of animal organization, will fill up an important chasm in comparative anatomy. The craniology of Dr. Gall will only be made use of in order to demonstrate the influence of the brain on the form of the excavations of the skull. It will appear in Latin and French, accompanied with engravings.

M. Mohs has made a mineralogical excursion through Carinthia, Carniola, &c. He has been particularly occupied with the situation of the lead mines at Villach.

The Imperial Academy of Petersburgh proposed a prize

* See Phil. Mag. vol xxix. p. 141.

of

of 100 ducats for the best memoir on the following subject: "Give an easy method for ascertaining, independent of all knowledge of botany, poisonous plants in an indubitable manner." Three memoirs were consequently given in; but the prize has not been awarded to either.

A similar prize has been offered for the best "chronology of the Byzantine authors from the foundation of the city of Constantinople to its conquest by the Turks." The memoirs on the above subject must be transmitted to St. Petersburgh on or before the 1st of July 1811.

XI. *Notices respecting New Books.*

THE Medical Society of London have in the press a volume of memoirs, containing several valuable communications, in medical and surgical science, from eminent resident and corresponding members of the society. The title of the volume will be "Transactions of the Medical Society of London, Vol. I. Part I." and it will be accompanied by engravings. Part II will appear in a few months afterwards, the society having come to the determination of giving publicity to their transactions more frequently than heretofore.

XII. *Intelligence and Miscellaneous Articles.*

DE LUC'S ELECTRIC COLUMN.

To Mr. Tilloch.

July 23d, 1810.

SIR,—NOTWITHSTANDING the changes which have happened in the state of the atmosphere, the small bells, which are in communication with De Luc's electric column, have continued to ring without ceasing, as far as my observations have gone, from the 25th of March to this day. Although we have of late had heavy rain accompanied with thunder and lightning, we have not had any very damp weather, which I imagine is the most likely to stop the motion of the small clapper, by depositing moisture on the insulating parts of the apparatus.—If you, or any of your readers, are acquainted with a method of preparing varnish of a better insulating power than those varnishes mentioned in Cavallo's Treatise on Electricity, I shall be glad to have it communicated to me, and others who are interesting themselves in making experiments with this new column.

I wish

I wish here to correct a mistake which I made in the account of the electric column printed in your Magazine for March last. I there have miscalled the ends of the column: that which I have named the zinc end should have been named the silver end, and the contrary. So that the effects on the electrometer of the coated jar were, respecting the *plus* and *minus* states, just what might have been imagined they would be. The mistake arose owing to the silver and paper being connected together; for, had the two metals been united, and the paper separate, the instrument would then have resembled more the usual construction of a Galvanic trough; and I should not, I imagine, have been led into any error respecting the names of the ends or poles of it. I remain, &c.

B. M. Forster.

THE OPULENT BLIND.

The plan to which we alluded in our last has been since published in a prospectus. For the purpose of this humane institution a convenient house has been taken at No. 5, Prospect Place, Lambeth. The prospectus states: That under the patronage of his royal higness the Duke of Sussex, a seminary is to be opened for the tuition of blind subjects of the higher classes of society, where they may be taught reading, writing, the means of corresponding with distant friends, music, geography, the belles lettres, languages, the rudiments of the sciences generally, and such a familiar acquaintance with prevailing accomplishments, as will enable the blind of both sexes to partake of the innocent amusements of society, including draughts, backgammon, chess, cards, dancing, &c. Among other additions to the plan of M. Haüy, who succeeded in a similar attempt at Paris before the revolution, and on whose model the institution professes to be formed, it adopts the idea of its pupils deriving, from a constant and consoling illustration of the Gospels, those dispositions to habitual cheerfulness and content which they are so eminently calculated to excite when contemplated properly.

ARTIFICIAL COLD.

Professor Leslie, of Edinburgh, in following out a series of experiments on the relations of air and moisture, has within these few weeks been led to a very singular and important discovery. Without any expenditure of materials, he can, by means of a simple apparatus, in which the action of certain chemical powers is combined, freeze a mass

mass of water, and keep it for an indefinite length of time in the state of ice. In the space of an hour he has, on a small scale, formed a cake of ice 6 inches in diameter, and three quarters of an inch thick. With very little trouble he can produce a permanent cold of 90 degrees of Fahrenheit below the temperature of the air, and might easily push it to 100 or even 110. The professor is now engaged in prosecuting these fruitful researches, and will soon, we hope, favour the public with an account of his process, and of the chief results.

SUPPOSED NEW EARTH.

M. Vinterl, of Pest in Hungary, has lately sent to the French Institute several specimens of an earth which he conceived to be new, and to which he gave the name of Andronia. A committee of the Institute, consisting of Messrs. Fourcroy, Guyton Morveau, Berthollet, and Vauquelin, have analysed this substance, and have determined that it is merely a compound of silex, lime, alumine, potash, and iron.

CRANIOLOGY.

The following observations have been published in the foreign journals on the system of craniology by M. Gall.

1. The Italian poet Dolce, who died in 1568, in his dialogue on the means of preserving and strengthening the memory, alludes to a head which is represented at page 8 of the Venice editions of 1562 and 1586, the cranium of which is divided and figured according to M. Gall's system; and under this wood-cut we read the following inscription: "In questa tu vedi ove è il senso commune, ove la fantasia, là cogitativa, la imaginativa, la stimulativa, la memorativa: ed anco l'odorato e il gusto."

2. The grand chancellor of Denmark, Schumacher, count Griffenfield, who died in 1699, must have practised cranioscopy with success, if we may credit M. Wedel Simonson, the author of a dissertation read before the medical society of Copenhagen. The same gentleman (M. Schumacher) maintained a medical disputation in 1650, *De nervis*; Bartholin being then president of the above society.

3. Frenair (a French author) says in his biography of Laurence Sterne, who died in 1768, and which was prefixed to the French translation of Sterne's works, "that an eminent surgeon had dissected the brain of Laurence Sterne, under the persuasion that he would find something extraordinary in its configuration."

4. Swedenborg, who died in 1774, taught that good or

bad

bad qualities had an influence on the form of the cranium.

5. The principal theorem of M. Gall, that the brain impresses on the cranium its different forms, is also to be found in the "*Fragmens Physiognomiques*" of Lavater, Leipsic, 1775—1778.

A German traveller has recently discovered in the neighbourhood of the Red Sea the ruins of the ancient city of Dscherrasch, probably the Gerusa of antiquity. He found the remains of several public edifices, two amphitheatres, several palaces, a temple, &c.

DEATH.

Geology and natural history have lately sustained a severe loss by the premature death of Mr. William Martin of Macclesfield, Cheshire, a member of the Geological Society of London, and author of a most useful work, "Outlines of an Attempt to establish a Knowledge of extraneous Fossils on scientific Principles," in octavo; and also of "Petrificata Derbiensia, or Figures and Descriptions of Petrifactions collected in Derbyshire," in 4to. with coloured plates; of which 52 are contained in the 1st volume published less than a year before his death. We are truly concerned to learn that Mr. Martin has left a wife and young family without means of support; the profession he followed, that of a drawing-master, as commonly happens in country places, not having proved very lucrative. We should rejoice to hear that any considerable progress has been made by Mr. M. towards a second volume of the above highly interesting and useful work, and that some means were devised by the friends of geological science to alleviate the situation of his widow and orphan children.

LIST OF PATENTS FOR NEW INVENTIONS.

To the Rev. Henry Liston, of Ecclesmachen, in Scotland, and Charles Broughton, of Edinburgh, writer to the signet, for improvements in the construction of organs.—July 3, 1810.

To Samuel Hill, of Serle-street, London, Esq., for a method of joining stone pipes in a more effectual manner than has been before discovered.—July 3.

To James Hall, of Walthamstow, for a method of manufacturing a material from the twigs or branches of broom, mallows, and rushes, and other shrubs or plants of the like species, to be used instead of flax or hemp; and for the same purposes for which flax and hemp are now used.—July 3.

To

To John Kent, of Southampton, architect, for certain improvements in the method of making artificial stone.—July 3.

To Robert Howden, of Providence-row, Finsbury-square, baker, for an improved method of extracting foul air out of ships, whereby a constant succession of fresh air will be introduced; and at the same time moderating the degree of heat according to the climate. And also of extracting the foul air from mines and pits of every description, and of regulating the degree of heat, and of giving heat and a constant succession of fresh air to houses in general.—July 3.

To William Shakespear, of Birmingham, and Thomas Osler the younger, of the same place, for an improved method, or methods, of manufacturing glass or paste drops for chandeliers, lamps, and lustres.—July 5.

To Richard Varley, of Cheadle Mosley, in the county of Chester, for certain new additions to and improvements upon the machinery now in use for the roving, spinning, doubling, and twisting, of cotton, silk, flax, wool, mohair, and other materials used for the manufacture of twist, thread, or other kind of yarn.—July 7.

To George Hall, of the Strand, goldsmith, for certain improvements in the art of working and making spoons, forks, and such other articles of gold, silver, or other metals, as usually are or may be stamped or struck by means of seats and punches, or dies of any kind or description; and likewise in the tools or instruments to be used in carrying the said improvements into effect and practice.—July 18.

To Ralph Wedgwood, of Oxford-street, for his new character for language, numbers, and music, and the methods of applying the same.—July 18.

To George Stebbing, of Portsmouth, mathematical instrument maker, for certain improvements on the action and other parts of sea and land compasses.—July 18.

To Benjamin Agerday, of Handsworth, Staffordshire, for improvements in the construction of a toast-stand, (for the purpose of holding a plate before the fire,) a hearth brush or dust brush, and toasting fork, and occasionally in combining or uniting the said brush and toasting fork in one utensil or article.—July 18.

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For July 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	57	64°	59°	29·96	30	Fair
28	59	69	66	·99	50	Fair
29	66	68	56	·99	10	Showery
30	60	72	63	30·10	60	Fair
July 1	64	74	66	·07	65	Fair
2	60	74	64	29·81	70	Fair
3	60	66	55	·45	0	Rain
4	61	56	54	·36	0	Showery
5	60	67	64	·79	51	Showery
6	64	70	62	·98	62	Fair
7	65	74	64	·99	65	Fair
8	67	60	55	·80	10	Rain
9	60	70	58	·88	52	Fair
10	66	69	64	·75	35	Fair
11	65	75	62	·54	66	Stormy
12	60	70	60	·60	49	Showery
13	63	71	64	·56	68	Showery
14	66	69	57	·75	0	Showers with
15	64	69	56	·80	36	Ditto [thunder
16	66	68	55	·93	48	Fair
17	56	64	52	·55	33	Cloudy
18	52	65	55	·70	44	Fair
19	56	66	52	·80	62	Fair
20	56	65	54	·80	60	Fair
21	52	63	52	·94	45	Cloudy
22	53	64	57	30·11	46	Fair
23	57	70	54	·16	70	Fair
24	58	70	57	·14	53	Fair
25	59	73	64	29·96	40	Showers
26	66	63	60	·64	0	Rain

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

XIV. *On Pendulums.* *By* Ez. Walker, *Esq.*

The mechanism of pendulums is a subject which has of late years attracted much attention; but whether any real improvements have been made since the days of Harrison, is a question on which there are various opinions. Rods of zinc, pewter, lead, and other soft metals have been substituted for those of brass, to reduce the gridiron pendulum to a more simple form: but it has been found by experience that some of those soft metals, when under the pressure of the weight of the lens, do not long retain the same power of expansion and contraction.

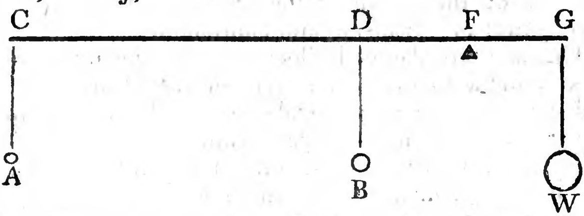
The late Mr. James Bullock, a very ingenious clock-maker, and a man of much experience, told me, that brass and steel were the only metals he could rely on, in the construction of compound pendulums.

The gridiron pendulum is constructed on the supposition that it is kept invariably of the same length by rods of different metals, which have their lengths duly proportioned to their expansions and contractions; but late writers have advanced several objections to this mode of compensation. The principal of these objections are: 1st, The length of the pendulum may be increased by its weight. 2dly, Where the rods pass through the connecting bars there is some friction, which causes them to move by starts, and not according to the increase and decrease of heat: and, 3dly, The difficulty of exactly adjusting the lengths of the rods. But there is another source of error in this pendulum, which has not, I believe, been attended to by writers on this subject.

Suppose that the distance between the centre of the lens and the point of suspension were not to suffer any change by the vicissitudes of heat and cold, still the length of the pendulum might vary. For as the ends of the compensation rods are connected by cross pieces, and as all these, except one, are put in motion by every variation in the temperature of the air, some moving in a direction contrary to the others; therefore it is evident, that the motions of these cross bars must alter the distance between the point of suspension and the centre of oscillation, unless their weights be adjusted according to their motions to and from the point of suspension.

Suppose the cross piece which connects the two extreme rods of the gridiron pendulum be fixed to the centre of the lens, and that the expansions of steel and brass be as three to five: then, if the expansion of one of the extreme steel rods raises the top bar three degrees nearer the point of suspension, the brass rod, which is joined to the steel rod at the top, will remove the cross bar fixed to its lower end, only two degrees from the same point.

To investigate the ratio of the weights of these cross pieces, that the centre of gravity of the pendulum may remain at the same distance from the point of suspension, in all degrees of heat;—Let CG represent an inflexible lever, considered as without weight, kept in equilibrio upon the fulcrum F, by three weights, A, B, and W. And let CF$=x$, DF$=y$, and FG$=z$.



Then *per* mechanics $A\times x + B\times y = W\times z$, or $Ax+By = Wz$. And supposing x and y to flow in contrary directions, we have $\overline{x+\dot{x}}\times A + \overline{y-\dot{y}}\times B = Wz = Ax+By$. Therefore, $A\dot{x} = B\dot{y}$, and consequently, $A : B :: \dot{y} : \dot{x}$.

Hence, if the weights of the cross pieces be inversely as their motions, they will not alter the distance of the centre of gravity from the point of suspension, and "the distance of the centre of suspension from the centre of percussion or oscillation, in the same body, will always remain the same; if the distance of its centre of gravity from the point of suspension, and the plane of its motion (in regard to the body) remain the same *."

This alteration might improve the gridiron pendulum; but it would be very difficult to exhibit a theorem completely accurate for a mode of compensation which is liable to so many irregularities. A pendulum of a more simple construction, and that might be more easily adjusted for heat and cold, is still an object that merits the attention of the astronomer.

The mercurial pendulum is founded on principles more simple and correct than any other compound pendulum that has yet been invented; but the manner of constructing it with a *glass rod* has prevented its being more generally used in the best clocks. This objection is now obviated, by the application of a steel rod with a glass vessel attached to it containing quicksilver, so that when the steel

* Emerson's Fluxions, page 312.

steel rod expands downwards the quicksilver expands upwards, and *vice versâ*. The rate of the clock will show whether the pendulum be over or under corrected; consequently by taking a little quicksilver out of the bob, or adding a little, the compensation may be adjusted to the utmost degree of exactness, and with very little trouble.

It may be supposed that a pendulum cannot be adjusted for heat and cold by the going of the clock, but the rate of my clock shows that this supposition is not well founded; for when a wooden pendulum was attached to it, its rate of going was affected merely by dryness and moisture; but with a mercurial pendulum its rate was affected only by heat and cold, the pendulum being a small matter under corrected; but this variation in its rate during twelve months was very little more than one second per day, although the temperature of the outward air did not vary less, by Fahrenheit's scale, than 70 degrees.

The following register of the going of this clock was computed from the sun's transits over the meridian, observed with a 3½ feet transit telescope.

Lynn, July 16, 1810. E. WALKER.

P. S. For an account of the greatest annual variation in the daily rate of the transit clock at the Royal Observatory for six years, see Phil. Mag. vol. xxxiv. p. 4.

An Account of the Going of a Clock with a mercurial Pendulum made by Mr. Barraud.

1809.	Daily Rate of the Clock.	No. of Days.	1809.	Daily Rate of the Clock.	No. of Days.
June 27					
July 1	+0″·14	4	Aug. 26	+0″·36	3
4	0·30	3	30	0·35	4
11	0·55	7	Sept. 7	0·22	8
13	0·37	2	9	0·42	2
14	0·38	1	15	0·20	6
16	0·18	2	17	0·28	2
19	0·16	3	21	0·53	4
26	0·30	7	24	0·54	3
31	0·29	5	29	0·48	5
Aug. 8	0·44	8	Oct. 1	0·43	2
13	0·30	5	7	0·40	6
17	0·28	4	8	0·41	1
23	0·20	6	11	0·10	3

TABLE (Continued).

1809.	Daily Rate of the Clock.	No. of Days.	1810.	Daily Rate of the Clock.	No. of Days.
26	+0″·38	15	April 11	+1″·18	13
Nov. 7	0·44	12	16	1·18	5
9	0·42	2	18	1·08	2
15	0·51	6	20	1·00	2
16	0·37	1	22	0·75	2
19	0·59	3	23	0·88	1
21	0·45	2	25	0·44	2
24	0·93	3	27	0·27	2
27	0·79	3	28	0·19	1
Dec. 2	0·82	5	29	0·23	1
4	0·68	2	May 1	0·29	2
10	0·83	6	8	0·52	7
13	0·91	3	11	0·56	3
15	0·85	2	13	0·55	2
21	1·14	6	14	0·55	1
28	0·89	7	18	0·63	4
1810.			20	0·62	2
Jan. 1	+0·85	4	21	0·39	1
5	0·75	4	23	0·44	2
10	0·62	5	25	0·29	2
18	0·99	8	26	0·39	1
Feb. 4	1·11	17	27	0·31	1
5	0·90	1	30	0·23	3
10	1·17	5	June 1	0·24	2
14	1·22	4	8	0·08	7
19	1·04	5	14	0·32	6
21	1·09	2	18	0·33	4
26	1·21	5	21	0·31	3
Mar. 11	1·24	13	25	0·15	4
18	1·05	7	29	0·13	4
22	1·16	4	July 1	0·14	2
23	0·96	1	7	0·14	6
25	0·88	2	9	0·14	2
29	1·08	4	12	0·16	3

XV. *The*

XV. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.* *By* HUMPHRY DAVY, *Esq. Sec. R.S. F.R.S.E. M.R.I.A.*

[Concluded from p. 32.]

IV. *On the Metals of Earths.*

I HAVE tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electrochemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogen gas; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind as that which has been employed in the experiment.

I have passed potassium in vapour through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour;

* The results of this experiment are opposed to the idea that potassium is a compound of hydrogen and potash or its basis; for if so, it might be expected that the hydrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogen as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

hour; so that there is great reason to believe, that the silex had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame, was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

Potassium in acting upon alumine and glucine, produced more hydrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable that even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks *, and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them; which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury, whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic

* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compoumd of potassium, sulphur, and charcoal, probably contains this substance likewise.

acid

acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded, by solution in acids, evaporation, and treatment with re-agents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the production of an amalgam, from the metals of the common earths, yet I succeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygen which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter, and that oxygen and inflammable bodies are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that the attractions of acids for salifiable

 bases

bases would be inversely as the quantity of oxygen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808 concluded that barytes must contain least oxygen of all the earths, and that the order as to the quantity of inflammable matter must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5 * of metal per cent. strontites 86 †, lime 73 5 *, magnesia 66 ‡.

The same proportion would follow from an application of Mr. Dalton's ingenious supposition §, that the proportion of oxygen is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, *i. e.* that every neutral salt is composed of one particle of metal, one of oxygen, and one of acid.

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition ‖ of phosphate of alumine, alumine would appear to contain about 56 per cent. of metallic matter,

* Mr. James Thompson, Nicholson's Journal, 1809, p. 175, and Berthier.

† Mr. Clayfield. Thomson's Chemistry, vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol. iii. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis being inversely as the quantity of oxygen contained by the basis, though gained from the comparison of the electrical relations of the earths, with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which, in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. Gay Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygen, might (as far as it is correct) be inferred from Mr. Dalton's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxygen, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygen and of acid are always multiples of these proportions. So that if a protoxide in becoming a deutoxide takes up more acid, it will be at least double the quantity, and in these cases the oxygen will be strictly as the acid. Mr. Dalton's law even provides for cases to which M. Gay Lussac's will not apply, a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron perfectly formed, (as some experiments which I have lately made seem to show,) there is probably only a single proportion of acid; and in the super-tartrite of potash there is only a single quantity of oxygen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

‖ Thomson's Chemistry, vol. ii. p. 581.

M. Ber-

M. Berzelius *, in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47 † of oxygen, which agrees very nearly with the quantity of hydrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston, and though Lavoisier insisted upon this analogy with his usual sagacity, yet still the alkalies, earths, and oxides have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls ‡; and the alkalies and alkaline earths are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and M. Guyton) precipitate the triple prussiate of potash; the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutrosaline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and

* In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

† I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, *Mem. d'Arcueil*, tom. ii. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by MM. Gay Lussac and Thenard.

‡ *Klaproth*, *Annales de Chimie*, tome x. p. 277.

earths,

earths, with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals!

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold-short iron; whilst, from another spot, it will make hot-short; but by a combination of the two in due proportions, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Hydrogen is the body which combines with the largest proportion of oxygen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would show that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined

bined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phænomena presented by sulphuretted hydrogen are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shown, and as is further proved by the researches of MM. Gay Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decompounded, unless a new combination is formed: thus it is not changed by charcoal ignited in the gas by Voltaic electricity; but it is decompounded by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygen being found in the new compound. This, at first view, might be supposed in favour of the idea that hydrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitromuriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxygenation.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus *, and of the boracic,

* The electrization of sulphur and phosphorus goes far to prove that they contain combined hydrogen. From the phænomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygen, though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxygen, till I discovered that similar phænomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted

boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygen, or by the peculiar state of their electrical energy.

The whole tenour of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phænomena with at least equal facility.

If hydrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygen, hydrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases, with hydrogen; the earths, the fixed alkalies, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are: First, The properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hydrogen or oxygen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hydrogen, whether in its simple form, or in combination with one proportion of oxygen, or a double proportion, from my experiments seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hydruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

sulphuretted hydrogen, and on phosphuretted hydrogen, I find that the phænomena differ very much according to the circumstances of the experiment, and in some instances I have obtained a larger volume of gas from potassium after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the Society. The idea of the existence of oxygen in sulphur and phosphorus is however still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium I find when heated in hydrogen, mixed with a small quantity of atmospheric air, absorb both oxygen and hydrogen, and become nonconducting inflammable bodies analogous to resinous and oily substances.

Second,

Second, The facility with which metallic substances are revived, in cases in which hydrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of six inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed: the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogen is essential to the production of the metal.

Third, Oxygen and hydrogen are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygen, and is restored by the addition of hydrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable that this is caused by the imperfection of the process*; and supposing potassium and sodium to produce the same quantity of hydrogen from ammonia and water, the circumstance, at first view, may be conceived favourable to the notion that they contain hydrogen, which under common circumstances of combination will be repellent to matter of the same kind: but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogen are decomposed by a metal, the quantities of hydrogen ought to be the same, or multiples of each other. Thus, in the decomposition of ammonia by potassium and sodium, two of hydrogen and one of nitrogen remain in combination.

* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogen evolved; *i. e.* whenever the metals of the alkalies act upon ammonia, supposing this body to be composed of three hydrogen, and one of nitrogen, in volume, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

combination, and one of hydrogen is given off; and in the action of water on potassium to form potash, the same quantity of hydrogen ought to be expelled. From my analysis * of sulphuretted hydrogen, it would appear, that if potassium in forming a combination with this substance sets free hydrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogen, will liberate the same proportion of hydrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogen, because, in acting upon different combinations containing hydrogen, they produce the evolution of equal proportions of this gas, then it might be proved that almost any kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals that I

* The composition may be deduced from the experiments in the last Bakerian lecture, which show that it contains a volume of hydrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hydrogen, and 32·73 of sulphur. When sulphuretted hydrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogen, it was found that when these gases were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common state; the phosphorus was dark-coloured, and the arsenic appeared as a brown powder, both were probably hydrurets: this is confirmed likewise by the action of potassium upon aseniuretted and phosphuretted hydrogen; when the metal is in smaller quantity than is sufficient to decompose the whole of the gases, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogen contain in equal volumes, more hydrogen than sulphuretted hydrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogen, it would appear that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

have experimented upon, in acting upon muriatic acid gas, evolve the same quantity of hydrogen, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogen in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygen, which may be supposed to exist in hydrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present occupy the time of the Society by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined to enable to decide upon their nature, and their relations, or to form any general theory concerning them which is likely to be permanent.

Explanation of the Figures.

Fig. 1. The apparatus for electrizing potassium in gases. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 20. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a*, *d*, *c*, the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action

of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of £1000 has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action; but reasoning from the effects of that part of it which has been used, some important phænomena may be expected from so great an accumulation of electrical power.

XVI. *Report of the Dublin Cow-Pock Institution, under the Patronage of His Grace the Lord Lieutenant, for* 1809.

An Abstract from the Register of Inoculations and Distribution of Matter.

	Patients Inoculated.	Packets issued to Practioners in general.	Packets to Army Surgeons.
1804	578	776	236
1805	1,032	1,124	178
1806	1,356	1,340	220
1807	2,156	1,790	320
1808	3,002	2,285	333
1809	3,941	2,540	214
Totals.	12,065	9,855	1,531

The directors of the institution have great pleasure in observing the progressive increase of vaccine inoculation, and the influence of experience in satisfying the public of its efficacy. Most of the above 12,065 patients being confined to a city where small-pox has been in general prevalent, must have been exposed in every possible way to its infection, by living in the same house, or frequently sleeping in the same bed with the infected. The anxiety of parents, too, has often led them intentionally to expose their children

children to small-pox infection. As far, however, as the immediate observation of the Institution extends, cow-pock has been found to resist all such trials, with three exceptions only.

It now appears by increasing experience, that in a very few instances the vaccine infection will form fairly on the arm, and go through its regular stages, without being absorbed into the blood. The same thing has repeatedly happened in inoculating for the small-pox, where no eruptive fever or eruption succeeded the inoculation. In the three cases of small-pox which have succeeded vaccination, the disease has been mild and of short duration.

The efficacy of cow-pock, as far as Dublin is concerned, does not rest upon the proofs adduced in its favour by this Institution, for it has been extensively practised during the last five or six years. There are grounds for believing that the number vaccinated throughout the city, including the above 12,065, does not fall short of 35,000. The cases of small-pox following cow-pock which have been reported, upon any reasonable authority, to the Institution, do not exceed six. No one who is acquainted with the careless and inattentive manner in which many practitioners have hitherto conducted vaccination, can be surprised to hear of cases of failure. The neglect of parents also to have their children examined at the regular periods after inoculation, tends to bring the practice into disrepute. To obviate this inconvenience, it has been the practice for some time at this Institution, to oblige parents to deposit a small sum, to be returned after the child has gone through the disease, provided they have attended agreeably to instruction; otherwise the sum is forfeited. This regulation has had the desired effect.

It was reported at an early period of the practice, that vaccination afforded only a temporary security, which was at first limited to three years. Numerous experiments, tried in different quarters, satisfactorily proved the falsehood of this assertion. A similar opinion has been lately revived, but the period of security extended to five or six years. Neither analogy nor experience justifies such an idea, and the history of casual cow-pock fully refutes the allegation, as numerous cases are on record of persons, after having casual cow-pock, resisting during a long life the small-pox, under every circumstance of exposure, inoculation, &c. Besides, had the preventive powers of cow-pock not been permanent, it is but reasonable to suppose that many of

 the

the above 12,065 must, under the existing circumstances of exposure, have taken the small-pox. Above twenty children who were vaccinated five or six years ago, have lately, by order of the directors, been submitted to variolous inoculation, but without the effect of producing small-pox. Similar experiments have been instituted, under the direction of other practitioners, with the like result. Nineteen children who had the cow-pock eight and nine years ago, have been lately inoculated with small-pox matter, at the Foundling Hospital, under the inspection of Mr. Stewart, surgeon-general, and Mr. Creighton, surgeon of the hospital, but with no other effect than local inflammation.—In a letter just received from Mr. Bryce, of Edinburgh, he observes:—"I have lately finished an experiment of inoculating about twenty children with the small-pox, who were vaccinated from eight years to five months.—The result is most satisfactory, and shows clearly that a pustule with surrounding inflammation is as readily produced five months after vaccination, as at the end of eight years, consequently that the security is as complete at the latter period as the former."

The following extract from the Report of the Small-Pox Hospital, London, should be recorded:—"Eleven thousand eight hundred patients, and upwards, have been vaccinated, of which number twenty-five hundred were afterwards proved to be secured from the natural small-pox, by receiving a further inoculation with small-pox matter, which took no effect. A number amply sufficient to satisfy the public mind, of the security and success of the new practice of vaccination."—December, 1802.—So great a number submitted to the test of variolous inoculation, and exposed in a hospital full of small-pox infection without effect, should of itself convince every reasonable mind of the efficacy of vaccination.——*Vide* Mr. Charles Murray's Answer to Mr. Highmore, p. 37.

A report having been lately circulated, that Dr. Jenner himself was beginning to entertain some doubt of the efficacy of his discovery, the directors thought it expedient to direct their secretary to write to him, and to lay his answer upon the subject before the public.

"Dear sir,—Your obliging letter of the 3d instant, inclosing the Annual Report of the Cow-Pock Institution, in Dublin, has just reached me. The former letter you allude to, has not yet been delivered. It is with the greatest pleasure I perceive the rapid increase of vaccination in your metropolis,

metropolis, and the uninterrupted success that has attended the practice, at once a proof of the zeal, industry and attention of the medical officers; for which I beg leave to make my most grateful acknowledgements.

"And now, sir, a few remarks on the very extraordinary communication you have make to me respecting Lady C——. It has been one of the usual devices of the enemies of vaccination, almost from the time of my first making it known, to represent me as having lost my confidence of its prophylactic powers, or, at least, that I was wavering on the subject. Can I, who, with the aid of my nephews, have vaccinated a number of persons little short of 30,000, without one single instance of accident or of failure, that ever reached my ears, for a moment entertain such an absurd idea? Or could I have ever thought of inoculating for the small-pox, while I hold that practice in abhorrence, and condemn it both publicly and privately? Believe me, the whole story you relate to me is an entire fiction, without the faintest shadow of foundation. Never from the commencement of my experiments to the present hour, have I used a particle of variolous matter, except for the purpose of putting some of those to a test on whom I made my first trials. For some years past, I have relied wholly on the vaccine lymph, for testing those on whom any material irregularity appeared in the progress of the pustule.

"Believe me, &c.

Berkeley, Feb. 19, 1809. "EDWARD JENNER."

While the directors, with such weight of evidence in its favour, feel themselves warranted in continuing to recommend vaccination as a preventive of small-pox, they cannot but regret that in a few cases it has been difficult to determine whether a patient has had the disease constitutionally or locally. They however confidently hope that by pursuing Mr. Bryce's test, and by increased attention to the progress of the disease, practitioners will be enabled to surmount the only objection to a practice which tends to preserve more than 30,000 lives annually, in the British Isles.

Mr. Bryce proposes that a second inoculation be performed about the sixth day after the first: the vesicle produced by this second inoculation is accelerated in its progress, so as to arrive at maturity, and again fade, at nearly the same time as the affection arising from the first inoculation. Mr. B. considers the acceleration of the second

 inoculation

inoculation to be the effect of the constitutional affection produced by the first; and therefore, if it shall be found that no such acceleration takes place, but that the second inoculation proceeds by a slow progress through all the stages, it is to be concluded, that no constitutional action has taken place from the first insertion of the virus; and when this is the case, the second inoculation must be regarded as a primary affection, and a third puncture made according to the plan laid down for conducting the second inoculation; and thus (he says) we may go on until the proper test be obtained; or until we be satisfied that the constitution completely resists the action of cow-pock.

Although small-pox is by no means exterminated from Dublin, among the poor, yet the general substitution of vaccine for variolous inoculation has considerably diminished the number of patients brought to the hospitals and dispensaries for advice. In the upper ranks of society death from small-pox is unheard-of, and the most extensive practitioners acknowledge that a case of small-pox in private practice is a very rare occurrence. And although the reintroduction of small-pox into society would add greatly to the emoluments both of physic and surgery, there is no liberal man in either profession who would not sincerely deplore such a calamity.

Signed by order,

January 10, 1810. S. B. LABATT, Secretary.

Foundling Hospital, Dublin, Jan. 4, 1810.

The following Report having been laid before the Governors of the Foundling Hospital, and appearing to be highly satisfactory:—Ordered, That three thousand copies thereof be printed, for the purpose of their being circulated as generally throughout the United Kingdom as possible.

By order,

A. BAILIE, Register.

AS some persons have lately attempted to prejudice the minds of the public by representing vaccine inoculation as a doubtful security against small-pox, limiting its influence to a certain period, and wishing us to believe that its preventive powers diminish in proportion to the distance of time from inoculation;—I have, therefore, at the request of the right honourable and honourable the governors of the Foundling Hospital, instituted such experiments as enable me (a second time) to congratulate the public on their successful event.

From

From my situation, as surgeon to the Foundling Hospital, I have had it fully in my power to select such cases as had been faithfully recorded by me to have undergone vaccination at the earliest period of cow-pock inoculation in this city, and such have been approved of by those gentlemen who have honoured me with their presence, to witness and subscribe their names to the progress and event of the following experiment on nineteen children chosen for the purpose, who were divided into two classes. The first nine comprehend those who in a state of infancy were vaccinated by me between the 30th of December 1800 and 3d of July 1801, now more than eight years. These were again inoculated with small-pox infection by George Stewart, esq. surgeon-general, on the 24th of July, 1804, (and witnessed by several gentlemen of the first respectability in their profession,) in like manner to disprove the assertions of Mr. Goldson, as may be seen in the twelfth volume of the Medical and Physical Journal, and with the most complete success—all having resisted the small-pox, although exposed to it in every way possible. These nine children, with ten others, who were also vaccinated by me in a state of infancy, from 15th of July 1801 to 30th of August 1802, upwards of seven years, were again submitted to small-pox inoculation, on Friday, 22d of December last; the infection taken from a child of Mr. Stafford's, No. 7, Hanbury-lane, in confluent small-pox, and the matter inserted in two places in the arm of each child, in a fluid state, and in the greatest quantity. In every instance, the punctures in the arm of each child from the third day inflamed, and continued until the seventh, when the inflammation gradually subsided, as certified by Mr. Stewart, and marked in a table, which, in another publication, will be more fully expressed;—which circumstance has proved the activity of the small-pox matter inserted, and which must have affected the constitution, were it in the least susceptible of the disease. Fourteen days have now elapsed, the inflammation of the punctures is entirely gone, and never was attended with the slightest fever, sickness, or eruption.

In corroboration of the above facts, conducted with every degree of accuracy, and which cannot admit of the smallest doubt on the minds of those gentlemen who have witnessed them, and hereunto subscribed their names; I can safely assert, that I have submitted upwards of five hundred infants and children, vaccinated by me at this Institution, and at the Dispensary for Infant Poor and Cow-

 pock

pock Inoculation, as established in the year 1800, to a like experiment, and with the same result in every instance.

Dublin, Merrion-square, West, January 4, 1810. J. Creighton,

George Stewart,	A. Colles,
Gustavus Hume,	William Hartigan,
S. Wilmot,	Philip Crampton,
Ralph S. Obre,	

Members of the Royal College of Surgeons in Ireland.

Edmund Connell,	William Dillon,
Samuel Bell,	James M'Creight,

Apothecaries.

XVII. *Information, that a further Publication of the late Mr. Smeaton's Engineery Designs and Papers is in hand.—Copy of a List of the principal British Strata, by the late Rev. John Michel, (of whose posthumous Papers on Geological Subjects, further Information is requested;)—with some Experiments of Mr. Smeaton's on Limestones,—and Queries respecting Mr. Tofield. Communicated by Mr.* John Farey.

To Mr. Tilloch.

Sir, As my eldest son was a few days ago employed, in examining the miscellaneous bundles of papers, which belonged to the late ingenious Mr. John Smeaton, the civil engineer, now in Sir Joseph Banks's possession, with a view to the further publication by Messrs. Longman, Hurst, Rees, and Co. of his Drawings and Reports on civil engineery, which so long and impatiently have been expected by those interested in this branch of the useful arts, he found a small scrap of paper (only four inches by three) in the handwriting of Mr. Smeaton, part of the cover of a letter, as appears by part of a seal and the London post-mark of November 21, 1788, on the back of it, which, having obtained Sir Joseph's permission, I think of sufficient importance, in a geological point of view, to request the favour of you to lay before your readers.

It relates to the order and thicknesses of the *strata* in England, as appears by Mr. Smeaton's title or endorsement on it, viz. "Mr. Michel's account of the south of England strata," which is as follows, viz.

	Yards
"Chalk	120
Golt	50

Sand,

	Yards
Sand, of Bedfordshire	10 or 20
Northampton lime and Portland limes lying in several strata	100
Lyas strata	70 or 100
Sand, of Newark ... .about	30
Red clay, of Tuxford and several	100
Sherewood Forest, pebbles and gravel ...	50 unequal
Very fine white sand	uncertain
Roch Abbey and Brotherton limes	100
Coal strata, of Yorkshire	"

The Mr. Michel alluded to, was, it appears, the late Rev. John Michel, rector of Thornhill, near Wakefield, Yorkshire, who was an intimate friend of Mr. Smeaton, the late Mr. Cavendish, &c. &c., and whose name must be very familiar to most of your readers, from his many valuable papers in the Transactions of the Royal Society of London, of which he was a member.

This account of the strata, imperfect as it is, appears to me important, as showing, that Mr. Michel was acquainted with the principal features of the south of England strata, at an earlier period than any thing was published on the subject, especially if we suppose, as is most reasonable, that this communication was made verbally by Mr. Michel to his friend Mr. Smeaton, very soon after November 1788, who took it down on the cover of a recent letter, as being the only piece of paper then at hand; for Mr. Smeaton's decease in September 1792, shows that it must have been prior to that time.

It appears to me probable, that this account was principally made from the result of Mr. Michel's observations, in his journeyings by the great North road between the place of his residence and London; The "chalk" being that which appears from near Hatfield to Baldock; the "golt" being the chalk-marle (and perhaps some alluvial clays also) thence to near Sandy in Bedfordshire; where, doubtless, the "sand" is situate, to which he alludes. In crossing Northamptonshire from Wansford to Stamford, the "limes" are first noticed, which he rightly associates with, and considers the same as, those of Portland-Island, though distant 170 miles therefrom in a straight line! The next are the "lyas" strata, which appear between Grantham and Balderton; and here, the use of a term for these strata of limestone, which was not then known or in use, I believe, nearer than Gloucestershire or Somersetshire, shows again that

that Mr. Michel had contemplated the identity of the British strata over wide spaces*. The "sand" of Newark is seen on its S.E. side near Balderton; the "red clay" of Tuxford is noticed as the produce of "several" other places, and is the gypseous earth, or red marle, which forms so conspicuous a figure across a large portion of the middle and western parts of England. Sherwood Forest "pebbles and gravel," over the northern skirt of which, this road passes between Tuxford and Doncaster, is noticed by Mr. Michel, as being "unequal" in thickness; and if his observations had been further extended, it would doubtless have appeared clear to him, that the same ought not to have been taken into his list of strata, any more than the numerous other patches of *alluvium on the surface* which he must have passed in this road, and has not noticed; and particularly so, if I am right in conjecturing, that the "very fine white sand," which he mentions, as of "uncertain" thickness, is enveloped, *as an accidental bed* in the "red clay," (which he had before mentioned) similar to what we find at Normanton on the S. of Derby, and some few other places, for the occurrence is rather rare, I believe, and should therefore wish much to learn, the precise spot or pits to which Mr. Michel here alludes; it being a part of the country which I have never visited.

The "lime" of Brotherton being associated with that of Roche-Abbey, 25 miles S.S.E. of it, shows again, that Mr. Michel had discovered some at least of those geological principles, which the labours of Mr. *William Smith* very soon after tended to confirm, and to render them of the utmost practical use and importance.

Mr. Michel was also aware, that the coal-strata known in Yorkshire, are under-measures to the yellow lime, above mentioned: it must however be observed, that the *thicknesses* in the above list are most of them (except perhaps the chalk, the golt, and the Balderton sand) greatly underrated; while many very thick or important strata (of which I intend to give a short account in my Derbyshire Report) are omitted altogether; as the Bagshot-heath sand, the

* The lyas or blue lias limestone having been much the object of Mr. Smeaton's notice, on account of its important quality of making a durable mortar which sets suddenly and very hard, even under sea-water, as he proved in the building of the Eddystone light-house and others of his great works; and with whose appearance at Aberthaw and Watchet on the opposite shores of the Bristol Channel, and numerous other places, he was so well acquainted, was probably the reason, why so very laconic a mention of these strata is here made by Mr. Smeaton.

London

London clay, the Woolwich or Black-heath sand, the Aylesbury limestone, the Clunch clay*, the Belford limestone and clays beneath it, the Barnack rag, and Colley-weston lime and slate, the Foston blue clay, and the Maidwell lime, all of which occur above the lias-clay; while the coal series above the yellow lime (under the Sherwood gravel as I suspect) and the important *blue beds* in the yellow limestone series, are unnoticed: enough however is contained in the above list, to show, that the late Rev. John Michel ought to be ranked among those, to whom geological science is indebted; and I take this method of addressing myself to those, who may be now in possession of his papers, to search for and communicate whatever details they may contain on the British strata, that will either further explain the above communication to Mr. Smeaton, or show the source, whence Mr. M. may have derived the above particulars of the South British strata†: which would be conferring a great obligation on

Your obedient humble servant,

12, Upper Crown Street, Westminster, August 4, 1810. JOHN FAREY, Sen.

XVIII. *An*

* Between the Bedfordshire or Woburn sand, and the Northamptonshire limes or Bath freestone, which clay extends under almost all the Lincolnshire fens, and most of those in Cambridgeshire and in Yorkshire.

† P. S. Since writing the above I have been informed, that Mr. Michel, whose death happened April 21, 1793, was at an early part of his life keeper of the Woodwardian collection of fossils at Cambridge, which is thought by some to be the very best general geological collection in existence, though made near a century ago, owing to the great care and minuteness with which the *localities* and attendant circumstances of the fossils therein, are described: essential particulars, which yet have appeared beneath the attention of too many of our modern mineralogists and geologists, as it should seem. It is not improbable, that a comparison of the fossils and their localities, in this celebrated collection, first suggested the ideas of a determinate order in the British strata to Mr. Michel, and the examination of his papers is therefore a matter of the greater importance from the probability, that some such arrangement of the facts in the Woodwardian catalogue, may be found among them. Perhaps also, the present keeper of the Woodwardian collection and papers will have the goodness to inform us, whether any such arrangement of the British strata in a series is to be found, or minutes of any such attempts, among the Woodwardian papers?

Another scrap of paper, found among Mr. Smeaton's loose memorandums, contains his experiments on twelve sorts of limestone, by dissolving 40 grains of each in aquafortis, and drying the clayey undissolved residuums in the sun, the weights of which are as follow, viz.

	Grains.
"Yellow lyas, of Axminster	$5\frac{3}{4}$
Ditto with shining spangles (mica probably) ..	$5\frac{1}{2}$
Yellow snake-stone, of Glastonbury	5
Blue lyas, of Watchet	$4\frac{1}{2}$
Ditto of Aberthaw	$4\frac{1}{2}$
Ditto of Bath	$4\frac{1}{4}$
Ditto of Axminster	$3\frac{1}{2}$
Yellow clump-stone, of Sherborne	3

White

XVIII. *An Analysis of several Varieties of British and Foreign Salt, (Muriate of Soda,) with a view to explain their Fitness for different œconomical Purposes. By* William Henry, *M.D. F.R.S. Vice-Pres. of the Literary and Philosophical Society, and Physician to the Infirmary at Manchester**.

Sect. I. *General Observations.*

In undertaking the series of experiments described in the following pages, I had not so much in view the discovery of novelties in science, as the determination, by the careful employment of known processes, and by the improvement of methods of analysis, of a number of facts, the establishment of which (it appeared to me probable) might have an influence on an important branch of national revenue and industry.

An opinion has for some time past existed, and I believe has been pretty general both in this and other countries, to the disadvantage of British salt as a preserver of animal food; and a decided preference has been given to the salt procured from France, Spain, Portugal, and other warm climates, where it is prepared by the spontaneous evaporation of sea water. In conformity with this opinion, large sums of money are annually paid to foreign nations, for the supply of an article, which Great Britain possesses, beyond almost any other country in Europe, the means of drawing from her own internal resources. It becomes, therefore, of much consequence to ascertain, whether this preference of foreign salt be founded on accurate experience, or be merely a matter of prejudice; and, in the

	Grains.
White lyas, with shining spangles, of Wells	1¾
Brown limestone, of Plymouth	1
———— of Chidley (Chidgley?)	0¾

Forty grains of burnt lime in flower, dissolved in aquafortis, left of clayey matter when dried in the sun, as follows, viz.

Blue lyas, of Watchet	4½
——— of Briddistow	3¾

The Watchet (residuum) made into a ball just stuck together, the Briddistow scarcely."

When I was at the house of Mr. Jessop the engineer (who was formerly a pupil and assistant of Mr. Smeaton) at Butterley in Derbyshire, he mentioned that a Mr. *Tofield*, a civil engineer of the southern part of Yorkshire, formed a design 30 years ago, of investigating the British strata. I shall be thankful to any of your readers who can communicate any particulars of this undertaking, and of its author, if they will do so.

* From Philosophical Transactions for 1810, Part I.

former

former case, whether any chemical difference can be discovered, that may explain the superiority of the one to the other.

The comparative fitness of these varieties of salt for the curing of provisions, which has been a subject of much controversy among the parties who are interested, can be decided, it is obvious, in no other way, than by a careful examination of the evidence on both sides. Where evidence, however, is doubtful, and where there exists, as in this case, much contrariety of testimony, it cannot be unfair to yield our belief to that which best accords with the chemical and physical qualities of the substances in question. Again, if salt of British production should be proved to be really inferior in chemical purity to foreign salt, it would be important to ascertain, as the basis of all attempts towards its improvement, in what, precisely, this inferiority consists. It seemed desirable, also, to examine whether any differences of chemical composition exist among the several varieties of home-made salt, which can explain their variable fitness for œconomical purposes.

Such were the considerations that induced me to undertake an inquiry, which has occupied, for several months past, a large share of my leisure and attention. I began the investigation, wholly uninfluenced by any preconceived opinions on the subject; and I had no motive to see the facts in any other than their true light, since I have no personal interest, either directly or remotely, in the decision of the question.

The principal sources of the salt, which is manufactured in this country, are rock salt, brine springs, and sea water. The first material is confined entirely, and the second chiefly, though not wholly, to a particular district of Cheshire. Of the extent and boundaries of this district, the process of manufacture, and other circumstances interesting to the mineralogist as well as to the chemist, an ample and excellent history has been given by Mr. Henry Holland, in the agricultural report of the county of Chester*. From his account, I shall extract, in order to render some parts of this memoir more intelligible, a very brief statement of the characteristic differences of the several varieties of salt, which are prepared in Northwich and its neighbourhood.

In making the *stoved* or *lump salt*, the brine is brought to a boiling heat, which, in brine fully saturated, is 226°

* Published in 1808.

of

of Fahrenheit. This temperature is continued during the whole process; and as the evaporation proceeds, small flaky crystals continue to form themselves, and to fall to the bottom of the boiler. At the end of from eight to twelve hours, the greatest part of the water of solution is found to be evaporated; so much only being left, as barely to cover the salt and the bottom of the pan. The salt is then removed into conical wicker baskets, termed *barrows;* and, after being well drained, is dried in stoves, where it sustains a loss of about one-seventh of its weight.

On the first application of heat to the brine, a quantity of carbonate of lime, and sometimes a little oxide of iron, both of which had been held in solution by an excess of carbonic acid, are separated; and are either removed by skimming, or are allowed to subside to the bottom of the pan, along with the salt first formed, and with some sulphate of lime; and are afterwards raked out. These two operations are called *clearing* the pan. Some brines scarcely require them at all, and others only occasionally. The whole of the impurities, however, are not thus removed; for a part, subsiding to the bottom, forms a solid incrustation, termed by the workmen *pan-scale.* The portion of this, which is lowest, acquires so much induration and adhesion to the pan, that it is necessary to remove it, once every three or four weeks, by heavy blows with a pick-axe. These sediments are formed, also, in making the other varieties of salt.

In preparing *common salt,* the brine is first raised to a boiling heat, with the double view of bringing it as quickly as possible to the point of saturation, and of clearing it from its earthy contents. The fires are then slackened, and the evaporation is carried on for 24 hours, with the brine heated to 160° or 170° Fahrenheit. The salt, thus formed, is in quadrangular pyramids or hoppers, which are close and hard in their texture. The remainder of the process is similar to that of making stoved salt, except that after being drained it is carried immediately to the storehouse, and not afterwards exposed to heat, an operation confined to the stoved salt.

The *large-grained flaky salt* is made with an evaporation conducted at the heat of 130 or 140 degrees. The salt thus formed is somewhat harder than common salt, and approaches more nearly to the cubic shape of the crystals of muriate of soda.

Large-grained or *fishery salt* is prepared from brine heated only to 100° or 110° Fahrenheit. No perceptible agitation,

agitation, therefore, is produced in the brine, and the slowness of the process, which lasts from seven or eight to ten days, allows the muriate of soda to form in large, and nearly cubical crystals, seldom however quite perfect in their shape *.

For ordinary domestic uses, stoved salt is perfectly sufficient. Common salt is adapted to the *striking* and salting of provisions, which are not intended for sea voyages or warm climates. For the latter purposes, the large-grained or fishery salt is peculiarly fitted.

On the eastern and western coasts of Scotland, and especially on the shores of the Firth of Forth, large quantities of salt are made by the evaporation of sea water. In consequence of the cheapness of fuel, the process is carried on, from first to last, by artificial heat, at a temperature, I believe, equal or nearly so to the boiling point, and varying, therefore, according to the concentration of the brine. The kind of salt, chiefly formed in Scotland, approaches most nearly to the character of stoved salt. In some places a salt is prepared, termed *Sunday salt;* so called, in consequence of the fires being slackened between Saturday and Monday, which increases considerably the size of the crystals.

I am indebted to Dr. Thomson of Edinburgh, (who gave me his assistance with great zeal and alacrity) for an opportunity of examining upwards of twenty specimens of Scotch salt, prepared by different manufacturers. That distinguished chemist, it appears from a letter which he addressed to me on the subject, was some time ago engaged in experiments on Cheshire salt. The particulars he has lost; and he retains only a general recollection of the facts, which confirms, I am happy to state, the accuracy of the results obtained by my own experiments.

At Lymington in Hampshire, advantage is taken of the greater heat of the climate, to concentrate the sea water by spontaneous evaporation to about one-sixth its bulk, before admitting it into the boilers. One kind of salt is chiefly prepared there, which most nearly resembles in grain the stoved salt of Cheshire. The process varies a little, in some respects, from that which has been already described. The salt is not fished (as it is termed) out of the boiler, and drained in baskets; but the water is entirely evaporated, and the whole mass of salt taken out at once, every eight hours, and removed into troughs with holes in the bottom.

* Cheshire Reports, p. 53, &c.

Through

Through these it drains into pits made under ground, which receive the liquor called *bittern* or *bitter liquor*. Under the troughs, and in a line with the holes, are fixed upright stakes, on which a portion of salt that would otherwise have escaped, crystallizes and forms, in the course of ten or twelve days, on each stake, a mass of sixty or eighty pounds. These lumps are called *salt cats*. They bear the proportion to the common salt, made from the same brine, of one ton to 100.

From the mother brine or bitter liquor, which has drained into the pits, the sulphate of magnesia is made during the winter season, when the manufacture of salt is suspended, in consequence of the want of the temperature required for the spontaneous evaporation of the sea water. The process is a very simple one*. The bitter liquor from the pits is boiled for some hours in the pans, which are used in summer to prepare common salt; and the impurities, which rise to the surface, are removed by skimming. During the evaporation, a portion of common salt separates; and this, as it is too impure for use, is reserved for the purpose of concentrating the brine in summer. The evaporated bitter liquor is then removed into wooden coolers eight feet long, five feet wide, and one foot deep. In these it remains twenty-four hours, during which time, if the weather prove clear and cold, the sulphate of magnesia, or Epsom salt, crystallizes at the bottom of the coolers, in quantity equal to about one-eighth of the boiled liquor. The uncrystallizable fluid is then let off through plug-holes at the bottom of the coolers; and the Epsom salt, after being drained in baskets, is deposited in the store-house. This is termed *single* Epsom salts, and after solution and a second crystallization, it acquires the name of *double Epsom* salts. Four or five tons of sulphate of magnesia are produced from a quantity of brine, which has yielded 100 tons of common, and one ton of cat salt.

On the banks of the Mersey, near its junction with the

* I am indebted for an account of this process, as well as of the method of making common salt at Lymington, to the liberal communication of Charles St. Barbe, esq. of that place. Though not strictly connected with the subject, I give his description of the mode of making Epsom salt, because no correct statement of the process has, I believe, been hitherto published. The analysis of sea water, indeed, by a justly distinguished chemist (Bergman), excludes, erroneously, the sulphate of magnesia from its composition, and his results have led to the opinion, that to manufacture this salt on the large scale, requires the addition either of sulphuric acid, or of some sulphate to the *bitter liquor*. (See Aikin's Chemical Dictionary, ii. 338.)

Irish

Irish Channel, the water of that river before evaporation is brought to the state of a saturated brine, by the addition of rock salt. The advantage of this method of proceeding will be obvious when it is stated, that 100 tons of this brine yield at least 23 tons of common salt, whereas from the same quantity of sea water, with an equal expenditure of fuel, only two tons 17 cwt. of salt can be produced *.

Within the few past years, an attempt has been made to apply rock salt itself to the packing of provisions. For this purpose it is crushed to the proper size between iron rollers. The trials which have been made, I am informed, are but few, and the results hitherto are not perfectly known.

The *bay salt* imported from foreign countries is well known to be prepared by the spontaneous evaporation of sea water, which, for this purpose, is confined in shallow pits, and exposed to the full influence of the sun and air. I have no addition to make to the accounts of its manufacture, which have already been given by various writers †.

As the results of the investigation, which forms the subject of this memoir, may be acceptable to many persons who can scarcely be expected to take an interest in a long detail of analytical processes, I shall present, in the following section, a general view of the experiments, and of the conclusions that may be deduced from them. In the last place, in order that other chemists may be enabled to repeat the analyses under similar circumstances, I shall describe minutely the methods that were adopted, some of which are new, and others reduced to greater precision. If, however, in the future progress of science, it should appear that any of these processes are imperfect, it may still be admitted that, for all useful purposes, they afford a fair *comparison* of the composition of the several varieties of culinary salt; since the sources of fallacy, that may hereafter be discovered, must have been the same in every case, and have produced in each an error of nearly the same amount.

Sect. II. *General Statement of the Results of the Experiments, and Conclusions that may be deduced from them.*

A comparison of the component parts of British and foreign

* See the Earl of Dundonald's "Thoughts on the Manufacture and Trade of Salt." London, 1785.

† Encyclop. Method. art. *Salins.* (Des Marais Salans) Aikin's Dictionary of Chemistry, ii. 224. Watson's Chemistry, vol. ii. p. 52. It is necessary to remark, that a great proportion of what is sold in London as bay-salt is Cheshire large-grained fishery salt.

foreign salts, and of different varieties of British salt with each other, will best be made by an examination of the following table, which comprehends the results of the analysis of equal weights of each variety.

Group	Kind of Salt.	Insol. matter.	Muriate of lime.	Muriate of Magnesia	Total earthy muriates.	Sulph. of lime.	Sulph. of magnesia.	Total sulphates.	Total impurities.	Pure muriate of soda.
		1000 parts by weight consist of								
Foreign baysalt.	St. Ube's	9	a trace.	3	* 3	23½	4½	28	40	960
	St. Martin's	12	do.	3½	* 3½	19	6	25	40½	959½
	Oleron	10	do.	2	* 2	19½	4¼	23¾	35¾	964¼
Brit. salt from sea water.	Scotch (common)	4	—	28 or*	28 or*	15	17½	32½	64½	935½
	Scotch (Sunday)	1	—	11½	11½	12	4½	16½	29	971
	Lymington (com.)	2	—	11	11 or*	15	35	50	63	937
	Do. (cat)	1	—	5	5	1	5	6	12	988
Cheshire salt.	Crushed rock	10	0·1/16	0·3/16	0·¼	6½	—	6½	16¾	983¼
	Fishery	1	0·¼	0·¾	1	11¼	—	11¼	13¼	986¾
	Common	1	0·¼	0·¾	1	14½	—	14½	16½	983½
	Stoved	1	0·¼	0·¾	1	15½	—	15½	17½	982½

I. The *total amount of impurities*, and the *quantity of real muriate of soda*, contained in each variety of common salt, may be learned by inspecting the two last columns of the table. From these it appears, that the foreign bay salt is purer, generally speaking, than salt which is prepared by the rapid evaporation of sea water; but that it is contaminated with about three times the amount of impurities discoverable in an equal weight of the Cheshire *large-grained* salt, and with more than twice that of those that are found in the *stoved* and *common* salt of the same district.

II. The *insoluble matter* in the foreign salt, after the action of boiling water, appears to be chiefly argillaceous earth coloured by oxide of iron, and is probably derived in part from the pits in which the sea water is submitted to evaporation. We may, perhaps, assign the same origin to the very minute portion of muriate of lime, which is not found in the salt prepared by evaporating sea water in metallic vessels, nor even in the mother liquor, or uncrystallizable residue. In sea salt prepared by rapid evaporation, the insoluble portion is a mixture of carbonate of lime with carbonate of magnesia, and a fine siliceous sand; and in the salt prepared from Cheshire brine, it is almost entirely carbonate of lime. The insoluble part of the less pure pieces of rock salt is chiefly a marly earth, with some sulphate of lime. The quantity of this impurity, as it is stated in the table, is considerably below the average, which in my experiments has varied from 10 to 45 parts in 1000.

Some

Some estimate of its general proportion, when ascertained on a larger scale, may be formed from the fact that Government, in levying the duties, allows 65lb. to the bushel of rock salt, instead of 56lb., the usual weight of a bushel of salt.

III. The *earthy muriates,* and especially that with base of magnesia, abound most in salt which is prepared by the rapid evaporation of sea water. Now since common salt, in all its forms, contains, as will afterwards appear, very little water of crystallization, it is probable that the muriate of magnesia, discovered by the analysis of sea salt, is derived entirely from that portion of the mother liquor which adheres to the salt after being drained, and which amounts to about one-seventh of its weight. The larger the size of the grain, the less is the quantity of this solution which the salt holds suspended; and hence the salt prepared at a lower degree of heat, being in larger crystals, is less debased by the magnesian muriate, than the salt formed at a boiling temperature. It is probable, also, that when the salt is drawn at intervals from the boiler, the proportion of the earthy muriate will vary with the period of the evaporation at which it is removed. For it may readily be conceived, that as the proportion of the earthy muriates in any brine is increased by the separation of muriate of soda, the greater will be the quantity of the muriates which the crystals of common salt, formed in the midst of the brine, will retain; thence it follows, that, so far as the earthy muriates only are concerned, salt must diminish in purity as the process of evaporation advances.

In the several varieties of Cheshire salt, the earthy muriates do not exceed one thousandth part of this weight, and they are precisely (or so nearly so that the difference is not ascertainable) the same in all. This will cease to be matter of surprise, when it is considered that the salt obtained by evaporating to dryness the whole of a portion of Cheshire brine, does not give more than five parts of earthy muriates in 1000. In the entire salt of sea water, according to Bergman, the earthy muriates form no less than 213 parts in the same quantity.

According to the proportion in which the earthy muriates are present in any kind of salt, will be its power of deliquescence, or of attracting moisture from the atmosphere. It is not entirely, however, from the salts with earthy base that common salt derives this quality; for the most transparent specimens of rock salt, which I find to consist of absolutely pure muriate of soda, attract much moisture from a humid atmosphere.

IV. The *sulphate of magnesia* and the *sulphate of lime* both enter into the composition of all the varieties of salt prepared from sea water; but the sulphate of lime alone is found in Cheshire salt. The proportion of sulphate of magnesia is greatest in that variety of sea salt which has been formed by rapid evaporation. In foreign bay salt its quantity is very insignificant.

From the table it may be seen, that the proportion of sulphate of lime is greater in foreign bay salt than in any variety of British salt, even than in those which are prepared from sea water with a boiling heat. The only explanation of this fact, that occurs to me, is, that during the rapid evaporation of sea water a considerable part of the calcareous sulphate is precipitated at an early stage of the process, and is partly removed in *clearing* the boiler, a process which can scarcely be performed during the formation of bay salt, in pits whose sides are composed of moist clay. The remainder of the selenite, thus precipitated by the rapid evaporation of sea water, enters into the composition of the pan-scale.

In the course of this inquiry I was induced to repeat the same experiments several times, on various specimens of salt bearing the same designation; and was surprised to find that the results by no means corresponded. In one instance, for example, fishery salt was found in 1000 parts to contain no less then 16 parts of sulphate of lime; while another specimen, nominally the same, contained only 11¼ parts of selenite in the same quantity; and a third only 5¼. At length it occurred to me that these differences were probably owing to the circumstance of the salt having been taken from the boiler at different periods of the evaporation. I requested, therefore, to be furnished with specimens of salt, drawn at different stages of the process from a given portion of brine, evaporated in the same boiler. These were submitted to analysis; and the results are shown in the following table.

		Sulphate of lime.
Common salt drawn from the boiler two hours after the first application of heat ..	contained in 1000 parts	16
Salt drawn four hours after do.		11
Salt drawn six hours after do.		3½

Hence it appears that there was a gradually increasing purity in the salt from sulphate of lime, as the process of evaporation

evaporation advanced, the greatest part of this earthy compound being deposited at an early stage of the process. Different specimens of the same kind of salt may, therefore, differ in chemical purity as much from each other as from other varieties. But when the impurities contained in a solution of muriate of soda are of a different species from those of Cheshire brine, and consist chiefly of the earthy muriates, the order will be reversed, and the purest salt, as I have already suggested, will be that which is first deposited, the contamination with the muriate of lime or of magnesia continuing to increase as the process advances to a conclusion *.

At an early period of the inquiry, it appeared to me probable that the differences between the several varieties of culinary salt might depend, in some degree, on their containing variable proportions of water of crystallization. It was found, however, by experiment, that the proportion of water in any variety of common salt, after being dried at 212° Fahrenheit, is not much greater or less than that which is contained in any other variety. Pure transparent rock-salt, calcined for half an hour in a low red heat, (=4° or 5° of Wedgwood's pyrometer,) lost absolutely nothing of its weight. It is remarkable, also, that the pure native salt, if free from adventitious moisture, may be suddenly and strongly heated, with scarcely any of that sound called *decrepitation*†, which is produced by the similar treatment of all the varieties of artificial salt. Even these varieties, however, exposed during equal times to a low red heat, do not lose more than from half a grain to three grains in one hundred. This comparison cannot be extended to the salt prepared at a boiling temperature from sea water; because the muriate of magnesia which these varieties contain, is decomposed at a red heat, and deprived of its acid.

* I cannot on any other principle explain the considerable differences, as to the proportion of muriate of magnesia, that were discovered in the several varieties of Scotch salt sent to me by Dr. Thomson. For this reason, in stating the analysis of Scotch salt, I have given, in the table, that result which was most frequently obtained; and have withheld the names of the manufacturers, because the differences were probably in a great measure accidental, and not the result of greater or less skill in the preparation. One specimen of Lymington salt which I examined, contained fully as much muriate of magnesia as any of the Scotch samples. The *cat salt* of that place, however, contrary to my expectation, proved to possess a very extraordinary degree of purity; a fact of which I satisfied myself by repeated experiments.

† Decrepitation is occasioned by the sudden conversion into vapour of the water contained in salts, when its quantity is insufficient to effect the watery fusion. It is a property peculiar to salts which hold only a very small proportion of water in combination; as muriate of soda, nitrate of lead, and sulphate of potash.

The following table shows the quantity of water contained in several kinds of salt, inferred from the loss which they sustain by ignition during equal times, after being first dried at 212°.

100 parts of large-grained fishery salt contain of water	3
100 .... foreign bay salt (St. Martin's)....	3
100 .... ditto..........(Oleron)	$2\frac{1}{2}$
100 .... ditto, Cheshire common salt......	$1\frac{1}{2}$
100 .... ditto......stoved salt....	$0\frac{1}{2}$

The loudness and violence of the decrepitation was, as nearly as could be judged, in the same order, and was most remarkable in the large-grained varieties.

To determine the proportions of real muriate of soda in those varieties of artificial salt which are nearly free from earthy muriates, I employed also the process of decomposition by nitrate of silver. The following are the quantities of fused *luna cornea* obtained from 100 grains of each of three varieties dried, previously to solution, at the temperature of 212° Fahrenheit.

100 gr. pure transparent rock salt gave of luna cornea	242
100....stoved salt, remarkably pure...........	239
100....fishery salt. do....................	237*

The proportion of ingredients in the several kinds of muriate of soda (setting apart the impurities) appears, therefore, to be nearly the same in all. And as the very minute quantity of water discovered by analysis is not constant in the several varieties, it may be inferred to be rather an accidental than a necessary ingredient; for in the latter case an invariable proportion might be expected, conformably to the important law, establishing an uniformity in the proportions of chemical compounds, which has been explained by Mr. Dalton, and confirmed by Drs. Thomson and Wollaston.

What then, it may be inquired, is the cause of those differences

* From 100 grains of pure artificial muriate of soda, previously heated to redness, Dr. Marcet has since informed me that he obtained 24·16 grains of fused luna cornea. The weights of the precipitates thrown down in my experiments by nitrate of silver are not, I am aware, exactly those which might have been expected from the table of the comparative proportions of water given in the text. Each experiment, however, was twice repeated with every precaution I could adopt, and with the same results. That different kinds of salt give different proportions of luna cornea, is proved also by comparing the experiment of Dr. Marcet with the results of Dr. Black and Klaproth, both of whom found the fused muriate of silver from 100 parts of common salt to weigh 235 grains.

differences which are acknowledged, on all hands, to exist among the several species of muriate of soda, so far as respects their fitness for œconomical purposes? If I were to hazard an opinion, on a subject about which there must still be some uncertainty, it would be that the differences of *chemical composition*, discovered by the preceding train of experiments, in the several varieties of culinary salt, are scarcely sufficient to account for those properties which are imputed to them on the ground of experience. The *stoved* and *fishery* salt, for example, though differing in a very trivial degree as to the kind or proportion of their ingredients, are adapted to widely different uses. Thus the large-grained salt is peculiarly fitted for the packing of fish and other provisions, a purpose to which the small-grained salts are much less suitable. Their different powers, then, of preserving food must depend on some mechanical property; and the only obvious one is the magnitude of the crystals, and their degree of compactness and hardness. Quickness of solution, it is well known, is pretty nearly proportional, all other circumstances being equal to the quantity of surface exposed. And since the surfaces of cubes are as the squares of their sides, it should follow that a salt whose crystals are of a given magnitude will dissolve four times more slowly than one whose cubes have only half the size.

That kind of salt, then, which possesses most eminently the combined properties of hardness, compactness, and perfection of crystals, will be best adapted to the purpose of packing fish and other provisions, because it will remain permanently between the different layers, or will be very gradually dissolved by the fluids that exude from the provisions; thus furnishing a slow but constant supply of saturated brine. On the other hand, for the purpose of preparing the pickle, or of *striking* the meat, which is done by immersion in a saturated solution of salt, the smaller-grained varieties answer equally well; or, on account of their greater solubility, even better.

With the hardness or strong aggregation of the several varieties of salt, it seemed to me not improbable that their specific gravity might in some degree be connected. The exact determination of this property in saline substances is, however, a problem of considerable difficulty, as will sufficiently appear from the various results which have been given, with respect to the same salts, by different experimentalists. Thus Muschenbroek makes the specific gravity of artificial muriate of soda to vary from 1918 to 2148, the mean of which is 2033. Sir Isaac Newton states it at 2143, and Hassenfratz at 2200*. All that was necessary for my purpose was an approximation to the truth; and the introduction of a small error could be of no importance, provided it were the same in every case, since the comparison would still hold good.

The specific gravity of rock salt, there can be little difficulty in determining with precision. A piece of this salt †, of such perfect transparency that I had reserved it as a cabinet specimen, weighed in the air 513 grains, and lost, when weighed in alcohol, 194 grains. The alcohol, at the temperature of 56° Fahrenheit, had the specific gravity of 820, and hence that of the salt may be estimated at 2170. Another specimen considerably less pure, and more approaching to a fibrous fracture, had the specific gravity of 2125 only.

For ascertaining the specific weights of artificial varieties of salts, I used a very simple contrivance. It consisted of a glass globe about 3$\frac{1}{3}$ diameter, having a stem or neck 10 inches long. Sixteen cubic inches of water (each 252$\frac{1}{2}$ grains at 60° Fahrenheit,) filled the whole of the globe, and about half an inch of the lower part of the neck; and from the line where the water stood in the instrument, it was accurately graduated upwards into hundredth parts of a cubical inch. Into this vessel I poured exactly sixteen cubic inches of a perfectly saturated solution of common salt; and then added 400 grains of the salt under examination, washing down the particles that adhered to the neck by a portion of the liquid, which had been previously taken out of the globe for the purpose. As much as possible of the air which adhered to the salt was dislodged by agitation, and the increase of bulk was then observed.

Care was taken that the salts were all of equal temperature and dryness, and that no change of temperature happened during the experiment.

	Hundredths of a cub. in.	Hence its specific grav. was‡
400 grains of the less pure kind of rock salt, broken down into small fragments, filled the space of.........	75	2112
400 grains of stoved salt...........	75	2112
400 do. (another sample)	76	2084
400 do. common salt	76	2084
400 large-grained fishery salt	83	1909
400 do. (another sample)..........	83	1909
400 St. Ube's	82	1932

* *Annales de Chimie*, vol. xxviii. p. 13.
† Foliated rock salt of Jameson. See his Mineralogy, vol. ii. p. 10.
‡ Distilled water at 1000 being taken as the standard.

If

If the above mode of determination at all approach to correctness, it would appear that the specific gravity of rock salt is diminished, by being broken into small fragments, from 2125 to 2112, probably in consequence of the quantity of air which the fragments envelop, and which cannot be entirely separated by agitation. From the numbers given in the last column, it is evident that the smaller-grained salts are specifically heavier than those which are composed of larger and more perfect crystals. A difference of only one or two hundredth parts of a cubic inch is perhaps entitled, in a process of this kind, to little reliance; and I do not therefore regard it as indicating any material difference in the specific gravity of the first four or last three salts submitted to experiment. But when the difference amounts to eight hundredths, as between the small- and large-grained salt, it may safely be imputed to an inferior specific gravity in that species, which occupies so much greater a proportional bulk *.

The last series of experiments proves decisively, that in an important quality, (viz. that of specific gravity,) which is probably connected with the mechanical property of hardness and compactness of crystals, little or no difference is discoverable between the large-grained salt of British, and that of foreign manufacture. If no superiority, then, be claimed for British salt as applicable to œconomical purposes, on account of the greater degree of chemical purity which unquestionably belongs to it, it may safely, I believe, be asserted that the larger-grained varieties are, as to their mechanical properties, fully equal to the foreign bay salt. And the period, it may be hoped, is not far distant, when a prejudice (for such, from the result of this investigation, it appears to be,) will be done away, which has long proved injurious to the interests and prosperity of an important branch of British manufacture.

[To be continued.]

XIX. *Description of a Metallic Thermometer for indicating the higher Degrees of Temperature.*

To Mr. Tilloch.

Sir, I beg leave, through the medium of your Magazine, briefly to mention the principle of a *new thermometer*, con-

* M. Hassenfratz seems to have suspected that a difference in the specific gravity of the same salt may be occasioned by a variation in its state of crystallization. *De la Pesanteur spécifique des Sels, Ann. de Chim.* xxviii. p. 17.

contrived by me, for the purpose of exhibiting the difference in temperature, or degrees of heat, which takes place between the mercurial thermometer, the scale of which terminates upwards, at 600°, and that of baked clay, or Wedgwood's thermometer, the scale of which commences at 1077° of Fahrenheit, or red-heat, thus forming an intermediate or *connecting thermometer* between the two above mentioned.

A metallic composition is formed, not liable to alteration in its quality or quantity by repeated exposure to heat, the melting point of which is at a little below 600° of Fahrenheit, and its boiling point at 1200°. A case resembling in form the glass case for the ordinary thermometer, but somewhat larger, contains the metallic composition, and the scale consists in a slender *graduated rod*, equal in height at the commencement of the scale, that is when the metallic composition is just liquid to the top of the tube; the graduated rod terminating at the bottom in a thin, circular, flat plate, which rests or floats as it were upon the liquid metal; and in proportion as the latter expands and rises in the tube by heat, the graduated rod is buoyed up, or raised above the top of the tube, passing through a perforated cover to the *maximum*, or boiling point*.

The same principle, I might observe, admits of being extended, for the purpose of ascertaining the variation in temperature up to the most intense heat, perhaps, that can be required.

It is unnecessary to state here, that the influence of the incumbent atmosphere upon the surface of the liquid metal within the open tube is too inconsiderable, even at the commencement of the scale, to deserve notice, and at a higher temperature diminishes to nothing; especially if the whole of the liquid contained in the thermometer, *as ought to be the case in the use of every thermometer*, be completely immersed or subjected to the temperature, the degree of which it is intended to indicate.

A method similar to the above, I should think, might be applicable to the purpose of showing in a ready way the degree of expansion in metals by heat; but the elongation of a cylinder of any metal, by increase of temperature, is

* The thermometer case and graduated rod are at present formed of pipe-makers' clay previously prepared by having been exposed to a sufficient degree of heat.

The scale of this new thermometer is an exact continuation of the scale in the mercurial thermometer; the *lower degrees* of the former corresponding with, or indicating like temperatures with, the *upper degrees* of the mercurial thermometer.

much

much too small to admit of its being a *convenient measure* of temperature.

I should not despair, however, (availing myself of every advantage, viz. increasing the length of a metallic wire, by giving it a spiral form, in order to comprise a considerable length in small compass; with the application of the lever-index, and a good magnifier,) of constructing a thermometer upon this principle, so as to render the scale apparent even to single degrees; using silver for the lower temperatures, and platina for the higher, or employing iron wire, *only* up to its ultimate point of expansion in a solid state *.

I am, sir,
Your obedient servant.
RICHARD WALKER.

Queen-street, Oxford,
Aug. 6, 1810.

XX. *On Crystallography. By* M. HAUY. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 69.]

THEORY OF THE LAWS TO WHICH THE STRUCTURE OF CRYSTALS IS SUBJECTED.—GEOMETRICAL PART.

Preliminary Notions.

1. THE theory which I here propose to submit to calculation has for its object, to determine all the different forms which may arise from a superposition of decreasing laminæ following known directions and laws, on the various faces of a solid, the figure of which is also given †.

2. The solid which I call *nucleus* or *primitive form* is always one of the six following: 1st, the parallelopipedon; 2d, the regular hexahedral prism; 3d, the rhomboidal dodecahedron; 4th, the octahedron; 5th, the tetrahedron, which in this case is always regular; 6th, the bipyramidal dodecahedron.

3. By subdividing each of these solids parallel to its different faces, and sometimes also in other directions, we obtain the integrant molecules, which are always either parallelopipedons, triangular prisms, or tetrahedrons.

* For the means of rendering exceedingly minute divisions distinct, see a method described in the Monthly Magazine for May 1810.

† I presume that my readers are acquainted with that part of my treatise in which the same theory is detailed by simple reasoning. I shall therefore now confine myself to resuming in a succinct manner the most general principles of this theory.

4. When

4. When the nucleus being a parallelopipedon is divisible only by planes parallel to its six faces, it is evident that the integrant molecule is itself a parallelopipedon similar to this nucleus.

5. But even when the integrant molecules differ from the parallelopipedon, they are always situated in the interior of the nucleus, in such a manner that being taken by small groups they compose parallelopipedons; and the decrements which give the secondary forms are always made by rows of these parallelopipedons as in the case first mentioned.

I give the name of *subtractive molecules* to the small parallelopipedons, divisible or not divisible, the subtraction of which determines the decrements of the laminæ of superposition.

It follows from what has been said, that the subtractive molecule is a kind of unity, to which we may refer the structure of all crystals in general, so that we are at liberty to adhere to the data which it furnishes in the application of calculation to every possible crystalline form. To know afterwards if this unity be indivisible, or if it has fractional parts, is a matter of observation which may be interesting in natural history, but independent of which the theory would not admit of our proceeding towards the object in view.

6. In the case where the nucleus itself differs from the parallelopipedon, we may always substitute for it a solid of that form, either by abstracting from some of its faces, if there are more than six, or by multiplying the subdivisions always in the direction of the natural joints, if it be a tetrahedron. But we frequently obtain more simple results, by giving the preference to the true nucleus.

7. The decrements undergone by the laminæ of superposition may be effected in all imaginable directions. The limits of these directions are the edges and the diagonals of the faces of the nucleus. Between these two limits there is an infinity of intermediate ones, according as the small solids, the rows of which determine the quantity of the decrements, are considered as double, treble, quadruple, &c. of the subtractive molecule. I call *decrements on the edges* those which take place parallel to the edges of the faces of the nucleus; *decrements on the angles*, those which take place parallel to the diagonals; and *intermediary decrements*, those which are made parallel to lines comprehended between the edges and the diagonals.

I shall now successively treat of the different primitive forms above mentioned, and give, relatively to each of them, the

the method of calculating the results of all the laws of decrements of which it is susceptible. I shall begin with the parallelopipedon, which is as it were the term of comparison to which the other forms refer.

1. *Theory of the Parallelopipedon.*

8. Let AG (fig. 1, Pl. IX) be a parallelopipedon, the faces of which may have whatever respective dimensions and measurements of angles we please. Let us conceive this solid subdivided, by plans secting parallel to its different faces, into a multitude of elementary parallelopipedons which will be the integrant molecules. Each of the same faces will be separated in its turn into a certain number of small parallelograms, which will be the exterior faces of as many molecules.

If we choose any two of the six faces in question, provided they are opposite, we may consider the solid as an assemblage of laminæ distinguished by the secting plans parallel to these very faces.

9. Let us now imagine new laminæ formed of small parallelopipedons similar and equal to the foregoing, which are placed as if in steps above various faces of the generator parallelopipedon, in such a manner that the facets in contact coincide exactly, like what takes place in the interior of this solid. Here there are three cases to be distinguished. The first is that in which the laminæ extend by their edges so as to envelop completely the generator parallelopipedon, which will grow without changing its form. The second is that in which the laminæ would remain on a level by their edges with the faces adjacent to the generator parallelopipedon, in which case it is easy to see that they would form re entering angles at the places of the ridges DC, BC, CG, &c. In the third case, the laminæ will go on decreasing, following certain directions, in such a manner that each will be exceeded by the foregoing in a quantity equal to one or more rows, either in breadth or height.

Of these three cases, the first is relative to the primitive forms given immediately by crystallization, and admits of no difficulty. The second is foreign to our views, because nature presents us with no example of it in simple crystals. We shall dwell at some length upon the third, which is properly the object of the theory.

10. Let us conceive in the first place that the decrements are produced in breadth on all the ridges by subtraction of an equal number of rows, and let us confine ourselves for the

the moment to the consideration of the effect of the decrement which takes place parallel to the ridge BC, ascending above the parallelogram ABCD.

If we suppose that the form of the integrant molecule which is similar to the generator parallelopipedon is determined, and that the law of decrement is known, it will be easy to find the angle formed with ABCD by the face produced in virtue of this decrement.

Let *a g* (fig. 2) be one of the molecules, of which the faces analogous to those of the parallelopipedon, fig. 1, are marked with the same letters. From the point *c* I draw *c s* and *c r* perpendicular on *b c*. Now, by the hypothesis, the relation between these two lines is given, as well as the angle *r c s* which measures the incidence of *a b c d* upon *b c g h*.

Now let *o p* (fig. 1) be the distance between the ridge BC and the first lamina of superposition, which distance is regarded as being measured on the plane ABCD. It is clear that *o p* is equal to *c r* (fig. 2) multiplied by the number *n* of rows subtracted. Therefore $op = n \times cr$. From the point *p* (fig. 1) raise *p u* lying upon that of the lateral faces of the first lamina, which is turned from the same side, and equal to the height of this face. We shall have $pu = cs$ (fig. 2) and $opu = scr$. Complete the triangle *u p o* (fig. 1). It is visible that the line *o u* will coincide with the face of the secondary crystal, which rises on the ridge BC, and that the angle *p o u* will measure the incidence of this face on the parallelogram ABCD. Thus, since in the triangle *u p o* we know the two sides *o p*, *p u*, and the comprehended angle *o p u*, it will be easy to get the angle *p o u* which gives the incidence wanted.

11. The triangle *p o u* is called *mensurator triangle;* and I shall subsequently give this name to all the triangles which perform the same function.

12. Let us now consider the effect of the decrement which takes place parallel to the same ridge BC, by descending on the face BCGH. Let *o i h* be the mensurator triangle, in which *o i* is the distance between the ridge BC and the first lamina of superposition, *i h* coincides with that of the lateral faces of this lamina, which looks towards the ridge BC, and besides it is equal to the height of the face in question; finally, *o h* is laid on the face which results from the decrement.

Let n' be the number of rows subtracted. We shall have *o i* (fig. 1,) $= n' \times cs$ (fig. 2). Also *i h* (fig. 1) $= cr$ (fig.

(fig. 2), and $oih = rcs$. Thus it will be easy to determine the angle which the face produced by the decrement forms with BCGH (fig. 1)

13. It may happen that the two decrements which act on both sides of the ridge BC have such a connexion with each other that the two faces which will result from them will coincide upon one and the same plane, so that the side oh of the triangle oih is upon the direction of the side ou which belongs to the triangle upo, as we see in fig. 3. To prove this, we may remark, that in this case the triangles upo, oih are similar, as well on account of the equality of the angles opu, hio, and the parallelism of the sides op, ih, as on account of the coincidence of the sides ou, ho upon one and the same direction.

Therefore $pu : op :: oi : ih$.

Or rather cs (fig. 2) $: n \times cr :: n' \times cs : cr$.

Which gives $n' = \frac{1}{n}$.

That is to say, the case in question will happen every time that the decrements which take place on proceeding from BC towards GH are in the inverse ratio to those which take place in going from BC towards AD, or, what comes to the same thing, at all times when there is on one side a decrement in height equal to that which shall be on the opposite side. We may easily conceive that the two faces will be still on one and the same plane in the peculiar case of a decrement by one row on both sides.

14. Hence we may conclude, that in all circumstances similar to those which have been cited, we may make abstraction of one of the two decrements, by considering the face which results from it as the continuation of that which arises from the other decrement.

We see what would have been necessary for determining in a similar way the incidences of the faces produced by the other decrements upon the analogous faces of the generator parallelopipedon.

15. The greatest number of faces which the secondary solid can have, is twenty-four, since the generator parallelopipedon has twelve ridges, each of which is the line of departure of two decrements which act in an opposite direction. These faces will all be triangles, or some triangles and others trapeziums, according as the generator parallelopipedon will be found more elongated in one direction than in the other, or as the decrements which will take place parallel to certain edges will follow a more rapid course than those which would act parallel to other edges.

The

The smallest number of faces which the secondary crystal can have is twelve. Then all the decrements considered two by two, setting out from one and the same ridge, will be inverse to each other.

The simplest case is that in which the generator parallelopipedon being a cube, we have $n=1$ and $n'=1$. On this hypothesis, the secondary solid is a dodecahedron with rhombic planes all equal and similar, as we have explained in the reasoning part of the work.

16. We now proceed to the method of determining the decrements upon the angles. But we may previously remark, that in this case the decreasing parts of the laminæ of superposition form angles alternately re-entering and salient, in such a way, however, that all the ridges of molecule situated at the places of the salient angles are on one and the same plan: we shall consequently designate the series of these ridges by the name of *lateral face.*

This being done, let us conceive that decrements in breadth are produced by equal numbers of rows on all the angles of the parallelopipedon, fig. 1; and let us take for instance that which takes place upon the angle BCD. Let Ckl be the mensurator triangle, in which Ck measures the distance between the point C and the first lamina of superposition, kl is regarded as being applied on the corresponding lateral face, the height of which it measures, and Cl coincides with the face of the secondary crystal, produced by the decrement in question.

Having traced the diagonals db, fh (fig. 2) on the bases of the molecules, I draw ct perpendicular upon db, and xz perpendicular as well upon db as upon fh.

Let N be the number of rows subtracted. We shall have Ck (fig. 1) $= N \times ct$ (fig. 2), and kl (fig. 1) $= xz$ (fig. 2); besides the angle Ckl (fig. 1) will be equal to that formed by the plane $bdfh$ (fig. 2) with fgh. Now these three quantities are regarded as being known, since the form of the molecule is determined. Thus it will be easy to find the angle kCl (fig. 1) which measures the inclination of the face produced by the decrement upon the parallelogram ABCD. We shall conduct ourselves in the same manner in order to calculate the effects of the decrements on the other angles.

17. Let us now consider the hypothesis in which the decrements which take place on the two angles DCG, BCG would have such a connexion with that which acts on the angle BCD, that the faces produced by these three decrements would coincide on one and the same plane.

Let

Let AG (fig. 4) always be the generator parallelopipedon. Suppose that the decrement which takes place in breadth upon the angle BCD has such a measurement, that the lower edge of the first lamina of superposition passes by *m r*, in which case such of the lines C*m*, C*r* will contain as many ridges of molecule equal to *c d*, or *c b* (fig. 2), as there will be rows subtracted by the decrement. Having taken upon CG (fig. 4) a part C*c* equal to *c g* (fig. 2), make a plane pass by the points *m*, *c*, *r*. I say that this plane is parallel to the face which results from the decrement. In order to prove it, having drawn indefinitely the lines *m s* and *r u* parallel to CG, I prolong them each upwards, so as to have M*m*, or R*r* equal to C*c*. Now these prolongations M*m* and R*r* represent two of the ridges situated on the lateral face of the first lamina. Therefore the face produced by the decrement passes by the points M, R. But besides it passes by the point C, which is the term of departure of the decrement; therefore the plane MCR coincides with it. Now the small lines C*c*, M*m*, R*r*, being three longitudinal ridges of molecule, situated parallel to each other between the two planes *mcr*, MCR, it is visible that these two planes are themselves parallel, *i.e.* that *m c r* is parallel to the face which arises from the decrement.

The same reasoning applies to the hypothesis in which the decrement should take place in height. In this case it would be necessary, in order to make the plane *m c r* be parallel to the face produced, that we should have *c m* = *c d* (fig. 2); *cr* = *c b*; and that the line C*c* (fig. 4) should contain as many times *c g* (fig. 2) as there would be ranges subtracted in the direction of the height.

18. Let us suppose that the plane MCR is prolonged above the faces CDFG, BCGH, and consider the prolongations as two faces which would be the effect of two decrements, the one upon the angle DCG, the other upon BCG. These decrements being equal, we shall confine ourselves to that which acts upon the angle DCG. Since the plan *c m r* is parallel to the face which results from this decrement, it is clear that *c m* coincides with the lower edge of the first lamina of superposition applied upon CDFG, and that C*r* contains as many ridges of molecule as there are ranges subtracted in height.

19. If the decrement relative to the angle BCD takes place by one row, it is evident that the two other decrements relative the one to the angle DCG, the other to the angle BCG, will also take place by one row: since then the three lines C*m*, C*r*, C*c*, are equal each to one ridge of molecules,

molecules, the three decrements must necessarily have the same measurement.

20. But if the decrement relative to the angle BCD is produced by more than one row, then the two others will necessarily be intermediate, and it will be sufficient to have the law of the first decrement for determining the two others. Let us suppose, for example, that the decrement in the angle BCD is made by three ranges in breadth. In this case C*m* and C*r* will each of them be equal to three ridges of molecules, and C*c* will be equal to one ridge. Therefore the decrement on the angle DCG is produced in such a manner that there are three ridges of molecules subtracted in the direction of CD, upon one alone in the direction of CG; and besides this decrement is made by three rows in height, since C*r* answers to three ridges of molecules. It is the same with the decrement which takes place on the angle BCG.

21. In all cases of this description, the theory only considers the effect of the decrement which takes place according to the ordinary laws, because there results from it a much more simple solution; and the two other decrements, of which abstraction has been made, are considered as intervening in a subsidiary manner to second the effect of the first, and prolong towards the parts adjacent the face to which it has given birth.

22. The greatest number of faces which the secondary crystal can have, in the hypothesis of a decrement on all the angles, is twenty-four, since there are eight solid angles each composed of three plane angles, which are the terms of departure of as many decrements. The *minimum* of the number of faces in the same hypothesis is eight; and although strictly speaking there are always twenty-four decrements, we only consider eight, which gives us the facility of employing ordinary laws only, for determining the form of the secondary crystal.

23. The simplest case is that in which the generator parallelopipedon being a cube, all the decrements are done by one row. The secondary solid is then a regular octahedron.

But it may happen that the three decrements which take place around one and the same solid angle are all intermediary. In this case it is sufficient that one of them be determined, in order to render it easy to conclude from thence the two others, by the help of a construction similar to that which we have previously employed.

24. Let us suppose that fig. 5 represents the generator parallelopipedon, marked with the letters relative to the method

method of indicative signs. Let us conceive that there is made upon the angle O, ascending, a decrement which produces a face parallel to the plane *n r s*, and the expression of which is $(\overset{\frac{1}{2}}{O} D^3 F^4)$, whence it follows that $on = 3cd$ (fig. 2), $Or = 4cb$, and $Os = 2cg$.

This being done, the expression of the decrement on the left of the angle O will be $({}^{\frac{1}{4}}O D^3 H^2)$, and that of the decrement to the right of the same angle $(O^{\frac{1}{3}} F^4 H^2)$.

25. In order to determine the angles formed by the faces produced by the intermediary decrements with the corresponding faces of the nucleus, what presents itself as the simplest is to consider every little group of molecules, which results from the decrement, as forming one single molecule; which brings back the calculation to that which is employed for the ordinary decrements on the angles.

Let us take for example the decrement on the angle O ascending, represented $(\overset{\frac{1}{2}}{O} D^3 F^4)$. It is easy to judge that in this case, the group which represents two subtractive molecules placed the one above the other, is that which we see fig. 6, and in which the side mn is composed of three ridges of molecule, the side np of four ridges, and the side nk of two ridges, on account of the decrement by two ranges in height.

Having traced on the bases the diagonals mp, io, I draw vt perpendicular upon mp, then us perpendicular as well upon mp as upon io.

Let nty (fig. 7) be the mensurator triangle, in which nt being regarded as lying on the plane AEOI (fig. 5) will be equal to the same line (fig. 6). Besides, we shall have ty (fig. 7) $= us$ (fig. 6), and the angle nty (fig. 7) will be equal to that formed by the plane $mpoi$ (fig. 6) with the triangle iko. Thus it will be easy to find the angle ynt (fig. 7) which measures the inclination wanted.

26. The solutions of problems of this kind are often simplified in practice, by a series of the regular form of molecules. Let us suppose, for example, that the latter are cubes. Let us designate each of their ridges by unity. We shall have (fig. 6) $mn = 3$, $np = 4$, $nk = 2$, $mp = \sqrt{(mn)^2 + (np)^2} = \sqrt{25} = 5$. $nt = \frac{mn \times np}{mp} = \frac{12}{5}$. $us = nk = 2$.

Thus also nt (fig. 7) $= \frac{12}{5}$, $ty = 2$.

Thus $n\,t : t\,y :: \frac{12}{5} : 2 :: 6 : 5$. Besides, in this same case, the angle $n\,t\,y$ is straight; from which we see how easy it is to find the angle $y\,n\,t$.

27. The mensurator triangles relative to the decrements on the angles may be substituted for those which we have considered in the decrements on the edges, and serve equally well for determining the secondary forms. Let us suppose, for example, that AG (fig. 8) represents a cubical nucleus, which undergoes decrements by two ranges on the four edges of the base ABCD, and that we wish to know the angles of the pyramid SADCB produced by this decrement. Having traced the diagonals BD, AC, I draw from their point *o* of intersection the line *op* perpendicular upon CD, then *sp*. If I take upon *po* the part *pr* equal to two ridges of molecule, and from the point *r* I raise *ru* perpendicular upon ABCD, and which by the hypothesis will be found equal to one ridge of molecule, the triangle *upr* will perform the function of the ordinary mensurator triangle, and by means of the right angle *urp*, and of the relation 2 : 1 between the sides *pr* and *ur*, we shall easily find the incidence of DSC upon the base ABCD, as well as the values of the other angles. For, on account of the similar triangles *upr*, *spo*, every thing is reduced to the calculation of the angles of a straight pyramid in which the side BC of the base, which is double of *po*, is to the axis *os* in the relation of 4 to 1.

On the other hand,—If I take upon C*o* the part C*n* equal to two diagonals of molecule; and if from the point *n* I raise *nz* perpendicular upon ABCD, C*n* will represent the distance from the point C to the first lamina of superposition, taken in the direction of C*o*, and *nz* will be equal to one ridge of molecule; from which it follows that the triangle *z*C*n* may also perform the function of mensurator triangle.

We shall therefore have $Cn : nz :: 2\sqrt{2} : 1$, and because the triangle *z*C*n* is similar to the triangle *s*C*o*, the question considered under this new point of view will be reduced to seek the angles of a straight pyramid, in which the demi-diagonal C*o* of the base is to the axis *os*, as $2\sqrt{2} : 1$, which is sufficient for having all the rest. We shall have occasion more than once thus to substitute one mensurator triangle for the other, when there will result from it more facility in resolving the problems.

All the details upon which we have acted ought to be regarded

regarded as preliminary notions intended particularly to enable students clearly to understand the use of the mensurator triangles which will incessantly recur in the applications which we shall make of the calculus to the laws of decrements. We shall now proceed more particularly to the methods relative to this object; and as the rhomboid, which likewise comprehends the cube, is of all the kinds of parallelopipedon the most fertile in diversified results, and at the same time that which is the most easily adapted to the employment of general formulæ, we shall first give the theory of this solid; after which we shall resume that of the parallelopipedons of a different form.

*** Understanding that an English Translation of the whole of Mr. Haüy's valuable work on Crystallography is now preparing for the press, we intend, for the present, to suspend our labours upon it, as a full translation cannot fail to answer the object we had in view, better than those disjoined portions which can alone be admissible into the pages of a periodical work. —Should the proposed translation not appear in some reasonable time, we may hereafter resume our labours.

XXI. *Dr.* Healy *on Cupping.*

To Mr. Tilloch.

No. 1, Clarendon-street, Dublin.

Sir, I request you will permit me to contradict an observation which has been made, in the Retrospect of Discoveries, stating that the mode which I propose for cupping without the assistance of the syringe, is so far from new, that it occurred nearly 2000 years ago, to Hero of Alexandria, and that the figure is exhibited in the *Mathematici Veteres.*—I consulted the Parisian edition, and find his contrivance (as described page 207) entirely different from mine. Suffice it to say, a partial exhaustion is produced by the mouth from a secondary cavity, and two stopcocks are made use of. The syringe, which is an improvement, and answers for the secondary cavity of Hero, is the usual mode at present of producing the vacuum, and not, as the observer states, the spirit lamp or tow. The apparatus which I propose will still, I imagine, be found new, more œconomical, and less complicated, than any that has been hitherto adopted.

Your much obliged,

July 19, 1810.

Robert Healy, M.B.

 XXII. *Obser-*

XXII. *Observations on the Purity of Standard Gold. By* M. FABBRONI, *of Florence, corresponding Member of the French Institute. To which are subjoined Notes by* M. D'ARCET, *Assay Master of the French Mint**.

ALMOST all naturalists (following perhaps implicitly the assertion of Pliny†) maintain that native gold is never found pure; *i. e.* entirely free from alloy, chiefly of silver, and that the finest is scarcely from 0·875 to 0·917, or 21 or 22 carats. The gold in dust, in spangles, or in sand, which is brought from Africa, is most frequently within these limits. I have seen some gold from the country of Bambuck in Africa, which was 0·927, or 22 carats and one-fourth; in the mint of Florence it has also been seen at 0·958, or 23 carats: this gold had been brought from Morocco. The carat in Tuscany is divided into eighths.

It is probable that in the early ages money was coined with native gold, in the state in which it was found, there being no grounds for supposing that they took pains to refine it.

It has been thought that the oldest gold coin known is that of Battus IV, which was melted or struck at Cyrene in Africa in the time of Pisistratus: it does not appear that the standard of this gold was known. Of all the Grecian coins which are in the hands of the collectors of medals, the most ancient are the beautiful pieces of Philip the father of Alexander. That enterprising man, who from his infancy conceived the idea of ascending the throne of Macedon, and becoming master of Greece, was fortunate enough to find several rich mines of gold, which he knew how to prize. Mount Pangea annually furnished him with gold to the amount of 5,229,000 francs, and from thence he derived the most powerful resources for the success of his political designs and military talents. It is not known whether this metal of Philip's underwent any particular operations before passing to the mint. There are some

* *Annales de Chimie*, tome lxxii. p. 25.

† Pliny says, lib. 33, that there is no kind of gold more perfect than spangle gold; that gold obtained by searching the beds of rivers does not require melting, and that it is native and perfect gold. But Pliny says in the same book that lead is more malleable and heavier than gold; which proves that the gold which he regarded as pure was in reality an alloy. He says likewise a little further on, that all gold is mixed with silver, and that the gold which is the least alloyed with silver came from Albicratum in Gaul, and that it contained only $\frac{1}{30}$. From all this it is evident that Pliny's opinion is not to be followed, and recourse must be had to experiments.

grounds,

grounds, however, for thinking that it was used in the state in which it was found.

Patin assayed a *statera* of gold of this king (the denomination of his coin among the Persians and Macedonians), and found it to be 23 carats and a half, or 0·970. We cannot allow ourselves to believe that the metallurgists of that monarch thought to purify gold by adding to it only a 48th part of alloy, but we can easily suppose that the gold was found in this state of fineness.

If alloys were added to gold from a bad design, or with the mistaken idea of covering the expense of manufacturing it; this has degenerated into fraud, and has no limits; if the alloy was added with the view of making the money harder, it was a futile attempt. Neither of these motives could sway with Philip, because he enjoyed abundant mines of gold, and because, as he wished to appear generous, he would have made his coin of pure gold, if he thought it necessary to refine it: or he would have added more alloy, if policy suggested that he should not employ it as it came from the bowels of the earth *. It should seem, therefore, that his mines furnished him with gold at 23 carats and a half (0·979), as it is found to be in his coins, if there be no error in Patin's analysis: but it might perhaps be interesting to confirm this fact by a new experiment.

Chevalier Fossombroni, an eminent mathematician, in digging the foundation of a house near Arezzo, found a statera of Philip in good preservation, which he was kind enough immediately to give us to be examined by analysis.

On one side of this piece, as in most of Philip's coins, there is a head of Apollo, and on the reverse a car with two horses: the name is on the exergue:—on similar coins we see under the legs of the horse a monogram or type indicating the mint where the coin was struck. On the piece in question there was a trident, which means Trœzene.

Fourteen of these coins are preserved in the cabinet of the Florence gallery: the face and the reverse of eleven of them are similar to that of Arezzo; but they have various distinguishing marks, one only bearing the same mint-mark

* I transmitted to M. Mongez the analysis of an ancient coin with the effigy of Philip: its examination also proved, that under the reign of that prince alloys were used in the making of money, the composition of which was natural, or at least unknown; for it contained silver 368, gold 184, copper 448.

It is not likely that so complex an alloy would have been used at a period when the modes of analysis were so little known, as to fall far short of the degree of exactness which may be attained even by employing the touchstone and prepared acid now in use.

with that found at Arezzo. The weight of two of these, completely similar in external appearance, was precisely 176 grains of Florence each. The same weight was found in another distinguished by a monogram formed by a large K, and a little o; the same weight in another which had a thunderbolt; the same in another with a vase; and lastly, in another with a grain of wheat, the mark of the Leontini. These six weights of the largest staterae which remain, and which are equal, gave grounds for concluding that the above was the weight prescribed for the Greek money. This being granted, we may infer that the drachma weighed 88 grains. (Romé de l'Isle assigns three grains more to the great Attic drachma.) The proof of the accuracy of this calculation is to be found in the Athenian demi-drachma, or Asiatic drachma, or the fourth part of the statera of Philip, which is preserved in the same gallery: this fragment weighs precisely 44 grains. The face of this small piece of gold presents the head of Hercules covered with a lion's skin, and on the reverse we see the bow, the vase, and club. M. Millin has communicated to me the weight of five staterae preserved in the Imperial library, which are as follow: No. 1. 160·5 grains; No. 2. 161 grains precisely; No. 3. 161 grains; No. 4. 162 grains precisely; No. 5. 162 grains. The two heaviest seem to be so from having been less worn. The largest would answer to 175·16 grains of Florence, and would be lighter by gr. 0·84 than ours, which ought therefore to be regarded as less worn and more precise.

Mr. Greaves, in England, weighed two staterae of Alexander, one of which was 133 English grains, and the other 133 and a half. He thought that the half-grain had been wasted by friction, and he concluded that the drachma ought to be reputed as being precisely at 67 grains. The second weight as given by Mr. Greaves would be equivalent to 87 grains and six-tenths of Florence. Snellius found the statera of Philip and of Alexander to weigh 179 Dutch grains, which makes 124 and a half English; and this, according to the foregoing comparison, would give to the drachma in Florence weight 87 grains 0·9; all of them being a little lighter, but closely approaching that which we had fixed at 88 grains.

Barthelemy, in France, found after various weighings that the drachma was precisely 81 French grains and an eighth: now, by the foregoing comparison, we ought to have for the drachma in Florence weight 87 grains and three-fourths. But this last author wishes to suppose a friction

friction of seven-eighths of a grain for 2,200 years wearing, and he gratuitously makes the drachma to be 82 whole French grains, which would make 88 and a half of Florence weight. It is best to banish entirely from our calculations all suppositions of friction, because, by admitting this to have been the case, we might draw a variety of vague conclusions. The weight of 88 grains which we have assigned to the gold drachma is confirmed by a silver one of this very Philip, also preserved in the Florence cabinet: this piece has on its face the head of Hercules without a beard, covered with a lion's skin; and on the reverse a Jupiter sitting, holding the eagle in his right hand and a spear in his left. It is distinguished from the others by a lyre, and the letter A under the seat. This drachma is also a proof of the exactness of its weight in its half, also in silver, of the same king, which weighs precisely 44 grains: on its face is the head of Jupiter with the diadem; on the reverse is a figure on horseback, with the name on the exergue and a mark which is unintelligible. Besides, there are four pieces of four drachmas of Alexander of the same metal, the face and reverse of which are similar: all of these weigh 14 pennyweights and 16 grains, and prove completely that the weight of the drachma is 88 grains. These tetradrachmas are distinguished in the type by the addition of various signs, as we have said with respect to the stateræ: one has in front a lamp, and under the seat a moon and a star: another has in front the letter T with a circumflex accent, and under the seat the letter E: another has in front a buckler, and under the seat a serpent: the fourth has in front a crown, and under the seat a monogram composed of M. Finally, we have also a real drachma of this king of the precise weight of 88 grains, and which is distinguished by a monogram formed by an H, the cross bar of which has a kind of circumflex accent.

Among the tetradrachmas of Thrace there is one in the Florence collection, and the twelfth of the list, heavier than the rest; it weighs precisely 14 pennyweights and 16 grains: here we have a proof of the identity of the weights of the Thracians and Macedonians, as long ago supposed by the learned.

After the weighing of the Arezzo coin was finished, it was submitted to the cupel and to quartation. The standard was found to be the same with Patin's examination, *i.e.* 0·979, or 23 carats and a half: it contained only half a carat, or 0.021, of silver.

The art of assaying was known in the earliest times, as

 attested

attested by the Holy Scripture: we find it at such adegree of perfection even in the time of Pliny*, that by means of it the standard of gold was fixed at 21 carats, or 0·875, at 21 carats and 7-24ths (0·888) up to 23 carats and 11-32ds (0·973). In those days they must have assayed in the dry way, first by separating from the gold the viler metals by means of lead, and afterwards even the silver, with sulphur or with the sulphurets †.

They were also acquainted with the method of refining and purifying gold in large quantities, by cementing or burning it, as Strabo informs us, with an aluminous earth, which by destroying the silver left the gold in a state of purity. Pliny says that for this purpose they put the gold on the fire in an earthen vessel, with treble its weight of salt; that it was afterwards again exposed to the fire with two parts of salt and one of argillaceous *schistus*: this would surely effect the decomposition of the salt, and the volatilization of the muriatic acid in a state of ignition and dry, which would penetrate the substance of the gold, and would separate the silver in the form of volatile muriate: this is the object of the process of cementation among the moderns ‡. But Agatharchides has transmitted to us a peculiar method practised in the mines situated between the Nile and the shores of the Red Sea, in which we recognise the well-known property of the muriatic acid in the separation of silver.

This writer says (if we may trust to the text being accurate) that in these places gold is inclosed in marble; that the miners burn or calcine the ore; that they break it with

* The art of assaying was certainly very imperfect at these periods: under the emperors the standard of gold and silver was still judged of by the colour assumed by the coin in the fire, and by the tint given to the touchstone on which the metal was rubbed.

These methods, although practised by expert workmen, could yield but very inaccurate results, which a variety of circumstances might influence; such as a complexity in the alloy, or a different alloy, &c.

Archimedes would not have applied the laws of specific gravity to the determination of the standard value of king Hiero's crown, if he had known a better method.

We know also that under the triumvirate of Mark Antony every street of Rome erected a massive statue to Marcus Gratianus, who had discovered and put in practice one of the processes of assaying mentioned above: this denotes the infancy of an useful art, the first steps of which were strongly encouraged as being intimately connected with public happiness.

† By employing the alkaline sulphurets, the solution of gold might have been effected: it is the metallic sulphurets, however, which ought to be used in this process.

‡ M. de Robilant, in his detail of the processes of the mint at Turin, says that cementation is the mode of refining generally adopted at Venice, Genoa, and at Florence, where they make gold sequins almost entirely pure.

hammers,

hammers, afterwards pound it, bruise it, and wash it; and finally, that the gold, when put into a close crucible, with a little lead, salt, a little tin, and barley meal, was exposed over the fire for five days.

The money-coiners of Darius certainly employed this method, or a similar one, when this enlightened king wished to give his subjects the noble and useful example of a mint made with the purest gold, similar to that of fine silver, which his satrap Ariander afterwards did.

To conclude:—It is not easy to form an intelligible idea of the docimastic method, which Agatharchides has transmitted to us. But if in the operation which he describes there is no mention made of *cementation*, but of a true and prolonged fusion, it remains to explain how we can reconcile with the object in view, the employment of a close crucible held over the fire, as he describes: far less can we comprehend the use of the barley meal.

When we reflect, however, on the ingenious method described by Hellot as being practised at Lyons, in order to refine, purify, and separate cupelled silver from the small quantity of lead which adheres to it after the first refining, we may perhaps comprehend what is meant *.

In Lyons they use crucibles thirteen inches high, and five broad at their orifice. About three inches deep of pounded charcoal is then put into the crucible and kept down by a lid, or rather a triangular piece of the crucible, which is kept in its place. On this lid or false bottom they put 60 or 65 pounds of long and thin ingots, in order to be melted and purified. The wind furnace employed for this purpose is 14 inches high, seven in diameter at the grate, and nine at the top. The metal in melting was observed to fall three inches from the edges of the crucible: *then, when it had acquired a sufficient degree of heat, it was seen to boil with the force and the agitation of water exposed to the heat of a strong fire;* and in this state it was kept for seven or eight hours.

The elastic fluid, which in this case is extricated from the charcoal at the bottom of the crucible, produces the agitation above alluded to; and it forms, as it were, a kind

* It would be necessary, before deciding finally on the process detailed by Agatharchides and that which is practised at Lyons, to repeat them, taking the greatest care to apply the modern methods of chemical analysis, and above all the pneumato-chemical apparatus: it would be necessary to determine the nature of the gas, which passes through the melted silver, and ascertain why the gas is formed under a certain pressure, and why it does not pass out through the pores of the crucible. The experiment of M. Fabbroni does not seem to me to be conclusive.

of

of bellows ingeniously placed at the bottom of the crucible.

We know that charcoal, when put into close vessels of metal or glass, is not altered, although it becomes red. Theory dictates this, and several experiments confirm it. But the observation of the fact related by the judicious Hellot also proves, that in this case the charcoal below the melted silver is decomposed, and continually furnishes elastic fluid; since this excellent chemist found that silver kept over a similar fire, without charcoal being placed below it, undulated at the surface, going from the centre to the edges, and *vice versâ*; but that in fact it does not bubble with so much noise: from whence then does the elastic fluid originate in this case?

Priestley, the founder of the modern pneumatic chemistry, demonstrates in the plainest manner, what has been since confirmed by several other experiments, that earthen vessels heated until they admit light to pass through them, are filters, or rather sieves, which allow even the external air to enter; that caloric and light penetrate by the bottom of the crucible, and with them air, attracted chemically by the charcoal which is inside: its oxygen coming in contact with the charcoal, which is in a state of incandescence, inflames a portion of it, is combined with itself and with the caloric, forming carbonic acid, an elastic fluid, which, by the uninterrupted action of the fire, augments and acquires a sufficient elasticity to overcome the pressure of a column of seven inches of liquid silver, which is above, and passes through it agitating it violently. The small residue of lead, which is united with and diffused over the mass, being put by a continual agitation in contact with the carbonic acid gas, and with the atmosphere (the latter and perhaps the former are decomposed by a greater affinity under certain circumstances), is oxidated, and by the diminution of specific gravity is constrained to occupy the upper surface.

In fact, Hellot saw a kind of yellowish oil rise from the inside of the silver which floated on the top of the crucible; this oil was a pure oxide of lead melted and formed by the contact of the atmospheric air, which is continually renewed. The refiners collect this melted oxide by absorbing it with glass or with sour earth; this earth being most easily removed from off the silver which it covers, and the metal then remains limpid and pure.

On comparing this method with the process of Agatharchides, reported here so imperfectly, we may suppose that the

the barley, or the barley meal, was used instead of charcoal, in order to form what the Lyonnese call the soul of the crucible, which was placed at the bottom of it, where it was retained by a covering (from which probably comes the expression of *closed crucible*), on which the gold in fusion was placed by means of a little lead (in order to vitrify the base metals which it might contain,) and some common salt, sulphuret of antimony or of lead, in order that they may lay hold of the fine silver and volatilize it with the lead, or reduce it to scoriæ. The elastic fluids, extricated from the vegetable matter by the action of the fire, form the office of bellows for incessantly agitating the metal during several days, which makes all the impurities swim above, and which ought to be skimmed off as the Lyonnese do.

Properly speaking, a fire which lasts five days gives rather an idea of the cementation of the moderns, and analogous to that which Pliny has communicated to us, than a real fusion in close crucibles,—a circumstance which would be directly contrary to the object in view. Thus, in Hungary, in order the better to open all the interior parts of the gold to the muriatic acid reduced into vapour in the cementation, they are accustomed to add lead to the mass, which is afterwards reduced into small hollow drops, or, in other words, into grains. It may be that the lead indicated by Agatharchides has the same object: the tin may have been taken by an equivoque for crude antimony, for galenum, or for the native sulphuret of lead: it is possible likewise that the barley meal was merely intended to serve for the equal distribution of the little salt, a stratum of which must be placed on the gold, and perhaps assisted to decompose it, as argil or sulphate of iron now does.

In order to obtain some light upon this curious subject, there were put into a crucible covered by another crucible turned upside down, thirty pennyweights of barley-meal, and an ounce of common salt: it was then made red-hot, and kept 36 hours in this state. There was put in, more for the sake of curiosity than any thing else, a thin piece of gold weighing 24 grains, and a piece of silver weighing 40 grains. The lower crucible was half full, and an opening was left at the joining of the two crucibles to let out the elastic vapour.

After this space of time, the apparatus being cooled was opened, and there was found a small earthy residue slightly saline and whitish, weighing eleven grains and a half. The gold was above; it was increased one eighth of a grain in weight,

weight, because it was evidently whitened by the fusion of very minute particles of silver detached from the small fragment of silver, which was then found adhering immediately over the gold, in the form of agglutinated dust: this fragment of silver weighed six grains and one-eighth. We afterwards boiled this gold (which was silvered only on its surface) for some time in pure nitric acid; when it lost completely its silver colour; and when assayed it was found to be 24 carats.

We proceeded afterwards to examine the small earthy residue, in which we found, in saline particles, only a few atoms of muriate of soda, and scarce a trace of muriate of copper. The muriate of silver, which, on account of the loss suffered by the piece of silver, ought to have formed a weight of 45 grains and a half, was certainly evaporated with the other elastic vapours. Eleven grains and a half only of muriatic acid concurred in the formation of this muriate: the thirteen pennyweights and a half of the same acid, which besides contained the common salts employed in this experiment, have therefore been dissipated (by not paying attention to the little copper) by a decomposition effected by means of the vegetable matter which was joined with it: but what is difficult to account for, and which is foreign to our object, is the evaporation of ten whole pennyweights of soda, contained in the common salt, and which ought to have remained fixed at the bottom of the vessel: it had therefore become volatile, either by decomposition, or by a new composition, and it had escaped by the aperture in the apparatus.

It is not probable, therefore, that Philip made use of similar methods of refining, either by flux or by cementation, because, we repeat, he would have reduced the gold to a state of perfect purity, as Darius wished to do subsequently; or he would not have limited himself to so small a portion of alloy, or perhaps this alloy would not have been silver. And if he employed gold in the state in which he found it, we must be forced to admit that nature furnished it at 23 carats and a half fine, or of the standard of (0·979*).

Many

* Reaumur says (Mémoires de l'Académie des Sciences 1718, p. 87,) that the gold brought from the bed of the river Ceze is at the standard of 18 carats eight grains.

Gold of the Rhone	..	..	..	20 carats.
of the Rhine	..	..	..	21¼
of the Arriege	..	..	..	22¼

Reaumur also observes, that the standard varies in the same piece of native

Many persons will probably doubt that gold is found in nature so near a state of perfect purity, although Strabo intimates that perfectly pure gold was found in the Alps, and Pliny is cited as asserting that no silver was ever found in it. But without remaining in suspense with respect to the assertions and opinions of others, I am enabled from my own experiments to remove all uncertainty, having ascertained that native gold is 24 carats (1000).

I had for some time the charge of the fine collection of natural history belonging to our sovereign, who was fond of that science. His majesty possessed many specimens of mineralized gold and native gold, among which I remarked two well formed crystals of gold, viz. one cubical, the other prismatic with four faces, surmounted by a pyramid with four faces also. It would be interesting to know what substances united to the gold have determined these various figures formed naturally in the bowels of the earth, and which are totally different from those which are produced in the laboratories of the chemist after melted gold is cooled. The cube is very pale, the prism is higher coloured; but these two crystals, which I found by chance, (when choosing among several natural grains,) are unique in this depôt; so that it would be improper to subject them to an examination which would alter their form. An unshapen specimen, but at the same time a remarkable one, from the Brazils, enriches this collection. The weight of this piece is about fourteen pounds; and I examined a bit of it by the cupel and by quartation, without omitting also to examine its solution in the nitro-muriatic acid, with the sulphate of iron, and with neutral salts with a base of potash. I have been convinced by all these operations, that this was pure gold of 24 carats without any alloy of inferior metal.

From all that I can learn, therefore, it appears that gold is found in a native state in various degrees of purity, and to prove this has been the object of the present dissertation.

tive gold. M. Deluc informs us that the gold found at Wicklow in Ireland was alloyed with a ninth part of its weight in silver.

M. Fabbroni is the first who has demonstrated that native gold is found in a state of purity: this is a most important observation; but it does not seem to destroy the idea that native gold is a natural alloy of gold and silver: a principle established by a great number of facts, and to which we as yet know but one exception.

It would be interesting to ascertain whether lead is present in ancient coins or medals: this would be the surest method of determining whether the ancients refined their gold, or employed it as nature presented it.

XXIII. *An Estimation of the Loss of Weight which takes place in cooking Animal Food.*

It is well known that, in whatever way the flesh of animals is prepared for food, a considerable diminution takes place in its weight. We do not recollect, however, to have seen any where a statement of the loss which meat sustains in the various culinary processes, although it is pretty obvious that a series of experiments on this subject would not be without their use in domestic œconomy.

We shall here give the result of a series of experiments which were actually made on this subject in a public establishment, premising that, as they were not undertaken from mere curiosity, but, on the contrary, to serve a purpose of practical utility, absolute accuracy was not attended to. Considering, however, the large quantities of provisions which were actually examined, it is presumed that the results may be safely depended upon for any practical purpose. It would no doubt have been desirable to have known not only the whole diminution of weight, but also the parts which were separated from the meat in the form of aqueous vapour, jelly, fat, &c., but the determination of these did not fall within the scope of the inquiry.

	lbs.	*ozs.*
28 pieces of beef weighing	280	0
Lost in boiling	73	14

Hence the weight lost by beef in boiling was in this case about 26½lbs. in 100lbs.

	lbs.	*ozs.*
19 pieces of beef weighing	190	0
Lost in roasting	61	2

The weight lost by beef in roasting appears to be 32 per cent.

	lbs.	*ozs.*
9 pieces of beef weighing	90	0
Lost in baking	27	0

Weight lost by beef in baking, 30 per cent.

	lbs.	*ozs.*
27 legs of mutton weighing	260	0
Lost in boiling, and by having the shank-bone taken off	62	4
The shank-bones were estimated at four ounces each; therefore the loss by boiling was	55	8

The loss of weight in legs of mutton, in boiling, is 21⅓ per cent.

35 shoulders

	lbs.	ozs.
35 shoulders of mutton weighing	350	0
Lost in roasting	109	10

The loss of weight in shoulders of mutton, by roasting, is about $31\frac{1}{3}$ per cent.

	lbs.	ozs.
16 loins of mutton weighing	141	0
Lost in roasting	49	14

Hence loins of mutton lose, by roasting, about $35\frac{1}{2}$ per cent.

	lbs.	ozs.
10 necks of mutton weighing	100	0
Lost in roasting	32	6

The loss of necks of mutton, by roasting, is about $32\frac{1}{3}$ per cent.

We shall only draw two practical inferences from the foregoing statement :—1st. In respect of œconomy, it is more profitable to boil meat than to roast it. 2dly, Whether we roast or boil meat, it loses, by being cooked, from one-fifth to one-third of its whole weight.

XXIV. *Letter from M.* Vitalis, *Professor of Chemistry at Rouen, to M.* Bouillon Lagrange, *on the Amalgam of Mercury and Silver, called* Arbor Dianæ *.

The process described by Baumé, and which is generally adopted for obtaining the amalgam of mercury and silver, known in chemistry by the name of *arbor Dianæ*, is not the only one which is capable of exhibiting the beautiful crystalline forms which characterize this curious production. I attained the same object, by modifying the common method; and this modification admits of our extracting with facility the metallic vegetation from the liquor, and preserving it, without any alteration, out of the vessel in which it has been formed.

The operation is very simple. In the nitric solutions of mercury and silver, both being well saturated and diluted with the quantity of water prescribed by Baumé, I suspend a small knot of fine linen rag doubled up, and containing five or six drachms of very pure mercury.

The metallic solutions soon penetrate to the mercury, which is inclosed in the rag, and we soon see some beautiful spiculæ formed and grouped around the rag, adhering to the nucleus of mercury, which serves as a kind of support to them.

* *Annales de Chimie*, tome lxxii. p. 93.

These

These spiculæ progressively increase in size, and in a short time exceed an inch in length.

When we perceive that the metallic vegetation makes no more progress, the piece of rag with the spiculæ may be withdrawn from the liquor, and by means of a silk thread fastened to the cork of the bottle, the whole may be suspended under a bell glass. The crystals, which are tetrahedrons, may thus be kept as long as wanted.

I have in my own laboratory a crystallization of this description, which has preserved all its original beauty for these two years past.

It may be easily seen that, in the above process, the play of attractions is a little different from the common method as developed by M. Fourcroy in *Systême des Connoissances chimiques*.

The solidity of the metallic crystals obtained by my method, compared with the softness of the threads, the assemblage of which forms the common *arbor Dianæ*, led me to think that the proportions of mercury and silver are not the same in both cases; and I would have endeavoured to have ascertained the difference, if M. Vauquelin, to whom I communicated the circumstance, had not intimated his intention of taking up the subject at full length, and publishing his experiments in some future number of the *Annales de Chimie*.

The configuration of the above crystals also suggests some interesting inquiries, which I may probably be able to accomplish at a future time.

XXV. *Analysis of the Atropa Belladonna. By M.* Vauquelin *.

The experiments which I am about to detail were made with a view to ascertain whether this plant, which is of the same family with the tobacco plants, contained the acrid principle which has been found in the latter, but which, as will be shown in the sequel, it does not.

1. The expressed and filtered juice of the belladonna has a dark brown colour, with a bitter and nauseous taste. It is freely coagulable by heat, and by the aqueous infusion of gall nuts.

2. The substance coagulated by fire in the juice of belladonna is of a yellowish gray, becomes black on desiccation,

* *Annales de Chimie*, tome lxxii. p. 53.

and

and presents a smooth and polished fracture like that of the resins. It burns with decrepitation, becomes soft, and gives out vapours of the smell produced by horn when subjected to the same operation.

3. The juice of belladonna, distilled until reduced to the consistence of liquid extract, only furnished a water which had a fetid, herbaceous taste, and by no means the acerbity of that of tobacco. The only re-agent among all those resorted to, which slightly disturbed it, was acetate of lead.

4. The juice concentrated to the consistence of extract having been treated by alcohol, a part was dissolved: the solution deposited upon cooling, crystals of nitrate of potash and a little muriate of potash.

The alcohol separated from these crystals of nitrate of potash, and evaporated, left as a residue a brownish yellow matter of an extremely bitter and nauseous taste, which, on being taken up again by alcohol well dephlegmated, left a new quantity of insoluble matter, and also deposited some crystals of the same salt.

The matter cleansed as much as possible by the above process, from the greatest part of the saltpetre and from the substance insoluble in alcohol, I evaporated the latter, and submitted the residue to the following experiments:

1. It is dissolved abundantly and speedily in water, and it is even deliquescent in the air.

2. The solution is of a yellowish brown; it has a very bitter and disagreeable taste.

3. It reddens in a very intense manner turnsole paper.

4. It is precipitated in abundance by the alcoholic solution of gall nuts, and is not so by the acetate of lead when the latter is sufficiently diluted in water; because, as this matter contains a little muriate of potash, it would precipitate the acetate of lead without this precaution.

5. This solution mixed with sulphuric acid diffused a very sensible smell of acetic acid.

6. The same solution is precipitated by the nitrate of silver in a true muriate of silver.

7. Caustic potash develops in the solution of this substance a fetid smell, very like that of an old ley which has passed over linen and begins to turn putrid: ammoniacal vapours also rise, which may be made perceptible by weak nitric acid, presented at some distance from the mixture.

8. The addition of some drops of sulphate of iron gives a much deeper colour to the solution.

9. The extract itself exposed to burning coals, bubbles up,

up, and exhales pungent acrid vapours, in which ammonia cannot be distinguished.

We may conclude from the effects produced in the solution of the extract of belladonna by the various re-agents above employed: 1st, that it contains a free acid; 2d, an alkaline muriate; 3d, a small quantity of an ammoniacal salt.

The nature of the acid which exists in this substance can be nothing but acetic acid, since the sulphuric acid develops the smell of it, and the acetate of lead does not form any precipitate in it, which would take place if it were malic, tartarous, or oxalic acid. A part of this acid ought to be combined with the potash, and it is without doubt this combination which communicates to the extractive mass the property of attracting humidity from the air.

But it is neither these salts nor these acids which give poisonous qualities to the matter; these certainly reside in the vegetable substance itself: what then is the order of composition, which thus forms out of the same principles both our food and our poison? This is a difficulty which chemistry has not yet overcome, and unfortunately it is behind this barrier that secrets the most important to humanity are retained.

For want of the means of ascertaining accurately the differences which exist between vegetable compounds whose properties are diametrically opposite, we shall have recourse to their effects.

One of the means resorted to as the most proper for guiding us as to the nature of the substance of belladonna soluble in alcohol, was its decompostion by means of heat. I introduced therefore two grammes and seven-tenths into a glass retort, and administered the heat by degrees, until the water of solution was distilled: there passed over a yellow ammoniacal liquid, afterwards a thick oil which had a very singular disagreeable smell.

The examination of the liquid product enabled me to recognise a good deal of ammonia, partly free and partly combined; for the addition of some drops of caustic potash rendered the ammoniacal smell much stronger, and the oil was thick, black, and very acrid.

The charcoal remaining in the retort weighed one gramme, and had an alkaline and *prussiated* taste: when washed in boiling water, it yielded a ley which when mixed with sulphate of iron furnished a quantity of prussian blue very considerable with respect to the small quantity of matter employed. This charcoal after having been lixiviated and dried still weighed $\frac{7}{10}$ of a gramme.

The

The above quantity of charcoal, independently of that which was incrusted in the retort by the violence of the fire, and which I could not detach, is more considerable than any furnished by most of the other vegetable matters which I have hitherto had occasion to distill; for the 2·7 grammes of extract, in the state in which I took it, certainly contained more than 0·7 of a gramme of water and of nitrate and acetate of potash.

It seems that it also contains a great quantity of azote and of hydrogen, since it gave on distillation a great deal of ammonia, prussic acid, and oil. But as this matter may have contained a little nitrate, I supposed that a part at least of the azote, forming the ammonia and the prussic acid, had been produced by the nitric acid.

In order to clear up this doubt, I mixed six grammes of gum arabic, believed not to contain any azote, with a tenth part of saltpetre, and after submitting it to distillation I examined the products. The liquid which passed was in part ammoniacal, and its smell became still stronger by the addition of potash; which proves that an acid was formed at the same time with the alkali.

The charcoal remaining in the retort, weighing two grammes, and which was extremely phosphoric, contained prussiate of potash, like that of my matter. But although I employed in this experiment three times more gum, and probably more saltpetre, this mixture did not furnish such a great quantity of ammonia or prussic acid as the nauseous principle of the belladonna did.

Taking it for granted, therefore, that the saltpetre contained in the two grammes of this principle had given rise to prussic acid and to ammonia, we ought not to infer that the vegetable matter in question has not furnished some itself. This is the more probable, as its solution is precipitated by the infusion of gall nuts. However this may be, the experiment proves that it is difficult to judge by distillation, whether the organic matters which contain saltpetre are of a vegetable or animal nature.

The results of this analysis, although still very imperfect, are nevertheless sufficient to show that the article in question contains a considerable quantity of charcoal, hydrogen, and azote, and but little oxygen, if we may judge by the small quantity of carbonic acid which is formed during its decomposition in the fire.

From what has been said, may we be permitted to infer that the narcotic effects of belladonna on the animal œconomy are owing to the superabundance of the radical combustibles,

bustibles, and particularly to that of the charcoal over that of the oxygen in the principle of this plant soluble in alcohol?—Without going the length of positively affirming it, it is nevertheless certain that all the vegetable substances which produce analogous effects are rich in charcoal, hydrogen, and azote, whereas substances that are highly oxygenized produce contrary effects.

It must also be admitted, that many vegetable products equally abundant in these two principles do not possess the same virtues; but the azote, which is always found associated with hydrogen and carbon in the narcotic plants, does not exist, at least in the same quantity, in those as in the others.

Examination of that Part of the Belladonna which is insoluble in Alcohol.

1. This substance dissolved in water communicates to it the property of frothing when agitated.

2. Its solution is abundantly precipitated by the aqueous infusion of galls.

3. By the nitrate of barytes into a matter which is partly soluble in the nitric acid.

4. By the muriate of lime into a precipitate entirely soluble in the nitric acid.

5. This solution reddens turnsole paper.

6. The nitrate of silver produces no effect on it.

7. When burnt in a crucible, it leaves an alkaline and hepatic charcoal.

We may conclude from these effects, that this part of the belladonna is composed of an animal matter, of sulphate of potash, of acidulated oxalate with the same base, probably some nitrate, and that it contains no muriate. We may also conclude from these effects, that there are no earthy salts in it, since the muriate of lime forms in it a precipitate, as well as the nitrate of barytes.

I have ascertained by some experiments on a larger scale, that the precipitates produced in the solution of the substance in question by the nitrate of barytes, were in the first instance oxalate of lime, and in the second sulphate of barytes.

The oxalate of lime had taken up with it a great quantity of animal matter, which gave it a brown colour; which indicates that this salt has a strong affinity with animal matters, and explains the reason of mural calculi, which, as we all know, are composed of oxalate of lime, and are of a much deeper colour than the other species of calculi.

After

After having successively precipitated, as I have already said, the sulphate of potash and the acidulated oxalate of potash, I evaporated the liquor, which was always coloured, and which contained nitrate of potash and muriate of lime, and I treated it with the nitric acid in order to ascertain if it contained gum; but not having obtained an atom of saccho-lactic acid, I concluded that this substance does not contain gum. It is merely formed of oxalic acid and a yellow matter. This substance seems therefore to be entirely of an animal nature.

From what has been said above, we find that the juice of belladonna contains the following substances:

1. An animal substance, which is partly coagulated by heat, and partly remains in solution in the juice, in consequence of the free acetic acid which exists in it.

2. A substance soluble in alcohol, which has a bitter and nauseous taste, which on being combined with tannin becomes insoluble, and furnishes ammonia by its decomposition in the fire.

3. Several salts with a base of potash, viz. a good deal of nitrate, muriate, sulphate, acidulated oxalate, and acetate.

The refuse or husks of the belladonna, from which the juice had been extracted, having been washed in warm water, dried, and afterwards burnt, furnished ashes composed of a considerable quantity of lime, phosphate of lime, iron, and silex.

This lime announces that the plant contained oxalate of lime, which had been decomposed by the fire. It is by no means doubtful, that that part of the belladonna which is soluble in alcohol is not the only substance which in this plant produces a deleterious effect on the animal œconomy; for it is the only one which has any taste; and the well known effects of all the other substances which accompany it have nothing in them resembling those of the plant in question.

To put this assertion beyond all doubt, I administered to a dog a certain quantity of this principle mixed up with crumbs of bread.

First Experiment. About mid-day, I gave the animal a gramme of extract inveloped in ten grammes of paste.

Symptoms. In about three quarters of an hour the animal seemed inclined to sleep; he held down his head, and seemed unable to keep it up: he laid his head on the ground several times, and slight convulsions agitated his legs: his jaws also moved as in the act of chewing. These effects lasted about three quarters of an hour, and the dog then resumed his former appearance.

Second Experiment. At two o'clock I gave him two grammes of extract with twelve of paste: the above symptoms re-appeared, but they were feebler and of shorter duration.

Third Experiment. At three o'clock, I made him swallow four grammes of the same extract, with about 30 grammes of paste.

A few minutes afterwards he was seized with a continual but uncertain and difficult motion, chiefly in the abdominal region: he uttered some plaintive moans.

At half past three he experienced great difficulty in moving, and frequently fell on his hind feet: his respiration was much obstructed. He endeavoured several times to force his way through the wall, which indicated a kind of delirium: he was now seized with trembling in all his muscles.

At a quarter past four the animal lay down, and appeared to be plunged in a profound sleep; his pulsations were repeated faster than could be counted.

At half past four he vomited the paste which he had taken, some time after which he rose up; but he still walked with difficulty, sometimes falling on one side, and sometimes on his crupper. He held his head very low, his eyelids fell, and he did not distinguish objects; at least he continually ran against the walls, and the furniture of the laboratory: his nose was no longer affected by the smell of ammonia, and his ears seemed also to have lost their functions, for the loudest noise made no impression on him.

He had not lost his memory, however; for, upon placing him in the same posture in which he was made to swallow the paste, in order to give him some vinegar and water, he became furious, as if all his powers were suddenly renewed. From this moment, the symptoms which he had exhibited insensibly diminished, and about eight o'clock in the evening he recovered all his senses; but he was still much fatigued. Next day he ate as usual.

Every one must recognise in the above symptoms the effects of narcotics, and drunkenness carried to the highest pitch, from which resulted a kind of delirium. It is probable that if the animal had not vomited the greater part of the matter before it produced its effects, it would have died.

XXVI. *Case of Hydrocele, improperly treated as Rupture. By* JOHN TAUNTON, *Esq., Surgeon to the City and Finsbury Dispensaries, and to the City Truss Society for the Relief of the Ruptured Poor.*

To Mr. Tilloch.

SIR, IT is not the least of the evils which accompany a state of disease among the poorer classes of this large metropolis, that their complaints are frequently misunderstood, and consequently treated in a manner which tends to increase rather than to alleviate their sufferings. The superficial and hasty view which is but too often taken, even by regular medical practitioners, of the diseased victim of poverty on the one hand, and the allurements held out by mercenary and ignorant pretenders to medical skill on the other, are the causes of this additional affliction to the poor.

Those who officiate as medical officers to the numerous public charities which do honour to this great city, have daily opportunities of witnessing the melancholy effects of the errors thus committed. The following case of this kind, which occurred lately under my own inspection, and which had nearly terminated fatally to the patient, is one of the many illustrations of this observation which may be adduced.

Thomas Erskine, æt. 53, servant to Mr. Thomas Butcher, of Charing Cross, a few years ago received a kick in the scrotum, which occasioned a swelling, and which has continued ever since. At first it was attended with extreme pain; but this soon ceased, and the tumour assumed an indurated appearance. The poor man applied to two regular surgeons in his immediate neighbourhood, who informed him that his complaint was a rupture, and recommended a truss. Attracted by an alluring advertisement from some truss-makers in Soho, the patient applied to them: these gentlemen, after examining the patient, and affecting a great deal of medical and anatomical knowledge, confirmed the opinion of the surgeons, and applied a truss to the tumour, for which they charged the exorbitant price of a guinea. This happened three years ago, and the patient has ever since worn the instrument thus applied, with more or less inconvenience. A few weeks ago he was admitted a patient at the City Dispensary, when on examining him I found the case to be a decided hydrocele. The operation of tapping was immediately performed, and the patient in

 a few

a few days was restored to his former state of health. Pilulæ rhei cum terebinth. formed the only prescription which I found necessary to administer.

I am, &c.

Greville Street, Hatton-Garden, August 24, 1810. JOHN TAUNTON.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

The experiments detailed in Mr. Davy's paper respecting the muriatic acid, of which we gave a brief report in our last Number, are so highly interesting, that no apology can be necessary for again bringing the subject before our readers, and endeavouring to present the results in a concise yet perspicuous form. But before proceeding to this, we must beg our readers to correct two typographical errors in our last report. In page 71, line 20, for "nine modes," read *nice modes;* and in line 22, for "nine deductions," read *some deductions.*

The conclusions drawn by Mr. Davy from the series of facts with which this valuable paper is enriched, will serve to extend and enlighten the theory of chemistry to even a greater extent than any of the brilliant discoveries formerly made by this indefatigable philosopher. The following are the conclusions to which we allude:—

1st. The oxymuriatic acid is (as far as our knowledge extends) a *simple substance,* which may be classed in the same order of natural bodies as oxygen gas; being determined like oxygen to the positive surface in Voltaic combinations, and like oxygen, combining with inflammable substances, producing heat and light.

2dly. That its combinations with inflammable bodies are analogous to oxides and acids in their properties, and powers of combination, but they differ from them in being for the most part decomposable by water.

3dly. That hydrogen is the basis of the muriatic acid, and oxymuriatic acid its acidifying principle.

4thly. That the compounds of phosphorus, arsenic, tin, &c, with oxymuriatic acid, approach in their nature to acids, and neutralize ammonia and other salifiable bases.

5thly That the combination of ammonia with phosphorus acidified by oxymuriatic acid is a peculiar compound, having properties like those of an earth, and is not decomposable at an intense red heat.

6thly.

6thly. That oxymuriatic acid has a stronger attraction for most inflammable bodies than oxygen; and that on the hypothesis of the connexion of electrical powers with chemical attractions, it must be highest in the scale of negative power; and that the oxygen which has been supposed to exist in oxymuriatic acid has always been expelled by it from water or oxides.

FRENCH NATIONAL INSTITUTE.

The readers of the Philosophical Magazine must have seen from the accounts which have lately appeared in our pages, of the labours of the French chemists, that those gentlemen had questioned the accuracy of the inferences drawn by Mr. Davy from the numerous experiments he had made, in the course of his electro-chemical researches, respecting the nature of the alkalies and the earths; maintaining that the metallic bodies obtained from these substances, in place of being simple, as asserted by Mr. Davy, were compounds of the respective alkalies and earths with hydrogen; or, in other words, that the new bodies were *hydrurets*. Of this opinion were Gay Lussac, Thenard, and most of the French chemists. Berthollet among the rest warmly contested the correctness of Mr. Davy's inferences, and maintained the accuracy of the French conclusions. They have now, however, changed their opinion, and done justice to our countryman.

At a meeting of the French National Institute in the end of June, Messrs. Gay Lussac and Thenard read a notice containing the results of a great variety of experiments on the new metals; from all of which they conclude, after a most rigorous investigation, that professor Davy was perfectly correct in his inferences, and, with a degree of frankness honourable to themselves, renounce their former opinion that these new metals are hydrurets.

We cannot but take notice here of an assertion made in the Report of the Labours of the Institute, (published in a former volume of the Philosophical Magazine,) which savours of a blundering, but probably not intended, plagiarism. The Report states, that Messrs. Gay Lussac and Thenard discovered the mode of metallizing ammonia by potassium; whereas these gentlemen themselves, who have more than once uncandidly assumed Mr. Davy's facts, acknowledge this to have been that gentleman's discovery, in their paper on ammonia.

The result of the present contest, we cannot but hope, will serve as an admonition to the editors of some of our periodical

periodical works, not to be hasty to commit themselves in questions of science, on the authority of reports drawn up by jealous rivals for national fame. We could name a most respectable journal which has fallen into this blunder. As to some more obscure writers, who have ventured to talk about "the *pretended discovery* of the decomposition of the alkalies," they will probably show a little more modesty in their remarks in future.

XXVIII. *Intelligence and Miscellaneous Articles.*

M. Wiebeking, director of roads and bridges to the king of Bavaria, has discovered a method of constructing wooden bridges, which in point of strength and solidity promise a duration of several centuries. They are also remarkable for the elegance of their form and the width of the arches. A bridge has been constructed on the above plan over the river Roth, five leagues from Passau, consisting of a single arch two hundred feet wide: another has been made for a large city, two hundred and eighty six feet wide. These arches may be so constructed as to admit of ships of war or merchant vessels passing through them, an aperture being made in the centre, which can be opened and shut at pleasure. Another advantage possessed by these bridges is that of being speedily taken to pieces: if it be necessary to stop the progress of an enemy, the arch may be removed in one day, and the abutments in another, without cutting the smallest piece of timber.

With respect to the advantages in point of œconomy resulting from the adoption of M. Wiebeking's plan; it has been estimated that a stone bridge of similar dimensions to a wooden one of a given size would cost two millions of florins, whereas the latter would cost only 50,000 florins; and on the supposition that a wooden bridge will only last 100 years, it follows that, taking the interest on the principal sum into the computation, there will result a saving of eleven millions six hundred and eighty thousand florins.

The Pharmaceutical Society of Paris has announced the following as prize questions for the present year:

1. Ascertain as far as possible, whether there exists in vegetables an identical principle which chemists have designated by the name of extractive?—Ought we to retain the

the ancient classification adopted for pharmaceutical extracts, divided, according to Rouelle, into gummy, resinous, gummo-resinous, resino-gummy, and saponaceous extracts?—Can a more methodical and more exact classification be established by means of chemical experiments made on the principal substances in pharmacy furnished by extracts?—Indicate, according to the nature of their different constituent principles, the mode of preparation best adapted for each, and the nature of the menstrua which ought to be employed."

2. "What is the present state of pharmacy in France? what rank does it hold in the healing art? and what are the ameliorations of which it is susceptible?"

The prize offered for the best memoir on the first question is a gold medal of the value of 200 francs. That offered for the best paper on the second question is of the value of 100 francs. The memoirs to be transmitted to Paris on or before the 1st of October 1810.

The following account of a new optical instrument is extracted from a recent French journal: "It is well known that the art of perspective consists in representing on a plane surface objects in the position in which the eye perceives them. Descriptive geometry furnishes the means of doing this; but the method which it teaches presupposes science, and demands time. The painter, without having recourse to geometry, draws on a simple purview from habit and practice in his art. However excellent his eye, and however skilful an artist may be, he cannot flatter himself with obtaining geometrical precision. A new instrument has therefore been invented, by means of which every draftsman, without knowing the rules of perspective, may design with ease and correctness all kinds of subjects on every scale not exceeding 5 decimetres square. This invention belongs to M. Roggero, of the Conservatory of Arts and Manufactures.

"Some very ingenious instruments have been already contrived with this view, and among others that of Mr. George Adams, who has been peculiarly distinguished. But from the great number of joints of which the mechanism is composed, all of these instruments were more or less liable to disadvantages, which M. Roggero's instrument has overcome. He has also united solidity to precision in the transmission of the movements, besides having furnished his instrument with an achromatic glass, by means of which we may trace the perspective of objects placed at a distance."

On

On digging lately at Frescati in Italy, not far from the ancient Tusculum, a quantity of arms, vases, human bones, and a broken statue were found. The latter seemed to be that of a Roman consul; and a few days afterwards another statue was found resembling that of a Roman matron.

M. Vincenzio has lately published at Rome two scientific works: one entitled *Lettere scientifiche*, and the other, *Spiegazione di due fascetti di gemme antiche*. The same author has also written a dissertation, to prove that the colossal horses of the Quirinal have been changed in their places, and that they have been in fact badly placed originally.

ICHTHYOLOGY.

Mr. Joseph Foster, fishmonger in Carlisle, has at present in his possession a pilot fish, the only one we believe that has appeared on these coasts.—The fish is of the order of thoracici, which comprehends seventeen genera and upwards of two hundred and twenty species. It is found in the Mediterranean and Atlantic, chiefly towards the equator. The body is shaped somewhat like that of a mackarel; the head is long and smooth, and the snout advances some distance beyond the mouth. It has two small fins near the head; another running along the back from the head to the tail; and one under the belly, of similar length. The colour in general is brownish, changing into gold; and there are several transverse black belts. Mariners observe, that this fish frequently accompanies their vessels: and as they see it generally towards the fore part of the ship, they imagine it is employed in guiding and tracing out the course; whence it has received its name. Probably it is either amusing itself, or pursuing its prey. It sometimes attends the dog-fish and the shark; and swims at the height of a foot and a half from the snout of the latter; imitates all its movements, and seizes with address any part of the spoil which the shark allows to escape. Though so small as not to exceed six inches, it will keep pace with ships in their swiftest course.—The one in Mr. Foster's possession was caught a few days ago in the Solway Frith.

A species of hemp, manufactured from the leaves of a particular kind of palm, which abounds in Sierra Leone and its neighbourhood, has recently been sent to this country; and being made into cord, subjected to experiments calculated to ascertain its strength, as compared

with

with the same length and weight of common hempen cord, the result was very satisfactory—it being found that hempen cord broke with a weight of 43lbs. three-fifths, while the African cord did not give way to less weight than 53lbs. two-fifths, making a difference in favour of the latter of 10lbs. in 43lbs.

LECTURES.

Theatre of Anatomy, Blenheim-Street, Great Marlborough-Street.

The Autumnal Course of Lectures on Anatomy, Physiology, and Surgery, will be commenced on Monday the 1st of October, at Two o'clock, by Mr. Brookes.

In these Lectures the Structure of the Human Body will be demonstrated on recent subjects, and further illustrated by Preparations, and the functions of the different organs will be explained.

The Surgical operations are 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 Knowledge, and every possible attention will be paid to their accommodation as well as instruction.

Anatomical Converzationes will be held weekly, when the different Subjects treated of will be discussed familiarly, and the Students' views forwarded.—To these none but Pupils can be admitted.

Spacious Apartments, thoroughly ventilated, and replete with every convenience, are opened all 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.	£.	s.
For a Course of Lectures, including the Dissections,	5	5
For a Perpetual Pupil to the Lectures and Dissections,	10	10

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.

Theatre of Anatomy, Greville-Street, Hatton-Garden.

Mr. Taunton will commence his Autumnal Course of Lectures on Anatomy, Physiology, Pathology and Surgery, on Saturday, October 6th, at Eight o'clock in the Evening precisely, to be continued every Tuesday, Thursday, and Saturday, at the same hour. In this Course of Lectures it is proposed to take a comprehensive view of the Structure and Œconomy of the Living Body, and to consider the causes, symptoms and treatment of surgical diseases, with the mode of performing the different surgical operations. An ample opportunity for professional improvement will also be afforded by the attendance of the Pupils, if they are so inclined, at the Finsbury and City Dispensaries, to which Mr. Taunton is Surgeon. Further particulars may be had on application to Mr. Taunton, at his house in Greville-street.

St. Thomas's and Guy's Hospitals.

The Autumnal Courses of Lectures at these adjoining Hospitals will begin the first of October, viz.

At St. Thomas's. Anatomy, and the Operations of Surgery, by Mr. Cline and Mr. Cooper.—Principles and Practice of Surgery, by Mr. Cooper.

At Guy's. Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged, that no two

two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instruction. Terms and other Particulars may be learnt at the respective Hospitals.

Dr. Clarke's and Mr. Clarke's Lectures on Midwifery, and the Diseases of Women and Children.

The Winter Course of these Lectures will commence on Friday the 5th of October, at the house of Mr. Clarke, No. 10, Upper John-Street, Golden-Square.

The Lectures are read every day from a Quarter past Ten o'clock in the Morning till a Quarter past Eleven, for the convenience of Students attending the Hospitals. The Students will have Labours when properly qualified. For particulars apply to Dr. Clarke, No. 1, New Burlington-Street; or, to Mr. Clarke, No. 10, Upper John-Street, Golden-Square.

Mr. Stevenson, of Great Russel-Street, Bloomsbury, who as Pupil is intimately acquainted with the Practice of the late Mr. Saunders, is preparing a practical Work on a frequent Disease of the Eye, which we understand is nearly ready for publication.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Charles Dyer, of Boston, North America, now residing in Westminster, a patent (in consequence of a communication made to him by a certain foreigner residing abroad) for certain machinery for cutting and heading of nails and beads of all kinds and sizes, from strips or plates made of iron, copper, or any other metal capable of being rolled into plates.—July 26, 1810.

To Thomas Wade, of Nelson Place, Kent Road, in the county of Surry, gent., for his method, or process, of imitating lapis lazuli, porphyry, jasper, the various sorts or kinds of marble, and all other stones usually wrought, carved, sculptured, or polished; also inlaid or Mosaic work to be used for or in the formation or manufacture of chimney pieces, slabs, funeral monuments, and for every other purpose to which such stones and marbles are, or may be, applied.—July 26.

To Edgar Dobbs, of the Borough of Southwark, gentleman, for a variety of compositions for making a water-proof cement mortar and stucco, the same being also applicable as durable colouring washes for buildings.—August 2.

METEO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For August 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July 27	60	62°	57°	29·50	10	Stormy
28	60	57	55	·63	0	Stormy
29	61	64	56	·80	38	Cloudy
30	60	65	55	·68	36	Cloudy
31	60	65	54	·80	28	Showery
August 1	59	66	56	·82	20	Showery
2	57	68	58	·95	42	Fair
3	59	66	57	·70	26	Showery
4	59	67	58	·54	33	Showery
5	60	70	56	·64	45	Showers with
6	55	69	55	·64	46	Ditto [thunder
7	56	68	56	·62	30	Showery
8	58	66	57	·70	29	Showery
9	60	64	56	·95	32	Showery
10	61	67	57	·72	39	Fair
11	62	68	55	·57	41	Showery
12	60	69	58	·81	36	Showery
13	60	68	59	·71	44	Fair
14	60	69	55	·69	33	Showery
15	59	63	54	·50	36	Fair
16	50	50	49	·60	0	Rain
17	51	61	49	·92	65	Fair
18	49	66	51	30·20	63	Fair
19	50	61	52	·21	0	Small rain
20	51	69	54	·28	60	Fair
21	56	69	58	·26	53	Fair
22	56	70	59	·03	50	Fair
23	58	74	64	·02	50	Fair
24	60	74	65	·01	61	Fair
25	61	76	64	29·99	59	Fair
26	60	71	68	30·01	48	Fair

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

ERRATUM.

Our readers are requested to correct the following typographical error in the name of the author of the valuable paper on tunnels, given at page 34, of the present volume. For Lennox *read* Lennon.

XXIX. *A Sketch of a History of Pus.* By George Pearson, *M.D. F.R.S. Senior Physician of St. George's Hospital, &c. &c.*

I was induced to write this historical sketch for three purposes: *namely*; 1st, To inform myself of the facts already published on the subject. 2d, To, perhaps, assist some others with information. 3d, To manifest whether or not my own investigation had produced any accession of knowledge.

The word *pus*, so very commonly used in our language, is plainly the Greek word πυος or πυον abbreviated by the Latins, with the change of writing in Roman characters. It appears from the original writers that this term denoted any thick, white, opake, clammy, animal fluid, such as the matter of abscesses, and of ulcers or sores; also the thick milk called *colostrum* secreted immediately after parturition. I am of opinion that philological investigations are unsuitable in a writing of the same kind as this now offered; yet I think it may be useful or even necessary to remark, that the etymological import of the word *pus*, is that of *putridity* or *corruption*; which denotes a state of broken down texture, not only of animal and vegetable matters, but of any mineral substances whatever, such as stones and metals*. Accordingly the word *pus* appears to have signified, among the Greeks and Romans, an animal fluid from matter in a state of broken down aggregation, or of corruption; and such were the fluids above named. Hippocrates distinctly describes the pus of abscesses and ulcers from its simple, obvious properties; viz. it is a *thick, white, inodorous, uniform, smooth* fluid—when it is of a good or "laudable" kind. But according to its variations from these properties, it was asserted to be of a bad kind. It is especially said that good pus has not the least offensive smell. It was considered among the ancients to be of great moment to know the properties of this fluid, particularly for the purpose of determining whether or not the sputum in pulmonary disorders was produced by an ulcer

* The acceptation of the term *putridity*, and of its derivatives according to their confined meaning since the cultivation of chemistry as a distinct science; and not according to the original extensive sense, is one of the causes of the erroneous doctrines of fevers called *putrid*, which disgrace the pages of some of our most eminent writers. The modern meaning of *putrefaction* being that of the process of new compositions and decompositions in animal or vegetable substances effected by chemical attractions, and characterized by fœtor, it is apparently incompatible for matter in a living state to exist during such chemical agencies.

or

or abscess. On that account mention is made of the trials with water and fire. Pus sinks, it is observed, in not only mere water, but in salt water; while the secreted slimy substance of the bronchial membrane called *mucus* is frothy, and floats on water:—the former substance is readily diffusible through water, but the latter is not so. If pus be contained in sputum, it emits a most offensive smell while burning on an ignited coal. From the earliest writers it appears that the judgement was regulated by observation of the circumstances of the discharge of purulent matter, as well as of its properties: that from suppurated tubercles was distinguished from the abscesses called vomica, and empyema by acute inflammation, such as of peripneumony and pleurisy; but a third source from the lungs, now well ascertained, viz. by secretion fron the bronchial membrane without any breach in its continuity or loss of its substance, was made known in consequence of the observations published about the same time by the learned De Haen of Vienna, and by our most ingenious countrymen Mr. John Hunter and Mr. Hewson. Previously to these pathologists, Mr. Sharp in his *Critical Essays*, p. 142; and Mr. Gataker in his *Essays*, p. 97, as well as, I believe, other authors, had asserted that *puriform* matter is producible without ulcer or loss of substance or breach, by inflammation, or merely by irritation occasioned by extraneous bodies in gonorrhœa, ophthalmia, &c. It was, however, generally considered as doubtful, whether or not this matter was the same as that of ulcers and abscesses. This indecision was founded, probably, on prejudice rather than on any actual observation of differences of properties. The proof by dissection that membranous surfaces, in their entire state, produce matter of the same sort as that of sores and abscesses, was hitherto the most important pathological fact brought to light subsequently to the Greek writers.

About 40 years ago, Simpson of St. Andrews, De Haen, Gaber, Pringle, Cullen, Fordyce, Hunter, Hewson with his pupil Hendy *, were the prominent parties in the discussion of the point—the matter from which pus was immediately derived. The first person who considered pus to be the product of vessels becoming, or at least performing the office of, glands, as far as I know, was Simpson. De Haen's observations confirmed this opinion; but the deposition of an opake white matter from serum of blood on standing, induced Gaber, Pringle and Cullen to account for the pro-

* *Tentamen Inaugurale de Secretione glandulari.* Edinburgi, 1774.

duction

duction of pus on the supposition of a similar deposition from effused serum in abscesses and ulcers. Fordyce applied his chemical science, of which he was a master, on this occasion:—he interpreted all the phænomena of suppuration by means of the principle of purulent fermentation, which compounds pus from the supposed constituent or elementary ingredients of any kind of animal matter,—muscles, nerves, membranes, blood *, &c. This composition, however, was effected solely by the agency of vital powers acting on such animal matters. Hewson supported by new arguments the doctrine of the production of pus by secretion †. Hunter seems to have convinced the medical public, by his ingenious observations and reasoning, that pus is a secreted fluid; and, with the exception that inflammation is essential to its production, his doctrine has been for the last 20 or 30 years generally admitted to be well established ‡. But the minds of the thinking part of the medical faculty seem to have been still left in an unsatisfied state with regard to the notion they ought to entertain of the substance to be called pus. On account of the few properties of it known, they probably, and very reasonably, apprehended that different things might be denoted by this term; or that things in reality the same might be denoted by different terms, being supposed to be different from one another. Accordingly, in the course of the last 40 years, inquiries into the nature and properties of this fluid have been instituted, and been espe-

* Van Swieten seems to have entertained a somewhat like opinion:—"Pus non fit in vasis sed extra vasa, in vulnere generatur ab effusis humoribus, calore corporis fotis et mutatis. Si enim pus omne in vulnere hærens linteis carptis mollissimis abstersum fuerit tenui liquido non pure post horam vulneris superficies undique madida apparebit; sed si per viginti quatuor horas emplastro tectum fuerit vulnus, illo ablato pus apparebit. Unde pus fit extra vasa; sed materies, unde fit, per vasa adfertur."—*Commentaria*, tom. i. p. 230.

† Pus is found in cavities sometimes without ulceration;—globules are perceived in it like those of milk—the quantity, the time of production, and properties of pus are varied by the state of the constitution, particularly by the passions—purulent matter is only the coagulable lymph altered, chiefly by inflammation, in flowing through secretory vessels. See *Experimental Essays*, by William Hewson, F.R.S. 1772.

‡ I am unable to state precisely the date of Mr. Hunter's doctrine on this subject; but I learn from the Dissertation on Pus, by E. Home, esq. F.R.S. 1788, that he had delivered it, for many years preceding this publication, in his lectures. Mr. Home states Mr. Hunter's conclusions; "that the vessels of the part take on the nature of a gland, and secrete a fluid which becomes pus."—"Pus is a secreted fluid, at least it is formed from a similar structure of vessels as other secretory organs from the blood."—"Changes in the constitution affect the state of the pus, which could not be the case if it were made up of the solids and fluids of the part." For further proofs I refer to the ingenious Dissertation of the author.

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cially promoted by the honorary rewards offered by several associated bodies. Hence some improvements have been made. But physicians were still continually complaining of the disadvantage in practice, from the distinguishing properties of pus not being satisfactorily determined; above all, for the purpose of judging in pulmonary diseases whether or not the sputum was purulent. In the year 1778, the late Mr. Charles Darwin received the gold medal from the Æsculapian Society at Edinburgh, for his supposed discoveries of the criteria by which pus and mucus are distinguishable. It was asserted that, water being added to a dissolution of pus in sulphuric acid, a precipitation takes place, but such a dissolution of mucus affords on the addition of water merely suspended flakes;—that pus is diffusible through diluted sulphuric acid, but mucus is not;—that these effects are also observed with water, or salt water. In 1787, Dr. Brugman, in his Inaugural Dissertation on Pus, among a number of other experiments, which I do not think necessary to be noticed, relates that dry volatile alkali (carbonate of ammonia) with an equal quantity of pus becomes viscid, semi-transparent and white; that caustic ammonia partially dissolves it, and the rest yields a very viscid fluid, but on adding water the whole is deposited in a viscid state;—that all neutral salts thicken pus, and still more so the earthy salts, and most of all the metallic salts;—that alcohol condenses it by uniting to its aqueous parts, but neither coagulates nor dissolves it. But previously to these experiments, Mr. Hunter had observed, that pus is "coagulated by sal ammoniac" (muriate of ammonia), which he, and subsequently Mr. Home, depend upon, as affording a criterion between pus and other animal fluids. Grassmayer is quoted by several authors for the fact that pus is precipitable in a gelatinous state by caustic alkaline lixivium, but if mucus be present it is suspended. The mistake in the fact that pus was highly putrescible, was perhaps first exposed by my fellow-student Dr. Hendy, which was subsequently confirmed by Mr. Home. Several foreign authors, as Plenciz, Murray, Schroeder, Salmuth, Quesnay, either adopted subsequently, or coincided in, the opinion that pus is a secreted fluid. Mr. Home's ingenious Dissertation on Pus was published in 1788, and I find no accession of facts from that date up to the present time. His work is valuable, not only for his own observations, but for a just exposition of those of Mr. Hunter. It is here attempted to be shown that "pus is composed of globules swimming in a transparent aqueous fluid, yet that

that the globules are formed in the fluid after its secretion, while lying upon the sore or other inflamed surface, in different times, according to the state of the constitution and secreting part, the pus being secreted in a transparent condition *;—that inflammation is the absolute cause of the formation of this fluid; that the globules in pus are not soluble in cold water like those of blood, but are decomposed by boiling water, and the fluid in which they swim is not coagulable by heat, but is by sal ammoniac, which serum or the blood itself is not: the globules also are smaller than those of chyle, but much larger than those of pancreatic juice; and they are of the same size, but less numerous than those of milk;—that the distance from the heart influences much the condition of the pus;—that the depravation of this fluid is in proportion to the flaky or curdy particles seen floating in the fluid with the globules; and that the flaky parts are in the greatest proportion where the inflammation has been least, or the process on other accounts most defective.

In this historical account of the fluid under inquiry, if I have omitted to state the observations of any other authors, that must be imputed to my unacquaintance with them. I also purposely do not notice various pathological facts; such as the effects of pus of variolous eruptions, siphylitic, and other contagious diseases. As it appears from the confession of physicians that the nature and distinguishing properties of pus have not been satisfactorily ascertained, I engaged in an inquiry into the properties of this substance; some of the results of which I shall offer to the public.

XXX. *Remarks on the Rev.* C. J. Smyth's *Letter on Systems of Tuning Musical Instruments.* Vol. xxxv. p. 448.

After bestowing due praise on Mr. A. F. C. Kollmann's improvement of the theory of musical composition, Mr. S. makes objections to Kirnberger's temperament, which Mr. K. had recommended on page 9 of his "New Theory;" and concludes with several assertions of little

* In a former paper on Expectorated Matter, (see Phil. Mag. vol. xxxv. p. 12—20,) I erroneously assigned the discovery of the globularity of pus to Mr. Home. It is but justice in me to declare that this Gentleman no where claims the discovery: on the contrary, I have since found that he refers it to Mr. Hunter. I take for granted Hewson's observation of this fact already quoted, was subsequently to Mr. Hunter's; but I find no authority for the exact period of the discovery by either party.

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weight, because (and I consider his "palatable dishes") they are mere matters of *taste*. And it is curious that he should presume "organ-tuners will continue to tune in the same way as their ancestors did *before them*, till arguments are produced to prove the superiority of *Kirnberger's* temperament" to theirs; for a person but slightly acquainted with the subject might from this suppose there are no better unequal temperaments. However, there are others which, for my own part, I do decidedly prefer. Undoubtedly Kirnberger's system is one of the worst; and in the ancient system (as M. de Bethizy observes, *Exposition*, p. 130, 1764), the sounds in some of the scales are so altered that they are insupportable to a delicate ear. The equal temperament has been preferred by Couperin, Marpurg, Rameau, Cavallo, professor Chladni, and many other eminent philosophers and musicians: it is certainly the best for piano fortes; but for the organ perhaps a good unequal temperament is better, on account of the loudness of the beats.

As one of your musical readers, I am obliged to Mr. S. for undergoing "the drudgery of calculation" on our account: he would still further merit our thanks by sending to your valuable Magazine tables of the numbers of vibrations, the monochord-lengths, and the beats in fifteen seconds, belonging to the other unequal temperaments that have been proposed; and I think he ought to send a table for the *common* system, as the chief end of his communication seems to be, to compare it with Kirnberger's, and to show its superiority *.

As to the generality of tuners (and many of them are very conceited men), I believe they know but little or nothing of harmonics. They learn one method by ear only, and remember it as they would a tune, without knowing a rule on which either is founded. That the ear and the memory alone are sufficient, after proper exercise, I am well convinced; for I can tune my harp with the same accuracy diatonically and without sounding two strings at a time, as it can be tuned in the usual way by consonances; and I have a pupil, only twelve years old, whose ear and remembrance of sounds are so accurate, that she can, while in a different room from the instrument, name any num-

* It would be an improvement of the first column of these tables, to follow the German *tablature*, described in art. 34 of Dr. Callcott's Musical Grammar, 2d edit. 1809. Mr. S. in some future communication, would much oblige me by stating precisely what he means by the term *Wolf* in tuning.

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ber of notes that can be struck with one hand in any part of the piano forte which she has been a little while accustomed to play on.

I know one tuner who, after studying ratios a little, thinks with Eximeno :—" Qual sciocchezza non è questa, supporre la musica fondata in certe ragioni, che bisogna guastare per ridurre la musica ad esecuzione? Almeno insegnasse la matematica a far questo guastamento; ma dopo un grand' apparato di ragioni matematiche, ciascun le guasta per la pratica a modo suo. I Francesi hanno fatto per il temperamento del cembalo difusissimi calcoli; ma tutti egualmente capricciosi che inutili, poichè in fine *l'istinto* senza riguardo a' numeri c' insegna ad accordar gli strumenti, come c' insegna a metter insieme le lettere per formar le parole, &c." p. 71.

August 11, 1810. M.*

XXXI. *An Examination of the Instructions given in an anonymous Pamphlet published in* 1809, *for Tuning an Equal Temperament of the Musical Scale. By a* CORRESPONDENT.

To Mr. Tilloch.

SIR, THE table by your correspondent the Rev. Mr. Smyth, at page 452 of your last volume, has enabled me to make a comparison of the method of tuning laid down in a recent pamphlet, sold by Becket and Co.; and perhaps you will oblige me in laying the same before your readers, with the necessary plate and extracts from the pamphlet alluded to; which, after some well directed sarcasms, explains the nature of the musical scale and the necessity of temperament, by a professed extract from Earl Stanhope's pamphlet, that was reprinted in your 25th volume. It then proceeds to extract Mr. Emerson's opinion, in favour of the Equal Temperament; founded solely, on there being no *fifth* in it, which is tempered more than $\frac{1}{100}$ part of a note; while the more important circumstances are overlooked, that there are in it 12 major thirds, each of which is more than $\frac{1}{14}$ part of a note too sharp, and 12 minor sixths as much too flat; also 12 major sixths almost $\frac{1}{13}$ part of a note too sharp, and 12 minor thirds as much too flat.

In page 21 of the pamphlet, I find an expression, di-

* Can any of your readers favour me with any particulars respecting the manufacture of *tuning-forks*, and the standard by which they are tuned?

rectly contrary to what your correspondent Mr. Smy says, at page 450, as to the temperament now *in use* upon the organ, viz. Organs are "universally tuned according to the equal temperament:" and the author continues, "the *trials* which I have myself made of the equal and unequal temperaments, amongst the latter of which was that recommended by Earl Stanhope, have induced me to adopt the equal temperament as the best for practice."

"The method of tuning a piano-forte, &c. according to the equal temperament, is explained by the following instructions, and further elucidated by the annexed tuning table*, to which the reader is desired, as he proceeds, to refer."

"Observe, that the capital letters in the tuning table, indicate the notes when tuned; the small letters, the notes to be tuned from them; and the crosses the notes already tuned, with which the tuning notes are to be tried in chords, as will be shown in the instructions; and that these crosses stand for the natural notes, except when otherwise marked."

1. "By means of a tuning-fork, tune the C next above the middle C, and from the former, tune the middle c a perfect octave.

2. "From middle C tune g, next above it, *a little* flatter than a perfect fifth: and in order *to ascertain* whether it is too flat or not flat enough, try it in a chord with the two C's already tuned. If it make a tolerably good fifth, with the C below, and at the same time a tolerably good fourth, with the C above, *it is well tuned.*"

Here, sir, I have to remark, that *the little* which the fifth Cg is to be flattened, supposing the tenor-cliff c of the usual concert pitch, or to make 240 complete vibrations in one second of time, is just such, according to Mr. Smyth's table, that it may beat 12 times in one quarter of a minute, or 15 seconds, a space of time always to be understood when a number of beats are mentioned, in what follows: and that the pretended *trial* in the latter part of this precept, is inapplicable and ridiculous, since every complement of a tempered concord, to the octave, is alike tempered with itself, *whatever that degree of temperament may be*, and consequently whether well or ill tuned, *for the purpose of an equal temperament*, is almost as remote from this test, as would be the hour or minute of the day, on which such fifth was tuned.

* See Plate V.

3. "From

3. "From G tune g below it, a perfect octave, and *try* it in a chord with the C between them."

4. "From the last tuned G tune d *a little* flatter than a perfect fifth, and try it in a chord, with the G above it already tuned, *until* you have G d a good fifth and d G a good fourth."

> This fifth G d must beat 9 times, and the fourth d G 18 times*, and the pretended *trial* is nugatory and absurd, for its professed purpose.

5. "From D, tune a, *a little* flatter than a perfect fifth, and try it in a chord, with C above it, *until* you have D a a good fifth, and a C a good third."

> The fifth D a must beat 13 times: and the first trial or check of any use which occurs, is the *minor* third a C, beating 326 times, but which no organist in England would, I think, call "*a good third*."

6. "From A, tune a below it, a perfect octave, and *try* it in a chord with D between them."

7. "From the last tuned A, tune e a flat fifth, and *try* it with C and G, *until* you have three good thirds, A C, C e, e G.

> Now since the *minor* third A C beats 163, and the thirds C e and e G, 142 and 244 respectively, such must be *tried a long time*, so that all idea of perfect chords is forgot, before any musician would pronounce them "*good thirds*."

8. "From E, tune b a flat fifth, and try it with G, *until* you have two good thirds E G and G b."

> This fifth E b must beat 15 times, and the thirds above mentioned will beat 244, and 214, and consequently are not "good thirds."

9. "From B, tune b below it a perfect octave, and try it with D and G."

> Here b D beats 183, and G B beats 214; on which I forbear to comment.

10. "From the last tuned B tune f* a flat fifth, and try it with D and b *until* you have D f* a good third, and f* B a good fourth."

> Now B f* beats 11 times, and 160 and 22 are the other beatings.

11. "From F*, tune f* below it a perfect octave, and try it with A and D."

* Because the *minor* consonance (the 4th) is the uppermost; see Dr. Smith's Harmonics, 2d edit. p. 93.

Here

Here f*A beats 137, and DF* beats 160 times.

12. "From the last F* tune c* a flat fifth, and try it with A and F* *until* you have Ac* a good third, and c* F* a good fourth."

The fifth F*c* beats 8½, and the major third Ac* 120; of course the fourth C* F* beats 17, the same with its complemental fifth above it.

13. "From C* tune g* a flat fifth, and try it with E and B *until* you have Eg* a good third, and g*B another good third."

The fifth C*g* beats 12 times; also Eg* beats 180, and g*B beats 308.

14. "From G* tune g* below it, a perfect octave, and try it with E and B."

g*B beats 154, the half of the above, and EG* 180 as above, and the repetition, and the whole of this step is unnecessary.

"As you now proceed by tuning the fifths downwards, the lower note is to be sharpened, which is the same thing in effect as flattening the upper note, when tuning upwards."

15. "From C above middle C, tune f *rather sharper* than a perfect fifth, and *try* it with A and C, *until* you have f A a good third, and C f a good fourth."

Here the fifth f C beats 16 times, and so does the fourth C f, and the trial of it is useless and *fallacious*, for proving whether f C is sharpened *the proper quantity* for an equal temperament of the scale. But f A beats 190, and this furnishes a check of some use.

16. "From F tune a* a sharp fifth, and try it with D, *until* you have a*D a good third, without sensibly injuring the fifth."

The fifth a*F beats 10½ times, and the third a*D beats 127 times.

17. "From A* tune a* above it, a perfect octave, and try it with D and F."

Here A*D beats 127 times as above, and a*F 10½ times, the same as the last, which shows this to be a useless step.

18. From the last tuned A* tune d* a sharp fifth, and try it with G and A* (below) until you have A*d* a good fourth, and d*G a good third."

Here

Here the fifth d*A* beats 14 times, and the fourth A*d* the same, and the third d*G beats 169 times.

From the above comparisons of Mr. Smyth's table, with the rules in this pamphlet, it will I think appear plain, that the writer of them had no proper conception of the nature of an equal temperament, and that it is extremely *unlikely that he had ever heard or calculated such a temperament*, decidedly as he speaks of *his trials of it:* and I fear, sir, that this is no uncommon case, in the present rage for writing principles of music, principles of tuning, theories of harmonics, instructions for tuning, &c. &c. by persons who ought first to employ themselves, in studying the very elements of the science of harmonics.

Among the recommendatory criticisms for different Reviews, of the pamphlet before me, one for the *Phil. Mag.* has been forgotten, which I suggest should have run thus:

> A careful examination of the instructions contained in this pamphlet, for tuning an equal temperament, convinces us, that the assumed name *Musicus Ignoramus* of its author, is no *misnomer;* and that the *wit* displayed therein, vastly exceeds its science, or its usefulness.

I beg pardon for obtruding so long a letter on your attention, and am, sir,

August 6, 1810. Yours, &c. &c. &c.

XXXII. *An Analysis of several Varieties of British and Foreign Salt, (Muriate of Soda,) with a view to explain their Fitness for different œconomical Purposes.* By William Henry, *M.D. F.R.S. Vice-Pres. of the Literary and Philosophical Society, and Physician to the Infirmary, at Manchester.*

[Concluded from p. 119.]

Sect. III. *Account of the Methods of analysing the several Varieties of Muriate of Soda.*

The method of analysis which I adopted, in examining the several varieties of muriate of soda, was as follows.

When the salt was in a state of solution, a measured quantity was evaporated to dryness in a sand heat, which was carefully regulated, to avoid the decomposition of the muriate of magnesia, if any of that salt were present in the solution *.

* Muriate of magnesia, according to Dr. Marcet, begins to part with its acid at a temperature a few degrees above that of boiling water. This fact explains the observation of Mr. Kirwan, that too great a heat, employed in the desiccation of muriate of magnesia, decreases considerably its solubility in alcohol. (Kirwan on Mineral Waters, p. 275.)

Each

Each specimen of salt was reduced to a fine powder, and was dried, in the temperature of 180° of Fahrenheit, during the space of two hours. This was done in order that the different experiments might be made on precisely equal quantities of salt.

I. *To separate the earthy Muriates.*

(A.) On 1000 grains of the dried and pulverized salt, (except in the case of the foreign salts, when only 500 grains were used,) four ounce measures of alcohol were poured, of a specific gravity, varying from 815 to 820, and at nearly a boiling temperature. To insure the access of the fluid to every part of the salt, they were ground together for some time in a mortar, and then transferred into a glass matrass, where they were digested for some hours, and frequently agitated. The alcohol was next separated by filtration, and the undissolved part was washed, as it lay on the filter, with four ounce measures of fresh alcohol.

(B.) The united washings were evaporated to dryness *, and to the dry mass a small portion of fresh alcohol was added, to separate the earthy muriates from a little common salt, which had been dissolved along with them. This solution might, however, still contain a minute portion of muriate of soda. It was therefore again evaporated, redissolved in hot water, and mixed with a solution of carbonate of soda. By boiling for some minutes, the whole of the earths were precipitated, and, after being well washed, were re-dissolved in muriatic acid. This solution, being evaporated to dryness, gave the weight of the earthy muriates, which had been extracted by alcohol †.

(B. a.) The dry mass thus obtained might consist either of muriate of magnesia, of muriate of lime, or of both. An aliquot part, therefore, was dissolved, separately, for the purpose of assaying it by the usual tests. Sometimes, as in the case of the earthy muriates procured from sea salt, muriate of magnesia alone was indicated, and any further process was rendered unnecessary. Muriate of lime was

* In this and all similar cases, the heat was very cautiously regulated towards the close of the process.

† By the analysis of artificial mixtures of pure muriate of soda with the earthy muriates in known quantities, I afterwards found that the full amount of the earthy muriates was not ascertained in this way of proceeding. The deficiency of the latter salts was about one sixth; but as the error must necessarily have been the same in all, it does not affect the comparison of different varieties of salt, as to their proportion of this ingredient. If the numbers in the 5th column of the table (indicating the total earthy muriates) be increased in the proportion of six to five, we shall then obtain the true quantities in each variety of salt.

in

in no instance found uncombined; but in the majority of cases (as in the earthy muriates obtained from Cheshire) salt was mixed with muriate of magnesia.

(B. b.) To the solution of two earthy muriates was added fully saturated carbonate of ammonia, which has the property of throwing down lime in combination with carbonic acid, but has no effect on the muriate of magnesia at ordinary temperatures. The solution of the latter salt, along with that of the excess of carbonate of ammonia, was therefore separated by filtration; and to the filtered liquor a solution of phosphate of soda was added, according to the formula of Dr. Wollaston*.

(B. c.) By direct experiments I had learned that 100 grains of muriate of magnesia, when thus decomposed by carbonate of ammonia, conjoined with phosphate of soda, give 151 grains of an insoluble ammoniaco-magnesian phosphate dried at about 90° of Fahrenheit. Hence it was easy, from the weight of the precipitate, to calculate how much of the former salt was contained in the mixture of muriate of lime and muriate of magnesia. Thus, if 20 grains of a mixture of the two muriates yielded 15·1 of ammoniaco-magnesian phosphate, it is obvious that the mixture must have consisted of equal weights of muriate of lime and muriate of magnesia.

(B. d.) The estimation of the proportion of muriate of lime, in a mixture of this salt with muriate of magnesia, was sometimes performed in a different way. To a cold solution of a known weight of the two salts, super-oxalate of potash was added; and the precipitate was collected, washed, and dried at about 160° Fahrenheit. Of this precipitate I had previously found that 116 grains are formed by the decomposition of 100 grains of dry muriate of lime. From the quantity of oxalate of lime it was easy, therefore, to infer that of the muriate, from whose decomposition it resulted; and this subtracted from the weight of the two salts, gave the weight of the muriate of magnesia.

II. *To separate and estimate the earthy Sulphates.*

(C.) The portion of salt which had resisted the action of alcohol, was dissolved by long boiling in sixteen ounce measures of distilled water, and the solution was filtered. On the filter a small quantity of undissolved matter generally remained, which was washed with hot water, till it

* See Dr. Marcet's analysis of the Brighton Chalybeate, published in the last edition of Saunders on Mineral Waters.

ceased

ceased to have any action. The weight of the insoluble portion was then ascertained.

(C. a.) By this operation were dissolved, not only the muriate of soda, but all the other salts, insoluble in alcohol, which might be mingled with it. To the solution, carbonate of soda was added; and the liquid, which in most cases gave, on this addition, an abundant precipitate, was boiled briskly for several minutes, in order that none of the earthy carbonates, which were separated, might remain dissolved by an excess of carbonic acid.

(C. b.) The precipitated earths were allowed to subside, and were well edulcorated with boiling water, the washing being added to the liquor first decanted from the precipitate. To these united liquids (after the addition of more muriatic acid than was required for saturation) muriate of barytes was added, till it ceased to occasion any further precipitate. The sulphate of barytes was then washed sufficiently; dried; ignited; and its amount ascertained.

To the earthy carbonates, an excess of sulphuric acid was added in a platina dish, and the mixture was triturated, till all effervescence ceased. It was then evaporated to dryness, calcined in a low red heat, and the weight of the earthy sulphates was ascertained.

(D. a.) The dry sulphates were washed with a small quantity of lukewarm water. In several instances, the loss of weight, thus sustained, was extremely trifling, nothing being dissolved but a very minute portion of sulphate of lime, of which earthy salt, solely, the residue was presumed to be composed.

(D. b.) But in other cases, a considerable loss of weight ensued; and in these, to the watery solution was added a mixture of equal parts of saturated solutions of carbonate of ammonia, and phosphate of soda. A precipitate more or less copious was produced, which was collected, dried at 90° Fahrenheit, and weighed.

(D. c.) By direct experiments I had determined, that 90 grains of this precipitate result from the decomposition of 100 grains of sulphate of magnesia, of such a degree of dryness, as to lose 44 grains out of 100, by exposure to a low red heat. Hence 100 grains of ammoniaco-magnesian phosphate indicate 111 grains of crystallized, on 62·2 of desiccated, sulphate of magnesia*. From the weight of the

* The assumption that crystallized sulphate of magnesia contains only 44 per cent. of water, though it was correctly true with the specimen on which I operated, is below the average, which, I find from several experiments, is about

the ammoniaco-magnesian phosphate, it is easy, therefore, to infer the proportion of sulphate of magnesia in any mixture of the two earthy sulphates.

(D. d.) It was possible, however, that in addition to the sulphates of lime and of magnesia, the quantity of which had been determined by the foregoing process, the specimen of salt under examination might contain also an alkaline sulphate. To decide this point, it was necessary to compare the amount of the acid, deducible from the weight of the sulphate of barytes (C. b.), with that which ought to exist in the sulphate of lime and sulphate of magnesia actually found by experiment. But to make this comparison, some collateral experiments were previously necessary.

(D. e.) By these experiments, I found that sulphate of lime prepared by double decomposition, then calcined in a low red heat, and afterwards dissolved in a large quantity of boiling distilled water, yields, when precipitated by a barytic salt, in the proportion of 175·9 grains of sulphate of barytes from 100 of the calcareous sulphate*. The same quantity of ignited sulphate of lime (=128 grains dried at 160° Fahrenheit), precipitated by super-oxalate of potash, gives 102·5 of oxalate of lime; or, precipitated by sub-carbonate of potash at a boiling heat, 74·3 grains of carbonate of lime†. One hundred grains of crystallized sulphate of magnesia (=56 desiccated) afford, when precipitated by muriate of barytes, 111 or 112 of the barytic sulphate.

(E.) By a comparison of the above proportions with those obtained in the analysis of any specimen of common salt, we may learn whether it contain other sulphates beside those with earthy bases. For example, if the precipitate (D.) consist of carbonate of lime only, and bear to

about one half the weight of the salt. Mr. Kirwan states the water of crystallization to be 53·6 in 100 grains; but this, I believe, a little exceeds the truth.

* This result corresponds, within a fraction of a grain, with one obtained in a somewhat different way by Dr. Marcet, and very nearly with an experiment of my friend Mr. James Thomson, who found the barytic sulphate, precipitated from 100 grains of sulphate of lime by nitrate of barytes, to weigh 173 grains.

† On reversing this experiment, I found that 100 grains of carbonate of lime, saturated with sulphuric acid, and calcined in a low red reat, afford 135 of sulphate of lime. A similar experiment of Mr. Thomson gave 134·6 grains. Dr. Marcet also informs me, that from 93·55 grains of pure marble he obtained 125·95 grains of sulphate of lime, proportions which exactly coincide with those of Mr. Thomson.

the

the sulphate of barytes (C. b.) the proportion of 74 to 175, or very nearly so, we may infer, that no other sulphate is present but that of lime. The same conclusion will follow, if, after having decomposed one half of the watery solution (C.) by muriate of barytes, and another half by oxalate of potash, we find that the sulphate of barytes bears to the oxalate of lime, the proportion of 175·9 to 102·5. Now these proportions were, as nearly as could be expected, obtained in the analysis of Northwich salt; from whence we may conclude, that the only sulphate which it contains is gypsum, or the sulphate of lime.

It must be remembered, however, that the calcareous sulphate, contained in any variety of common salt, cannot be in a state of complete desiccation, but would lose 22 parts out of 100, by exposure to a red heat*. It becomes necessary, therefore, either to increase, in the proportion of 5 to 4, our estimate of the sulphate of lime obtained by the foregoing rule, or, more simply, to assume that 100 grains of sulphate of barytes indicate 73 grains of sulphate of lime, dried at 160° Fahrenheit, =57 ignited.

(F.) When sulphate of lime and sulphate of magnesia were both ascertained, and other sulphates also might possibly be present, as in the varieties of salt from sea water, the calculation became a little more complicated. In this case, after determining the quantity of both sulphates, (by the processes D. &c.) I estimated how much sulphate of barytes they ought respectively to afford; and then compared the estimated quantity, with that which was actually obtained. The earthy carbonates, for example, precipitated from 1000 grains of Lymington salt, which had previously been digested with alcohol, were converted into 31 grains of calcined sulphates, consisting of 19 grains of dry sulphate of magnesia, and 12 grains of dry sulphate of lime. Now from the magnesian sulphate 38 grains of sulphate of barytes should result, and from the sulphate of lime, 21 grains, the sum of which is 59. But the quantity actually obtained was 59·8. There is only, therefore, an excess of 0·8 grain of the actual above the estimated quantity, a difference much too trivial to be admitted as an indication of any sulphate with an alkaline base; and arising, probably, from unavoidable errors in the experiment.

* This I find to be the loss sustained by 100 grains of artificial selenite, dried at 160°, and then ignited. The same quantity of crystallized native selenite, I learn from Dr. Marcet, loses 20·7 grains, by being calcined in a strong red heat.

(F. a.)

(F. a.) If in any mixture of salts, free from the earthy muriates, we are certain that no other sulphates exist beside those of lime and magnesia, their estimation becomes extremely simple. Decompose two equal quantities of the salt in question, the one by muriate of barytes, the other by oxalate of potash. From the weight of the latter precipitate, we may calculate the quantity of sulphate of lime. Suppose, for example, the oxalate of lime (as was actually the case with the precipitate from 1000 grains of Lymington salt) to weigh twelve grains; these denote 15 of sulphate of lime, dried at 160° Fahrenheit, which quantity, if decomposed, would give $20\frac{1}{2}$ of sulphate of barytes. The latter number ($20\frac{1}{2}$), subtracted from the weight of sulphate of barytes actually obtained (say 60), gives $39\frac{1}{2}$ grains for the sulphate of barytes resulting from the decomposition of sulphate of magnesia. The quantity of the latter salt, it will be found therefore by applying the rule already given (D. e.), must be 35 grains.

(F. b.) The same object may be accomplished by decomposing two equal quantities, the one by oxalate of potash, the other by the compound solution (D. c.). From the weights of the precipitates, it is easy to calculate from how much of the calcareous and magnesian sulphates they have resulted.

(G.) When the salt left by alcohol was known to contain muriate of soda and sulphate of magnesia, but no sulphate of lime, the presence of alkaline sulphates was investigated in the following manner. The salt was dissolved in water, and the solution was divided into two equal portions. To the one muriate of barytes was added, and to the other, the compound precipitant of carbonate of ammonia, and phosphate of soda. If the sulphate of barytes, thus produced, bore to the ammoniaco-magnesian phosphate the proportion of 112 to 90, it was concluded that no other sulphate had been decomposed, but that with base of magnesia.

(H.) At one time I expected to have ascertained the quantity of sulphate of soda, in an artificial mixture of that salt with sulphate of magnesia and muriate of soda, by the following formula. To a solution of the three salts, heated to a boiling temperature, I added sub-carbonate of ammonia, which decomposes the sulphate of magnesia only. I had then a solution containing muriate and sulphate of soda, with sulphate of ammonia, and some carbonate of ammonia. This solution was evaporated to dryness, and the mass was sufficiently heated to expel the ammoniacal salts. I found, however, that at this temperature the sulphate of ammonia acted upon the muriate of soda, and produced an additional and not inconsiderable quantity of sulphate of soda.

Having determined, by the foregoing processes, the quantity and kind of the earthy muriates, the amount of the insoluble matter, and the proportion of sulphates, the weights of all these different impurities were added together; and the sum being deducted from the weight of the salt submitted to experiment, the remainder was assumed as the amount of the pure muriate of soda in the specimen under examination *.

Though I purposely refrain from giving the details of the several analyses, which were made according to the foregoing plan, from the conviction that they would be both tedious and unnecessary, yet there are a few circumstances which it may be proper to mention more fully than can be done in the form of a table.

1. The *brine* which I examined was from Northwich, and was sent to me in the state in which it was taken from the spring †. At the temperature of 56° Fahrenheit, it had the specific gravity of 1205. It was perfectly limpid, but lost a little of its transparency when raised to a boiling heat, in consequence of the deposition of a very minute quantity of carbonate of lime and oxide of iron. It was immediately precipitated by muriate of barytes, oxalate of ammonia, and alkaline solutions, both mild and caustic. Eight ounce measures, evaporated to dryness in a sand heat, gave 1230 grains of salt, which, for the sake of distinction, I term *entire* salt. It proved, on analysis, to contain in one thousand parts ‡;

* I have deemed it unnecessary to state, in the table, the quantities of acid and base in the several varieties of muriate of soda. They may readily be estimated from the proportion, deduced by Dr. Marcet, of 46 acid, and 54 soda, in 100 of the pure muriate. In this determination he assumes, that 100 parts of luna cornea, after being melted and heated to redness, consist of 19·05 parts of acid, to 80·95 oxide of silver. This statement agrees very nearly with the recent one of Gay Lussac, who makes 100 parts of silver to combine with 7·60 oxygen, and this oxide to neutralize 25·71 parts of real muriatic acid.

† I have lately been informed that this brine had been pumped out of a rock-salt mine, into which, from the impossibility of obtaining the salt in a solid form, it was allowed to flow. Hence it was fully saturated with muriate of soda.

‡ The specific gravity and proportion of earthy sulphates in Cheshire brine appears to differ considerably in the brine of different springs. See Holland's Cheshire Report, p. 45, &c.

Carbonate

Carbonate of lime and oxide of iron	2
Muriate of lime, and muriate of magnesia, in nearly equal proportions	5
Sulphate of lime	19
Muriate of soda	974
	1000.

2. The *mother liquor,* or brine that remains after separating all the common salt, which it is thought worth while to extract, had the specific gravity of 1208. The dry salt contained,

Muriate of magnesia	35
lime	32
Sulphate of lime	6
Muriate of soda	927
	1000.

3. The *clearings* of the brine, which are raked out of the pan when the salt first begins to granulate, contained in 1000 parts,

Muriate of soda	800
Carbonate of lime	41
Sulphate of lime	159
	1000.

4. Of the substance called by the workmen *pan-scale,* two specimens were analysed, the one containing a large proportion of muriate of soda, the other very little. The first variety consisted of

Muriate of soda	950
Carbonate of lime	10
Sulphate of lime	40
	1000.

The second variety was composed of

Muriate of soda	100
Carbonate of lime	110
Sulphate of lime	790
	1000.

Circumstances, however, are constantly occurring to vary the proportion of ingredients, both in the clearings and in the pan-scale. If, for example, the brine be short of the point of saturation with common salt, it acts, when admitted into the pan, upon the muriate of soda which the pan-scale contains, and we obtain the second variety. But

 if

if the brine be fully charged with salt, it effects no solution of the muriate of soda, carried down along with the gypsum; and then the first species of pan-scale results.

5. The *salt oil*, or mother liquor from sea water, a specimen of which I received from Dr. Thomson, had the specific gravity of 1277. It was abundantly precipitated by muriate of barytes; by pure ammonia, but not by the carbonate; and was not changed by oxalate of potash, either immediately or after an interval of some hours. One thousand parts of the dry salt consisted of

Muriate of magnesia	874
Sulphate of magnesia	70
Muriate of soda	56
	1000.

6. The *salt brine*, or liquor which drains from the Scotch salt, had the specific gravity of only 1188. It was affected by the same tests as the salt oil, but less remarkably. The dry residue contained

Muriate of magnesia	205
Sulphate of magnesia	135
Muriate of soda	660
	1000.

7. The *mother liquor*, or *bittern pan Lymington*, presented, on analysis, an unaccountable variation from the similar fluid sent from Scotland, and gave a much larger proportion of sulphate of magnesia. A considerable quantity of this salt had, moreover, crystallized in the bottle which contained the liquid. Its specific gravity was 1280. One thousand parts of the dry salt contained of

Muriate of magnesia	640
Sulphate of magnesia	260
Muriate of soda	100
	1000

8. The pan-scale from Lymington contained

Muriate of magnesia	29
Desiccated sulphate of magnesia	18
Carbonates of lime and magnesia* ...	127
Sulphate of lime	216
Muriate of soda	610
	1000.

* The proportion of these carbonates I was by an accident prevented from determining.

From

From the very near approximation of the proportions between the sulphate of barytes and ammoniaco-magnesian phosphate, obtained in the analysis of all these products of sea water, to those which result from the decomposition of two equal quantities of sulphate of magnesia, it may be inferred that they contain no sulphate of soda *. For example, to decide whether the Scotch salt contains an alkaline sulphate, or not, I dissolved 1500 grains in a pint of boiling water, and evaporated till fourteen drachm measures only remained, the common salt being removed as soon as it was formed. The residuary liquid was divided into two equal portions, one of which gave 18½ grains of sulphate of barytes, and the other, 14 grains of ammoniaco-magnesian phosphate. The proportion between these numbers is so nearly that which has been already assigned, (viz. 112 to 90,) that we may safely infer the total absence of sulphate of soda. This salt, indeed, is considered as incompatible with muriate of magnesia; but after digesting, for two or three days, 100 grains of the former, with 20 of the latter, evaporating to dryness, and washing the residuum with repeated affusions of alcohol, I found that two grains of the muriate of magnesia had escaped decomposition.

Manchester, June 19, 1809.

XXXIII. *Analysis of the Scammonies from Aleppo and Smyrna; to which are subjoined some Observations on the red Colour given to Turnsole by the Resins.* By Messrs. BOUILLON LAGRANGE and VOGEL †.

THE two species of scammony in question are procured from the root of a plant which grows in Syria. It seems that it is by an incision made in the root that the juice is extracted; each root yields about two drachms only. The juice thus extracted is dried in the sun, and then exposed for sale: at least it is in this way that the finest and purest scammony is obtained. Frequently, however, the inhabitants of Syria and Natolia, in order to procure a greater quantity of the sap, extract it by expression, not only from the root, but from the stalks and leaves: occasionally also they adulterate the scammony by mixing the juice procured from

* I employed more attention in investigating the presence of sulphate of soda in the products of sea water; because this salt is stated to be one of its ingredients by the Bishop of Llandaff, (Chemistry, vol. ii. p. 62,) and by other chemical writers.

† *Annales de Chimie*, tome lxxii. p. 69.

 it

it with that of some other milky and acrid plants, and sometimes they increase its weight by the addition of charcoal or other foreign substances. In order to ascertain that scammony does not contain any of these heterogeneous matters, we ought to break the pieces of the juice, and pick such as are brilliant within, rejecting those which appear too black, burnt, or sandy.

The scammony of Aleppo is light, of an ash gray, brilliant and transparent in its fracture. That of Smyrna is very compact, heavy, and of a deeper colour: it is also more difficult to reduce into powder than that of Aleppo.

Examination of the Scammony of Aleppo.—When the scammony is pure, it melts entirely on a plate of heated iron, and gives out nauseous vapours: when pounded in water, the liquor is of a milky whiteness.

Boiling water makes it run into a mass. The liquor becomes yellow, has a bitter taste, and is neither alkaline nor acid, which proves that this substance is not adulterated with ashes, as some authors assert.

Alcohol at 40 degrees forms a slight precipitate in this aqueous liquor, and with the acetate of lead we obtain yellowish flakes soluble in the nitric acid.

The alcoholic tincture of scammony is of a brownish yellow colour. This liquor reddens turnsole tincture: there remains, after the evaporation, a resin of a yellowish white and transparent.

This resin is entirely dissolved in the nitric acid, which is coloured yellow. The addition of water slightly disturbs the liquor.

This substance is equally soluble in a solution of pure potash, even cold, and the liquor acquires a yellow colour: if this solution be made with the help of heat, the colour is brown. Water even in great quantity does not precipitate resin. Even when saturated by the muriatic acid, it does not separate the resin. This triple compound of resin, acid, and potash, ought to excite the attention of practitioners: it would perhaps be possible in this way to find a solvent for resins which water does not affect.

That part of scammony which is insoluble in alcohol, when dried, acquired a gray colour.

When treated with boiling water, it coloured it yellow, and alcohol precipitated it in white flakes.

In order to determine the proportion of the constituent principles of the scammony of Aleppo, we took 100 parts of this substance, which we dissolved in alcohol: the liquor was coloured yellow. There remained, after the treatment by by alcohol, a matter of a gray colour, which, when dried, weighed 0·26.

The alcoholic solution was evaporated to a syrupy consistence. Cold water precipitated from it a resin forming a homogeneous mass: the supernatant liquor was transparent and colourless. Evaporated to dryness, we obtained a brown matter soluble in water and in alcohol, forming a precipitate by the acetate of lead. This substance seems to be what is called the extractive matter: its weight was found to be 0·2 after having been dried.

The resinous mass separated and dried had a yellow colour, and weighed 0·60.

We afterwards treated the 0·26 of matter which was insoluble in alcohol, with boiling water. There remained after the evaporation a gluey matter, weighing 0·3, having all the characters of gum. The rest was merely the refuse of vegetable matter and a little silex.

The distillation of the scammony of Aleppo presented nothing remarkable. It gave as products, a very acid brown liquor and a light blackish oil. The charcoal resulting from the operation was black, brilliant, and compact; it contained carbonate of potash, carbonate of lime, alumine, silex, and a little iron.

Examination of the Scammony of Smyrna.—The fusion of the Smyrna scammony is less complete than that of Aleppo: instead of going into a mass with boiling water, it became knotty, and the water was dyed yellow. It is neither acid nor alkaline: the acetate of lead precipitates yellowish flakes from it.

100 parts of this scammony taken up by boiling alcohol, although less charged with resin, gave a deeper-coloured tincture than that which was made with Aleppo scammony. We obtained from the evaporation of the alcohol a brownish transparent resin, the weight of which was 0·28. We found 0·66 of insoluble matter in the alcohol. This residue treated with boiling water coloured it yellow: it had a putrid sweetish taste, and alcohol precipitated from it flakes soluble in water. The liquor evaporated left a thick gluey matter like mucilage, soluble in weak nitric acid when warm; precipitating, on cooling, a white pulverulent matter which presented all the characters of mucous acid.

In this experiment, the water had only taken up 0·8 of the matter which was insoluble in the alcohol. The rest was submitted, with the help of heat, to the action of the nitric acid, which dissolves it with effervescence. Ammonia

monia added to this nitric solution formed a precipitate soluble in potash. The potash and the oxalate of ammonia also occasioned a precipitation. This residue is composed therefore of alumine and carbonate of lime, besides the refuse of vegetable matter, and that substance which is insoluble in water and in alcohol, a substance which seems to be oxygenated extract.

This substance incinerated left a whitish powder, soluble in a great measure, and with effervescence, in the muriatic acid. This solution contains alumine, lime, and a little iron. The portion not soluble in the muriatic acid, when treated by potash, gave a siliceous precipitate on the addition of an acid.

The water which had served to precipitate the resin, left, after the evaporation, a brown substance, weighing 0·5, of a bitter taste, attracting humidity from the atmosphere, soluble in alcohol, and abundantly precipitated from the aqueous solution by the acetate of lead. This substance presented all the properties of the extractive principle.

It results therefore from this analysis: 1st. That 100 parts of Aleppo scammony are thus constituted:

Resin	60
Gum	3
Extractive principle	2
Vegetable and earthy matter, &c.	35
	100.

2. That Smyrna scammony contains:

Resin	29
Gum	8
Extractive principle	5
Vegetable and earthy matter	58
	100.

As the resin obtained from both kinds of scammony is much the same, excepting that the Aleppo resin is yellow, transparent and friable, whereas that of Smyrna is more highly coloured and more difficult to pulverise, we thought it would be useful to ascertain if there was any difference in their medicinal properties. Several physicians have since made experiments with both kinds on individuals of similar habits, and have observed no difference in point of purgative properties.

We may conclude therefore, from what precedes, that scammony is a true gum resin mixed with a little extractive matter.

matter. It contains indeed much less gum than the other gum resins, but enough, however, to form a milky liquid with water.

The action of the alcoholic tincture of scammony on turnsole, naturally led us to ascertain whether the property of reddening this blue colour was owing to an acid. Our experiments not having enabled us to acquire a direct proof, we tried some resins in a comparative manner, which we submitted to the following experiments.

1. Sandarach. This resin is converted into a knotty or grumous mass on being boiled with water. The filtered liquor remains clear: when properly evaporated, it slightly reddens turnsole tincture: the taste is bitter: it does not change the infusion of violets, is not precipitated by alcohol or by the acetate of lead; which proves that it contains neither gum nor extractive principle. It is therefore a pure resin.

The resin which had been treated with boiling water, was dissolved in alcohol. This liquor reddens turnsole tincture strongly, and has no action upon syrup of violets.

We also digested sandarach reduced into powder in alcohol, adding to the liquor, when warmed and filtered, boiling water, which precipitated the resin from it. The filtered liquor was turbid upon cooling. It had the strong smell of sandarach resin: its taste was bitter; and its action on turnsole tincture was so weak, that we could not presume the existence of a free acid.

2. Mastich. This substance presents nearly the same phænomena with the above: the resin however runs into a mass in boiling water like turpentine. The water has a bitter taste, and has no action either upon turnsole or upon violet syrup. The resin, on the contrary, reddens turnsole tincture strongly.

3. Olibanum forms in hot water a thick magma, which is separated with difficulty from the liquor, even by filtration. This water has a blackish brown colour, is not precipitated by the acetate of lead, and does not change the colour of turnsole, but alcohol precipitates it in abundance; which proves that this substance is composed of gum and resin.

The alcoholic tincture reddens turnsole tincture strongly.

If we carefully heat in a sand bath the resins which have most action on the colour of turnsole, no acid is sublimed.

When treated with lime according to Scheele's process, no calcareous benzoates are formed.

4. Ammoniacal gum resin, myrrh, gum elemi, galbanum,

banum, tacamahaca, resin of common jalap, Venice turpentine, oil of turpentine, and several other resinous and gummo-resinous substances, gave the same results with those obtained from the scammonies, sandarach, and olibanum. From these facts we may infer that it is still difficult to resolve this question: Is it to the presence of an acid in the resins, that we ought to ascribe the reddening of turnsole?

If the acids alone had the property of reddening the blue vegetable colours, we should not hesitate in recognising the existence of this property in the resins, although experiments have not yet proved it. As to the infusion of violets, over which the resins have no action, this property is found in the sublimated benzoic acid, which strongly reddens turnsole tincture, and which does not change the colour of violets. Has this acid, notwithstanding its solubility in water, any analogy to the resins? We shall abstain from deciding on this subject, although we are induced to believe that *this substance* is a compound of a *vegetable acid*, and a small quantity of resin, which perhaps gives it the concrete state: lastly, as all the vegetable acids are soluble in water, it is still difficult to ascribe to the presence of an acid, the property which resins have of reddening turnsole. It seems probable therefore, until some new experiments prove the contrary, that we may regard it as being one of the characters of the resins to redden the blue colour of turnsole.

XXXIV. *On prime and ultimate Ratios; with their Application to the first Principles of the fluxionary Calculus. By Mr.* MARRAT.

RATIO denotes the relation which two quantities bear to each other.

The two quantities must be of the same kind, otherwise no comparison can be made between them.

The measure of a ratio is obtained by considering what part, or parts, one term of the ratio is of the other. Thus, let a and b denote the terms of a ratio, or let $\frac{a}{b}$ express any ratio; then, its measure is had by considering what part, or parts, a is of b.

Let us denote a by 6, and b by 2, then, $\frac{6}{2}=\frac{3}{1}$, or 3 is the measure of the ratio $\frac{6}{2}$.

If $a=2$ and $b=6$, then, $\frac{2}{6}=\frac{1}{3}$; or $\frac{1}{3}$ is the measure of the ratio of $\frac{2}{6}$; and so on for other quantities.

The

The part b of the ratio is called the *antecedent*, and a is its *consequent*.

The antecedent may be equal to the consequent, and then the ratio is called a ratio of equality; though it would be more proper to say, the *terms* of the ratio are equal:—when the terms of a ratio are equal, its measure is always equal to unity.

If the terms of a ratio vary, the measure of the ratio may have any magnitude whatever; and if one term remain constant while the other varies, the measure of the ratio will vary with the varying term.

Let $\frac{a}{b}$ represent any ratio, and let a remain constant, while b is variable; it is obvious that if b decrease, the measure of the ratio will increase; and, when b is become indefinitely small, the measure of the ratio is then indefinitely near to a; and when b entirely vanishes, the measure of the ratio is exactly equal to a.

On the contrary, when b increases, the measure of the ratio decreases. Again, let b remain constant while a is variable: then, as a increases the measure of the ratio increases, but it decreases as a decreases, and when a entirely vanishes the measure of the ratio is equal to $\frac{1}{b}$.

As another example, suppose we have the ratio $\frac{x+a}{x}$, where x is variable and a constant; the measure of this ratio may vary through all possible degrees of magnitude, as in the preceding example.

1. Let x continually increase; then, the measure of the ratio $\frac{x+a}{x}$ will decrease; and when x is indefinitely great, it will become nearly a constant ratio, or a ratio of equality; that is, the *terms* of the ratio will be nearly equal: because the addition of a to a quantity x which is indefinitely great, will alter the measure of the ratio only in an indefinitely small degree: hence it continually verges to a ratio of equality as a limit.

2. Let x decrease; then, the measure of the ratio $\frac{x+a}{x}$ will increase; and when x is indefinitely small, the measure of the ratio is indefinitely near to a: when x vanishes, the ratio is equal to a, exactly.

From the above illustrations it is exceedingly obvious, that a ratio in which the terms continually vary, or where one is variable and the other constant, or where part of one term is constant, as in this latter example,— it is obvious, I say, I say, that the measures of such ratios never can attain the limits which we have assigned to them: they may, however, continually approximate towards them; and when the measures of the ratios differ from those limits by less than any assignable difference, they may be *said to be* equal.

This being allowed, it is evident that in making use of those ratios, after having supposed that they have attained to such ultimate states, or limits, we continually approximate towards true results; and when the results thus obtained, differ from the true results by a quantity indefinitely small, they may be said to be indefinitely near the truth, and in practice the indefinitely small error may be neglected, as being of no sensible magnitude.

This is all that Newton meant in his first lemma in the *Principia*, where he says that "quantities, and the ratios of quantities, which in any finite time converge continually to equality, and before the end of that time approach nearer the one to the other than by any given difference, become ultimately equal."

It was the calling those results *true* which are only approximations indefinitely near the truth, that gave the *author* of the *Analyst* so much advantage in exposing the errors in the metaphysics of the fluxional calculus; and it was very inconsiderate in *Philalethes* (supposed to be *Dr. Jurin*) to argue that the error occasioned by neglecting a certain *very small* quantity did not affect the *result* of any operation:—that did not in the least tend to overthrow the arguments adduced by the *author* of the *Analyst*, since it was the error in *principle* that he struck at, and not the *quantity* of error that the making use of a false principle might produce.

The conclusions obtained by the method of fluxions are not *absolutely true*, nor did Newton ever consider them as such; they are approximations, which produce no *sensible errors;* and had his host of defenders proceeded no further than this, all the arguments that could have been brought forward against this method must have vanished.

But instead of giving up what was evidently untenable, all the varied arguments which imagination aided by science could suggest, were brought forward in order to get rid of the difficulties which Berkley had pointed out, but without effect; for truth is at all times consistent with itself, and what is once wrong can never be proved to be right.

Newton was desirous of determining the areas of curvilinear figures; this was at all times a great desideratum, and had exercised the talents of philosophers in all ages. The

The first method which the ancients made use of for this purpose was the method of *exhaustions:* an example of the use of this method is given by Euclid in the second proposition of his twelfth book, where he compares the circle and square, and proves that any two circles are to each other as the squares on their diameters: the same method was also made use of by Archimedes in determining the quadrature of the parabola. The argument here made use of is called *reductio ad absurdum,* which, though strictly logical, is often tedious, because every proposition must be divided into two cases, in one of which it must be shown that the former of the two quantities to be compared is not greater than the latter; in the other, that it is not less. It was with a view of shortening this mode of reasoning, that Cavalerius invented the method of *indivisibles:* in this method, every line is supposed to be made up of a number of other lines whose lengths are indefinitely short, and every curvilinear figure is considered as a polygon of an indefinite number of sides:—these principles were in many cases extremely easy and convenient, and produced true results; they, however, often led their followers into perplexities, and sometimes into error.

It was to avoid the tediousness of the method of *exhaustions,* and the errors in the method of *indivisibles,* that Newton invented his method of *prime and ultimate ratios;* the principles of which he laid down in the first lemma of the *Principia,* as observed above. Several eminent mathematicians have endeavoured to demonstrate Newton's lemma: it however certainly admits of no direct proof; it is itself a definition, and requires only to be illustrated, or explained.

By introducing the doctrine of motion into geometry, much has been effected. Newton employed his method of prime and ultimate ratios to the quadrature of all kinds of spaces, by supposing one or more of the sides of the figure to be in motion, and to generate those figures by the motion of their extreme points. The application to right-lined figures was natural and easy; and to apply it to a square, we will suppose that the square is generated by the motion of two right lines perpendicular to each other, and that move parallel to two other right lines placed at right angles.

Let x denote the length of each side at any given position of those lines, and let $\dot{x}$ be the increase, in the length of each side, caused by the motion of the two moveable sides; then, $x+\dot{x}$ will be the length of each side so increased,

creased, and the fluxion of the area is $(x+\dot{x})^2-x^2$; that is $=2x\dot{x}+\dot{x}^2$. Now in order that we may neglect $\dot{x}^2$ without affecting the result of the operation, we must suppose that $\dot{x}$ is a quantity less than any assignable, or that it is only in a nascent state: according to this supposition, the error, by neglecting $\dot{x}^2$, will be extremely small, and will no way affect the fluxionary increase of the square; but, except $\dot{x}$ vanishes, and that would annihilate the fluxional increase altogether, we are obliged to acknowledge that the result is not strictly and logically true.

Again, let x and y denote the sides of a rectangle, and, by the motion of those sides, let them become $x+\dot{x}$ and $y+\dot{y}$; then, the fluxion of the area of this rectangle will be $(x+\dot{x})\times(y+\dot{y})-xy$, or $=x\dot{y}+\dot{x}y+\dot{x}\dot{y}$. Here, also, that the rectangle $\dot{x}\dot{y}$ may be neglected, $\dot{x}$ and $\dot{y}$ must be indefinitely small, or in a nascent state; but even then an error is committed, and, however trifling it may be, the result will not be strictly and geometrically true. Fluxions, then, do not produce results which are exactly true; but, as was observed above, they give us approximations differing from the truth by less than any assignable quantity, however small, and, therefore, may be esteemed as true with respect to their *practical* conclusion. To proceed further would be of no use: the application of those principles to *curvilinear spaces* is given in every book of fluxions. What has been given above may probably be of some use to students, as it may possibly serve to elucidate the principles of a science, which has been the instrument by which almost all the improvements in philosophy have been brought to light. The principle of such a science ought to be established upon a sure foundation; and should what has been said be of any use in removing the cavils that have been made against the fluxionary calculus, a service will be done to philosophy, and the writer of this essay may at least hope to be excused for endeavouring to contribute something towards elucidating the elements of those very useful but too much neglected studies.

I remain, sir,

Your very humble servant,

Boston, Sept. 10, 1810. W. MARRAT.

XXXV. *Com-*

XXXV. *Comparative Examination of the Mucous Acid formed by the Action of the Nitric Acid: 1st, on the Gums; 2dly, on the Sugar of Milk.* By M. LAUGIER*.

M. VAUQUELIN ascertained by his experiments on gum arabic and gum tragacanth the existence of a very considerable quantity of lime in these substances.

The perusal of his experiments suggested the following reflections:—

1. What becomes of the lime contained in these gums when we treat them by the nitric acid, with the view of procuring mucous acid?

2. Is it not combined with the oxalic acid which is formed almost at the same time with the mucous acid?

3. The oxalate of lime being more insoluble in water than the mucous acid, is it not precipitated with this acid, when we wash the residue after the operation? and does it not alter in a sensible manner its properties?

4. What ought to be the means of ascertaining the presence of the oxalate of lime in the mucous acid obtained from the gums, and of separating this calcareous salt from the acid whose purity it injures?

With a view to resolve these questions, I undertook the following experiments:

I digested with eight parts (480 grammes) of pure nitric acid at 360° one part (60 grammes) of gum tragacanth; I heated the mixture until it was reduced into a honey-like substance, and I added a sufficient quantity of water.

The latter would not dissolve a white pulverulent matter, which I gathered on a filter, and which when dried in the air weighed 9 grammes and a half, and this was mucous acid. The liquor containing the soluble portion of the mixture was of a yellow colour. I evaporated it, and did not take it from the fire until I saw it covered with a slight pellicle which was formed at its surface: by and by, upon cooling, the liquor deposited a great quantity of crystals, some in laminæ, others in needles very well defined as oxalic acid. With the view of separating this last acid from the mucous acid, I poured upon the mixture alcohol at 40°, which dissolved the oxalic acid without touching the mucous acid which I collected on a filter. The second portion of mucous acid weighed two grammes 0·10. The alcoholic solution furnished, on a gentle evaporation, a co-

* *Annales de Chimie*, tome lxxii. p. 81.

loured

loured mass, which I redissolved in water in order to obtain whiter and purer crystals.

The mother waters of this second portion of mucous acid and of oxalic acid, contained a mixture of oxalic and malic acids, which I separated from each other, by means which I shall not detail, because they would lead me away from the principal object of my experiments.

The first portion of mucous acid which I obtained weighed nine grammes and a half, it was very white; when dried it had the grumous appearance of starch. This was the substance which I employed in my experiments. I rejected the second portion, because it did not seem to be of the same purity.

With the view of ascertaining the presence of the oxalate of lime in this mucous acid, I mixed one part of the nitric acid as above, with ten parts of distilled water, and poured this mixture upon the nine grammes and a half of mucous acid. I exposed the whole to a heat of 40 or 50 degrees during twice 24 hours, taking care to stir it from time to time, to facilitate the action of the solvent. I decanted the supernatant liquor, in which ammonia immediately produced the precipitation of a white earthy salt, in silky filaments, which had all the physical properties of the calcareous oxalate.

A second portion of weak nitric acid, added to the sediment of the foregoing liquor, and left to itself during the same time, furnished with ammonia a new quantity of oxalate of lime.

It required eight portions of weak nitric acid, successively added, to clear entirely from oxalate of lime the mucous acid submitted to the experiment. Every time the ammonia, when mixed with the decanted and filtered liquor, separated from it a quantity of calcareous oxalate, the proportion of which diminished at each digestion in a striking manner. The ninth portion exhibited but very minute traces of it.

The eight precipitates united together gave a total weight of two grammes three decigrammes.

It was important to ascertain, if this substance, which was foreign to the mucous acid, and whose appearance and physical characters appeared to me to be similar to those of the oxalate of lime, was really this calcareous salt.

With this view I boiled this substance, with a saturated solution of carbonate of potash; and when the reciprocal decomposition of the two salts seemed to me to be completed, I collected on a filter the portion which was deposited,

posited. This sediment, not so white as the first calcareous salt, and in coarser powder, was dissolved with great effervescence in the nitric acid. Its solution, which was of a sharp pungent taste, was not precipitated by ammonia itself, but very abundantly by the oxalate of ammonia.

The liquor which floated above this carbonate of lime, and which contained an excess of carbonate of potash, was supersaturated by the acetic acid and evaporated to dryness; the residue was treated by alcohol, in order to separate the acetate of potash from the oxalate of the same base which is not soluble in this liquid. The mixture when heated for a few moments was thrown on a filter, where the oxalate of lime remained, whereas the alkaline acetate passed through with the alcohol.

The portion insoluble in this liquid was dissolved in distilled water: a drop of this solution mixed with half a spoonful of lime water, formed in it a pulverulent precipitate, evidently oxalate of lime; and the same solution furnished by evaporation crystals of oxalate of potash.

The experiments which I have described, cannot leave any doubt as to the nature of the calcareous salt, the presence of which alters the purity of the mucous acid obtained from gum tragacanth.

The same experiments repeated on gum arabic, and on that which is known in commerce by the name of gum of Bassorah, which is insoluble in water, furnished me with nearly the same results.

I observed, that in proportion as the mucous acid lost by the nitric acid the oxalate of lime which rendered it impure, it assumed a more flaky appearance.

Scheele, to whom we owe the discovery, at first called it saccho-lactic acid, because he obtained it by treating the sugar of milk with nitric acid. This denomination ceased to be convenient, the moment it was proved that it might be procured from the gums by a similar process; and this induced M. Fourcroy to substitute the appellation of mucous acid for that of saccho-lactic acid.

But is the mucous acid furnished by the sugar of milk perfectly similar to that which we obtain from the gums? Is it altered like the latter by containing a remarkable quantity of oxalate of lime; or rather does it contain but the smallest quantity of this calcareous salt; or, finally, is it totally deprived of it? It appeared to me to be interesting to find out a solution for these questions, and I set about applying the process just described to the mucous acid furnished by the sugar of milk.

 I took

I took, in consequence, one part of sugar of milk, which I boiled with eight parts of nitric acid of the same strength with the foregoing. I separated by decantation the first portions of mucous acid which were formed, and I added to the residue a new quantity of nitric acid. A second portion of mucous acid was deposited, which when united with the first gave a total weight of twelve grammes, or the fifth part of the sugar of milk submitted to the experiment.

I remarked that, after washing, this mucous acid, diluted in water, had an appearance equally flaky with that of the gum when it was deprived of its oxalate of lime by the weak acid. This remark inclined me to think that this acid was much purer than that of the gum, and this opinion was confirmed by the nitric acid having had no action on it. It did not take up from it the smallest quantity of oxalate of lime, after a long continued digestion, for the ammonia did not take the slightest effect on the supernatant liquor.

In addition to this, what leaves no doubt as to the perfect purity of the mucous acid of the sugar of milk, is, the circumstance of its easily and entirely dissolving in boiling water. This entire solubility in boiling water proves that it enjoys a greater purity than the mucous acid of gum, even when the latter has been purified by the means above mentioned: in fact, the latter, well freed from oxalate of lime, still leaves, when it is boiled with distilled water, an insoluble flaky matter forming the 0·06 of its weight, which dries into a gray horny semitransparent body, similar in appearance to the mucous substance which connects the molecules of animal concretions, although on burning coals it does not furnish the ammoniacal and fetid smell of animal compounds, and which furnishes on calcination carbonate of lime. The too small quantity which I obtained of it did not admit of my making experiments which would have thrown more light on the nature of this body.

From the facts detailed in this memoir, we may draw the following consequences:

1. There exists a very remarkable difference between the mucous acid procured from gums, and that which we obtain from the sugar of milk by the action of the nitric acid.

2. This difference consists in the first being constantly altered by the mixture of a quantity of oxalate of lime in proportion to that of the earth which the gums contain, whereas the mucous acid of the sugar of milk does not offer the slightest trace of this calcareous salt, and seems perfectly pure.

3. We

3. We may procure the mucous acid from the gum in the same state of purity, by a very simple process, which consists: 1st, in taking from it, by successive digestions in very weak nitric acid, the whole of the oxalate of lime which it contains: 2d, in boiling it in water, which dissolves it without dissolving the flaky matter which the nitric acid does not take up.

4. When thus deprived of substances foreign to its nature, the mucous acid of gum is entirely similar to that of the sugar of milk, enjoys all the properties which characterize this acid, and may be employed with the same advantage in the most delicate experiments which require that this acid should be of a perfect purity.

I am convinced that there is a circumstance in which the mucous acid obtained from gum is mixed with mucite of lime, instead of the oxalate which I have mentioned. This happens when we substitute in the preparation of the mucous acid, the nitric acid diluted in water, instead of the concentrated nitric acid, and consequently when we conduct the operation slowly instead of hastily. It is easy to ascertain the difference of the results which we obtain.

If we employ the weak acid, the mucous acid is at first produced alone, and it is precipitated, carrying with it the lime, with which it forms a salt nearly insoluble, and we may separate it from the mixture before the formation of the oxalic acid, which requires the concentration of the acid. If, on the contrary, we make use of concentrated nitric acid, the formation of the two acids, although always successive, is very thick; and we may easily conceive that in this case the oxalic acid, as soon as it is formed, seizes the lime, in virtue of the more powerful affinity which it exercises on this earth.

I shall add another fact which led me to recognise a singular property in mucous acid, which I intend to examine more in detail than I am able to do at present.

When we gently evaporate to dryness the solution of pure mucous acid made in boiling water, without separating the crystalline sediment which is formed during evaporation, we observe that the moment there is no more liquid, the crystals become yellow, then brown, and are converted into a viscous tenacious-like matter, which undergoes a kind of fusion, and becomes very hard on cooling. The mucous acid which has undergone this change, has a much more acid taste than usual; it is infinitely more soluble in water,—has become entirely soluble in alcohol, and has therefore changed its properties in part. I thought at first that I had thus produced the conversion of the mucous acid either into malic acid or tartarous acid; but the experiments which I made to verify this opinion, do not yet appear sufficient to permit me to venture an opinion on the nature of the change which takes place in the experiment which I have described.

XXXVI. *On the Prussic and Prussous Acids.* By Mr. R. PORRETT, *Junior, of the Tower.**

CONSIDERABLE differences of opinion exist among the most celebrated chemists respecting the composition of the prussic acid; some agreeing with Fourcroy and Vauquelin, that oxygen is one of its component parts, and others with Berthollet and Proust, who dispute its presence. Mr. Proust, in his history of the prussiates, asserts, "That there is no fact that indicates oxygen to make a part of this acid, and that from the well-known affinities of its three elements, added to the circumstances under which it is formed, it can scarcely be thought that it does." This difference of opinion implies a want of some decisive experiments, which may set the question for ever at rest; and those which I am going to relate I am induced to think are of that description.

Some time back, I proposed to myself the discovery of a method of preparing a triple prussiate of potash, in a pure state, which should be free from the objections to which the processes in general use are subject. In reflecting on the means most likely to attain this end, it occurred to me, that I should succeed if I decomposed prussiate of iron by double elective attraction rather than by single, employing, instead of a pure potash, that alkali, in combination with a substance uniting the properties of solubility when combined with potash, strong attraction for oxide of iron, and insolubility when united to that oxide. The only substances I could think of possessing all these requisite properties were the succinic acid and sulphur; as the high price of the former precluded its use for this purpose, I determined to employ the latter. I therefore took one ounce of dry sulphuret of potash, and one ounce and a half of the best prussian blue, previously well washed and powdered, and put them into a Florence flask, two thirds

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xxvii.——The Society voted their silver medal to Mr. Porrett for this communication.

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filled with distilled water; a disengagement of sulphuretted hydrogen, of ammonia, and of caloric immediately took place. The materials were boiled slowly together for three hours, occasionally replacing the water which evaporated. The whole was then thrown on a filter; what remained on the filter was black, and consisted of sulphuret of iron and undecomposed prussiate of iron. The liquid that passed through, I found on trial to consist of triple prussiate of potash, and hydroguretted sulphuret of potash. In order to complete the decomposition of the latter, I boiled the liquid again, for the same time as before, with another half ounce of prussian blue, and when cold filtered it. The filtered liquid (A) was now nearly colourless, and free from hydroguretted sulphuret. On pouring a little of it into a solution of oxy-sulphate of iron, I was very much surprised to find that solution changed to a deep blood-red colour, without any precipitate ensuing, instead of forming with it a precipitate of blue prussiate of iron. So unexpected a phænomenon determined me to undertake an examination of this liquid; with this view I subjected it to the action of the chemical agents mentioned in the following table.

TABLE I. with Liquid A.

Chemical Agents.	Effects.
Paper stained with turmeric	No change of colour.
Paper stained with litmus	Do. Do.
Potash	No disengagement of ammonia, nor any apparent change.
Lime	Do. Do.
Diluted sulphuric acid	An expulsion of sulphurous acid; the liquid becomes slightly opalescent.
Nitric acid (pure) ...	The acid assumes a red colour, but this effect is not permanent.
Oxy-muriatic acid ...	This acid loses its smell.
Muriatic acid (pure) .	No change.
Muriate barytes	A white precipitate.
Tincture of galls	No change.
Nitro-muriate platina	A heavy brilliant ochre, yellow precipitate.

TABLE I.—(Continued.)

Chemical Agents.	Effects.
Muriate gold	Dark olive brown precipitate.
Nitrate silver.......	A precipitate at first white, but quickly passing to yellow, red, and lastly to brown.
Sulphate silver......	A dull white or stone-coloured precipitate.
Oxy-nitrate mercury .	A white precipitate.
Oxy-nitrate lead	A white precipitate.
Supersulphate copper	A dull white precipitate.
Muriate bismuth	No precipitate.
Sulphate iron	No change.
Oxy-sulphate iron...	The solution assumes a deep blood-red colour. No precipitate.

The effects of the sulphuric acid and of the muriate barytes clearly proved the existence of sulphite of potash in the liquid, while that of the oxy-sulphate of iron indicated the presence of some other principle to which the liquid was indebted for its peculiar characters; the separation of this principle in a pure state became therefore a necessary preliminary operation to its examination: after a few trials I succeeded in effecting this separation. The following is the process I employed.

The liquid was evaporated by a gentle heat to dryness; upon the saline residuum alcohol was poured till it ceased to extract any thing: by this means the whole of the sulphite and sulphate of potash was left behind, and the alcohol when filtered held in solution that part only which had the red tingeing property with solutions of iron. The alcohol was now got rid of by distillation, and the salt it left in the retort was redissolved in water. This solution (B) gave the following results with the different metallic solutions.

TABLE II. with Liquid B.

Metallic Solutions.	Effects.
Nitro-muriate platina	A precipitate similar to that in Table I. but in a smaller quantity, and longer in forming.
Muriate gold	Light olive precipitate, some gold reduced.

Nitrate

TABLE II.—(Continued.)

Metallic Solutions.	Effects.
Nitrate silver	A grayish white precipitate, not changing colour.
Sulphate silver......	A clear white precipitate.
Nitrate mercury.....	A copious white precipitate.
Oxy-nitrate mercury .	A white precip. in small quantity.
Nitrate lead	No precipitate.
Oxy-nitrate lead	No precipitate.
Superacetate lead....	No precipitate.
Hyperoxymuriate lead	A slight white precipitate.
Supersulphate copper	A dull white precipitate.
Muriate tin	No precipitate.
Muriate bismuth	No precipitate.
Sulphate iron	No change.
Oxy-sulphate iron ...	Same as Table I.
Oxy-sulphate manganese	The crimson colour disappears; no precipitate.
Sulphate zinc.......	No change.
Nitro-muriate cobalt .	No precipitate.
Nitrate nickel	No change.

It is necessary to remark, that in the preceding table, as well as in Table I, several of the nitrates and muriates were slightly reddened, though not in a degree to be compared with the oxy-sulphate of iron. I have not noticed this in the table, because I am not certain whether this effect was not owing to a minute portion of oxide of iron which might have been introduced into those solutions by the acids employed to make them, as both the nitric and muriatic acids of commerce generally contain some; an excess of nitric acid, even if pure, might also cause this effect, as Table I. may convince us. The solutions with which this effect occurred to me were those of bismuth, silver, mercury, lead, cobalt, gold, and platina.

The liquid B is not altered by exposure to the air.

Its effect on oxy-sulphate of iron is the same, whether this sulphate is neutral, or contains an excess of acid, or is supersaturated with carbonate of ammonia.

Sulphuric acid destroys the colour produced on oxy-sulphate of iron, provided the three liquids are in a concentrated state. If there is much water present, no change ensues.

Having obtained the tingeing principle B, separate from

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the other salts with which it was contaminated, I asked myself to what were its formation and the simultaneous disappearance of the prussic acid, during the second ebullition, owing? I could imagine but five causes for this that were likely to have been efficient, concerning each of which I made a question to be resolved by experiment, viz.

Question I. Was it owing to the complete separation of the oxide of iron from the triple of prussiate by the sulphur, and the subsequent decomposition of the simple prussiate by the heat of ebullition long continued?

Question II. Was it owing to the action of the sulphurous acid produced?

Question III. Was it owing to the action of the sulphuretted hydrogen?

Question IV. Was it owing to a combination of the prussiate of potash and sulphur?

Question V. Was it owing to the de-oxidation of the prussic acid, by the hydroguretted sulphuret?

To answer the first question, it is only necessary to attend to the results afforded by long-continued boiling of the simple prussiate of potash. I shall state these results as I find them recorded by professor Proust.

They are carbonate of ammonia, carbonate of potash, and some simple prussiate that escapes decomposition, even after four or five successive distillations: there is, therefore, no analogy between the products of this experiment and the liquid A; for, had the latter contained carbonate of potash, it must have changed turmeric paper brown; had it contained carbonate of ammonia, it must have done the same, and likewise have given out ammoniacal gas when potash and lime were added; it must also have turned blue the solution of copper; and had it contained prussiate of potash, it must have produced prussiate of iron when added to the green sulphate of that metal: it will be seen by referring to Table I. that none of these effects were produced. Were further evidence necessary of the dissimilarity of the two liquids, it might be mentioned that professor Proust poured alcohol on the saline residuum of his distillation of the prussiate, which took up a part that he found to be prussiate of potash: had any of the tingeing salt B been present, the alcohol must have dissolved that likewise, and it could not have escaped his observation. We have, therefore, ample grounds for negativing the first question.

In order to answer the second question, I passed sulphurous acid gas for a long time through a solution of triple prussiate of potash; the prussic acid was expelled, and sulphite

sulphite of potash formed; but this sulphite was not mixed with any tingeing salt. On the supposition that the disappearance of the prussic acid, in the liquid A, might have been owing to its having been expelled entirely by the sulphurous acid, and that the tingeing liquid resulted from the mutual action of the other principles, namely, the oxide of iron and hydroguretted sulphuret of potash; I subjected a mixture of these materials to long boiling, but could not by this means produce a liquid that tinted oxy-sulphate of iron red. Sulphurous acid gas, passed through water in which prussian blue was diffused, did not in the least affect that compound. These experiments completely refute the opinion on which the second question was grounded.

To enable me to reply to the third question, I passed sulphuretted hydrogen gas for several hours through a solution of triple prussiate of potash, on which it was found to have no effect.

We shall be little disposed to allow that there is any foundation for the fourth question, when we consider the circumstances of the last-mentioned experiment, in which sulphur in the state of the most minute division was offered to the triple prussiate, without any combination ensuing; and also when we compare the effects of the metallic solutions in Table II. with those which would ensue with liquids containing sulphur. But, if any doubt should still be entertained on this subject, the following experiment will perhaps remove it: Into a solution of prussiate of mercury throw some pieces of phosphuret of lime, the oxide of mercury of this prussiate will thus be reduced and separated from the liquid which is to be filtered; some of this liquid poured into carbonate of iron turns it red, the red colour soon disappears, and a white precipitate begins to form; this white precipitate soon changes to green, and if a little nitric or oxy-muriatic acid be now poured upon it, it becomes a perfect blue prussiate of iron. This experiment, in which a liquid turning a solution of iron red was produced without the employment of a particle of sulphur, goes very far to negative our fourth question; and when considered in conjunction with the preceding ones, we can hardly do otherwise than dissent from the supposition which gave rise to that question.

But if the experiment last adduced tends to refute the fourth question, it very strongly supports the fifth; for the changes of colour observable were undoubtedly owing to successive stages of oxidation by the contact of the atmosphere. In confirmation of this question, it may likewise be be asserted, that the long boiling with the hydroguretted sulphuret is a powerful de-oxidating process. But it will be said to me, If it is really true that the prussic acid has been de-oxidated by this process, you ought to be able to recompose that acid from the solution B by oxidation. This struck me very forcibly; and being anxious to give this last proof of the truth of my deductions, I attempted the recomposition of this acid by several oxidating processes for some time without success: I had at last, however, the particular satisfaction of succeeding completely by the agency of nascent hyper-oxymuriatic acid. The method I employed was the following:

A little hyper-oxymuriate of potash was put into the bottom of a glass tube. Over this some of the liquid B mixed with a few drops of diluted sulphuric acid was poured. The heat of a candle was then applied to the bottom of the tube; and as soon as a violent action commenced, the heat was withdrawn: by this process the prussic acid was reproduced, and was proved beyond the possibility of a doubt by the formation of blue prussiate of iron, when poured into a mixture of green and red sulphate of that metal. Blue prussiate may also be produced at once, by substituting for the diluted sulphuric acid, a solution of green sulphate of iron, with excess of acid.

Having thus succeeded in proving that the tingeing principle of the liquid B was sub-oxidized prussic acid, my next object was to obtain that principle in a free state: for we must recollect that we have hitherto considered it only in combination with potash, with which it formed a neutral salt; this circumstance gave me reason for supposing it an acid, and I therefore determined to attempt its separation by abstracting its base by a stronger acid. The following was the process I employed for the purpose.

The liquid B was evaporated nearly to dryness, and put into a retort with diluted sulphuric acid; a receiver was then adapted to it, and about two-thirds of the liquid distilled over by a gentle heat; what remained in the retort was sulphate of potash. The receiver contained a colourless liquid, with a faint, sour, disagreeable smell, and a decided acid taste. This liquor I have named, in conformity with the principles of the new nomenclature, *prussous acid*, and its salts *prussites*, of which the liquid B contained one in solution, namely the prussite of potash.

The effects of the prussous acid on the earthy and metallic solutions, as far as I have tried them, are noted in the following table.

TABLE

TABLE III. with Prussous Acid.

Chemical Agents.	Effects.
Muriate lime	No change.
Muriate barytes	No change.
Muriate gold	The gold precipitated metallic.
Sulphate silver .... / Nitrate silver	Copious white precipitates.
Prussiate mercury ...	No change.
Nitrate mercury	Copious grayish white precipitate.
Oxy-nitrate mercury .	Very slight precipitate white.
Oxy-sulphate iron ...	Solution turns blood-red. No precipitate.
Nitro-muriate platina	No precipitate.
Nitrate lead	No change.
Oxy-nitrate lead	Solution becomes red, but hardly any precipitate formed, unless heated, in which case a copious white precipitate ensues. The red colour disappears, a rapid action takes place between the two liquids, and some of the nitric acid of the solution is decomposed.
Hyper-oxymuriate lead	A slight precipitate, probably of muriate of lead.
Super-sulphate copper	Solution becomes slightly turbid.
Muriate bismuth .. / Nitrate nickel .... / Muriate tin ...... / Nitrate cobalt ...... / Sulphate iron ..... / Sulphate manganese / Sulphate zinc	No precipitates.

I cannot conclude this part of my memoir without giving a more simple and expeditious process for preparing prussite of potash, than that which I at first discovered. It is the following:

Pour a solution of prussiate of mercury into hydroguretted sulphuret of potash, till the mutual decomposition of the two liquids is completed; prussite of potash is instantly formed, and may be separated by filtration from the solid combination of the sulphur and mercury.

I wish

I wish also to observe, that the proportion of prussian blue I have mentioned for boiling with the sulphuret is much larger than is necessary, as I have since succeeded in obtaining prussite of potash when the proportion of prussian blue was only equal to that of the sulphuret, but long boiled with the latter in two distinct and equal portions. The prussite of potash thus obtained is, however, mixed with a much larger quantity of hydroguretted sulphuret than when a greater portion of prussian blue is employed.

Whether the prussous acid can be applied to any use, time and future experiments must decide. It appears to me to be a very delicate test of silver and of iron in solution.

The preceding experiments, by proving the presence of oxygen in prussic acid, give it a stronger claim than it before possessed for being placed among the acids.

The prussous acid possessing stronger acid properties than the prussic is a curious, though not a solitary, instance of the effect of oxygen in diminishing acidity, when its quantity exceeds a certain fixed proportion; in this respect the prussic acid is analogous to the oxy-muriatic.

To recur to the attempt which gave rise to the researches that are the subject of this memoir, I beg leave to state, that I have succeeded in producing pure triple prussiate of potash, by stopping the process before the change which produced the prussite ensued, and by subsequent purification of the lixivium from sulphates and sulphites, by acetate of barytes; from sulphur by acetate of lead; and, lastly, from the acetate of potash thus formed by crystallization; but on account of the complication of this process, I hesitate to recommend it for general use.

Tower, London, April 21, 1809. ROBERT PORRETT, Jun.

P.S.—It is essential to the success of the experiment, in which the prussic acid is regenerated from the liquid B by the nascently per-oxygenized muriatic acid, that the excess of acid remaining in the liquid, after the oxygenizing process, should be neutralized by an alkali previous to pouring it into the solution of iron, which should likewise be perfectly neutral.

May 8, 1809. ROBERT PORRETT, Jun.

XXXVII. *Memoir on the Muriate of Tin. By M.* BERARD, *Ex-Professor of Chemistry in Montpellier.*

The preparation on a large scale of muriate of tin has furnished me with the opportunity of observing some interesting facts. They will serve as appendages to those which have been described by various writers, and perhaps may tend to accelerate the discovery of an uniform and certain method of preparing and using the *composition for dyeing scarlet,* which is a kind of salt of tin.

The solution of tin by the muriatic acid, as described by various authors, and as practised by Baumé, is operated by pouring on one part of the metal in a state of very minute division, four parts of common muriatic acid, and assisting the chemical action by the heat of a sand-bath. The water, which serves as a vehicle to the acid, is decomposed, the oxygen oxidizes the metal, which is then combined with the acid, while the hydrogen is liberated in the gaseous state, carrying off with it some particles of the metal employed, which render it very fetid. But the action is slow, and the solution is effected in an imperfect manner. I observed that a very great part of the acid employed was evaporated and lost; and that if we wished to operate the entire solution of the metal, we must not only add acid in the place of that which was evaporated, but also keep up the action by heat for several days. I tried to perform this operation in the cold way, and two months were insufficient. Bayer and Charlard, in their inquiries upon tin, employed six months in the operation.

M. Chaptal assists the chemical action between the muriatic acid and the tin, by placing, when he has prepared this acid, the metal in the vessels of Woolf's apparatus, in which was the water which ought to receive the vapours. The heat which is extricated produces the best effect, and the action becomes very brisk towards the end of the operation. But this ingenious contrivance still leaves something to be desired, in so far as the acid only dissolves the fourth of its weight of tin, and we must terminate the solution by other means.

We may operate the solution of tin still better by receiving into a large receiver, in which we have introduced a sufficient quantity of the metal in minute division, the vapours of muriatic acid, which are liberated from a mixture of muriate of soda in powder and of sulphuric acid weakened to the 40th degree of Baumé's areometer. On operating a simple

simple distillation in this manner, the vapours of muriatic acid are very easily condensed and combined.

If we direct vapours of oxygenized muriatic acid into a vessel containing tin and common muriatic acid, the solution takes place perfectly, and in a short time. The acid at 20° then takes up one third of its weight in tin.

I tried various mixtures of muriatic acid and nitric acid, from one-sixth part of the latter up to one-tenth; all of them acted on the tin with extreme heat and violence, the substances being forcibly ejected from the vessel. One part of nitric acid, or the aqua fortis of commerce, at 35° of Baumé's areometer, and twelve parts of common muriatic acid, at 20°, form a mixture very well adapted for the solution of tin, which is thereby operated in a very short time. This acid when mixed takes up about one-third of its weight of tin, and the solution extends to the 45th degree.

I tried to make the action of the muriatic acid, and that of the atmospheric air, alternately concur on tin divided into small pieces, in operating its solution, and I succeeded completely. For this purpose I filled a large glass saucer with the tin, and covered it with muriatic acid at 20° for a few hours; I then poured the acid into another vessel, and it had already ascended to the 25th degree. The tin becomes black the moment it comes in contact with the air. There is an absorption of atmospheric oxygen gas, an extrication of caloric, which renders the metal very hot, and a lighted candle when plunged into the saucer is speedily extinguished. As soon as the vessel began to cool, I replaced the acid, which acted with new vigour, and in a short time was as high as 35°. I withdrew it again, in order to give the action of the air to the tin, and I replaced it in the same manner, that it might once more act. I repeated this operation from time to time until the action ceased. In two days the solution was at 45°, which it would attain even in one day if we employed a series of saucers filled with tin: while the acid acts upon some of the vessels, the air acts on the rest, and thus the operation is never interrupted.

The muriatic solution of tin, when recent, combines speedily with oxygen gas from the atmosphere, as Messrs. Pelletier, Guyton Morveau, and other celebrated chemists have observed. It is sufficient to turn upside down a bell-glass, full of atmospheric air, on a capsule or saucer filled with this liquor, in order to see the latter ascend into the bell-glass until all the oxygen be absorbed. The absorption is still more rapid, and becomes almost total in a short time, when

when the bell-glass is filled with pure oxygen gas. In order to facilitate the combination of the oxygen gas with this recent solution, I made to pass through it a great quantity of atmospheric air, by means of a pair of bellows, the pipe of which goes to the bottom of the liquor. When it is not sufficiently saturated with tin, it takes a new portion of it in proportion as it absorbs the oxygen from the atmosphere.

The oxygenated muriatic acid gas is absorbed by this solution with great energy, as Pelletier has very aptly observed. He had even proposed the solution thus saturated with oxygenized muriatic acid gas for dyeing scarlet. I requested several artists to try it, but none of them adopted it. It should seem that the combination of atmospheric oxygen gives it nearly the same properties with those of oxygenized muriatic acid gas. When it has absorbed much of the oxygenized muriatic acid gas, it becomes proper for dissolving a new quantity of tin; and as soon as it has dissolved it, again its state becomes changed, having become capable of absorbing more oxygen.

The muriatic solution of tin at 45° of density gives upon evaporation crystals of muriate of tin. The crystallization takes place the more easily the less recent the solution is, or the greater the quantity of oxygen which the solution has absorbed. The mother water, which swims above the crystals, is of a very great density, particularly after several crystallizations. The density is still more considerable, if we set it to evaporate before exposing it to the air. It is even sometimes slightly smoking, and may then furnish crystals by diluting it with pure water. A flask containing 14 parts of distilled water, contained 28 parts of mother water coming from the first crystals. The same flask contained 31 parts, when this same liquor had given by concentration several layers of crystals. These mother waters are capable of being combined with the oxygen of the atmosphere, when the solution has not been previously saturated with it. It is sufficient to expose them to the air, or to act with a pair of bellows, as already pointed out with regard to the simple solution. This combination produces more crystals; and if the exposure of the mother waters to the air takes place over a very great surface, we obtain a muriate of tin crystallized in very thin and slight scales Baumé had observed this last method of crystallizing. The oxygenized muriatic gas is combined with the mother waters with a good deal of energy, a considerable quantity of caloric is extricated, and after cooling, the liquor goes into a silky a silky mass of crystals of muriate of tin. If we purify the crystals of muriate of tin by solutions in pure water and by crystallization, they assume more consistence and more density.

The crystallized muriate of tin is very soluble in cold water; the solution takes place very speedily, and produces a considerable decrease of temperature. The mean decrease of temperature in the experiments which I made was 9° of Reaumur, the temperature of the atmosphere and that of the substances employed being 5°. The mixture of the mother waters and of pure water produces no change of temperature.

As I had observed that these mother waters became a little fuming on being concentrated, I tried to distil both the highly concentrated mother waters and crystallized muriate, to see if I could not obtain a muriate of tin similar to that which was known by the denomination of *fuming liquor of Libavius*: I obtained at first a weak muriatic acid, and afterwards the muriate passed into the receiver, where it was sublimed into the neck of the retort in a white mass formerly known by the name of *butter of tin*. With the same view I passed muriatic acid gas as dry as possible through the concentrated mother water of muriate of tin: it became fuming, and gave crystals on its mixture with pure water. But I ought to observe that the fuming liquor of Libavius exhales vapours much thicker and more abundant, the whiter and the denser it is.

The combinations of muriatic acid and tin in the state of solution, of crystals and of mother water, are always effected with an excess of acid; and we see from what has been said, that all of them are susceptible of infinite variations in their state. We must not be astonished, therefore, if the results which they produce in dyeing are so uncertain and so different from each other. The least variable state of muriate of tin seems to be that of very white and well-formed crystals. It is in this state that this mordant ought always to be employed in dyeing, by associating it with a greater or less quantity of pure nitric acid, according to the shade which we wish to obtain: such a composition can alone be always uniform and give constant results.

By taking advantage of the facts contained in this memoir, it would be easy to describe a simple and advantageous process for preparing on a large scale the muriate of tin in crystals: I have nevertheless met with some very embarrassing difficulties in the execution, which I have succeeded in removing; and the full description of my labours will be given in a subsequent memoir.

XXXVIII. *The*

XXXVIII. *The Case of a Man who died in consequence of the Bite of a Rattle-snake: with an Account of the Effects produced by the Poison.* By Everard Home, *Esq. F.R.S.**

Opportunities of tracing the symptoms produced by the bite of poisonous snakes, and ascertaining the local effects on the human body when the bite proves fatal, are of such rare occurrence, that no well described case of this kind is to be met with in any of the records that I have examined. I am therefore induced to lay before this Society the following account, with the view of elucidating this subject, in which the interests of humanity are so deeply concerned.

Thomas Soper, 26 years of age, of a spare habit, on the 17th of October 1809, went into the room in which two healthy rattle-snakes, brought from America in the preceeding summer, were exhibited. He teased one of them with the end of a foot rule, but could not induce the snake to bite it, and on the rule dropping out of his hand, he opened the door of the cage to take it out: the snake immediately darted at the hand, and bit it twice in succession, making two wounds on the back part of the first phalanx of the thumb, and two on the side of the second joint of the fore finger. The snake is between four and five feet long, and when much irritated bites the object twice, which I believe snakes do not usually do.

The bite took place at half past two o'clock. He went immediately to Mr. Hanbury, a chemist in the neighbourhood. There was at that time no swelling on the hand, and the man was so incoherent in his language and behaviour, that Mr. Hanbury considered him to be in a state of intoxication, and gave him a dose of jalap to take off the effects of the liquor, and made some slight application to the bites. It appeared on inquiry, that the man had been drinking, but that before he was bitten there was nothing unusual in his behaviour. After leaving Mr. Hanbury, the hand began to swell; which alarmed him, and he went to St. George's hospital. He arrived there at three o'clock. The wristband of his shirt had been unloosed, and the swelling had extended half way up the forearm before his admission. The skin on the back of his hand was very tense, and the part very painful. At four o'clock the

* From the Philosophical Transactions for 1810, Part I.

swelling extended to the elbow, and at half past four it had reached half way up the arm, and the pain had extended to the axilla. At this time Mr. Brodie, who visited him in my absence, first saw him: he found the skin cold; the man's answers were incoherent: his pulse beat 100 strokes in a minute, and he complained of sickness. Forty drops of aqua ammoniæ puræ, and thirty drops of spiritus ætheris vitriolici in an ounce of mistura camphorata, were given to him, but did not remain on his stomach. The wounds were bathed with the aqua ammoniæ puræ, and the arm and forearm had compresses wetted with camphorated spirits applied to them. At five o'clock he took two drachms of spiritus ammoniæ compositus, and 30 drops of æther, in an ounce and a half of mistura camphorata, which remained on his stomach. At six o'clock his pulse was stronger; at half past seven his pulse was very feeble, and 30 drops of æther, and the same quantity of aqua ammoniæ puræ were given in water. At half past eight it was repeated. At nine o'clock he had the feeling of great depression, his skin was cold, his pulse weak, beating 80 strokes in a minute. The dose was increased to 50 drops of both medicines, and repeated. At a quarter past ten o'clock the pain had become very violent in the arm: his pulse was stronger, but fits of faintness attacked him every 15 minutes, in which the pulse was not perceptible, but in the interval his spirits were less depressed. In the course of the evening he had two stools. At half past eleven o'clock I first saw him. The hand, wrist, forearm, and arm were much swelled up to the top of the shoulder, and into the axilla. The arm was quite cold, and no pulse could be felt in any part, not even in the axilla, the swelling preventing me from feeling the axillary artery with any degree of accuracy. The wounds made on the thumb were just perceptible; those on the finger were very distinct. His skin generally was unusually cold. I took some pains to diminish his alarm of danger, and found his mind perfectly collected: he said he hoped he should recover. At one o'clock in the morning of the 18th, he talked indistinctly: his pulse beat 100 in a minute; the attacks of faintness came on occasionally. The medicine was repeated every hour.

At eight o'clock in the morning of the 18th, his pulse beat 132 strokes in a minute, and was very feeble. The swelling had not extended beyond the shoulder to the neck, but there was a fulness down the side, and blood was extravasated under the skin as low as the loins, giving the

the back on the right side a mottled appearance. The whole arm and hand was cold, but painful when pressed; the skin was very tense; on the inside of the arm below the axilla, and near the elbow, vesications had formed; and under each of the vesications there was a red spot in the cutis, of the size of a crown piece. The skin generally over the body had become warm. He was low and depressed; there was a tremulous motion of his lips, and the faintings recurred at nearly the same intervals as in the preceding evening. The last dose of medicine was rejected by vomiting, but some warm wine remained on his stomach. The arm was fomented. At twelve o'clock, in addition to the above symptoms, there was a starting of his limbs. He had attempted to take some broth, but his stomach did not retain it. The skin of the whole arm had a livid appearance, similar to what is met with in a dead body, when putrefaction has begun to take place, unlike any thing which I had ever seen in so large a portion of the living body. An obscure fluctuation was felt under the skin of the outside of the wrist and forearm, which induced me to make a puncture with a lancet, but only a small portion of a serous fluid was discharged. My colleague, Dr. Nevinson, was present at this visit, and we agreed to continue the internal use of the volatile alkali, with the view of rousing the stomach to action, not considering it as having any specific power over the poison. At eleven o'clock in the evening, finding that his stomach did not always retain the medicines, nor even small quantities of brandy, which were given him, I directed the volatile alkali to be left off, and two grains of opium to be given, and repeated every four hours. At this time his pulse was scarcely perceptible at the wrist, the fainting fits were not less frequent. The vesications and red spots were increased in size.

October 19. At nine o'clock in the morning his pulse was scarcely perceptible: his extremities were cold; the vesications were larger, and the size of the arm was diminished. He was drowsy, probably from the effect of the opium. He had taken nothing but brandy during the night. At three o'clock in the afternoon he was more depressed: spoke only in whispers: the vesications were increased: the fainting fits less frequent. The arm was diminished in size, and he had sensation in it down to the fingers. At eleven o'clock at night his pulse beat 130 in a minute, and was low. The opium was left off. A stool was procured by clyster. He was ordered to have a glass

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of camphoaated mixture occasionally, and wine and brandy as often as he could be induced to take them.

October 20. He had dozed at intervals during the night; his spirits were better, and his extremities warmer. At nine o'clock he took coffee for breakfast. He afterwards took some fish for dinner, but it did not remain on his stomach; he therefore took brandy and coffee at intervals, half an ounce at a time, as larger quantities did not remain on his stomach.

October 21. He had slept at intervals during the night, but was occasionally delirious: his pulse 120 in a minute. Brandy and jelly were the only things that stayed on his stomach. The size of the arm was reduced, but the skin was extremely tender.

October 22. He had slept during the greatest part of the night: his pulse beat 98 in a minute: he took some veal for dinner, and brandy at intervals. In the evening his pulse became full and strong: he was ordered wine instead of brandy. The right side of the back down to the loins was inflamed and painful; and had a very mottled appearance, from the extravasated blood under the skin.

October 23. His pulse continued full, and the arm was very painful, though reduced in size. The vesications had burst, and the exposed cutis was dressed with white ointment. Stools were procured by an opening medicine. He took some veal and porter for dinner; the wine was left off. In the evening he had a saline draught with antimonial wine.

October 24. There was no material change.

October 25. His pulse had increased in frequency, but in other respects he was nearly the same. His bowels were opened by medicine.

October 26. The arm was more swelled and inflamed.

October 27. The inflammation of the arm had increased: his tongue was furred, and his pulse was very frequent. He attempted to sit up, but the weight of the arm and the pain prevented him. The arm was bathed with spirits of wine and aqua ammoniæ acetatæ in equal quantities.

October 28. A slough had begun to separate from the inside of the arm below the axilla, and a purging had come on, for which he was ordered chalk mixture and laudanum. In the night he had a rigor.

October 29. The purging had abated; his pulse beat 100 in a minute, and was feeble. A large abscess had formed on the outside of the elbow, which was opened, and

and half a pint of reddish brown matter was discharged with sloughs of cellular membrane floating in it. The lower part of the arm became much smaller, but the upper part continued tense. A poultice was applied to the wound. The lower portion of the arm and the forearm were covered with circular stripes of soap cerate. He was ordered to take the bark, and allowed wine and porter.

October 30. The redness and swelling of the upper part of the arm had subsided: the pulse was 100 in a minute. The purging had returned. The bark was left off: the chalk mixture and laudanum were given, and an opiate clyster administered.

October 31. The pulse beat 120 in a minute. The discharge from the abscess had diminished, the purging continued, and at night he had a rigor.

November 1. The pulse was 120. His voice was feeble; he had no appetite; was delirious at intervals. Ulceration had taken place on the opening of the abscess, so that it was much increased in size. He drank two pints of porter in the course of the day.

November 2. His pulse was very weak; his countenance was depressed; his tongue brown; the ulceration had spread to the extent of two or three inches. Mortification had taken place in the skin nearer the axilla. His stomach rejected every thing but porter: in the night he was delirious.

November 3. The mortification had spread considerably: the purging continued: the forefinger, which had mortified, was removed at the second joint.

November 4. He died at half past four o'clock in the afternoon.

Sixteen hours after death, the body was examined by Mr. Brodie and myself, in the presence of Mr. Maynard, the house surgeon, and several of the pupils of the hospital.

With the exception of the right arm which had been bitten, the body had the natural appearance. The skin was clear and white; and the muscles contracted.

The wounds made by the fangs at the base of the thumb were healed, but the puncture made by the lancet at the back of the wrist, was still open. That part of the back of the hand, which immediately surrounded the wounds made by the fangs, for the extent of an inch and a half in every direction, as also the whole of the palm, was in a natural state, except that there was a small quantity of extravasated blood in the cellular membrane. The orifice of the abscess was enlarged, so as to form a sore on the outside of the arm, elbow, and forearm, near six inches in length. Around this, the skin was in a state of mortification, more than half way up the outside of the arm, and as far downwards, on the outside of the forearm. The skin still adhered to the biceps flexor muscle in the arm, and flexor muscles in the forearm, by a dark-coloured cellular membrane. Every where else in the arm and forearm, from the axilla downwards, the skin was separated from the muscles, and between these parts there was a dark-coloured fluid, with an offensive smell, and sloughs of cellular membrane resembling wet tow, floating in it. The muscles had their natural appearance every where, except on the surface, which was next the abscess. Beyond the limits of the abscess, blood was extravasated in the cellular membrane, and this appearance was observable on the right side of the back as far as the loins, and on the right side of the chest over the serratus major anticus muscle.

In the thorax the lungs had their natural appearance. The exterior part of the loose fold of the pericardium, where it is exposed, on elevating the sternum was dry, resembling a dried bladder. The cavity of the pericardium contained half an ounce of serous fluid, which had a frothy appearance, from an admixture of bubbles of air. On cutting into the aorta, a small quantity of blood escaped, which had a similar appearance. The cavities of the heart contained coagulated blood.

In the abdomen, the cardiac portion of the stomach was moderately distended with fluid: the pyloric portion was much contracted; the internal membrane had its vessels very turgid with blood. The intestines and liver had a healthy appearance. The gall bladder was moderately full of healthy bile. The lacteals and the thoracic duct were empty; they had a natural appearance.

In the cranium the vessels of the pia mater and brain were turgid with blood; the ventricles contained rather more water than is usual, and water was effused into the cells connecting the pia mater and tunica arachnoides. It is to be observed, that these appearances in the brain and its membranes are very frequently found in cases of acute diseases, which terminate fatally.

The following cases were sent from India, to my late friend Dr. Patrick Russell: they arrived after his death, and Mr. Claude Russell very kindly gave them to me, knowing the subject of them to be one in which I had taken an interest. As they correspond in many of the circumstances with that which has been detailed, I have inserted

inserted them in this place, as well as an experiment which I had an opportunity of making in the West Indies, on the effects of the snake's poison on animals.

A boy, a slave of a gentlemen in India, was bitten by a snake called Kamnlee by the natives, in the lower part of the arm, at eight o'clock in the evening. The blood flowed very freely for some time. He died next day at noon in great pain.

A sepoy, 60 years of age, was admitted into the hospital of his regiment, under the care of Mr. Perrin, assistant surgeon, at four o'clock in the afternoon of the 15th of October, 1802, in consequence of his being bitten by a cobra di capello on the back part of the hand. At the time of his admission he complained of pain running up the arm. He immediately took a drachm of eau de luce, and this dose was repeated every half hour, and the same remedy was applied externally as a lotion to the arm and forearm. At four o'clock in the morning of the 16th of October, the pain began to increase, and the arm to swell with great hardness and stiffness, and tumour in the axilla, with much inclination to vomit. He took twelve grains of Dr. James's powder, which brought up a great quantity of bilious matter. He drank copiously of warm water, but no perspiration was induced. He appeared relieved for a short time. At eight o'clock in the morning the arm was distended, painful, and discoloured. He took four ounces of brandy, and repeated it every hour until twelve o'clock, with a drachm of eau de luce occasionally. At this time he was a little revived. The brandy was reduced to two ounces, which were carefully and regularly given every hour, until twelve at noon on the 17th of October, when the arm was more free from pain, but much swelled, hard, and black: his spirits and pulse also were considerably relieved. The eau de luce was now omitted, but the brandy was continued every hour, until twelve o'clock at noon on the 18th of October, when the stiffness and tumor in the axilla had disappeared; the arm was still swelled, but was softer, and less painful. The brandy was omitted: at night he took six grains of Dr. James's powder. On the 19th of October the arm was less, softer, with little or no pain; a blister was formed and burst on the back of the hand, which discharged three ounces of black fœtid pus. On the 20th, an abscess burst on the hand, in the same situation as the blister, which discharged a large quantity of a fluid having an offensive smell. He was directed to

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take a drachm of Peruvian bark in port wine, every two hours. On the 22d the swelling was gone, but the discharge was considerable. From this time the man gradually but slowly recovered, with the loss of the use of his forefinger, which remained permanently extended, and some of the other fingers were affected in a less degree.

In this case, the swelling of the arm was slower in coming on, and less extensive; the pain running up to the axilla, which preceded it, was mistaken for the effect of absorption.

In the year 1782, while in the island of St. Lucia, I made the following experiment:

A spotted dark-coloured snake, about two feet in length, having the poison fangs on each side double, with the corresponding surfaces grooved, so as to form a canal for the poison, was put into a square tin box, open at the top, in which a half-grown rat was confined. The rat expressed great terror, and remained crouching in one corner of the box, with its eyes fixed on the snake, who lay coiled up at some distance, they were allowed to remain a few minutes in this situation: I then raised one end of the box, which caused the snake to slide along the smooth surface, till it came in contact with the rat, which it immediately bit. The rat died in a minute after the bite. I removed it immediately from the box by means of a pair of long forceps. The wounds made by the fangs were marked by two specks of blood immediately below the shoulder blade. On dividing the skin with a scalpel, the cellular membrane under it was found entirely destroyed: the muscles were detached from the ribs, and from a small portion of the scapula. The parts immediately surrounding the bite were exceedingly inflamed; as far as I could trust to memory, the appearances very much resembled those produced on the muscles of a dog's thigh, by the application of white arsenic, in consequence of which, death ensued in about sixteen hours.

Fifteen hours after the death of the first, a second rat was bitten by the same snake. This rat was much irritated, and bit the snake in the neck, so violently, that the latter died in about ten minutes. The rat continued very lively for about six hours, and then died. On examination after death, the bite was found to have been inflicted on the left side of the navel, and the abdominal muscles at that part were in the same state as in the other rat, but in a less degree.

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It appears from the facts which have been stated, that the effects of the bite of a snake vary according to the intensity of the poison.

When the poison is very active, the local irritation is so sudden and so violent, and its effects on the general system are so great, that death soon takes place. When the body is afterwards inspected, the only alteration of structure met with, is in the parts close to the bite, where the cellular membrane is completely destroyed, and the neighbouring muscles very considerably inflamed.

When the poison is less intense, the shock to the general system does not prove fatal. It brings on a slight degree of delirium, and the pain in the part bitten is very severe: in about half an hour, swelling takes place from an effusion of serum in the cellular membrane, which continues to increase with greater or less rapidity for about twelve hours, extending during that period into the neighbourhood of the bite; the blood ceases to flow in the smaller vessels of the swoln parts; the skin over them becomes quite cold, the action of the heart is so weak, that the pulse is scarcely perceptible, and the stomach is so irritable, that nothing is retained in it. In about 60 hours these symptoms go off, inflammation and suppuration take place in the injured parts, and when the abscess formed is very great, it proves fatal. When the bite has been in the finger, that part has immediately mortified. When death has taken place under such circumstances, the absorbent vessels and their glands have undergone no change similar to the effect of morbid poisons, nor has any part lost its natural appearance, except those immediately connected with the abscess.

In those patients who recover with difficulty from the bite, the symptoms produced by it go off more readily, and more completely, than those produced by a morbid poison which has been received into the system.

The violent effects which the poison produces on the part bitten, and on the general system, and the shortness of their duration, where they do not terminate fatally, has frequently induced the belief, that the recovery depended on the medicines employed; and in the East Indies eau de luce is considered as a specific for the cure of the bite of the cobra di capello.

There does not appear to be any foundation for such an opinion; for, when the poison is so intense as to give a sufficient shock to the constitution, death immediately takes place, and where the poison produces a local injury of sufficient

ficient extent, the patient also dies, while all slighter cases recover.

The effect of the poison on the constitution is so immediate, and the irritability of the stomach is so great, that there is no opportunity of exhibiting medicines till it has fairly taken place, and then there is little chance of beneficial effects being produced.

The only rational local treatment to prevent the secondary mischief, is making ligatures above the tumefied part, to compress the cellular membrane, and set bounds to the swelling, which only spreads in the loose parts under the skin; and scarifying freely the parts already swoln, that the effused serum may escape, and the matter be discharged as soon as it is formed. Ligatures are employed in America, but with a different view, namely, to prevent the poison being absorbed into the system.

XXXIX. *On extracting liquid Sugar from Apples and Pears.*

THE high price of sugar in France, occasioned by circumstances connected with the war, has induced the French chemists to endeavour to discover processes by which saccharine substitutes may be extracted from vegetable substances produced in the Old World. On this subject M. Dubuc has lately published in the *Annales de Chimie* various experiments on extracting sugar from apples and pears. As these were chiefly for the purpose of ascertaining the quantity produced by different varieties of apples and pears produced in France, it will be quite sufficient for the English reader to state the process and the general result.

Boil eight quarts of the juice of ripe apples in a brass pan for about a quarter of an hour, and then, for the purpose of neutralizing the acid of the fruit, add, in four separate portions, about two minutes after each other, ten drachms of finely pounded chalk. The chalk occasions an effervescence in the juice, by the escape of the carbonic acid from the chalk in the form of gas. The boiling is to be continued for eight or ten minutes longer, and the mixture to be kept stirred, to multiply the points of contact between the juice and the chalk.

The whites of three eggs beat up in three glasses of cold water are then to be added at once to the mixture, and well stirred into it for the purpose of clarifying the syrup. Let

it

it still boil for a quarter of an hour. The white of the eggs, coagulating by the boiling, entangles the impurities of the juice, which is then to be strained through a flannel strainer supported at the four corners.

When about half-cooled strain it again, that it may be well clarified.

By these operations the juice loses about one-third of its weight. What remains is to be reduced to about one-half of its bulk by boiling; after which the heat must be lowered; but the evaporation must be continued below the boiling point, until the syrup be so concentrated, that on cooling it may be of the consistence of common treacle.

Those who are acquainted with chemical processes will know when it is sufficiently concentrated by observing the pellicle formed on its surface. A vessel capable of containing a quart, or two pounds of water, will contain 2lb. 10 oz. of syrup or liquid sugar.

This liquid sugar is represented as savoury, fresh, and capable of sweetening water very well, or even milk without curdling it.

In one of M. Dubuc's experiments the juice had a milky look, even after the white of eggs was added. To remedy this, he employed twelve drachms of powdered charcoal, and stirred and boiled the mixture for about ten minutes; after which he strained it once through a conical bag, and when nearly cold passed it through the filter a second time, the sediment of the first filtration being left to make the filter the closer.

The success of this experiment induced him to try to obtain the clarified liquid sugar by using chalk and charcoal only, without employing white of eggs. To six quarts of apple juice boiled for a quarter of an hour, he added, at four separate times; two minutes from each other, a mixture of seven drachms of chalk, and one drachm of small coal in fine powder. The boiling was continued till the liquid was reduced one half: when half cooled it was passed through flannel, as directed above, and when nearly cold was strained a second time, and lastly it was evaporated with the above-mentioned precautions.

The process for extracting the saccharine matter from pears differs not at all from the above; but more chalk seems to be required to saturate and separate the acid.

If the fruit be suffered to lie bruised for about 24 hours before expressing the juice, the produce of sugar will be greater,—this process contributing in some way or other to the development of the saccharine principle.

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When the boiling heat is too long continued, the colour of the syrup becomes darker. Does not this serve to suggest that the process might be improved by employing a water bath, instead of applying the heat directly to the boiler or kettle?

XL. *On Musical Time.*

To Mr. Tilloch.

Sir, It has long been a matter of just complaint among musicians, that no method has yet been invented to regulate musical time. The terms Largo, Adagio, Andante, Presto, &c. seem to be mere terms of expression, and not the definite characters of time; for it is absurd to suppose that these terms mean any portion of time whatever, so that the performer is left entirely to use his own taste and judgement. To remove this imperfection, Loulie, a French musician, invented an instrument called The Musical Chronometer, for the purpose of measuring time by means of a pendulum.

But this instrument, though it appears perfect in theory, could never be brought into practice, either from the trouble of adjusting it at the beginning of every movement, or the difficulty which the performer experienced in conforming to mechanical rules.

Another chronometer of a more simple construction has since been invented, consisting of a tape graduated into feet and inches, with a plummet affixed to it. The way of using this instrument is to prefix one of the notes to each movement, and also the length of the pendulum, which vibrates once during its performance. But surely this method must be attended with as much uncertainty as to find the time that a person would be in walking a mile, by finding what time it would take him to walk a yard.

Although these modes of introducing chronometers have hitherto failed, yet I am inclined to believe, that, by a proper use of time-keepers, it will be very easy for the present and future composers to fix the time to their music, so as not to be misunderstood even by a young performer. This may be done very correctly without any other instrument than a pocket-watch which shows minutes and seconds: Thus,

Let the composer take notice of the number of minutes and seconds that elapse during the performance of any movement, according to the time in which he intends it should be played or sung, and let these numbers be written at the beginning of it. The words Largo, Adagio, Andante,

dante, Presto, &c. should still be used as terms of expression,—not as the definite characters of time.

Suppose, for example, that a piece consists of three movements:—the first is performed in 10′, 40″; the second in 6′, 30″; and the third in 8′, 10″. These figures being written at the beginning of each respectively, will convey an exact idea of the author's time to all future performers. And thus a check may be put upon the licentiousness of the fiddle-stick; for some performers are so rapid in their movements, as to neglect both taste and expression. This rapid mode of playing seems to be a growing evil; for it has been said by good judges of the subject, that Handel's music was performed much slower a century ago, than it is in our best concerts at this time.

St. Austin Street,
Sept. 23, 1810. W.

XLI. *Comparative Analysis of Socotrine and Hepatic Aloes. By M.* Tromsdorff. *Extracted by M.* Vogel*.

Besides the two kind of aloes known by the name of socotrine and hepatic, there are two others, one of which, *lucid aloes*, is extremely rare, and the other, *caballine aloes*, is so inferior, and so variable in its qualities, that M. Tromsdorff did not think it worth alluding to in his inquiries.

After having spoken of the natural history and of the extraction of the juice of the plant, an analysis of the two kinds is given, and it is this part of his work that we proceed to notice.

Experiments on Socotrine Aloes.

Action of water. a.) Four ounces of socotrine aloes pounded, were boiled with three pounds of distilled water in a silver vessel. The aloes, being entirely dissolved, presented a transparent liquid of a deep yellow; but, when allowed to cool, a yellow powder was precipitated. When the liquor was quite cold it was decanted and filtered, and a brown transparent mass remained at the bottom of the vessel.

After desiccation, this substance weighed one ounce, and exhibited the following character:—

1. It was transparent, of a brownish yellow, very brittle, and of a bitter taste.

2. It melted at a gentle heat.

* *Annales de Chimie*, tome lxviii. p. 11.

3. It

3. It was insoluble in water, but very soluble in alcohol and in liquid potash.

4. When a lighted candle was applied to it, it burned with a brisk flame.

From the above it is evident that this substance was the resinous part of the aloes. It is also very remarkable, that this great quantity of resin, joined to the other parts of the aloes, is easily soluble in warm water; but it is separated from it on cooling.

b.) The aqueous solution, which contained three ounces of dissolved parts, acted in the following manner:—

1. It was perfectly transparent, of a golden yellow colour: when placed in contact with the air, it became of a brown colour, but without being turbid.

2. It reddened turnsole paper.

3. The alkalis and the alkaline carbonates deprived it of the property of reddening the blue colours, but these solutions produced no other changes in it.

4. Some drops of muriate of iron at the maximum produced a black colour.

5. The nitrates of silver and of lead disturbed it slightly; nitric acid restored its transparency to the liquor.

6. The sulphuric, nitric, and muriatic acids precipitated from it a small quantity of a yellow powder, which acted like a resin, and which did not exceed 0·02.

7. A solution of animal gelatine experienced no change in it.

c.) The aqueous solution was evaporated to dryness in the sand-bath: there remained a mass similar to aloes, and of a bitter taste. It was completely dissolved in hot or cold alcohol.

Ether which was digested with part of this powder was not coloured with it, and did not dissolve a single atom of it.

These properties induced the author to take that part of the aloes for the principle which M. Hermstadt designated by the name of *saponaceous principle*, or soap of plants; the essential character of which is solubility in water and in alcohol, but insolubility in ether.

This saponaceous principle is found in several vegetables, as in saffron, rhubarb, &c.: it is nevertheless probable that there are different species of a more or less bitter taste.

Action of Alcohol. a.) Four ounces of aloes were digested with 16 ounces of alcohol. The solution was complete, and there only remained on the filter 12 grains of ligneous matter which was contained in the aloes.

b.) The

b.) The alcoholic liquor was of a deep yellowish red. When mixed with its weight of water, it was introduced into a retort, and the alcohol was distilled from it.

After cooling, the liquor was not turbid: it was then evaporated to dryness, and the dry mass being redissolved in boiling water, precipitated, after cooling, resin, which when dry weighed an ounce. This experiment in other respects only confirmed the proportion of resin found after the treatment with water.

Experiments on Hepatic Aloes.

Action of Water.—Sixteen ounces of hepatic aloes were subjected to the same experiments with socotrine aloes. The aqueous solution left, upon cooling, three ounces of resin, the water having dissolved 13 ounces of matter.

The solution was also acid, and blackened the muriate of iron at the maximum; it was slightly disturbed by the nitrates of silver and of lead.

When evaporated to dryness, there remained a mass very soluble in hot and cold water, without affording any resinous sediment.

Alcohol dissolves it also, but ether has no action on it.

b.) The three ounces of resinous precipitate being dissolved in alcohol, there remained a residue weighing two ounces insoluble in this menstruum. We shall speak of this presently.

c.) The alcoholic liquor, when evaporated to dryness, left a resinous mass, which had the following properties:

1. Insolubility in warm or cold water.
2. Great solubility in alcohol, in ether, and in a solution of caustic potash.
3. It melted easily at a gentle heat, and was soon carbonized.
4. Great inflammability, burning with a brisk flame.

d.) The two ounces of residue (b), insoluble in alcohol and in ether, were divided into three parts, and treated as follows:

1. Distilled in a retort, there passed into the receiver a fetid oil, with an ammoniacal liquor, and a great quantity of charcoal remained.
2. The concentrated or the diluted acetic acid had no action on it.
3. A boiling solution of caustic potash dissolved the substance entirely. The liquor was not disturbed by an addition of water, but the acids precipitated from it a brown spongy mass, which was somewhat elastic.

This

This precipitate when collected and distilled in a retort yielded an ammoniacal liquor, from which it should seem that the substance in question is nothing more than a coagulated vegetable albumen.

Action of Alcohol.—Four ounces of hepatic aloes were dissolved in alcohol: there remained an insoluble mass, weighing 4½ drachms, which was albumen.

The alcoholic solution was evaporated to dryness, and the residue was boiled with water. It was entirely dissolved; but upon cooling the resin separated from it. By this means we obtained three ounces of saponaceous principle, and 2½ drachms of resin.

From all the above experiments the author has drawn the following consequences:

1. Socotrine aloes are completely dissolved in boiling water. The resinous part is separated from it by cooling.

2. It is also dissolved in alcohol without leaving any residue.

3. The parts which are soluble in water contain more bitter principle than those which are soluble in alcohol, although these last are not entirely free from it.

4. The hepatic aloes differ from the socotrine, in so far as they contain an albuminous vegetable matter, and less resin than the latter.

5. It is not completely dissolved in boiling water, for the coagulated albumen resists it.

6. It is not wholly dissolved in alcohol. This is the way in which we may distinguish it very evidently from socotrine aloes, even when their physical characters are the same.

XLII. *Analysis of Aloes.* By *M.* Braconnot*.

§ I. Aloes are procured from several plants which bear the same name: at Morviedris in Spain the *aloë vulgaris* furnishes three sorts, which only differ from each other in the way in which they are prepared. In the West Indies the substance in question is extracted from the *aloë barbadensis*, which, as well as the foregoing species, is regarded by some writers as a variety of the *aloë perfoliata*, and which is cultivated in the most wretched soils. The *aloë spicata*, a distinct species from the above, also furnishes juice of a good quality; but the purest and most valuable is brought in bladders from the island of Socotra, situated at the entrance of the Arabian Gulph in the Indian Seas: it is obtained

* *Annales de Chimie*, tome lxviii. p. 20.

tained by cutting transversely the leaves of the *aloë perfoliata socotrina,* placing earthen vessels underneath it to receive the juice, which is thickened in the sun.

The aloes which was made the subject of the examination is of a yellowish red, and semi-transparent: it presents, in its fracture, several yellow points which glister on a red ground: reduced to powder it is a fine yellow colour: it has a very bitter taste, and a smell which some persons think is not disagreeable: it does not become electrical on friction.

When exposed to a heat of $80° + 0$ of Reaumur, it begins to soften, and then melts: on account of its being easy of fusion, it is much easier to pulverize it in winter than in summer. If we present a piece of it to the flame of a candle, it melts with a crackling noise, and inflames.

§ II. 50 grammes of aloes were distilled at a heat very gentle at first, and incapable of decomposing it, when the following products were obtained: 1st. Eight grammes of water charged with an essential oil which gives aloes their smell. 2d. At a greater heat there passed over 8·7 grammes of almost colourless water, in which I found one gramme of acetic acid, but no ammonia, on adding quicklime in powder to the liquor. 3d. Five grammes of a heavy red oil soluble in alcohol. 4th. A great quantity of oleaginous hydrogen gas and carbonic acid. 5th. There remained in the retort (which had begun to melt) twenty grammes of a hard charcoal very voluminous and honeycombed, which retained a great quantity of hydrogen, which we saw burnt by exposing it a long time in a crucible at a strong heat in order to incinerate it, which was impossible: it preserved all its blackness, its shining appearance, and a great hardness: it had lost however 12·5, which I attribute in a great measure to the hydrogen. The 7·5 grammes which remained did not contain any potash. This charcoal was treated with muriatic acid: the filtered liquor was precipitated by ammonia, which separated oxide of iron and a small quantity of phosphate of lime: the carbonate of potash precipitated some decigrammes of carbonate of lime.

If we heat nitric acid on this charcoal, we obtain a small quantity of tanning matter which precipitates strong glue.

§ III. Aloes in powder, bruised in a glass mortar with cold water, yielded a mass which, squeezed through the hands, was tacky like turpentine. We succeeded in obtaining a complete solution by adding water in successive quantities, but it required a great quantity; the last portion which remained to dissolve was similar to the first in point of bitterness

bitterness and its other properties: this solution became frothy on being shaken.

One hundred and forty-eight grammes of water at 32°+0 of Reaumur were sufficient entirely to dissolve four grammes of aloes, with the exception of one decigramme of an impure ligneous matter: the liquor became turbid as it cooled, and deposited part of the matter dissolved. This solubility of aloes in water increases in such a manner, in consequence of heat, that we may obtain a syrupy solution, which then ceases to deposit any sediment.

When tried by the re-agents, the solution of aloes in water presented the following effects:

1. It reddened turnsole tincture in a very marked manner.

2. The alkalis and lime water render the colour darker, without precipitating any thing from it.

3. The sulphate of iron produces a brown colour, and a precipitate of the same colour soon afterwards.

4. The decoction of gall nuts forms a flaky yellowish precipitate. The supernatant liquor is much less bitter, and weaker in colour.

5. The subacetate of lead also produces a precipitate in this liquor. The supernatant liquor becomes almost colourless.

6. The nitrate of copper and of lead and muriate of tin produce slight sediments in it, but which do not appear to me to be true chemical combinations; for solutions of muriate of soda and of the other neutral salts produce quite as much. These saline matters therefore act on the solution of aloes in the same manner as upon that of tannin in water, by weakening the action of this fluid on the not very soluble matter which is dissolved in it.

The above solution of aloes, which was of a fine golden colour, was put into three bottles: the first, which held a pint, was entirely filled with it and well corked: the second, which was of the same capacity, was half filled and left open: the third, being a medicine phial, was one quarter filled. In two months and a half the following phænomena were observed: The liquor of the first bottle had preserved its colour without alteration; that of the second was a very dark red, and was discoloured by the oxygenized muriatic acid, which produced a flaky precipitate. In the third a quantity of mucus was formed. The coloured liquor of these two last bottles had acquired a kind of viscosity. It would seem, in fact, that there is a substance produced analogous to gelatine; for the decoction of gall nuts formed in it it a precipitate very abundant in comparison of that which is produced in the recent solution of aloes.

These facts, in my opinion, amply prove that aloes does not constitute a species of the resins.

§ IV. Alcohol at 38° entirely dissolves aloes very speedily, particularly if heat be employed; which announces the absence of gummy or extractive matter in this substance. The filtered liquor was of such a deep red colour that its transparency could scarcely be perceived: water produces an abundant sediment in it of a pale yellow colour, owing to this liquid which is retained in it, for it resumes its primitive brown colour on desiccation.

If we evaporate the alcoholic solution of aloes, we remark that the least motion, the slightest breathing on the liquid, produces a kind of crystallization in it, which disappears and then is reproduced. Although alcohol dissolves this substance very well, this is not the case with the fixed and volatile oils. I exposed to heat a mixture of oil of olives and aloes, and this last substance remained in a melted state at the bottom: the essence of turpentine, which I boiled with the aloes, acted nearly in the same manner: the volatile oil nevertheless assumed a slight amber colour.

§ V. Alkaline solutions dissolve aloes cold and with much facility: combinations are formed in which the bitterness seems in some measure marked. Acids produce in these solutions abundant precipitates which are coloured on desiccation. The volatile alkali diluted in water, also dissolves aloes perfectly: after having filtered the liquor, it was of a deep red colour: and it was evaporated slowly, to drive off the excess of ammonia. In proportion as this liquor was thickened the surface exhibited a continual motion, which seemed to indicate a tendency to crystallization; for we remarked other needles which successively appeared and disappeared. On continuing the evaporation almost to dryness, we obtained crystals in needles attached to a resinous-like mass: on heating this matter with a certain quantity of lime and water, a very evident extrication of ammonia takes place.

§ VI. The weak acids have not a very remarkable action upon aloes: nevertheless they dissolve it better than water, which whitens the solution of aloes in distilled vinegar. The mineral acids act much more energetically upon it. Nitric acid dissolves it very well when cold, and there results a deep red liquor, from which water throws down an abundant precipitate.

Ten grammes of aloes were treated in a retort with eighty

 grammes

grammes of nitric acid at 36°, taking care to administer the fire with caution. There was a brisk re-action, and liberation of abundant red vapours. When they disappeared, the retort was removed from the fire, and the liquor which it contained was of a deep yellow colour. It deposited upon cooling a great quantity of a flaky yellow substance. The liquor, when evaporated to the consistence of honey, was diluted in water and filtered. There remained in the filter a yellow substance, which, after having been washed and dried, formed one fourth of the aloes employed in the experiment. I thought at first that this matter was a portion of the aloes which had escaped the action of the nitric acid: but the following properties soon convinced me that it was an acid with some analogy to the yellow acid, and the detonating matter which Messrs. Fourcroy and Vauquelin obtained by the action of the nitric acid on animal substances, but which differs from it in several respects.

The yellow aloetic acid, when well washed and dried, is of a very fine yellow colour, and extremely bitter. It does not crystallize, reddens blue turnsole paper, and effervesces with the alkaline carbonates.

It has an agreeable aromatic smell, particularly when it is gently heated. It melts like nitre, gives out an aromatic vapour mixed with bitterness, and leaves an abundant charry residue.

When distilled at a gentle heat, it furnished all the usual products of vegetable substances, and ended by detonating, producing at the same time a purple flame. A very abundant charcoal remained, forming the third part of the substance employed.

This acid is not very soluble in water. It required two hectogrammes and a half of this fluid at 10°+0 Reaumur to dissolve entirely two decigrammes of it. This solution was of the fine red colour of arterial blood. The muriate of tin produced in it a precipitate of the colour of wine-lees, and the sulphate of iron heightens the colour.

Fifteen grammes of alcohol at 38° could only dissolve a decigramme of this yellow acid, and the solution was of a very deep red colour.

The mineral acids, warm, dissolve this yellow matter without extricating any thing from it; but it is soon deposited afterwards on account of its insolubility.

Potash forms with it a combination capable of crystallizing, and of a deep-red. This red salt detonates with the violence of gunpowder, either on exposing it to a certain heat, or by touching it with a lighted coal, and leaves after its

its combustion a slight charry trace, and a remarkable smell of prussic acid, which might lead us to suspect the presence of azote.

We may easily produce this red detonating substance, by pouring on the yellow acid of aloes a slight warm solution of caustic potash, which has but a weak dissolving action upon it.

The nitric liquor, from which the yellow aloetic acid has been separated, was saturated by potash. A very small quantity of red detonating matter was deposited at the end of four-and-twenty hours. Nitrate of lime, which was poured upon it, produced an abundant precipitate of oxalate of lime: when well washed and dried it weighed 3½ grammes. The liquor separated from the oxalate of lime was precipitated by the nitrate of lead. The sediment, when treated with one third of its weight of weak sulphuric acid, furnished about one gramme of malic acid, partly dried.

§ VII. It results from the above facts, that aloes is not a gum resin as has been thought, since we do not find in it either the one or the other of these associated principles: nor can we class aloes among the resins, although it resembles them much more than the gums. It is therefore a principle *sui generis*, which I propose, from its properties, to call *resino-amer*. This principle is probably widely diffused, and has its species like other vegetable substances. It is this which had been at first confounded with the resins, which have been sometimes taken for oxygenated extractive matter, and which M. Vauquelin has amply described in his interesting memoir upon different species of quinquina. It is also the same substance which is deposited more or less abundantly from the decoctions of many of the bitter plants, in which febrifuge virtues have been for a long time recognised; such as the *artemisia absynthium*, the *centauria calcitrapa* and *benedicta*, chicory and fumitory *.

It is true that the virtues of these plants have been reckoned less efficacious than the astringent febrifuges: and I am persuaded that in kina, the principle which acts specifically against the fever, and the periodical return of diseases, is owing to the combination of the resino amer with tannin, or some similar substance. My colleague, Dr. Haldats, directed by these views, is about to enter upon some important experiments, of which he will give an account,

* It appears to me that the resiniform matter found in the bile by M. Thenard greatly resembles the *resino-amer* of aloes.

and which may perhaps lead to some great and useful discoveries.

We know that aloes taken internally act as a very active tonic, and are powerfully antiseptic when applied externally. Surgeons daily use aloes in tincture, as a detergent for old ulcers, caries, and gangrenes, which proceed rapidly. Would it have this antiseptic property if taken internally? We know it besides for its febrifuge and purgative virtues:—but it has certainly never been known before, that it ceases to purge the instant it is united to gall-nuts in powder, as I have had occasion to verify.

XLIII. *A Fatal Case of Inguinal Hernia, by* John Taunton, *Esq. Surgeon to the City and Finsbury Dispensaries, and to the City Truss Society for the Relief of the Ruptured Poor.*

To Mr. Tilloch.

Sir, Should the following case of hernia (which was attended with some important peculiarities) be deemed worthy of a place in your valuable Magazine, the recording of it will give me pleasure.

Mr. J. H. æt. 53, an able-bodied man, of a good constitution, has always lived a very regular life, and enjoyed good health, has been subject to hernia in the left groin for many years; for which complaint he constantly wore a truss, which prevented him from suffering any serious inconvenience.

On the 5th of August the intestine passed through the abdominal ring, and formed a tumour of considerable size in the left side of the scrotum. The tumour was very tense and painful on pressure, but was apparently reduced with considerable difficulty by a surgeon who resided near the patient.

The abdomen continued painful on pressure, the pain being referred principally to the umbilicus and region of the stomach, with a sensation of heat. Fomentations and the warm bath were employed without any relief. The bowels remained in a constipated state: no stool could be procured either by medicines taken by the mouth, or by cathartic glysters, several of which were injected.

The hiccough became very troublesome; every thing taken by the mouth was rejected by the stomach; feculent matter was vomited in large quantities; the tongue was much

much furred; the pulse irregular, frequent, and intermitted. There was also great thirst and fever.

The countenance became livid; the eye had that peculiar stare which often precedes death from strangulated hernia; the extremities became cold; the skin generally cold and clammy, in a partial state of cold perspiration.

These symptoms ended in death in 15 days from the first attack; nor does it appear (although the symptoms of strangulated hernia continued from the beginning of the disease) that any attention was ever directed to the hernia, beyond that of pressing the protruded viscera within the external abdominal ring on the first day of the disease.

The medicines were cathartics, opiates, saline draughts, and glysters. Fomentations to the abdomen and the warm bath were also used.

These particulars were related to me by two of the professional gentlemen who attended him, as I did not visit the patient during life, but only attended to examine the parts after death, when the following appearances were noted.

The whole of the thoracic viscera were healthy. The gall bladder was distended with bile, and contained several small biliary calculi.

The liver, spleen, pancreas, and omentum were healthy; the stomach, duodenum, jejunum, and ilium were much distended with flatus. The jejunum and ilium inflamed: the inflammation increased as the intestines were turned downwards to the left abdominal ring, through which a convolution of the ilium had protruded about twelve inches before its termination in the cœcum. The protrusion formed a tumour about as large as a middle-sized apple, and situated on the anterior part of the spermatic process, between the peritoneum and abdominal muscles, so as not to form any tumour visible on the external part of the body; but there cannot be a doubt but it might have been discovered during the life of the patient by pressure.

The stricture was produced by the peritoneum only.

There were not any adhesions between the hernial sac and intestine, nor had the sac suffered from chronic inflammation. The portion of intestine contained in the sac was highly inflamed, but not in a state of gangrene. The intestines below the stricture were empty and much contracted; the inflammation extended along the intestine only about four inches below the part where the stricture was situated.

The rest of the abdominal viscera were perfectly healthy.

The appearances, on dissection, of this case show, that if an operation had been performed early, there is every reason to suppose that the life of the individual might have been preserved.

It also proves the necessity of carefully examining every part of the abdomen usually the seat of hernia, when the symptoms of that disease exist.

I cannot too earnestly recommend the early performance of an operation in strangulated hernia, when it resists the usual means of reduction *. For want of attention to this circumstance alone, many valuable lives have been lost to the community, and their families left unprotected; their widows and orphans become a burden to the public, relying for their support only on parochial assistance.

Sept. 26, 1810. JOHN TAUNTON.

XLIV. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF COPENHAGEN.

THIS academy has proposed the following prize-questions for 1810:—*In Mathematics.* A body which has the form and figure of a cylinder, such as Congreve's rockets, is projected at a certain elevation or angle with the horizon, and is continually impelled by the flames which issue from it. The substance which feeds the fire is gradually consumed, and the weight of the body diminished. This being the case, 1. What is the curve described by that body? 2. If the inflammable matter contained by the cylinder burns in such a manner that the inflamed strata are neither parallel to each other, nor perpendicular to the axis, to what perturbations will the rocket be subject? how are they to be prevented or corrected? 3. As it is necessary that the cylinder be perforated and hollowed, so as to afford the flame a greater surface, and to increase the force of the flame that issues from it, it is required to know what form or figure is most advantageous for the excavation? The society wishes that attention be paid, if possible, to the resistance and pressure of the air; but yet the prize will be adjudged to the best answer to the above three questions.

In Natural Philosophy.—Philosophers have long bestowed great pains on seeking to discover the connexion that subsists between electricity and magnetism, which exhibit phænomena so similar and so different. Modern observations and discoveries have furnished new means of pro-

* Few, if any, would be the fatal cases in this disease, if the time and the performance of the operation were sufficiently attended to.—Hay's Observations on Surgery.

secuting

secuting these researches. The older philosophers have left us numerous experiments on this subject, which do not exactly correspond with the principles of the experimental philosophy of the present day. Some philosophers have made new and important experiments, which have not been sufficiently examined or repeated. The Royal Society, thinking that this part of experimental philosophy may be considerably improved, offers a prize to the writer, who, taking experience for his guide and support, shall give the best exposition of the mutual connexion between electricity and magnetism.

In Philosophy.—1. There are persons who still deny the utility of physical doctrines and experiments in explaining the phænomena of the mind and soul: others, on the contrary, contemptuously reject psychological observations and reasons, in researches which relate to the body, or restrict the application of them to certain diseases. It would be useful to discuss these two opinions, to show and establish more clearly how far psychology and natural philosophy may be combined; and to demonstrate, by historical evidence, what each of these sciences has hitherto contributed to the advancement of the other. 2. The idea of an universal and characteristic language, proposed by Leibnitz, having never been sufficiently explained by himself, and appearing to have not been understood by any person, the question is, to give an accurate and luminous designation of that language, to point out the way that is capable of leading to this desirable object, and at the same time to examine how far the methods hitherto tried in certain sciences, for instance, in mathematics and chemistry, might be correctly applied to philosophy and the other branches of human knowledge. For the best answer to each of these questions the academy offers a gold medal of the value of fifty Danish ducats. Answers to all, except the last, the term of which is extended to 1811, must be sent before the conclusion of 1810, either in Latin, French, English, German, Swedish, or Danish, to M. Buyge, professor of astronomy at Copenhagen.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this Society, on Saturday 21st of July last, Mr. Campbell of Carbrook read some observations on the cause of the antilunar or inferior tide, impressing the Newtonian theory on that subject; and Dr. Thomas Thomson read an account of two natural combinations of hydrogen and carbon, viz. carburetted hydrogen and supercarburetted hydrogen, neither of them containing any oxygen.

XLV. *In-*

XLV. *Intelligence and Miscellaneous Articles.*

To Mr. Tilloch.

Sir, An extraordinary accident lately happened to my neighbour, Mr. Watts, chemist, in the Strand, which has excited the attention of several persons of his profession. I am anxious your ingenious readers should know some particulars respecting it; and if you will indulge me by inserting briefly an account of the affair, I shall feel obliged, as it might in future prevent a more serious evil.

Mr. Watts had taken into his premises, as usual, a carboy of aquafortis, and from some unknown cause, the following morning, his warehouse appeared to be on fire; there being a great quantity of smoke seen issuing from many parts of the building. On entering the apartment, the carboy was on fire, and more than half consumed. I saw the remains of the basket and straw taken into the yard. The air quickly revived the fire, and I have no doubt but I could very easily have blown it into a flame. Particular inquiry was made respecting the straw, and it appears to have been perfectly clean and new. There was no turpentine, or other inflammable spirits, within a foot of the spot where the carboy stood; and it has very much surprised all who have seen it, how the acid could ignite such materials without the aid of other agents. Perhaps some of your scientific correspondents can assign a cause for this strange event, which does not appear to be generally known, and may point out a remedy for preventing a more serious conflagration. I am, sir,

Your obliged humble servant,

R. Teed.

Lancaster Court, Strand,
11 September, 1810.

The French Government has recently ordered all the superb remains of Roman architecture at Nismes to be cleared from the rubbish with which they have been for several centuries confounded. All the modern buildings, which disfigured these monuments of antiquity, have consequently been removed, and the decayed or ruinous parts of the original architecture have been strengthened and repaired.

BETHLEM HOSPITAL.

Application was made to Parliament, in the last session, for an Act to enable the Governors of Bethlem Hospital to exchange,

exchange, with the City of London, the present contracted site of the hospital, for a piece of ground, containing nearly twelve acres, situated in Saint George's Fields; on which spot the unhappy subjects of mental derangement will, in addition to their former advantages, possess such superior requisites of air and exercise as they have never yet enjoyed, which are not only likely to add in a considerable degree to their comfort, but also to accelerate their cure. The plan of the ancient structure is very capable of improvement, and has long indeed required it. The Governors therefore have advertised for plans for the new building, and offered premiums of £.200 for the best, £.100 for the second, and £.50 for the third best designs, in the full confidence of being adequately assisted in their anxious desires to erect an hospital which may be at once a monument of a benevolent and enlightened age, and an honour to a great and distinguished nation.—The present intention of the Governors is to erect a building capable of containing four hundred patients, but not to confine themselves even to that enlarged number, if they shall be enabled, by the liberality of the public, to proceed further in their design.—The funds of the hospital, which are applicable to the purposes of a new building, amount, however, at this time, to little more than £.27,000, while the cost of a new hospital, upon the scale proposed, can hardly be estimated at a smaller sum than £.100,000.—To effect, therefore, so desirable a purpose as that in view, it will be obvious, that nothing short of a liberal subscription on the part of the public at large can suffice. The Governors have therefore published an address, most earnestly entreating all corporate bodies, as well as individuals, throughout the kingdom, to contribute, by their benevolence, more extensive means of relief and cure, than have ever yet been afforded, to the unfortunate subjects of the most afflicting malady with which it has pleased the Almighty in his wisdom to visit his creatures. Their appeal we are confident will not be in vain, in a country whose greatest characteristic is its noble and generous solicitude to alleviate the miseries, administer to the necessities, and heal the diseases of its people.

Subscriptions are received by Richard Clarke, Esq. Chamberlain of London, (the Treasurer of Bethlem Hospital), Bridge Street, Black-friars; and by most of the banking-houses in London.

MATHE-

MATHEMATICS.

It is well known to mathematicians, that the doctrine of solid angles was left in a very imperfect state by Euclid, and has scarcely at all been advanced by subsequent geometers; one of the latest commentators on Euclid, Professor Playfair, having remarked that "we have no way of expounding, "*even in the simplest cases*, the ratio which one of them "bears to another." Dr. Gregory, of the Royal Military Academy, has recently invented a theory of solid angles, which is at once simple, satisfactory, and universal in its application. By means of this theory, the relative magnitudes of solid angles may be ascertained, not only when they are of the same class,—as those formed by the meeting of three planes, those by the meeting of four planes, the angles at the vertices of cones, &c.: but angles of one class may be compared with those of another, with respect to magnitude; and their mutual relations determined, by processes as obvious and elementary as the usual operations in Plane Trigonometry. He finds, for example, that the solid angles of the regular tetraëdron, octaëdron, hexaëdron, and of the right-angled cone, are denoted by the numbers 87·73611, 216·35185, 250, and 292·89322, respectively; the maximum limit of solid angles being expressed by 1000.

Having been favoured with a most exquisite original portrait of Buchanan, by Titian, we have procured it to be engraved by Woolnoth in his best manner, as one of the embellishments of the present Number. Such of our readers as wish to possess proofs (of which a few have been worked off) of this admirable likeness may obtain them from the Publishers of the Magazine, at five shillings each.

Notice respecting the Preface to the 4th Edition of the Encyclopædia Britannica.

In writing the preface to the Encyclopædia Britannica, some mistakes having occurred, relative to the writers engaged in the publication, the conductors of that work beg leave to assure their subscribers and the public, that they are wholly unintentional; as it never could be their design to detract, in any way, from the merits of the authors whom they employed. They understand, in particular, from Dr. Kirby, that the article *Physiology*, attributed by mistake to another gentleman, was written by him; and that the following articles, viz. *Farriery*, *Geography*, *Geology*,

Geology, *Materia Medica*, *Prescriptions*, *Russia*, *Amusements of Science*, and *Spain*, were also contributed by him.

This notice is to be printed separately, and may be had by the subscribers to the Encyclopædia, from the Publishers of that work in London and Edinburgh.

Rupture is so general a disease, and in its aggravated state so frequently and suddenly fatal, that every information which promises relief, particularly from the regular practitioner, ought to be universally known. We therefore give the following extract from a work lately published by Mr. Edward Geoghegan, in which an improvement in the treatment is suggested.—" I place the patient in a recumbent position, with his shoulders a little raised to relax the trunk, but the pelvis not raised, as that would put the fasciæ on the stretch. The knees are to be drawn up. If the parts have not been irritated by handling them, or the body disturbed by jolting it about, or by any such roughness, I proceed directly to apply cloths wet with cold water, expose the entire body naked to the air, the doors and windows being open. This practice usually succeeds within an hour*. If it does not, I surround the hernia with my hand or hands at about its middle, in the way that I would grasp a gum elastic bottle, to press out its air or other contents, by gently approximating its sides, always holding in view, that the tumour is to be *emptied*, and not *pushed up*. I never press the hernia in any direction, or at all towards the abdomen. When it is small, it may be done with the finger and thumb of one hand. Having applied the hands, I do not remove them for fifteen or twenty minutes, aware that reiterated impulses irritate, and that the effects of compression are lost each time that it is intermitted."—In cases of great pain and tension he omits this practice.—This practitioner differs from every other so far, as that the usual directions are to press the protruded bowel up towards the belly, which he takes great pains to show is improper, and insists that the contents should be merely squeezed out.

LECTURES.

Dr. Adams's Lectures on the Institutes and Practice of Medicine will commence on Monday the 8th Oct. next, at Eight o'clock, at Dr. Anderson's Lecture-rooms, 47, Frith-street, Soho.

* In some cases where I could not immediately attend, I have directed that cold applications should be used until my arrival: and after an hour, they informed me that they were seized with a shivering, that they heard the wind rush out of the hernia, and that they were instantly relieved.

On

On the same day Dr. Anderson will begin his Course of Lectures on Practical Chemistry.

Lectures on Materia Medica form a part of the above Courses.

Further particulars may be known by applying to Dr. Anderson, as above, or to Dr. Adams, 2, New Bridge-street.

Dr. Clutterbuck will begin his Autumnal Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, &c. on Friday the 5th October, at Ten o'clock in the morning precisely, at his house, No. 1, Crescent, New Bridge-street; where further particulars may be had; or at the General Dispensary, Aldersgate-street. The Lectures are given daily; Theory and Practice, Mondays, Wednesdays, and Fridays; Materia Medica and Chemistry, on Tuesdays, Thursdays, and Saturdays, at the same hour.

George-street, Hanover-square, and St. George's Hospital.

On Saturday, Oct. 6, a Course of Lectures on Physic and Chemistry will recommence in George-street, at the usual morning hours: viz. Therapeutics at Eight, the Practice of Physic at Half after Eight, and the Chemistry a Quarter after Nine, by George Pearson, M.D. F.R.S. Senior Physician to St. George's Hospital, of the College of Physicians, &c.

Clinical Lectures are given, as usual, on the Patients of St. George's Hospital, every Saturday morning at Nine o'clock.

LIST OF PATENTS FOR NEW INVENTIONS.

To Charles Williams, of Gravel-lane, Southwark, mill-wright, for a machine for grinding or cutting malt, splitting beans, and any other kind of grain, and various other articles.—Aug. 2, 1810.

To Marc Isambard Brumel, of Chelsea, for certain machinery for the purpose of making or manufacturing shoes and boots.—Aug. 2.

To Thomas Collins, London, warehouseman, for an improved mode of making ladders, which being formed of different pieces, and capable of being put together by socket joints, will be found extremely useful for the purposes of escalade, engineering, escapes from fire, erecting of buildings, and for all other purposes for which ladders of any description are necessary.—Aug. 10.

To

To William Whitmore, of Dudmarton, Salop, esq. for a magnetic toy to facilitate the teaching of children to spell, read and cypher, in any tongue, with ease to the teacher, pleasure to the children, and proportional expedition.—Aug. 14.

To Peter Durand, of Hoxton Square, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for a method for preserving animal food, vegetable food, and other perishable articles, a long time from perishing or becoming useless.—Aug. 25.

To James Walker, of Wapping, in the county of Middlesex, ship-chandler, for his machine or vessel for the safe conveyance of gunpowder, and for its preservation from injury by damp.—Sept. 7.

To James Weldon, of the county and city of Litchfield, engineer, for his further new improvements on a mill for grinding bark and other articles.—Sept. 7.

To Joseph C. Dyer, of Boston, State of Massachusetts, one of the United States, now residing in the city of Westminster, merchant, who, in consequence of a communication made to him by a certain foreigner residing abroad, is become possessed of a machine for cutting or removing all the various kinds of furs which are used in hat-making from the skins or pelts, and for cutting the said skins or pelts into strips or small pieces.—Sept. 7.

To David Mathews, of Rotherhithe, engineer, for his improved method of constructing and building locks with a groin or Gothic conic arch. Also an improved form of the gates, and an improved method of opening and shutting the same.—Sept. 7.

To Joseph Johnson, of the county of Surry, gentleman, for his new mode of communicating intelligence from one apartment of a house to another by means of machinery or apparatus, which he denominates a domestic telegraph.—Sept. 17.

To Jonathan Varty, of Liverpool, coach-maker, for his improvements in the axle-trees of carriages.—Sept. 17.

To Peter Brown, of Henrietta-Street, Covent-Garden, Middlesex, gentleman, for his new construction of buoys for ships or vessels, and for mooring-chains or similar purposes.—Sept. 26.

To Richard Seaton, of Berwick-Street, Middlesex, liquor-merchant; and Thomas Rice, of Whitecross-Street, Middlesex, spring roasting-jack-maker, for their new burner upon an improved construction, applicable to all kinds of lamps.—Sept. 26.

METEO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For September 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
August 27	58	74°	62°	30·08	63	Fair
28	57	74	64	·13	74	Fair
29	58	71	67	·10	65	Fair
30	59	75	68	29·98	52	Fair
31	66	77	69	·90	53	Fair
Sept. 1	68	78	70	·85	61	Fair
2	70	80	69	·90	60	Fair
3	69	72	58	·80	58	Cloudy
4	58	62	54	·72	26	Cloudy
5	55	68	58	30·00	55	Fair
6	56	68	51	·00	45	Cloudy
7	50	64	49	·32	51	Fair
8	49	64	50	·12	55	Fair
9	51	68	56	·05	50	Fair
10	53	68	54	29·91	41	Fair
11	50	59	50	·70	0	Rain
12	58	58	45	·61	0	Rain
13	48	63	47	30·06	36	Fair
14	52	68	48	·20	42	Fair
15	47	61	51	·38	33	Cloudy
16	53	64	57	·28	42	Cloudy
17	57	67	58	·09	38	Fair
18	58	68	49	·05	30	Cloudy
19	51	67	56	·10	22	Cloudy
20	56	63	59	·10	10	Foggy
21	58	66	58	·05	15	Foggy
22	56	68	56	29·95	32	Fair
23	57	62	52	·96	20	Showery
24	56	66	54	30·09	42	Fair
25	58	69	56	·11	80	Fair
26	57	67	55	·05	82	Fair

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

XLVI. *On the New Mountain Barometer.* *By Sir* Henry C. Englefield, *Bart. F.R.S. and F.S.A.*

To Mr. Tilloch.

Sir, The experience of three years having ascertained the convenience and utility of the mountain barometers, made on the principles of which a description, drawn up by me, was inserted in your Journal, (vol. xxx. p. 46,) I am induced to address you again on the subject; both to inform the public of some improvements made in their construction since my former letter, and to propose some mode of collecting, for general benefit, the observations made by individuals.

The improvement in the construction is principally in the cistern. It had been found that when exposed to great motion in an unfavourable position, which in long journeys is not easily avoided, the agitation of the mercury had several times cracked the tube towards the top, in a fissure scarcely perceptible to the eye, yet sufficient to let in slowly a small portion of air. To remedy this inconvenience, the cistern has now a bottom of leather on which a screw presses in the usual mode, so as to force the mercury nearly to the top of the tube when packed for carriage. This screw is to be unscrewed as far as it can, when the barometer is prepared for use; and the leather bag is so adjusted, that there is no reason to fear that the capacity of the cistern thus unscrewed for use, will ever be sensibly different from itself at different times. It may be just mentioned, that when the barometer is carried by a careful person, it is by no means necessary to screw up the bag between every station; as, when unscrewed, the instrument is in precisely the same state that it always was, in those of the first construction*.

Mr. Jones, at the desire of several gentlemen, has endeavoured to add a gauge point and adjustment to keep the mercury in the cistern ever to the same height, as in other mountain barometers, but such addition has been found in practice productive of more inconvenience than advantage. He now, therefore, measures the content of every tube separately, and engraves on the mounting the correction to be made to the results, as stated in the former paper; and by this method it is presumed that all errors from the want of a gauge point must be prevented. Mr. Jones has

* The screw which frees the cistern for use, is protected by an outer cap from being spoiled by idle curiosity, or injured by a blow, which often happened to those barometers where this screw was unprotected.

now sold above 150 barometers of this construction. Of these, it cannot be doubted that by far the greater part has been purchased by gentlemen both able and desirous to use them for the purpose of measuring heights; and I know that a great number of valuable observations have been made with them in different parts of our islands. While, however, these observations remain in the hands of the observers, the public is little benefited by them; and I doubt not that if it were generally known that a deposit for them was provided, all those gentlemen who have made observations of altitudes with these, or any other good mountain barometers, would readily send their observations, and contribute their part to the common stock of valuable information which would be deduced from the publication either of the observations themselves or the results of them.

For this purpose Mr. Jones, late of Mount-street, now of Kenton-street, Brunswick-square, the same ingenious artist who made these barometers at first under my inspection, has kindly consented, at my request, to receive and arrange all such observations as may be transmitted to him (post paid or franked) by the gentlemen who have made them; and I shall be happy not only to assist him in computing them, but will readily superintend the publication of them, either in the literary journals, or in a separate work, as may in process of time appear the most eligible. It seems the most desirable that the names of the observers should be published with their observations, as giving the stamp of authenticity to them: this, however, will be done, or omitted, as the several contributors may wish.

As it is to be hoped that the communications may be numerous, it will materially diminish the labours of arrangement, if a general form be adopted in sending the observations; and it is hoped that the specimen here annexed will be found convenient to the observers themselves, as well as to those whose province it may be to collect them.

Extensive geological observations would be in this case out of their place; yet it might be useful, and productive of little additional labour or trouble, if the soil of the spot where each observation was made could, if possible, be specified. Another observation nearly connected with that of the barometer and thermometer for altitudes, is the temperature of the waters at or near the places of observation. Wells of 40 or 50 feet deep are, for this purpose, more to be depended on than springs, which often run at so small a depth below the surface of the ground as to be much affected by the heat and cold of summer and winter.

It

It will, however, be best to make observations as often as possible both on the one and the other, as it has been ascertained, both by Mr. Cavendish and the late Dr. Hunter, that the temperature of the waters at any given place is a most accurate measure of its mean heat; a determination of which is not only an object of considerable curiosity in itself, but of very great consequence in an agricultural point of view.

The annexed form for registering the observations scarcely requires an explanation. The first column is for numbering the observations, which extremely facilitates the reference to them. The succeeding columns are fully explained by their titles. The last, called Results, is added, in order that those persons who choose it may place in one view the observations, and the altitudes deduced from them. Printed sheets in this form, ready for use, may be had of Mr. Jones. The back of each page is left blank, for the convenience of inserting any other notes or observations.

I am, sir,

Your humble servant,

H. C. ENGLEFIELD.

N°	Place of Observation.	Weather.	Wind.	Time.	Barometer.	Th. A.	Th. D.	Results.
1	October 6 Steyne, Brighthelmston . . .	Sun	NW	2·15	30·268	63	63	1 and 2
2	Stand on Race-ground	Do.	Do.	3·0	29·870	62	62	400 feet.
3	Stand again	Do.	Do.	3·32	29·861	62	61	
4	Steyne again . . .	Do.	Do.	4·15	30·278	61	61	

XLVII. *On the Land Winds of Coromandel, and their Causes. By* WILLIAM ROXBURGH, *M.D.**

THE land winds on the coast of Coromandel are those hot winds which blow at a particular season of the year, and hour of the day, from the western hills, commonly called the Ghauts, towards the Bay of Bengal. In the more inland countries, as above the Ghauts, they are not

* From *Transactions of the Medical Society of London*, Vol. i. part I. just published.

 confined

confined to any regularity, though they are felt sometimes with a great degree of severity, and for hours together.

I understand also that in the upper parts of Bengal they are sometimes experienced very severely; but whether from the west or the northward, or in what part of the year, I have not been able to ascertain. As far as this only tends to prove the insufficiency of the denomination, it would signify little, although in other respects it would be of more moment.

As they are generally supposed to be peculiar to this country, and are felt during several months in the year, we should imagine their history and causes to have been perfectly investigated and understood; but, I know not why, neither the one nor the other have as yet been satisfactorily explained.

The most plausible reason generally given for the great accumulation of heat in them is the heat of the season in which they prevail, and the long tract of country over which they have to pass. That this, however, is not the true cause, it shall be my endeavour to demonstrate; to which I will add an attempt to point out the most probable one, founded on known chemical principles.

Respecting the theory I have to offer, I regret that it has found but few patrons in this country, which, however, I flatter myself may be ascribed more to the manner in which it has been proposed, than to the foundation on which it is constructed.

In order to facilitate the explanation of my sentiments, as well as to show that the land winds really deserve some attention from the philosopher, I shall briefly recount the phænomena accompanying their beginning and progress, as well as the effects by which they are generally followed.

Could my pen equal my sensations, I should be able to paint their effects in the most lively colours, aided by eight years experience in a country the most noted on the coast* for their intensity.

The land winds are preceded in the latter end of March or in the beginning of April by whirlwinds, which between eleven and twelve o'clock at noon hurry in various directions, mostly from west to east, towards the sea. These are called by the natives Peshashs or Devils, because they sometimes do a little mischief to the lighter buildings.

About the same time, or a little after the appearance of the whirlwinds, we may observe all ranges of hills gar-

* Samulcotah in the Northern Circars.

nished

nished as it were with clouds, which become daily darker and heavier, until they discharge themselves with much thunder and lightning in a heavy shower of rain. After this marked phænomenon the land winds set in immediately with all the violence of which they are capable.

Their commencement is generally in the latter end of April, or beginning of May, and their reign lasts to the earlier days of June, during which period they generally exert their violence from ten or eleven o'clock in the morning until about three or four o'clock in the afternoon.

In this season the atmosphere is commonly hazy and thick, except that in the evenings and nights the sky is serene and clear, provided the land winds do not continue the whole day.

The rising sun which portends a land wind day appears of a fiery red, and as if involved in mist, which mist is changed afterwards into clouds that lie heavy on the Ghauts.

The land wind of each day is almost always preceded by a long calm, and immediately by a cloud of dust.

Their diurnal violence is terminated along the coast about two or three o'clock, by the setting in of the sea-breeze, which wafts delight and health as far as its influence extends, which is not more than ten or twelve miles inland. An abatement of their intensity from thence to the Ghauts is all that can be hoped for.

The sea-breeze regularly begins in the afternoon at one or two o'clock, blowing pretty steadily until sunset, when it dies away gradually, and at sunrise it is again perceptible, though weakly.

When I say its influence is only felt ten miles inland, I do not wish to be understood that it does not extend further: I mean only its powerful refreshing properties, which it loses in proportion to the distance from the sea, and in an inverse ratio to its strength, which is not great. In general it arrives at thirty miles distance from the sea in the evening, and is then only agreeable by the ventilation it effectuates.

In the country above the Ghauts, as in Mysore, the east wind prevails also in the afternoon, but from a period much earlier, or cotemporaneous with the sea-breeze on the coast, which renders it clear that this inland breeze either does not extend further than to the Ghauts, or really originates there; a point which deserves to be ascertained, as another phænomenon depends upon this circumstance.

 Should

Should the sea-breeze fail, as sometimes happens, the land wind decreases gradually until it dies away in the beginning of the night, which, on account of its calmness, is dismal to a degree: next morning, a little motion of the air is again perceptible, but at the usual time the wind sets in as strong and hot as the day before. Every thing we put our hands upon is then distressing to the touch, which must be the case when the temperature of the body is inferior to that of the atmosphere. This we experienced for almost a fortnight in the year 1799 in the Northern Circars, when the thermometer at eight o'clock in the night stood at 108°, and at noon at 112°. Shades, globes, tumblers, then very often crack and break to pieces, and the wooden furniture warps and shrinks so much, that even the nails fall out of doors and tables, &c. In their greatest intensity, however, I have never seen the thermometer rise higher than 115°, viz. in the coolest part of the house, though some say they have observed it at 130°.

The Ghauts, and the hills at no great distance from them, are then seen lighted all night by spontaneous fires, and often in a very picturesque manner.

These illuminations appear, in general, about the middle of the mountains, and seldom or never extend to the top or bottom of them. They take place especially on those hills on which the bamboos grow very thick; which has probably led the natives to explain this phænomenon so rationally, by ascribing it to the friction of these bushes against each other.

Lieutenant Kater, of his majesty's 12th regiment, thinks that the corky bark of the *adenanthera pavonina* is often spontaneously inflamed, as he has frequently found, on his surveys, its bark converted into charcoal, and several of these trees burnt down to the roots, although they were not in the vicinity of any other trees.

In Europe I know these spontaneous ignitions have been much discredited; and I doubt not but should these few sheets ever be published, many objections will be raised against what I have related: but I have endeavoured to state facts only, which a luxuriant imagination might have painted in more striking colours, but I am sure not with stricter adherence to truth.

The land winds are noted for the dryness which they generally produce on the face of the country, as well as on that of the animal creation. This sensation is particularly felt in the eyelids, which become in some measure quite stiff

stiff and painful. This is owing to the immediate volatilization of all humids that irrigate our organs, and which, in this particular one, probably gives rise to inflammations of the eyes, so frequent at this time of the year *.

The continuance of this wind causes pain in the bones, and a general lassitude, in all that live; and, in some, paralytic or hemiplectic affections. Its sudden approach has, besides, the dreadful effect of destroying men and animals instantaneously.

It is not very uncommon to see large kites or crows, as they fly, drop down dead; and smaller birds I have known to die, or take refuge in houses, in such numbers, that a very numerous family has used nothing else for their daily meals than these victims of the inclemency of the season and their inhospitality. In populous places it is also not very uncommon to hear, that four or five people † have died in the streets in the course of a day, in consequence of being taken unprepared. This happens especially at the first setting in of those winds.

The natives use no other means of securing themselves against this wind but shutting up their houses, and bathing in the morning and evening; Europeans cool it through wetted yats‡ made of straw or grass, sometimes of the roots of the wattie§, which, wetted, exhale a pleasant but faint smell. It will be incredible to those that have never witnessed it, but the evaporation is really so great, that several people must be kept constantly throwing water upon the tats (eight feet by four) in order to have the desired effect of cooling a small room.

It would be scarcely necessary to observe, if it were not in contradiction to public opinion, that the cold produced is not a peculiar property of the wind, but depends upon the general principle, that all liquids passing into an aëriform state absorb heat, and cause immediately around them

* The eye flies, so often supposed to occasion it, produce a transient and sharp pain in the eye, but never, I believe, a lasting inflammation.

It is generally thought infectious, and may be so by the interference of the eye flies carrying the contagious matter from an affected eye to a sound one.

† Four people dropped down dead at Yanam, in the year 1797, an hour after my arrival there from Masulipatam: and at Samulcotah four or five died the same day on the short road between that place and Peddapore: the number of inhabitants of either of these places does not exceed, I believe, five thousand.

‡ The frame of them is made of bamboos in the form of the opening in the house to be tatted, let it be door or window, which is then covered with straw in the manner every one thinks best suited to retain the water longest.

§ *Andropogon muricatum.*

a diminution of it, and consequently a relative coldness. On the same principle depends also the cooling of wine and water, in the land wind seasons, the latter in light earthen vessels which allow an oozing of the water through their pores, and the former in bottles wrapped in a piece of cloth or in straw, which must be constantly kept moistened.

The great violence of these winds is at last terminated by frequent showers of rain, in June, in the low countries, and by the greater quantity of the regular rains falling in the inland countries, which seem to suspend the partial formation of clouds along the Ghauts, and to leave them clearer, and visible at a greater distance than they had been at any other period of the year before.

After the enumeration of so many disagreeable circumstances, I am naturally led to an investigation of the causes that produce them. Before this can be done, however, I must prove, according to promise, that the theory of our philosophers is founded in error.

They ascribe, as already observed, the extraordinary heat which distinguishes these winds from most others, to the absorption of caloric in their passage over an extensive tract of country, at a time when the sun acts most powerfully in our latitudes.

According to this theory, the heat should increase in proportion to the space over which this wind is to travel; it should be hotter on the coast than it is at any part of the country inland, or, which is the same, it should decrease by degrees from the eastern to the western sea of the peninsula. Experience, however, teaches us the reverse; for it is hottest near the Ghauts, and among the valleys between those ranges of hills, than at any place on the coast; and the heat of those winds decreases also as they approach the Bay of Bengal, and in a direct ratio from the Ghauts to the sea: accordingly, it is at Ambore* hotter than at Vellore†, and at this place again than at Arcot ‡, Conjeveram §, and Madras, where the land winds are seldom felt with any degree of severity.

* A place situated in the most western valley of the Ghauts, immediately at the foot of the steepest ascent into the Mysore country.

† Lies in a spacious valley nearly at the entrance of the Ghaut mountains, and has the advantage of an open communication with the flat country to the north-east.

‡ A large city, the capital of the nabobs of the Carnatic, east of the ranges of hills called the Ghauts.

§ . . . miles east of the latter place in the road to Madras, a large populous place. I have chosen this tract or line as the most known, although not the hottest; for Ellore, Rajahmundry, and Samulcotah in the Northern Circars, are by far more exposed to these winds.

Time

Time is another measure applicable to the acquisition of heat, as it increases to the greatest pitch which a body is capable of receiving in proportion to its continuance: the land winds should therefore be cooler when they set in at ten or eleven o'clock, and hottest at their termination in the afternoon; they should be so at least at noon, when the sun is nearly vertical, and has the greatest influence on the substances from which heat is to be attracted. The contrary, however, comes nearest to the truth; for it is known that these winds set in with their greatest violence and heat at once, which rather abate than increase, as might be expected.

We should, on this principle, further suppose the heat would increase gradually with the return of the sun to our latitudes, from its southern declination, and stand always in proportion to its position. We find, however, that experience also contradicts this point of the theory under discussion; for after the sun has passed our zenith*, the land winds set in at once with all their intensity, in the manner before described, and they cease as abruptly before its return again †.

A material change in the temperature of this climate is certainly effected by the approach of the sun from the south; but the heat which is thus caused, and which increases by imperceptible degrees, is never so great, and is only felt by those who expose themselves to it unprotected; for the air remains proportionally cool, and our houses afford, in this season, a pleasant retreat. We find it far otherwise in a land wind; for this penetrates our inmost recesses, and renders life miserable every where.

I have before observed, that winds equally hot with those of periodical duration are felt in all parts of the country, and at different seasons; a circumstance alone sufficient, if proved, to overthrow the groundwork of the old theory.

For a confirmation of this, I will appeal to the general observation, that immediately before a long rain the weather is sultry, and that a single shower is always preceded by a warm disagreeable wind.

We are very particularly reminded of the approaching great monsoon in October by the oppressive heat we have in the calm evenings of that month, which, I am persuaded, would equal that of the land winds in May, if the atmo-

* The sun is in the zenith at Madras about the 26th of April.

† The sun is again in our zenith on its southern declination about the 19th of August.

sphere

sphere was not cooled in the latter part of the night by breezes that have wafted over extensive inundated plains.

I can refer, secondly, to my Meteorological Journal, according to which, the 4th of June 1800, at Madavaram, a place not far from Bengalore, the thermometer rose for a short time to 104° just before a slight shower of rain, and at a time when heavy clouds darkened the western hemisphere.

Further, in the months of March and April, 1804, we had often at Bengalore, in the afternoons, strong gusts of wind from the eastward, which, in common, were styled land winds, and were really as hot and disagreeable as moderate land winds are in the Carnatic. I could have multiplied instances of this kind, but am of opinion that in a fact so much known it would be perfectly needless.

The last refuge of the defenders of this theory is the valleys of the Ghauts, in which they pretend the heat is generated by the concentrated and reflected rays of the sun.

I will not deny but the heat occasioned by these causes may contribute much to raise the heat of the land winds; but the sudden appearance of the latter, their usual strength, and abrupt disappearance, all militate against that explanation as a principal cause.

The heat of these winds should in this case, to say a few words more on the preceding subject, decrease regularly from the point where it is greatest towards the opposite, on both sides, as is the case on the coast of Coromandel. On the contrary, we find that, immediately on our having ascended the Ghauts, or on the top of hills * elevated above the clouds, we have escaped their heat all at once. It is hereby remarkable, that the direction of the wind remains to appearance nearly the same every where. In Mysore, for example, the wind is, in the land wind season, west during the greater part of the day; in the afternoon it is from the east, and commonly warmer than the former.

This, together with what had been said before, will, I hope, be thought sufficient to establish my opinion re-

* Major Lambton, at the top of Carnatighur, one of the highest hills in the Carnatic, about three thousand two hundred feet above the level of the sea, found, in the middle of the land wind season, the thermometer at 79° and 80° in the mornings, and, at noon, 82° and 84°, when it was below at 103° and more.

This observation may be the more depended upon, as the Major remained for a considerable time on the top of this hill, in the pursuance of his most accurate survey, in the course of which he pays great attention to this as well as to all other points that could influence his learned labours.

lative

lative to what *can not* be the cause of the heat in the land winds.

It remains now to point out a theory, supported on a firmer basis, which I shall endeavour to do in the following pages. It is founded on a chemical principle, and will explain, I think, the heat of these winds in a satisfactory manner.

The principle itself needs no demonstration, as it is admitted as a general law; viz. that "all bodies, when they become more dense, suffer heat to escape; or, what is the same, they give out heat." For example, when gases or aëriform substances become vapours, they discharge as much heat as was necessary to keep them in their former gaseous state: further, vapours in condensing into fluids are known to do the same, as also fluids, acquiring solidity.

I am sorry that the quantity of heat set free in the condensation of vapours required for a pound of water has escaped my memory; but I recollect it was very considerable. We know, however, that a great deal of it is required for the evaporation of the same measure, and it is but reasonable to admit that the same quantity with which it has combined should be discharged on its returning to its former state of fluidity.

In order to apply this principle to explain the presence of heat in our land winds, I must first observe. that the atmosphere in January, February, and March, is perfectly clear and serene; and then I will call to mind what has been said of the phænomena of those winds, that they are preceded by clouds on and among the Ghauts, and that a heavy shower of rain from that quarter announces their arrival; that during their continuance clouds are observed to lie on the Ghauts; and that the atmosphere, even in the low country, is hazy and thick. I must add also, that the countries west of the Ghauts are at this season frequently visited by heavy showers of rain, accompanied with much thunder and lightning, and sometimes with hail. Here in the Mysore country I have found the heaviest showers of this kind to come from the north-west*, which is exactly in the direction of the countries remarkable for the great heat of the land winds in this season. At times, we have also showers from the east and south-east, and my attention shall not be wanting to ascertain whether it is not at the time when the land winds blow hottest in the Carnatic.

* The hottest land winds in this season (1804) at Madras were, I understand, from the north-west; which corresponds with the direction from which the rains came in Mysore at that period.

By this we see, that the clouds formed on the Ghauts, charged with water and electricity (by causes I am not now to investigate), are drawn to the westward, whilst the heat which, during the formation of these clouds, must necessarily be discharged, is carried to the east or to the lower parts of the coast, and causes the properties for which the land winds are so remarkable.

I have acknowledged already, that the heat occasioned by the power of the sun in this season, contributes to the aggregate of it in the wind; but I must observe also, that it acts only as a secondary cause, and passively, by preventing its absorption and diminution in the career over a variety of substances, particularly moisture, with which it would combine, if they had not been previously removed or incapacitated.

In colder climates, this absorption takes place in a greater degree, as substances are abundant with which the heat produced by the formation of rain can combine and become imperceptible *. It is, however, there also often remarked, that the heat of the sun in a cloudy day is more powerful than at any other time. In common this is ascribed to the reflection of the rays of the sun from the clouds; but I opine it is often the consequence of the formation of water in the clouds, which obscure the sky at that moment.

It has been observed, that the heat of the land winds is not felt on the top of high hills, or on plains of a very inconsiderable perpendicular height above those in which it rages most violently; as for example, in Mysore near the Ghauts, which is only about five hundred feet higher than the valleys immediately below. This might be considered a weighty objection against my theory; as heat, considered in the light of an elastic fluid, expands equally on all sides; and from whatever cause it proceeds, it should be supposed to extend even further where it meets with less resistance, as from the air in higher regions, which is known to be lighter and more penetrable than near the earth.

But the reverse takes place; for almost immediately above the clouds no other heat is perceptible than what might be owing to the nature of the climate.

This circumstance may be accounted for by the diminished density of the air in the lower parts of the country,

* Earl Dundonald's Treatise, p. 20. "The frequent changes in the degree of heat and cold in the atmosphere are to be ascribed more to the alternate disengagement and fixation of heat by chemical combination, than to the effects of the solar rays."

produced

produced by the heat of the season, which would naturally cause the wind to rush thither, with all its contents, and with greater impetuosity. The coolness of the atmosphere on elevated situations may be ascribed also to the evaporation of the uppermost strata of the clouds, which accompany the land winds.

Many arguments I have dispensed with, which might have been produced to elucidate and to establish my theory, as they were chiefly such as could be collected from simple inference, and from affirmative application of doctrines advanced before.

I will only add, that both the sirocco and samiel may be owing to similar causes as those which appear to be productive of the pernicious, or rather disagreeable, effects of our land winds.

XLVIII. *Hints respecting a New Theory on the Orbits of Comets.* *By* Mr. W. Crane, *of Edinburgh.*

To Mr. Tilloch.

Sir, The following theory, for any thing I know, is original: should it be deemed worthy of a place in the Philosophical Magazine, its insertion will much oblige

Your humble servant,

W. Crane,

Sept. 27, 1810. Student of Medicine, Edinburgh.

> "Hast thou ne'er seen the comet's flaming flight?
> The illustrious stranger passing, doubles wide
> Heaven's mighty cape, and then revisits Earth."—*Young.*

The difficulties with which this intricate branch of astronomy is surrounded, the short part of an orbit of a comet that is visible to us, and the rarity of their appearance, have given rise to innumerable theories, many of which have no sooner been advanced than they were immediately abandoned as erroneous.

The school of Peripatetics assigned comets no place in our planetary system, they only considered them as sublunary things made up of the exhalations in the terrestrial regions; which was the opinion of many, until Tycho Brahe and Kepler proved by observation that they were beyond the moon, and consequently not composed of terrestrial vapours: this was further confirmed by the observations made by Cassini, of that seen in the year 1665, and of another that appeared in April 1680. Cartesius thought them to be permanent bodies, like the planets, and to be constantly carried

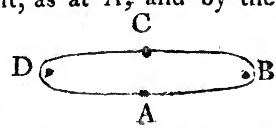
carried from one vortex to another in right lines: but Cassini supposed from his observations that they moved in circles very eccentric, and containing the earth's orbit within them; and from hence was led to think the comet of 1680 and 1681 was the same as appeared in 1577. By means of this and some others he had an opportunity of seeing, he determined that comets moved through the constellations Antinous, Pegasus, Andromeda, Taurus, Centaur, Scorpio, and the bow of Sagittarius, which he called the zodiac of comets. That this is not the case, later observations have proved. The comet that appeared in September 1808 was first seen in Serpentarius, it then passed through the right shoulder of Hercules, the Lyre, and disappeared in the tail of the Swan, which is a course widely different from the zodiac laid down by Cassini. James Bernouilli, in his System of Comets, published in 1682, considers them to be satellites moving about a primary planet, which revolved around the sun, at a distance equal to 2583 semidiameters of the Magnus orbis, in four years and 157 days, although Saturn, who is 258 times nearer, makes only one revolution in about 30 years. This primary, he says, we can never see, on account of its smallness and immense distance, and these comets or satellites are only visible when they descend towards us in perigeum. In this theory we have a greater body revolving round and carried with a smaller; which is contrary to what is observed with regard to the other planets and their satellites. May we not consider this as one of those theories which, had it not come from so great a man, would have been buried long ago in oblivion?

Sir Isaac Newton, Dr. David Gregory, Dr. Halley, and others, imagine them to move in very eccentric ellipses, having the centre of the sun in one of their foci: but some in their calculations have substituted a portion of a parabola having the same vertex and focus, which they observe is its true trajectory, if it never returns. This supposition only leads us from one difficulty to another; for we may next ask, By what means did it come within the attraction of the sun, and from whence? Are we to suppose it passes from one fixed star to another in a serpentine direction, which is the theory adopted by Mr. Cole of Colchester? All the celestial phænomena with which we are acquainted, are obedient to certain laws of attraction, and move either in circles or ellipses, but none in the manner above mentioned.

But to return to the former theory; that is, that they move

move in very eccentric ellipses, in one of the foci of which is placed our sun; for, as La Place says in his System of the World, analogy leads us to imagine that comets move in orbits, which, instead of being nearly circular, like those of the other planets, are very eccentric, and the sun extremely near that part in which they are visible to us; and to observe the same law as the other planets.

Hence is it not probable that they revolve about two fixed stars, placed in the two foci of their orbits? This opinion, I think, is strengthened by the amazing eccentricity of their orbits, which, as was observed above, approaches very near to a parabola, no comet has yet been seen that would answer to an hyperbola: of this amazing distance, the exceeding small part we see before a comet approaches the sun, and when it leaves him, would not differ much from a right line. Again, as the two foci of the ellipse in which it moves are so very distant, is it not probable there are two attracting powers? that, is one in each focus; and as the attraction of one body begins at the point where the other ends, let us conceive the comet to be put in motion a little beyond that point, as at A, and by the time it arrived at B, its centrifugal force becomes great enough to throw it within the attraction of the focus D, which we will suppose at C: it is now acted upon by the attractive power at D, and acquires in moving from C to D a velocity great enough to bring it again to A; and thus it will revolve about the two fixed stars B D, in a very eccentric ellipse.



This will also account for their appearance from every part of the heavens: and it is supposed that more than 450 have been seen in different directions; for about the same fixed star many may revolve, yet only one about the same two fixed stars.

Ferguson, in his Astronomy, estimates the nearest fixed star at about 32,000,000,000,000 miles distance from the earth, consequently it is 32,000,082,000,000 miles from the sun; and Adams, in his Astronomical Essays, says that the comet seen by Brydone at Palermo in 1770 moved at the rate of 60,000,000 miles an hour. Now admitting this to be its average rate, and that it performed a revolution once in 129 years, which is the period assigned to that which appeared in 1661, we shall have 67,802,400,000,000 miles for the length of its orbit; and it is not improbable that this would be the perimeter of an eccentric ellipse whose foci were the distance above mentioned.

All

All Nature is held together by an universal bond: the vegetable kingdom is joined to the animal by the sensitive plant; birds and fishes by the bat and beaver; the monkey joins beast to men; and the sun by his vast influence binds the worlds together that form our system. Let us extend our views a little further, and we shall have the blazing comet uniting the systems of other suns to ours, forming the links of that chain by which the universe is supported.

XLIX. *Description of a Machine for securing Persons attempting Depredations without affecting their Life or Limbs. By Mr.* Robert Salmon, *of Woburn* *.

Sir, I beg leave to submit to the Society of Arts, &c. a mantrap, which I hope will meet with their approbation. To those who live in the country it is needless to explain the frequency of petty depredations committed on gardens, orchards, &c. and which are sometimes very vexatious. Few persons would like to endanger the life or limb of the depredator by setting the common steel man-trap, yet it is presumed there are but few who would not wish to detect the offender. The instrument which I have the honour to submit to the Society is for the purpose of catching and holding the person without injury. At the Agricultural Meeting at Woburn last summer, an ingenious invention for a similar purpose was produced by Sir Theophilus Biddulph; it consisted of a wood box, containing two springs in iron barrels, and two chains passing over and round them: when this was set, the chains were withdrawn from round the barrels, and extended to a certain distance. A trigger then kept the trap from closing. The whole was then covered over with thin iron plates; so that if a person set his foot on those plates his leg dropped into the box, and the chains closed round it and held the leg; but as the box was about three feet square and a foot deep, it was requisite that it should at setting be let into the ground, which would be a work of considerable labour, and when done it would be difficult to dispose of the stuff from the hole, or to conceal the trap; and as the whole apparatus was cumbersome and expensive, it appeared to me not to be well applicable in practice.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xxvii.——The silver medal of the Society was voted to Mr. Salmon for this communication, and one of the machines is reserved in the Society's repository for the inspection of the public.

I think

I think it right to give this explanation in justice to Sir Theophilus Biddulph, from whom my idea of the utility of something of the kind arose, as also to show the difference between his invention and the trap I have made, which is so very simple as hardly to require explanation. When set, it only requires that the two keys be withdrawn, and that the trap be covered with a few loose leaves or mould. To the trap I have attached a piece of chain and a screw to be screwed into the ground, so as to prevent its being carried away; but against any person that may be caught such a precaution is perhaps unnecessary, for any person who is caught will find the jaws of the trap close so fast on the leg that he cannot drag the trap far without great pain, and will consequently be glad to stand still and to call out for relief. For the convenience of explanation I have applied mufflers to the jaws of the trap, so that any person may put in his leg without the least inconvenience. I have even tried it without, yet, though void of danger, the sensation is not pleasant. The muffle will of course be omitted when set for use, as it is not then necessary to guard against a little inconvenience, otherwise the springs might be made weaker. I remain, sir,

Your most obedient humble servant,

Woburn, Feb. 12, 1809. ROBERT SALMON.

To C. TAYLOR, M.D. Sec.

P. S.—Permit me strongly to recommend to the notice of the Society the earth screw attached to the trap, as excellent for the purpose of fixing any thing steadily in the earth. This screw is far superior to the common way of driving an iron point or stake therein.

I have employed it for several years in fixing cross-staffs and other surveying instruments with great advantage. The very act of driving a spiked instrument into the earth leaves it loose with some play or movement, which prevents it from being easily secured; but with a screw of this kind at the bottom of the instrument it is firmly fixed in the ground, and a turn of the screw will again fix it, if it should by any means be moved or loosened. It may also be screwed into the ground with any instrument upon it, which would be spoiled by the act of driving it in.

Description of Mr. Salmon's Man-Trap, which detains the Offender, without injuring or maiming him. See Plate VI. Fig 1.

The principal figure in the fore-ground of Plate VI. is a perspective view of this machine. Fig. 1. ABC is a frame of wrought iron, about 18 inches square; it has an eye projecting from it to receive a short chain, the other end of which is fastened to an iron screw, shown separately at D, screwed into the earth by the key or handle E: this screw is about 14 inches long, and, when screwed into hard ground, will hold so firmly, that there is no danger of its being drawn out, even by two or three men; and having a small square end, it cannot be turned without the key or handle E; so that an offender would find it extremely difficult to remove the trap: *e e f g* are two iron frames moving on centres in the frame ABC; these frames have a constant tendency to close together, by means of two springs *p p*, fixed in the frame AB, and acting against pins projecting from the upright sides of the moveable frame *e e*; *k k* are two small iron rods jointed to the upper rod of the moveable frame *g*, and passing through small locks *l l*, fixed to the other frame *f*. These locks contain clicks which are pressed by springs into the teeth, as may be seen upon the rods *k k*, so as to prevent the two bars *f g* from being drawn asunder when they have been closed by means of the springs *p p*. The internal mechanism of the locks is explained by figures 2, 3, on a larger scale at LM, in the same plate; one side of the lock is supposed to be removed to exhibit its interior parts, where *k* represents the rack, or that part of the rod which is cut into teeth, *r* is the click, which engages the teeth of the rack, and prevents its being drawn through the lock: the click is pressed against the teeth of the rack by a spring, which is plainly seen in the figures; the locks are attached to the ends of the bar *f* of the moveable frame, by the bar passing through the locks, and when the lids are riveted on it is confined in such a manner that it cannot be got out. But as it is necessary to open the bars *f g*, and draw the clicks back from the teeth of the racks, Mr. Salmon has contrived two different methods of accomplishing this object. Figure 3. M is that which is used in the model left at the Society's Repository; a small key or screw S is put down through a hole in the lid of the lock, and is received into a hole lapped with a screw in the click: by turning the screw it lifts the click out of the teeth of the rack; so that the moving frames *f g* can be opened apart from each other, till they lie flat upon the frame AB. The iron cross *m* is then put between the two rods *f g*, the screws S of the two locks are to be withdrawn from the locks, and the trap is set for use. If an offender should place his foot within the square of the frame, he would tread down the

cross-

cross *m*; and having thus removed the obstruction, the two frames *e e f g* are closed together by the springs *p p*, so that the bars *f g* inclose his leg, and the clicks in the locks prevent the bars being opened without the screws S. In some of the machines which Mr. Salmon has made since the model was deposited with the Society, the locks are made like figure 2, L, where a common key is to be introduced, and, when turned round, catches the tail of the click; it may have wards to prevent the using of a false key, though no wards are shown in the plate. Part of the screw D for securing the trap from being carried away by depredators, is shown on a larger scale at N, in order that the peculiar form of its threads may be better seen, which fix it firmly in the earth. Such screws would be very serviceable in fastening horses at grass, &c.

L. *An Account of a New Method of increasing the charging Capacity of coated Electrical Jars, discovered by* JOHN WINGFIELD, *Esq. of Shrewsbury* *. *Communicated by Mr.* JOHN CUTHBERTSON, *Philosophical Instrument-Maker, Poland Street, Soho; with some Experiments by himself on that Subject.*

IN my treatise entitled *Practical Electricity and Galvanism*, page 103, I have said that breathing into coated electrical jars increased their charging capacity to such an astonishing degree, that their discharge would fuse four times the length of wire more than they could in ordinary circumstances; which I proved by experiments 147 and 156. Since that publication, large electrical batteries are become more general, and the number of jars increased; so that batteries containing thirty, sixty, and even a hundred jars are frequently met with; and, when so numerous, breathing into each jar is very disagreeable; and not only that, but in very dry states of the atmosphere, when most wanted, is even ineffectual, as those jars first breathed into lose that property which was produced in them by breathing, before the last can have obtained it: so that various other means have been tried; such as wetting the inside of the jars, and putting wet sponges into them, or by greasing and oiling the uncoated part in the inside; all of which gave very uncertain results, till John Wingfield, esq. communicated to me,

* A gentleman who has lately very much distinguished himself, not only in the electrical science, but in all other branches of experimental philosophy.

he had discovered, that pasting of paper on the inside and outside of the jars above the coating, had the effect of preventing the jars from exploding to the outside coating, and believed that their charging capacity would be increased thereby.

I embraced the first opportunity to try the effect of that discovery with single jars.

Experiment I.—I took a very thick jar (which had been used to show the phænomena of voluntary explosions without breaking) twelve inches high, and the coating nine inches, containing in the whole about 171 square inches; it was applied to the conductor of a plate electrical machine, and six turns of the plate caused a voluntary explosion to the coating: the state of the atmosphere not being very dry, it required eight and twelve turns to produce a second and a third explosion: a fourth could not be produced; but when cleaned and dried as before, six turns caused a voluntary discharge.

Experiment II.—A slip of paper one inch broad was taken, of sufficient length to fit round the outside of the jar when the two ends were pasted together: this was slipped on to its outside to about one inch from the coating: the uncoated part being rubbed clean and dry, and applied to the machine, eleven turns of the plate produced a voluntary discharge to the outside coating.

Experiment III.—The paper ring was then slipped down to touch the coating, and then applied to the conductor: no voluntary discharge could be produced; and when discharged in the common way, its power did not seem to be increased,—to prove which,

Experiment IV.—The common discharging electrometer (which is always fixed to the basement of my machines) was used, to try to what distance the discharge could be made to pass from the knob of the conductor to the ball of the electrometer; which was found to be one inch and five-eighths.

Experiment V.—A piece of iron wire, $\frac{1}{100}$th part of an inch in diameter and one inch in length, was hung to the electrometer, through which a second discharge was made to pass, and the wire was blued.

Experiment VI.—The paper ring was then taken off and breathed into twice; the discharge was then produced at the distance of two inches, and the wire was fused into balls.

Experiment VII.—The jar was then rubbed clean and dry, and a piece of the same sort of wire and the same length was hung to the electrometer in the same manner as as before, and it appeared that the greatest charge it could take had not the least effect upon the wire: thus it appears that a paper ring so applied does not increase the charging capacity of jars in the same degree as breathing.

Experiment VIII.—The jar was highly charged, and examined in the dark: the paper ring appeared luminous all round the uppermost edge.

Experiment IX.—The ring was taken off, and pasted on in the inside close to the coating: 23 turns caused a voluntary explosion through the ring to the outside coating.

Experiment X.—A second ring three quarters of an inch broad was pasted on close to the other: the same number of turns produced a voluntary explosion, and the paper was torn by the discharge, which was repaired and left to dry.

Experiment XI.—When dry, no voluntary explosion could be obtained.

Experiment XII.—Its greatest power was then tried, and was found to be exactly the same as in Experiment VI. (when it was breathed into): it discharged at two inches distance, and the same length of wire was fused into balls.

Experiment XIII.—A second jar was taken of a larger size, being 13 inches high, and its coating seven inches; in the whole it contained about 190 square inches: after being rubbed clean and dry, it was applied to the conductor of the machine: twelve turns of the plate produced a voluntary explosion to the outside coating.

Experiment XIV.—A paper ring was put round the uncoated part on the outside at about $1\frac{1}{4}$ inch distant from the coating: eleven turns of the plate produced a voluntary explosion to the outside coating: the paper ring was then pushed down to the coating, after which no voluntary explosion to the coating could be obtained; but it discharged itself to the electrometer ball standing at the distance of $2\frac{3}{8}$ inches from the knob of the conductor.

Experiment XV.—The same sort of wire, two inches long, as used in Experiment VI, was hung to the electrometer, and the discharge made it blue with several bendings,—a proof that it had been nearly red hot.

Experiment XVI.—A ring of common writing-paper one inch broad was pasted on the inside close to the coating, and when dry no voluntary explosion to the coating could be obtained; but it discharged itself to the electrometer ball standing at the distance of $2\frac{5}{8}$ inches, and the wire was fused into balls.

Experiment XVII.—The paper rings were now taken off, and the uncoated part made clean and dry: 19 turns pro-

 duced

duced a discharge to the electrometer ball at the same distance, and the same length of wire was slightly blued.

Experiment XVIII.—The jar was then breathed into, and a discharge was produced at the same distance, but the wire was not fused.

Experiment XIX.—The same jar was breathed into a second time, and a discharge was caused at the same distance, and the wire was fused into balls exactly the same as when the paper rings were on.

Experiment XX.—A third jar nine inches high and four inches diameter, the whole containing about 64 square inches, when rubbed clean and dry, two turns of the plate caused a voluntary discharge to the outside coating.

Experiment XXI.—A paper ring was pasted on both sides close to the coating, and one inch from the top, after which no voluntary explosion could be obtained, but the electric fluid was seen to run over the brim of the glass to the outside coating as quick as the machine could give it; the discharging distance was seven-eighths of an inch: it had not power sufficient to make any impression on one inch of wire.

Experiment XXII.—The paper rings were then cut narrower at different times, and tried, which increased the discharging distance, when there remained only one quarter of an inch which seemed to be the most favourable above the coating: the discharging distance was $1\frac{3}{8}$ inch, and the wire was fused, and dispersed in balls.

Experiment XXIII.—The paper rings were taken off, and the jar carefully breathed into: six turns of the plate caused a discharge to the electrometer standing at the distance of $1\frac{1}{4}$ inch, and one inch of wire was fused, and dispersed in balls, equal with the last experiment.

The above experiments are sufficient to prove that paper rings pasted on to electrical jars in the manner explained, do hinder voluntary explosions, and increase the charging capacity of coated jars, in the same degree as breathing into them.

Further experiments and observations, setting forth the advantages that electricians are likely to obtain from the above discovery, will be the subject of a future paper.

LI. *Method*

LI. *Method of constructing commodious Houses with Earthen Walls. By Mr.* ROBERT SALMON, *of Woburn**.

DEAR SIR, HAVING for some years past practised at this place the art of *pisé*, or constructing walls with earth, and having in consequence been several times both publicly and privately called on to communicate my observations thereon, I have been led to consider that the best mode of generally communicating what I know on the subject would be through the medium of the Society of Arts, &c. I have accordingly, by the waggon, forwarded a case containing a model of my frames and apparatus for performing the work, with every particular in my power to give, for the information of any persons inclined to build in that way, and they will, I hope, be found worthy a place in the collection of the Society.

To such as may be inclined to see specimens of this work, and may not have an opportunity of going far distant from London, I can recommend a house and other works built, and some of them inhabited by my brother, Mr. William Salmon, Builder, at Henley-Hill, near Barnet, Herts.

I have the honour to be,
The Society's and your most obedient servant,
Woburn Park, Dec. 8th, 1808. ROBERT SALMON.

To C. TAYLOR, M. D. Sec.

Description of the Engraving of Mr. Salmon's Method of building Pisé or Earthen Walls.

Fig. 4. of Plate VI. is a perspective view of the apparatus or moulds, in which the earths are rammed to form a wall. The mould consists of two long planks F *f*, twelve feet long, twenty inches broad, and one inch thick, each made in two breadths; they are strengthened by several pieces of wood nailed across them. Holes are made through these pieces of wood at top and bottom, to receive iron bolts, which hold the two boards parallel to each other, fourteen or sixteen inches asunder, which is the thickness of the wall intended to be formed between them. The bolts have a large head at one end, and a key passes through the other, to keep the planks together. When a wall is to be built, the foundation is laid in brickwork, which is carried about nine

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, vol. xxvii.——The Society voted twenty guineas to Mr. Salmon for this communication, and models of the apparatus are reserved in their Repository for public inspection.

 inches

inches above the ground; upon this brickwork the planks are placed, and bolted together. Two boards, like that shown at G, are placed between the planks at the ends, to form the ends of the mould; these boards are placed between the two bolts *a a*, which are seen close together at the end of the moulds, and are held fast by that means; the earth is now to be rammed in between the moulds by the rammer with an iron head X. When the mould is filled with earth and well rammed down, the keys are to be taken out of the bolts, and the bolts drawn out; the planks are then removed, and put together again, a length further upon the wall, the bolts at the end being put through the holes left in the wall, only one of the end boards is now put in, and the ramming proceeds as before: in this manner straight walls may be built of any length; and when the lower course is finished, then the mould may be taken to pieces, and put together again upon that course, the lower bolts of the frame being put through the bolt holes which the upper bolts made in the wall at the first operation, to insure that the upper part of the wall is in the same place, and exactly over the lower. When a wall is to be built thinner than usual, a block of wood must be placed under the head of each bolt, so as to diminish the space between the planks.

When the angle walls of buildings are to be made, the apparatus is put together, as shown in the plate; four of the planks are put together to form a right-angled mould, one end of each of the planks F and H is furnished with double bolts, the other ends have each two eyebolts fixed into them, as shown separately at *b d*; then a bolt *n* connects the two moulds, so as to form a hinge; the planks are kept together, so as to be perpendicular to each other, by a long iron rod K, hooked into eyebolts fixed in the planks. The outside planks of the mould are joined together in a different manner, see fig. 5, that of one frame being longer than that of the other, and has two pair of holes through its end O, to receive the bolts *t t*, which are fastened to the ends of the other shorter plank, and the keys are put through the ends of the bolts, to secure the planks together; a piece of wood P is occasionally placed between the end of the short plank and the side of the other, to increase the space between the planks, to make a thicker wall, the two bolts at the end of the plank being received into the notches in the piece of wood, and these bolts are then put through the holes Z Z of the long plank. In building the angle wall, it is necessary that the vertical joint formed between each mould should not be over one another, but arranged in the same manner

manner as the joints of brickwork: this is accomplished by making the lower course of wall upon the brickwork only half the length of the mould, which is done by placing the end board G of the mould in the middle of it. The next course over this is to be made the whole length of the mould, the next one only half, and so on, as shown in the figure.

Improved Moulds and Description of making Earth Walls, by Mr. R. SALMON, *of Wôburn, Bedfordshire.*

The model of the frame in possession of the Society is made to a scale of an inch to a foot, the frame at large is made of 1¼ inch deal, ploughed and tongued together. The bolts and pins or keys of iron, as are also the plates on the holes in the sides of the frame. These plates are put to prevent the keys from cutting into the wood, and the holes from gulling and wearing.

This sort of mould is calculated for making walls, either fourteen or sixteen inches thick, and the model (or perspective view of it in the distance of Plate VI.) shows how the mould is to be applied for making the corner of a building of the sixteen inch wall; the same moulds may be applied for a fourteen inch wall. *ff* being the outer sides. FH the inner sides. When employed for straight walls, or making good between the corners of buildings, the two returns of the frames are used in pairs, *ff* and FH make two sets of frames. The board marked G must be of width equal to the thickness of walls to be made, and are for the purpose of stopping the earth, and making ends or jaumbs to doors or windows, or wherever wanted. The piece of wood P is two inches thick, and is for the purpose of making out the external sides of the moulds, from a fourteen inch to a sixteen inch wall: by introducing this piece between the two sides *ff*, and putting the fixed iron pins in the outer holes ZZ, and taking away the blocks under the heads of the outer bolts, the sides of the frame will then be sixteen inches, as under, and thereby adapted for a sixteen inch wall. Fig. E are pieces of wood about 1¼ inch square, and cut to the length of the thickness of the wall, and are for gauges to be applied on top of the bolt, to keep the keys from drawing the sides too close together.

In beginning the wall, some of them are necessary at the bottom, the more firmly to support the frame on the brick or stone work. They are then worked into the wall, and, after the frame is taken down, drove out. After the first course, they are only necessary to the top irons, aud may be taken out as soon as the earth is rammed up near them, so that no holes are left in the upper courses of the wall, more than the bolt holes.

When these frames are used, one side is placed in such a direction, that the front or end may be required to be taken away, and then by means of the angular iron brace K, the other return is sure to stand at right angles with the first. Care should then be taken, in the first course, to set the sides level: that being done, the other upper courses, from the nature of the frames, and manner of using them, must of course come upright and level without any particular care, and a wall being properly begun, cannot well get wrong. After the first course of a building is done, the moulds should be moved to another, and so on till all the courses are up; and as the top holes of each preceding course become the bottom holes in the succeeding ones, no difficulty will be found in fixing the mould after the first course is properly done.

Fig. 6. shows the iron pin and staples that keep the internal angle of the frame together. K, fig. 4. an iron stay to set the returns at right angles. This is only wanted where other means of setting the building square are not to be obtained.

Having described the frame, and means of applying it generally, it may be necessary to observe the following particulars in the process. Having carried one course round the building, it frequently happens that the top thereof becomes too dry to attach to the next succeeding course, and therefore it is adviseable that, as soon as the frame is set for the succeeding course, a small quantity of thick grout, composed of $\frac{1}{5}$ lime, and $\frac{4}{5}$ earth, be poured on top of each course, immediately before the first layer of earth is put in. A very small quantity is sufficient, and will add much to the strength of the work, by cementing the courses well together at the joints. The workman should also, with the corner of his rammer, in ramming home to the upright joints, cut down a little of that part of the wall, up to which he works; this will make the upright joints key together, and unite in a solid manner. Having thus proceeded and got up the walls, the next thing will be to stop the bolt holes, with mortar made $\frac{1}{4}$ lime and $\frac{3}{4}$ earth the same as the wall.

The earth proper for this work should be neither sand nor clay, but partaking of both. Clay is particularly objectionable, as is also chalk, or calcareous earth of any sort. Sand is also not proper, unless accompanied with some binding quality: the bolder and coarser the sort of earth the better.

When

When used, it should retain no more moisture than just to make it adhere together, under the pressure of the thumb and finger. Notwithstanding earths bordering on sand appear to make the strongest work, nevertheless good earths may often be found in parts that do not abound with sand. Those that abound with a mixture of grit or fine gravel are generally the best. Having provided proper earth, as much should be put in each layer as to form about an inch and a half when compressed by ramming.

The rammer X should not be more than half an inch wide on the edge, in order that it may more forcibly compress every part of the earth, which a flat rammer would not do so well.

In making the walls, about three inches in thickness of loose earth should be put in each course, which done, the same, by means of a trowel made for the purpose, is drawn back and cleared from the face of the wall, and the space then filled up with the facing composition, forming on an average about one inch in thickness; the whole then is firmly rammed, (in which, and properly preparing the facing stuff, much depends the perfection of the work) till it is quite hard, when it will be compressed to about one inch and a half in thickness. The common facing stuff is composed of lime one part, and earth, the same sort as used for walling, three parts. The lime and earth mixed and slacked together, the same as for mortar. The more it is slacked and wetted the better, provided time can be allowed for it again to dry and pulverize, so as to be fit for ramming. The better sort of facing stuff may have a small quantity more of lime in it.

The foundation should be of brick or stone, carried up nine inches above the ground; and if a plinth is to be shown, then one course above the same should be of brick or stone, to prevent the water that might lodge on the plinth from damaging the earth wall.

The proper season for performing this work is any time that the earth is to be procured sufficiently dry for the purpose; the more early in the season the better, in order to give it time to dry before finishing, or if late it would be adviseable not to finish till the year after it is built.

Windows and doors may be left in the walls wherever wanted, by fixing the head of the moulds and carrying up quoins to form the same: in erecting which some bond timber should be laid in coarse mortar and rammed in with the earth. Lintels may also be laid at the proper height. This method is cheapest, where only one window or door of a

size

size is wanted; but if many, the readiest way would be to make some rough frames of boards of width equal to thickness of walls, and place them in the situation of the windows and doors. When done, the earth is rammed up to them, laying bond timber at the sides and lintels over them. In both cases the windows and door-frames are to be put in their places and fastened to the bond-timber, after the wall is up. The bond timber, lintel, and plates, should be kept as thin as possible, in order to prevent any disagreement between the earth and timber in the shrinking or drying of the same. The bond timber about 4 inches by 1½; floor or wall plates 6 inches by 2; lintels about 4 inches thick; and it may be worthy of notice that any slabs or rough stuff may be used, the earth being sure to ram close to it and keep it in place.

For common cottages, when the whole of the walls are up and covered in, the holes should be stopped with very coarse mortar made the same as the facing stuff, but used wetter, and the wall then lime-washed over with lime and sharp sand, which should be made up in small quantities and used while hot. This may readily be done by adding a knob of lime and sand a little at a time as it is used.

For better kind of cottages the better sort of facing stuff may be used, and then, as before, the whole lime-whited; or if it be required to make the finishing as perfect as possible, the following is the best mode, viz. with water and a brush thoroughly wet and soak the face of the wall for two or three yards in superficie at a time; all which part, during the said wetting, should be continually rubbed and worked about with a hand float, till such time the face is rubbed smooth and even, by which the facing composition will so wash up as to become a pleasant regular colour, the face smooth and hard when dry, and not liable to scale off as a coat of plastering would do. This finishing will be still improved by a small quantity of lime being put in the water used for soaking the face, and if, after the wall is well soaked and rubbed, as above mentioned, there be thrown thereon with a brush some of the lime and sand, (such as used for lime-whiting,) and that also worked into the face; the face will then become as perfect and hard as stucco.

Having explained the frames as constructed by me for performing earth walling, as also the manner of finishing it, I beg leave to lay before the Society some observations on these, compared with the original French means and manner of performing the same, as described in the first volume of Communications to the Board of Agriculture.

The

The sides of the frames, as formerly constructed, were supported on joists or cross pieces of timber, which pieces were cut into the top of each course of walling. The sides were then kept together by upright timbers framed into the cross pieces or joists, and the tops of the upright pieces were wristed and held together by ropes going across the frame from one side to the other. In consequence of this construction, by experience I found much labour was lost in cutting the channels to lay the cross pieces in. These channels, after the buildings were up, took labour and materials to fill them in, and rendered the walls less strong. Also the difficulty of getting the frame rightly placed every time it was moved, and the elasticity of the rope across the top, made the whole very imperfect, so much so that all work done in that manner was untrue and unsound; as the rope, however tight it might be strained, would yield to a certain degree. The labour of moving was great, and when the frames were set, the cross ropes and uprights above the sides were much in the way of the workmen.

On examining the model I have the honour to send, it may be seen that these frames being once set true, they require very little care afterwards: being kept together by iron bolts, no elasticity can occur, and the earth will be as firmly compressed as if rammed between two walls. No cutting away for cross pieces is required, nor any holes but the small bolt holes to make good; and as nothing sticks up above the frames, the workman cannot be impeded. In consequence of these alterations the work may be more cheaply and truly executed than with the old sort of frame.

Previously to entering into the expense of this sort of work, on my conceptions as to its advantage, it may be necessary briefly to state from whence such is collected.

About sixteen or eighteen years ago, the late Duke of Bedford directed a foreigner, who was then making some walls in Lancashire, to come and make some specimens here, and wishing to know how far it might be usefully introduced, I was directed to give attention and every aid to the man employed. Accordingly frames of the old sort were made, exactly like those before described, and with them some specimens being made, the man returned. These specimens I considered were very bad walling, and in attending to the execution thereof, seeing sufficient room for improvement, I was directed further to practise it. Frames were then constructed like the model, and several walls erected, among which were some cottages now standing, and

and lastly, the house I now live in. This has been built about twelve years, and is a sufficient proof of the utility of the practice: the house being as close, warm, and dry in the walls, as if built of any materials whatever.

With regard to the expense of the walls of this sort, as labour is the principal part of the expense, and as in some places labour is dearer than in others, the best mode of estimating it at different places will be from the quantity that a man should do in a day, and which I have found to be 1¼ yard superficial, in the common day's labour of ten hours.

At this place the expense may be estimated as follows:

	£.	s.	d.
Labour to making facing composition, fitting in and ramming to a 16-inch wall, where the earth is at hand (labourer's being 1*s.* 10*d.* per day) per yard superficial	0	2	2
Value of lime used in the composition rammed into the face of a yard superficial (lime being 8*d.* per bushel)	0	0	3
Lime and labour to rubbing up and finishing the outside face of the wall	0	0	3
Total finished and faced on one side	0	2	8
If a wall to a garden or otherwise, and finished and faced on both sides, then add	0	0	8
Total for walls finished on both sides	0	3	4

At this place the value of a yard of brick-work is more than ten shillings, of walling only 14 inches thick, the bricks being 42*s.* per 1000, and lime 8*d.* per bushel; consequently the economy of the *pisé* must appear; and the same difference will be found in any other place where lime and bricks bear the same price, and proper earth can be found at hand. But as attempting this sort of work, where it is not applicable, or improperly doing it so as to lead to failure, may prevent its introduction where it would be useful, I shall endeavour to point out any precautions that have struck me, and every thing that has appeared to make against it.

Many persons have supposed, and it has been asserted, that almost any earth will do: but such is certainly very erroneous; for proper earth cannot in all places be found; and it being difficult to describe it, or to be sure when it is found, it seems adviseable, before the entering on any considerable

siderable work, that the experimentalist should first do a small piece, and let it stand with the top only covered for a winter at least.

It has before been observed that the excellence of the work depends on its having due compression, as well as being of proper soil. If the compression be not perfect, although the soil be good, the walls will be unsound; and unfortunately it so happens, that when a wall is built and badly rammed, its imperfection cannot readily be observed, and further, the defect is likely only to be found but by its failure: and hence arises the greatest bar to its general introduction; for, as it requires considerable labour to build a wall, it requires exertion to do it in proper season; and if the labourer be employed to do the work by task, it becomes his interest to get on and do it slightly, and if done by day, it will not advance so rapidly: consequently, in either way, it will require great attention from a careful overlooker.

From the foregoing comparative statement of *pisé* against brickwork, persons unacquainted with building are inclined to suppose that the whole expense of the building will be in proportion thereto: contrary to this, it only affects the walling,—the roof, floor, &c. remaining the same as before, excepting as it may reduce the quantity of bond timber and lime used in plastering the inside; this latter is less than when plastered on brickwork, the face of the wall being so much truer than brickwork.

A working drawing, on a scale of one inch to a foot, is left with the Society, for the inspection of any person inclined to construct the apparatus.

LII. *Memoir on the Alterations which the Light of the Sun undergoes on passing through the Atmosphere. By M.* Hassenfratz. *Read to the Class of Mathematics and Physics of the French Institute, 20th October* 1806.*

The sun presents different colours to our eyes: its disk appears white, yellow, orange, or red, according to the purity of the air, the height of the orb in the horizon, the latitude of the places where we observe it, and their elevation above the level of the sea.

In the torrid zone, the disk of the sun is always white, when the air is pure, and when it is at the zenith of the place.

In the frigid zone, the disk of the rising or setting sun is always red in the shortest days of the year.

* *Annales de Chimie,* tome lxvi. p. 54.

On

On high mountains, at an equal height above the horizon, the disk of the sun is constantly seen whiter than in valleys and plains.

In general, the white, yellow, orange, or red colour of the disk depends (if the purity of the air be the same) on the thickness of the strata of air which the ray passes through before reaching the eye of the spectator: the thinner the strata which are passed through, the whiter is the disk: and *vice versâ*; the disk being at first yellow, then orange, and afterwards red. When the air is filled with exhalations, when it contains solid or liquid substances in suspension, and when its purity is thereby affected, the disk of the sun is sometimes coloured; but more frequently the intensity of its light diminishes, and the disk remains white*.

The cause of the colouring of the sun's disk is one of those problems which ought to occupy the attention of philosophers, and which interests them the more in proportion to its influence in the phænomena of optics.

It has been strongly asserted, that this effect was caused by rays subtracted from the fasciculus during its passage through the air; but we are entirely ignorant what is the number and species of the molecules subtracted. Each of the colours of the disk may result from the separation of one or more coloured molecules: the only condition that ought to be fulfilled, is, that the colours engendered by the subtracted rays should be complementary to those which are perceived.

The azure colour in which the sky appears to our eyes, has induced some persons to suppose that it was by the subtraction of some blue molecules reflected by the air that the colour of the disk was produced: others have thought that the purple, violet, blue and green rays, being more refrangible and more reflexible than the others, were separated from them in passing through the atmosphere; and that the red, orange and yellow rays combined with them, which had not been reflected, occasioned by their junction the colour of the disk: finally, others have presumed that the violet and green molecules were reflected by the air at the same time that the red molecules and some green molecules were refracted.

Although the colour of the sun's disk seemed to announce an action of the air on the coloured molecules; and

* *Hist. de l'Acad. des Sciences*, 1721. De Mairan relates an observation of the sun having appeared the whole day through vapours. Its disk was white, and its lustre as usual, but without any rays.

although

although it ought to be presumed that the solar spectrum should undergo variations flowing from this action; as no astronomer, to my knowledge, has announced, that in the experiments which have been repeated at various times, and in different places, variations have been observed in the colour of the spectrum, and as all are silent as to a phænomenon so singular; it might be supposed that the cause of the colouring of the disk depended on an order of alteration with which the spectrum was not affected.

Those hypotheses which attribute to the molecules of air properties so different were presented under an aspect more or less seducing; all of them could be discussed, defended, and adopted, if the reasonings and authorities of authors had been sufficient: but as none of those who have proposed them have supported them by any positive facts, I appeal to experience.

The most natural experiment, and that which every author ought to make before proposing his hypothesis, is the analysis of the rays of light when the disk of the sun is presented under colours so various. This analysis has been effected, and I now present the results to the class.

I fixed upon some fine days in the summer of 1799, when the sky was pure and the disk of the sun white, which always happens towards noon, when the sun is at its greatest elevation.

With this view I introduced a solar ray into a dark room through an aperture of the size of 25 decimillimetres: I received it on the surface of a prism, which I turned so as to make the angles formed by the ray refracted, and the two faces of the prism equal. I observed at one and the same time both the series of the colours of the spectrum and its length at 36 decimetres distance from the prism. I remarked that all the colours were perfectly distinguished from the purple to the red, and that on the evening of the same day, at sunset, when its disk appeared yellow, the spectrums formed by the solar rays were not so long: the purple no longer existed, and a greater or less portion of the violet was wanting, and sometimes it was even entirely wanting.

The experiments thus made at noon and evening were recommenced in the fine weather of the following years, and gave the same result.

I have the honour to present to the class three spectrums obtained with the same prism on the 13th of January 1801. They are remarkable for their length, and for the colours which form them. At half past ten o'clock A. M., the spectrum, fig. 1, (Plate VII.) was 145 millimetres long; at at noon, fig. 2, 185; at four o'clock P. M., fig. 3, 110; and at ten minutes past four, fig. 4, 100.

When the spectrum was longest, the yellow interposed between the green and the orange was pure; when the violet disappeared, the yellow assumed a deeper tinge: it was coloured red, and partook of the orange colour.

I repeated these observations several times, at various times of the year, and always with the same success; the spectrum increased or diminished in length according as the colour of the disk was whiter or yellower.

Finally, on the 15th of January 1805, I remarked, on observing the decomposition of the light of the setting sun, that when the disk of that orb was of a fine red, the length of the spectrum was diminished more than one half: it was no more than 70 millimetres long; whereas at mid-day it was 185; and in the series of colours separated by the prism, we could only distinguish the red, orange, and green.

M. Gerard, draftsman to the polytechnic school, having been present on the above occasion, drew and coloured the solar spectrum in question. Figure 5 is the copy of the drawing, and fig. 6 presents the image of the sun when it was received on a white card in the dark room: this tint is deep orange, approaching the blush of dawn.

The subtraction of one or several coloured rays in the fasciculus which the sun sends us, when its disk is yellow, orange, or red, may be easily remarked in the irises observed at different hours of the day, either in the series of colours which they present, or in the breadth of the coloured arcs. I have several times verified this fact since my experiment in 1802; an experiment which, I must confess, then seemed to be very important. I have even remarked in the sky, when the disk of the sun was red, irises which contained only red, orange, and green, like the figure of the spectrum which I have presented to the class.

From these facts we may conclude, that among the causes which may produce the alterations observed in the colour of the sun's disk, one of the most important is the subtraction of the coloured rays intercepted by the medium which they pass through; and the coloured molecules separated from the fasciculus of white light are, the purples and a part of the violets, when the disk appears yellow: the purples, the violets, the yellows, and a part of the indigo blues, when the disk appears orange: the purples, the violets, the indigo blues, the blues, the yellows, and a little of the orange, when the disk appears red: finally, that there may be a term, a colouring of the disk in red, under the polar circle, at which all the coloured molecules, at least the red,

are

are subtracted by the air, and that in this case the spectra and the irises ought to present a single colour only, which is the red.

LIII. *On the Application of the Barometer for indicating the Weather, and for measuring of Heights in the Atmosphere.* By RICHARD WALKER, *Esq.*

To Mr. Tilloch.

SIR, IN order to prognosticate the weather by means of the barometer, one general rule should be premised, viz. that, previously to observing the barometer, the state of the weather at the time should be accurately noticed in every particular. Hence, to speak figuratively, we might affix this motto to the barometer, "Tell me what the weather is, and I will tell you what it will be."

The circumstances to be collected previously to inspecting the barometer are, 1st, The state of the atmosphere, respecting its degree of clearness or cloudiness: 2dly, The direction of the wind, together with its steadiness or variableness: and 3dly, The altitude and density of the clouds.

Signs of Fair Weather.

1. The barometer *rising* may be considered as a general indication that the weather, comparatively with the state of it at the time of observation, is becoming clearer.

2. The atmosphere apparently becoming clearer, and the barometer above RAIN, and rising, show a *disposition* in the air for fair weather.

3. The atmosphere becoming clear, and the barometer above CHANGEABLE, and rising, indicate fair weather.

4. The atmosphere clear, and the barometer near FAIR, and rising, denote *continued* fair weather.

5. Our prognostic of the weather is to be guided, relatively, thus: If, notwithstanding the sinking of the barometer, little or no rain follow, and it afterwards rise, we may expect continued dry weather.

6. If, during a series of cloudy rainy weather, the barometer rise gradually, though yet below RAIN, especially if the wind change from the south or west towards the north or east points, clear and dry weather may be expected.

7. The weather for a short period, viz. from morning until evening, may commonly be foretold with a considerable degree of certainty. If the barometer has risen during the night and is still rising, the clouds are high and apparently dispersing, and the wind calm, especially if it be in or about the north or east points, a dry day may be con-

 fidently

fidently expected :—the same rule applies for predicting the weather from evening till morning.

8. During the *increase* of the moon there seems to be a greater disposition or effort in the air for clear dry weather than in the *wane:* but this disposition does not usually commence till about three or four days after the new moon, and ceases about three or four days after the full moon.

9. The barometer should be observed occasionally thrice in the day, or oftener when the weather is changeable, in order to notice whether the mercury be stationary, rising, or sinking; for from this circumstance, together with the direction of the wind and the apparent state of the air at the time, is information to be collected, and a continuance of the same, or a sudden change of the weather, to be foreseen*.

10. Lastly, Observe always—The higher the mercury shall stand in the scale in each instance, and the more regularly progressive its motion shall be, the stronger will be the indication: likewise, The more the wind inclines towards the north or east points, the greater will be the disposition in the air for fair weather.

The indications of *rainy weather* will obviously be the direct reverse of those rules which predict fair weather.

Frost is indicated in winter by the same rules that indicate fair weather, the wind being in or about the north or east points, and the thermometer sinking towards 32.

A fall of snow seldom comes without a previous frost of some duration, and is indicated by the sinking of the barometer, especially if the mercury be below CHANGEABLE, and the thermometer at or near the freezing point.

When the temperature of the air is about 35°, snow and rain sometimes fall together; at a *warmer* temperature than 35° it seldom snows, or rains at a *colder* temperature.

Thunder is presaged by the same rules which indicate rain, accompanied by sultry heat; the thermometer being up to 75.

Storms, hurricanes, and high winds, are indicated by the barometer falling *suddenly*, or sinking considerably below MUCH RAIN.

The barometer is known to be rising or sinking by the mercury having either a convex or concave surface, or by the perceptible rise or descent of the mercury if at the time of observation the barometer be gently rapped†.

If at any time the weather should differ widely from the indications

* A barometer, conveniently portable, merely for the purpose of ascertaining whether the atmosphere is becoming *denser* or *rarer*, is a great desideratum, but, I should apprehend, not very easy to be constructed.

† The best index for these observations is a plate of metal extending as far as the *middle* of the column of mercury in the barometer, having a *pointing line* across the centre of the plate.

indications of the barometer, it may be presumed, as is sometimes known to happen, that a particular spot is affected by local circumstances.

After a long continued series of wet weather, we may, when the weather becomes fine, expect an uninterrupted continuance of dry weather.

If, after a long series of wet weather, the barometer rise above CHANGEABLE, and the wind veer steady to the north or east points, a continued duration of fair weather may be expected.

Slow and progressive variations in the barometer, with a fixed and steady state of the wind, indicate permanency with the change.

The barometer standing at or above FAIR, denotes *generally* fair weather, although the atmosphere wear at the time an unfavourable aspect.

Lastly, The greater coincidence there is of the circumstances enumerated in the rules above mentioned, the stronger may our confidence be in the expectation of fair weather, and in the continuance of it when present, by the barometer whilst high, remaining stationary, or varying but little, and the state of the atmosphere, and direction of the wind, disposed to be settled.

In this variable climate, there is no reliance, I think, to be placed on any rules, beyond those above mentioned, for indicating the weather for any length of time together, or for any distant period.

Many of these rules, perhaps, may appear trite, and as if collected from the observations of others; but, unconscious of retaining those of any other person in my mind, I give these as the result of my own experience.

A Summary Method for ascertaining without the Use of Logarithms, *the various Degrees of Elevation in the Atmosphere, by means of the Barometer, which, by the Examples that follow, will be found to give a tolerably close Approximation to the geometrical Measurement.*

The following Tables are applied thus:

1st. Note down the height of the barometer at the *lower station*, in inches, tenths, and hundredths *; and the temperature of the air, as indicated by the thermometer.

2dly. In a similar manner note down the height of the barometer at the *upper station;* and the temperature of the air (if this differ from the former observation).

3dly. Subtract the quantity shown by the barometer in the *upper station*, from that which is shown in the *lower station.*

* I have judged it unnecessary to descend below *hundredths* on the barometer.

4thly. Seek in the column of temperatures, Table I., for the temperature observed at the stations, (or for the mean of the two, if they differ,) and note down the number of feet, tenths, and hundredths, which are placed opposite to that temperature in the adjoining column.

5thly. Make corrections (if requisite) from Table II.

6thly. Multiply the numbers, so found and corrected, by the difference between the altitudes of the barometer at the lower and upper stations.

7thly. Add to the product the increased ratio, from Table III., which finishes the process.

Table I. exhibits the number of feet, tenths, and hundredths, in perpendicular altitude, indicated by the descent of the mercury in the barometer for each *hundredth* part of an inch; which descent varies according to the *temperature of the air*, at the rate shown in the corresponding column. This table is constructed on the supposition that the barometer at the *lower station* is at 30 inches.

N. B. Although this calculation be given for each *hundredth* part of an inch of variation on the barometer, it may be used for variations of *one tenth*, or of *one inch*, by merely altering the denomination of the figures in the column, thus:

	Feet. Tenth Hund.
The variation of 100th part of an inch, at temperature 90°, indicates altitude of	9, 9, 5.
	Feet. Tenth.
The variation of a 10th part of an inch	99, 5.
	Feet.
The variation of an inch	995.

Table II. shows the alteration which must be made in the numbers taken from Table I., when the barometer is *not* at 30 inches in the *lower* station. If it be *lower* than 30 inches, the number of *feet*, which are placed opposite to the barometer's height in this table, must be *added* to the feet of altitude corresponding with the descent of the mercury in the barometer for *one inch*. If it be higher than 30 inches, *subtract* instead of adding.

N. B. If the variation of the barometer be not so much as an inch, but only some tenths of an inch, then the figures in the second column will not represent *feet*, but *tenths of feet* to be added or subtracted.

Table III. shows the number of feet to be *ultimately* added to each 100 feet of altitude. This number varies from 1 to 46, according as the barometer in the *upper station* stands between the altitudes of 31 and 14 inches.

N. B. A table for the correction, or equation of heat, in the barometer, when the temperature of the atmosphere differs at the upper and lower stations, is provided for here, by the adjustment of the table of temperatures.

TABLE

Table I.

Temperatures.	Altitudes for the Variation of every 100th of an Inch on the Barometer. (Feet. Tenth. Hund.)
90	9,9,5
88	9,9,0
86	9,8,5
84	9,8,0
82	9,7,5
80	9,7,0
78	9,6,5
76	9,6,0
74	9,5,5
72	9,5,0
70	9,4,6
68	9,4,2
66	9,3,8
64	9,3,4
62	9,3,0
60	9,2,6
58	9,2,2
56	9,1,8
54	9,1,4
52	9,1,0
50	9,0,6
48	9,0,2
46	8,9,8
44	8,9,4
42	8,9,0
40	8,8,6
38	8,8,2
36	8,7,8
34	8,7,4
32	8,7,0
30	8,6,6

Table II.

Height of Barometer. (Inches. Tenths.)		Feet.
5	Subtract.	15
4		12
3		9
2		6
1		3
30,0		0
9	Add.	3
8		6
7		9
6		12
5		15
4		18
3		21
2		24
1		27
29,0		30
9		33
8		36
7		39
6		42
28,5		45

Table III.

Increasing Ratio.

Inches. Tenths.	Feet.
31,0	1
30,0	1
29,0	2
28,0	3
27,0	4
26,0	5
25,0	6
5	7
24,0	8
5	9
23,0	10
5	11
22,0	12
7	13
3	14
21,0	15
7	16
3	17
20,0	18
7	19
3	20
19,0	21
7	22
3	23
18,0	24
7	25
5	26
3	27
17,0	28
8	29
6	30
4	31
2	32
16,0	33
8	34
6	35
4	36
2	37
1	38
15,0	39
*14,0	46

Example

* On the top of Chimborazo, one of the Andes, in South America, and the

Example 1.

	Inches. Tenth Hund.	
Lower station	29,0,0	
Upper station	28,0,0	
Difference	1,0,0.	
Temperature of air 60° .... =	926	feet to 1 inch of variat^{n}.
For 29 inches at lower station, add	30	feet from Table II.
	956	
Increased ratio, 3 to each 100, Table III =	28	
Estimated height ..	984	feet.

Example 2.

	Ft. Tenth.	
Lower station	30,0,1	
Upper station	29,5,3	
Difference	0,4,8.	
Temperature of air 60° .... =	92,6	to each 10th of an inch variation.
(Lower station 30 inches, no correction)		
Multiply 92,6 feet by 48 tenths .... =	444,5.	
Add for increasing ratio 1½ per 100 . =	6,	
Estimated height ..	450,5.	
Height by geometrical measure	451,2.	

Example 3.

	Inches. Tenth Hund.	
Lower station	29,9,7	
Upper station	26,2,8	
Difference	3,6,9	
Mean temperature of air 54° .... =	914	feet to each inch of variation.
For 29,9 at lower station add (Table II.)	3	
	917	

the highest mountain in the world, a barometer would stand nearly as low as 14 inches, its summit being 20,280 feet above the level of the sea. The greatest height to be relied on, to which any person has ascended with a balloon, is that at which the barometer stands at about 24 inches, which is equal to 6,027 feet; though some are said to have reached 16,000 feet.

Multiply

	Feet. Tenth.
Multiply 917 by 3,6,9 ....=	3383,7
Add, increasing ratio 5 per 100....=	169,
Estimated height..	3552,7
Height by geometrical measure......	3558 feet.

Example 4.

	Inches.Tenth Hund.
Lower station	29,9,3
Upper station	16,8,1
Difference	13,1,2

Mean temperature of air 64° ....=	934	feet to each
For lower station 29,9 add..........	3	inch of
		variation.
	937	

	Feet. Tenth.
Multiply 937 by 13,1,2.........=	12,293,4
Add increasing ratio 29 per 100..=	3,565,
Estimated height..	15,858,4
Height by geometrical measure.....	15,833 feet.

N. B. In the three last examples, the heights of the barometer at the different stations, and the geometrical heights corresponding thereto, are extracted from different works of unquestionable authority. I am, sir,

Your obedient servant,

Queen-street, Oxford, Oct. 13, 1810.

RICHARD WALKER.

LIV. *Œconomical Process for the Preparation of the sublimed Muriate of Mercury (Calomel): to which is subjoined, an easy Method of purifying the Calomel used in Commerce. By M.* PLANCHE*.

ANXIOUS to avoid the inconvenience which results from the employment of corrosive sublimate in the preparation of calomel; convinced by experience that the various methods proposed with this view by Van Mons and Brugnatelli were insufficient; and considering the discrepancy which prevails in most of the pharmacopœias of Europe, as to the

* *Annales de Chimie*, tome lxvi. p. 168.

doses of corrosive sublimate, and of metallic mercury, which ought to be used in this preparation; M. Planche, after numerous trials, has ascertained that it is sufficient, in order to obtain calomel, to sublime a mixture of sulphate of mercury at the *minimum*, and of dry muriate of soda.

Preparation of the Sulphate of Mercury. Introduce into a stone retort placed in a reverberating furnace one part of crude mercury, and one part and a half of sulphuric acid at 66° of Baumé's areometer. Fix an adopter and a tubulated receiver to the retort, which must be made to communicate either with distilled water contained in Woulf's flasks, if we wish to collect the sulphurous acid; or with the external air, if the situation of the place admit of the gas being set at liberty. Gradually heat the retort until the acid boils, and keep up the fire while the acid vapours are disengaged in abundance, taking care to slacken it towards the end of the operation, *i.e.* when the drops of the liquid which passes from the retort into the bell-glass succeed each other slowly, and when there is a diminution of the white vapours. After this operation, which lasts four or five hours, the retort may be broken; or, rather, we may separate by means of tongs the sulphate of mercury, which is easily detached.

The acid sulphate of mercury thus obtained is very white and very friable; it passes to the yellow colour on the addition of the most trifling quantity of cold water. In order to carry this salt to the state of sulphate at the *minimum*, the author combines it with quicksilver in the following manner.

He takes 18 parts of the above acid sulphate of mercury, and eleven parts of mercury. He triturates them together in a mortar, or in a porcelain capsule, adding by degrees six parts of cold water.

The first portions of water make the sulphate assume a yellow colour, which soon disappears on shaking it. Heat is developed. The matter assumes a very deep gray colour. After a few minutes trituration, he adds a sufficient quantity of water to give to the whole the consistence of a thick broth; and he continues to triturate until the mass has become of a dirty white, and the mercury has totally disappeared, which lasts for five or six hours when the mass is considerable. He afterwards dries this substance in a stove at a temperature of 30° to 35° of Reaumur.

M. Planche is of opinion, that the mercurial mass which results from this operation is in the state of sulphate at the *minimum*, and he proves it by the following experiments.

1. It

1. It is soluble in distilled water, and its solution does not alter the tincture of turnsole or the syrup of violets.

2. It is precipitated black by lime water, and gray by ammonia.

Preparation of the mild Muriate of Mercury. To convert the sulphate of mercury into calomel, mix intimately, on a porphyry stone, equal parts in weight of the sulphate of mercury as above designated at the *minimum*, and of sea salt purified and dried: introduce the mixture into matrasses with flat bottoms two-thirds of which are left empty, and proceed to sublimation in the usual way. After the operation, which lasts five or six hours, there will be found in the arch of the subliming vessel a loaf of calomel of the weight of about 30 ounces, if four pounds of mixture are operated upon. This salt is as white as that of commerce, and purer than that which we commonly meet with as coming from the laboratories of Switzerland.

In order to add to its purity, particularly when the heat has not been well managed, the author of the memoir proposes the following method, which perfectly succeeded with him, and which has the advantage of having no action on the mercurial salt.

Purification of Calomel. Pulverize the calomel in a mortar of marble or of hard stone. Pass it through a fine hair sieve in order to obtain a homogeneous powder tolerably fine. Introduce the pulverized salt into matrasses of the same form as in the foregoing operation: afterwards cover it with a layer about two lines thick of fine sand, previously washed with water slightly sharpened with muriatic acid, in order to free it from the carbonate of lime and oxide of iron which are mixed with it, and sublime as before directed. The calomel purified by this process is very pure. M. Planche has presented to the Institution a loaf of it very regularly crystallized, and in whiteness equal to that of corrosive sublimate.

LV. *Description of a Process by means of which we may metallize Potash and Soda without the Assistance of Iron. By M.* Curaudau *.

The decomposition of the alkalis, which I have never regarded as simple bodies, having been long an object of my inquiries, I became anxious to repeat the experiment according to which Messrs. Thenard and Gay Lussac have announced

* *Annales de Chimie*, tome lxvi. p. 97.

announced that potash and soda might be converted into metal by means of iron; but not having obtained results more satisfactory than those who to my knowledge have repeated the same experiment, I continued my inquiries, which appeared to me to be the more likely to be successful, as Mr. Davy had thrown so much light upon certain phænomena which I had observed, but could not till then account for.

In short; if, according to the hypothesis of the celebrated English chemist, potash and soda were metallic oxides, was it not more than probable that the prussic calcinations were nothing else than the combination of this metal with charcoal? Such at least was my opinion then, and it will be seen how far it was well founded, since I succeeded in metallizing potash and soda, by heating strongly one of these two alkalis with charcoal; a process which, as we shall find, enters into the prussic calcinations.

The metallization of potash and of soda taking place with one or other of the two mixtures which I have pointed out, and succeeding equally well in stone retorts as in iron pipes, we may employ the former or latter process indiscriminately. As to the nature of the vessel, I prefer an iron one, because it is more permeable to the caloric, and less subject to fuse than stone, particularly when the latter is penetrated with alkali; an inconvenience which prevents us from bringing the operation to an end: which does not so frequently happen with the iron.

First Process. Mix exactly four parts of well pulverized animal charcoal with three parts of carbonate of soda dried in the fire without having been melted: combine the whole with a sufficient quantity of linseed oil, but so as not to make a paste.

Second Process. Take two parts of flour, and mix them intimately with one part of carbonate of soda prepared as in the foregoing experiment: add to the mixture a sufficient quantity of linseed oil, but so as not to prevent it from being in a pulverulent state.

Whatever be the kind of vessel employed for calcining the substance in question, and whether the first or second mixture be used, we must always begin by heating gradually: but as soon as the matter is a dull red, we may increase the fire until we see in the inside of the retort or iron pipe a fine celestial blue light, the areola of which is greenish. To this light there soon succeeds a very abundant vapour, which obscures the whole inside of the vessel, and which is the metal extricating itself from the mixture. The

The fire must no longer be increased, for at this temperature the retort begins to melt; and if the iron resists better, it is because the alkali penetrates it less speedily than it does the stone; and also, because the heat which it receives is sooner transmitted to the matter.

In order to collect the metal as fast as it is formed, introduce into the vessel a piece of iron well scoured; and as we must not give it time to become red, it must be withdrawn in four or five seconds: it is then entirely covered with metal, which may be removed by suddenly plunging the iron into a glass cucurbit filled with spirits of turpentine. This cucurbit ought to be dipped in a bucket of water, in order to prevent the spirits of turpentine from boiling. Still, however, in spite of this precaution, it is sometimes so heated as to take fire when the pieces of iron are introduced into it.

Requisites for the Operation. It requires three persons to perform the operation well. One must work the bellows, and take care of the fire: the most expert of the attendants collects the metal as it is produced, and plunges with the utmost celerity the pieces of iron into the turpentine: the third assistant removes the metal which adheres to the iron, and afterwards dips it into the water, as well to cool it as to remove the alkali which has escaped metallization, and that which is formed by the combustion of the metal before its immersion into the turpentine. He takes care also to clean the pieces of iron well before using them.

This operation requires the most dexterous manipulation while the metal is forming. The bellows must also be carefully managed; for, if the fire be suddenly slackened, the metal ceases to be set free, and the pieces of iron are covered with pure alkali only: if, on the other hand, the fire is hastily increased at this instant, the vessel melts, and the experiment is fruitless. This proves, therefore, that the temperature ought to be uniform and steady. I have observed that it is always at the heat of melting iron that the metal is produced. It rarely happens that an iron pipe serves twice, and the retorts melt long before the whole of the metal is obtained which the alkali can produce.

I purpose subsequently to make known any observations which I may happen to make on this metallic produce: in the mean time I think I may infer from my experiments, that the production of the metal is not owing, as has been said, to the deoxygenation of the alkali, but is on the contrary a new compound, into which hydrogen seems to have entered

entered in combination, and which in my opinion would be in a very condensed state in it *.

To conclude:—During the whole operation, hydrogen is extricated, alkali not metallized, and radical prussic gas. I have collected this last product in particular in great quantities.

These results tend, therefore, to prove either that hydrogen is one of the constituent parts of the alkalis, and the disengagement of which is favoured by charcoal, or rather that the charcoal itself is a compound of which hydrogen is one of the principles. We must choose between one or other of these hypotheses.

LVI. *Reflections on some Mineralogical Systems.* By R. Chenevix, *Esq. F.R.S. and M.R.I.A., &c. Translated entire from the French, with Notes by the Translator* †.

FUNDAMENTAL PRINCIPLES AND GENERAL EXAMINATION OF THE WERNERIAN SCHEME.

Nature has given to all bodies properties which are either immediately or mediately sensible. This is the basis of all systems of mineralogy. One mean of rendering our learning useful is by establishing unities, to which every thing may be referred, and which we afterwards adopt either wholly or in part. We seek a principle the most general and least variable possible, to employ it as a basis for the determination of these unities: in natural history, it is agreed to call the latter by the name of *species*. In the vegetable and animal kingdoms, the faculty of reproducing individuals fecund and similar to their parents constitutes a species.

This principle of specification has been received as lawful; but it is not applicable to the mineral kingdom. Does there exist any other? If we consult the ancient works on mineralogy, we are tempted to believe that it is totally wanting. In more modern times, M. Werner is engaged in seeking and noting in minerals every thing that immediately strikes

* The opinion expressed in this paragraph has been since retracted by the French chemists.—Edit.

† As the learned author has not thought proper to publish these ingenious and scientific reflections in his native language, a circumstance much to be regretted, the translator has, for the sake of perspicuity, taken the liberty of classing them under different heads, according to the subject discussed: he has also ventured occasionally to introduce in [ ] the real or approximate synonyms of the various minerals mentioned by the author, in order to enable the English reader to form more precise notions of the different substances, and feel the force and justice of the author's reasoning. B.

strikes the senses; and he has succeeded in uniting the principles which enable him to class and distinguish minerals into a body of doctrine. The colour, brilliancy, fracture, and other properties, have been examined in a point of view the best calculated to attain this object: the advantages which a knowledge of the latent qualities may offer have not been neglected;—these labours have procured the author the approbation of the learned world. In this manner a great step has been made; and if it does not conduct us entirely to our object, it at least demonstrates the difficulty of attaining it. M. Werner has said (Memoir of Daubuisson, *Journal de Physique*, Frimaire, 16th year), "that all the minerals which have the same constituent parts, both with respect to quality and quantity, form only one species; and that all those which differ essentially belong to different species. If, in the same species," he adds, "divers minerals having the same characters (one only excepted) differ from others in two or three characters (a greater number would induce a difference of species) from those which we have designated, they form a particular subspecies. Finally, When an individual in a species, or subspecies, presents but one different character, it forms a variety."

To the word *essentially* I have two objections. In the first place, it does not excite the same idea in all minds, and we can have no precise notion respecting it, while it has no fixed signification. In the second place, the chemical means which could enable us to pronounce with some certainty on what belongs essentially or accidentally to the composition of a mineral, are entirely omitted. But let us suppose, for a moment, that we have acquired the necessary knowledge. *The chemical composition is therefore the true basis of specification in this system**. We also learn from the above quotation, that when two minerals of the same species differ in one

* To this the Wernerians object, that it is degrading to mineralogy to be dependent on chemistry; that it is possible for a man to be a very good mineralogist without being previously a chemist; and that they are two distint and independent sciences. In support of these positions, they sometimes appeal to the increasing number of botanical nomenclaturists who are not vegetable physiologists: but the allusion only tends to place mechanism before science; the former are to the latter what sculptors and lapidaries are to scientific mineralogists. The Wernerians, therefore, when they reject chemical science, and build solely on their external characters, place themselves on precisely the same basis as the lapidaries and sculptors; they become artists, but not men of science properly so called. They may indeed be most acute observers, very accurate reporters of their observations, and even pioneers in the fields of mineralogical science; but they ought not to aspire to be world-makers, or attempt to raise any superstructure without the aid of chemistry; while mineralogy and particularly geology are not less sciences of deduction than of observation.—Trans.

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character, they are varieties; if in two or three, they form subspecies. If the number of dissimilar specific characters exceed three, the minerals thus characterized must belong to different species. Here, then, is a second idea of the species which has just presented itself; and to reconcile it with the first, we have only to believe that the variation of more than three specific characters is an inseparable consequence of an essential difference in the chemical composition of minerals.

We cannot be perfectly sensible why the number three should be that which characterizes a change in the chemical composition, and we are tempted to believe that it has been taken at random. It is nevertheless founded on a motive. The species is subdivided into subspecies and variety; the variety, therefore, is the last subdivision of the species; and to determine the last subdivision but one, the variation of two or of three has been chosen. It is, therefore, necessary to carry beyond three the difference of species; that is to say, (according to the passage already quoted,) the essential difference in the chemical composition. I have said that there was a very good motive for choosing the number three; but I have not said that it was susceptible of being acknowledged by nature.

In establishing the difference of species solely on the difference of any determinate number of specific characters, we render it independent of every consideration either of the value of the characters, or of the extent in which the shades of these characters differ. In this hypothesis it matters not, that two minerals have two or even three characters so different that they form, if we may so speak, the extremes of the series of characteristic analogies: if we cannot discover a fourth which is also different, these two minerals must belong to the same species: or let any number whatever of minerals that I can divide into two series form a difference in three specific characters; let all those of the first portion be opake, ductile, and so soft as to be cut with the nail; let those of the second be transparent, not ductile, and so hard as to resist the file; let all the other characters be entirely similar in every respect, still the individuals which compose these two portions are of the same species, since the number of different characters does not exceed three. Moreover, let us suppose all these minerals of a brownish red colour, except one only, which may be a reddish brown. Here we must now renounce these principles in which the single shade between brownish red and reddish brown, (for M. Werner places them in two different species

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among the colours) effects the translation of a mineral from one species to that of another, as it is that which, superadded to the first, forms the fourth characteristic difference, while the other minerals always remain in the same species as before.—It is the last pound under which the camel succumbs.

Here is a second example of the very serious inconveniences arising from making the species depend on any fixed number of characters. Form is a character, crystallization is comprehended in forms. M. Werner admits the prism only where there exist certain proportions between its height and its breadth; if we diminish the former, the prism becomes a table. The prism and table are considered as making two different species of primitive forms. Take a crystal of calcareous spar in a hexahedral prism, and another crystal of the same in a hexahedral table; here is a different primary specific character in these two specimens of calcareous spar. Suppose the table very near becoming a prism, and the prism approaching very near to the table; and suppose the one translucid and the other opake; add two specific characters not less insignificant than these two, and behold a new species at very little expense. But, had the causes which determine crystallization added some molecules of carbonate of lime to the calcareous spar, in the direction of the axis of the prism which was considered a table, it would have been saved from this forced separation from its equals.

In establishing a subspecies, if the liberty of choosing two or three for the number of characters by which the observer decides, leave any influence to the particular value of each character, it is necessary that this value should rest on a solid basis; otherwise we risk the danger of making arbitrary dispositions, and the same mineral may be found belonging to as many different species as there will be persons who shall examine it. M. Werner, indeed, has distinguished some characters by the order of importance in the determination of the species, as well as in the diagnosis of minerals. He gives sometimes to the specific gravity, sometimes to the colour, more value than to the greater part of the other characters. I have seized every opportunity to acquire clear ideas on this subject, either in consulting M. Werner himself, or addressing those who had profited most by his instructions; and all that I have been able to learn amounts only to this, That the value of a character varies from one species to another: thus, then, to decide on it, it is necessary to know the species, that is to say, in

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algebraic language, for to find the value of x, we must commence with knowing it.

WERNERIAN DIVISION OF THE EXTERNAL CHARACTERS.

M. Werner has divided the external characters into generic characters, specific characters, and characters of varieties which influence the systematical distribution of minerals under analogous denominations. The colour, lustre, and specific gravity undergo subdivisions. White, gray, black, blue, green, yellow and brown, are *species* among the colours. The shades of these characters form subdivisions, and they are pronounced in adding an epithet to the word which designates the specific character. Thus, celadon- or sea-green is a variety of green; gosling- (*serin*) green is another; sky-blue is a variety of blue, as sulphur-yellow is one of yellow. These distinctions cannot be mistaken as soon as we understand to pronounce the attributes of the specific colour according to the rules. But the difference between celadon-green and gosling-green is really greater than between sky-blue and celadon-green, and the same between gosling-green and sulphur-yellow: that is to say, the varieties of the same species differ more from each other than two species differ. This mode of distribution may suffice for the nomenclature, but by no means for the thing; it satisfies the ear, because the ear does not judge of colours.

The division of external characters into specific and generic characters, and characters of variety, places us in a new difficulty; for we here see a third principle of classification relative to minerals. We had the number of different characters, and the value of each character; now we have the intensity of these same characters. It is also impossible to see clearly how we ought to form species and varieties in minerals: if it is by the number solely, we exclude the importance of characters, and the shades are all of the same value; if we concede any thing to the importance, we must modify the rules respecting the number; and if the character of variety be sufficient to establish the mineralogical variety, as but one is wanting, What shall we do when it is a specific character which differs? How many characters of variety are equivalent to a specific character? How many to a generic? In all these, too, we must carefully avoid taking the least possible difference of characters to establish the mineralogical subdivision, which is not itself the smallest.

Unity of principle in a system of classification is that which tends most to give it precision. If we feel ourselves obliged to admit several principles, it loses this advantage, unless

unless that the consistence of these principles be not so necessarily united as to prevent their separation. For example; if a certain colour was an inevitable consequence of the presence of a certain constituent part, we might adopt the colour as a principle of classification, at the same time with the presence or absence of this constituent part. But, in doing this, we would admit at bottom but one single principle as the basis of the system, since the existence of the one would necessarily imply that of the other. As M. Werner has admitted external characters to form the basis of his system, at the same time that he explicitly declares, that "all minerals which have essentially the same constituent parts both with respect to quality and quantity form the same species," we must suppose that he has discovered certain connexions which exist between these characters and the essential chemical composition of the same mineral. The results of chemical analysis, nevertheless, do not correspond with this supposition; and the science which unfolds the composition of minerals pronounces it in a manner that does not agree with our received ideas of the external characters. At the first glance over the classification of M. Werner, we may perceive the difficulty in which this contradiction involves us; for the desire of reconciling two things dissimilar in themselves, has introduced an uncertainty which prevails over all its parts. If we wish that this celebrated author should remain faithful to his principles, I see no other mode than to suppose that he takes the testimony of external characters as the index of the chemical composition, rather than the results of chemistry itself.

Other authors, who have published works according to the principles of M. Werner, tell us, that although this philosopher considers all minerals which correspond in external character and chemical composition, as belonging to the same species, he does not pretend that his arrangement should agree with the experiments of the chemist. This is to speak at hazard, and to avow frankly that he regards theoretical assertions as superior to experience, and the system which he has adopted as preferable to the principles of science. It would therefore only be when the chemical results agree with the external resemblances of minerals, that they could occupy a place in this system. We see sensible characters combined with chemical composition to determine a species; but if they do not agree with the results of chemistry, this science can be of no utility.

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Such ideas will not be very generally received among those who have studied this science, nor even by those who are most disposed to discover its imperfections.

INCONSISTENCY AND UNCERTAINTY OF THE WERNERIAN PLAN OF SPECIFICATION.

We now perceive the difficulty of reconciling the immense number of principles which this system has founded, and the contradictions which the minerals themselves must render unavoidable in whoever adopts them as a basis. Let us examine, in a few examples, if their celebrated author has been able to draw any uniform laws from them. Five things are to be known, viz.

1st. If all the minerals which have essentially the same chemical composition are ranged in the same species.

2dly. If all those which have an essential difference in their chemical composition are placed in different species.

3dly. If all the minerals which differ in more than three specific characters, whatever may be the number of those which they have in common, belong to different species.

4thly. If all those which do not differ in more than three different characters, are ranged in the same species. And,

5thly. If the minerals are always divided into genera, species, and varieties, according to their difference; that is to say, if those placed in separate genera always differ more from each other, than those which belong to species, or to different varieties, &c.

The relative condition in the 1st Art. is violated in the most striking manner by Werner's zirconian genus, which is divided into three species, and to which chemical analysis gives the same results. In corundum and adamantine spar we have two species * with the same chemical composition: it is the same in appatite, asparagus-stone and phospholite. Gypsum and *fraueneis* (selenite) are in a similar state; and carbonated lime presents us with no less than the alarming number of 14 species, which contain eight subspecies and six varieties.

In the 2d Art. we have beryl which contains glucine earth, and schorlous beryl which contains none, but which has instead of it fluoric acid. These minerals, without any affinity

* Here the Wernerians make a distinction without a difference: corundum is used as synonymous with the adamantine spar of Kirwan, and imperfect corundum of Greville and Bournon; while diamond spar is made a distinct species, although forming only the subspecies *corindon harmophane* of Haüy, or adamantine corundum of Brogniart.—TRANS.

affinity in their chemical compositions, belong to the same species. (June 1805*.)

I shall not stop to give examples proving that the 3d rule has been abused: it has been violated at almost every step. In the greater part of the minerals which are but varieties of the same species, if we examine them closely, we shall find more than three specific characters which are dissimilar. The division of characters into generic, specific, and characters of variety, and the little precision which exists in all that has been said on the number and importance of characters, render this examination irksome.

As to the 4th Art. I asked the celebrated author of the system of external characters, if there existed a sufficient disparity between the properties of sulphated barytes and sulphated strontian to constitute them two species; and he answered No. Here chemistry makes two genera where the external characters would not have two species.

For the 5th Art. there is not less difference between the garnet and pyrope, quartz and *eisenkiesel* [iron flint, Jameson], beryl and emerald, than between the common or compact feldspar and *hohlspath* (*macle* of the French), potter's clay and *schieferthon* [slate clay of Jameson, and *argile feuilletée* of Brogniart], mountain cork [*asbeste tressé* of Haüy, or *A. suberiforme* of Brogniart], and amianth, calcareous spar, pisolite [peastone of Jameson, and *chaux carbonatée concretionée* of Haüy] and compact, common and fibrous limestone.

I have chosen only a few examples; but they are sufficient to prove that there is not one of the rules proposed to serve as a basis to the system which has not been infringed; sometimes one prevails, sometimes another; and we can only refer the consequent instability to the insufficiency of the principles.

PHILOSOPHERS AND PHILOSOPHY OF FREYBERG.

During a residence of 18 months at Freyberg, where I had every day occasion to admire the precision and accuracy with which the learned professor recognized minerals at the first view, and where I was more than ever convinced by the example of others of the difference which exists between the institution of species and the knowledge of individuals,

* Professor Jameson has even gone further, and divided, after Werner, beryl into two subspecies, calling the one "*first* subspecies, precious beryl" (beryl of Kirwan), and the other "*second* subspecies, schorlous beryl," the pycnite of Haüy, and shorlite of Kirwan; thus indicating a relation in numerical order which has no existence in nature.—TRANS.

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I neglected no means of forming to myself a distinct idea of the former. Sometimes they spoke to me of the chemical composition; but when I cited the zircon and hyacinth, I was answered, that the external characters made the difference. If heavy spar and celestine were the subject, they again referred to chemistry. Often they spoke to me of approximate characters (*caractères des rapprochemens*), or characters of agreement and disagreement, of which no mention has been made in the enumeration; and they quoted to me, as a reason for placing potter's clay (*glaise*) and schistose argil in the same species, that both are disunited in water. To justify the separation of chalk from mineral agaric [rock milk, Jameson; and spongy carbonated lime, Haüy], of foaming earth [or schaum earth of Jameson, silvery chalk of Kirwan, talcous pearly carbonated lime of Haüy] from *schiefferspath* [slate spar of Jameson, argentine of Kirwan], they relied on the external characters; and to prove that *bitterspath* [muricalcite, Kirwan; *chaux carbonatée magnesifere*, Haüy; *chaux carbonatée lente picrite*, Brogniart] justly makes a species different from calcareous spar, they turned about to chemistry without daring openly to claim its support. Sometimes the colours were but shades or accidents; sometimes they offered characters of the highest importance. At other moments they confessed to me that *they made species by instinct*: and when I complained of not being satisfied with some conclusions indicated by this guide, they answered, "*One is not always in his instinct.*" Finally, after being detected in every manner, they referred the specification to the *tact* of the observer *. But, in this respect, who should venture to make species if not M. Werner alone?

If I have spoken of these details which I often collected

* True philosophers are deeply indebted to Mr. C. for this clear and manly exposition of a system not of science but of delusion worthy only of the lowest religious jugglers and fanatics. The "mineralogical instinct" is certainly a new faculty discovered in the human mind by the philosophers of Freyberg, whose ardent zeal in propagating their opinions furnishes a better proof of their passions than of their logic or reasoning powers. It may, perhaps, be laid down as a general truism, applicable in every branch of natural philosophy, that all schemes or systems of natural knowledge may be esteemed scientific or dogmatic just in proportion as their followers embrace them by reason or by passion. Science is properly a creature of reason, and modestly retires whenever the passions or affections appear: opinions, being originally suggested by the feelings, are naturally supported and propagated by the passions, while science can only be maintained and disseminated by close abstract reasoning. Hence it is not difficult to conceive why some of the more imprudent Wernerians have expressed themselves with so much violence against the volume containing the above statement of facts and reflections.—Trans.

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in conversation, and if I have quoted the words of other persons as well as M. Werner, it is to prove that in the *system of external characters there are no principles of specification which could serve as the basis of any science*; for, if there had been any, it is more than probable that some one would have been able to show me them; and, until that I receive a clear and distinct answer on this head, I shall be pardoned for believing and saying that there are none.

BASIS OF A SCIENTIFIC SYSTEM OF MINERALOGICAL SPECIFICATION.

If in the multitude of properties which distinguish bodies we are fortunate enough to find those which lead to a more certain and exact determination, let us hope that when properly unfolded they may be converted into principles, and that a science shall spring up from the whole. The precision of the terms which it shall employ will be the measure of its accuracy, and the definitions become its language.

The knowledge which we have hitherto acquired, furnishes us with two means of appreciating in bodies those qualities which escape the cognizance of our senses. These means are physical and chemical; they unite, to the advantage of being able to appreciate with more precision the properties which on the first view are but imperfectly discovered, that still greater, of developing the new properties which are only manifested by indirect means. Having seen the little success attending the system of immediately sensible properties, and the little hope which remains of improving it, since M. Werner has not been able to make it better, let us have recourse to the succour of these two sciences to establish mineralogical species.

Physics and chemistry furnish us two modes of attaining the final results of the division of bodies. Without entering into useless metaphysical discussions on infinity, we may suppose any substance whatever reduced to the finest and most imperceptible particles which the mind can imagine. This is the last point of physical division, and one of these grains presents us with the physical element of a body. Yet this element may be still very compound in another point of view, and undergo another species of division by means which are properly the province of chemistry. When the latter is also carried to its ultimate point, we obtain the chemical element. By physical element we understand that which occupies the smallest portion of space which we can conceive; chemical element supposes the least possible number of component principles.

The former eludes our senses by its extreme tenuity long before it has attained its limit; the latter would not be less correctly represented by a mountain of pure silica than by the smallest atom. The function of the one in nature is to aggregate itself in quantities more or less considerable to form masses, from those particles which we can perceive only by the aid of the mircroscope, to those enormous piles which we can scarcely embrace in imagination; the office of the other is to form bodies which we call compound: thus the simplicity of one of these elements does not affect the other. They have nothing in common, but as being the results to which we are led by the only two means of division hitherto known. We may affirm that in every case we can obtain these results, or that we cannot be obliged to take the limits of our knowledge for those of nature. This is sufficient; and we are not in opposition to philosophy, when, in making some efforts to advance towards the end, we substitute the one for the other; and when we find a representation, which in every thing essential resembles the object of research, we may dispense with a rigour which would in some respects be superfluous.

Hence, from the combination of these elements under different circumstances, results that infinite variety of nature which we call fantastical when we do not comprehend it; and it is by depriving the products of nature of the accidents which alter them, that we bring them back to that simplicity in which alone they are constant. What, then, remains for genius to do, but to investigate nature in a manner in which it cannot escape our researches, and to obtain unequivocal proofs, or else consider it in a state in which it ceases to be changeable?

MECHANICO-CHEMICAL OR CRYSTALLOGRAPHICAL SYSTEM OF HAUY, AND HIS DEFINITION OF MINERALOGICAL SPECIES.

Now, what has the author of a mineralogical system founded on internal properties effected in our times? Instead of stopping at the surface, he has penetrated into the interior of the mineral, and a new world has presented itself to his contemplation. He has seen it in its simplicity, considered the elements which compose it, examined their habits and mutual relations, discovered the chain which in an invariable manner unites the final results of the only two means of division of which we know the possibility, and has defined the species. "The mineralogical species," says M. Haüy, "is a collection of minerals whose integral molecules

molecules are similar, and composed of the same elements united in the same proportion." It is the assemblage of all the minerals which agree, with respect to the final results of division, in their physical and chemical molecules, in the true expression of nature reduced to its greatest simplicity*.

This definition of the mineralogical species is rigorous, and leaves nothing to be desired; but it requires a knowledge of the integral molecules. In the first place, it requires us to ascertain what its form is in all cases similar to itself: in the second place, we must be able to determine the nature and relations of its chemical elements. The first problem consists in finding the planes which terminate the small solid called the integral molecule, or, what amounts to the same thing, a solid which may resemble it; for it is not the absolute but the relative dimensions of this molecule which are required. But, the planes which terminate this solid can be but those which are parallel to the different directions in which a mineral is divisible without breaking, or what has been denominated the direction of the cleavage.

It is otherwise with the problem respecting the chemical element. We know that there exist vacuums between the molecules of bodies, and that even these vacuities are very considerable: hence it is that foreign matters have so often interposed themselves, and altered the sensible characters of a group of molecules or of a mass. Suppose that all the directions of the cleavage parallel to the planes which terminate the physical molecule are ascertained. Whatever may be the dimensions of the piece in which these directions are found, we have the representative of the molecule; but as these dimensions necessarily exceed those of the molecule itself, it follows that the piece contains more than one molecule, and hence foreign matters may deposit themselves in the interstices. Hitherto chemistry possesses only the means of distinguishing the simple parts which compose the physical elements from those which are interposed. Hence a source of uncertainty in the results of chemical analysis; and, in order that it may enjoy all the

* Most assuredly this conception, even were it devoid of basis, would do honour to the human intellect. Man is placed in the middle of the universe, as if to contemplate the infinite space which surrounds him. On whatever side he looks,—whether he contemplates those worlds whose volumes and remote distances are to him without measure, or whether he considers the atoms which form them and the laws by which these atoms are united,—every thing is to him infinite, and begets in his mind that sentiment of sublimity originating in a grandeur for which we have no expression.

confidence

confidence which the state of our knowledge should insure it, we should apply it only to the physical molecules which have been previously separated one by one to carry off the foreign matter interposed. But, as the true physical molecule is situated beyond our means, and the thought only can reach it, a knowledge of the chemical element would seem to be too remote for us ever to aspire to it. Nevertheless nature and labour offer us some means. First, it does not always happen that the physical molecules are embarrassed by foreign matter: next, suppose several minerals whose physical division gives, for instance, an irregular tetrahedron, but in all of uniform dimensions, and that chemistry finds in one the elements *a, b, e, d, e;* in another, *a, b, c, d;* in short, that *d, e,* and others if we please, may be variable, but that *a, b, c,* may be sensibly invariable in all the different pieces: now the species is unchangeable; therefore we have a right to conclude that *a, b, c,* are the chemical elements of the species, and that the others are accidental. It is thus that chemistry itself furnishes a method of correction which has been found sufficiently rigorous, and the two molecules are still in our power.

Taking the point in its most general sense; every time that we can discover in any mineral whatever the relative connexions of the simple component substances which have been observed to be invariable, as well as the relative dimensions of the solid which is produced by division in all the divers directions of the cleavage, we have every thing necessary to define a species. All minerals, however, do not present these data; and this principle of specification, however precise it may be, does not embrace the whole of the mineral kingdom.

Let us suppose a thousand individuals or mineral molecules of a single species suspended in the same solvent. By a diminution of the dissolving power, these individuals would tend to unite themselves in groups; it might then happen either that the molecules should assume such an arrangement as the aggregate would easily yield to mechanical division, whence we might extract the integral molecule of the species or its representative; or that the molecules might unite confusedly in an irregular mass, so that the type of the species could not be recognized. Again; if we suppose the molecules of several species in the same solvent, we shall have two analogous cases; the molecules of each species might unite to form aggregates of sensible dimensions, and afterwards concur in the formation of the mass, in which each species would be perfectly discernible;

ble; or, they might be so blended together in their origin, that in the mass which would be produced, it would be impossible to discover one simple species whose molecules had contributed to form it.

TRUE PRINCIPLES OF FORMING MINERALOGICAL SPECIES.

In the mineral kingdom, therefore, we must admit the following four conditions, arising from circumstances which have presided at the formation of minerals:

1st. Simple minerals whose molecule we are able to discover:

2d. Simple minerals whose molecule eludes our researches:

3d. Compound minerals in which the simple component minerals are discernible:

4th. Compound minerals in which we cannot distinguish the simple components.

Of these four conditions there is but the first which gives the species with strictness, and which truly appertains to science; but the others belong to nature, and must not be excluded from the method of classification. If, then, we find in any mineral, characters sufficiently marked to establish a well-founded opinion that it is of the same species with some one of the first section, we refer it by analogy to this, and consider it as belonging to the same species.

What is carbonated lime?—It is a mineral composed of 0·55 lime, and 0·45 carbonic acid, and which has for its molecule an obtuse rhomboid, whose great angle is 101° 32′ 13″. Here is carbonated lime defined; and it is evident that in our principles a mineral which has these properties necessarily belongs to this species. What is compact carbonated lime?—It is a mineral whose chemical composition makes us presume with the utmost likelihood that it is of the same species as crystallized carbonated lime, and that it differs only in the circumstances under which it has been formed, not having permitted the symmetrical arrangement of its molecules, so that one might extract from the mass the solid, which it represents. Here the type is but presumed; and it is only after strong proof from analogy that we resolve to class in the species of carbonated lime, a substance which cannot be proved strictly to belong to it.

In granite, gneiss, and porphyry we distinguish the pieces of simple minerals of which they are composed. They appear to have enjoyed in their formation all the circumstances which could favour the union of the molecules of the same species

species together, to form masses of a perceptible magnitude. If the result appears to be invariable, at least in what relates to the general mode of aggregation, should it be every where the same, we shall be obliged to admit that nature here also works by immutable laws, and that we must find means to comprehend them in the system of science. These masses will therefore be mixed species, and appertain to geognosis; they will be strictly geognostic species, seeing that the simple minerals of which they are composed are so, that in their union they have observed invariable laws, and that it is no more difficult to pronounce on three species united, provided that the specific character be there distinctly visible, than on three species when they are separated.

The minerals in the fourth condition constitute the greatest difficulty in mineralogy. This science here finds its cryptogamia.

The analogy which led us to assign a place to the confused mixtures of molecules of one species, abandons us as soon as we wish to apply it to pieces which are composed of imperceptible molecules of divers species. In the supposition that we cannot discern those molecules of each species, it is impossible to refer the piece with propriety to any one. But if we observe in these masses the same constancy of character as in the mixtures of perceptible species, although we can demonstrate nothing in their constitution, we must assign them a place, and the appreciation of nature here be abandoned in some measure to the conscience of the observer. The first difficulty is to know what is the number of different species, the molecules of which have contributed to the formation of the mass. Suppose an aggregate in which we cannot discover any form of molecule, which at the same time effervesces with acids, and emits fire with steel, of which one part dissolves in muriatic acid, leaving carbonic acid gas to escape, while the other is entirely insoluble; that the dissolved part be lime, the other silica; To what species shall it be referred? Is it even possible to refer it to any? There is carbonated lime and silica, and our operations inform us that there has been a mixture of the two species. But this advantage, however weak it may be, no longer exists, if all the molecules which are found in the same mass act in the same manner with the same chemical and physical instruments; and we have no more resources to learn if it is composed of molecules of one species, of two, or of several. I shall cite the agate, jasper, hornstone, and the long list of species which are

are found in the argillaceous or clay genus of Werner, of which we are ignorant, and perhaps for ever shall be ignorant, to how many simple species they owe their origin. What do we know of the family of argillaceous schist, of serpentine, of *pierre ollaire* [potstone, Jameson; *talc ollaire*, Haüy, and *serpentine ollaire*, Brogniart], of pipe-clay, and of fuller's earth; except that we do not conceive why they have been made strictly species? Whenever a mineral gives no true representative of the species, and that we do not there find other physical or chemical properties to refer it with sufficient certainty to any whatever, in which it may be strictly admissible, it is better to make it a species of convention, in order to complete the outline which nature has traced.

DISTINCTION BETWEEN MINERALOGICAL SPECIES.

In this point of view, the species of the mineral kingdom should be divided into four sections, corresponding with the four conditions of which I have already spoken. The first should contain the species strictly so called (*espèces de rigüeur*); the second, those by analogy; the third, those which I call geognostic; and the fourth, those of convention. All belong to nature; the first only appertains to science, if we wish to preserve to this word that idea of rigour which it necessarily carries with it.

The principle of M. Haüy embraces all those which are known in the first section, therefore this system embraces all the mineralogy which is capable of becoming a science.

The method of M. Werner extends to all the mineral kingdom. "Who embraces too much, badly binds," it is said: thus we know not what is a species, because all are species. We have a measure without unity.

I pretend not that the system of mineralogy should be subjected to the division indicated by these four sections; but if we wish to consider the bodies which compose this kingdom with respect to the rank which they ought strictly to occupy, we can no longer divide them otherwise; and even in classing them according to more essential principles, it would not be useless to mention this, in order that each individual may be estimated at its just value.

The advantages that mineralogy has derived from the philosophic spirit which directed the researches into the true type of the species, and the happy application of an exact method of determinating it, have been immense. All at once it is become a science; it is supported by fixed principles

principles susceptible of demonstration, and has resolved problems, enlightened futurity, and anticipated the results of analysis.

SYSTEM OF HAUY AND WERNER CONTRASTED.

By a happy anticipation of chemistry, which has been confirmed by experiment, we owe to the system of latent properties the union of beryl with emerald, granatite with staurolite, as well as the separation of chabasie from analcime [both are denominated cubic zeolite by Werner and his disciple Jameson], stylbite [foliated zeolite, Jameson] from mesotype [radiated zeolite, Jameson], and the acanticone from thallite. It has left existing the harmony by which nature has united zircon, hyacinth, and zirconite; garnet and pyrope, quartz and *eisenkiesel* (iron flint). It has not made a mineralogical species of heliotrope, composed, according to Werner, who admits it as a species, of chalcedony and green earth, of prase, which consists of quartz and *strahlstein* [actinolite, Jameson; actinote, Haüy; amphibole actinote, Brogniart]. It has not placed sapphire and corundum in two different genera; but agreeing with chemistry, and renouncing prejudices, it has not classed a fossil entirely composed of alumine in the siliceous genus merely because it is hard. It has no repugnance to the admission of the diamond among combustibles. In the argillaceous genus, where the subdivisions are so little characterized, it has not made 32 species no more than 14 in carbonated lime, nor two in sulphated lime, or four in sulphated barytes; and above all, we have not 103 species in the earthy fossils. It has not transposed a mineral this year to the side of a species from which it was separated in the preceding, and which some months after will be chased from the side of its new neighbour to pursue its fortune elsewhere. It has not made different species, the one after the other, traverse the whole list of minerals, without being able to find where to fix themselves, like those importunate guests, who go every where and whom all persons evade. Its principles are fixed; and although it occupies more time to pronounce, it virtually decides sooner, as it disposes more surely and leaves nothing arbitrary. It neither makes distinctions without differences, approximations [*rapprochemens*] without analogies, nor species without characters.

Notwithstanding, it will not pretend that minerals should persevere in retaining a rank which principles refuse them; nor will it deem the circumstance a misfortune, that the science

science varies in gaining new means of improving itself. Since the publication of his work, M. Haüy has already made considerable changes in his system, but all of them were foreseen and previously indicated, with the single exception of sphene, which he then knew only by some indistinct crystals. When one has laid down certain principles, the path of science is then found circumscribed, but its march is direct: if it change, it is only for a rational melioration, and it proceeds in advancing. If delivered to the current of opinions, or of hypotheses, it is discussed in every sense, and fortunate if it does not retrograde. For it there is no more surety, each one buffeting it at his pleasure. To reform is a great art, and to retouch without defacing requires great ability. Principles produce improvements, arbitrariness induces revolutions.

M. Haüy had formerly determined the molecule of *spargelstein* [asparagus-stone, the *chaux phosphatée chrysolithe* of Haüy and Brogniart] as well as that of appatite; but at an interval of several years, as the idea of comparing his results did not occur till M. Vauquelin had discovered that *spargelstein* is a phosphate of lime. Here chemistry was found to agree with crystallography.

A mineral was discovered, which some thought to be of the calcareous genus, which dissolved in acids without either effervescing or emitting fluoric acid, but which nevertheless gave traces of a combination with an acid; and M. Werner pronounced it a phosphate of lime. He was not deceived; but the difference which there is between reasoning and divining, is, to set out on a principle, or to start at hazard. Yet, even in setting out on a principle, we are not always sure of reasoning. Phosphorus in burning is phosphorescent and odorous; quartz when rubbed has the same properties; therefore silica is composed of phosphoric acid and lime. It is thus that a very celebrated German professor spoke, and he pretended to reason: his pretensions were so much the greater, that the combination of phoshoric acid and lime is neither phosphorescent nor odorous.

[To be continued.]

LVII. *On the Decomposition of Water by Charcoal.* By M. TORDEUX, *Student of Chemistry in the Polytechnic School* *.

In the note at the end of the observations of M. Figuier, on the sulphurets contained in the soda of commerce, in a preceding

* *Annales de Chimie*, tome lxvi. p. 318.

preceding Number of the Annales, M. Figuier adduces an example of the explosions which sometimes take place in soap-works, which he ascribes to the hydrogen gas mixed with atmospheric air, existing in the interior of the vat, above the caustic lixivium; and he explains the formation of this gas, by supposing that the sulphurets which crude soda contains, set free a quantity of hydrogen exceeding that which is necessary for the formation of the hydrogenated sulphuret, when we treat this kind of soda with water.

We know that when an alkaline sulphuret is put into water, the latter is partly decomposed. A sulphate is formed, and the hydrogen set at liberty is combined with the remains of the sulphur and the base, in order to form a hydrogenated sulphuret. We know also that in this experiment there is no extrication of gas if we operate at a low temperature.

Hence it is evident that the hydrogen gas which swims over the soap-maker's ley, does not proceed from the decomposition of the water by the alkaline sulphuret.

I have been led to ascribe the production of this gas to the charcoal always met with in the soda of commerce, by a remark which I made several months ago. I had observed that potash purified by lime, which had been long in contact with vegetable substances, and which was strongly coloured by the charred substances which it had taken up from them, when fused in a crucible, gave out a great deal of gas which took fire spontaneously; and when the alkali was red hot, its combustion resembled that of hydrogen gas.

It appeared to me on reading the memoir of M. Figuier, that the hydrogen of which he speaks might have been produced by a nearly similar cause. I made some experiments on this subject, and the object of this note is to detail the results.

The potash, on which I made the first observation, besides charred substances, also contained a quantity of water, the more considerable, as it had not been reddened in the desiccation; and all circumstances being favourable, it appeared to me that the carbonic acid might have been formed in this case by the resulting attraction of charcoal for oxygen, and of potash for this acid; and that the hydrogen gas must have been extricated pure or carburetted.

In order to ascertain if this was really the case, I distilled in a stone retort, potash similar to that which I had used in the crucible: in an instant the heat was sufficient to drive

off

off water from the potash, and a gas began to be set free, which issued incessantly during parts of the operation. This gas was insoluble in water; it had a feeble empyreumatic smell; it did not disturb lime water, and it was inflammable, burning like a mixture of hydrogen gas and carburetted hydrogen gas: it made lime water turbid after its combustion; when mixed with oxygen in Volta's eudiometer, it detonated by the electric spark.

The disengagement was kept up a long time at a weak heat; nevertheless I increased the fire until the bottom of the retort was red hot: I always obtained the same product, only the hydrogen became purer.

After some time the disengagement of gas slackened. I increased the fire: and when the retort was very red it began again; but the gas which I obtained this time was entirely absorbed by the water, and by the lime water which it rendered turbid. It was no longer inflammable, and proved to be pure carbonic acid. At the end of the operation, however, it left a combustible residue, when it was shaken with lime water: this residue was probably gazeous oxide of carbon. The potash had become almost white, and the retort was attacked.

It appears to me that we may explain this operation in the following manner: The water in presence of the charcoal and of the potash, acts in the same way as when it is in contact with an alkaline sulphuret or phosphuret. Carbonic acid and a carbonate are formed; since the potash purified by lime may contain at this temperature a greater quantity of carbonic acid than that which it contains already; and if when the retort is red hot it is extricated from this acid, this perhaps is merely owing to the combination of the potash with the earths of the retort, a combination which does not admit of the presence of carbonic acid. Lastly, the gaseous oxide of carbon certainly proceeds from the decomposition of a little acid by a residuum of charcoal.

I confirmed this experiment on potash very much charred and carbonated, obtained in the following manner:—I evaporated to dryness alcohol containing a great quantity of potash in solution; not effervescing with the acids, but very high coloured, although transparent. The evaporation was effected in a silver vessel in order to obtain pure potash: in proportion as the operation advanced, the potash became very black; and towards the end it swelled up, giving out an inflammable gas: finally, it became dry and spongy. It was treated with water, and evaporated to dryness without filtering; it was black like charcoal, and ef-

 fervesced

fervesced a little. It was in this state that I submitted it to distillation in a stone retort, as I had done with respect to the lime potash. The results of the operation were absolutely similar; and when I removed the potash from the retort, it was white, and exhibited some effervescence.

I should in all probability have obtained the same results with soda purified by lime, if I had subjected it to the same experiments, considering the great resemblance which exists between these two alkalis.

In order to assimilate my experiments a little with those which are performed on a great scale in the soap-works, it remained to form the caustic lixivium of the soap-makers, and to observe what took place in this operation. With this view I made a paste with 500 grammes of pulverized alicant soda, and 250 grammes of lime newly slacked. I diluted it in water, and left it ten or twelve days at a temperature from 10° to 15° of Reaumur in a proper apparatus. Some bubbles of azotic gas only were set free. Although the result of this last experiment teaches us nothing satisfactory, I am not less inclined to think that the hydrogen gas, whether pure or carburetted, which is produced in soap-works, is owing, as I have observed above, to the decomposition of water by charcoal. In fact, it is not to be doubted that the circumstances of this experiment are extremely different from those which we meet with in the manufactories where large masses are operated upon, or where the soda employed is better adapted for the operation, either from containing more charcoal, or from being in more minute division. In short, there is a variety of circumstances which must necessarily modify the results.

LVIII. *Cases illustrating the Effects of Oil of Turpentine in expelling the Tape-worm.*

Case I.

By John Coakley Lettsom, *M. D. and President of the Medical Society.*

Early in September 1809, I was consulted by J. P. esq., about thirty-five years of age, on account of an uneasiness in the abdomen, with dyspepsia, which were supposed to originate from tænia, or tape-worm, as small portions of it had occasionally been evacuated by the rectum.

I prescribed a course of the male fern, with occasional

* *From Transactions of the Medical Society of London*, vol. i. part I.

cathartics,

cathartics, as recommended by Madame Nouflet. In this plan he persevered for the space of three months, in which period he discharged, at two different times, about eight yards of the tænia. In April 1810, he again applied to me, in consequence of labouring under his former complaints; adding, that he imagined, from the long use of the plant I had recommended, his pains, and particularly the dyspepsia and general debility, had increased.

At this time I ordered the oleum terebinthinæ rectificatum, in a dose of nine drachms by weight, and after it a little honey to remove the heat and unpleasant taste it might occasion. In a week after taking the oil, he called upon me agreeably to my request, and gave me the following information: That, as far as he could judge, in swallowing this medicine, it occasioned less heat than would have resulted from taking the same quantity of brandy, or other spirit; and that the taste and heat it produced were soon removed by the honey.

In about three hours after taking this dose, a laxative motion was produced, without any discharge of tænia: but soon afterwards, with the second stool, more than four yards of the worm were discharged, and also a quantity of matter, resembling, as the patient expressed it, the substance and skins of the tænia. On the surface of this evacuation he noticed the oil floating, together with the tænia and the substance mentioned. It produced little or no pain, and certainly much less than the purgative he had taken after the use of the male fern. The subsequent motions contained no tænia, nor any of the substance before noticed. He experienced no pain or heat in the urinary passages, though the urine continued to impart a terebinthinate smell for three or four successive days.

My patient has since remained in perfect health, enjoying a degree of comfort to which he had been a stranger for the preceding half year.

From this and other instances, I am induced to conclude, that the best method of taking the oil is without any admixture; and that the dose of nine drachms occasions very little inconvenience: and further, that this quantity, perhaps owing to its quick purgative effect, excites no irritation in the urinary passages, although it imparts its peculiar smell to the urine.

I do not recollect that it has been heretofore observed, that the oil has been evacuated in its original state. It might hence be inferred, that it is most efficacious when

 exhibited

exhibited uncombined, in which state it is not attended with any particular inconvenience.

It is well known that tænia may exist in a healthy state of the system; and that hence its presence cannot be accurately ascertained by any other circumstance than the actual discharge of portions of the worm itself. Sometimes, indeed, there is felt a heavy pain in the epigastrium, attended with dyspepsia and emaciation; but these are not pathognomonic symptoms, as they may arise from other causes.

[To be continued.]

LIX. *Intelligence and Miscellaneous Articles.*

NATIONAL VACCINE ESTABLISHMENT.

The Board appointed by His Majesty's Government to regulate the affairs of this establishment has ordered that the following description of the vaccine vesicle, and instructions relative to vaccination, which have been presented by the director, be strictly observed by the vaccinating surgeons.

Description of the regular Vaccine Vesicle.

When vaccination succeeds, a small red spot is observable on the third day, the day the operation is performed being reckoned the first. If the spot is touched, an elevation is felt; and if examined with a magnifying glass, the little tumour appears surrounded by a very slight efflorescence.

The spot gradually enlarges; and between the third and sixth day a circular vesicle appears. The edge of the vaccine vesicle is elevated, the centre depressed. The colour is at first of a light pink, sometimes of a blueish tint; and changes by degrees to a pearl colour. The centre is somewhat darker than the other parts.

The vesicle is hard to the touch.

In its internal structure it is cellular; the cells being filled with transparent lymph.

The vesicle commonly augments till the tenth or eleventh day.

In the early stages, there is usually round the base an inflamed ring; or this takes place on the seventh or eighth day; towards the ninth it spreads rapidly, and near the tenth forms an areola of about an inch and a half in diameter.

This areola is of the usual colour of inflamed skin; it is hard, and accompanied with some degree of tumefaction.

It

It continues out for a day or two, and then begins to fade; sometimes forming two or three concentric circles.

After the areola is formed, the vesicle begins to decline; the centre first turns brown, and the whole gradually changes into a hard smooth scab, of a very dark mahogany colour. This dry crust usually drops off about the end of the third week, leaving a permanent cicatrix.

Varieties in the Progress and Appearance of the Vaccine Vesicle, not preventing the Success of Vaccination.

The first appearance is seldom earlier, but often later than has been described. In some rare instances the vesicle commences even a fortnight or three weeks after vaccination; but if the process is then regular, it is equally efficacious.

When the vesicle is slightly ruptured before the sixth day, if it resume its proper form, and the process continue quite regular, success is not prevented: nor is it, when the crust of a regular vesicle is rubbed off in the decline of the disease.

The irregular and imperfect Vesicle and Pustule, which are not to be depended upon.

In these deviations there is usually a premature itching, irritation, inflammation, vesication, or suppuration. Or the progress of the vesicle is too rapid, its texture soft, its edge not well defined, its centre elevated, and the contents discoloured or purulent. Or instead of a proper areola, a premature efflorescence of a dusky purple hue takes place, and the scab is of a light brown or amber colour.

The irregular vesicle or pustule is more liable to be broken than the other, both from its more pointed form and softer texture, and also from its being usually so irritable as to provoke scratching. When broken, or even without this happening, ulceration often ensues.

A vesicle, apparently regular at first, sometimes does not augment to the proper size, but dies away without completing the regular process. This usually leaves no cicatrix, or one which is almost imperceptible.

When those, or any other considerable deviation from the regular course of the disease take place, no dependence can be placed upon the operation. In such cases vaccination should be repeated.

Probable Causes of irregular Vesicles and Pustules.

These accidents may be occasioned by matter or lymph being taken from an irregular vesicle or pustule at any period, or from a regular vesicle at too late a period; or by lymph, though originally pure, which has been injured by long keeping, by heat, or otherwise. Or they may be caused by performing the operation with rusty or unclean lancets, or in a rude manner, or by injuring the vesicle at an early stage, and thereby exciting too much inflammation, or interrupting the regular progress of the disease. Herpetic eruptions, and other cutaneous affections have also been supposed the cause of these irregularities, and occasionally to prevent the vaccine lymph having any effect.

The Methods of taking Vaccine Lymph for Vaccination.

The lymph of a regular vesicle is efficacious from the time it is secreted, till the areola begins to spread. It may therefore commonly be taken till the ninth day; but not after the areola is formed.

The lymph is to be taken by small superficial punctures made in the vesicle with the point of a lancet. Time should be allowed for the liquid to exude, which will form small pellucid drops. When requisite, a very slight pressure may be cautiously applied with the flat surface of the lancet. Great delicacy is requisite in this operation; for if the vesicle is rudely treated, or too much opened, inflammation and ulceration may ensue.

Lymph intended to be used immediately, or in a few days, may be received on a lancet; but this is an improper instrument for preserving it longer; for the lymph soon rusts the lancet, and it is then liable to be inefficacious or injurious. Quills and toothpicks succeed; but small bits of ivory shaped like the tooth of a comb, and properly pointed, are the most convenient instruments; and to render them more certain, they should be charged repeatedly.

In order to preserve lymph for a long period, the best method is by two bits of square glass. The lymph is to be received on the centre of one of them, by applying it to a punctured vesicle. When fully charged and dry, it is to be covered with another bit of glass of the same size, and wrapped up in paper or in gold-beater's skin.

In whichever way the lymph is taken, it should be allowed to dry without heat in the shade, and be kept in a dry and cool place. When inclosed in a letter, if great care is not taken, it may be injured by the heat of the melted wax in sealing the packet.

The Mode of Vaccinating.

Liquid lymph is better than dry, because it seldomer fails, and the operation is more lightly and quickly performed. Therefore, in every instance where it is practicable, the patient

tient from whom the lymph is to be taken should be present, and the lymph should be transferred from the one to the other.

Vaccination is generally performed in the arm near the insertion of the deltoid muscle; but in order to hide the scar, and in adults who are likely to use the arm much, it may be adviseable to vaccinate the outside of the leg, a little above or below the knee.

The skin being stretched, a lancet charged with vaccine lymph should be held with its flat surface to the skin; and the point insinuated slantingly through the cuticle till it touches the cutis. It should be retained there for a few seconds.

The lancet should be dipped in water and wiped after each operation, even when several successive inoculations are to be performed.

Dry lymph on glass may be moistened with a very little cold or tepid water on the point of a lancet, allowing it some time to dissolve, and blending it by a little friction with the lancet. It must not be much diluted, but of a thick consistence: it is to be inserted in the same manner as the recent fluid.

When quills, ivory lancets, or toothpicks charged with dry lymph are used, the lymph should not be diluted; but a puncture having been first made with a common lancet, the point of the instrument is to be inserted, and held in the puncture half a minute or more, that the lymph may gradually dissolve and remain in the wound. If the part of the instrument which is charged be afterwards wiped repeatedly upon the edges of the puncture, it will tend still further to ensure success.

Vaccinated patients must be cautious not to wear tight sleeves, nor to injure the vesicle by pressure, friction, or any other violence; lest considerable inflammation or ulceration should ensue.

One perfect vaccine vesicle is sufficient; but for various reasons it is always prudent to make two or three punctures, especially when the danger of receiving the small-pox is imminent, the lymph dry, or the patient's residence distant. Besides, greater security is obtained against a chance of failure from the derangement or destruction of one vesicle by accidental injury, or by the taking of lymph for vaccination. When two punctures are to be made in one limb, they should be at least two inches asunder, on account of the irritation they may occasion.

One vesicle should be always permitted to go through its course without being punctured.

Lancets for vaccination should be kept clean and bright.

Constitutional Symptoms.

Constitutional symptoms sometimes occur at a very early period, but more commonly from the seventh to the eleventh day. These are drowsiness, restlessness, a chilliness succeeded by heat, thirst, head-ach, and other marks of febrile affection. Now and then sickness or vomiting takes place, especially in infants.

The constitutional symptoms are in general slight and transient.

In a great proportion of cases there is no perceptible indisposition; nevertheless, the person vaccinated is not the less secure from the future infection of the small-pox, provided the progress of the vesicle has been regular and complete.

Care should be taken not to confound the symptoms of other diseases with those produced by vaccine inoculation.

Medical Treatment.

In general no medicine is required in this mild affection; but if the symptoms happen to run a little higher than usual, the same remedies are to be applied as if they proceeded from any other cause.

No preparatory medicines are necessary before vaccinating, and commonly no cathartics need be given afterwards.

Should the local inflammation exceed the usual bounds, which rarely happens, unless from tight sleeves, pressure, or friction, it may soon be checked by the frequent application of compresses of linen dipped in water, in liquor Plumbi Acetatis dilutus, or in a solution of one drachm of Plumbi Superacetas in a pint of water. These are to be applied cold.

If the scab be rubbed off prematurely, and ulceration take place, cooling and astringent applications may be used; such as a drop of liquor Plumbi Acetatis, which should be allowed to dry on the part, and then be covered with compresses dipped in water, or in either of the preparations of lead above-mentioned, and frequently renewed.

When ulceration is deep or extensive, a poultice either of bread and milk, or of bread with any of the preparations of lead may be applied, as the case seems to require. They must never be applied till they are nearly or quite cold.

In such foul and obstinate sores as resist the foregoing applications, the Unguentum Hydrargyri Nitratis, mixed with an equal quantity of Unguentum Cetacei or other similar applications, may sometimes be resorted to with advantage. And at other times these sores may be healed by the Ceratum Plumbi Superacetatis, or the mildest applications.

The

The irregular vesicles and pustules are frequently followed by ulceration at an early period, which is to be treated in the same manner as if it proceeded from the regular vesicle.

When the patient has been previously exposed to the infection of small-pox, this disease will be either superseded or not, according to the time which has elapsed before vaccination.

Medical gentlemen in all parts of the empire may be supplied with vaccine lymph, without any expense, from the National Vaccine Establishment.

Applications for lymph, letters, and communications respecting vaccination, will meet with proper attention: they should be addressed to Dr. Hervey, register, Leicester-square; and when from a distance, put under a cover, directed to The Right Hon. the Secretary of State for the Home Department.

Board Room, 21, Leicester Square, Feb. 22, 1810.

MOUNT VESUVIUS.

Naples, Sept. 24.

The recent eruption will make the year 1810 an epoch in the annals of Vesuvius, on account of the manner in which it began, and the disasters it has produced. It is considered as a very extraordinary circumstance that this eruption was not preceded by the usual indications; every convulsion of Vesuvius being previously announced by the drying up of the wells of Naples. This phænomenon did not take place on this occasion, and, to the great surprise of the inhabitants, Vesuvius began to emit flames on the night of the 10th of September.

On the morning of the 11th the flames became more intense, and the lava began to flow from the east and south-east sides of the mountain. Towards evening the conflagration increased, and about twilight two grand streams of fire were seen to flow down the ridge of the volcano; night produced no change in this state of things.

On the morning of the 12th a hollow sound was heard, which continued increasing; the fire and smoke also augmented in intensity, and towards evening the horizon was obscured. The breeze, usual in these parts, having blown from the south-east, dissipated the accumulated clouds. The mountain continued to vomit lava and a dense smoke, which even at a distance was strongly sulphureous; the hollow noise in the sides of the mountain continued to increase.

Curious

Curious to witness as near as possible one of the most astonishing phænomena of nature, and forgetting the misfortune of Pliny, I set out from Naples, and at eight in the evening I reached Portici. From thence to the summit of the mountain the road is long and difficult. About half way there is a hermitage, which has long served for refuge and shelter to the traveller:—a good hermit has there fixed his residence, and takes care to furnish for a moderate sum, refreshments, which to the fatigued traveller are worth their weight in gold. The environs of this hermitage produce the famous wine called Lachryma Christi.—From the hermitage to the foot of the cave there is a long quarter of a league of road, tolerably good; but in order to reach from thence the crater, it is necessary to climb a mountain of cinders, where at every step you sink up to the mid-leg. It took my companions, myself, and our guides, two hours to make this ascent; and it was already midnight when we reached the crater.

The fire of the volcano served us for a torch; the noise had totally ceased for two hours; the flame had also considerably decreased:—these circumstances augmented our security, and supplied us with the necessary confidence in traversing such dangerous ground. We approached as near as the heat would permit, and we set fire to the sticks of our guides in the lava, which slowly ran through the hollows from the crater. The surface of this inflamed matter nearly resembles metal in a state of fusion; but as it flows it carries a kind of scum, which hardens as it cools, and then forms masses of scoria, which dash against each other, and roll all on fire, with noise, to the foot of the mountain. Strong fumes of sulphuric acid gas arise in abundance from these scoria, and by their caustic and penetrating qualities render respiration difficult.

We seemed to be pretty secure in this situation, and were far from thinking of retiring, when a frightful explosion, which launched into the air fragments of burning rocks to the distance of more than 100 toises, reminded us of the danger to which we were exposed. None of us hesitated a moment in embracing a retreat; and in five minutes we cleared in our descent a space of ground which we had taken two hours to climb.

We had not reached the hermitage before a noise more frightful than ever was heard, and the volcano, in all its fury, began to launch a mass equal to some thousand cartloads of stones and fragments of burning rocks, with a projectile force which it would be difficult to calculate. As the projection was vertical, almost the whole of this burning

ing mass fell back again into the mouth of the volcano, which vomited it forth anew to receive it again, with the exception of some fragments which flew off, to fall at a distance, and alarm the inquisitive spectator.

The 13th commenced with nearly the same appearances as those of the preceding day. The volcano was tranquil, and the lava ran slowly in the channels which it had formed during the night; but at four in the afternoon a frightful and continued noise, accompanied with frequent explosions, announced a new eruption: the shocks of the volcano were so violent, that at Fort de L'Œuf, built upon a rock, where I then was, at the distance of near four leagues, I felt oscillations similar to those produced by an earthquake.

About five o'clock the eruption commenced, and continued during the greater part of the night. This time the burning matter flowed down all the sides of the mountain, with a force hitherto unprecedented; all Vesuvius seemed on fire. The lava has caused the greatest losses: houses and whole estates have been overwhelmed; and at this day families in tears and reduced to despair, search in vain for the inheritance of their ancestors, buried under the destroying lava.

At ten at night the hermitage was no longer accessible; a river of fire had obstructed the road. The districts situated on the south-east quarter of the mountain had still more to suffer. Mount Vesuvius presented the appearance of one vast flame, and the seaman at a great distance might contemplate at his leisure this terrific illumination of nature.

SERPENTS IN AMERICA.

The following is translated from the Reading (Pennsylvania) Eagle.—A daughter of Mr. Daniel Strohecker, near Orwigsburg, Berks county, Pennsylvania, about three years of age, had been observed for a number of days to go to a considerable distance from the house with a piece of bread which she obtained from her mother. The circumstance attracted the attention of the mother, who desired Mr. S. to follow the child and observe what she did with it. On coming to the child he found her engaged in feeding several snakes, called yellow heads or bastard rattle-snakes. He immediately took it away, and proceeded to the house for his gun, and killed two of them at one shot, and another a few days after.—The child called these reptiles in the manner of calling chickens; and when its father observed, if it continued the practice they would bite her—the child replied—

"No,

"No, father, they won't bite; they only eat the bread I give them."

NATIVE MAGNESIA.

Although magnesia enters into the composition of many mineral substances, yet its existence in the mineral kingdom, in an uncombined state, has, till within these few years, been unknown.

At Hoboken, in New Jersey, on the estate of Mr. John Stevens, is found a mineral which, agreeably to the experiments of Professor Bruce, of New Jersey, contains in the hundred parts,

Magnesia	70
Water of crystallization	30
	100.

SHOWER OF METEORIC STONES IN NORTH AMERICA.

Raleigh, New Connecticut, March 1, 1810.

On Tuesday, the 30th of January last, at two o'clock P.M. there was a fall of meteoric stones in Caswell county. Their descent was seen for a considerable distance round, and two reports distinctly heard at Hillsborough, a distance of 30 miles.—A fragment weighing a pound and three quarters struck a tree in the new ground of a Mr. Taylor, near where some woodcutters were at work, who, apprehending the fate of Sodom and Gomorrah, ran home without once looking behind them. Encouraged, however, by a woman, whose curiosity was superior to her fears, they returned with her to the place, and brought away the stone, which was still hot. We understand that Governor Williams of the Mississippi territory, now in Rockingham, intends sending it to the Chemical Society in New York to be analysed: it is, he informs us, of a dark brown colour, porous, and probably contains iron.

COFFEE.

A foreign journal announces that a M. Bamas, a cloth-manufacturer in the department of the Seine and Marne, has succeeded in growing coffee in France. He sowed some Mocha coffee, and obtained a produce of about 15 pounds of beans possessing the proper flavour and form. Perhaps the most important circumstance attending this experiment was his neither employing a green-house nor glass frames, nor any unusual shelter, but simply preparing the soil with some care.

DE LUC'S ELECTRIC COLUMN.

We have again to notice the ringing of the small bells by means of De Luc's electric column. They were ringing on the evening of the 24th of August, and had been so doing, without being observed to have stopped, for a period of 152 days and a half. This long continuance renders it not improbable that (as was suggested in our Magazine for March) "the weight of the clapper may be so adapted to the power of the apparatus, as to cause small bells to continue ringing for several years without intermission."

If any of our mechanical readers can suggest to us an easy method, by which a pendulum vibrating can give motion to wheel-work, they are requested to communicate such contrivance to us. It is much wished that an instrument may be made, which by the motion of an index hand and dial-plate may show the number of vibrations in a given time, as it would be very interesting to observe what alterations may take place in different states of the atmosphere.

LECTURES.

Middlesex Hospital.

Medical Lectures, 1810–11, by Richard Patrick Satterley, M.D. Fellow of the Royal College of Physicians, Physician to this Hospital, and to the Foundling Hospital; and Thomas Young, M.D. F.R.S. Fellow of the Royal College of Physicians.

Dr. Satterley's Course of Clinical Instruction will begin the first week in November: the attendance on the Patients will be continued daily, and Lectures will be given once a week, or oftener when it may be necessary, at Eleven o'Clock.—Mr. Cartwright, Surgeon to the Hospital, will undertake such occasional demonstrations of morbid anatomy as may be required for the illustration of the respective cases. The objects of the Course will also be extended to such remarkable peculiarities in the diseases of Children, as may occur in the Foundling Hospital. Terms of admission, to Pupils of the Hospital, Five Guineas.

Dr. Young will begin, in February, a Course of Lectures on Physiology, and on the most important parts of the Practice of Physic; in particular the Nature and Treatment of Febrile Diseases: he will deliver them on Tuesdays and Fridays, at Seven o'Clock in the Evening. Admission, Two Guineas: to former Pupils, One Guinea.

Those who are desirous of attending either of these Courses, are requested to leave their names with the Apothecary at the Hospital, from whom further particulars may be known.

Electrical and Electro-Chemical Science.

Mr. George Singer will commence a Course of Lectures on Electrical Phænomena, comprehending all the new Discoveries, and illustrated by numerous original Experiments. Early in the ensuing season a Prospectus of the Plan of Instruction may be had of Mr. Cuthbertson, 54, Poland-street; or of Mr. Singer, at the Institution, 3, Princes-street, Cavendish-square.

Surry Institution.

The Annual Courses of Popular Lectures at the Surry Institution, Blackfriars Bridge, commenced on the 15th ult. and will continue every succeeding Monday and Thursday Evening, at Seven o'Clock, during the Season.—The following Gentlemen have been engaged for the following Departments, viz.

Zoology	George Shaw, M.D. F.R.S.
Music	Mr. S. Wesley.
Zoonomy	John Mason Good, Esq.
The Chemistry of the Arts	F. Accum, M.R.I.A.
Natural Philosophy and Astronomy	Mr. Hardie.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Norris, late of Manchester, cotton merchant, for his new mode of sheathing or covering the bottoms of ships or vessels with certain matter or materials, so as to be a substitute for copper.—Sept. 26.

To Samuel Hobday, of Woodcock-Street, in the parish of Aston, near Birmingham, snuffer-maker, for his lever, by the application of which alone, or with the addition of a rack, he can make snuffers to act without springs.—September 26.

To Marck Isambard Brunel, of Chelsea, gent. who, in consequence of a communication made to him by a certain foreigner residing abroad, is become possessed of an apparatus for giving motion to machinery; part of which is also applicable to hydraulic and pneumatic purposes.—Oct. 1.

To Benjamin Milne, of Bridlington, in the county of York, collector of the customs, for an improved bell- and gun-alarm.—Oct. 1.

To Joseph C. Dyer, of Boston, state of Massachusetts, one of the United States, now residing in London, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, who is become possessed of certain improvements in the construction and method

method of using plates and presses, and for combining various species of work in the same plate for the kind of printing usually called plate printing, designed for the objects of detecting counterfeits, for multiplying impressions, and saving labour.—Oct. 1.

To George Miller, of Panton-street, near the Haymarket, musical instrument-maker, for his method of making wind instruments commonly called military fifes, of substances never before used for that purpose.—Oct. 1.

To John Towill Rutt, of Goswell-street, in the county of Middlesex; John Webb, of Hoxton, in the said county; and John Tretton, of the city of London, card manufacturers, for their improved apparatus to machines for making fillet, sheet, and hard cards, such as are used for carding wool, cotton, flax, silk, and all substances capable of being carded.—Oct. 8.

To Ebenezer Parker, of Highfield, in the parish of Sheffield, in the county of York, silver-plater; and Francis Cleeley, of Sheffield aforesaid, surgeons' instrument-manufacturer, for their method or plan of making an adjusting bedstead on a double frame with a four-fold method, for the relief of sick, lame, infirm and aged persons.—Oct. 8.

To John Hazledine, of Bridgenorth, in the county of Salop, engineer, for his manifest improvements in the construction of a plough for the cultivation of land.—Oct. 8.

To George Hodson, of the city of Edinburgh, North Britain, ash-manufacturer, for his improved method of separating the alkaline salt from the acid as it exists in the following substances, viz. kelp, black ashes, soaper's salts, spent leys, sosa natrose, rock salt, common salt, brine, sea water, caput mortuum of aqua-fortis, caput mortuum of oil of vitriol, and caput mortuum of salt used by bleachers, being on a principle entirely new.—Oct. 8.

To Charles Francis, of Phœnix Wharf, Nine Elms, in the parish of Battersea, Surry, temper lime-burner; and William Waters, of Princes-street, in the parish of St. Mary, Lambeth, Surry, potter, for their improved method of joining pipes.—Oct. 8.

To Henry Stubbs, of Piccadilly, in the county of Middlesex, blind-maker, for his new grand imperial aulæum, from three to 18 or 20 feet wide, without seam, and to any length or colour, for decorating the most superb or useful room, for such as drapery, curtains, and fringes, chairs, sofas, tables, &c. or finished on one side only for ornamental hangings, borders, and every other species of decoration.—Oct. 8.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For October 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Sept. 27	53	68°	54°	29·92	53	Fair
28	55	68	56	·90	50	Fair
29	51	66	56	30·01	36	Fair
30	56	64	57	29·98	30	Fair
October 1	58	64	51	30·11	30	Fair
2	55	63	52	·24	50	Fair
3	50	64	51	·22	62	Fair
4	52	64	52	·25	41	Fair
5	51	64	52	·09	50	Fair
6	52	61	50	29·91	20	Fair
7	48	66	55	·98	18	Foggy
8	53	65	56	·94	52	Fair
9	54	61	55	·90	30	Cloudy
10	55	59	54	·82	30	Cloudy
11	52	59	48	·90	30	Fair
12	47	57	44	·85	55	Fair
13	40	57	43	30·05	51	Fair
14	46	56	46	·20	36	Cloudy
15	46	57	47	·19	32	Fair
16	42	56	55	29·84	30	Cloudy
17	57	62	56	·55	15	Showery
18	56	64	55	·45	35	Fair
19	50	59	58	·78	35	Showery
20	56	64	49	·68	30	Rain
21	50	59	56	·62	36	Showery
22	57	61	48	·35	48	Stormy
23	49	55	44	·57	42	Fair
24	46	52	41	·83	24	Fair
25	42	50	40	30·16	34	Fair
26	39	49	44	·35	42	Fair

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

ERRATUM.

Page 231, line 26 from the top, (Mr. TAUNTON's paper on Inguinal Hernia,)—for *turned* read *traced.*

LX. *Description of a Camp Telegraph, invented by* Knight Spencer, *Esq. Secretary to the Surry Institution.*

To Mr. Tilloch.

Sir, The important advantages resulting to the naval service from the introduction of the telegraph by Sir Home Popham, now universally adopted, are too well known to be here insisted upon.

That telegraphic signals have been productive of great advantages to land armies, for more than 3000 years, is very easily proved.

That *the most important advantages have resulted* to the French arms, from the use of the telegraph, in the present age, is too well authenticated to be doubted.

That commanders of British armies have felt the absolute necessity of adopting some mode of telegraphic communication, is proved by the late campaign in Sicily, and, the present campaign in Spain.

That many attempts have been made to introduce the telegraph into our land-service universally, cannot be questioned.

To what cause, then, is it to be attributed, that to the present moment this powerful instrument remains to British armies (generally speaking) nearly a useless invention?

The only *rational* answer to this question seems to be, that, hitherto, no practicable system has been offered, and the attempts to introduce it must, probably, have failed;—either, from the intricacy of the machines, or, the difficulty of transporting them into situations where they could be used.

Whatever cause may have hitherto retarded its introduction, it will hardly for a moment be contended, that, were a telegraph produced as certain in its operations as the present fixed telegraph, and at the same time so simple and portable as to require no separate establishment, either for its transport or management, it would not be a most important acquisition in the field.

With this conviction on my mind, I have endeavoured to obviate the supposed difficulties; and the result, which I call my Camp Telegraph, I request permission to lay before the public through the medium of your respectable Magazine;—indulging the hope, that it may meet the attention of those who have sufficient influence to bring the subject fairly under the consideration of his majesty's government.

vernment. Perhaps it may not be improper to state, that my invention has already been honoured with the approbation of several general and other officers very capable of forming correct opinions on the subject;—and that I have frequently asked a question with it at the distance of *six miles*, and have received an answer within *three minutes*. Any officer of ordinary capacity will be able, after two hours' application, to direct a station; any private will perform the duty of a signal-man after half an hour's drill;—and, the apparatus not being more cumbrous than a serjeant's pike, there seems no necessity whatever for a separate establishment to manage it.

EXPLANATION.

To work the Camp Telegraph, which is *numerical*, the director of each station must be assisted by three privates or others, to be called signal-men; one of whom must be furnished with a staff 13 or 14 feet high, on which must be mounted two flexible balls, about three feet diameter, as described below:—this is called the *centre-point*. The other two signal-men must each be furnished with a staff ten feet high, mounted with one flexible ball.

The signals must be made by one or both of the signal-men taking an ordered number of paces to the right or left of the centre-point; in the rear of which the director takes his stand, during the time of making communications.

All signals must be made by order of the director of the station, who must give the word for the necessary number of paces. These are to be taken by the signal-men, in *double-quick time*, carrying their balls at the trail; and when they have arrived at the point or points ordered, the balls must be instantly elevated.

All signals must be repeated by the corresponding station; and when the director of the station making the communication, observes this is done, he gives the word "*Down*," and his signal-men must then retire in double-quick time to the rear of the centre-point, carrying their balls at the trail. The word "*Down*" must likewise be given by the director of the station *receiving a communication*, the instant he observes the signal-men at the corresponding station begin to retire.

A. (Plate VIII.) Is the signal of *communication*, and is made by placing one of the signal-men at 20 paces to the right, and the other at 20 paces to the left, of the centre-point.

B. Is

B. Is the signal of a *point or period*, and is to be made at the close of a number, as 275, by placing one signal-man three paces to the right, and the other three paces to the left, of the centre-point.

C. Is the signal of *error*, and is to be made when your correspondent has mistaken your last signal :—Suppose you had made the signal No. 2, which is 20 paces to the right, and your correspondent answers with 20 paces to the left, which is the signal No. 7. Then make the signal of error, by placing one signal-man three paces to the left, and the other 10 paces to the right of the centre-point; and when your correspondent has repeated this signal, thereby convincing you he is sensible of his error, *repeat the signal that had been mistaken*, and, if rightly answered, proceed as before.

D. Is the *repeating* signal, and is to be made if the last communication is not understood. It is made by placing one signal-man three paces, and the other 20 paces, to the left.

NUMERALS.

No. 1. Is made by placing one signal-man three paces to the *right* of the centre-point.

2. By placing one signal-man 20 paces to the right.

3. By placing one signal-man 10 paces, and one 20 paces, to the right.

4. By placing one signal-man at three, and one at five paces, to the right.

5. By placing one signal-man at 18, and one at 20, paces to the right.

6. By placing one signal-man three paces to the *left* of the centre-point.

7. By placing one signal-man 20 paces to the left.

8. By placing one signal-man 10, and one 20, paces to the left.

9. By placing one signal-man at three, and one at five, paces to the left.

0. By placing one signal-man at 18, and one at 20, paces to the left.

The flexible ball is constructed in the following manner:

Take an ash or deal staff of the required length, and the substance of a stout pike. Take twelve whalebones, four feet six inches long, and fix them at nine inches from the top of the staff, in the way the whalebones of umbrellas are fixed:—fix the lower ends of these whalebones to a strong slide (like the slide of an umbrella), the pipe of which must be 18 inches long, and project upwards. To the top of this pipe, stretchers 18 inches long must be affixed, and also to the middle of each whalebone, like the stretchers of an umbrella, to keep the ball stiff when in use. There must then be a strong umbrella-spring fixed on the staff, at three feet from the upper fastenings of the whalebones, or top of the ball, so that, when the slide is pushed up, the whalebones will form a sphere of three feet diameter.

The skeleton of the ball being thus prepared, it is to be covered with glazed linen, half black and half white, divided *vertically*. Letter G is a drawing of the skeleton of the ball, but only showing two whalebones instead of twelve. When the balls are not in use, they will be unsprung, and covered with strong cloth cases.

SIGNALS BY NIGHT.

To make these, it will require two lamps, about nine inches square and 12 inches high, to be elevated, one above the other, at the distance of three or four feet, for the centre-point: and one lamp for each signal-man, to be fixed on the top of the ball-staff.

Each lamp must have two hollow lenses, about four inches diameter, filled with different-coloured transparent fluids—(say pale green and pale red),—which will distinguish them from common lights. They must be suspended upon a pin, put through a strong iron frame, resembling the frame of a sign which is fixed upon an upright sign-post, so that when the staff is raised they will swing perpendicularly; and when they are carried at the trail, they will still be in a perpendicular position.

The reservoir for the oil must be made like those for the agitable lamps; the wicks must be flat, and about one inch broad.

E. is a front view of the lamp for night signals.

F. is a side view of the same.

A code of numerical signals, and a numerical vocabulary applicable

applicable to the land service, arranged upon the plan of Sir Home Popham's for the naval service, will be necessary.

When a tent or any other object is fixed upon as a centre-point, it is then generally unnecessary to use the double ball.

When stations are taken *below the horizon*, the white sides of the balls are to be turned to your correspondent, and it is advantageous to have the men in white or fatigue dresses.

When stations are taken *above* the horizon, the black sides are to be turned towards your correspondent, and then it is advantageous to have the men in uniform.

I am, sir,

Your obedient servant,

Surry Institution, Nov. 6, 1810. KNIGHT SPENCER.

LXI. *On the Penetration of Balls into uniform resisting Substances.* *By* W. MOORE, *Esq.*

To Mr. Tilloch.

SIR, SHOULD the following paper on the destruction of an enemy's vessel at sea by artillery be thought deserving a place in your excellent Magazine, you are at liberty to make use of it accordingly.

I am, sir,

Your most obedient servant,

Royal Military Academy, Woolwich, November 10, 1810. W. MOORE.

LEMMA I.

If two spheres of different diameters and different specific gravities impinge perpendicularly on two uniform resisting fixed obstacles, and penetrate into them; the forces which retard *the progress of the spheres will be as the absolute resisting forces or strengths of the fibres of the substances directly, and the diameters and specific gravities of the spheres inversely.*

Let R and r denote the absolute resisting forces of the two substances; F and f the retardive forces; D, d the diameters of the spheres; Q, q their quantities of matter, and N, n their respective specific quantities. Then the whole resistances to the spheres, being by mechanics proportional to the quantities of motion destroyed in a given time, will be as the absolute resisting forces of the sub-

 stances

stances and quantities of resisting surfaces jointly; or as the resisting forces of the substances and squares of the diameters of the impinging spheres: that is, $\frac{M}{m} = \frac{R}{r} \times \frac{D^2}{d^2}$.

But in general $\frac{M}{m} = \frac{F}{f} \times \frac{Q}{q}$: therefore equating these two values of the whole resisting forces, we obtain $\frac{F}{f} \times \frac{Q}{q} = \frac{R}{r} \times \frac{D^2}{d^2}$, and $\frac{F}{f} = \frac{R}{r} \times \frac{D^2}{d^2} \times \frac{q}{Q}$: and since $\frac{q}{Q} = \frac{d^3}{D^3} \times \frac{n}{N}$, it is $\frac{F}{f} = \frac{R}{r} \times \frac{D^2}{d^2} \times \frac{d^3}{D^3} \times \frac{n}{N} = \frac{R}{r} \times \frac{d}{D} \times \frac{n}{N}$: that is, the forces retarding spheres penetrating uniform resisting substances are as the absolute strengths of the fibres of the substances directly, and the diameters and specific gravities of the spheres inversely.

Lemma II.

The whole spaces or depths to which spheres impinging on different resisting substances penetrate; are as the squares of the initial velocities, the diameters and specific gravities of the spheres directly, and the absolute strengths of the resisting substances inversely; or, $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{D}{d} \times \frac{N}{n} \times \frac{r}{R}$.

For by mechanics we have $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{f}{F}$; and by the preceding lemma $\frac{f}{F} = \frac{r}{R} \times \frac{D}{d} \times \frac{N}{n}$, which substituted in the above it becomes $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{D}{d} \times \frac{N}{n} \times \frac{r}{R}$.

These being premised, I now proceed to resolve the following most important

Problem:

To find a general formula which shall express the quantity of charge for any given piece of ordnance to produce the greatest destruction possible to an enemy's ship at sea; it being supposed of oak substance of given thickness, and at a distance not affecting the initial velocity of the shot.

By Lemma 2, we have, generally, $\frac{V^2}{v^2} = \frac{S}{s} \times \frac{d}{D} \times \frac{n}{N} \times \frac{R}{r}$. Also the charges of powder vary as the squares of the

velocity

velocity and weight of ball jointly*. Hence, since it has been determined from experiment that a charge of half a pound impelled a shot weighing one pound with a velocity of 1600 feet per second; we shall, considering V the velocity of any ball impinging on the side of the vessel, have for the expression of the charge impelling it through the space $S = \frac{S d n R v^2 w}{2 D s N r \times \overline{1600}|^2}$.

Now to apply this in the present instance, it is first necessary that a case be known concerning the penetration of a given shot into oak. Such a case is presented at page 273 of Dr. Hutton's Robins's New Principles of Gunnery. It is there asserted that an 18-pounder cast-iron ball penetrated a block of well-seasoned oak, such as ships of war are generally built with, to the depth of $3\frac{1}{2}$ inches when fired with a velocity of 400 feet per second. Making, therefore, this the standard of comparison for all cases where the object is of oak substance, we shall have for the charge *generally* $\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{S n R w}{D N r}$; or, because the balls are of the same specific gravity, and the substance the same, or $R = r$, and $N = n$; it will be $\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{S w}{D} = \cdot 045 \times \frac{S w}{D}$; that is the charge varies as the space to be penetrated and weight of ball directly, and diameter of the ball inversely.

But the charge by the question being to produce the greatest effect possible in the destruction of the vessel; S in the above formula must always be put equal to the given thickness of its side *plus the radius of the ball;* since it is well ascertained that, for a shot to produce the most damage to any splintering object, such as oak; it must lose all its motion just as it quits the superior or further surface of it. Hence the charge in question is $= \cdot 045 \times \frac{(S' + \frac{1}{2}D)w}{D}$. S' being the thicknes of the side of the vessel, w the weight of the ball, and D its diameter.

We have supposed, that the resistance opposed to the ball's motion is uniform throughout the entire penetration;

* This law of variation of the charges does not exactly obtain in practice after a certain charge, on account of the definite lengths of the guns; but it is presumed the deviation from it, if known, would not materially affect our results.

which

which is not strictly true; since that resistance depends partly on the quantity of the surface resisted, which continually varies until the ball has penetrated to the depth of its radius; when it continues uniform till it arrives at the further surface of the object; where the resistance again commences its variation. These deviations from uniformity are about sufficient to set against that of the law of variation of the charges before mentioned; the velocities from them falling somewhat short of the law there prescribed after a certain charge.

EXAMPLE I.

An enemy's ship is in sight; required the charge for the 42 pounder guns to destroy her as quickly and completely as possible when the ships have approached near to each other: the side of the enemy's vessel (a seventy-four) *being* $1\frac{3}{4}$ *foot thick of oak timber.*

The diameter of a 42-pounder of cast-iron being =·557 foot; we get $\cdot 045 \times \frac{(S + \frac{1}{2}D)w}{D} = 6\cdot88306$lbs. or 6lbs. 14ozs. for the weight of the charge required.

TABLE

Containing the various charges for the 12-, 18-, 24-, 32-, 36- and 42-pounder guns, for producing the greatest effect in all cases of action: the substance or object being of oak materials, and its thickness together with the radius of the ball from 1 foot to that of 5 feet, regularly increasing by 1 in the inches.

Nature of Ordnance.	Thickness of the Side of the Vessel, plus the Radius of the Ball.			
	12 Inches.	13 Inches.	14 Inches.	15 Inches.
Pounder 12	lbs. 1·439242	lbs. 1·559178	lbs. 1·679116	lbs. 1·799052
18	1·928571	2·089285	2·249999	2·410714
24	2·336650	2·531371	2·726091	2·920813
32	2·830470	3·066343	3·302215	3·538088
36	3·061630	3·316766	3·571901	3·827038
42	3·393180	3·675949	3·958710	4·241475

Nature of Ordnance.	Thickness of the Side of the Vessel, plus the Radius of the Ball.			
	16 Inches.	17 Inches	18 Inches.	19 Inches.
Pounder	lbs.	lbs.	lbs.	lbs.
12	1·918987	2·838926	2·158863	2·278800
18	2·571428	2·732142	2·892856	3·053571
24	3·115533	3·310254	3·504975	3·699696
32	3·773960	4·009833	4·245705	4·481578
36	4·082173	4·337310	4·592445	4·847581
42	4·524240	4·806905	5·089770	5·372535

	20 Inches.	21 Inches.	22 Inches.	23 Inches.
	lbs.	lbs.	lbs	lbs.
12	2·398737	2·518674	2·638612	2·758547
18	3·214285	3·374999	3·535714	3 696428
24	3·894417	4·089137	4·283859	4·478580
32	4·717350	4·953323	5·189195	5·425068
36	5·102717	5·357853	5 612988	5·868124
42	5·655300	5·938065	6·220830	6·670262

	24 Inches.	25 Inches.	26 Inches.	27 Inches.
	lbs.	lbs.	lbs.	lbs.
12	2·878484	2·998420	3·118358	3·238292
18	3·857142	4·017856	4·178570	4·339284
24	4·673 00	4·868021	5·062741	5·257463
32	5·660940	5·896813	6 132685	6·368559
36	6·123260	6·378396	6·633531	6·888668
42	6·786360	7·069125	7·351890	7·634655

Nature of Ordnance.	Thickness of the Side of the Vessel, plus the Radius of the Ball.			
	28 Inches.	29 Inches.	30 Inches.	31 Inches.
Pounder 12	lbs 3·358228	lbs. 3·478164	lbs 3·598100	lbs. 3·718036
18	4·521340	4·682054	4·842768	5·003482
24	5·452184	5·646905	5·841626	6·036347
32	6·504432	5·840305	7·076178	7·312051
36	7·143804	7·398940	7·654076	7·909212
42	7·917420	8·200185	8·482950	8·765715

	32 Inches.	33 Inches.	34 Inches.	35 Inches.
12	lbs 3·837972	lbs. 3·957908	lbs. 4·077844	lbs. 4·197780
18	5·164196	5·324910	5·485624	5·646338
24	6·231068	6·425789	6·620510	6·815231
32	7·547924	7·783797	8·019670	8·255543
36	8·164348	8·419484	8·674620	8·929756
42	9·048480	9·331245	9·614010	9·896775

	36 Inches.	37 Inches.	38 Inches.	39 Inches.
12	lbs. 4·317716	lbs 4·437652	lbs. 4·557588	lbs. 4·677524
18	5·807052	5·967766	6·128480	6·289194
24	7·009952	7·204673	7·399394	7·594115
32	8·491416	8·727289	8·963162	9·199035
36	9·184892	9·440028	9·695164	9·950300
42	10·179540	10·462305	10·745070	11·027835

Nature of Ordnance.	Thickness of the Side of the Vessel, plus the Radius of the Ball.			
	40 Inches.	41 Inches.	42 Inches.	43 Inches.
Pounder 12	lbs. 4·797460	lbs. 4·917396	lbs. 5·037332	lbs. 5·157268
18	6·449908	6·610622	6·771336	6·932050
24	7·788836	7·983557	8·178278	8·372999
32	9·434908	9·670781	9·906654	10·142527
36	10·205436	10·460572	10·715708	10·970844
42	11·310600	11·593365	11·876130	12·158895

	44 Inches.	45 Inches.	46 Inches.	47 Inches.
12	lbs. 5·277204	lbs. 5·397140	lbs. 5·517076	lbs. 5·637012
18	7·092764	7·253478	7·414192	7·574906
24	8·567720	8·762441	8·957162	9·151883
32	10·378400	10·614273	10·850146	11·086019
36	11·225980	11·481116	11·736252	11·991338
42	12·441660	12·724425	13·007190	13·289955

	48 Inches.	49 Inches.	50 Inches.	51 Inches.
12	lbs. 5·756948	lbs. 5·876884	lbs. 5·996820	lbs. 6·116756
18	7·735620	7·896334	8·057048	8·217762
24	9·346604	9·541325	9·736046	9·930767
32	11·321892	11·557765	11·793638	12·029511
36	12·246524	12·501660	12·756796	13·011932
42	13·572720	13·855485	14·138250	14·421015

Nature of Ordnance	Thickness of the Side of the Vessel, plus the Radius of the Ball		
	52 Inches.	53 Inches.	54 Inches.
Pounder	lbs.	lbs.	lbs
12	6·236692	6·356628	6·476564
18	8·378476	8·539190	8·699904
24	10·125488	10·320209	10·514930
32	12·265384	12·501257	12 737130
36	13·267068	13·522204	13·777340
42	14·703780	14·986545	15·269310
	55 Inches.	56 Inches.	57 Inches.
	lbs.	lbs.	lbs.
12	6·596500	6·716436	6·836372
18	8·860618	9·021332	9·182046
24	10·709651	10·904372	11·099093
32	12·973003	13·208876	13·444749
36	14·032476	14·287612	14·542748
42	15·552070	15·834840	16·117605
	58 Inches.	59 Inches.	60 Inches.
	lbs.	lbs.	lbs.
12	6·956308	7·076244	7·196180
18	9·342760	9·503474	9·664188
24	11·293814	11·488535	11·683256
32	10·680622	13 916495	14·152368
36	14·797884	15·053028	15·308156
42	16·400370	16·683135	16·965900

In

In this Table the first column contains the nature of the ordnance, and the numbers in the other columns are their respective charges of gunpowder in pounds, when the thickness of the object to be destroyed is as specified at the top of the columns. If the thickness be given in inches, and parts of inches, take such parts of the difference between the charge for the given number of inches and the next greater; and add them to the charge first found for the given number of inches for the charge required.

The value of the decimal part of each will be had by multiplying it by 16, the number of ounces in a pound, and pointing off in the product from the right hand towards the left, as many places for decimals as are contained in the given decimal, and retaining the number on the left of the point for the ounces, increasing it by $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, or 1, when the first figure of the decimal is 2, 5, 7 or 8 respectively. This hint is merely given for those practitioners who may not be very conversant in decimals.

Scholium.

This question is not only of the utmost importance and practically useful in naval engagements, but in several instances also of military operations; as the bursting open gates of besieged cities with promptitude and effect, and breaking up all fortifications composed of wooden materials; especially those of a splintering nature, to which the above charges apply most correctly. In the case of a naval action where the object to be penetrated is of oak substance, the ball by having a small motion when it quits the ship's side tears and splinters it excessively, breaking away large pieces before it, which are not so easily supplied in the reparation: whereas on the other hand, if the shot had any considerable velocity when it quitted the side, the effect produced would be merely a hole, which would be stopped instantly by the mechanic employed for that purpose; and indeed in a great measure by the springiness of the wood itself; for I have seen in his majesty's dock-yard at Woolwich, captured vessels, having a number of shot-holes in them, almost entirely closed by the wood's own efforts; and that required nothing more than a small wooden peg or a piece of cork to stop them up perfectly: all the damage, therefore, the shot can do under such circumstances of swift celerity is merely killing those men who may chance to stand in the way of their motion.

If any object to be destroyed be so thick that it cannot be completely pierced by any common engine; or if it be of of a very brittle nature, such as stone or brick, then that charge is to be used which will give the greatest velocity to the shot to produce the maximum effect. But in many cases of bombardment this charge is by no means to be preferred; for though the effect produced each individual time be greater, yet in any considerable time the whole effect would be less than that from a smaller charge oftener fired, on account of the extreme heat it would give to the engine after a few discharges; and in consequence of which greater time would be required for cooling the gun and preparing it for further service.

Example II.

Required the charge for a 24-pounder shot to burst open the gates of a city with the greatest ease possible, they being of elm *one foot thick.*

Here the object to be penetrated being elm, the small letters in the general formula $\frac{Sdv^2w}{2Ds \times 1600^2}\left(=\frac{(S' + \frac{1}{2}D)dv^2w}{2Ds \times 1600^2}\right)$: must be made to denote the several numbers of some experiment made in the penetration of this substance. Taking, therefore, the experiment of Dr. Hutton contained in the 5th problem of his elegant Exercises on Forces, we have $d = \frac{1}{6}$ ft. $v = 1500$, and $S = \frac{13}{12}$ ft.; also by the question $S' = 1$ ft. $D = \cdot 46$, and $w = 24$ lbs. therefore $\frac{(S' + \frac{1}{2}D)dv^2w}{2Ds \times 1600^2}$ $= \frac{1 \cdot 23 \times \frac{1}{6} \times 1500^2 \times 24}{2 \times \cdot 46 \times \frac{13}{12} \times 1600^2} = \frac{830 \cdot 25}{191 \cdot 36} = 4 \cdot 33868$lbs. or 4lbs. $5\frac{1}{2}$ozs. nearly the weight of the charge required in this case.

Retaining the experiment of Dr. Hutton as a standard for all cases where the substance to be penetrated is of elm, we shall have by reduction $\frac{(S' + \frac{1}{2}D)dv^2w}{2Ds \times 1600^2} = \cdot 0676 \times \frac{(S' + \frac{1}{2}D)w}{D}$: the charge for any piece the diameter of whose shot is D, and weight w; S' being the thickness of the object as before.

It is not unworthy of remark, that the gates of a besieged place, or any like things, might be effectually broken open by the gun itself, charged only with powder; by placing it close to the gates with its muzzle from them; the momentum of recoil being generally sufficient to force such objects completely.

LXII. *Cases*

LXII. *Cases illustrating the Effects of Oil of Turpentine in expelling the Tape-worm**.

Case II.

By Thomas Hancock, *M.D.F.M.S., Physician to the Finsbury Dispensary.*

[Concluded from p. 308.]

I have used the ol. terebinth. in only one case of tænia. Jane Woodward, a poor woman, about 45 years of age, first applied to me at the London Electrical Dispensary, some months ago. She had been for more than seven years afflicted in a very distressing manner with this complaint, and was four times a patient in different hospitals; where, by the use of active remedies, she obtained temporary relief from pain, and frequently voided large portions of the tænia per anum. So soon as she had recovered a little strength, by indulging her appetite, after the violent operation of purgatives in these hospitals, her abdomen began to increase considerably in size, and small detached portions of tænia, about an inch or more in length, apparently endowed with life, continued to pass at times through the rectum; so that she was prevented from earning her bread, by this very distressing circumstance. She had generally recourse to purgatives on these occasions, and their operation had regularly the effect of reducing the size of the abdomen; but her disease continued. I may also observe, that, after these courses of medicine, she had less of rumbling in the intestines, and felt less pain, than when she freely indulged her appetite; for then, to use her own expression, "the worms appeared to gain strength," according to the increase of her own strength.

About two weeks after the application of electrical sparks to the abdomen, she discharged a portion of tænia, seven yards in length, without any appearance of head, which lived in cold water nearly three hours after its expulsion. Mr. Chamberlaine informs me he has known the tænia live nearly as long in water which was much above the temperature of the human body; a sufficient proof of the extraordinary tenacity of life in this animal.

Electricity was continued for some weeks longer; but as her pains also continued, and no more of the worm came away, my friend Mr. Chamberlaine kindly offered to try the

* From *Transactions of the Medical Society of London*, vol. i. part I. just published.

effect

effect of his electuary of the dolichos pruriens. The patient took this at first without, but afterwards with, the scobs stanni in large quantities, and for a considerable time: but though, as she asserted, these medicines, more than any she had ever taken, relieved her sufferings, they produced no discharge of tænia.

I now heard of the ol. tereb. having been administered in this complaint, and resolved to give it a fair trial, especially as my patient was herself very anxious to use any remedy that promised the slightest probability of success. I may premise, that her abdomen was enlarged as formerly, her stools slimy, and, in short, all her symptoms indicated that she had still large portions of tænia in her intestines. I ordered her at first small doses of this oil, viz. two drachms twice a day, mixed with treacle to disguise its taste. This produced no other effect than an increase of pain and uneasiness, and particularly on going to stool, as if it irritated the rectum. The dose was now increased to half an ounce, at longer intervals. The first dose in this quantity, which she took without treacle, produced a little sickness and confusion of ideas, and afterwards operated as a purge. She complained of no uneasiness whatever in the urinary organs. After these doses, she passed such a quantity of slimy mucus, with such relief in all her painful symptoms, that she earnestly entreated I would allow her to take a double dose. The quantity of an ounce, which she now took, always produced a great degree of giddiness, as if she was intoxicated, which came on shortly after taking it, and continued for an hour or more, until the violent cathartic effect which followed, removed it.

Although this medicine was repeated, after this manner, every two or three days for a fortnight or more, by her own particular desire, there was no appearance of tænia in her stools. I could not, however, but observe, that the mucus which was discharged so abundantly by the operation of the ol. tereb. sometimes exhibited the appearance of white films, as if the substance of the worm had been broken down. She took the very large dose of an ounce and half two or three times, after the medicine began to lose its effect, with results similar to those I have described. In short, by her own account, violent purging was the only thing that relieved her; and all kinds of strengthening remedies, as well as nourishing diet, uniformly increased her sufferings, so that she abstained from food when her appetite craved it, in order to avoid the anticipated pain.

I have since heard that she went into the London Hospital, and

and had again taken the ol. tereb.; for I strongly advised her to discontinue its use some time before she left the Finsbury Dispensary, having lost all hopes of its ultimately curing her.

Case III.

By Samuel Fothergill, *M.D.F.M.S., Physician to the Western Dispensary.*

A soldier, aged about 40, applied to the Western Dispensary, the 28th of October 1809. He stated that he had been subject to tape-worm during the last four years, previously to which he had served with the army in Egypt, and attributed the origin of his complaint to the badness of the water which he drank in that country. He is now a private in the Middlesex militia. He complains of gnawing pains in the abdomen, irregular appetite, debility, and anxiety. He is somewhat emaciated, and his complexion is rather sallow. Whilst with his regiment, he had occasionally taken, by order of the surgeon, a variety of worm-medicines, and small pieces of tape-worm were passed at times; they even sometimes came away alive, without medicine having been taken, and without a stool.

I directed him to take pulv. scammon. cum calomel. ℈j. every third morning. Two doses operated freely, but only a few very short pieces of tape-worm were brought away. I now directed him to take half an ounce of the oil of turpentine. He took it as ordered, November 9, in a little tea, sweetened with honey. In a quarter of an hour he was seized with retching, and in the course of the day passed four copious stools, in one of which was a tape-worm of several yards in length. The portion which the patient brought me I found measured four yards; he threw the smaller pieces away; but thought that altogether the length might be ten yards. The worm was dead, and had a livid appearance: the patient remarked that the pieces which formerly passed from him were of a whiter colour and brighter aspect.

The dose of the medicine was increased to six drachms, and was repeated twice a week for the space of a month. During the first fortnight small pieces of worm continued to pass away, both after taking the medicine and at other times; but in the second fortnight the stools were natural, and contained no vestige of tænia. The remedy was consequently discontinued; and the man called some weeks afterwards to acquaint me that he had remained entirely free from all symptoms of his complaint, and had regained his strength and cheerfulness.

He was generally a little sick after taking the medicine, and for a day or two was affected with a severe pain in the back part of his head, but complained of no other unpleasant effects from its use.

Cases IV. and V.

By George Birkbeck, *M.D.F.M.S., Physician to the General Dispensary.*

Dr. Birkbeck stated to the Society that he had administered the oil of turpentine, successfully, to two middle-aged females who had long been troubled with the tape-worm.

In the first case half an ounce was given: no unpleasant sensation occurred whilst it was swallowed, but considerable confusion of ideas and vertigo, with a slight degree of nausea, were soon produced. In a short time a discharge from the bowels took place; this was quickly followed by another, with which more than four yards of the worm were evacuated. The patient, in consequence of the frequent spontaneous escape of small portions of tænia, and the expulsion of a larger quantity about twelve months before by an active purgative medicine, had an opportunity of comparing the ordinary appearance of the worm with that which it now presented. Instead of being whitish, smooth, full, and in motion, she represented it to be dark-coloured, shrivelled, filmy, and lifeless. A second dose of the oil did not expel any more of the worm, nor, when he last saw her, about three months afterwards, had it again appeared. In that interval she had not been disturbed by any of the unpleasant feelings to which she was before subjected.

Considerable derangement of the general health and great pain in the pit of the stomach were produced by the tape-worm, in the second case in which the oil of turpentine was employed. Although one tea-spoonful only was introduced, sickness and acute pain followed: this dose was repeated several successive mornings, always with the same immediate effects; but occasionally it was succeeded by the expulsion of large portions of the worm. The worm was represented to have the appearance before noticed. The patient had sufficient resolution and confidence to continue for some time the use of the medicine, and at length became free, not only from any further appearance of tænia in the stools, but likewise from all those sensations which had so long denoted its presence in the intestines.

CASE VI.

By JAMES SANER, *Surgeon, F.M.S.*

A woman, about 40 years of age, came to me in May last, very much agitated, having just voided about six or eight feet of tape-worm. She told me, that pieces had come away for the last seven years whenever she took a dose of jalap, which she had done that morning. She never found any thing to relieve her so much as the jalap, though she had taken a great deal of medicine from respectable practitioners, and had also been under the care of a noted empiric for two years.

I thought this a good opportunity for trying the ol. tereb. rectificat. I therefore gave her one ounce with an equal quantity of syrup of saffron. In less than two hours she returned to me with about eight feet of the worm, with the head attached. She was very much gratified by this, as she had been told to look for the small black head. The medicine did not produce any unpleasant sensation; merely a slight degree of nausea, a giddiness as if intoxicated, and a frequent desire to void urine, though without pain.

The day after, she complained of a feeling of emptiness in the stomach. I gave her the decoct. cinchonæ for a few days, which completely removed the sensation, and she has remained perfectly well ever since.

P.S.—The woman informed me she used to eat raw meat formerly, as it seemed to ease her stomach more than any thing else; but since she voided the worm, she has had no craving for it.

CASE VII.

By the same.

Since communicating the above, I am sorry to say I have had a case of tænia, where the ol. tereb. rect. has not so completely answered my expectation.

Being very sanguine in my opinion of it, in consequence of my former success, I mentioned the case to a relation of mine, who informed me he knew a labouring mechanic (a Russian) who had voided large pieces of tape-worm for a number of years. He persuaded this man to visit me, and I gave him the same dose I had given my former patient. It brought away a very large quantity, but so very soft that I could not measure it. As I could not perceive any thing like the head of the worm, I advised him to repeat the dose in a few days, which he very readily complied with, as he had suffered very little from the first.

 I gave

I gave him the same quantity as before (viz. ol. tereb. rect. et syr. croci āā ℥j). This produced violent retchings, tenesmus, strangury, and great pain in the back; the urine was also a little tinged with blood. The strangury and tenesmus continued nearly a week, and the patient was not able to work for several days after. As he had not voided any portion of worm with the last dose, I concluded that he was quite well, but requested he would call on me again in about two months. He called last week, and I advised him to try his old remedy (a drachm of jalap), which had its usual effect, in bringing away a large quantity of the worm. I fear I shall not be able to induce him again to try the ol. tereb., from the severe symptoms which it produced when he last used it.

Aug. 27, 1810.

LXIII. *A short Account of the Improvements gradually made in determining the Astronomic Refraction.* By T. S. Evans.

The principal object which the astronomer has in view, is to determine the real places of the heavenly bodies, from the apparent ones observed from a point situated on the earth's surface. In general, it is necessary to reduce them to what they would have been found, were the observer situated in the sun's centre: and it is very seldom that they do not require to be reduced to some other point. Various equations and corrections are of course necessary for this purpose, but none of greater importance than the *refraction*, which is caused by the atmosphere that surrounds the earth, and produces in every ray of light that traverses it, a greater or less deviation from its rectilinear course, according to the density of the air, and the altitude of the object above the horizon. Perhaps there is nothing that has opposed so great an obstacle to the improvement of astronomy as refraction, and nothing requires greater attention by every one who makes observations of any accuracy, since there are very strong reasons for presuming that it is different, in some degree, in almost every different situation. Most of the principal astronomers from Tycho Brahe down to the present time have done something which tended to improve the method of finding it: but further observations and experiments are still wanting, for there is, even now, an uncertainty of several seconds in it, at low altitudes. To bring under one point of view, and in the compass of a small sketch,

sketch, the various endeavours of these illustrious men, is the humble attempt of this short essay; which, it is hoped, will have the desired effect of stimulating others, who possess the means, to the consideration of the subject, that we may shortly be enabled to discover its quantity with the greatest accuracy, at all altitudes, and under all changes of the atmosphere.

There appears to be but little doubt that the astronomic refraction was known to the ancients, since it is expressly mentioned by Ptolomy, although not made use of in his calculations*. He says, near the end of the eighth book of the Almagest, that in the rising and setting of the heavenly bodies there are changes which depend upon the atmosphere: and he mentioned it more at length in a work on optics which unfortunately has not been handed down to us†.

Alhazen, an Arabian writer, who is generally supposed to have lived about the year 1100, and to have taken the greater part of his optics from the works of Ptolomy, speaks also decidedly of it, and shows the manner of convincing ourselves of its existence by experiment ‡.

"Take," he says, "an armillary sphere which turns round its poles, and measure the distance of a star from the pole of the world when it passes near the zenith in the meridian, and when it is rising or setting near the horizon, and you will find the distance from the pole less in the latter case." He then demonstrates that this must arise in consequence of the refraction, but he does not state its quantity.

In the collection of observations made by Bernard Walter, published by Willebrord Snell, in the year 1618, it is stated, the observations were so exact that they pointed out to Walter the quantity by which the altitudes of the stars and planets were increased on account of the refraction.

Tycho Brahe §, however, appears to have been the first who asserted, with any degree of accuracy, that the refraction elevates the heavenly bodies rather more than half a degree when in the horizon. But either his instruments or his observations were not sufficiently correct to determine it with certainty for all degrees from the zenith to the horizon: and accordingly where these failed the rest was supplied by conjecture. He believed that the sun's refraction was 34′ in the horizon, and that it became insensible at 45° of altitude. For the stars, however, he assumed an en-

* La Lande's Astronomy, 2163, 3d edit. Encyclop. Yverd. art. *Refraction.* Encycl. Meth. do.

† La Lande's Astronomy, as above. Smith's Optics, p. 58. Remarks. Priestley's Hist. Opt. 4to, p. 18.

‡ Encyclop. Yverd. art. *Refraction.*

§ Progymn. p. 15.

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tirely different quantity, viz. 30′ in the horizon: but this, according to him, terminated at only 20° of altitude*.

The following is the manner in which it is related that Tycho made this discovery†. He had determined with one or two instruments, extremely well made, the latitude of the place, by observations of polaris above and below the pole. He determined it also by the sun's altitude in both solstices, and found it four minutes less by the latter. At first he doubted the goodness of his instrument, and therefore constructed with the utmost care as many as ten others of different sizes and forms, but they all gave nearly the same result. He could no longer attribute this difference between the two determinations of the latitude to any defect in the observations, and therefore endeavoured, by an attentive consideration of the subject, to find out the cause of this curious phænomenon. At length he supposed it could only arise from the refraction, which elevated the sun at the winter solstice, having then only 11° of altitude above the horizon. This conjecture agreed very well with the principles of optics; but still Tycho Brahe could scarcely persuade himself that the refraction was sufficiently large to produce so great a difference: on this account he made other instruments of ten feet diameter, whose axis corresponded exactly with the pole of the world; and with these he measured the declination of the stars out of the meridian ‡. He then found, that even in summer, the refraction, although insensible at the meridian altitude of the sun, was very considerable near the horizon; and that the defect was about half a degree in the horizon.

A copy of Tycho Brahe's Table§ of Refraction for a star is given in the margin.

Alt.	Refract.
0°	30′ 0″
1	21·30
2	15.30
3	12·30
4	11·00
5	10·00

In this state did the refraction continue for many years. Even Riccioli ‖ in 1665 supposed it nothing at about 26° of altitude: but he thought the moon had only 29′ of horizontal refraction in summer; the sun 30′, and the stars 30′ 37″.

* Mem. de l'Acad. av. s. renouv. tom. v. p. 82. Long's Astronomy, vol. i. p. 254, where a comparison is given of his Table with those of Newton and Flamsteed

† Encycl. Method.

‡ The greater part of these very curious and ingenious instruments are given in his *Astronomicæ instauratæ Mechanica*, printed at Wandesburg in 1598. This work is now become extremely rare, and to be met with only in a few of the great public libraries: on which account M. Jeaurat had the plates engraved again upon a reduced scale, and published in the Memoirs of the Academy of Sciences of Paris for the year 1763, p 120.

§ Progymn. p. 79. 104. Street's Astr. Carol. p. 119. Long's Astr. vol. i. p. 254. Maria Cunitia *Urania Propitia*. p. 286, fol. 1650.

‖ Astr. Reformat. Astr. ref. Tabul. p. 47.

It was not till after the year 1672, that a tolerably near table of refraction made its appearance, when the elder Cassini took the subject into consideration *. What led to this was the voyage of Richer to Cayenne in that year, upon the utility of which some very excellent remarks were made by Cassini, showing how far observations made in a situation so near the equator tended to confirm or disprove certain theories derived from observations made in Europe†. Several very useful deductions were drawn from a comparison of those made both at Paris and Cayenne; among others the refraction was settled upon more accurate elements than heretofore ‡, and a new Table computed, for the first time, of its quantity for all degrees, up to the zenith; an abridgement of which is given in the margin.

Table continued.

Alt.	Refract.
0°	30′ 0″
6	9·0
7	8·15
8	6·45
9	6·0
10	5·30
11	5·0
12	4·30
13	4·0
14	3·30
15	3·0
16	2·30
17	2·0
18	1·15
19	0·30
20	0·0

From the relation of his grandson, it appears, however, that Cassini had at one time computed three tables of refraction for all altitudes: one for winter, another for summer, and a third for spring and autumn: but several doubts having been suggested to him respecting this arrangement, although in appearance conformable to nature, and principally the observations of Richer at Cayenne, where the refraction was found little different from that at Paris, he changed his opinion; and, judging that since the great difference of heat of the torrid zone, from that of the temperate, which we inhabit, does not cause sensible differences in the refraction; therefore, the greatest heat or cold of our climate could not change it much; and he then fixed upon one table, which was that used by the astronomers of the Royal Observatory of Paris, up to the year 1745 §.

Alt.	Refract.
0	32·20
1	27·56
2	21· 4
3	16· 6
4	12·48
5	10·32
10	5·28
15	3 36
20	2·39
30	1·42
40	1·10
45	0 59
50	0·50
60	0·34
70	0·21
80	0·10
90	0· 0

It was always thought, before the time of Cassini, that the refraction did not extend its influence higher

* Mem. de l'Acad. avant son renouv. tom. v. p. 81.

† In the observations of Picard made in various parts of France, in the year 1674, there are several for ascertaining the refraction; and a table is given from them for each degree of altitude up to 22°.—*Mem. de l'Acad. av. son renouv.* tom. iv. p. 111.

‡ Mem. de l'Acad. avant son renouv. tom. v. p. 105. § Ibid. 1745.

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than 45° of altitude: and he is generally considered as the first who proved that it reached all the way to the zenith*. He also supposed that near the equator the horizontal refraction was less than in our climate by about one-third; that this difference decreased as far up as 60°, after which it was the same nearly for both climates.

From this discovery it followed, as a natural consequence, that the refraction must be greater near the pole than at Paris: and this was shortly afterwards proved to the Academy by the publication of a work expressly on that subject†. The king of Sweden, being in 1694 at Tornea in West Bothnia, near the latitude of 65° 45′, and observing that the sun did not set there in the summer solstice, sent the following year some mathematicians to make more certain and exact observations of this curious phænomenon. They are contained in this book, and Messrs. Cassini and De la Hire‡ concluded from them, that in the latitude of 65° 45′ the horizontal refraction must be 58′, or nearly double of that at Paris.

According to an observation made by some Dutchmen§ who passed the winter of 1596–7 in Nova Zembla, in latitude 76° north, the sun, which had entirely disappeared the 14th of November, began to rise again the 24th of January, viz. six days sooner than was expected according to astronomical calculations‖. If so, when the sun has been two or three months under the horizon, as the Dutchmen observed in 1597, the cold becomes dreadful, and perhaps the refraction increases prodigiously. M. le Monnier assures us, that he found by the observations printed in 1599, that the 24th and 27th of January 1597, there were more than 4½ degrees of refraction: that he could neither explain these observations, reject them as doubtful, nor suppose any error, as was done by most of the other astronomers, Kepler, Cassini, Scotto, and, lastly, M. le Gentil¶, who maintained that there were errors in the observations, and accordingly read a memoir on the subject. If it were not so difficult a task to winter in these high latitudes, we might expect such observations as would remove all doubt

* Mem. de l'Acad. 1700, p. 112.

† "Refractio solis inoccidui," &c. Holmiæ, 4to, 1695. These observations in Lapland were made by Messrs. Spole and Bilberg.

‡ In two papers of remarks on these observations published by them in the Mem. de l'Acad. 1700, p. 37.

§ Smith's Optics, p. 61. Remarks. Dr. Jurin's Notes on Varenius's Geography, vol. i. p. 441.

‖ Leipsic Acts, 1679.

¶ Voy. dans les Mers des Indes, tom. i. p. 395; tom. ii. p. 832.

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on the subject; and, perhaps, bring others to light of as great or greater importance*.

The refraction of the north being so considerable, is very useful to the inhabitants, who are deprived of the sun's light during many months; as it makes the sun rise much earlier, and set much later to them, than it otherwise would.

About the year 1725, Mr. Flamsteed, the English Astronomer Royal, published his table† computed from his own observations: and this was the one commonly used in England for many years afterwards.

Sir Isaac Newton also constructed one‡ from theory, which was first published by Dr. Halley in the Philosophical Transactions, No. 368, for 1721. He made the horizontal refraction 33′ 45″; whereas Mr. Flamsteed's was only 33′ 0″.

But although the refraction might be determined within a few seconds at all altitudes by observation; yet, the law of its increase from the zenith to the horizon was a subject that occupied the principal mathematicians and astronomers for more than a century§. Newton having discovered the general principles of attraction, found that the refraction was a consequence of this law of nature; and that it arose from the attraction of the atmosphere on the particles of light. On this principle the curve which a ray of light describes might be determined; since it is successively attracted by different layers of the atmosphere, increasing in density as they approach the earth, and, consequently, bending the ray more and more from the right line which it described in the vacuum previous to its reaching the atmosphere. There are many authors who have endeavoured to find from theory the curve described by this ray in its course, by the assumption of various hypotheses: but perfection and our attempts to arrive at it, as is well observed by the elder Cassini in discoursing on this subject, are like the progress of certain curves and their asymptotes. The principal of these writers on the subject are, Bernouilli[1], Boscovich[2], Bouguer[3], Cassini[4], Des Cartes[5],

* Encyclop. Meth. art. *Refraction.*

† Hist. Célest. vol. i. p. 396; also Hodgson's Math. vol. i. p. 367. Long's Astronomy, p. 254.

‡ Long's Astr. p. 254.

§ In 1714, Cassini published in Mem. de l'Acad. for that year, some methods of finding the refraction by observation, and of determining its quantity by theory. He has also given a table of it for the first 30° of altitude, computed, first, according to a rectilinear, and, secondly, according to a circular hypothesis which he there assumes.

1 Hydrodyn. 1738, p. 221.

2 Oper. tom. ii.

3 Prix de 1729. Mémoires, 1739, p. 407; 1749, p. 75.

4 Epist. ad Montanari, 1665. Refrassioni è Parallosse, &c. 1671. Mem. for 1714, and his Astr. vol. i. p. 11. Paris, 1740, in 2 vols. 4to.

5 Dioptrique, 4to. Paris, 1637.

De

De la Grange[6], Euler[7], Gregory[8], Hodgson[9], Huygens[10], Kramp[11], Lambert[12], Laplace[13], Mayer[14], Newton[15], Oriani[16], Thomas Simpson[17], Brook Taylor[18], Heinsius[19], Tobias Mayer[20], La Hire[21], d'Alembert[22].

It was conjectured by many of the early writers, that the refraction was subject to variations depending upon the weather: but it then amounted to little more than a conjecture, on account of the indifferent manner in which astronomic instruments were divided. Picard found by meridian altitudes of the sun in 1669, that it was greater in winter than in summer. He found also that it was less by day than by night. In the observations given at the end of his journey to Uraniburg *, to settle the latitude of that place, and its difference of longitude from Paris, for the purpose of comparing the observations of Tycho Brahe with those made at the Royal Observatory of Paris, he found the horizontal refraction for the first limb of the sun that made its appearance above the horizon there 33′ 2″, and for the second 32′ 37″. So that in the small interval of time that the sun took to rise, the refraction was diminished 25 seconds by the warmth arising from the sun's presence.

A quadrant being also directed by him from the top of Mount Valerian towards the summit of the church of Nôtre Dame at Paris, he found the depression 20′; but the sun had scarcely risen, when it was increased to 22′; exhalations being raised by the sun's presence, and the medium between Paris and Mount Valerian become more equal; whereas, before the sun rose, the air of Paris was more dense than that of Mount Valerian †.

The density of the atmosphere being the immediate cause of the refraction, it was very natural to suppose that it must decrease as this density became less; whether by causes which diminished its weight, or by the expansion produced by heat: and, indeed, astronomers were not long after this,

6 Nouveaux Mémoires de Berlin, vol. iii. 7 Mem. de Berlin, 1754, tom. x.
8 Astronomy, vol. i. p. 358. edit. of 1715, in 8vo.
9 Mathematics, vol. i. p. 367. Fluxions. p. 133.
10 Traité de la Lumiere, p. 44. Dioptrica, 4to, 1703.
11 Analyse des Refract. Astr. et Terres. 4to. Strasburg, 1799.
12 Les Proprietés Remarquables de la Route de la Lumiere. A la Haye, 1759. Another edition in German, 1773.
13 Mécanique Céleste, vol. iv. p. 231.
14 Tables, 1770. 15 Principia, b. i. sect. 14.
16 Ephem. de Milan, 1788. 17 Mathematical Dissertations, 1743.
18 Methodus Incrementorum, 4to. Lond. 1715. Propos. 27, p. 108.
19 Dissertatio de Computo refractionum Astron. 4to. Leipsig, 1749.
20 De Refractionibus Astronomicis, 4to. Altorf. 1781.
21 Mem. de l'Acad. pour 1702, p. 52.
22 Opuscules Mathématiques, tom. viii. p. 297.
* Mem. de l'Acad. av. s. ren. tom. i. † Encycl. Meth. art. *Refraction.*

before

before they discovered that very sensible differences were occasioned by these circumstances.

But all the honour of introducing corrections on account of the variation of density in the atmosphere, as indicated by the barometer and thermometer, is due to Messrs. Lowthorpe and Hauksbee; the former of whom, in 1698, proved by a very simple experiment, in the presence of the Royal Society, that the refractive power of air is directly proportional to its density*: and the latter, by repeating and extending the same course of experiments in the year 1708, with the machinery pointed out by the former, found that the variations of refraction, depending on the barometer, are proportional to the alteration of height of the mercury in the tube: and by a series of these experiments, he furnished us with a table of the corrections which it is necessary to make on account of the changes of heat indicated by the thermometer. These experiments, although not quite conclusive on the subject, were yet made with as much accuracy and care as the nature of the machinery, and the state of experimental philosophy of that time, would admit. An example is also given, towards the end of his paper, on the mode of applying them to correct the refraction. By these, Hauksbee found that a volume of air expressed by unity, when the thermometer was at 130° above zero, became, at 50° below, one-eighth more dense: or, which is the same thing, that the air lost one-eighth of its density, for an elevation of 180 degrees of Fahrenheit's thermometer; which is exactly the difference of heat between melting ice and boiling water. But although this one-eighth, as will be shown hereafter, was too small; yet it laid the foundation for other experiments, since made by several philosophers, by which the quantity of expansion has been determined more accurately.

We have already shown that the refraction near the pole is greater than in our climate †; the degree of cold being more intense. It was also found to be less in the torrid zone, where the heat is greater than in Europe. Bouguer made a variety of observations at Peru ‡, the result of which he has given us. In 1740, he came down into an island situated in the river of Emeralds, called Isle of Inca, where he determined the refraction from 1° to 7° of altitude: and the table which he computed therefrom, shows the refrac-

* Hauksbee's Exper. 4to, 1709, p. 175.

† It was, however, found by Capt. Phipps, in his voyage to the North, in 1773, that the refraction in latitude 80° was the same as in England. But this was in summer.

‡ Vide Mem. Ac. 1739, and his Fig. de la Terre.

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tion to be about one-seventh less than in Europe*. The horizontal refraction he found to be 27′: but at 6′ of altitude it is 7′ 4″; and at 45° it is 44″. Bouguer then gives a table † for Quito, which is more elevated above the level of the sea. M. le Gentil ‡ found it greater at Pondicherry in India, although in the torrid zone.

The refraction diminishes when we are elevated above the level of the sea. Bouguer observed § the quantity of it at Chimboraço, 2388 toises above the level of the sea, and found it in the horizon only 19¼′. At the cross of Pitchinca, 2044 toises above the sea, he found it 20′ 48″; at Quito, 1479 toises above the sea, 22′ 50″: but at the level of the sea 27′. These observations, when joined with the theory, produced the following rule; That if we take the excess of 5158 toises above the elevation of the place, with regard to the level of the sea, the refraction will be as the square root of this excess. Thus the square root of 5158 toises is 27′, for the horizontal refraction at the level of the sea, in the torrid zone: and the square root of the excess of 5158 above the elevation of the place will be its horizontal refraction. The quantity 5158 is the height above which the refractive matter no longer produces any sensible effect, at least in the torrid zone ||.

But although by this time considerable attention had been paid to the subject, yet great differences were to be found in the tables then most in use. Thus at the altitude of 30°, according to Flamsteed, the refraction was 1′ 23″; Newton 1′ 30″; Cassini 1′ 42″; and de la Hire 1′ 55″; leaving an uncertainty of more than half a minute: and it must have been very mortifying to an observer, after having taken the utmost pains to avoid errors of two or three seconds, to find his reduced observations liable to so great an error, according to the choice of his table of refraction.

It is indeed rather extraordinary, that in a memoir published by Cassini de Thury, among those of the Academy for 1745, he attempted to reconcile a number of observations with each other, by considering the state of the thermometer only, without at all noticing that of the barometer; although at that time Hauksbee's experiments had been published about 37 years.

He concludes his paper, as is very natural to suppose, without being able to make the observations agree: nor does it clearly appear that the French noticed the above-

* This Table is in the memoir above cited.

† Mem. 1749. Conn. des Mouv. Célest. p. 1765. ‡ Mem. 1774. Voyage, tom. i. § Mem. p. 1749. || Encycl. Method. art. *Refr.*

mentioned

mentioned experiments made by Hauksbee till about the year 1749 *. It is also worthy of remark, that although the necessity of introducing corrections on account of the alterations of the barometer and thermometer were likewise shown to be absolutely necessary by Dr. Halley †, and the circumstance mentioned, and in some degree admitted by Le Monnier ‡, yet it does not appear that he followed the advice of his illustrious contemporary, but merely endeavoured, as Cassini did, to reconcile his observations with the state of the thermometer at the time of making these observations, without taking the barometer into account§.

[To be continued.]

LXIV. *Some Particulars respecting the Thunder-storm at London, and in its Vicinity, on the* 31*st of August* 1810. *By Sir* H. C. Englefield, *Bart. F.R.S. and F.S.A.*

To Mr. Tilloch.

Sir, As the stroke of thunder, which was felt in London at about half after two o'clock in the morning of the 31st of August last, was, perhaps, the most violent and awful ever experienced in this country, you may not think the following account of it from an eye-witness, and who was very near the spot where it fell and did mischief, unworthy of insertion in your Journal.

I was with three friends in a coach standing at a house where we had supped. The house-door was still open, and there was a strong light from a large lustre in the hall, full on the coach, and two very bright lamps at the door of the house. This circumstance was in favour of our seeing the nature of the light distinctly; for, had we been in the dark, its excessive brightness would have so dazzled our eyes as to prevent all distinct vision. As we got into the coach there was a small mizzling rain, and a very strong flash of distant lightning in the N.E., but no thunder that we could hear. The servants at the door said there had been much distant lightning for an hour or two.

The sky over head appeared very dark, but the lights prevented accurate observation of it. We were just seated in the

* Mem. de l'Acad. 1749, p. 106.—Probably this was on account of some reflections made by him on the French philosophers who repeated his experiments before the Royal Academy of Paris, and failed in their results.—Vide his book, p. 196.

† Philosophical Transactions 1720, No. 364.

‡ Hist. Céleste, 4to. Paris, 1741.

§ See the whole of his *Discours prelim*, prefixed to the work before cited.

the carriage, and my eyes were directed out of the front window nearly towards the tree which was struck, but which however I could not see. Two of my companions were looking out of the window towards the house-door, from which we were distant five or six feet. We were at once enveloped by an excessively bright diffused blue light of more than instantaneous duration, which appeared to explode into sparks moving in zigzag lines in all directions. My friends saw them between the carriage and the door, and their motion was so strong as to make the pillars of the porch appear to vibrate. The whole had very much the effect of what in artificial fire-works is called a balloon, which as it bursts throws out, from its luminous centre, squibs in all directions. Simultaneous with these zigzag sparks an astonishingly loud, heavy and single explosion took place, similar in sound to the discharge of an enormous cannon directly at us; but incomparably more violent.

The explosion seemed quite on the ground, and was accompanied by a sensation of a dull concussion, as if a vast weight had fallen from a great height on the soft earth close by us. The sound rose in the air, rolling and echoing for a very long time much like common thunder.

Astonishment and terror kept us silent for a little while: we then agreed to quit the coach and take shelter in the house, the door of which remained open. A few heavy drops of rain then fell. On re-entering the hall we found the servants standing aghast at the stroke, which had seemed to them to threaten to crush the whole building. A very heavy rain now came on, which lasted for a few minutes. We were all in fearful expectation of another explosion, but nothing followed. The rain ceased, and we set out. As we passed the gate which leads to the palace from Kensington, we stopped, and asked the sentinel what he had seen and felt. He told us that he could give no distinct account, for that he was dazzled and nearly stunned by the stroke, and was scarcely himself for a minute or two, but that it seemed to him that a vast cannon had been fired at him. In our way to town we saw several severe flashes of lightning to the N. W. with very distant thunder, and by the time we arrived in town the sky was nearly clear, and the stars very bright.

The succeeding day was bright sunshine, and for the season extremely hot; the thermometer being ·84 in the shade, and free from reflected heat. In the evening there was a severe thunder-storm and heavy rain, but which did not cool the air, for both Saturday and Sunday were nearly as hot

hot as Friday, and the nights uncommonly hot, though very bright star-light. Having been informed that mischief was done at Kensington Palace, by the tremendous flash I had witnessed, I went to view the spot. A large elm in the outer Palace-yard, near the Guard-house, and about 120 yards from the spot where our carriage stood, was struck in a manner rather uncommon. A main root about the size of a man's thigh was blown out of the ground to the length of twelve feet from the trunk of the tree, and was broken into three pieces. The trunk of the tree was barked at intervals, not in a continued line, and this injury quitted the main stem at the lowest large branch, and followed that branch up to a fork where some decay appeared in the wood. Beyond that, no injury appeared, nor was the main stem or any other branch higher up affected. The whole appearance of the tree, as well as the sensation I felt from the explosion, lead me to think that the shock was from the earth to the passing cloud.

The part of the Palace directly opposite to the tree is a long building with large arched windows. In these 48 panes of glass were broken by the concussion. This building is about 50 yards from the tree.

The sentinel at the Duke of Sussex's door was knocked down by the shock, and remained, as he said, senseless for some minutes.

Another carriage had just quitted the door where we were, and which was perhaps still nearer the tree than we were. The horses stopped short, and remained motionless. The gentleman in the carriage, when he recovered from his surprise, spoke to his coachman, who as well as the footman declared themselves stunned and blinded. After a pause of a few minutes they however recovered, and felt no further ill effects.

I have been several times as near mischief in storms as I now was; but I am certain that I never saw or heard any lightning or thunder which could be at all compared in tremendous severity to this: indeed it was of a different kind from any other, as the sound was not sharp and crackling as thunder very near usually is, but deep and heavy. Two of the gentlemen who were with me have been often in the southern parts of Europe and the Mediterranean, where storms are much more severe than is usual in England; but they agreed with me that they never had witnessed any thing at all like this. Its effect in London, though the nearest part of the town is full two miles from the explosion, was very singular. Almost every body was waked

waked by it, and waked with the idea of a cannon fired close to them.

The watchmen in the streets, and the toll-man at Hyde-park corner, described the air as completely on fire, and the tremendous sound as being quite close to them. It is not improbable that the discharge, whether to or from the cloud, took place in several points at once. If the account in the papers of a sentinel being struck down, near the Horse-Guards, was true, this must have been the case, and will account for the explosion having been so violent in London.

I am, sir,

Your obedient servant,

Tilney-street, Nov. 7, 1810. H. C. Englefield.

LXV. *Researches on the oxymuriatic Acid, its Nature and Combinations; and on the Elements of the muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution*. By* H. Davy, *Esq. Sec. R.S. Prof. Chem. R.I. F.R.S.E.*†

The illustrious discoverer of the oxymuriatic acid considered it as muriatic acid freed from hydrogen ‡, and the common muriatic acid as a compound of hydrogen and oxymuriatic acid; and on this theory he denominated oxymuriatic acid dephlogisticated muriatic acid.

M. Berthollet §, a few years after the discovery of Scheele, made a number of important and curious experiments on this body; from which he concluded, that it was composed of muriatic acid gas and oxygen; and this idea for nearly 20 years has been almost universally adopted.

Dr. Henry, in an elaborate series of experiments, made with the view of decomposing muriatic acid gas, ascertained that hydrogen was produced from it by electricity; and he attributed the phænomenon to water contained in the gas ||.

In the Bakerian lecture for 1808, I have given an account of the action of potassium upon muriatic acid gas, by which more than one-third of its volume of hydrogen is produced; and I have stated, that muriatic acid can in no instance be procured from oxymuriatic acid, or from dry muriates, unless water or its elements be present.

In the second volume of the *Mémoires d'Arcueil*, MM. Gay

* Communicated to the Royal Society at the request of the Managers of the Royal Institution.

† From the Philosophical Transactions for 1809, Part II.

‡ Mem. Acad. Stockholm for 1774, p. 94.

§ *Journal de Physique*, 1785, p. 325.

|| Phil. Trans. for 1800, p. 191.

Gay Lussac and Thenard have detailed an extensive series of facts upon muriatic acid and oxymuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oxymuriatic acid is not decomposable by any substances but hydrogen, or such as can form triple combinations with it.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oxymuriatic or muriatic acid gases, by the Voltaic battery, effects no change in them; if it has been previously freed from hydrogen and moisture by intense ignition in vacuo.

This experiment, which I have several times repeated, led me to doubt of the existence of oxygen in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more rigorous investigation than had been hitherto attempted for its detection.

If oxymuriatic acid gas be introduced into a vessel exhausted of air, containing tin; and the tin be gently heated, and the gas in sufficient quantity, the tin and the gas disappear, and a limpid fluid, precisely the same as Libavius's liquor, is formed:—it occurred to me, that if this substance is a combination of muriatic acid and oxide of tin, oxide of tin ought to be separated from it by means of ammonia. I admitted ammoniacal gas over mercury to a small quantity of the liquor of Libavius; it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white colour; some of it was heated, to ascertain if it contained oxide of tin; but the whole volatilized, producing dense pungent fumes.

Another experiment of the same kind, made with great care, and in which the ammonia was used in great excess, proved that the liquor of Libavius cannot be decompounded by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oxymuriatic acid, and I have stated that two compounds, one fluid and the other solid, are formed in the process of combustion, of which the first, on the generally received theory of the nature of oxymuriatic acid, must be considered as a compound of muriatic acid and phosphorous acid. It occurred to me, that if the acids of phosphorus really existed in these combinations, it would not be difficult to obtain them, and thus to gain proofs of the existence of oxygen in oxymuriatic acid.

I made a considerable quantity of the solid compound of oxymuriatic acid and phosphorus by combustion, and saturated it with ammonia, by heating it in a proper receiver filled with ammoniacal gas, on which it acted with great energy, producing much heat; and they formed a white opake powder. Supposing that this substance was composed of the dry muriates and phosphates of ammonia; as muriate of ammonia is very volatile, and as ammonia is driven off from phosphoric acid, by a heat below redness, I conceived that, by igniting the product obtained, I should procure phosphoric acid; I therefore introduced some of the powder into a tube of green glass, and heated it to redness, out of the contact of air, by a spirit lamp; but found, to my great surprise, that it was not at all volatile nor decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance that a substance composed principally of oxymuriatic acid, and ammonia, should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.

It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water: it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were by combustion, or the action of ignited hydrat of potash. When brought into the flame of a spirit lamp and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow colour, and left a fixed acid having the properties of phosphoric acid. When acted on by red-hot hydrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hydrat of potash.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of MM. Gay Lussac and Thenard; and on the sulphuretted muriatic liquor of Dr. Thomson; but no decomposition took place; nor was any muriate of ammonia formed when proper precautions were taken to exclude

exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat; but which seemed no further decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of colour, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia; but as these compounds did not present the same uniform and interesting properties as that from the phosphoric sublimate, I did not examine them minutely: I contented myself by ascertaining that no substance known to contain oxygen could be procured from oxymuriatic acid, in this mode of operation.

It has been said, and taken for granted by many chemists, that when oxymuriatic acid and ammonia act upon each other, water is formed; I have several times made the experiment, and I am convinced that this is not the case. When about 15 or 16 parts of oxymuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gases, and from five to six parts of nitrogen are produced; and the result is dry muriate of ammonia.

Mr. Cruikshank has shown that oxymuriatic acid and hydrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water; and MM. Gay Lussac and Thenard have stated that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oxymuriatic acid gas and hydrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapour, and a condensation of from $\frac{1}{10}$ to $\frac{1}{20}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oxymuriatic acid and the hydrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though, in proportion as the gases were free from oxygen or water, this condensation diminished.

I mixed together sulphuretted hydrogen in a high degree of purity and oxymuriatic acid gas, both dried, in equal volumes: in this instance the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oxymuriatic acid, was formed on the sides of the vessel; no vapour was de-

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posited; and the residual gas contained about $\frac{19}{20}$ of muriatic acid gas, and the remainder was inflammable.

MM. Gay Lussac and Thenard have proved by a copious collection of instances, that in the usual cases where oxygen is procured from oxymuriatic acid, water is always present, and muriatic acid gas is formed: now, as it is shown that oxymuriatic acid gas is converted into muriatic acid gas by combining with hydrogen, it is scarcely possible to avoid the conclusion, that the oxygen is derived from the decomposition of water, and, consequently, that the idea of the existence of water in muriatic acid gas is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxygen in oxymuriatic acid gas.

MM. Gay Lussac and Thenard indeed have stated an experiment, which they consider as proving that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious that in this case they formed the same compound as that produced by the action of oxymuriatic acid on lead; and in this process the muriatic acid must lose its hydrogen, and the lead its oxygen; which of course would form water: these able chemists, indeed, from the conclusion of their memoir, seem aware that such an explanation my be given, for they say that the oxymuriatic acid *may be* considered as a simple body.

I have repeated those experiments which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find that, when mercury is made to act upon one in volume of muriatic acid gas, by Voltaic electricity, all the acid disappears, calomel is formed, and about ·5 of hydrogen evolved.

With potassium, in experiments made over very dry mercury, the quantity of hydrogen is always from nine to eleven, the volume of the muriatic acid gas used being 20.

And in some experiments made very carefully by my brother Mr. John Davy, on the decomposition of muriatic acid gas, by heated tin and zinc, hydrogen equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted.

It is evident from this series of observations, that Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oxymuriatic and muriatic acids may be considered as an expression of facts; whilst the view adopted by the French school of chemistry,

chemistry, and which, till it is minutely examined, appears so beautiful and satisfactory, rests, in the present state of our knowledge, upon hypothetical grounds.

When oxymuriatic acid is acted upon by nearly an equal volume of hydrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oxymuriatic acid is attracted from the hydrogen, by the stronger affinity of the metal; and an oxymuriate, exactly similar to that formed by combustion, is produced.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oxymuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxygen may be conceived to be supplied to the tin, and hydrogen to the oxymuriatic acid.

The compound formed by burning phosphorus in oxymuriatic acid is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxygen, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hydrogen to the oxymuriatic acid to form common muriatic acid gas.

None of the combinations of the oxymuriatic acid with inflammable bodies can be decomposed by dry acids; and this seems to be the test which distinguishes the oxymuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if M. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oxymuriatic acid with potassium: muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, is is decompounded: the oxymuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hydrogen are set free.

The vivid combustion of bodies in oxymuriatic acid gas, at first view, appears a reason why oxygen should be admitted in it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids, become ignited during their mutual agency; and such an effect might be expected in an operation

tion so rapid, as that of oxymuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oxymuriatic acid consists of an acid basis united to oxygen, exists in the general analogy of the compounds of oxymuriatic acid, and metals, to the common neutral salts: but this analogy, when strictly investigated, will be found to be very indistinct; and even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water, and the metals of bases with hydrogen; and that, in the case of the action of oxymuriatic acid and metals, the metal furnishes hydrogen to form muriatic acid, and a basis to produce the neutral combination.

That the quantity of hydrogen evolved during the decomposition of muriatic acid gas by metals, is the same that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hydrogen with oxymuriatic acid, one quantity must always be separated. Hydrogen is disengaged from its oxymuriatic combination, by a metal, in the same manner as one metal is disengaged by another from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hydrogen is that which seems to adhere to oxymuriatic acid with the lesat force.

I have caused strong explosions from an electrical jar to pass through oxymuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

I electrized the oxymuriates of phosphorus and sulphur for some hours, by the power of the Voltaic apparatus of 1000 double plates: no gas separated, but a minute quantity of hydrogen, which I am inclined to attribute to the presence of moisture in the apparatus employed; for I once obtained hydrogen from Libavius's liquor by a similar operation: but I have ascertained that this was owing to the decomposition of water adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter.

As there are no experimental evidences of the existence of of oxygen in oxymuriatic acid gas, a natural question arises concerning the nature of these compounds, in which the muriatic acid has been supposed to exist, combined with much more oxygen than oxymuriatic acid, in the state in which it has been named, by Mr. Chenevix, hyperoxygenized muriatic acid.

Can the oxymuriatic *acid* combine either with oxygen or hydrogen, and form with each of them an acid compound; of which that with hydrogen has the strongest, and that with oxygen the weakest affinity for bases? for the able chemist to whom I have just referred, conceives that hyperoxymuriates are decomposed by muriatic acid. Or, is hyperoxymuriatic acid the basis of all this class of bodies, the most simple form of this species of matter?

The phænomena of the composition and decomposition of the hyperoxymuriates may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxygenized, from hyperoxymuriate of potash, by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though a little oxymuriatic acid is generated, yet oxygen is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange-coloured fluid, produced by dissolving hyperoxymuriate of potash in sulphuric acid, affords only oxygen in great excess, and oxymuriatic acid.

When solutions of muriates, or muriatic acid are electrized in the Voltaic circuit, oxymuriatic acid is evolved at the positive surface, and hydrogen at the negative surface. When a solution of oxymuriatic acid in water is electrized, oxymuriatic acid and oxygen appear* at the positive surface, and hydrogen at the negative surface; facts which are certainly unfavourable to the idea of the existence of hyperoxygenized muriatic acid, whether it be imagined a compound of oxymuriatic acid with oxygen, or the basis of oxymuriatic acid.

If the facts respecting the hyperoxymuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oxymuriatic acid, potassium, and oxygen. We have no right to

* The quantity of oxymuriatic acid in the aqueous solution is so small, that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

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assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxygen combined with the potassium, which we know has an intense affinity for oxygen, and which, from some experiments, I am inclined to believe, is capable of combining directly with more oxygen than exists in potash, than with the oxymuriatic acid, which, as far as is known, has no affinity for that substance.

It is generally supposed that a mixture of oxymuriatic acid and hyperoxymuriatic acid is disengaged when hyperoxymuriate of potash is decomposed by common muriatic acid *; but I am satisfied from several trials, that the gas procured in this way, when not mixed with oxygen, unites to the same quantity of hydrogen †, as common oxymuriatic acid gas from manganese; and I find, by a careful examination, that the gas disengaged during the solution of platina, in a mixture of nitric and muriatic acids, which has been regarded as hyperoxymuriatic acid, but which I stated some years ago to possess the properties of oxymuriatic acid gas ‡, is actually that body, owing its peculiar colour to a small quantity of nitromuriatic vapour suspended in it, and from which it is easily freed by washing.

Few substances, perhaps, have less claim to be considered as acid, than oxymuriatic acid. As yet we have no right to say that it has been decompounded; and as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxygen.

May it not in fact be a *peculiar* acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxygen or oxides, in their

* If hyperoxymuriate of potash be decomposed by nitric or sulphuric acid, it affords oxymuriatic acid and oxygen. If it be acted upon by muriatic acid, it affords a large quantity of oxymuriatic acid gas only. In this last case, the phænomenon seems merely to depend upon the decomposition of the muriatic acid gas, by the oxygen, loosely combined in the salt.

† This likewise appears from Mr. Cruikshank's experiments. See Nicholson's Journal, vol v. 4to, p. 206.

‡ The platina, I find by several experiments made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hydrogen of the muriatic acid attracts oxygen from the nitric acid. Oxymuriatic acid gas is set free, and nitrous gas remains in the solution, and gives it a deep red colour. *Nitrous* acid and muriatic acid produce no oxymuriatic acid gas. Platina, during its solution in perfectly formed aqua regia, gives only nitrous gas and nitrous vapour; and I find, that rather more oxymuriatic acid gas is produced, by heating together equal quantities of nitric acid of 1·45, and muriatic acid of 1·18, when they are not in contact with platina, than when exposed to that metal. The oxymuriatic acid gas produced from muriatic acid by nitric acid, I find combines with about an equal volume of hydrogen by detonation.

properties and powers of combination; but differing from them, in being for the most part decomposable by water? On this idea muriatic acid may be considered as having hydrogen for its basis, and oxymuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oxymuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oxymuriatic acid, may be regarded as analogous bodies. The combinations of oxymuriatic acid with lead, silver, mercury, potassium, and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

It is needless to take up the time of this learned society by dwelling upon the imperfection of the modern nomenclature of these substances. It is in many cases connected with false ideas of their nature and composition; and, in a more advanced state of the inquiry, it will be necessary for the progress of science, that it should undergo material alterations.

[To be continued.]

LXVI. *Of the Bogs in Ireland.*

The first Report of the Commissioners appointed by Parliament to inquire into the nature and extent of the several bogs in Ireland, and the practicability of draining and cultivating them, has just made its appearance. It consists of seven folio pages, and an Appendix containing, 1. Instructions of the Commissioners to their Engineers—3 pages: 2. Names of the Engineers, Surveyors, Clerks, and other Officers appointed and employed by the Commissioners; with their Salaries and Rewards—1 page: 3. Account of all Sums of Money paid by or under the Authority of the Commissioners—1 page: 4. Report of Mr. Richard Griffith, jun. Civil Engineer, on the Practicability of draining and improving a Part of the Bog of Allen—41 pages. It is accompanied with a Map of Part of the Bog of Allen; transverse Sections of Lullymore Bog; a Section of a subterraneous River in Lullymore Bog; and a Section of a Turf Bank in Timahoe Bog.

The commissioners, after some preliminary observations, state, that in forming their opinions on the points connected with their inquiry, they derived their principal assistance from the Great Ordnance Survey of Ireland, executed by General Vallancey, the Chairman of their Board, it being the only map which defines either the situation or boundaries

boundaries of the bogs with any tolerable accuracy. They then report as follows :—

" From inspection of this map we were enabled to consider the greater part of these bogs as forming one connected whole, and to come to the general conclusion, that a portion of Ireland, of little more than one fourth of its entire superficial extent, and included between a line drawn from Wicklow head to Galway, and another drawn from Howth head to Sligo, comprises within it about six-sevenths of the bogs in the island, exclusive of mere mountain bogs, and bogs of less extent than 500 acres, in its form resembling a broad belt drawn across the centre of Ireland, with its narrowest end nearest to the capital, and gradually extending in breadth as it approaches to the Western Ocean. This great division of the island extending from east to west is traversed by the Shannon from north to south, and is thus divided into two parts : of these the division to the westward of the river contains more than double the extent of the bogs which are to be found in the division to the eastward ; so that, if we suppose the whole of the bogs of Ireland (exclusive of mere mountain bog and of bogs under 500 acres) to be divided into twenty parts, we shall find about seventeen of them comprised within the great division we have now described, twelve to the westward and five to the eastward of the Shannon, and of the remaining three parts, about two are to the south and one to the north of this division : of the positive amount of their contents we have as yet no data that can enable us to speak with any precision; but we are led to believe, from various communications with our engineers, that the bogs in the eastern division of the great district above described amount to about 260,000 English acres, which on the proportion already mentioned would give rather more than one million of English acres as the total contents of the bogs of Ireland, excluding however from consideration mere mountain bogs, and also all bogs of less extent than 500 acres, of each of which description the amount is very considerable : of the extent of the latter some idea may be formed from a fact which we have learned from Mr. Larkin, that in the single county of Cavan, which he has surveyed, there are above ninety bogs, no one of which exceeds 500 Irish acres, but which taken collectively contain above 11,000 Irish, which is equivalent to above 17,600 English acres, besides many smaller bogs varying in size from five to twenty acres.

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"Most of the bogs which lie to the eastward of the Shannon, and which occupy a considerable portion of the King's county and county of Kildare, are generally known by the name of the Bog of Allen: it must not however be supposed that this name is applied to any one great morass; on the contrary, the bogs to which it is applied are perfectly distinct from each other, often separated by high ridges of dry country and inclining towards different rivers, as their natural directions for drainage, so intersected by dry and cultivated land, that it may be affirmed generally there is no spot of these bogs (to the eastward of the Shannon) so much as two Irish miles distant from the upland and cultivated districts.

"With this first and general view of the subject, we had no hesitation in selecting at once the whole of the eastern portion of the great district above referred to, as the object of our first inquiries, forming in itself one whole, whose parts had more or less connexion with each other, lying in the centre of Ireland, in the immediate vicinity of some of the richest and best cultivated counties; intersected also by the two great lines of navigation the Grand and the Royal Canals, and presenting in common apprehension very considerable obstacles to improvement; the overcoming of which would in itself demonstrate the practicability of the improvement of the bogs of Ireland in most other cases.

"We were further induced to form this selection on the general principles of beginning at the end of the great division above referred to, which lies nearest to the capital, and proceeding gradually to its termination at the Western Ocean; not however considering ourselves precluded from making occasional exceptions, where particular circumstances might appear to require it.

"The proportion which the bogs in this district bear to the entire of the bogs of Ireland, appeared to us a further inducement; and we are the more disposed to mention this, as we find that by some we have been thought to have embarked in the first instance on too great a scale: on this we shall merely observe, that having two years allotted to us for the duration of our commission, we undertook at once rather less than one third of our task, in the supposition that it would require about eight months for its execution.

"Having determined to give in charge the whole of this district, it became the next object of our consideration, on what principle we should subdivide it into the smaller districts, referred to in the first article of our instructions, for the purpose of being assigned to separate engineers.

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Major Taylor's excellent map of the county of Kildare furnished us with every necessary information, so far as that county was in question: but of the King's county there was no map published; and as it contains not less than 124,000 English acres of bog, it became a most important object to possess ourselves of the necessary information with respect to them.

"We therefore thought ourselves fortunate in finding that Mr. Larkin, a surveyor of eminence, had surveyed the county for the grand jury; and we contracted with him to furnish us with a map of it, on the large scale required by our instructions; and Mr. Larkin making himself responsible for the accuracy of the survey, we agreed to give him for it 300*l.* being at the rate of less than three farthings per acre for every acre of bog it contained. With these and the assistance of other documents, we divided all the bogs, containing above 500 acres, in the counties of Kildare, King's county, Tipperary, Westmeath, and Longford, into seven districts: of these we gave the one which forms the north-eastern part of the Bog of Allen, in charge to Mr. Richard Griffith; the south-eastern to Mr. Brassington; the north-western to Mr. Townshend; the south-western to Mr. Longfield; a district lying principally in Westmeath to Mr. Jones; and the bogs in the county of Longford, and on both banks of the river Inny, to Mr. Edgeworth.

"We also gave a large district of bog in the county of Tipperary, which runs nearly parallel to the suir from Roscrea to Cashell, in charge to Mr. Aher, wishing to take advantage of the circumstance of his being able to give a portion of his time to that district, although not to any other, on account of his other engagements.

"We next laid down the principles which were to govern our expenditure, in such manner as to secure that the amount of our disbursements should depend in every instance on the degree of labour to be performed.

"With these views, we fixed the pay of engineers at two guineas a day for every day actually employed, and one guinea a day in lieu of allowances for travelling and board and lodging. That of their surveyors at one guinea a day for each, while employed, to be at once their pay and in lieu of all allowances of every description. For the staffmen, chain-men, and labourers, we intrusted the engineers to make the best bargains in their power, not exceeding three shillings per day in any instance; and these terms we trust will appear extremely moderate when compared

with

with those usual in Great Britain, and considering the hardships attendant on this peculiar service. The appointment of the engineers we necessarily hold in our hands, and select them under the obligation of our oaths; the appointment of the surveyors we commit entirely to the engineers, holding the latter responsible for the qualifications of the persons they employ.

"We account with every engineer once a week, and he makes his return to us upon his oath.

"To give an idea of the scale and nature of our expenditure, we subjoin, as the second and third articles of our Appendix, copies of accounts already called for by your honourable house.

"Owing to the winter season having set in, almost immediately after the appointment of the engineers, and which was particularly unfavourable to the execution of the survey, we have as yet received but one of their reports, although they are most of them, we believe, in a state of considerable forwardness.

"This report we have determined on laying at once before your honourable house, considering it as sufficient in itself, to enable the public to form a pretty accurate opinion of the degree of information which may be expected from the execution of our commission; and feeling also, that if we deferred it any longer, we should have no other opportunity before the opening of the next session: we have accordingly subjoined it as the fourth article of the Appendix to this Report.

"The district reported on contains 36,430 English acres of bog, and forms the eastern extremity of the Bog of Allen. The map furnished to us by Mr. Griffith is on a scale of four inches to an Irish mile, and is accompanied by sections of the bog of nearly 200 miles in extent.

"As these maps and sections could not be engraved without enormous expense, we have subjoined to this report a map executed on a scale as much reduced as is consistent with clearness, and which scale we propose to apply universally in the different maps which in the execution of the commission it will become our duty to furnish to your honourable house; and this map we have accompanied with three lines of sections of the bog, to serve at once as specimens of the manner in which the sections are executed, and to convey a clearer view than could be expressed in words, of the internal structure of a great bog; a view, we believe, materially different from any of those generally received.

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"There are many, we believe, who consider the bogs of Ireland to be low and marshy tracts of country not very dissimilar in their composition from the fens of Lincolnshire: others, aware that the substance of which they are formed greatly differs from that of the fen districts, attribute nevertheless the origin of both to pretty nearly the same causes; while an opinion, more prevalent, and perhaps not less erroneous than either of the foregoing, attributes their formation to fallen forests, which are supposed at some former period to have covered these districts, and to have been destroyed either by the effects of time, or by hostile armies in the early wars of Ireland.

"The facts stated in Mr. Griffith's report are obviously inconsistent with any of these suppositions; the bogs which he has surveyed being every where in elevated situations, and the trees which have hitherto been so constantly found buried in the edges of these bogs, where alone it is probable they have generally been sought for, are very rarely to be found in the interior parts at least of this district.

"Without entering in this report into any inquiry as to the origin of peat bogs, we are however anxious to give to such persons as have not had an opportunity of examining them, some idea of the general appearances which they actually present.

"It appears from Mr. Griffith, that each of the four bogs included in the subject of his report, is a mass of the peculiar substance called peat, of the average thickness of 25 feet, no where less than twelve, nor found to exceed 42; this substance varying materially in its appearances and properties, in proportion to the depth at which it lies: on the upper surface, covered with moss of various species, and, to the depth of about ten feet, composed of a mass of the fibres of similar vegetables in different stages of decomposition proportioned to their depth from the surface, generally however too open in their texture to be applied to the purposes of fuel: below this generally lies a light blackish brown turf, containing the fibres of moss still visible, though not perfect, and extending to a further depth of perhaps ten feet under this. In the instance exhibited in the section at the close of Mr. Griffith's report, are found small branches and twigs of alder and birch, but we do not understand him as being of opinion that such is by any means generally the case: at a greater depth the fibres of vegetable matter cease to be visible, the colour of the turf becomes blacker, and the substance much more compact, its properties as fuel more valuable, and gradually increas-

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ing in the degree of blackness and compactness proportionate to its depth: near the bottom of the bog it forms a black mass, which when dry has a strong resemblance to pitch or bituminous coal, and having a conchoidal fracture in every direction, with a black shining lustre, and susceptible of receiving a considerable polish.

"We have requested Mr. Griffith to make a chemical analysis of these different strata, which he has done in the laboratory of the Dublin Society, and an account of which, with the section above alluded to, forms the Appendix to his Report. Immediately below this lower stratum, there is generally found a thin stratum of yellow or blue clay, varying in thickness from one to six feet; in some places the peat rests on a thinner stratum of yellowish white marl, containing on an average about 60 per cent. of calcareous matter; this stratum of clay in this district universally rests on a solid mass of clay and limestone gravel mixed together, and extending to an unknown depth.

"We should further consider the peat moss as partaking in its general nature of the property of sponge completely saturated with water, and giving rise to different streams and rivers for the discharge of the surplus waters which it receives from rain or snow: these streams in this district almost universally have worn their channels through the substance of the bog down to the clay or limestone gravel underneath, dividing the bog into distinct masses, and presenting in themselves the most proper situations for the main drains, and which, with the assistance of art, may be rendered effectual for that purpose.

"Such is the internal structure of the bogs in this district.

"Viewing them externally, they present surfaces by no means level, but with planes of inclination amply sufficient for their drainage: the highest summit of any part of the bogs in this district, is 298 feet above the level of the sea, taken at an ordinary spring-tide in the Bay of Dublin; while the lowest point any where on their surface is 84 feet lower than the highest, and therefore 214 feet above the level of the sea.

"It requires a mere inspection of the map and sections, to be convinced that there is no part of these bogs from which the water may not be discharged into rivers in their immediate vicinity, and with falls adequate to their drainage; and we observe, that in the instance of the Bog of Timahoe, a part of its water is discharged into the sea at Drogheda, and another part below Waterford."

The

The commissioners then report their opinion of the probable expense of these operations, &c. &c. By Mr. Griffith's report to the commissioners, the total amount of estimate for draining the several bogs contained in the eastern division, or district No. 1, is 147,032*l.* 6*s.* 11*d.* and the quantity of land to be gained 22,490 Irish (equal to 36,430 English) acres.

The following description of a section of a turf bank in Timahoe Bog (see Plate IX.) is copied from the conclusion of Mr. Griffith's report:—

"The foregoing section is an exact representation of a turf bank on the southern edge of Timahoe Bog.

"The surface of the bog has been partially drained for about 20 perches into the interior, which has occasioned the upper and most porous part to subside three feet, the fibres of moss having lost their watery support, and not being sufficiently strong in themselves to retain their former elevation. The annual growth of moss on this bog being prevented by the absence of water, it may be considered as dead.

"In the Report, page 30, I have stated, that in drained bogs, when the bog-mosses, &c. which compose the upper surface shall have subsided, and by the near approach of their mossy fibres (which when alive are kept asunder by water) and their exposure to the atmosphere shall become (to a certain degree) putrid, it will be found that various grasses of good quality, and even white clover, will vegetate spontaneously on its surface.

"The bog, of which the section is the face, has now been superficially drained for three years, and the effect above described has taken place to a certain degree, as the common meadow, the tivrin, or jointed grass, and white clover, are now growing on its surface, though sparingly; and the surface of the bog has been so far acted upon by the atmosphere as to have totally lost the texture of the moss, and to have assumed a close-grained earthy appearance; whilst in the bed immediately below it, the mossy fibres are so perfect, as to render the different species perfectly distinguishable to the botanist, as may be seen by the specimens which I now lay before the commissioners.

"DESCRIPTION AND ANALYSIS.

"No. 1. .. .. 2 feet thick.

"Surface of bog decomposed by exposure to the atmosphere: mass compact; contains rarely any vegetable remains; where they occur they are chiefly composed of fibres of of moss in the last stage of decomposition, and decayed branches of heath.

Colour, ... reddish-brown :
Specific gravity .. ·895.

"1,440 grains of this substance yielded but 20 grains of white ashes, which are found to be composed of vegetable matter.

"No. 2. .. .. 3 feet thick.

"The mass here is very open-grained and fibrous; the moss is usually so perfect, that the different species are easily discernible to the botanist: the sphagnum palustre is observed greatly to predominate.

Colour, ... light reddish-brown :
Specific gravity .. ·356.

"1,440 grains of this substance yielded but 12 of white ashes, of similar composition to No. 1.

"No. 3. .. .. 5 feet thick.

"Mass open-grained and fibrous; varieties of moss visible, but not so perfect as in No. 2: used as turf, but burns badly, on account of the openness of its texture, and its containing no empyreumatic oil.

Colour, ... pale yellowish-brown :
Specific gravity .. ·408.

"1,440 grains of the dried peat yielded but 11 grains of white ashes, of similar composition to the foregoing.

"No. 4. .. .. 8½ feet thick.

"Mass tolerably compact, but still fibrous; when used as turf, it burns tolerably well.

Colour, ... deep reddish-brown :
Specific gravity .. ·871.

"1,440 grains of the dried peat yielded 12 grains of yellowish white ashes, composed of vegetable matter, with a tinge of oxide of iron.

"No. 5. .. .. 3 feet thick.

"Mass compact; fibres of moss rarely discernible*; numerous twigs, and small branches of birch, alder, and fir-trees, are observable amongst the peat in this part of the turf bank. Upon near inspection it was found, that all the branches and twigs were quite hollow; the wood being decayed had disappeared, leaving the bark perfect. This division of the turf bank, when used as turf, burns pleasantly, but quickly.

Colour, ... blackish brown :
Specific gravity .. 1·030.

* Branches are not found contained in the body of the Bog generally, and even at the edges not universally.

	Grains.
"Analysis.—1,440 grains yielded, of volatile empyreumatic oil	140
Of water, containing a small portion of oil that could not be separated	834
Light porous charcoal	298
Carbonated hydrogen gas, which burned with a clear blueish-white light, equal, if not superior, to the coal gas	168
	1,440

"500 grains of this charcoal yielded 15 grains of light yellowish-white ashes, composed of vegetable matter, and a slight tinge of oxide of iron.

"No. 6. .. .. 3 feet thick.

"Mass compact; contains no vegetable remains; when used as turf burns swiftly, and with a bright flame; it is usually denominated greasy turf, from its inflaming quickly like grease.

Colour, ... dull yellowish-brown:

Specific gravity .. ·694

	Grains.
"1,440 grains yielded, of volatile empyreumatic oil	180
Water, containing a small portion of oil that could not be separated	816
Light porous charcoal, which, when broken, exhibited a faintly shining lustre	327
Gaseous product, which, when ignited, burned with a blueish-white light, similar to No. 5	117
	1,440

"500 grains of this charcoal yielded 16 grains of yellowish-white ashes, similar in quality to No. 5.

"No. 7. .. .. 10 feet thick.

"Mass very compact; no vegetable remains visible; when used as turf burns slowly, and with an unpleasant smell.

Colour, ... blackish-brown.

"Fracture earthy, with a tendency to conchoidal; lustre, when first broken, faintly glimmering.

Specific gravity .. .. 1·057.

	Grains.
"1,440 grains yielded, of volatile empyreumatic oil	138
Of water, containing a minute portion of oil that could not be separated	538
A very compact charcoal, internal lustre glistening	590
Gaseous product	174
	1,440

"500

"500 grains of this charcoal yielded of deep reddish-brown ashes 50 grains, which are chiefly composed of oxide of iron.

"No. 8. .. .. 4 feet thick.

"Mass very compact; contains no vegetable remains; is seldom used as turf, owing to the unpleasant smell it gives out when ignited.

Colour, ... black.

"Fracture conchoidal in every direction; lustre shining; exhibits a strong resemblance to pitch or pitch coal; and is susceptible of a high degree of polish.

Specific gravity .. .. 1·236.

"Analysis.—1,440 grains yielded, of volatile em-

	Grains.
pyreumatic oil	124
Water, containing oil that could not be separated	582
Charcoal very compact, internal lustre strongly glistening	566
Gaseous product, which burned with a bright light, but unpleasant smell	168
	1,440

"500 grains of this charcoal yielded 96 grains of brick-red ashes, which are found to be chiefly composed of oxide of iron.

"No. 9. .. .. 3 feet thick.

"Marle; colour, yellowish-white; does not adhere to the tongue.

"100 parts contain:

	Grains.
Carbonate of lime	64
Silex	24
Alumine	12
	100

"No. 10. .. .. 4 feet thick.

"Yellowish-blue clay; adheres strongly to the tongue.

"100 parts are found to contain:

Alumine	72
Carbonate of lime	6
Silex, coloured by oxide of iron	22
	100

"Being very much pressed for time in making the foregoing analysis, I have been obliged to attend merely to the most useful results. I hope, however, in a future Report, to be able to lay before the commissioners a more detailed analysis, containing a minute examination of the composition of the ashes contained in the charcoal, and also the exact composition of the gaseous products."

LXVII. *On purifying Olive Oil for the Pivots of Chronometers. By* Ez. Walker, *Esq.*

To Mr. Tilloch.

Sir, After all the experiments which have been made to decrease friction in time-keepers, nothing has yet been found to answer this purpose so well as oil. But it has long been known that the application of this fluid to marine chronometers is attended with very pernicious consequences; for it gradually loses its fluidity during a long voyage, and adheres to the parts of the machine, by which all regularity in its performance is destroyed. Hence I was led to suppose, that time-keepers might be improved if oil of a better quality, than that which had been commonly used, could be procured.

About the year 1799, I made several experiments to separate from olive oil some of those impurities which it is known to contain, and I succeeded so far as to separate a thick mucilaginous matter from the best I could procure. This mucilage is an opake whitish matter, heavier than oil but lighter than water. The oil from which the mucilage has been taken is exceedingly transparent in a fluid state, but after it has been frozen it appears much whiter than common oil exposed to the same degree of cold.

About ten years ago I sent a small quantity of this oil to Mr. Barraud, requesting him to make trial of it, and in March 1802 he gave me the following account:—

"I have," says Mr. Barraud, "just received a chronometer, in which the oil you favoured me with was used; which having performed a voyage of 16 months to and from India, is vibrating as freely as at first, and keeping the rate it went out with to a fraction of a second."

Since that time Mr. Barraud has frequently applied to me for more of this oil, and continues to use it in his best time-keepers; but to be informed more particularly respecting it, I wrote to him requesting to know the result of his long experience, and the following extract is taken from his interesting answer:—

"To

"*To Mr. Walker.*

"London, 13th October, 1810.

"Dear sir,—It is, I believe, upwards of ten years since you first favoured me with some of your purified oil, which I have ever since constantly applied to my chronometers; and on their return from a long voyage I have always found your oil in good condition, much better indeed than any which I had before been able to obtain; nor has the superior quality of yours been confined to my own observation.

"The late Mr. John Brockbank was complaining to me, some years ago, of the bad state in which he found the oil in his chronometers on their return from India, many of which had failed in consequence, although the oil he used was the best he could obtain. I then mentioned the success which had attended yours, and at his request furnished him with a small quantity, which he applied to his chronometers, and afterwards very gratefully acknowledged the advantage he had derived from its use; having found, on the return of his chronometers from India, your oil in excellent condition, and deemed it far superior in quality, for such purposes, to any he had before been able to procure.

"I have presented one of the last phials, which you favoured me with, to Mr. Vulliamy of Pall Mall, who purposes to give it a trial; but I hope you will be induced, by what has been already ascertained, to make your discovery known.

"I am, * * *

"P. P. Barraud."

Pure oil, such as I have at different times sent to Mr. Barraud, may be obtained by attending to the following directions.

Put a quantity of the best olive oil into a phial with two or three times as much water, so that the phial may be about half full. Shake the phial briskly for a little time, turn the cork downwards, and let most part of the water flow out between the side of the cork and the neck of the phial. Thus the oil must be washed five or six times.—After the last quantity of water has been drawn off, what remains is a mixture of water, oil, and mucilage. To separate these from each other, put the phial into hot water for three or four minutes, and most part of the water will fall to the bottom, which must be drawn off as before.

The oil must then be poured into a smaller phial, which, being nearly full, must be well corked, set in a cool place, and suffered to stand undisturbed for three or four months, or until all the water shall have subsided, with the mucilage on the top of it, and the oil, perfectly transparent, swimming upon the top of the mucilage. When *time* has thus completed the operation, the pure oil must be poured off into very small phials, and kept in a cool place, well corked, to preserve it from the air.

Lynn, Nov. 13, 1810. E. WALKER.

LXVIII. *A further Set of Fifteen Corollaries, to the* Musical Theorems *in Page* 39, *by means of which, the Temperaments of any one of the Concords being given, all the other Temperaments and all the Wolves can be calculated with the greatest facility. By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR, THE six musical *Theorems* which you did me the favour to print in your 147th Number having given much satisfaction to several of my musical acquaintances, I am induced to send you 15 other *Corollaries* in addition to the 12 inserted at page 44: they are naturally divisible into three classes, viz.

1st. When the temperament of the *fifth* is given.

Corollary 13. The temperament of the IIId is $= 11\Sigma + \text{m} - 4 \times$ temperament of the Vth.

By theorem 3, $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}\text{m}$ is the temperament of the IIId, and $\frac{4r}{s}\Sigma + \frac{4t}{u}\text{m}$ is 4 times the temperament of V, by theorem 1: the sum of these is $11\Sigma + \text{m}$, whence the truth of the corollary is manifest: as indeed it is, by merely abbrevating and ordering theorem 3.

Corollary 14. $11\Sigma + \text{m} - 3 \times$ temperament of V $=$ temperament of VI.

Here $\frac{11s}{s}\Sigma + \frac{u}{u}\text{m} - \frac{3r}{s}\Sigma - \frac{3t}{u}\text{m} = \frac{11s-3r}{s}\Sigma + \frac{u-3t}{u}\text{m}$, as in theorem 6;—and so of all the following.

Corollary 15. $11 \times$ temperament of V $- 12\Sigma - \text{m}$ $=$ Vth wolf.

Corollary

Corollary 16. 8 × temperament of $V - \Sigma$ = IIId wolf.

Corollary 17. 9 × temperament of $V - \Sigma$ = VIth wolf.

2d. When the temperament of the *major third* is given.

Corollary 18. $\frac{1}{4}$ × temperament of III $- 2\frac{3}{4}\Sigma - \frac{1}{4}m$ = temperament of V.

Corollary 19. $\frac{5}{4}$ × temperament of III $+ 2\frac{3}{4}\Sigma + \frac{1}{4}m$ = temperament of VI.

Corollary 20. $18\frac{1}{4}\Sigma + 1\frac{3}{4}m - \frac{11}{4}$ × temperament of III = Vth wolf.

Corollary 21. $21\Sigma + 2m - 2$ × temperament of III = IIId wolf.

Corollary 22. $23\frac{3}{4}\Sigma + 2\frac{1}{4}m - \frac{9}{4}$ × temperament of III = VIth wolf.

3d. When the temperament of the *major sixth* is given.

Corollary 23. $3\frac{2}{3}\Sigma + \frac{1}{3}m - \frac{1}{3}$ × temperament of VI = temperament of V.

Corollary 24. $\frac{4}{3}$ × temperament of VI $- 3\frac{2}{3}\Sigma - \frac{1}{3}m$ = temperament of III.

Corollary 25. $\frac{11}{3}$ × temperament of VI $- 28\frac{1}{3}\Sigma - 2\frac{2}{3}m$ = Vth wolf.

Corollary 26. $28\frac{1}{3}\Sigma + 2\frac{2}{3}m - \frac{8}{3}$ × temperament of VI = IIId wolf.

Corollary 27. $32\Sigma + 3m - 3$ × temperament of VI = VIth wolf.

In order to prevent mistakes in the use of Corollary 9, page 43, it may be proper to remark, that the expression there given, is for the *diesis* between ✻C and ♭D, ✻F and ♭G, and ✻G and ♭A, besides those enumerated; but not between ✻E and ♭F or ♭C and ✻C, where the half tones fall, which have a different value, for which I will give a theorem on some future occasion.

Also page 45, line 5, after "Σ only," add,—see the other equalities of wolves in Scholia 3 and 4.

In Scholium 6, after $\frac{1}{11}$c, $\frac{7}{11}$c, and $\frac{8}{11}$c, add, in each case, —*very nearly*.

At the end of Scholium 11, page 52, add,—The *sharp* and *flat* are here, by Cor. 10, each equal to $36\Sigma + f + 3m$, as in Mr. Marsh's Theory of Harmonics, p. 16.

I am, sir,

Your obedient humble servant,

Westminster, Nov. 15, 1810. JOHN FAREY.

LXIX. *On the Barometer.* *By* RICHARD WALKER, *Esq.*

To Mr. Tilloch.

SIR, IN consequence of its having been intimated to me, that a short rationale, or general view of the various changes in the weather, and the indications of the barometer, as connected with them, might not be unacceptable, I beg leave to transmit the following, which may be considered as an appendage to the paper "On the Application of the Barometer for indicating the Weather, &c." you did me the favour to insert in your last Number.

Water exists in the atmosphere in two different states, viz. 1st, in a state of *chemical combination;* that is, so completely incorporated with the air, as to form with it one homogeneous transparent fluid;—and, 2dly, in a state of *mechanical combination;* which is, when the minute particles of water are merely suspended in the air, forming that state of the atmosphere, which is denominated cloudy or misty.

The dense state of the air being fittest for the *chemical combination* above mentioned; clear, dry weather, generally speaking, accompanies the higher degrees of the mercury in the barometer, whilst, a rare state of the air being less capable of receiving the water into chemical combination, it is then merely suspended in a state of *mechanical combination*, forming clouds, mists, &c.

Hence it follows, that, when the mercury stands at or near FAIR, clear dry weather is indicated *generally;* and when at or near RAIN, cloudy or wet weather; and when fluctuating mid-way, changeable weather.

It occasionally happens, however, that the atmosphere is cloudy, and even wet, whilst the barometer is as high as FAIR; and clear and dry, whilst the barometer stands as low as RAIN. The reason of this, in the first instance, is, that the air, having become replete or over-loaded with water, is incapable (by an alteration of temperature, viz. the air and its contents having become colder) of retaining or suspending it in a state of *chemical combination;* and in the latter case, which happens after rain, succeeding a continued dry state of the atmosphere, which having swept down the vapour with it in its descent; the air, though then in a rarer state, is yet sufficient to retain the proportion of water, now much reduced in quantity, in a state of chemical combination.

The

The particular or more immediate indication of the weather which is coming, arises from the alteration which is *taking place* in the density of the atmosphere, and which the barometer exhibits by the *rising or sinking state* of the mercury; the weather becoming comparatively *clearer* as the atmosphere is becoming denser, and *duller* as the atmosphere is becoming rarer *.

Hence, if the barometer were as portable and as convenient for reference as a watch, we should seldom be at a loss to know, at least for short intervals, what kind of weather was coming †.

The ordinary range of the barometer in this climate is from RAIN to FAIR; rising however, occasionally, as high as SETTLED FAIR; and sometimes, though very rarely, as high as VERY DRY: and sinking, occasionally, as low as MUCH RAIN; and sometimes, though very rarely, as low as STORMY.

It is scarcely necessary to observe that north and east winds, in consequence of passing to us from a colder climate, and over land, bring a denser, colder, and dryer atmosphere; and south and west winds, coming to us from a warmer climate, and over the sea, bring a rarer, warmer, and damper atmosphere; and moreover, that the capacity of air for retaining water in a state of chemical combination is *increased* by coming from a colder to a warmer temperature; and *diminished*, by coming from a warmer to a colder temperature.

It must be equally apparent, that the greater or less elevation of the clouds depends upon their own degree of density, and that of the atmosphere which supports them.

With regard to the immediate causes of the direction and changes of the wind in this climate, I consider them as involved in too much obscurity and uncertainty to say any thing satisfactorily about them; and with respect to electricity, which though doubtless a powerful agent in meteorological effects, I consider it rather as a matter of curious speculation than of practical utility.

I have therefore only to add, that by a due consideration of the causes enumerated above, connected with the more

* The difference that might be supposed to arise in the height of the barometer from the effects of different degrees of heat on the atmosphere, may in observations of this nature be entirely disregarded, these effects being very nearly equalized by the expansion and contraction of the mercury in the barometer, from the same cause.

† As the atmosphere is almost constantly varying in its degree of density; so is the barometer, which is an accurate measure of its density, as constantly varying in its altitude, and should therefore be frequently referred to.

obvious

obvious effects of the sun's varying influence in raising and dispelling vapours, we may, I think, account pretty satisfactorily for the various vicissitudes of weather, which mark the different seasons throughout the year; and, by the relation of the barometer to those causes, be enabled to foresee, with a considerably greater degree of certainty than is commonly supposed, the different changes of weather which are, at all times, about to take place.

I am, sir,

Your obedient servant,

Queen-street, Oxford,
Nov. 17th, 1810.

RD. WALKER.

LXX. *Reflections on some Mineralogical Systems. By* R. CHENEVIX, *Esq. F.R.S. and M.R.I.A., &c. Translated entire from the French, with Notes by the Translator.*

[Continued from p. 303.]

SUPPOSED DEFECTS OF THE CRYSTALLOGRAPHICAL SYSTEM.

M. HAUY has been reproached for his principle of specification and his definition of the species: one alleging that, according to him, muriated soda and sulphated lead are of the same species; another, that the two indications of the species are often in contradiction, and that the same form of molecule does not always accompany the same chemical composition, and *vice versâ*. Some do not like the octaëdron for a primitive form, because, to preserve unity of form in the integral molecule, it is necessary to suppose empty spaces; others, in short, reproach him with the difficulty of finding all the directions of the cleavage (or the construction of the cleft), and the system also with a want of generalization; and finally, as a dernier resource, that we must return to the system of external characters.

Will it be believed that I have heard the first of these objections made by a celebrated philosopher, a professor who draws around him from all parts of the world the zealous lovers of mineralogy, and who repeats it at least once a year in his public lectures? that I have seen it printed and published in a work which passes for one of the best on the systems of this professor? It is of German origin, and proceeds, no doubt, from the circumstance that they have not yet learned to count even to two on their fingers; a great misfortune for a philosopher. Identity of form in the integral molecule,—this is one condition in order that two minerals

minerals should be of the same species; and hitherto muriated soda and sulphated lead are in this state. Identity of chemical composition,—here are two conditions, if I have rightly calculated; and muriated soda and sulphated lead are not of the same species.

M. Haüy himself has answered the second objection in a manner which leaves nothing to be added. It is true, that with the same form of integral molecule we have a different chemical composition; but let us observe under what circumstances.

There are three geometrical figures which perform the office of integral molecule: Admirable simplicity of nature, that with such slender means can compose forms in an infinite number! These figures consist of those with four sides, the least number possible to contain a solid; those with five, and those with six; all are the most simple. But they are all susceptible of an infinite variety in the dimensions of their sides and in the inclination of the faces which terminate them, although all have a fixed term of regularity towards which they tend. It is but in these terms, which in this respect are the limits, that we find the identity of the physical with a diversity in the chemical molecule. The repetition of regular forms which are the limits of others, such as the cube and regular tetraëdron in the different species, appears to me to prove nothing against the system of M. Haüy; on the contrary, it gives us cause to admire the mechanism of nature, which delights in multiplying its severe and rigorous features amidst the variety in which it indulges. The most regular forms are also the most simple; and I take it for an axiom, that it is but in approaching simplicity that we approach nature.

Let us examine if the reverse of this be equally true; that is to say, if with the same chemical composition we find a difference in the physical molecule. There is only one case well ascertained; it is that of carbonated lime and arragonite. Chemists the most celebrated in the art of analysis, Messrs. Vauquelin, Thenard, and Klaproth, have found no difference between these two minerals; and I acknowledge that I have repeated the analysis of six different specimens of arragonite, comparatively with the carbonated lime, and I am convinced of their identity of composition. M. Haüy found a difference in the form of their integral molecules. How admirable is this science, to which,—in the first years of its existence, and before that time has impressed it with those marks of rigour which continued observations invariably effect,—only one objection can be made against it that

that appears to have any foundation! How many do we not find against those sciences which have been refined by the lapse of ages, and which have resisted the persecutions of inquisitors armed with all the severity of bad intention? If the cavils of certain persons had been listened to against the axioms and definitions in mathematics, we should now, indeed, have been destitute of this route. Others would have deprived us of physics and chemistry, at the same time with the very matter which served them as an object, and, scarcely will it be believed, our own existence. If we make a thousand steps in advance, and one remains which we are unable to pass, should we for this abandon that which we have already attained? The philosopher ought at least to wait, and watch with a calm eye and unshaken patience the moment when nature shall betray itself, if our efforts can effect nothing on it. The state of science is a state of expectation.

Even when we have a rigorous demonstration that chemistry and mineralogy do not correspond in this solitary case, what then shall we say? In every thing which nature presents to our contemplation, it leads us, by views taken in all directions, to the point where we find ourselves arrested. In the mass of our learning, what system comprehends it entirely? yet, notwithstanding their imperfections, systems still serve us. No one thing appears better determined than the species in zoology, as it consists in a categorical answer to a very simple question. There are, however, animals respecting which it is still disputed whether they should be admitted as species or varieties. The vegetable kingdom has also its causes of uncertainty. Nevertheless, these two kingdoms offer a greater number of characters, as the beings which they embrace are endowed with more marked and more elevated qualities than those which belong to the mineral kingdom. Why then should more rigour be required of the latter, with less means? Why is it wished to deprive us, on a single deposition against it, of a system which is supported on mathematics and confirmed by chemistry?

Ferriferous carbonated lime * is a mineral often cited by those

* Brown spar of Jameson, sidero calcite of Kirwan, *chaux carbonatée brunissante* of Brogniart, who makes it the ninth subspecies of carbonated lime (or calcareous spar), and separates it from what he calls *fer spathique*, or sparry iron, which he arranges among the metals, on account of its superior specific gravity, and its power of occasionally attracting the magnet. The property of becoming brown when exposed to nitric acid or the fire, he thinks, authorizes the above name; but this is common to both kinds. This

those who profess another mineralogical belief than that of M. Haüy. It is ranked in the same species as simple carbonated lime, although it often contains but one-third carbonate of lime, and the remainder iron or manganese: evident marks also indicate an additional structure of the cleavage in the direction of the great diagonal, which leads to solids of two or more forms.

We have observed that there exist vacuums between the molecules of bodies, in which foreign matter may lodge. It may therefore happen that particles of iron or oxided manganese are deposited in the vacuums of carbonated lime: when this is the case, we have the mineral in question. But it does not thence follow that the integral molecule should change its form, as the oxided iron does not enter it, and as it, in fact, undergoes no change in its chemical composition. One of the indispensable properties of the molecule of minerals being not to vary but by two indications at once, then whatever enters not the integral molecule, although it forms a part of the mineral, should not change it: this law is constant, whether it relates to the quantity of extraneous substances, or to their nature, their form, or their tendency to crystallization. But as foreign matter can interpose itself between the molecules with which it is surrounded or enveloped, it may influence their relative dispositions; whence will result secondary forms, which shall differ according as the spaces between the molecules are empty, or more or less filled. The passage of light may also be obstructed by the interposition of opaque matter, and the colour must participate in that of the interposing substances. The molecules being enveloped may be further removed from immediate contact, and thus offer a greater facility of separation: the junctures also may be

This supposed improvement, therefore, of Haüy's arrangement, by subdividing and transposing the varieties of this subspecies, exists more in names than characters; for even those of specific gravity and of attracting the magnet depend solely on the preponderance of iron over the manganese; and sometimes the quantity of manganese is very considerably greater than that of the iron, without any difference in exterior character. To change, torture, or reject a system for such a trifling and merely apparent anomaly, would betray more ignorance of the diversity of nature than scientific experience; to attempt to modulate an otherwise complete system to such things, would be to pay more attention to exceptions than to rules, and evince nothing but the foppery of minuteness, which never can exist in minds expanded by true science. Mr. Chenevix's chemical explanation, indeed, of this phænomenon is perfectly sufficient, and more satisfactory than M. Brogniart's scheme of minute division, which may be sometimes very right and sometimes very wrong. His epithet *brunissante*, or browning, adopted from the Germans, is equally as applicable to his sparry iron as to this carbonated lime; that of Haüy, although long, is correct and intelligible,—ferro-manganesiferous carbonated lime.—Trans.

be more distinct, and the specific gravity greatly augmented, as the spaces otherwise empty or occupied with air shall be filled with heavier matter. The action of chemical menstrua may likewise be modified; and if a molecule of a substance easily soluble is surrounded with molecules of another which resists chemical solvents much longer, the former may be in some measure protected from their power. Other effects may take place; but, we repeat it, the integral molecule remains the same. Now, in the ferriferous carbonated lime we do not find the same variety of forms as in the simple carbonate; the former is opaque, the latter is transparent; the one has lines which seem to mark a direction of the cleavage more than in the other; the specific gravity of the first is 3·784, that of the second is 2·718; and the ferriferous carbonate is more easily dissolved in acids. These are all the phænomena which result from the interposition of extraneous substances in the empty spaces between the molecules of one of the bodies exhibited in this example.

EXAMINATION OF THE IDENTITY AND UNIFORMITY OF THE INTEGRAL MOLECULE.—OBJECTIONS ANSWERED.

But I shall be told that the molecule differs, since there is a direction of the cleavage more in the one than in the other. It is the foreign substances which render the junctures more sensible in one case than in another; and it is possible that the same junctures exist in the simple as well as in the ferriferous carbonate, without our having as yet commonly observed them. I can almost venture to assert that they do exist. I have a specimen of carbonated lime very white and very well crystallized in an obtuse rhomboid of a primitive form, and of the same specific gravity as the simple ordinary carbonate, but of a milky transparence, and in which the lines are as distinct as in any specimen whatever of ferriferous carbonated lime. This specimen, nevertheless, presents in analysis no trace of any substance foreign to the purest Iceland spar. It is therefore very probable that simple as well as ferriferous carbonate may be divided in the direction of the great diagonal: hence it is expected to overturn the system of M. Haüy. But in all that we know, and all that we seek to know, it is uniformly our own horizon which we substitute for the limits of nature. M. Haüy has found in carbonated lime three directions of the cleavage parallel to the faces of an obtuse rhomboid, and no more. He stopped there, and has not wandered in the regions of imagination. Although his work

work was finished, that of nature was not; and it may still present some specimen which shall reveal its secret. If we find that carbonated lime may be divided more than once, it thence results only that we have taken for an integral molecule that which we have been able to observe, but not that which really exists; precisely as chemistry declared that the emerald was composed of silica, alumine, iron and lime, until it was discovered that what was considered only as alumine contained also glucine, and that the iron was combined with chromium. But for this chemistry did not lose its importance. The integral molecule likewise may be found different from what has been believed, if what I have here observed should prove true; yet the general system of the molecule is unshaken.

A recent analysis, however, appears to have excited much interest in this point. Some of this pretended ferriferous carbonated lime has been found, in which there exists scarcely a trace of lime*. I readily believe it, but shall not for that renounce the method. In M. Haüy's collection there is a mineral which at one end is ferriferous carbonated lime, excessively yellow, containing iron, striated and dividing in rhombs. In extending from this end the colour fades, and the other characters which distinguish the common from the ferriferous carbonated lime become weaker till they finally disappear. Whoever ex-

* The Wernerians, however, cannot consistently avail themselves of this defect, as the mineral still retains the same *external* characters; it is the chemists only who are entitled or qualified to decide on it, and they will not be very precipitate in pronouncing a sentence, since Mr. Davy has proved that even one per cent. of oxygen can produce effects on the external character of substances, which would serve the Wernerians not merely for a specific but even a generic difference. Should they object to the introduction of lime in the name of this mineral, they must recollect their own *holzstein*, woodstone, or petrified wood, which they have thus denominated, and made a particular species in flint genus, although they will not pretend that it contains any vegetable or woody matter; only that, like the mineral under consideration, it owes its form to that substance. Mr. Jameson, indeed, makes an apology for considering a petrification (not petrefaction, as he erroneously writes it, and which the learned Dr. Kidd applies to *incrustations*), "a particular fossil species," by alleging "that woodstone differing in its external characters from all other fossils, the justness of the Wernerian method is evident." Upon this principle he should have divided his species, as the colours, and even specific gravity of petrified oak, ash, &c. are very different. He adds, that "it receives a good polish, and serves for the same purposes as agate." I have examined many specimens of petrified wood in various countries, but have never been so fortunate as to find any that could be substituted for agate, or was susceptible of a polish even equal to coarse marble. Surely the professor cannot have noticed such characters merely to make Werner's fine chemical theory of the solution and infiltration of agate less fanciful, or give an example of transition from wood to "petrifaction agate, which is *wood* penetrated with *several* of the fossils that constitute agate?"—Trans.

amines

amines this specimen attentively and unprejudicedly, must believe that the whole has been common carbonated lime, which has been exposed to the action of a sulphuric or other solution of iron, that the carbonate of lime has been gradually dissolved, and yielded its place to the oxide of iron, precipitated from its solvent by the double affinity of the lime for the acid in the solution of iron, and the oxide of this metal for the carbonic acid. The carbonated lime has served as a matrix to the oxide of iron, and it is very possible that the direction of the laminæ in the way of the great diagonals may be rendered more sensible by the solution of the old and precipitation of the new molecules which came to deposit themselves in this abode. If, however, I have admitted the existence of a ferriferous carbonated lime, where there is no lime, it is because I have seen petrified wood where there was no wood.

But, in taking the first explanation generally, it seems that, far from making the case of which I have spoken the subject of an objection, it is for chemistry to draw from it the greatest possible advantage. A grand effort which yet remains to be made, is to distinguish between mixtures and combinations. Let us suppose a mass, A, composed of any number of physical elements, which have for chemical molecules the substances $a+b+c$, and another mass, B, the chemical elements of which may be $m+n$. It is demanded if a mixture or a combination shall take place in putting the bodies A and B together? If by physical division we find a molecule similar to the molecule A, another similar to that of B, and that by chemical division we found the chemical elements $(a+b+c+m+n,)$ it is evident that we have a mixture. But if we find a new molecule, C, with the same chemical result $(a+b+c+m+n)$, we have a combination. It is therefore for chemistry, enlightened by physical (or mechanical) division, to resolve this great problem; and it is to restrain its influence too much to confine within the province of mineralogy, that which may guide our researches over the whole of nature or the material world.

Let us take one particular case,—the neutral tartrate of potash. This salt, like all others, has a physical and chemical constitution peculiar to itself. If, in adding to it tartaric acid in a quantity sufficient to convert it into acid tartrate of potash, and in submitting these two substances to all the chemical means of combining them, we find the same form of physical molecule in the one part, and the form of the physical molecule of tartaric acid in the other; we we can safely say that no combination has taken place between these two substances, whatever may be the difficulty experienced in separating them. If, on the contrary, these two molecules lose the form peculiar to them, as tartrate of potash and tartaric acid, to effect another conjointly, we should conclude that they have combined to form but one. By afterwards adding some soda as a neutralizer, we may learn the true state of these principles, with respect to each other, in consulting crystallotomy to know if molecules of tartrate of potash and also those of tartrate of soda are found, or molecules of another form, which will be that of a tartrate with a double base.

The great quantity of foreign matter which has been found interposed between the physical molecules, without effecting any change in their form, has shaken the faith of many persons. The *gres* (sandstone) of Fontainbleau (quartziferous carbonated lime, Haüy), in which the law is the same as in the pure calcareous spar which sometimes accompanies it, is an example. But the degree of tendency to a regular form may be much greater in one mineral than in another, and surmount all the obstacles which the mixture of heterogeneous species could oppose to it. This is what we see in our laboratories; it is what the *gres* of Fontainbleau shows us in that of nature. The power of becoming symmetrical may vary in crystallizable substances, like the capacity of saturation in salifiable bases.

As to the octaëdron considered as a primitive form, it has been observed that sections by planes parallel to its faces, divide it again into eight tetraëdrons and six octaëdrons, and this in succession to the ultimate point; so that, to maintain the unity of the integral molecule, we must suppress in the mind one of these two figures. In order to adhere to the most simple, as that which appears to have the most just title to a place in nature, the preference has been given to the tetraëdron, in supposing that between the molecules there are empty octaëdral spaces. Geometry has found that this hypothesis embraces one third of the quantity of matter in a mineral which has this primitive figure, and that the two other thirds are empty. But this takes place precisely in the species of the calcareous genus which has the greatest specific gravity, while nothing is said of the vacuums in the other species. Here, it is said, there is a contradiction.

If we were still ignorant of all that we have learned respecting the density of bodies, this objection would be some-

somewhat specious. In the present age natural philosophers cannot admit it.

Specific gravity may be viewed in two points:—where the molecules of all the bodies have the same density, and then the variation of specific gravity between one mass and another depends on their particular arrangement with respect to each other, which admits of more or less empty spaces in the different bodies in nature; or where the molecules themselves have a different density in each substance, the relative disposition being the same in all. It is evident that, on the first supposition, the molecule and the body which it contributed to form would have properties different in this respect, and that we could never learn any thing of the true specific gravity of the molecule, however correctly we knew that of the body. Platina, for example, which I estimate at nearly 21·437, might have a molecule ten times (100 times if it is wished) more dense than it; and the lightness of the aggregate would result from the space, in which 214370 molecules of platina might lodge, if the arrangement was the most favourable possible for this effect, containing only 21437. The specific gravity of cork has been estimated at 0·24. A space therefore which contains 21437 molecules of platina could contain but 240 of cork, in this hypothesis; yet a molecule of platina loses in water the same quantity of its weight as a molecule of cork.

In the second supposition we discover the weight of the molecule having that of the mass; and the number of molecules in a given volume of platina would be equal to that which the same volume of cork would contain.

There exists, indeed, a third hypothesis, composed of these two, and which supposes at the same time a variation in the density of the molecules and in their arrangement. I shall not discuss what hypothesis should be preferred in sound philosophy. I speak at present like those mineralogists who judge all by their senses, and I admit every thing which they wish, in order to refute all.

In the hypothesis where the density of the molecules is supposed variable, it is evident that the objection falls of itself; for I am able to give those molecules whatever density I choose, within reasonable limits. Now when in fluated lime there would be the two thirds vacant which form the subject of reproach, the density of the molecule would be but three times greater than that of the mass. Yet the density of a molecule of gold would be six times greater than in this supposition. Thus, by admitting

mitting in the molecule of fluated lime the necessary density to obviate this objection, we do not commit any indiscretion.

Now let us take the contrary hypothesis, and say that the molecules of all bodies have the same density, so that the variation of specific gravity in them all depends solely on the vacuums which exist between them. If in any body whatever the quantity of vacuum be zero, we have an absolute plenum; and the heaviest body which we know should be that which would present us with this state of things. Platina furnishes us with an example, while this collection of hypotheses contains the fact most favourable to the objection.

Chabaneau has found the specific gravity of platina to be 2·400, others 2·300, some 2·200, and I have observed it beyond 2·1; that of fluated lime is 3·191. Let us simplify the expression of these relative weights, and we shall have, in an equal volume, the quantity of matter in platina, in fluated lime, and in water :: 1 : $\frac{1}{7}$: $\frac{1}{21}$. There is then $\frac{6}{7}$ of vacuum in fluated lime, or $\frac{18}{21}$, while the hypothesis of M. Haüy notices only $\frac{14}{21}$. There remain, then, $\frac{4}{21}$ more than are wanting to combat the objection. There are also necessarily vacuums in other calcareous species. In taking platina, as above, for an example of an absolute plenum, we shall have, according to the data which the specific gravity of this body furnishes, $\frac{185}{210}$ of vacuum in carbonated lime; $\frac{180}{210}$ in phosphated as well as in fluated, and $\frac{187}{210}$ in sulphated lime. The hypothesis of M. Haüy, instead of being incompatible with what observation teaches respecting the density of fluated lime, is not sufficient to account for its lightness; and the $\frac{40}{210}$ of which it takes no notice, as well as the vacuums in other species which are passed over in silence, are so many particular cases of a verity demonstrated in physics: its great notoriety doubtless made M. Haüy believe that it was unnecessary to anticipate this objection by answering it at first. Instead, therefore, of accusing him of having carried off too much matter from fluated lime as a mineralogist, we should rather consult him as an examiner of nature (*physicien*), on the fate of nearly $\frac{10}{38}$ of which he is silent. It is difficult to conceive how any one could permit himself to make such objections, especially when we *consider the source whence they sprung*.

The divisibility of carbonated lime before mentioned, by supersections which yield molecules of two different forms, and also what we have just observed respecting the

 octaëdron

octaëdron as a primitive figure, give occasion for some reflections which I shall here venture, although I feel by their importance, and by the considerations necessary to give them due illustration and support, that these points merit being treated separately and at greater length. It is possible that there had been but one single form of molecule in all nature, and that this form was a tetraëdron. In the octaëdron the existence of this figure is inevitable, since it results from sections parallel even to the faces of the octaëdron. It also occurs, and simultaneously with other figures, from supersections made in the direction of the diagonals of all the faces of a parallelopiped. The triangular prism likewise affords it, but of different forms, by supersections in the direction of the diagonals of the lateral faces. As we must necessarily allow of vacuums between the molecules of bodies, we may suppose that the interstices are those portions of spaces, from which every other figure (except * the tetraëdral molecule of the body,) would have disappeared. The form and the quantity of these interstices, conjointly with the presence of a greater number of the proper molecules of the body, will produce all the diversities of specific gravity which are known in nature; and when we consider that a portion of space cannot be inclosed by less than four planes, we observe in the tetraëdron that mark of simplicity which nature impresses on all her works.

"All the molecules in nature are spherical," said a most celebrated German, in showing me that with small balls of ivory he produced all the figures which he wished. "The English and the French have not yet advanced so far," he added. "From reason," said I to myself. The probability that nature would have given the preference to such a solid rather than to another for an universal molecule, every thing otherwise being equal, would be inversely as the number of planes which terminate them. Between the sphere and the tetraëdron it would be as four to infinity.

Besides its being hitherto impossible to extend the system of the integral molecule to all minerals, there are naturalists who reproach it with the difficulty of finding the directions of the cleavage in many cases, the trouble of calculating them, &c. We should no longer use the microscope, the

* This exception enclosed in ( ) appears superfluous; the author is speaking of the presumed vacant spaces or vacuums between the molecules; but neither the figure nor the forms of these vacuums interest the practical crystallographer, as tangible solids are quite sufficient to establish the validity of the general principle.—Trans.

telescope,

telescope, nor the chronometer, for they also are very difficult to execute. Let us content ourselves with dressing, sleeping, and eating, convinced that without the pendulum and the telescope the stars will continue their course, and bring back the hours of sleep and the restoration of our powers.

The last objection to which I shall pay any attention is that which says, briefly, "We must abandon the system of M. Haüy for that of the external characters, as the integral molecule cannot be observed in all minerals." One of the great advantages of the system of M. Haüy, one of its principal beauties, is to follow nature, and to speak as she does. Where she has finished her work in the highest manner of which it is susceptible, M. Haüy does the same; and if she produces a mineral endowed with all the characters which, according to us, compose the most perfect state, it is classed and defined as such. If she has been sometimes less rigorous in impressing her mark of perfection, the system follows the same course; while the method of external characters renders equally the honours of rigorous classification to sapphire and to the alumina of Halle. To say that we should make no use of an excellent system because cases occur where it is unavailable, is to say to a patient, Lie not on a feather bed; for, if you are deprived of it, you will be reduced to the necessity of sleeping on a board. It is to tell a man in health not to take nourishment, for if the provisions become deficient he could no longer eat.

WERNERIAN TRANSITION OR PASSAGE.

Before terminating these considerations on the species, there is a mineralogical being of which it is necessary to say a few words. It is a being which is neither of this nor that species, but belongs to all. It is not sapphire, for instance, but it resembles it; it is not ruby, but it would be perhaps if it were not something else. It is so constituted that, with a real and material existence, it lives by borrowing, as to its modifications, and puts on the characters of others. It is a hermaphrodite mineral; an infant with two fathers that both disown it; that the other kingdoms of nature reject as a monster; but that mild and easy mineralogy has received into its bosom, and called it *passage*.

There are two manners of conceiving the existence of this interesting refuse of the organized kingdoms.

Let us first suppose a mineral less hard, less brilliant, less blue and lighter than sapphire; but harder, more brilliant, bluer and heavier than alumina of Halle. It will represent to us what should be understood by a passage from alumina of Halle to sapphire. It is in this manner that they have given a very great number of passages; too great, indeed, to cite them. Let us take some analogous cases in another kingdom.

A great naturalist has told us that the paw of a bat brings it near to man; and that every one may see the organ of flight, which merits it a place among birds. Here then is a *passage* from a bird to man. But what is meant by that? Is it understood that nature, having succeeded in making a bird, conceived the project of forming a more perfect being, but that her first essay produced nothing better than the image of a hand and a horrible grimace? Or, is it pretended that, if in the metempsychosis, the lord of the earth should become a bird, he must pass under the form of this hideous animal?

Of all that we can consider as *passage*, there is nothing so marked as an animal engendered between two individuals of different species. Such is the mule; every member of which participates in the qualities of one or other of its parents. In the capital of the beautiful kingdom of Valencia I learned the following facts from eye-witnesses. A silk weaver kept a stallion and a mule in the same stable. One night in winter the mule was taken ill, rolled on the ground, and appeared ready to die. At last it brought forth a foal, so well formed that the finest mare could not have produced a better. The stallion and mule were left together during eight years, in which time the latter brought forth five male and two female foals*. Now the mule was half horse half ass; its offspring were half horse half mule. But, will it be said that the latter, which were perfect horses, contained a portion of the ass, which portion of ass might have passed by the mule to become horse? Most assuredly no sensible person will say so. Nature has not instituted the mule species; and when in successive generations all traces of the ass are effaced in the foals of the mule, it

* As this fact has been questioned by some French theorists, from the forced and miniature experiments of Buffon, it is not foreign to the present subject to say that the writer of this note has also heard it from unimpeachable eye-witnesses who were well acquainted with the whole circumstances, and that he knew a gentleman, an amateur mineralogist in Valencia, who found one of the offspring of the mule the most serviceable horse that he ever possessed.—Trans.

it is Nature which resumes her rights, and puts a limit to a race of monsters.

The other circumstance which gave birth to the brilliant idea of *passage*, is that which takes place when a mineral is an aggregate of two simple species: such are the heliotrope, composed of quartz and green earth; and prase, composed of quartz and green schorl (*rayonnante*) [actinolite of Kirwan and Jameson; amphibole* actinote of Haüy and Brogniart]. It is at first a very great and irreparable error to consider mixtures as species, the essence of which is simplicity.

[To be continued.]

LXXI. *Notices respecting New Books.*

The Philosophical Transactions for the Year 1810, Part II, has just made its appearance. Its contents are:—Supplement to the First and Second Part of the Paper of Experiments for investigating the Cause of Coloured Concentric Rings between Object Glasses, and other Appearances of a similar Nature. By William Herschel, LL.D.F.R.S.—On the Parts of Trees primarily impaired by Age. In a Letter from T. A. Knight, Esq. F.R.S. to the Rt. Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—On the Gizzards of Grazing Birds. By Everard Home, Esq. F.R.S.—Observations on atmospherical Refraction as it affects astronomical Observations; in a Letter from S. Groombridge, Esq. to the Rev. Nevil Maskelyne, D.D. F.R.S. Astronomer

* The union of amphibole (hornblende) with actinote (actynolite) is another fact highly honourable to the study of crystallography. Haüy announced his opinion of their identity, which was fully confirmed by M. Laugier's analysis in the *An. d'Hist. Nat.* vol. v. p. 73.

Amphibole	*contains*	*Actinote*
Silica	42·02	50·00
Oxide of iron	22·69	11·00
Magnesia	10·90	19·25
Lime	9·80	9·75
Alumina	7·69	0·75
Oxide of manganese	1·15	0·50
Chromium	0·00	3·00
Potash	0·00	0·50
Water and loss	5·75	5.25
	100·00	100·00

The object of this analysis being to compare the nature and proportions of the constituent principles of amphibole and actinote, between which crystallography had found a perfect analogy, the comparative result was such, that it appeared necessary to blend them together under the same species of mineral, the latter presenting only some new varieties of colour of the former.—Trans.

 Royal.

Royal. Communicated by the Astronomer Royal.—Extract of a Letter from the Rev. John Brinkley, D.D. F.R.S. Andrews Professor of Astronomy in the University of Dublin, to the Rev. Nevil Maskelyne, D.D. F.R.S. Astronomer Royal, on the annual Parallax of α Lyræ.—On the Mode of Breeding of the Oviviviparous Shark, and on the Aëration of the fœtal Blood in different Classes of Animals. By Everard Home, Esq F.R.S.—On Cystic Oxide, a new Species of Urinary Calculus. By William Hyde Wollaston, M.D. Sec. R.S.—Researches on the oxymuriatic Acid, its Nature and Combinations; and on the Elements of the muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution. By H. Davy, Esq. Sec. R.S. Prof. Chem. R.I. F.R.S.E.—Observations upon Luminous Animals. By J. Macartney, Esq. Communicated by Everard Home, Esq. F.R.S.—Observations and Experiments on Pus. By Geo. Pearson, M.D. F.R.S.—Presents received by the Royal Society from November 1809 to July 1810.

GEOGRAPHY.

Mr. Myers, of the Royal Military Academy, will shortly complete an Introduction to Historical, Physical, and Political Geography; accompanied with Maps, and adapted to the higher Classes of Pupils, under both public and private Tuition. Mr. M.'s inducement to the undertaking, and his guide in its accomplishment, has been utility; and to attain this object he has condensed into one moderate-sized Octavo Volume the most valuable matter of more extensive Systems. In the construction of the Maps, particular attention is paid to *simplicity*, *perspicuity*, and *accuracy*; and it is presumed that these qualities, so essential in every elementary Treatise, will be found to prevail, in a superior degree, throughout the whole performance.

LXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On the 22d of Nov. Dr. Wollaston read a paper "On some of the Combinations of oxymuriatic Gas and Oxygen, and on the Chemical Relations of these Principles to inflammable Bodies." By H. Davy, Esq. Sec. R.S.

In this paper Mr. Davy details a great number of experiments which he has made on the combinations of oxymuriatic gas and oxygen with the metals of the fixed alkalies, the metals of the earths, and the common metals; with a view

view to illustrate the nature, properties, and combinations of oxymuriatic gas, and its relations to inflammable bodies, as compared with those of oxygen. He also offers some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they combine. And lastly, he concludes his paper by some observations on the impropriety of the present nomenclature, in reference to the oxymuriatic gas and its combinations; and proposes some concise modes of distinguishing these novel bodies.

Mr. Davy made some previous experiments on the combinations of potassium and sodium with oxygen; and of potash and soda with water, from which he concludes that those metals when burnt in oxygen gas are at their highest state of oxygenation—and at their lowest, when in the state of potash and soda. He also found that ignited potash contains about 16 per cent. of water, and ignited soda 22·9 per cent.

The spontaneous inflammation of the metals of the fixed alkalies in oxymuriatic gas, affords a proof of the intensity of their attractions. In these operations, no water is separated, but mere binary combinations formed; the same as those produced by igniting muriate of potash and soda. Similar compounds are formed when dry potash and soda are heated in oxymuriatic gas, and oxygen is evolved.

Mr. Davy mentions a simple mode by which pure sodium may be obtained. It is by mixing common salt which has been ignited to redness, with potassium, and exposing the whole to a red heat in a glass tube or retort; for every two parts of potassium employed, one part of sodium is obtained.

As the muriates of lime, barytes and strontites remain unaltered by any simple attractions, even at a white heat, Mr. Davy conceived that these compounds consist merely of the metallic bases of the earths in union with oxymuriatic gas, and the experiments he has made confirm this conclusion. Thus when lime, barytes, &c. were heated in oxymuriatic gas, oxygen was expelled, and substances exactly similar to the dry muriates were formed.

In operating on the metals, Mr. Davy employed green glass retorts holdiug from three to six cubical inches of gas, they were furnished with stop-cocks. The metal was first introduced into the retort, it was then exhausted and filled with oxymuriatic gas, the heat of a spirit lamp was employed in the processes. The products from arsenic, antimony, and bismuth, were the butters of arsenic, antimony, and bismuth; and on the addition of water, the white oxides and

and muriatic acid. Tin produced Libavius's liquor, mercury, corrosive sublimate, silver and lead, horn silver, and horn lead. Iron, a beautiful, volatile, crystallized substance which gave the red muriate of iron on the addition of water.

Mr. Davy also found that oxymuriatic gas decomposes the metallic oxides at a heat below redness;—those of the volatile metals more easily than those of the fixed metals, and protoxides more readily than peroxides. Mr. Davy notices two beautiful experiments on the agency of oxymuriatic gas on white oxide of arsenic, and black oxide of iron. In these cases, no oxygen was evolved, the portion separated from one part of the oxides combined with the other part, and the products were butter of arsenic and arsenic acid, and ferruginous sublimate and red oxide of iron.

Mr. Davy notices an experiment in which he decomposed the gray oxide of tin by muriatic acid gas. In this case, water rapidly separated and Libavius's liquor was formed.

Mr. Davy conceives that these new inquiries confirm all the conclusions he has drawn in his recent paper on "Oxymuriatic Acid, &c."

LXXIII. *Intelligence and Miscellaneous Articles.*

NEW ENGINE.

An engine has been lately invented by Mr. Taylor of Holwell, Engineer to the Tavistock Canal, which may be put into motion either by water or steam, without any alteration in its construction or in any of its parts. It is extremely simple, and may be erected at a moderate expense. Its power when worked by water is, as in other hydraulic machines, in proportion to the quantity employed and height of the fall. When steam is substituted, the area of the piston determines the effect.

It may probably be a valuable machine in cases where a falling stream may be had equal to useful purposes at one period of the year, and deficient in a proper supply in dry seasons. Many mines and manufactories are in this situation, and might by a single engine of this sort work uninterruptedly, saving the expense of coal when the stream of water was sufficient, and using the boiler only when the supply of water fell short. It is of the kind of hydraulic machines usually called Pressure Engines; various constructions of which have been attempted, but none have yet been very successfully made, at least upon a large scale.

The

The difficulty which has attended the opening and closing valves of sufficient water-way, having presented great obstacles to a regularity of movement,—this objection is surmounted in this instance by the invention of a new valve, which admits apertures of large size, and is opened and closed with any required velocity, and is applicable to the passage either of water or steam.

To Mr. Tilloch.

Sir,—If you will please to communicate the following fact through the medium of your very intelligent Magazine, some of your readers will probably favour the public with their opinions upon the subject.

On my passage from Rio de Janiero to this place, on board the ship Favorite, Capt. Atkinson, on July the 14th, being in latitude 31° 56′ and longitude per account 39° 30′, at six A. M., when below, felt a very singular sensation which lasted near a minute. All below ran upon deck, feeling the ship shake as if she was passing over a wreck, rubbing very hard; or as if some very heavy body was rolled from one end of the ship to the other. To the officer and people on deck the sensation was as if the ship was going over a bar, touching, but not stopping; this lasted nearly the space of a minute. Some ran for the lead, which was hove in the shortest time possible,—no bottom: others sounded the pump,—no difference;—each looked astonished and panic-struck. The sea was smooth, a gentle breeze westerly, all sail set; the ship was loaded with coffee. At about seven A. M., an hour after, the same sensation was felt, less sensible and of shorter duration.—Could it arise from electricity?

Any of your readers favouring the public with their opinion, will much oblige

Your humble servant,
A Passenger.

METEOR SEEN IN HOLLAND.

Wall (on the Meuse), Sept. 22.

On the 19th of this month, between the hours of five and six in the evening, a luminous meteor appeared to the south, and about the distance of a quarter of a league from the small commune of Brezeau: persons who attentively examined it assert that it was nearly a quarter of an hour in collecting, floating over the place where it was first seen; and

and that when all its parts had united, it appeared all at once as a very considerable globe of fire, taking a northerly direction: it spread terror amongst the inhabitants of the village, who believed their houses would be burnt, and they themselves perish. This globe was accompanied by a frightful noise, which was heard at the distance of more than a league and a half, and sometimes resembled the rolling of a rapid chariot; at others, the noise of rain driven by the wind. It was followed by a very thick fog, and carried up from the ground every thing it met in its passage. In crossing a river it absorbed water, which soon afterwards fell in rain. It wandered for some time near the village.

One thing certain is, that the roof of a house was thrown down, which is the only trace it has left. It was accompanied and followed by an abundant rain, much lightning, and loud claps of thunder. Continuing in the same direction, it suddenly turned into a column of fire, which, with the fog, rose towards the heavens. This made many persons believe the fog was smoke. It remained about a quarter of an hour in this state, a quarter of a league to the north of the village, and at a short distance from the forest of Beaulieu. This column now sunk a little, and at last it suddenly disappeared, leaving a thick fog which had no smell. This phænomenon lasted three quarters of an hour, and travelled over the space of half a league.

MR. MUNGO PARKE.

November 8, 1810.

The painful incertitude respecting the fate of this adventurous character yet exists. An account has however been received in town this week, which again revives the almost extinguished spark of hope. It is stated by a very respectable gentleman, Capt. Davison, commander of a vessel of Messrs. Anderson, lately returned from the Coast of Africa, that on the 26th of July last a Moor arrived from the interior at Bunce Island, in the river Sierra Leone, from whom the following particulars were learned.—In January 1809 Mr. Parke was seen by the Moor, at a short distance from Tombuctoo, in a state of very bad health, in one of the natives' huts, after having been imprisoned by a native chief. He was, however, all that time at liberty, and had received permission to proceed on his route. Capt. Davison interrogated the man frequently and minutely; and, from the consistency of his answers, entertains no doubt of the correctness of his narrative.

To

To Mr. Tilloch.

Sir,—Should the following idea be considered by you as an improvement in writing and printing numbers consisting of many digits, its insertion in your publication will confer an honour on, Sir,

Your very humble servant,

Spitalfields, 19th Nov. 1810. A Reirtalp.

When a number such as 69,470,600,078,406,300,097 presents itself, though pointed in periods of three figures, the manner of expressing it in words does not immediately occur to the mind. The mode I would beg leave to offer as an improvement is, besides pointing it in periods of three figures, to place one accent over the seventh figure, or millions; two accents over the 13th figure, or billions; and so on, increasing the accents at every myriad, thus:— $6\overset{\prime\prime\prime}{9},470,60\overset{\prime\prime}{0},078,40\overset{\prime}{6},300,097$,—by which we can perceive at once that the two first figures denote trillions, without the usual mode of reckoning according to the Numeration Table.

EARTHQUAKE.

Extract of a letter, dated St. Michael (Azores), August 24.—"One of those dreadful phænomena never witnessed in your country has plunged many here in unspeakable wretchedness and affliction, and continues to occasion great terror to all the inhabitants of this island. On the 11th of August, at ten P. M. slight shocks of an earthquake were felt at intervals of a few minutes for four hours. During this time the inhabitants, under the influence of alarm for their personal safety as well as property, were running to and fro in the greatest distress. Between two and three a dreadful rocking was experienced throughout the whole island; several houses, unable to resist its violence, were thrown down, and many others were greatly damaged; and such persons as sought safety in the open air were dashed to the ground. Hitherto the calamity had been confined in its effects, and though great injury had been sustained, we had to congratulate ourselves on the loss of few lives; but we were yet to witness a most dreadful spectacle. On the 12th at mid-day, a hollow-rumbling sound was heard, the clouds gathered, and the wind was hushed into silence; the rocking returned, and in a few minutes after the village of Gozas, situated on a plain, comprising 22 houses, was swallowed up, and in the spot where it stood a lake of boiling water gushed forth. Many of the unfortunate inhabitants, who had previously retired to the elevated ground, beheld the sight with a degree of horror and

and amazement which enchained all their faculties; their whole property swept away in a few minutes, and in the place where their once beautiful gardens and flourishing orchards stood, nought now appeared but a vast expanse of water! About thirty-two persons, it is calculated, have lost their lives by this awful and calamitous event, and cattle and property to a considerable amount destroyed. A great degree of alarm continues to pervade the whole island, as on the east side an orifice has been discovered, resembling the crater of a volcano, and out of which flames occasionally burst through. Hitherto they have been unaccompanied by any ejection of volcanic matter."

On Wednesday, October the 3d, the Society for Relief of Widows and Orphans of Medical Men, in London and its Vicinity, held their Half-yearly General Court at the usual Place of Meeting—the Gray's Inn Coffee-House, Holborn; at which time their Annual Election of Officers and Directors took place, and the following were the arrangements made for the ensuing Year, viz.

PATRON,

His Royal Highness the Duke of Kent,

PRESIDENT,

James Ware, Esq.

VICE-PRESIDENTS,

Sir F. Milman, Bart.	Dr. Squire	Mr. Heaviside
Dr. Garthshore	Dr. Dennison	Mr. Moore
Dr. Lettsom	Mr. Howard	Mr. Nevinson
Dr. Blane	Sir W. Blizard	Mr. Rendall.

TREASURERS,

Dr. Denman, Dr. J. Sims, Dr. Dennison.

DIRECTORS,

Physicians.

Dr. Temple	Dr. Walshman	Dr. R. Pearson
Dr. Stone	Dr. S. H. Jackson	Dr. Croft.
Dr. Frampton	Dr. Shaw	

Surgeons.

Mr. Ed. Browne	Mr. Steele	Mr. Ramsden
Mr. Mathias	Mr. H. L. Thomas	Mr. Lewis.
Mr. Milward	Mr. C. M. Clarke	

Apothecaries.

Mr. Field	Mr. Coates	Mr. Starr	Mr. Pilliner
Mr. Upton	Mr. Seaton	Mr. Malim	Mr. Moore, Jun.

TRUSTEES,

Right Hon. Marquis Townshend, President of the Society of Antiquaries;

Right

Right Hon. Sir J. Banks, Bart. K.B. President of the Royal Society;
Isaac Hawkins Browne, Esq. M.P. F.R.S.
James Ware, Esq.

SECRETARY,
Mr. William Chamberlaine.

SOLICITOR,
Okey Belfour, Esq.

COLLECTOR,
Mr. George Hunt, No. 2, Cock-Court, Ludgate Hill.

BANKERS,
Messrs. Vere and Co., No. 77, Lombard-Street.

HONORARY MEMBERS,
Right Honourable Marquis Townshend,
Right Honourable Sir J. Banks, Bart. K.B.
Isaac Hawkins Browne, Esq. M.P.
Jame Vere, Esq.
Sir William Watson
Okey Belfour, Esq.
William Morgan, Esq.
Charles Chevalier, Esq.
Sir Frederick Baker, Bart.

LIST OF PATENTS FOR NEW INVENTIONS.

To Edmund Griffith, of the city of Bristol, Esq. for certain improvements in the manufacture of soap for the purpose of washing with sea-water, with hard water, and with other waters.—Oct. 8, 1810.

To Richard Woodman, of Hammersmith, in the county of Middlesex, boot- and shoe-maker, for his method of manufacturing all kinds of boots, shoes, and other articles.—Oct. 8.

To Edward Manley, of Uffculme, in the county of Devon, clerk, for his apparatus for writing.—Oct. 8.

To John Fraser, collector of natural history, now of Sloane Square, in the county of Middlesex, for his discovery of certain vegetables, and a way of preparing the same, so as they may be usefully applied in the manufacturing of hats and bonnets, chair-bottoms, and baskets, and for other articles or purposes.—Oct. 15.

To John Wheatley, of Greenwich, in the county of Kent, coach-builder, for his improved axle-tree for wheels of carriages, and also improved wrought- or cast-iron boxes and cast-iron stocks to receive the spokes of the wheels.—Oct. 15.

To Thomas Mann, of Bradford, in the county of York, stuff-merchant, for certain improvements in the construction of artificial legs.—Oct. 31.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For November 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct. 27	44	46°	39°	30·05	31	Cloudy
28	47	49	40	29·62	0	Rain
29	38	43	33	·65	25	Fair [the night
30	36	44	32	·89	38	Fair, with snow in
31	33	44	43	·95	25	Cloudy
Nov. 1	44	47	40	·63	22	Cloudy
2	39	47	41	·80	20	Cloudy
3	42	46	41	·87	0	Rain
4	42	46	35	·84	10	Showery
5	33	41	39	·65	25	Cloudy
6	40	43	37	·12	10	Showery
7	38	42	37	28·92	5	Showery
8	40	46	37	29·00	0	Rain
9	36	51	42	·47	21	Fair
10	41	48	43	28·50	0	Stormy
11	43	44	44	29·30	5	Cloudy
12	43	45	40	·61	22	Fair
13	40	45	37	30·10	25	Fair
14	39	42	43	29·93	25	Rain
15	51	54	47	·50	15	Fair
16	52	57	50	·25	26	Fair
17	47	51	44	·40	22	Fair
18	43	48	47	·55	16	Fair
19	47	51	46	·56	5	Rain
20	44	47	50	·60	10	Cloudy
21	53	56	47	·40	0	Rain
22	47	47	45	·65	0	Stormy
23	45	53	47	·76	21	Fair
24	47	50	46	·65	0	Rain
25	44	49	41	·56	10	Showery
26	43	44	41	·25	0	Rain

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

LXXIV. *Theoretical Suggestions for the Improvement of Practical Surgery.* *By* A CORRESPONDENT.

1st. IN that part of the operation of amputation when the bone is to be sawed through, it appears to me that a steady support to the bone would materially facilitate and secure the correct action of the saw: In the present mode, when the only means of steadying the bone and resistance to the action of the saw is made by the grasp and manual force of frequently agitated assistants, the difficulty of dividing the bone, without splintering and ruggedness, is very considerable. Might not a perpendicular prop from the floor, with a semicircular hollow to receive the bone, be of great effect in rendering it steady? When a retractor is used, might not such prop form part of that instrument? Carpenters, when they saw timber, always take care to make it steady previous to the application of the saw; Why should not the same mode be used when sawing the bones of the arm or leg? The soft parts could not be injured by such a method; as by the present mode of amputation, by double incision, a considerable length of bone is bared before the saw is used, and why might not the proposed support be applied to that part?

2d. In the operation of trepaning the skull, when the scalp is sufficiently removed, it is essential to remove just so much of the pericranium and no more, as the head of the trephine will include; because the cranium, when denuded of its pericranium, will, like other bones denuded of their periosteum, grow carious. This part of the operation is now generally performed by an instrument called a raspatory, or by scraping the skull with a small scalpel. Would not this be performed much more complete by having a head adjusted to the trephine handle, precisely the dimensions of the serrated head, and which head would have a circular cutting edge, with a species of concave plane or scraper within it? One turn of such an instrument as I can imagine, and as any person could easily contrive, but which I find difficult to describe by words, would completely remove the exact portion of pericranium and no more*.

3d. Were surgeons to make themselves acquainted with the implements used in different mechanical professions, it is possible some valuable additions might be made to the

* We have reason to believe that there is such an instrument as the one above alluded to. It was said to have been the invention of Mr. Henry Cline, junior.

present instruments of surgery. They should likewise observe, with a professional eye, the various mechanical improvements which are daily taking place. Might not the circular saw be introduced in some operations? Small circular saws, cutters, or wheels with toothed edges of different sizes and thickness, might perhaps be used with effect in insulating and removing the depressed angular pieces of bone which occur in fractures of the skull. When the trephine is inadmissible, a circular cutter applied to the edge of the fracture, might, if used with proper precaution, cut away the bone with safety, and make a space sufficient to admit the elevator. Such a cutter might be turned by the hand, as great velocity would be dangerous. A method could easily be contrived to apply such an instrument with the requisite steadiness to the part.

4th. The centre point of the trephine necessarily protrudes beyond its teeth; in consequence of which, when the inexperienced operator neglects too long to remove it, the most serious effects are sure to follow. Might not this be easily prevented by having a shoulder, as mechanics term it, to surround the point, just so far down from the extremity of the point, as to permit the saw to fix itself, and no more?

5th. Would not a contrivance be useful, in trepanning the skull, to fix the head in the most favourable posture?

6th. The best shape for the points of one description of piercing instruments has, I think, never yet been exactly ascertained; and it is certainly a question of considerable importance. I mean those piercing instruments, where breadth of instrument is requisite immediately on insertion; for, as to common needles, and other small instruments merely for piercing, it is evident the more acute we make their points, the better. In some instruments, however, where a point is merely necessary for their insertion, when that point is much prolonged beyond the *efficient* part of the instrument, it becomes injurious: What point will suit such an instrument best? Is it well ascertained that the drill or shear point is the most advantageous? If the point formed an acute angle, sloped to one side, would it not answer as well? What is the proper angle for such a point? Mechanics pierce brass, copper, and steel with drills of different shapes: May not there be an appropriate point for piercing animal membranes? The French discovered by experiments (fatal experiments!) that the descending blade of the guillotine cut best when sloped to a certain angle. However confident we may be in our opinion, to experiment we should always have recourse, when possible; and in satisfying

tisfying our present doubts, it is very possible. An apparatus might easily be contrived for the purpose. All that is requisite is, a piece of brass, about four inches long and two inches square, supported by two or more feet, and perforated longitudinally, to admit a thick steel pin, which pin is to be fitted to the perforation in the brass, so as to move very freely, and to exceed the brass about two inches or more in length. On the upper end of this pin, I would have a small flat piece of brass fixed, capable of holding weights; and in the lower end, a hole made, into which the different shaped points on which we are desirous to make experiments are to be fixed, similarly to the shifting feet of common compasses. Immediately below this pin I would place a species of drum, consisting of a small box, with bladder or other elastic substance stretched over it to imitate the animal tunics. Now it is evident, that by placing different weights on the upper end of the pin, until the point we are experimenting on pierce the stretched bladder, we may be able exactly to appreciate the comparative advantage of the different kinds of point.

7th. The operation of couching frequently fails, from the cataract or opake crystalline lens being of a soft consistency; which the couching needle, instead of depressing, passes through and divides. If the broad part which is, I believe, frequently used in depression, were made concave so as to fit the convex edge of the lens, might it not in some degree remedy this evil?

8th. Considering that the majority of calculi in the bladder are more or less spherical, it appears to me that the forceps now used in lithotomy is not of the most advantageous construction. By each side of the beak of the usual forceps being concave, with an oval hole at the bottom of the concavity, similar to the forceps once used in extracting polypi, the edges of which hole, and the sides of the concavity, might have teeth, Would it not be more likely to lay hold of even an irregularly spherical calculus than the forceps at present used, with toothed beaks and flat sides? and which seems better contrived for crushing a soft calculus to pieces, than for holding it fast and withdrawing it whole.

9th. Grown bold by practice, I shall now venture, as my last suggestion, to propose another kind of forceps for extracting the stone. Let us suppose a forceps with the beaks formed of two narrow elliptical rims, jointed so as in some degree, by the pressure upon a calculus, to conform themselves to its size. To the edges of these rims I would attach a piece of linen or leather, forming to each beak a small purse or sack. When these forceps should be closed upon a calculus in the least spherical, the steel rims would extend to let it pass, and it would then be completely surrounded. The advantages of this forceps would be, that the calculus could not escape; and the bulk to be withdrawn through the wound, would be very little more than the exact bulk of the calculus.

LXXV. *Researches on the oxymuriatic Acid, its Nature and Combinations; and on the Elements of the muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution.* By H. Davy, *Esq. Sec. R.S. Prof. Chem. R.I. F.R.S.E.*

[Concluded from p. 361.]

It is extremely probable that there are many combinations of the oxymuriatic acid with inflammable bodies which have not been yet investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of Gay-Lussac and Thenard is the compound containing the maximum of phosphorus. The crystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oxymuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phosphoric and muriatic acid; and the liquid, I believe only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oxymuriatic acid.

It is evident, that whenever an oxymuriatic combination is decomposed by water, the oxide or acid or alkali or oxidated body formed must be in the same proportion as the muriatic acid gas, as the oxygen and hydrogen must bear the same relation to each other; and experiments upon these compounds will probably afford simple modes of ascertaining the proportions of the elements, in the different oxides, acids, and alkaline earths.

If, according to the ingenious idea of Mr. Dalton, hydrogen be considered as one in weight, in the proportion it exists in water, then oxygen will be nearly 7·5; and assuming that potash is composed of one proportion of oxygen, and one of potassium, then potash will be 48, and potassium

potassium * about 40·5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oxymuriatic acid will be represented by 32·9, and muriatic acid gas, of course, by 33·9; and this estimation agrees with the specific gravity of oxymuriatic acid gas, and muriatic acid gas. From my experiments, 100 cubical inches of oxymuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74·5 grains; whereas by estimation they should weigh 74·6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38·4 grains.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxygen with which the inflammable matter will combine †.

In considering the dry muriates, as compounds of oxymuriatic acid and inflammable bodies; the argument that I have used in the last Bakerian lecture, to show that potassium does not form hydrate of potash by combustion, is considerably strengthened; for from the quantity of oxymuriatic acid the metal requires to produce a muriate, it seems to be shown that it is the simplest known form of the alkaline matter. This I think approaches to an experimentum crucis. Potash made by alcohol, and that has

* Supposing potash to contain nearly 15·6 per cent. of oxygen.

† I have stated in the last Bakerian lecture, that during the decomposition of the amalgam from ammonia, one in volume of hydrogen to two of ammonia is evolved: it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. If it be supposed that the hydrogen arises from the decomposition of water; then the oxygen that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hydrogen, in an equal volume of muriatic acid; or if it be said that ammonium is a compound of two of ammonia and one of hydrogen in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oxymuriatic acid and ammonium, supposing they could be immediately combined. I once thought that the phænomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, The metal of ammonia, in which hydrogen was so loosely combinned as to be separable with great ease, and which, in consequence of the small affinity of the basis for water, it had little tendency to combine with oxygen. The second, the metals of the alkalies and alkaline earths, in which the hydrogen was more firmly combined, but in combustion, forming water capable of being separated from the basis. And, thirdly, the metals of the earths and common metals, in which the hydrogen was more intimately combined; producing by union with oxygen, water not separable by any new attractions. The phænomena of the action of potassium and sodium upon muriatic acid, referred to in the text, seem however to overturn these speculations so far as they concern the metals from the fixed alkalies.

 been

been heated to redness, appears to be a hydrate of potash, whilst the potash formed by the combustion of potassium must be considered as a pure metallic oxide, which requires about 19 per cent. of water to convert into a hydrat.

Amongst all the known combustible bodies, charcoal is the only one which does not combine directly with oxymuriatic acid gas; and yet there is reason for believing that this combination may be formed by the intermedium of hydrogen. I am inclined to consider the oily substance produced by the action of oxymuriatic acid gas, and olefiant gas, as a ternary compound of these bodies; for they combine nearly in equal volumes; and I find that, by the action of potassium upon the oil so produced, muriate of potash is formed, and gaseous matter, which I have not yet been able to collect in sufficient quantity to decide upon its nature, is formed. Artificial camphor, and muriatic ether, as is probable from the ingenious experiments of M. Gehlen and M. Thenard, must be combinations of a similar kind, one probably with more hydrogen, and the other with more carbon.

One of the greatest problems in œconomical chemistry, is the decomposition of the muriates of soda and potash. The solution of this problem will, perhaps, be facilitated by these new views. The affinity of potassium and sodium for oxymuriatic acid is very strong; but so likewise is their attraction for oxygen, and the affinity of their oxides for water. The affinities of oxymuriatic acid gas for hydrogen, and of muriatic acid gas for water, are likewise of a powerful kind. Water, therefore, should be present in all cases, when it is intended to attempt to produce alkali. It is not difficult after these views to explain the decomposition of common salt, by aluminous or siliceous substances, which, as it has been long known, act only when they contain water. In these cases the sodium may be conceived to combine with the oxygen of the water and with the earth, to form a vitreous compound; and the oxymuriatic acid to unite with the hydrogen of the water, forming muriatic acid gas.

It is also easy, according to these new ideas, to explain the decomposition of salt by moistened litharge, the theory of which has so much perplexed the most acute chemists. It may be conceived to be an instance of compound affinity: the oxymuriatic acid is attracted by the lead, and the sodium combines with the oxygen of the litharge and with water to form hydrat of soda, which gradually attracts carbonic acid from the air.

As

As iron has a strong affinity for oxymuriatic acid, I attempted to procure soda by passing steam over a mixture of iron filings, and muriate of soda intensely heated: and in this way I succeeded in decomposing some of the salt: hydrogen came over: a little hydrate of soda was formed; and muriate of iron was produced.

It does not seem improbable, supposing the views that have beeen developed accurate, that, by complex affinities, even potassium and sodium in their metallic form may be procured from their oxymuriatic combinations: for this purpose the oxymuriatic acid should be attracted by one substance, and the alkaline metals by another; and such bodies should be selected for the experiment, as would proouce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the Society to some of the theoretical relations of the facts noticed in the preceding pages.

That a body principally composed of oxymuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phænomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form in this instance a substance neither fusible nor volatile, at a white heat. It could not have been expected that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oxymuriatic acid, would have appeared incredible, according to all the existing analogies of chemistry. The experiments on which these conclusions are founded, are, however, uniform in their results: and it is easy to repeat them. They seem to show, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oxymuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common re-agents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? and that an intense attrac-

 tion,

tion, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Besides the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been referred to, it is probable that other compounds of like nature may be formed of the oxides, alkalies, and earths, with the oxymuriatic combinations, or of the oxymuriatic compounds with each other; and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and, as it would seem at first view, contradictory facts. For if, as I have said, oxymuriatic acid gas be referred to the same class of bodies as oxygen gas, then, as oxygen is not an acid, but forms acids by combining with certain inflammable bodies, so oxymuriatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hydrogen, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oxymuriates of phosphorus and tin.

Like oxygen, oxymuriatic acid is attracted by the positive surface in Voltaic combinations; and on the hypothesis of the connexion of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

I shall occupy the time of the Society for a few minutes longer only, for the purpose of detailing a few observations connected with the Bakerian lectures, delivered in the two last years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute inquiries have enabled me to correct or extend.

I have already mentioned that there are considerable differences in the results of experiments, made on the action of potassium, on sulphur and phosphorus, and their combinations with hydrogen, according to different circumstances of the process. I shall now refer to such of these circumstances as I have been able fully to investigate.

The able researches of Dr. Thomson have shown that sulphur, in its usual state, contains small quantities of acid matter; and though, in my first experiments, I conceived

that

that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe that this is not the case; for by subliming some similar sulphur in nitrogen, I find that litmus paper placed in the upper part of the retort is slightly reddened.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid; sulphuretted hydrogen likewise is soluble in muriatic acid: and this circumstance led me to under-rate the quantity of sulphuretted hydrogen given off in experiments of this kind *.

In acting upon sulphuretted hydrogen by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe that the violence of the combustion occasioned the decomposition of a considerable quantity of the gas; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

In all late experiments in which sulphur or sulphuretted hydrogen was concerned, I have used muriatic acid saturated with sulphuretted hydrogen over mercury. I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green-glass, or plate-glass lined with sulphur and filled with very pure nitrogen or hydrogen. In making potassium act upon sulphuretted hydrogen, I have employed the gas only in the quantities of from one to three cubical inches, and have made the combination in narrow curved tubes of green-glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them to enable me to form conclusions, which I may venture to say cannot be far from the truth.

When one grain of potassium, which would give by the

* This circumstance has been pointed out by MM. Gay Lussac and Thenard, in a paper printed in the *Journal de Physique* for December, in which these gentlemen endeavour to show that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it produces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their inquiries.

action

action of water about one cubical inch and $\frac{1}{10}$ of hydrogen is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light, and from $\frac{1}{14}$ to $\frac{1}{10}$ of a cubical inch of sulphuretted hydrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hydrogen, affords from $\frac{9}{10}$ to $\frac{11}{10}$ of a cubical inch of pure sulphuretted hydrogen.

When more sulphur is used so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hydrogen evolved by the action of the acid, is from $\frac{7}{10}$ to $\frac{9}{10}$; but if heat be applied to the combination, so as to drive off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phænomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur nearly as three to one in weight; and in which the quantities are such that the compound burns into neutral sulphate of potash.

When a grain of potassium is made to act upon about 1·1 cubical inches of sulphuretted hydrogen, all the hydrogen is set free, and a sulphuret of potassium containing one-fourth of sulphur is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

When sulphuretted hydrogen is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hydrogen disengaged, and a compound of sulphuretted hydrogen and sulphuret of potash is formed, which gives sulphuretted hydrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

From a number of experiments I am inclined to believe that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about $\frac{3}{8}$ of a grain of phosphorus to form a phosphuret; which when acted upon by muriatic acid, produces from $\frac{8}{10}$ to $\frac{10}{10}$ of a cubical inch of phosphuretted hydrogen.

Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hydrogen, and sets free rather more than four cubical inches of hydrogen; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

If,

If, according to Mr. Dalton's ideas of proportion, the quantity in which sulphur enters into its combinations were to be deduced from its union with potassium, in which it seems to form about one-fourth the weight of the compound, the number representing it would be 13·5. I have lately weighed sulphuretted hydrogen and sulphureous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. Kirwan: that of sulphureous acid gas I find is 20967. Sulphuretted hydrogen, as I have shown, contains an equal volume of hydrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxygen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphureous acid from 100 of oxygen in volume; from which I am inclined to believe, that sulphureous acid consists of sulphur dissolved in an equal volume of oxygen; which would give the number as 13·7 * nearly, considering the acid gas as containing one proportion of sulphur, and two of oxygen; and these estimations do not differ from each other materially.

I have made several experiments on the combustion of phosphorus in oxygen gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorb in combustion about 34 of oxygen in weight: and considering phosphoric acid as composed of three proportions of oxygen and one of phosphorus, the number representing phosphorus will be about 16·5, which is not very remote from the number that may be deduced from the composition of phosphuret of potassium.

The numbers which represent the proportions in which sulphur and phosphorus unite with other bodies, are such, as do not exclude the existence of combined portions of oxygen and hydrogen in their constitution; but it may be questioned, whether the opinion which I formed, that the

* The estimation from the composition of sulphuretted hydrogen, must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted hydrogen at 35 grains the 100 cubical inches, which was not far from the mean between the estimations of Mr. Kirwan and Mr. Thenard. According to this last experiment, sulphuretted hydrogen is composed of one proportion of hydrogen, represented by 1, and one of sulphur represented by 13·4.

inflammable

inflammable gas disengaged from them by electricity, is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture, is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hydrogen, and probably contains that body. It is not unlikely, that in all cases, phosphorus and sulphur contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur, is probably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained, yet from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe, that they sometimes exist in minute quantities in common phosphorus and sulphur, and with hydrogen give to them their variable properties.

The colours of different specimens of phosphorus, as well as of sulphur, differ considerably; the red colour of phosphorus as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll sulphur is of a very pale yellow, the Sicilian sulphur of an orange colour, and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish-green colour. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxygen in Sicilian sulphur, which is probably owing to the presence of oxide of sulphur, which may give rise to sulphuric acid in distillation, or to sulphuric acid itself.

Conceiving that, if definite proportions of oxygen and hydrogen existed in sulphur and phosphorus, they ought to be manifested in the agency of oxymuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial, on the combination of sulphur with oxymuriatic acid gas, I employed five grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel in which it had been in contact with warm water: in this case more than a half a cubical inch of oxygen gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapour had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime: in this case, though Sicilian sulphur was used, no oxygen gas was evolved; and not a half a cubical inch of muriatic

muriatic acid; the quantity was the same as in the last experiment; and it was found, that between 16 and 17 cubical inches of oxymuriatic acid gas disappeared; the whole of the sulphur was sublimed in the gas, and the liquor formed was of a tawny orange colour.

No oxygen was expelled during the combustion of phosphorus in oxymuriatic acid gas, nor could I ascertain that any muriatic acid had been formed; three grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas.

It would seem from these quantities, that the sulphuretted liquor formed by subliming sulphur in oxymuriatic acid gas, consists of one proportion of sulphur, represented by 13·5, and one of oxymuriatic gas represented by 32·9, and that the phosphoric sublimate must be composed of three portions of oxymuriatic gas, represented by 98·7 and one of phosphorus represented by 16·5.

LXXVI. *Reflections on some Mineralogical Systems.* By R. Chenevix, *Esq. F.R.S. and M.R.I.A., &c. Translated entire from the French, with Notes by the Translator.*

[Continued from p. 391.]

There is not a shepherd among those whose eyes and mind have never extended beyond the flocks which they keep, the plains which nourish them, and the day which affords them light, who could not convince the mineralogist of the absurdity, should the latter wish to teach him that a flock of wethers and ewes was a flock of animals of a new species; and if the miner could perceive the mineralogical individual, as the shepherd sees his wethers and ewes, the doctrine of passages would excite laughter from Siberia to Peru. It is below any other criticism*.

Finally,

* This is too severe; since M. Werner, not content to imitate Buffon in world-making and forming the habitable globe of a ball of glass, invokes the shade of Moses, and furnishes us with "*transition* rocks, which are *supposed* to have been deposited during the *passage* or *transition* of the earth from its *chaotic* to its habitable state" This knowledge, doubtless, is so perfectly within the sphere of our senses, that we must congratulate the champions of *eternals* on their singular modesty and consistency. "Hence," continues the passenger or transitionist, "they contain the *first* traces of organic remains and *mechanical* depositions, and are denominated *transition* rocks. They are also highly important, as connecting the primitive with the floetz rocks, and thus preserving the *beautiful series* of *transitions* which are to be traced from the oldest primitive to the newest alluvial formations." Unhappily these

Finally, the physical molecule is considered as without parts: it can only change all at once; therefore in the system of integral molecule, there can be no intermediate or demi-species of passage. In the system of external characters, passages are conceived, and all may be passages, if it is wished, for it cannot be said why any being is a species.

In geology things are somewhat different. Granite is composed of quartz, mica, and feldspar. By withdrawing the influence of mica, if the quantity of feldspar begins to diminish in the granite, the latter will change its appearance until that by continual variation it becomes gneiss; and ultimately, when there will be no more feldspar, we shall have micaceous schist. This micaceous schist may lose its quartz or its mica, until it becomes on the one side mica and the other pure quartz. We can therefore suppose all these minerals proceeding from granite, as a common centre, to pure quartz, feldspar and mica, by three or more divergencies like passages. But for what? It is that in them all, we have only the limits which are rigorous or definite.

In this pretended chain, with the aid of which theorists have so often sought to bind all parts of the universe, we see breaks at every step, and, far from possessing the whole, we have yet scarcely a few links. It was wished to force them to unite, but the feeble clasps that men have substituted break in defiance of them.

A celebrated analyst has applied this word *passage* to a remarkable error. In making experiments on a new substance, he observed that it changed colour under circumstances which produced the same effect on metallic oxides, while that its other properties tended to those of the earths. Hence he concluded that it constituted the passage or transition from earths to metals.

I have seen in Germany, in a beautiful cabinet of petrifications, the head of a bear perfectly preserved and petrified.

these lovely transition rocks, like all other beautiful things are not numerous, and Mr. Jameson knows only, "1st, transition-limestone; 2d, transition-trap; 3d, grey-wacke; and 4th, flinty slate." He adds, in the true style of German logic, p. 146, of what he calls "Elements of Geognosy," that "transition-limestone, which appears to be the oldest member of the *transition* class, is a *simple* rock!" The idea of *transition* and of *simplicity* is wonderfully philosophical and congruous. It will prepare the mind of the chemist for what follows, namely, that this *simple* rock contains petrifications of marine animals, as corallites, encrinites, trochites, &c., and that it is "very frequently traversed by small veins of calcspar; it is not particularly metalliferous," but that "we possess very little satisfactory information respecting either the kind, repository, or quantity of ore it contains;" yet the author hesitates not to declare that it is both a *simple* and a *transition* (*i. e.* compound) rock!—TRANS.

I in-

I inquired if it was the common or the white Polar bear [*Ursus maritimus*], and was answered, "It may be the *passage* from one to the other." (*Es mag wohl ein übergang seyn.*) The mineral kingdom, in appropriating this unfortunate bear, has rendered it the subject of an absurdity.

With the word passage we may associate two other favourites in the same class; *modification* and *tendence**. Formerly manganese was considered as a modification of iron; nickel, cobalt, lime, magnesia, the earths and alkalis, almost all were modifications. It might have been said that in modifying nature produced all. When a man is afraid of saying he *does not know*, he speaks of modification. But philosophy, in appropriating to itself the sciences, has banished this fear; and, in fact, what are all these pretended modifications, but modifications of our ignorance?

In the same cabinet of petrifications I saw a disciple of the transcendent philosophy who admired each specimen, was enraptured with a lichen, and in ecstasy before a fish. "You believe," said he, "that these are real impressions of animals and plants. No; they are *tendencies* in nature to form them; *tendencies* to organization,—trials." Conducting him gently near a beautiful piece of Florence marble, "Behold," said I, "a *tendency* in nature to build ruins." I also demonstrated to him, by graphical granite, that nature had a *tendency* to write!

The philosopher supported his opinions on the circumstance that among the petrifications we find natural species which no longer exist: now, it is contrary to the system of dualism that a species should be extinguished, as then the sum of all the quantities in the universe would be no longer equal to zero. I observed to him that these species might be concealed for the moment in caverns. Yet he occupies a distinguished place in the mines of Germany, and will soon appear before the world in the character of an author. How little honourable are these dreams, of which transcendentalism is so proud, even to human weakness!

SYSTEMATIC PRINCIPLES OF CLASSIFICATION.

Besides the division into species, five or six other general ones are admitted in mineralogy (Emmerling, p. 27, vol. i. 2d edition; and Brochant, vol. i, p. 45), classes, genera, subspecies, &c. There are, it is said, as many classes as fundamental principles, (*grund bestandtheile*) marking, and predominating in, the combination of minerals; the earths,

* To these may be added Mr. Jameson's *English* terms of *floetz, suite, formation suite, drusy, &c. &c.*—TRANS.

salts,

salts, combustible minerals and metals. There are as many genera as chemical principles (*chemische bestandtheile*) predominating in, or at least characterizing, the fossil combinations. I know not what difference it is wished to make between fundamental and chemical principles, and the shades which separate them are not explained in a satisfactory manner. Consequently I understand nothing of this partition, in classes, genera, &c. In the system of M. Haüy there are four classes analogous to those of M. Werner; acidiferous substances, terreous substances, immetallic combustible substances, and metallic substances. In the first class the alkaline and earthy nature constitutes the orders, and each individual base forms a genus. In the second class there are no other subdivisions than the species. The third class contains two orders, the simple and compound combustibles. The fourth, three orders, according to the oxidability and reductability of the metals, and each individual metal forms a genus. All this is clear and precise, and is not obscured by any superfluous explanations.

M. Werner has divided the chemical elements into two functions in the classification of minerals. They are either predominant in quantity or characterizing. In coppery pyrites, iron is the most abundant principle; yet copper gives the character to the mineral. Chemistry accounts for the abundance of a principle: yet the particular characters of a fossil, its oryctognostic and other properties decide on its characterizing power. But as all is drawn from the testimony of our senses, and every thing is made to speak to them in this system, the minerals in such a classification are transported to the place which the characterizing principle may assign them, however contradictory it should be to the abundant principle. Thus 0·15 of silica prevails over 0·76 of alumine, and places spinelle in the siliceous genus, while 0·26 alumine against 0·46 silica transports the schist to the argillaceous genus, and gives it its name. They have endeavoured to explain the difference between the abundant principle and the characterizing principle, by means of an enveloping matter (*umbullendes stoffes*) and the attempt has had the success of most others for explaining that which we do not understand; it has confined the difficulty to one word. I shall not examine if this principle has been observed in the distribution of minerals in genera, because this part of the classification is of little importance in comparison with that which treats of the species. It must however be observed, that it appears more and more every day that we have gratuitously attributed to some elements the idea

idea of certain exclusive properties. Silica is not the only substance which in its aggregation can acquire extreme hardness; there are other earths which can become harder than it: an aggregate of alumine surpasses it in this respect, as we see in sapphire. And what shall we say of the diamond? Most assuredly we shall not consider it, like a celebrated German who said to me, when I made some objections to him on the place which he assigned this fossil and the new discoveries respecting it; "And who will tell me that carbon is not also an earth?"

DIAGNOSTIC OR DESCRIPTIVE MINERALOGY

We shall now proceed to the second part of mineralogy; to the means which assist us in the diagnosis or art of knowing minerals. This will comprehend the art of making them known to others, or that of describing them.

M. Werner has divided the diagnostic characters into, 1st, external characters; 2d, chemical or internal characters; 3d, physical characters; and, 4th, empirical characters. Perhaps it may surprise some to see the latter epithet thus confined to only a part of this system.

The preference given by M. Werner to external characters is manifest from what follows: (Brochant, Introduction, p. 30.) He examines these characters corresponding to the five following questions: "What are the characters which always manifest themselves, and in all minerals? These are the external characters and the chemical characters, although the smallness of the specimens often prevents the latter from being discovered. (Is there not here a little contradiction in the characters which *always manifest* themselves, and that *often* cannot be *discovered?*)—What are those which most certainly indicate an essential difference? The chemical characters; nevertheless, the external characters equally indicate the state of aggregation.—What are those which we can determine more exactly? The external characters, because the others require delicate operations.—What are the most easily and most promptly found? The external characters, because they immediately strike our senses.—What are those which we can discover without destroying the mineral? The external characters."

M. Werner principally employs but two chemical characters; fusibility by the blow-pipe, and the proof of acids. The physical properties which he mentions are electricity, magnetism and phosphorescence, with their modifications; and even the indication of these characters has no other object but to complete the description of the minerals. In the art of merely and simply recognising them, the principal resources must be drawn from the external characters. Werner engages his pupils to use them to the almost total exclusion of any other succour. They are enjoined to confine themselves as much as possible to the limits of their senses; the use of the microscope is prohibited, and the world which exists beyond their organs must not be viewed by them. They must not employ a drop of acid, to determine whether a body effervesces with it, till the last extremity*. The strong light of the sun, used to discover sparkling, ought to be considered but as a microscopic mean which is not always in our power. Almost every thing that nature or art offers to facilitate our researches is denied, and we are reduced to the simple light of the day and our five senses.

M. Haüy, after having founded the specification on the form and composition of the integral molecule, adduces means of attaining the diagnosis more easy and more prompt than the inquiry into that form or composition. It is true, he takes little pains to describe; and he does well, because he can define; but besides the character taken from the integral molecule, he adds others with which physics and chemistry have furnished him. Thus, for borated magnesia we have as a geometric character the cube; but this figure is a term or limit, and consequently is common to other minerals. Physical characters are therefore added; as specific gravity, hardness, elasticity, and a chemical character drawn from the appearances when exposed to the action of the blow-pipe. All the more striking characters are given together, which tend directly and absolutely to separate the substances with which they might be confounded in consequence of the identity of form in the integral molecule. This method, indeed, requires some physical and chemical knowledge, while that of Werner dispenses with it surprisingly.

WERNERIAN ESTIMATE OF SPECIFIC GRAVITY AND OF ANGLES.

In the particular exposition given by Werner of the external characters, he treats in the first place of colour. This made many persons believe that he considered it as a principal character, and drew on him reproaches from which I hasten

* Hence, doubtless, the reason that the disciples of Werner are all so furiously hostile to chemistry, that they are so limited in their pursuits, so contracted in their notions, and so deficient in those principles of general science which contribute to meliorate the state of human existence, and improve society.—TRANS.

to

to exculpate him*. Among the metals, indeed, he considers it as of great weight, and in this he is supported by chemistry. It is because the colour attracts the sight, that sense which first informs us of the presence of objects in general, that it occupies the first place. The other universal external characters are cohesion, unctuosity, coldness, gravity, smell, and taste. They are called universal, because they belong to all minerals. It must be confessed, however, that among them there are some which are of very little importance, and merit slight attention. But specific gravity is not of this number, and it will not be uninteresting to see the manner in which it is treated.

A good hydrostatic balance or an areometer is all that is necessary to take the specific gravity of a body, and the operation is one of the most easy. Only a little patience, less knowledge, and no reasoning are necessary to succeed. But Werner banishes all exact modes, and says in general, that a body *swims* on water; that it is *light* when, water being 1,000, it does not weigh 2,000; *moderately* heavy, if from 2,000 to 4,000; *heavy*, from 4,000 to 6,000; and *very heavy*, if above 6,000. All that we can say from these for-

* This is candid and liberal, becoming a man of science: But how does Werner's pupil estimate this character? "In giving (says Mr. Jam. vol. i.) an account of the crystallization of a mineral, we mention its fundamental figure or figures, describe their varieties, and arrange them according to their natural alliances. *Colour*, which is a very *important character*, must also be treated in a similar manner: the species and varieties must be correctly determined, and arranged according to their affinities with each other: otherwise, particularly in minerals possessing extensive *suites* of colour, as diamond and sapphire, it would be very difficult to recollect them, and when remembered would not convey to the mind a very *distinct picture* of this *highly interesting* character. (Here the truth has transpired involuntarily.) I have therefore been careful in the descriptions to determine the colours with precision, and to arrange them as much as possible in a natural order. In the treatise of Haüy, the colours are not arranged, and very seldom accurately determined: this is the case, although not in so great a degree, with a more useful work, The Mineralogy of Brochant." It is true, the colours are not arranged by Haüy; as he, like a man of real science, treats them as purely *accidental* characters. But what is the intrinsic value of the arranged "suites of colours?" There are perhaps no two persons living who have identically the same ideas of colours, still less can any two equally find terms to describe their own notions of the matter. It follows then that each individual will have his peculiar "suite of colours," and that this "suite" must be ranked with the "mineralogical instinct" of the Wernerians. I have seen a German, a French, an Italian, a Spanish, a Portuguese, and an English, mineralogist make the experiment together; each described separately his own ideas of the colour of a certain mineral in his native and in all the other languages: the descriptions were then compared, first with respect to the individual and nation, and next with respect to the six languages; and the disparity was such as would make any delicate mind feel ashamed of the system built on such a sandy and indefinite basis. Yet Werner has not hesitated to give his idea of the colour of a mineral as a name to it!—Trans.

mulæ is, that mineral substances in general are moderately heavy, since that of 233 minerals, whose specific gravity is given by Haüy (vol. i. p. 261), there are 133 between 2,000 and 4,000, and only 46 which are between 4,000 and 6,000. By describing a mineral thus, native sulphur, whose specific gravity is 2·0332, and telesia, which weighs 3·9941, would be included in the same expression.

The estimation of the angles is given with a precision worthy of that which characterizes the estimate of the specific gravity. An angle is very obtuse when it is greater than 120°; obtuse, if it is from 100° to 120°; a little obtuse, from 90° to 100°; right, if it exceed 90°; very acute, between 45° and 90°; acute, when it is 45°; and very acute, when it is less than 45°. (Brochant, vol. i. p. 97.) Thus we learn that a right angle is that which has more than 90°. I have heard M. Werner say, (and I have written his lectures as he delivered them,) that a difference of 10° did not prevent him from considering any angle as a right angle: thus we need not be much astonished at seeing cubic zeolite so called [analcime and chabasia, H.], as the great angle of its faces differs but 3° 30′ from the right angle.

WERNERIAN THEORY OF PRIMITIVE FORMS.

Crystallization is treated as a third article in the particular external characters of solid minerals, under the name of *regular external forms*. It is observed that there are seven species of principal forms, which may be considered as the nuclei of other forms; and in this point of view they fulfil the same functions as the primitive forms of Haüy.

Werner was at perfect liberty in his choice, as he set out on a gratuitous hypothesis. There was no consideration which impelled him to give a preference to such or such a form. He had before him the whole of geometry, with the unlimited permission of choice among all the figures which it possesses in common with mineralogy. We must believe that some principle adopted by reflection would preside at the choice he was about to make, and we can conceive none more worthy of preference than that which corresponds with simplicity.

M. Werner has chosen the icosaëdron, or body terminated by twenty faces; the dodecaëdron, by twelve; the cube, by six; the prism, pyramid, table, by an indeterminate number; and the lentil, or lens, by two, as it is pretended. The icosaëdron is a very complex figure; the prism, pyramid, and table, are in some measure indefinite; and the lentil, which we are told is composed of two faces, is, indeed, composed

posed of two bent faces, but they result from an infinite number of planes. The character of simplicity therefore is totally wanting in this choice.

Still, however, there are many more objections to this method. There is scarcely any figure which I have not heard considered in several points of view. The dodecaëdron with rhomboidal faces has been sometimes regarded as a hexaëdral prism, terminated at each extremity by a triëdral pyramid.

The hexaëdron appears entirely useless, as besides this figure there may be two modes of considering all the crystals which belong to it. The cube, for instance, is a hexaëdron, but at the same time it is a quadrangular prism with square faces. The rhomboid is also a hexaëdron and a quadrangular prism, with rhomboidal faces; and every tetraëdral prism terminated by planes as bases is a hexaëdron. Moreover, these figures may be considered as two mutilated triëdral pyramids, united, it is true, base to base, with the edge against the face. Here then is a crystal which belongs to three different species of principal forms; and such is the influence of this character in the specification of minerals, that the same mineral may very well belong to three species in the oryctognostic system.

The table is nothing but a prism extremely shortened. The geometer knows as well as any other what a table is; but I suspect that from Archimedes to Newton; from the first who failed in squaring the circle, till the learned German who told me that he had discovered a fourth dimension in space,—no geometer has treated it as a geometrical figure. This invention is purely mineralogical. But where does the prism finish, and the table commence? Is there a point where a crystal being no longer a prism is not yet a table? I do not see why the table should not be ranked among the imitative forms, as the club, bush, comb, mirror, and other usual instruments.

WERNERIAN PRETENSIONS TO THE DISCOVERY OF THE INTEGRAL MOLECULE.

There is an article in the external characters of Werner, which at first seems to have some resemblance to the form of Haüy's integral molecule: I mean what relates to the lamellated fracture. After having spoken of the perfection, imperfection, &c. of the laminæ, their direction and their form, he speaks of the structure of the laminæ (lamination), or of the cleavage (*durchgang der blatter*), and says that it may be double, triple, quadruple, and sextuple. If it were wished to enter into all the details of this subject,

 we

we could find nothing more proper to demonstrate the weakness and futility of the system of external characters; but it is not, in fact, worthy of attention. Let us take only two instances. Mica is given as a mineral which has only a single direction of cleavage; this supposes two faces terminated by planes. But two planes are not sufficient to contain a solid. What then terminates the other faces of mica? This is what Haüy found in discovering other directions of the laminæ, by which he was led to determine the primitive form and integral molecule of mica, which is a right quadrangular prism whose bases are rhombs. Consequently there are three directions of the cleavage, and each of these three has another parallel to it, whence result six parallel faces two and two, or a parallelopiped. According to Werner, hyacinth has but two directions of cleavage. Haüy found its primitive form an octaëdron with isoscele triangles, and consequently its integral molecule a regular tetraëdron, and four directions of cleavage. Werner stopped his researches where his senses abandoned him. Haüy has availed himself of all the means which a profound knowledge of the different branches of the natural sciences has put in his power; and in throwing a strong light on certain minerals, he has rendered sensible the fissures which could not otherwise have been perceived.

Some persons, indeed, have pretended to infer from what Werner says respecting the property of cleavage, that he also knew the form of the integral molecule, but having perceived its futility as a principle of classification, he abandoned the idea. Two notes (p. 28 and 127 of the French translation), in his Treatise on External Characters, have been pointed out as announcing clearly his opinion. I have been able to see nothing in the first, except that animals and vegetables have different parts, which we call organs, and that the separation of those parts destroys the animal or vegetable, while we can divide a mineral into as many small parcels as we please, without its ceasing to be the same mineral. But if we destroy its composition, then the mineral is destroyed. It is not therefore doubtful that their relations consist in their composition. In p. 31, there is the following remarkable but just observation: "The systems of those who have wished to *arrange* fossils by their external characters * have already furnished a proof of the inconvenience

* I have been assured by an old pupil and relative of Werner, that it was not originally his intention to form any system of mineralogy on external characters,

nience of this method, as we there see fossils *essentially* different placed together, while those of the same species are dispersed in consequence of some *accidental* variety." In a note, p. 127, he speaks of "aggregated parts, or of those which we can obtain by mechanical division, and of those whose union forms the preceding, or of simple parts which are not divisible without changing their nature. As to those which *compose* the *simple* parts, and which, in fact, are themselves *compounds* (I cite the words of the author), they take the name of constituent parts. I shall, however, call primitive constituent parts those which form the constituents, and which are neither compounds nor aggregates, but absolutely simple parts or the first elements of matter." It appears to me that there is nothing in all that I have quoted, which has any reference to the form of the integral molecule. It is there said that bodies have molecules and elements; and we also learn something new, such as *simple part* which are *compounded*, and *compounds* of *composed parts*; but there is not a word of integral molecule.

To me, indeed, it appears rather censuring than excusing M. Werner, to say that the discovery of the integral molecule is due to this philosopher. If he had perceived its existence, why has he abandoned it? why did he leave a field so fertile in brilliant discoveries to be cultivated by any other than himself? But these questions are superfluous, as we have seen that the learned mineralogist (or, if he will, oryctognost) of Freyberg had no knowledge of the integral molecule before M. Haüy. Judging, indeed, from the observations which I have heard him make more recently on this subject, it does not appear that he has yet sufficiently studied the matter to comprehend it perfectly even at the present day.

[To be continued.]

characters, but merely to digest, arrange, or methodize those characters, so that various chemists might easily discover whether it was identically the same mineral which they analysed, and that they might have less trouble and be more accurate in their descriptions of the subjects either produced or operated on. In his preface, indeed, he observes explicitly, "It will be seen that I have taken care that *no one should make use of these external characters to establish a systematic division of minerals*, as has been hitherto done; but solely to determine the idea of their exterior appearance, and fix the method of *describing* them." Had he still adhered to this judicious and necessary plan, he would have contributed very materially to facilitate the progress of mineralogical science; but the vanity of making worlds, forming mountains, transitions, primitive rocks, and finally deciding on the effects of water and the construction of the whole crust of the earth, has propagated the propensity for "those *monstrosities* known under the name of *theories of the earth*," which flatter the imagination, but retard the progress of reason and true science.—Trans.

LXXVII. *Memoir on the Diminution of the Obliquity of the Ecliptic, as resulting from ancient Observations. By M.* Laplace. *Translated from the* "Connoissance des Tems for 1811" *by* Thomas Firminger, *Esq.*

To Mr. Tilloch.

Sir, The variation of the obliquity of the ecliptic having been a phænomenon in astronomy of a nature to engage the most lively interest of those who have made this sublime and useful science the subject of their study, and as its investigation has never been fully developed till the appearance of that profound work the *Mécanique Céleste* of M. Laplace, I have no doubt the following comparison of ancient observations with the deductions derived from his formula will be highly interesting to many of your readers. The article is taken from the *Connoissance des Tems* for the year 1811; and the only apology offered for its translation is the extreme scarcity of that work in this country: it was drawn up by the profound mathematician and philosopher above mentioned, with a view to compare his deductions with the actual state of the system at an interval of time as great as observations of sufficient accuracy would admit; and the coincidence, taking into consideration the imperfect state of science in those ages, is remarkably striking. It presents to us not only one of the most undeniable proofs of the Newtonian principle of gravitation, a fabric on which the whole of the *Mécanique Céleste* is founded, but furnishes the historian with facts which give additional credit to the faithfulness of the narration. On this as well as on other occasions we have a right to form our opinion from analogy. We see the recorded observations agree, as nearly as the state of science in those days will admit, with what theory has assigned to them; and as they do not make a separate history of themselves, but are coupled with the history of the times in which they were made, the known truth of the one gives a satisfaction to the mind in appreciating the value to be affixed to the other. If we look to the account given in the Lunar Tables published by the Board of Longitude in France, we shall find those tables were compiled principally from the astronomical observations made in the Royal Observatory at Greenwich; and not only the epocha, but the present state of diminution in the obliquity of the ecliptic has been in a great measure determined from them: we shall not, therefore, be surprised to find, hereafter, a nearer coin-

coincidence in the actual state of the ancient observations, and the deductions drawn from theory, when we possess, as we hope soon to do, the means of settling this epocha and actual state of diminution at the present period. The grand mural circle now making for the Royal Observatory, by Mr. Troughton, will, it is expected, be in readiness for observation early in the ensuing year; and we cannot doubt but that the first object to be determined with it, will be the settling of the above-mentioned data, so essential in the theory and practice of astronomy. The observations of Mr. Pond have clearly tended to show errors in the mural quadrants at Greenwich of considerable magnitude; and whatever accuracy these instruments might originally possess, we have now no hesitation, from a comparison of contemporary observations, not only of Mr. Pond, but of Mr. Groombridge, Dr. Brinkly, &c., to express our satisfaction on this point. Indeed it is not difficult to account for the above-mentioned errors, when we take into consideration the construction of the mural quadrants, and of the observatory in which they are contained. These instruments are freely suspended, in a line nearly with their centre of gravity, by two pins from a large stone pier; the upper and lower parts are therefore very differently affected by every change of temperature in the atmosphere: for instance, if we suppose an increase of heat to be equally diffused over the whole instrument; the upper part (for we must consider the expansion as taking place from the point of suspension) will be less affected than the lower, it having the action of gravity counteracting the force of expansion, whilst the lower part is assisted in its expansion from the same force of gravity, and *vice versâ;* or, in other words, with its own weight, which in these instruments is very great, the whole I think amounting to nearly 1000 pounds each. We shall therefore observe, that it is scarcely possible for these instruments to have retained their original accuracy for but a very short space of time after they were first put up: the change however might be inperceptible for several years; but as we now know that metals when continually acted upon, do not retain their first figure except where the parts are duly balanced, we can easily see how the change of figure must necessarily happen. In running our eye over the observations made by Dr. Bradley with Bird's mural quadrant, we find frequent mention, when observing the stars in the night-time, and particularly when the difference in the temperature of the atmosphere within and without was very considerable,

siderable, that it was necessary to lower the quadrant a little; and it was a practice always to keep the plumb-line constantly over the point at bottom, without any regard to the variable temperature of the instrument. Admitting, therefore, the instrument at the commencement of observation to have had an equable temperature and to have been nicely adjusted; it is evident that a readjustment, after a partial effect has taken place, is to throw so much error into the observation. I have often been surprised that so sagacious a man as Dr. Bradley should not have noticed this circumstance. It accounts in the most satisfactory manner for the disagreement in the deductions of the observation of the stars in the feet of Gemini, used in carrying forward the error of collimation deduced from comparative observations with the zenith sector and quadrant. The variations in these instruments are likewise rendered still greater from the construction of the Observatory, which, having a slanting roof, receives the sun's rays almost perpendicularly upon it, and is so much heated in summer as to occasion a change of temperature in the top and bottom of the room amounting to 10° or 12°. Such a roof is perhaps capable of many facilities in the opening and shutting the necessary shutters, but I think it is the very worst form for an observatory. The adoption of circular instruments in the stead of quadrants is one of the greatest improvements in practical astronomy. In the instrument now making by Mr. Troughton, any part may be made the depending one, and the whole instrument being constantly turned about must preserve its proper figure.

The article which I have herewith sent you, sir, being a very long one, I will not trespass further at present, intending at some future period to take an extensive view of the progress and improved state of practical astronomy.

Your very obliged
and obedient humble servant,
THOMAS FIRMINGER.

ALTHOUGH the successive diminution of the obliquity of the ecliptic, as we approach to modern times, cannot now be controverted, still it is with the greatest interest that we witness at the end of ages the slow development of the great inequalities of the system of the world. In after ages, when with the results of theory a long series of very accurate observations can be compared, this sublime spectacle can be enjoyed much better than it is by us, to whom antiquity has only transmitted such observations as often-

oftentimes are doubtful. But even those observations, when submitted to sound criticism, may, owing to the distance of time when they were made, throw great light on several important elements of astronomy, and therefore deserve all the attention of geometers and astronomers.

OF OBSERVATIONS ANTERIOR TO OUR ÆRA.

Chinese Observations.

The Chinese observations I am going to relate, are extracted from the "Edifying Letters on the History of Chinese Astronomy by the learned Father Gaubil," published by Father Sauchet, and particularly from a precious manuscript sent from China by the said Father Gaubil in 1734, which I have published in the *Conn. des Tems* of 1809.

The most ancient observation that has reached us, relative to the ecliptic's obliquity, is Tcheou-Kong's, the brother of Vou-vang emperor of China, who, towards the year 1100 previous to our æra, had occupied himself with particular care in making astronomical observations. After his brother's death, he was regent of the empire, and his memory is still in great veneration among the Chinese, as having been one of the best princes that ever governed. His observations on the length of the gnomon at the solstices are the most ancient astronomical observations that can be used. All anterior observations of eclipses and solstices that have reached us, are related in too vague a way to serve for astronomical determinations; they are of service only to enlighten chronology; and if other observations are wanted that can be truly useful to astronomy, we must go back from the epocha of Tcheou-Kong to the time when the lunar eclipse was observed at Babylon, the year 720 previous to our æra, as related in the Almagestes of Ptolomy. This great antiquity of Tcheou-Kong's observations, and their vast importance, induce me to expect that the details I am about to enter into respecting them will be perused with interest. Here, first of all, is what Father Gaubil records in his History of the Ancient Astronomy of the Chinese, inserted in the xxvith vol. of "Edifying Letters," p. 142.

"Tcheou-Kong, as well as his father prince Ou-en-ouang, and one of his ancestors, prince Kong-hicou, of whom mention has been made, took a delight in observing the shades of gnomons. In the town of Tching-tcheou he drew with care a meridian line, he levelled the ground for observation, he measured the shadow at noon and afternoon; at night he observed the polar star. This prince also caused observations

tions to be made in places westward, eastward, northward, and southward, of Tching-tcheou.

"In the town of Tching-tcheou, a gnomon eight feet long gave at noon, at the summer solstice, a shadow of one foot five inches. The declination of the sun being supposed 23° 29′, the observation of Tcheou-Kong gives the north latitude 34° 22′ 3″. The centre of the town Hon-an-fou has been observed at a latitude of 34° 43′ 15″, with an instrument made by Chapontot, by several altitudes of the sun:—difference between the missionaries' observations and Tcheou-Kong, 21′ 10″, of which quantity Hon-an-fou would appear to be more to the northward than given by Tcheou-Kong's observation. Although the exact situation of the town of Tching-tcheou cannot be ascertained, it appears that the difference from Hon-an-fou cannot give a difference of 21′ 10″. A want of accuracy in the observations, particularly in the gnomon, might produce a part of this difference.

"The missionaries supposed a declination in the ecliptic of 23° 29′: they applied refraction, parallaxes, and diameter of the sun, agreeably to de la Hire's new Tables, and thought they were sure of the adjustment of the instrument. The difference may also arise from some change in the obliquity of the ecliptic."

I shall in the first place observe, that the Chinese divide the foot into ten inches, the inch into ten fen, the fen into ten li, the li into ten hao, &c.: so that the length of the shadow is one foot five inches. As to the latitude of 34° 43′ 15″ of the town of Tching-tcheou, the same as has been designed by the names of Loyang and Hon-an-fou, Father Gaubil, in a note of the page just cited from "Edifying Letters," says that that observation was made in June 1712: that according to one observation this latitude was found 34° 52′ 8″; and to a second, 34° 46′ 15″; lastly, that a third gave 34° 43′ 15." This last appears to him preferable to the other two. The difference of these results proves the want of exactness in those observations, which, combined with the incertitude of the exact place of Tcheou-Kong's observations, would render it highly desirable to know the length of the shadow in the winter solstice, at the time of that prince.

This is what I find on this subject in the MS. cited by Father Gaubil. (*Conn. des Tems* for 1809, p. 393.)

"At all times the Chinese have observed the sun's shadow at noon, and at other times; but the most ancient observation we are possessed of is that of Tcheou-Kong, brother

brother of Vou-vang, in the town of Loyang. According o tradition, a gnomon of eight feet cast at noon a shadow one foot five inches long at the summer solstice. This shadow is mentioned in the ancient book of Tcheou-li, and in other books, and the authors of the Han consider the observation as incontestable.

"Loyang is the town of Hon-an-fou in Hon-an. According to Father Regis's observation, this town is placed at the latitude of 34° 46′ 15″. Father Demaille observed, together with Father Regis, as well at Cai-fong-fon as at Hang-tcheou.

"A shadow of one foot five inches from a gnomon eight feet long, gives a latitude of near 34° 22′, supposing the declination of the ecliptic 23° 29′. Tcheou-Kong governed the empire for his nephew in the year 1100 before Christ; and it was he that caused the imperial palace to be built at Loyang, which was a second court of Tcheou's empire. Therefore, if we were to admit a declination of 29° 55′ at the time of the observation, the latitude would be 34° 48′ 51″; which is remarkable.

"It was again a tradition, that in the winter solstice Tcheou-Kong observed with the same gnomon a shadow of 13 feet. This tradition is not so certain as the former. This shadow would give a true altitude for the sun's centre of 31° 18′ 42″. The summer shadow gives 79° 7′ 11″;—difference 47° 48′ 49″; half of which, 23° 54′ 24″ 30‴, would be the ecliptic's obliquity; which is worthy of remark. If the calculation of the latitude was made from the shadow at the winter solstice, supposing the declination 23° 29′, it would give a much more northerly latitude than what the altitude in the summer solstice gives."

In vol. ii. p. 21, of his History of Chinese Astronomy, published by Father Sauchet, Father Gaubil attributes the same observation to the authors of the Astronomy of Sfefen in the said town of Loyang. But in the manuscript I have just quoted, he relates what follows, (*Conn. des Tems* 1809, p. 394.)

The authors of Sfefen's Treatise of Astronomy have noticed for Loyang at the two solstices, the shadows observed by Tcheou-Kong, and recorded in the first observation. These authors have given shadows for the other days of the year in the equinoxes. These shadows are so faulty that no dependance can be placed on the observations. The authors no doubt considered Tcheou-Kong's observation as unreformable.

"In several treatises of Chinese astronomy, the shadows in in the solstices at Loyang, attributed to Tcheou-Kong, are first set down; after which rules are given to add to, or subtract from, the length of these shadows, according as the places are further north or south than Loyang. What I here mention is clearly explained in some of the said works; but in others, the editors have not been careful to give the rules for the increase or decrease of the shadows observed by Tcheou-Kong, for application to places further north or south; whence it arises that in calendars for Nanking or Hin-tcheou, or other towns, the shadows are given for Loyang only."

From the foregoing, it appears to me that no doubt can be entertained of the quoted observation not wholly belonging to Tcheou-Kong. The learned Freret has calculated this important observation in the third part of his excellent Dissertation on the Certainty and Antiquity of the Chinese Chronology. This is what he says:

"The most ancient observation of the solstices that is known with certainty is prince Tcheou-Kong's, brother to Vou-vang the founder of the dynasty Tcheou. Tcheou-Kong was regent of the empire from the year 1104 to the year 1098. The observation was made in one of these six years. The precise date of the observation for the time of the cycle and moon is not marked, but the place of observation and length of shadows are known. This detail is related in the Tcheou-li, which is a part of the Li-ki or Book of Rites.

"A gnomon was made use of, of eight feet Chinese: at the summer solstice the shadow was one foot five-tenths, and in the winter it was 13 feet; which gives for the obliquity of the ecliptic 23° 54′ 14″; the same quantity nearly as was supposed by the ancient Greek astronomers Pytheas, Eratosthenes, Hipparchus, and Ptolomy.

"The altitude of the pole at Loyang, (place of the observation) as determined by the altitude of the sun above the horizon and by the resulting obliquity of the ecliptic, is found 34° 47′ 33″. Regis and Mailla, by an observation made with accurate instruments, have found it 34° 46′ 15″. By the obliquity of 23,29 as supposed by our modern astronomers, Loyang would be placed at 34° 32′, differing only 15′ 13″; which gives room to presume that the obliquity of the ecliptic must have changed.

"The observation of Tcheou-Kong was made at a time anterior to Solomon's reign, and about the war of Troy. Its exactness proves that observations must have been made in China several centuries back."

Freret's

Freret's calculations want a slight correction. By rectifying them, and allowing for the refraction and the parallax of the sun supposed to be 8″,7, I find 79° 22′ 39″,6 for the altitude of the superior edge of the sun's disk at the summer solstice, and of 31° 35′ 1″,8 for that of the said edge in the winter solstice. By subtracting the apparent semi-diameter of the sun at the two solstices, which I find to be 15′ 47″,7 and 16′ 14″,3 respectively, the corresponding altitudes to the centre will be 79° 6′ 51″,9, and 31° 18′ 47″,5, which gives 23° 54′ 2″,2 for the obliquity of the ecliptic, and 34° 47′ 10″ for the polar altitude; which, being nearly a mean between the three observations of the missionaries, proves the accuracy of Tcheou-Kong's determinations.

Freret by certain and ingenious calculations had in the same dissertation fixed the epocha of Tcheou Kong's regency between the year 1098 and 1101, before our æra. I shall observe that in this respect he agrees perfectly with Father Gaubil. I shall then suppose that these observations were made in the year 1100 before our æra. I have given in the 3d vol. of my *Méc. Cel.* b. vi. ch. 12, a formula by which the obliquity of the ecliptic may be determined for a very distant period: and t expressing a number of years elapsed since 1750, the value of this obliquity in decimal degrees will be

$$26^\circ,0796 - 3676'',61 - \cos(t43'',0446) - 10330,4 \ \sin\ (t99'',1227)$$

whereby $t = -2850$, which gives in decimal degrees the corresponding obliquity of the ecliptic $= 26^\circ,5611$, or in ordinary degrees $= 25^\circ\ 51'\ 53''$; which must be increased about 5″, because the obliquity of the ecliptic in 1750 exceeded to that amount the quantity used in the preceding formula: thus 1100 years before our æra, the obliquity of the ecliptic was 23° 51′ 58″,—a result which only differs 2′ 4″ from that given by the observed lengths of the gnomon shadow in the two solstices. A more perfect coincidence cannot be wished for, if allowance be made for the uncertainty attending this sort of observation, owing particularly to the penumbra which renders the shadow ill-defined.

If, together with Father Gaubil, the observation alone of the summer solstice was taken into account, and the polar altitude at Loyang was supposed with him to be 34° 43′ 15″, by subtracting its complement 55° 16′ 45″ from the altitude 79° 6′ 52″ of the sun's centre, determined by the length of the shadow in the summer solstice, the obliquity of the ecliptic would be 23° 50′ 7″. The result of my formula is very nearly a mean between that and the obliquity given

given by the observed lengths of the shadow at the two solstices. This coincidence is a remarkable confirmation of the value of the masses of Venus and Mars, which M. Delambre has determined by the comparison of a very great number of observations of the sun by means of the formulas, and of the perturbations of the earth's motion I have given in the 3d vol. of *Méc. Cel.*

Tcheou-Kong, by his observations, had determined the moment of the winter solstice, but they have not been transmitted to us. We only know that he fixed this solstice at 2° Chinese from V, a constellation which begins at ε of Aquarius, (vol. xxvi. of Edifying Letters, p. 124.) We shall also fix the epochas of this determination at 1000 years before our æra. Tcheou-Kong and the Chinese astronomers at that time referred the constellation to the equator; besides 2° Chinese = 1° 58′ 17″: subtracting this from 270°, the difference 268° 1′ 43″ was the right ascension of ε of Aquarius at the epocha of 1100 years before our æra.

In the beginning of 1750, the longitude of ε of Aquarius was 308° 14′ 10″; its latitude was north 8° 6′ 20″.

Comparing Bradley and Mayer's catalogues with Piazzy's, this star does not appear to have any sensible motion of its own, and its annual precession is 50″,1.

I find by the formulas of c. xii. of book vi. of *Méc. Cel.* for the epocha of 1100 years before our æra,

$$\psi = 40° \; 2' \, 43''$$
$$V = 23° \, 32' \, 49'',$$

ψ being the precession of the equinoxes from that epocha till 1750. This precession being referred to the equinox of 1750, is the obliquity of the equator on this ecliptic at the same epocha. Thus at this epocha the longitude of ε of Aquarius, computed from the intersection of the equator with the ecliptic of 1750, was in the year 1100 before our æra 268° 11′ 27″; whence I conclude its right ascension, relatively to the same intersection, to be equal to 268° 9′ 2″.

I afterwards find, by the formulas of the quoted chapter,

$$\varphi'' = 25' \, 44''; \; \theta = - \, 1° \, 33' \, 25'' :$$

φ″ being the ecliptic's inclination from that time, above that of the ecliptic of 1750, and θ being the longitude of its node upon that said ecliptic, computed from the fixed equinox of 1750. Whence I conclude that the right ascension of the true equinox with the preceding; that is to say, the equator's intersection with the fixed ecliptic of 1750, was in the year 1100 before our æra equal to − 42′ 12″; the right ascension of ε, relative to the true equinox, was therefore then 268° 51′ 14″, greater by 49′ 31″ than Tcheou-Kong's

Kong's determination. This difference will appear very small, when we consider the uncertainty of the precise epocha of the observation on which this determination is founded, and above all the uncertainty even of the observations. It would suffice to remove 54 years beyond the 1100dth before our æra, to reduce this difference to nothing, and then the observation would belong to the time of Ou-en-ouang, father of Tcheou-Kong, whom Father Gaubil mentions as having much loved and cultivated astronomy. The Chinese astronomers determined the moment of the solstice, by observing equal lengths of the gnomons' shadows forty or fifty days previous and after the solstice; and from that there may already be some error in Tcheou-Kong's determination. But the greatest error that is to be apprehended in the observation is in the manner of referring the solstice to the stars, in order to which the moment of the passage of such stars as crossed the meridian twelve hours after the moment of the solstice was observed: thus the right ascension of the opposite point to the summer solstice would be determined, and therefore also that of the winter solstice. But for so doing it was necessary to measure an interval of twelve hours. It appears that hour-vessels were used for measuring the time that a vessel was in filling to different heights with the water falling from a higher vessel (Treatise of Chinese Astronomy of Father Gaubil, published by Father Sauchet, Part I. p. 37.) It is easy to perceive how uncertain this manner of measuring time was, and three minutes of time, in an interval of twelve hours, are sufficient to account for the error of Tcheou-Kong's determination. The Chinese astronomers made likewise use of the moon's situation relatively to the stars in the lunar eclipses, to obtain the place of the sun, and therefore that of the winter solstice, at which they fixed the commencement of their year.

We must come down a thousand years, from Tcheou-Kong's epocha, before we find a second observation of the gnomons' shadows made in the solstices in China. Towards the year 104 before our æra, the astronomers Lieou-hiang and Lo-hia-hong observed the length of the shadow of an eight-feet gnomon at the winter and summer solstices. They found it 13 feet one inch four fen, or 13ft,14 at the former, and one foot five inches eight fen, or 1ft,58 at the latter (vol. ii. of Chinese History, published by Father Sauchet, p. 8). This observation is supposed to have been made in the town of Siganfou, then the capital of the empire: but this is an error which Father Gaubil has rectified in the quoted manuscript; in which is read as follows: (*Conn. des Tems*, 1809.)

"Lieou-hiang, father of Lieou-hia, wrote upwards of 50 years before Christ. This author says that an eight-feet gnomon gave the noon shadow in the winter solstice 13 feet one inch four fen, in the summer's it was one foot five inches four fen. Litchun-foung, an astronomer of the dynasty of the Tangs, complains that these shades were improperly applied to Siganfou. Lieou-hiang mentions neither the place nor the time of these observations."

The shade at the summer solstice is not exactly the same as that published in the quoted History of Chinese Astronomy; but I think that this last ought to be preferred, the shadow given in the MS. giving an evidently too considerable obliquity of ecliptic. It is very likely that in the manuscript Father Gaubil may have written, in a mistake, instead of eight fen the same number that he wrote for the winter solstice. Adopting therefore, 1ft,14 and 13ft,58 for the lengths of the shadows at the summer and winter solstices, and allowing for the refraction and the sun's parallax, I find 31° 2′ 23″ and 78° 33′ 41″ for the altitudes of the sun's centre, resulting from these observations. Half of their difference gives 23° 45′ 39″ for the ecliptic's obliquity. If we add it to the complement of 78° 33′ 41″, we shall have for the altitude of the pole 35° 11′ 58″, an altitude very different from that of Siganfou, which the Jesuits have found 34° 16′ 45″. Litchun-foung was therefore right to complain that these noon shadows had been improperly referred to Siganfou.

To compare my formula with this observation, I suppose that $t = -1850$, and then it gives for the obliquity of the ecliptic 23° 43′ 59″,4, and by adding 5″, as we have done for the preceding observation, we shall have 23° 44′ 4″,4, which only differs 1′ 34″,6 from the result of this second observation. These two observations are the only ones before the commencement of our æra, that Father Gaubil has made us acquainted with; and it is to be supposed that this learned missionary could not discover others: the destruction of books by fire, which took place 213 years previous to the Christian æra, having caused the loss of the greatest number of preceding observations.

[To be continued.]

LXXVIII. *Re-*

LXXVIII. *Reply to Mr. M.'s Remarks on Mr. Smyth's Comparative Table in* vol. xxxv. p. 488. *By Mr.* Smyth.

To Mr. Tilloch.

Sir, I shall esteem it a favour if you will insert the following answer to the gentleman who signs himself M. in your Magazine for September.

Mr. M. says it is "curious that Mr. S. should presume organ-tuners will continue to tune as their ancestors did before them, till irrefragable arguments are produced to prove the superiority of Kirnberger's temperament." Here, I confess, I stand convicted of inconclusive reasoning. The fact, however, I imagine to be this: an organ, with compound stops, will not admit of the major thirds being tuned sufficiently sharp to ameliorate, in any considerable degree, those greatly tempered chords which are called *wolves;* of which I wish the breed were extinct.

I am glad to find that Mr. M. agrees with me in opinion, that Kirnberger's is one of the *worst* unequal temperaments. Had Mr. M. stated in definite terms his own favourite system, it should have been submitted to examination.

Mr. M. says, "*perhaps* for the organ a good *unequal* temperament is preferable to the *Isotonic.*" I was not ignorant that even for this instrument the Isotonic has had its advocates; and Mr. M. presents to my view the names of Couperin, Marpurg, Rameau, Cavallo, professor Chladni, and many other eminent philosophers. Now, not being a philosopher myself, I take the liberty of asking one plain question, which relates solely to the temperament of the organ:—Can any man living *prove*, that there ever was one organ in Christendom tuned according to the *equal temperament*, in consequence of a peremptory order from any one of these gentlemen, and suffered to *remain* in that state? This is coming to the point.

A person disposed to cavil might raise arithmetical and philosophical doubts whether a *real* equal temperament has ever been heard.

I wish Mr. M. would inform us, and explain precisely, *what the system is* which he tunes so dexterously on his harp, by the *melody alone*, without striking consonances. Had his instrument so tuned been intended for melody alone, this mode of tuning might answer the purpose; but, as each of the strings has various relations to other strings, and a temperament of a diatonic interval, too small to produce a sensible effect in melody, will produce a very sensible effect in harmony, I congratulate Mr. M. upon a power which I never had the felicity of seeing exercised by any one person.

Never having heard of such a writer as *Eximeno*, I referred to Dr. Burney's History of Music, and there learn that Eximeno was possessed of eloquence, fire, and a lively imagination; but that his book has been called, in Italy, "a whimsical romance upon the art of music, in which he discovered a rage for pulling down, without the power of rebuilding."

I have annexed, in compliance with Mr. M.'s request, the beats of mean tone temperament in one second. I need not add that, by taking the first decimal, the beats will be obtained for ten seconds, which I would recommend. I have also subjoined the beats of Mr. Marsh's System; and wish to be informed by that gentleman if he has had an *organ* tuned according to this system.

Persons unacquainted with the theory of the beatings of imperfect (that is, tempered) consonances, may object to a table of beats, that so large a number as 80 or 50 in ten seconds cannot be counted. For the information of these gentlemen, I add, that no one can count these beats: in fact, they rather *howl* than beat; but they necessarily result from the temperament of the *slowly* beating consonances by which the temperament is laid.

I remain, Mr. Editor,
Yours, &c.

Norwich, Dec. 5, 1810. C. J. SMYTH.

P. S.—Please to correct an erratum in my paper, p. 250, second line from the bottom, for *policity* read *policy*.

MEAN TONE TEMPERAMENT.

Beats in one Second.

C	480.	8.	III.	4.	Vth.	6.	VI.
B	448·6065	8·366	53·8099	5·5740	4·1755	0	6·9675
Bb	429·3227	68·1522	0	5·3410	3·9793	80·4716	6·6893
A	401·2438	7·4628	0	4·9886	3·7314	0	6·2470
*	375·	6·9675	45·	4·6704	23·4212	0	40·9832
G	358·8838	6·6883	0	4·4648	3·3422	0	5·5310
*	335·4110	6·2470	40·2358	4·1755	3·1194	0	45 5768
F	320·9972	50·9832	0	3·9793	2·9916	60·1936	4·9870
E	300·	5·5810	0	3·7314	2·7870	0	4·6704
Eb	287·1053	45·5768	0	23·4212	2·6705	53·8099	4·4735
D	268·3273	4·9878	0	3·3422	2·1943	0	4·1830
*	250·7784	4·6704	30·0968	3·1194	2·3352	0	34·0761
C	240.	4·4735	0	2·9916	2·2424	45.	3·7314

Mr. MARSH'S

Mr. Marsh's System.

Beats in one Second.

C	480·	3.	III.	4.	V.	6.	VI.
B	451·3838	18·3838	31·4802	2·3802	2·5306	17·7984	15·4250
Bb	48·2557	40·0412	10·6067	3·2066	2·4007	47·0736	14 7447
A	402·7338	16·4028	9·9734	3·0162	2·2478	15·8794	13·7622
*	378·7240	15·4250	26·3800	2·8758	8·0076	14·9330	29·9746
G	359·3272	14·7447	8·8992	2·6912	2·0140	14·1686	12·2794
*	337·9052	13·7622	23·5368	2·5306	1·8994	13·3226	26·7434
F	320·5991	29·9746	7·9397	2·4006	1·7973	35·2398	10·9559
E	301·4859	12·2794	7·4665	2·2478	1·6901	11·8872	10·3023
Eb	286·0449	26·7434	7·0843	8·0076	1·6033	31·4802	9·7755
D	268·9919	10·9559	6·6613	2·0140	1·5081	10·6067	9·1919
*	352·9553	10·3023	17·6199	1·8944	1·4379	9·9734	20·0206
C	240·	9·7755	5·9436	1·7973	1·3456	26·3800	8·2014

LXXIX. *Copy of the Instructions given to their Engineers by the Commissioners appointed to inquire into the Nature and Extent of the several Bogs in Ireland; with further Particulars respecting the Bog of Allen, and its Substrata; accompanied with a transverse Section of Lullymore Bog, reduced from Mr.* Griffith's *Sheet Section. By Mr.* William Farey.

The secretary of the commissioners appointed to inquire into the nature and extent of the several Bogs in Ireland, and the practicability of draining and cultivating them, is directed by the Board to communicate, for the information of the engineers who may be employed, and of the proprietors of bogs, the mode in which they have been advised to proceed.

"1st.—They propose to divide the Bogs of Ireland into districts, and to assign each district in charge to one or more engineers.

"2d.—Each of the engineers is to provide a sufficient number of assistants, for whose qualifications he is to be responsible.

"3d.—The commissioners think it necessary to direct the attention of their engineers to the particular heads of inquiry contained in these instructions; but it is by no means intended to confine their judgement within these limits; on the contrary, where local circumstances point out a preferable mode of proceeding, the commissioners expect that it shall be fully stated, in addition to the information on the different points which they now suggest.

"4th.—They conceive, that the first steps towards the drainage of an extensive bog, should be to ascertain the proper

proper lines of direction for one or more main drains passing through it, to give vent to the waters which it contains, and for catch-water drains laid out along its edges, to intercept the springs and streams which flow into it from the adjoining lands.

"5th.—In laying down the situation of the main drains, the engineers are to consider, not merely the general declination of the bog towards the rivers or such or other natural outlets as may best answer for their drainage, but to keep in view the further object, where practicable, of converting these main drains, either immediately or ultimately, into channels of navigation, for the conveyance of the future productions of the bog, and of providing for the connexion of those navigable drains, where convenient, with the great lines of navigation already subsisting: where this is not possible, they are to consider how these different drains may be united to other canals, which may be formed hereafter. In laying down the situation of any navigable drain, the engineers are to attend to the situation of such manures as may be most suitable for the bog.

"6th.—Where the main drains are likely to be used as canals, they are not, in any instance, to be less than 14 feet broad at bottom, and five feet deep from the water surface. The breadth and depth of other main drains, and of the catch-water drains, must be proportioned to the quantity of water which they are to discharge.

"7th.—As there are no districts which are more liable to the inconvenience of a total want of water in dry summers, than level tracts of marshy ground, when once their drainage is effected, care must be taken in laying down the direction of the main drains, to allow them, where practicable, to be occasionally dammed up, so as to raise the water within two feet of the surface, for the purpose of promoting vegetation; and that the catch-water drains, in like manner, should supply water on the surface, for the use of cattle, or for the purpose of irrigation, where the mode of improvement shall be deemed advisable; and in situations where a sufficient supply cannot be procured by these means, the engineers are to consider where reservoirs may be most advantageously constructed, to be supplied, in time of flood, from the catch-water drains or rivulets in the vicinity of the bogs.

"8th.—Where locks may be necessary, the dimensions which the commissioners recommend are,

	feet.	in.
Length	70	0
Breadth	7	3
Depth over the fill of the gates ..	4	0

"9th.—In

" 9th.—In all cases where the bogs are wholly or partially surrounded by high land, whose natural inclination is to the bog, the engineers are to consider where catch-water drains may be necessary; and as it will generally happen that these catch-water drains may admit of a greater fall than it will be practicable to give to the main drains, care must be taken, where the catch-water drains are to join the main drains after their issue from the bog, that it shall be at such a distance, or on such a level, as to preclude the danger of the water to the catch-water drains, in time of floods, penning back the water of the main drains, so as to overflow the bog. Where the levels will not admit of the waters of the catch-water drains being conducted into the main drains without being subject to this inconvenience, provision must be made for conducting them through separate channels into the river, or other place, where the waters of the bog are to be discharged.

" 10th.—Each of the engineers is to prepare a map of the district assigned to him, distinguishing,

" 1. The extent and boundaries of the bogs which it contains.

" 2. The nature of the soil and country immediately contiguous to each bog, particularly specifying the situations of lime-stone, lime-stone gravel, marle, or other manures.

" 3. The surface of the bog, whether firm black bog, or shaking quagmire.

" 4. The situation of any springs, rivers, or lakes, which appear to occasion the wetness of any of the bogs.

" 5. The course of any rivers, streams, roads, or canals, by which the bog is intersected.

" 6. The drains and other works proposed by the engineers.

" 7. Such lines of new roads as appear most proper for the carriage of manure, for carrying out the future produce of the reclaimed bogs, and for communication with the roads in the vicinity.

" 11. These maps are to be accompanied with sections, delineating the surface and bottom of the bog, and the nature and depth (as far as may be necessary) of the under strata on which it rests.

" 12. The maps and longitudinal sections are to be drawn on a scale of four inches to an Irish mile, and the perpendicular scale of the sections to be $\frac{1}{20}$ inch on the foot.

" 13. They are to be accompanied with index maps, on the scale of one inch to a mile.

" 14. In taking the levels necessary for determining the sections, the engineers are to take care that their assistants proceed, in all cases, so as to cross and correct each other. The engineers are to be responsible for the correctness of the whole.

"15. The main drains are generally to be laid down so as to allow the collateral drains communicating with them, to embrace the greatest extent of surface the nature of the bog will admit of: but where the inequalities of level in its surface, or the outlets of discharge for the waters, present a choice of plans for its drainage, so as to induce any doubt in the mind of the engineer which plan may be most eligible, he is to submit the different plans to the commissioners.

"16. The engineers are to accompany the maps with written reports, containing generally whatever occurs to them on the subject of the drainage of the districts assigned to them, and particularly specifying,

"17. The probable expense of such drains, roads, canals, locks, and other works, as they recommend.

"18. The names of the proprietors who claim any right or interest in the bogs, and to what extent, and in what proportions, as far as they can learn.

"19. Whether any, and what tracts of bog in their districts have already been reclaimed, and what have been the manures used, and the modes pursued, in their amelioration, and what is the nature and the state of the crops which they actually produce.

"20. The probable value of the land when reclaimed, and the mode of culture which may be the best adapted for it, particularly distinguishing those parts that may be best suited for planting.

"21. Where any of the bogs proposed to be drained are at present used for the supply of fuel, the engineer is to report how far the quantity and quality of the fuel is likely to be injured or improved by the works which he recommends.

"22. Where the wetness of the bog appears to be occasioned by a lake on a higher level, the engineer is to report on the practicability and means of draining the lake; and also on the difference of levels in summer and winter of all rivers and lakes connected with the bogs.

"23. Where the bottom of the bog is lower than the river into which it would be convenient to discharge the waters of the drains, the engineer is to report on the practicability of lowering the river sufficiently to receive them.

"24. As in many instances the levels may not admit of the bogs being drained in the usual manner, in such cases

the

the engineer is to take into consideration the propriety of draining it by means of wind-mill pumps or other machinery; the expense of erection, and annual charge of which, he will include in his report.

"25. The engineers are further to consider what situations may best answer for the corn-mills which may become necessary, in consequence of the increased tillage of the reclaimed districts, and how far the water of the drains may be used in working them; and they are particularly to inquire as to the situations and circumstances of such mills already in existence, the supply of whose water may be affected by the projected drainages, and to consider and report, whether it be most expedient to provide reservoirs for their supply, or to purchase the interest of the proprietors.

"26. They are also particularly to report, where any of the proposed works appear likely to diminish the supply of water for the Grand or Royal Canals, or other navigations, or to interfere with their levels or embankments; and in what manner such injuries may be best obviated.

"27. In order to connect the respective surveys with each other, and to enable the commissioners to judge how far these drains may be applied to the purposes of internal navigation, they propose to direct, that the engineers to whom the districts nearest to Dublin may be allotted, shall ascertain with the utmost accuracy, the difference of level between the levels in their maps, and of the platform on the capital of the column erected in the memory of Lord Nelson; and to communicate the difference of level to the engineers who may have districts immediately beyond them, for the purpose of carrying forward the comparison. The commissioners intend afterwards to request the Ballast Office to mark at the Pigeon House Dock the level of high water in an ordinary spring-tide in the Bay of Dublin, so that by determining the difference of level between that and the platform on the column, the difference between the level of the sea and the various levels which are to be taken in pursuance of these instructions, may be correctly ascertained.

"28. To enable the commissioners to complete the connexion of the surveys by trigonometrical observations, if such should hereafter be deemed expedient, the engineers are to have permanent marks at the extremities of the several levels, and to lay down all remarkable objects which are likely to be permanent, such as raths, towers, castles, cairns, hill-tops, market-houses, &c.

"29. The various lines of levels are to be shown on the map by dotted lines.

"30. The

" 30. The engineers are to act under these instructions, in respect to all such bogs within their districts as contain by estimation or repute more than 500 Irish acres; of bogs of inferior extent, they need only report the existence and situation.

" 31. The commissioners intend to provide the best levelling instruments, which they will supply at the original price to such of the surveyors employed as are not already furnished with instruments of sufficient accuracy. They intend also to procure some rain-gauges, to enable them to determine the dimensions of the principal drains. They request that any gentleman disposed to assist by keeping an account of the rain in the vicinity of the bogs, will be so good as to signify his intentions to their secretary.

By order of the Board.

Dublin Society House,
Sept. 28, 1809.

B. McCarthy,
Secy to the Commissioners.

ADDITIONAL INSTRUCTIONS.

" 32. The commissioners have determined to alter the longitudinal scales of the sections referred to in the twelfth article of the instructions, from eighty perches to an inch (as therein directed) to forty perches to an inch, the scale which on further consideration they have preferred to adopt.

" The perpendicular scale of the sections (namely $\frac{1}{20}$ inch to a foot) is to continue unaltered.

" 33. As it will probably be found inexpedient to incur the expense of engraving the drawings of the sections, referred to in the eleventh article of the instructions, the engineers are to specify the lines of sections, and the amount of fall in each in their reports; and further to specify them upon their maps, so far as they may find it not inconvenient to do so.

" 34. The commissioners have determined that it will not be necessary (at least for the present) to execute the index maps referred to in the 13th article of the instructions.

" 35. The engineers are to prepare, for the purpose of being presented to parliament, maps on the scale of two inches to an Irish mile, reduced from their large maps, specifying every thing contained in the large maps. These reduced maps are to be sent in along with the reports of the engineers to the commissioners. The largest sized copper plate that can be allowed for these maps is twenty-six inches long by twenty broad. Where the district is so large that a map of the whole of it cannot be contained within these dimensions,

dimensions, the engineer must consider how the district may best be subdivided.

"36. The engineers are in all cases, both in their maps and in their reports, to express the contents of the bogs, both in Irish and English acres, and also to insert in their maps, scales of both Irish and English miles.

"37. The commissioners not judging it expedient for the present to lay down a meridian for the purpose referred to in the twenty-eighth article of their instructions, consider it sufficient that the engineers should construct their maps upon the magnetic meridian, the north of the magnetic meridian pointing to the top of the map, and the meridian line being parallel to the sides of it.

"38. The estimates referred to in the seventeenth article of the Instructions, are to include all the expenses, which in the judgement of the engineer will be necessary to reduce the bog to such a state that it shall be ready to receive agricultural improvements. These estimates are however to distinguish the expenses of the different descriptions of works, and of the different classes of drains recommended.

By order of the Board.

Dublin Society House,
May 16, 1810.

B. McCarthy,
Secy to the Commissioners.

To Mr. Tilloch.

Sir,—Having as an exercise, by direction of my father, reduced Mr. Richard Griffith's large section across the Bog of Lullymore, so as to agree in scale of length and in position nearly, with his map of this bog, printed in the 1st Report to Parliament on the Bogs in Ireland, from which you gave some extracts in your last number, and distinguished therein all the proposed drains, and shown by arrows whether they run northward (up) or southward (down), I take the liberty of sending a copy, and perhaps you may deem the same worth a plate in a future number of your Magazine, in order to explain, as it does (see Plate X.), the uneven surface and variable thickness of the peat in these vast bogs, the uncertain thickness and existence of the alluvial yellowish blue clay, (No. 10. in your last number, p. 371,) on which the peat frequently rests, and the very uneven and undulating form of the great bed of alluvial clayey limestone gravel, of vast thickness, which forms the floor and borders of this and most others of the bogs of this part of Ireland, except in a few places where strata appear,

appear, according to the report which Mr. Griffith, jun. has made on the subject; wherein, p. 15 and 16, the tract of land called the Island of Allen is thus described:—

"The surface of the Isle of Allen rises very quickly from the bog on all sides, particularly to the north-west, where it is composed (at least to a considerable depth) of limestone gravel, forming very abrupt hills. In those places where the face of the hills has been opened for the purpose of raising stone and gravel, the mass is composed of rounded limestone, varying in size from two feet in diameter to less than one inch; the largest are not so much rounded as the small, frequently their sharp angles are merely rubbed off; they are usually penetrated by contemporaneous veins of Lydian stone, varying in colour from black to light grey; the colour of the limestone is usually light smoke grey, rarely blueish black; when it is, the fracture is large conchoidal; that of the grey is uneven, approaching to earthy.

"The Lydian stone, when unattached to the limestone, has usually a tendency to a rhomboidal form, sometimes cubical, the edges are more or less rounded, the longitudinal fracture is even, the cross fracture is conchoidal.

"From the strong resemblance that subsists between the rolled limestone and its accompanying substances, and the upper beds of the limetone strata, which extend from the county of Tipperary, through Kilkenny, (where the lower beds are used for marble,) Carlow, Queen's County, King's County, Kildare, Meath, Westmeath, Dublin, &c. &c. there can be no doubt that the least accumulation of limestone gravel, which nearly covers the whole province of Leinster (forming steep ridges of hills frequently above 300 feet high, and sometimes approaching the summits of lofty primitive mountains) did originally form the upper beds of the limetone strata, which when now found in situ are seldom firm, on account of their alternating with thin beds of slate clay, usually much decomposed, and their being traversed by numerous fissures and veins of calcareous spar and Lydian stone.

"It is much more difficult to trace the course of the currents which first removed the limestone from its native bed, and afterwards having rolled the detached masses backwards and forwards, deposited them on the sides of hills, whose base had withstood the action of the waters, or by cross currents and eddies formed independent hills and minor ridges, the deposition of which, together with a subsequent deposition of a bed of clay, varying from one to six feet in in thickness, and which almost universally covers the surface of the gravel, by obstructing the course of the waters in a country having naturally but little fall, may, by creating a general stagnation in them, and thereby forming extensive shallow lakes, have caused the growth of the *Sphagnum palustre**, and other aquatic mosses and plants, of which the mass of our bogs is composed.

" This island, though separated from the southern range of hills by a low boggy valley, may on a general view be considered as a continuation of that range.

" Perhaps a short geological description of this ridge, (which, on account of its height and steepness, forms the most prominent and interesting feature in the county,) though apparently foreign to the general object of this report, may (by preventing ignorant people from searching for limestone and other manures where they do not exist) be considered as an useful and necessary appendage.

" Near Ballyteague Castle, in the northern edge of the Island of Allen, stratified limestone makes its appearance at the surface, dipping 20 degrees east of south, at an angle of 5 degrees from the horizon. The stone is principally used for building, as on account of its containing a large proportion (according to my analysis 15 per cent.) of silex, it requires much fuel to burn it into lime.

" The next rock visible crops out about two miles to the southward of Ballyteague, at the base of the Hill of Allen near the village called the Leap of Allen, the intermediate country being covered by hills of limestone gravel; it is a species of conglomerate, composed of rounded quartz pebbles, varying in size from minute sand to six inches in diameter, connected together by a red iron-shot, argillaceous cement; then beds of a deep brick red slate; clay much interspersed with mica is found interstratified with the conglomerate: the dip is 30 degrees east of south at an angle of 7 degrees from the horizon. Southward of this quarry, rises the Hill of Allen, a very steep conical hill about 300 feet high (reckoning from its base); it is composed of an irregular unstratified mass of fine-grained greenstone, the crystal of hornblende and feldspar being very minute; transparent calcareous spar is frequently observable in the mass, rarely large crystals of feldspar are found interspersed; the rock on approaching the summit of the hill becomes more crystalline, detached masses of beautiful porphyretic greenstone thickly studded with large crystals of

* Bog Moss.

feld-

feldspar, are frequently to be met with on the surface: I did not find any of this rock in its native bed.

"The hill called the Chair of Kildare and Dunmurry Hill, situated to the south-west of the Hill of Allen, are also composed of greenstone*; the Red Hills are conglomerate.

"Besides the general ridge which (with the exception of two low passes through which the bog rivers flow) surrounds the district, and the Island of Allen which divides the interior, there are frequently minor and more detached ridges, usually of moderate elevation, bounding the several bogs, and preventing the passage of the waters to the rivers or principal streams, which usually run in valleys beyond the ridges, and nearly parallel to the edge of the bogs.

"These interior ridges, where there is no river, usually form the line of separation between different bogs."

I am, sir,
Your obedient servant,
WILLIAM FAREY.

LXXX. *A short Account of the Improvements gradually made in determining the Astronomic Refraction. By Mr.* T. S. EVANS, *Master of the Mathematical School at New Charlton, near Woolwich, Kent; late of the Royal Observatory, Greenwich, and of the Royal Military Academy, Woolwich.*

[Continued from p. 349.]

IT would be endless to notice the different opinions respecting both the terrestrial and the astronomic refraction which are to be met with in the writings of various authors on the subject: and it would be equally useless to notice all the tables of its quantity given by them, some of which differ very much from others. It will be sufficient to mention those only who made some considerable advances towards obtaining it with greater accuracy.

The next of these in order was La Caille†, who in determining it certainly bestowed very great pains, by making and reducing an immense number of observations, and afterwards comparing them with others made at Greenwich by Dr. Bradley, at Gottingen by Mayer, at Bologna by Zanotti, and by La Lande who was then at Berlin. From these it appeared that the refraction at 45° of altitude was $66\frac{1}{2}''$; but this, as will hereafter be seen, was too great by

* This is the first discovery of rock of the trap formation in this part of Ireland.

† Mem. de l'Ac. de Sc. 1755, p. 547.

by some seconds. In his paper on the subject, which is divided into four parts, he proves, first, that the mean refractions are very nearly the same for the same apparent altitudes throughout the whole extent of the temperate zone; since those which were observed at Paris did not exceed those observed at the Cape of Good Hope but $\frac{1}{40}$ at most. In the second he determines the absolute quantity of the mean refraction for the apparent height of the pole at Paris, and gives the result of his observations with regard to the latitude of Paris and of the Cape of Good Hope. In the third he gives his table of mean refraction, and another of corrections depending upon the state of the barometer and thermometer; concluding with some reflections on its construction and use. In the fourth he compares his new table with the most celebrated of those that had before that time been in use among astronomers; and he then shows how it agrees with the observations of Bradley, Zanotti and Mayer.

But by La Caille's Memoir * it appears, that previous to this time M. Mayer had formed and communicated to him a table of astronomic refractions which he computed by means of an algebraic formula†, the coefficients of which he deduced from his own observations, and took into account the variations relative to those of the barometer and thermometer. He found the alteration of refraction for a depression of 15 lines in the barometer, the same as for a rise of 10 degrees in the thermometer, and the variation for each degree of the latter, according to his table, $\frac{1}{22}$ of the whole mean refraction, which he adapted for 28 inches of the barometer, and 0° of the thermometer‡. This proportion takes place down to 80° of zenith distance. Mayer considered also that the mean refraction is the same for all parts of the earth; and that the only variation which takes place, depends on the changes of the § weight and temperature of the atmosphere.

La Caille, in comparing Mayer's Table with observations,

* Mem. 1755, p. 555. † Vid. Mayer's Tables, 1770.

‡ French measure and Reaumur's therm.

§ It was perceived that the refraction near the horizon at Paris is sensibly affected by vapours, and the smoke which arises from the city, situated north of the observatory. Exhalations and the moisture of the atmosphere have certainly a considerable influence on it, and so has the situation of the place, being more or less elevated. The neighbourhood of a city, mountains or hills, forests, rivers, or marshy plains likewise affect it much; and La Caille was persuaded that an astronomer never had refractions purely celestial near the horizon, that is, of the nature of those 20° above it; local circumstances producing such considerable differences in them that he did not choose to insert in his table those for altitudes below 6°. Cassini de Thury believed that the refraction and its inequality were greater at Paris under similar altitudes on the south side than on the north, and at 4° he found it 20″ more. *Encycl. Meth.*

found

found that his correction for the thermometer was a little over-rated; and accordingly, for his new table, altered it to $\frac{1}{87}$ for each degree. And here it may be observed that La Caille did not correct his altitudes above 36° at Paris, and 30° at the Cape; first, because he only noted the barometer and thermometer in the night, when he observed stars below 30° of altitude. Secondly, because, that at 36° of altitude, where the mean refraction is about $1\frac{1}{2}$ minute, the variation which belongs to 10 degrees of the thermometer only amounts to $3\frac{1}{2}$ seconds; a quantity about equal to the limits of the errors of observations made with an instrument of six feet.

The formula given by Euler * appeared also about this time. It took into account the variation of the refraction depending upon the thermometer and barometer, but was certainly too complicated to be generally adopted. He shows however, that in very different hypotheses the refraction will be sufficiently exact, if taken in the inverse ratio of the degrees of heat, when the star or planet is not too near the horizon, but the precise quantity of this ratio was unknown to him.

In this state the refraction stood when Dr. Bradley took the subject into consideration, and began to find its quantity from his own observations. The rule which he adopted, although a very elegant one, he neither lived to complete nor to present to the world; but it was published after his death by Dr. Maskelyne †, and has commonly been used in England up to the present time. He found the mean refraction at 45° of altitude 57″, and, that at all other altitudes it was equal to 57″ multiplied by the tangent of the zenith distance, diminished by three times the refraction. Then supposing the mean state of the atmosphere to be at 29·6 in. of the barometer, and 50° of Fahrenheit's thermometer, he made the true or corrected refraction equal to $57'' \times t,(\text{Z.D.} - 3r) \times \frac{\text{barom.}}{29{\cdot}6} \times \frac{400}{350 + \text{ther.}}$ where it is to be understood that the mass of air is supposed to increase in bulk $\frac{1}{400}$ for each degree of Fahrenheit's scale.

A variety of experiments have been made at various times to ascertain the increase in bulk of a quantity of air represented by unity for a certain number of degrees of rise of the thermometer. The following is a list of some of them ‡ :—

* Mem. de l'Ac. de Berlin, 1754, p. 131.

† Pref. to 1st vol. of Obs. 1765. Phil. Trans. 1764 and 1787, p. 157. Req. Tables, &c.

‡ See La Lande's Astr. 2241. 3d ed. Thomson's Chemistry, vol. i. p. 489. La Place's Mec. Cel. vol. iv. p. 270. Phil. Trans. 1809, &c. &c.

	for 1°
M. Bonne	1·00 25777
Bradley	1 00 25000
Dalton	1·00 20701
De Luc	1·00 20888
Fahrenheit	1·00 25777
Gay Lussac	1·00 20868
Groombridge	1·00 21000
Hawksbee	1·00 06933
La Caille	1·00 22222
Mayer	1·00 20444
Shuckburg	1·00 22222
Mean of all except Hawksbee's	1·00 22490

The refraction deduced from Bradley's very neat and simple formula was in a few years adopted by nearly all the astronomers of eminence throughout Europe. The extreme facility with which it might be computed, and the corrections applied, whether from the formula itself or from tables ready calculated for that purpose, was a powerful recommendation in its favour; but its near agreement with observations soon established it.

In 1805, the very ingenious and profound M. de la Place in his *Mécanique Céleste** favoured the world with a chapter on this subject, wherein he has displayed as much sagacious penetration as deep mathematical learning and ability. He begins with considering the trajectory of a ray of light traversing the atmosphere; and by supposing all its layers spheric, and of variable density, according to some function of their height, he deduces a differential formula for the refraction whose integral he then finds; but, he observes, this equation supposes that the refractive forces of the layers of the atmosphere are directly proportional to their density, which is the result of Hawksbee's experiments. Nevertheless, it is possible that this assumption may not be strictly correct, and it would be useful if more experiments were made on the subject. He then finds that the hypothesis of an uniform temperature is erroneous, as well as that of the density decreasing in arithmetic progression, when the height increases in a similar progression; and he says, "the constitution of the atmosphere being comprised between the two limits of a density decreasing in arithmetic progression, and of one decreasing in geometric pro-

* Vol. iv, page 231.

progression, therefore an hypothesis which participates of both these progressions would seem to represent the refraction, and the observed diminution of heat in the atmospheric layers; accordingly he makes this curious assumption, and deduces a formula by means of it.

He then applies the same analysis to the finding of an equation for the refraction at altitudes below 12°, and gives us an expression which has the advantage of being independent of all hypotheses respecting the constitution of the atmosphere, resting only upon the nature of it in the place where the observation is made, as indicated by the barometer and thermometer, after which he determines the value of his coefficients. With respect to that depending upon the thermometer, he requested M. Gay Lussac to repeat his experiments, with all possible care, by graduating his thermometers exactly, and by paying the greatest attention to dry the tubes well, which he made use of: for it appeared to him, that upon this depended principally the great differences in the results hitherto found by philosophers. Attending well to the expansion of glass*, and to the corrections on account of the variableness of the barometer, during each experiment Gay-Lussac found, by a mean of twenty-five experiments, that a volume of air expressed by unity at zero of temperature of the centigrade thermometer became 1·375 at the heat of boiling water, under a pressure equivalent to that of a column of mercury = 0·76 of a metre in height.

The other coefficient was determined by M. Delambre, who, by comparing a great number of observations, found the refraction to be 186·″728 at 50° of apparent altitude†, the temperature being zero, and height of the barometer 0·76 metre.

After this he proceeds to consider the effect of moisture in the atmosphere, and concludes the following values for the increase of the refraction, for extreme humidity in the air from 15° to 45° of temperature.

Degrees.	Increase of Refraction.
15°	0·563″ t, Θ
20	0·744 t, Θ
25	0·977 t, Θ
30	1·274 t, Θ
35	1·651 t, Θ
40	2·122 t, Θ

Where Θ represents the apparent altitude.

* Upon this and some other points connected with the subject of this paper, see Thomson's Chemistry, book i, div. 2, sect. 4, edit. of 1810.

† According to the new centesimal division of the circle; but this, according to our division of the circle, will be 60″ 499872 at 45° of apparent altitude at 5..°, when the barometer is 29 92152 Engl. in., and Fahrenheit's therm.

"It results from this table," he says, "that the effect of moisture in the air on the refraction is very small; the excess of the refractive power of the aqueous vapour on that of the air being compensated in a great measure by its less density. We may nevertheless attend to it by means of the preceding table, in cases of extreme humidity. Observations of the hygrometer will point out the ratio of the quantity of vapour spread in a given volume of air to the quantity which would produce extreme humidity in this volume. The increase of refraction which corresponds with extreme humidity must then be multiplied by this ratio." He concludes the subject with the following remark:

"If we would take into account the figure of the earth in the theory of refraction, it is to be observed that at the point where the observer is situated, we may always conceive an osculatory circle to the surface of the earth, whose plane passes through the star: now the figure of the atmospheric layers is very nearly the same as that of the earth; the circles, concentric to the circle in question, are therefore osculators likewise of these different figures; and we may determine the refraction of the star by supposing the earth to be spheric, and of a radius equal to this osculatory circle. Thus wee see, 1st, That the refraction always takes place in the vertical plane: 2dly, That it is not the same on all sides of the horizon, since the osculatory circles are not the same in every direction; but we may rest assured, that the error is insensible, when the star is a little elevated. At the horizon, however, differences of some seconds may occur." Thus terminates one of the most masterly chapters on this subject ever written; after which he proceeds to treat of the terrestrial refraction.

Upon these theorems* found by La Place, reduced to rather a more convenient form, and with coefficients differing a little from his, Delambre has computed a set of Tables by means of which the refraction may be found with great facility. They were first published by Puissant † in a work closely connected with this subject, and are well arranged for use. The first of them gives the refraction for every degree of apparent zenith distance down to 80°, and for every 30′ from thence to 90°. They are adapted for 0·71 metre of height of the barometer, and 35° of the centigrade thermometer ‡. Besides the refraction and its difference for

* Page 271, and page 264 of the Mecan. Cel. vol. iv.

† Traité de Geodesie, at the end, 4to, 1805.

‡ Or, 27·953 English inches, and 95° of Fahrenheit's thermometer.

each degree, there are given the logarithms of the refraction in seconds and their differences: and in two auxiliary tables are given the logarithms of the factors for correcting it for the variations of height in the barometer and thermometer. To make these corrections depending upon the temperature always affirmative as far as the table extends, he has added respectively to the logarithms of the two last tables their greatest negative logarithm taken positively, and subtracted from it the logarithms of the first table.

To this succeeds a Table of mean refractions for true distances from the zenith for every degree down to 80°, and for every 10′ thence to 91°, with their differences, adapted for 0·76 metre of the barometer, and 12°·5 of the centigrade thermometer*, with two auxiliary tables for reducing it to any other state of the atmosphere. Directions are also given for using them, together with the formulæ from which they were computed, reduced to a more simple form.

In noticing the latest improvement made in this subject, for which we are indebted to one of our own countrymen, it is but fair to return him those acknowledgements to which he is so justly entitled. When gentlemen of fortune give up the gay amusements of the world, and turn aside from the pleasures of fashionable life, to cultivate science in retirement, they deserve our warmest thanks: and when we add to this the consideration that the science they cultivate is not only one of the most interesting and sublime, but of the utmost importance to a commercial nation like Great Britain, our thanks certainly must be changed into something more like gratitude.

There are, indeed, very few who can afford to purchase instruments of sufficient accuracy to make improvements in a science so far advanced; and still fewer of those that so amply possess the means of life, who would bestow that time and attention which are requisite in acquiring the necessary knowledge for this purpose, and turning it to useful account.

Such is the present state of the navy of England, and her maritime concerns, that the improvement of astronomy is a subject which calls seriously for attention; more especially, as we have but one public institution for that purpose. The commerce of France is nothing in comparison of ours, yet the greatest encouragement is given in that country to those who promote every science connected

* Or, 29·92152 English inches, and 54°·5 of Fahrenheit's thermometer.

with

with the navy; whilst it is well known, that at this moment a few of our best mathematicians are groaning under insults, degradations, and injuries, as severe as they are unprovoked and undeserved. Had France the tenth part of our naval power, with her present number of scientific men, the whole world must soon be subjected to her dominion: and where so much is at stake, it behoves us, by giving all the encouragement in our power, to place our navy as much above that of other nations in scientific knowledge, as it is in all other high and great qualifications.

The gradual decrease of the study of mathematics in this country has already been publicly noticed*; but its causes, although very evident, have not yet been mentioned. Perhaps, at some future period, this may form the subject of another communication. I have been led into this digression by considering the still more deplorable state if possible of astronomy, which at this moment is scarcely cultivated by half a dozen persons throughout the whole kingdom. But to return.

It has been doubted, notwithstanding what is stated by La Caille, whether the mean refraction of France be the same as that of England,. What gave rise to this was the use of Bradley's Table in the determination lately made there of the obliquity of the ecliptic, wherein a difference of some seconds was found, between the result obtained from observations made in the winter solstice, and that from others made in the summer. This doubt has not yet been satisfactorily removed: but the very accurate astronomic circles lately made by our English artists, who are undoubtedly the best in the world, will, it is presumed, with good assistance from theory, not only soon decide this question, but furnish us with such observations as will determine the refraction to a second, till we approach near the horizon.

A very material step has lately been made towards this, by the publication of Mr. Groombridge's valuable paper on Refraction †, wherein he has determined, by a great number of very accurate observations, both the quantity of mean refraction at 45°, and the coefficients for correcting it on account of the state of the atmosphere. The former of these he makes 58″·1192 by a mean of a great number of observations, when the barometer is at 29·6 inches, and the

* Edinburgh Review of La Place's *Mecanique Céleste.*
† Philosophical Transactions for 1810.

 mother-thermometer 45° of Fahrenheit without, or 49° within, which he considers as the mean state of the atmosphere*. With respect to the latter, he finds the multiplier for all degrees below 49° within, to be ·0024; and above 49° within, ·0023 †: but for those above or below 45° without, he finds it ·0021. Instead, however, of the number 3, which Dr. Bradley had adopted for his coefficient of r, Mr. Groombridge finds that 3·3625 agrees better with observations: consequently his numbers and coefficients will give us the following four equations.

1st. For the thermometer *within*, and *below* 49°, putting $d = 49 - h$.

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 + \cdot0024d).$$

2dly. For the thermometer *within* and *above* 49°, put $d = h - 49$.

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 - \cdot0023d).$$

3dly. For the thermometer *without* and *below* 45°, put $d = 45 - h$.

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 + \cdot0021d).$$

4thly. For the thermometer *without* and *above* 45°, put $d = h - 45$.

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 - \cdot0021d).$$

Or, in logarithms:

1st. Thermometer *within* and *below* 49°, put $d = 49° - h°$, then:

$$\text{L. tan.}\left(Z - \frac{26\cdot9}{8}r\right) + \text{L.}b + \text{L.}(10000 + 24d) + 6\cdot29303 = \text{L. Ref.}''$$

* Mr. Kirwan states that the mean temperature of any place is equal to $84 - 51s^2$, latitude. According to this the mean temperature of Blackheath would be 52°·8, but its exposed situation may possibly be the cause of this small difference.—*Estimate of the Temperature of different Climates*, or *Dalton's Meteorological Observations*, page 120.

† Mr. Dalton found likewise that the expansion of air from 55° to 133½, or for the first 78½°, was 167 parts, whilst the expansion from 133½° to 212°, or for the next 78½°, was only 158 parts, or nine less than the former. So that it appears there is a difference between the expansion of air for the high degrees and that for the low ones.—*Manchester Memoirs*, vol. v.; or *Thomson's Chemistry*, vol. i. p. 490, edit. of 1810.

2d. Ther-

2d. Thermometer *within* and *above* 49°, put $d = h^\circ - 49^\circ$, then:

$$\text{L. } t, \left(Z - \frac{26{\cdot}9}{8} r\right) + \text{L. } b + \text{L. } (10000 - 23d) + 6{\cdot}29303 = \text{L. Ref.}''$$

3d. Thermometer *without* and *below* 45°, put $d = 45^\circ - h^\circ$, then:

$$\text{L. } t, \left(Z - \frac{26{\cdot}9}{8} r\right) + \text{L. } b + \text{L.} (10000 + 21d) + 6{\cdot}29303 = \text{L. Ref.}''$$

4thly. Thermometer *without* and *above* 45°, put $d = h^\circ - 45^\circ$, then:

$$\text{L. } t, \left(Z - \frac{26{\cdot}9}{8} r\right) + \text{L. } b + \text{L.} (10000 - 21d) + 6{\cdot}29303 = \text{L. Ref.}''$$

But as it appears more simple to avoid the two numbers 49 and 45, and reckon the state of the thermometer from zero, we may reduce the whole of these equations to that temperature, and then find other multipliers for the number of degrees above that point; which is easily done as follows: Put R = the refraction at any given temperature; τ = the degrees of that temperature, ϱ = the refraction at zero, n = the multiplier for the state R, and ν = that for zero: then we have

$R + R\tau n = R(1 + \tau n) = \varrho$; whence $R = \frac{\varrho}{1 + \tau n}$; but on the contrary, $\varrho - \varrho\tau\nu = R = \varrho(1 - \tau\nu)$; consequently, $\varrho(1 - \tau\nu) = \frac{\varrho}{1 + \tau n}$, from which we get $\nu = \frac{n}{\tau n + 1}$: then, by substituting Mr. Groombridge's multipliers for n, we get the new multipliers for reducing the refraction from zero to any other temperature. By this we also obtain the advantage of having only three equations instead of four; which, putting h = height of thermometer above zero, will now be as follows:

1st. From zero to 49°, *within.*

$$58''{\cdot}1192 \times 1{\cdot}1176 \text{ tan. } \left(Z - \frac{269}{80} r\right) \times \frac{b}{29{\cdot}6} \times (1 - h\,{\cdot}002147) = \text{R.}''$$

2dly. For all degrees above 49 *within.*

$$58''{\cdot}1192 \times 1{\cdot}127 \times t, \left(Z - \frac{269}{80} r\right) \times \frac{b}{29{\cdot}6} \times (1 - h\,{\cdot}002067) = \text{R.}''$$

3d. For the thermometer without.

$$58''\cdot1192 \times 1\cdot0945\ t, \left(Z - \frac{269}{80} r\right) \times \frac{b}{29\cdot6} \times (1 - h \cdot 001919) = R.''$$

Or, in logarithms:

1st. From zero to 49° *within.*

$$L.\ t, \left(Z - \frac{269}{80} r\right) + 0\cdot3413143 + L.b + L.(1 - h \cdot 002147) = L.\ R.''$$

2dly. Above 49° *within.*

$$L.\ t, \left(Z - \frac{269}{80} r\right) + 0\cdot3394060 + L.b + L.(1 - h \cdot 002067) = L.\ R.''$$

3dly. For the thermometer *without.*

$$L.\ t, \left(Z - \frac{269}{80} r\right) + 0\cdot3322437 + L.b + L.(1 - h \cdot 001919) = L.\ R.''$$

In these equations, although very simple, there is some little arithmetic trouble in computing the first and last part of each expression. As to the first, it will be easiest done by a table of the product of 3·3625 into each of the nine digits: by means of which the product of this number into any other will readily be obtained, by only adding that of its several digits, into 3·3625. Or it may be done by multiplying r by $\frac{269}{80}$; or, by the following expression $3\cdot3625\, r = \frac{3 \times 9}{8} r - \frac{1}{80} r$, which will only require contracted multiplication and division.

The last may easily be effected by a very ingenious and simple method pointed out by M. Cagnoli*. Thus it is well known that $\cos.^2 = \text{rad.}^2 - \sin.^2$; therefore putting $x = 1 - hn$, and comparing it with the latter of these two expressions, we have

$$c^2, A = r^2 - s^2, A = 1 - hn = x:$$

consequently $s, A = \sqrt{hn}$ and the square of the cosine corresponding will be equal to x. Whence we have this rule. To the logarithm of h add the logarithm of n, and divide the sum by 2. Seek this number in the Table of Logarithmic Sines, and take out the logarithmic cosine cor-

* Trigonometry, 2d edit. 4to. Paris 1808, p. 95, or 1st edit. 1787, p. 102.

responding

responding to it, which is readily done at the same opening of the book. Double this logarithmic cosine, and it is the logarithm required of $1 - hn$*.

But in a letter dated 17th September 1810, which I had the honour to receive from Mr. Groombridge, he informs me that he has calculated on the data before mentioned a Table of Refraction for every 10′ down to 70° of zenith distance; for every 5′ from thence down to 86°; for each 4′ thence to 88°; each 3′ thence to 89°; and for every 2′ from thence to 90° 18′: together with an auxiliary Table for the correction depending on the difference of the barometer and thermometer from the mean state. He has also contrived some very simple methods of performing with great facility whatever arithmetic operations may be requisite in using them. Every sincere lover of the science will no doubt join with me in requesting that these tables and methods may form the subject of another communication to the world, whereby it is presumed the mode of finding the refraction will be made extremely easy, and an important service rendered to astronomy.

As an appendix to what has been said on the refraction, I shall take the liberty of adding the following method of finding the sun's parallax, which is rather more accurate than as it is usually given in the Tables. *Add together the logarithmic sine of the sun's zenith distance, the logarithmic distance for the given day taken from page* iii *Nautical Almanack, and the constant number* 0·94151 : *their sum, rejecting the tens in the index, will be the logarithm of the sun's parallax in seconds.*

It must however be observed, that I have taken the sun's mean horizontal parallax at 8″·74: for it was found† by observations made at the Cape of Good Hope, as well as others made by M. Pingré and Mr. Short, 8″·8; by M. du Sejour, 8″·84; by M. Lexell and M. de la Lande, 8″·6. The mean of these six determinations is 8″·74, which is the quantity we have adopted above.

* When the thermometer is below zero, this expression will of course become $1 + hn$, in which case it may, as above, be compared with $1 + t^2 = f^2$. but instances are extremely rare where any necessity for this occurs in England.

† La Lande's Astronomy, art. 1725, 3d edit.

LXXXI. *De-*

LXXXI. *Description of a Manometer, by means of which we may ascertain the Changes which take place in the Elasticity and in the Composition of a determinate Volume of Air. By M.* C. L. Berthollet*.

The appellation of manometer has been given to various instruments which have been contrived for ascertaining the differences of density of the strata of the atmosphere; for we cannot determine by the barometer the variations which depend upon heat and upon the hygrometrical state.

Otto Guerick describes a manometer, which was afterwards given by Boyle as his own invention; but neither of these writers distinguishes its use from that of the barometer. Varignon, Fouchi and Gerstner have since given various manometers.

These instruments have been generally used for ascertaining the changes of density in the air, by means of the difference between an empty globe and one full of air, but sealed hermetically, and put in equilibrium with a metallic weight; for, when the density of the external air changes, the globe undergoes a change in its weight, which answers to that which takes place in a volume of air equal to that which it occupies, while the metallic weight, which is of a small volume only, remains sensibly the same.

Bouguer employed a different method for comparing the densities of the atmospheric air†: he used a pendulum which he made to oscillate at various heights, in order to judge, from the losses of motion experienced by the pendulum in a given time, of the resistance of the air, and consequently of its density. His experiments seemed to him to confirm the opinion which he had been led to form, viz. that, from the height at which the barometer supports itself at 16 inches, to that at which it supports itself at 21 inches, there is a constant relation between the densities of the air and the weights which compress it; but that this relation varies from that height down to the level of the sea, which he attributes to a difference in the elasticity of the molecules of the air. This error may proceed from the difficulty of obtaining results free from uncertainty by means of the pendulum used by Bouguer, as has been proved by M. de Saussure‡, and from his neglecting to reckon the effect of the heat, and of the hygrometrical state of the air.

We might with propriety resort to these methods of ascertaining

* *From Mémoires de la Société d'Arcueil,* tom. i. p. 282.

† *Mem. de l'Acad. des Sciences,* 1753.

‡ *Journ. de Physique,* 1790.

ascertaining the density of the strata of the atmosphere, if any doubts remained as to the nature of the air, the proportions of its constituent parts, and the law which its dilatation follows by the elevation of temperature: but at present, as we have precise information on these subjects, and as the uncertainties which may remain on the indications of the hygrometer are much smaller than those which ought to result from the methods mentioned; it is more expeditious and more certain to adhere to the barometer, combining its indication with that of the thermometer and hygrometer.

The case is not the same with the manometer destined to determine the changes which take place in the elasticity of a quantity of air contained in a vase. Saussure directed towards this object the apparatus to which he gave the name of manometer, and by means of which he made some most important observations*: it is simply a barometer, the bulb of which is contained in a bell-glass which is hermetically closed, and into which we may introduce the substances which may affect the elasticity of the air, by an aperture in the neck of the bell-glass, but by establishing, at that instant, the communication between the internal and external air.

While the communication with the external air is interrupted, the barometer is insensible to the variations of the atmosphere, and it undergoes no change in its elevation except by the increase or diminution of the elasticity.

This is the manometer, the applications of which I wished to extend, and which I endeavoured to adapt to the observation of the phænomena which take place during vegetation, and generally those which animal and vegetable substances present, during life or after death, relative to the atmosphere with which they are surrounded.

In the first place we perceive that the barometer which performs the functions of the manometer, indicates the quantities of gas which are disengaged or absorbed in a given time; and as it is easy to ascertain a change even of one thousandth part in the height of the barometer, we may determine a change of one thousandth part in the quantity of the contained air, by the absorption or extrication of a gas.

But in order to make this estimate, there must be a thermometer suspended internally to indicate the same temperature with that of the first observation: or if the tem-

* *Essais sur l'Hygrometrie*, p. 109.

perature

perature be different, we ought to bring back the gas to the first by calculation.

This calculation requires that we should take in not only the change of elasticity produced by the temperature, but also that which flows from the tension of the vapour of the water which is formed or destroyed; and for the latter purpose we may use the observations of Mr. Dalton.

After having ascertained the variations which have taken place in the elasticity at different times of the observation, it is important to be able to determine the chemical changes which have taken place in the atmosphere of the vegetable or animal substance, and the nature of the gaseous substances which may be liberated or absorbed.

This object is attained by means of a stop-cock, above which we adapt into a reservoir a graduated tube filled with water: on opening the stop-cock the water falls into the manometer, and its place is supplied in the tube by an equal volume of gas: the stop-cock is closed, and we may carry the tube with the gas which it contains.

We thus obtain a quantity of the gas contained in the apparatus, every time that we wish to examine it, without producing any change in the pressure of that which remains, and in the elevation of the barometer: it is only requisite to submit the gas which has been extracted to chemical tests.

We determine the proportion of carbonic acid by the absorption of lime water, afterwards that of the oxygen by the hydrogenated sulphuret of lime, according to the method of M. de Marty*: and lastly, we test the residuum with oxygenated gas in the eudiometer of Volta, if we suppose an inflammable gas to exist in it. The remainder gives the proportion of the azote.

In most circumstances carbonic acid is formed, and more or less of it is dissolved in the water which has been introduced into the apparatus, according to its quantity, temperature, and the pressure to which it is submitted. M. Theodore de Saussure, in order to determine the quantity of carbonic acid which was absorbed in several of his experiments, contented himself with regarding it as equal to the volume of water which was in his apparatus. This determination is not sufficiently rigorous, since the quantity which is absorbed by the water varies much by the circumstances which have been detailed.

The quantity of carbonic acid which has been absorbed by

* *Journal de Physique*, tome lii. *Annales de Chimie*, tome lxi.

by the liquid contained in the apparatus, may be determined by precipitating this acid by lime water, or by water of barytes, either from the whole or from a part of the liquid: after that, we introduce the precipitate into a flask, adapt the tube of a funnel to it, through which we pour a quantity of dilute sulphuric acid; and by the loss of weight which takes place, we ascertain the quantity of carbonic acid which was dissolved in the liquid, and which is disengaged from the carbonate.—We may, by the processes which I have indicated, ascertain in a volume of air equal to that of a kilogramme of water, and contained in a manometer which has this dimension, the change which would be produced by the volume of one gramme of water; the production of a quantity of carbonic acid which does not exceed a centigramme in weight; and a variation in the proportions of the oxygen and azote which does not exceed a centieme: this is a precision which would seem to be sufficient for all the determinations which we would wish to establish.

We have besides the advantage of being able to repeat and compare the tests at different times, without interrupting the experiment, and to vary several of its circumstances: I have constructed manometers of different dimensions, in order to apply them to different objects.

Hitherto I have made but a small number of observations with this instrument, and I have not pursued them with the care which they require; but my chief object in this publication is to induce those to employ it who are occupied with experiments of this nature, and who have more leisure and perseverance than I have. I shall describe some early attempts.

M. Theodore de Saussure, to whom we are indebted for some learned and laborious researches upon vegetation, has shown, that in most of the cases where we suppose that the oxygen gas was absorbed by a vegetable or animal substance, there is simply formed a combination of the carbon of these substances with the oxygen of the atmosphere; that the volume of the gas did not diminish, except on account of the absorption of the carbonic acid by water; and that at the same time water was produced by the combination of the oxygen and hydrogen which existed in the substance; so that, although the residue had been deprived of a part of its carbon by the action of the oxygen gas, it is nevertheless found more carbonized afterwards, because it has been stripped of a greater proportion of hydrogen and oxygen than of carbon*.

* *Recherches Chimiques sur la Végétation.*

It

It appeared to me to be useful to examine whether these results, which give the explanation of several transmutations undergone by animal and vegetable substances, might conduce to general consequences, or if they ought to be restricted to a certain class of phænomena.

M. de Saussure had already remarked that the oxygen gas was absorbed by the oils, without forming a corresponding quantity of carbonic acid.

The theory of the solution of indigo by the alkaline bases which are combined with it, when it is deprived of oxygen, and of its precipitation by the oxygen of the atmosphere, which has been explained in the Elements of the Art of Dyeing, seemed established on sufficient proofs. Nevertheless the analogy, with the facts observed by M. de Saussure, might lead us to believe that the oxygen of the atmosphere served to form carbonic acid, with a part of the carbon of the indigo which had been rendered soluble.

A solution of indigo, made by means of the sulphate of iron and of lime, limpid and of a fawn-colour, after having been carefully separated from the sediment, was introduced into a manometer of 11 litres 632 capacity: the barometer was at $0^{m},75,74$, the thermometer at 12°: two days afterwards the liquor was completely colourless, and the indigo was precipitated in dark blue, the thermometer being at 12,5, the barometer had fallen six millimetres.

The filtered liquor was covered while in the air with pellicles of carbonate of lime, and precipitated abundantly with oxalate of ammonia: the blue precipitate retained on a filter did not effervesce with an acid, and gave with sulphuric acid a very deep solution of indigo.

Thus we see that the lime preserved its state during the precipitation of the indigo, and that carbonic acid is not formed.

On the other hand, the test of the air contained in the manometer has shown that it was the oxygen gas which alone had been absorbed by the indigo, whose precipitation it had operated. The experiment repeated a second time gave similar results: but we here neglect the calculations necessary for determining the quantity of the absorption, because we have not ascertained the weight of the indigo precipitated. We confine ourselves to the conclusion, that the quantity of oxygen which disappeared has not been employed in this case to form carbonic acid; but that it is combined with the indigo, to which it has thereby rendered its insolubility and colour.

I was anxious to compare the changes which are produced

duced by a colouring substance of a different species, which was campeachy wood.

The decoction of campeachy generally obtained has a blue colour, because we prepare it in copper vessels: it is of a fine red when glass or silver vessels are used.

This very clear decoction was cooled in a bottle with a ground stopper, in order that it might not be altered by the contact of the air, and placed in the manometer, the thermometer being at 18,5, the barometer $0^m.7593$: four days afterwards the liquor was turbid, and the temperature being the same, the interior barometer fell $0^m.03$. This lowering continued for two months, and in this time the liquor became very turbid and of a reddish fawn colour: a trifling sediment was formed, and some crusts.

At the end of the experiment the thermometer was 21,25, the total lowering of the barometer $0^m.050$, the air of the manometer when referred to its primitive pressure contained in 100 parts

Carbonic acid	3·91
Oxygen	6·55
Azote	89·54

There was at the end of the operation an increase of temperature of 3° 25, which requires the following correction in the volume of the gas at the primitive pressure of $0^m.7593$.

According to the determinations which M. Gay-Lussac communicates, the quantity by which a volume of air is dilated by 1°, is expressed by the height of the barometer which represents the tension of this air divided by 266,66, and becomes on setting out from the degree above zero equal to the quotient of the tension by this divisor, augmented by the number of degrees from which we begin to count the dilatation. In the present case, the height of the barometer at the commencement of the operation $= 0^m.7593$, the temperature 18°, the column of mercury corresponding to a dilatation of 1° will therefore be

$$= \frac{0^m.7593}{266\ 66 + 18} = 0^m.00266,$$

and that which is to take off for the dilatation is 3°.25 = 0,00864.

As to the vapour which ought to be formed on bringing the numbers of the table of Dalton to the degrees of the centigrade thermometer, and to the divisions of the metre, we find that the tension of the vapour being 21°,25,

$$= 0^m.01847 -$$

$$\text{and at } 18^\circ = 0^m.01536.$$

The

The column of mercury sustained by the elastic vapour which was produced during the experiment,

$$= 0^{m}.00311.$$

The manometer brought to the primitive data has therefore undergone a decrease

$$= 0^{m}.050 + 0^{m}.00864 + 0^{m}.00311 = 0^{m}.06175,$$

$$= 0.0813 \text{ of the volume of air used in the experiment.}$$

In order to know on what substance the absorption acts, we must keep an account of the quantity of carbonic acid which ought to be dissolved; and as we have neglected to do it by precipitation, as I have indicated, we shall confine ourselves to regard, with M. de Saussure, this quantity as equal in volume to the liquid.

The capacity of the manometer being four litres 676, the volume of the liquid = 0,565, the volume of the air in the experiment = 4 lit. 111, the volume of the carbonic acid dissolved by the liquor = 0 lit. 565, forms the 0,137: now on adding up the proportions of carbonic acid and of oxygen formed in the air, and supposing that the oxygen gas on combining with the carbon is replaced by a volume of carbonic acid precisely equal to its own, we find that there is wanting in 100 parts of air 10,54 of oxygen, or the 0,105 of the volume of the air, a quantity which only differs 0,032 from that of the carbonic acid which has been supposed to be dissolved by the liquid. This difference ought to be neglected, because the volume of the carbonic acid absorbed ought to be inferior to that of the water, either on account of the elevation of temperature, or on account of the diminution of the pressure.

If we compare this result with the preceding indication of the manometer, we find that there is only 0,02 of difference, a quantity which may be neglected, chiefly on account of the inaccurate valuation of the carbonic acid held in solution.

The phænomena, therefore, answer perfectly in this circumstance to the observations of M. de Saussure; the oxygen gas is not absorbed by the decoction of campeachy; but the latter changes it into carbonic acid, on giving up carbon to it: at the same time, without doubt, water is formed by the intimate union of the oxygen and hydrogen which existed in the substance, which thereby becomes more carbonized; and it is by these effects that we ought to explain the alterations which it undergoes in its properties.

Then this solution gives only a yellow precipitate with the nitro-muriate of tin, instead of a bright red precipitate: an an olive-coloured precipitate with the solution of highly oxidated iron, instead of a blackish blue precipitate: a fawn-red precipitate with the muriate of copper, instead of a blue precipitate.

Hence we see that, in the application to the arts, we may obtain from the campeachy a different colour, according to the kind of vessel in which we make the decoction: that the action of the air, at least when it is continued too long, changes its nature and decomposes it: so that the decoction kept by the name of campeachy juice may be spoiled, if we allow it to undergo the action of the air without some precaution.

Results were obtained different from the two foregoing, when we submitted gall-nuts to the test of the manometer, with the view of examining what passed in the development of the gallic acid: a portion of the oxygen of the air is transformed into carbonic acid by means of the carbon of the substance; but another portion also is liberated, the two elements of which it had furnished; and lastly, there is a considerable absorption of azote; a circumstance which requires ulterior observations.

Explanation of the Plate which represents the Manometer, and of the Method of using it.

Fig. 1 and 2. Vertical and horizontal projections of a cylindrical manometer formed by a vessel A with a large aperture, the neck of which has a copper rim B. The inside of this rim forms a screw for receiving the plate of copper E, which serves to close the manometer: it rests on a round pad of leather so arranged that when the lid is screwed down upon it, the vessel is very closely shut. G, G, buttons on which are fixed the notches of the key represented flat in R, and seen directly in S; this key serves to keep the vessel steady, while we turn and fasten the lid with the other key T; the square head of which embraces the button of the same form, which we see at E in the two projections.

a, *a*, *a*, three hooks fixed in the lid from which we may suspend a thermometer, a hygrometer, &c. D, a socket in which we fix with hard mastic a barometer with a syphon: as it would be difficult to give it, in this socket, a situation exactly vertical, and besides, as the inclination of the screw in the lid may remove it from this position, in order to give more precision to its indications, we rest the manometer on a rim of wood, having three screws in it *k*, *k*, *k*, which we move until the tube of the barometer be very vertical; which we may easily judge by means of the plummet I F, which

is

is to be adjusted successively in two positions which form a right angle with each other. This plummet is attached to a moveable brass scale H, to which we give only $0^m.04$ to $0^m.05$ of extent. This scale embraces, by means of two rings *b b*, not shut, the barometrical tube: it may thus be placed at any height on the barometer, and preserve the position which is given to it. It is used to determine the quantity which the height of the column of mercury has varied in the course of an experiment: if this quantity exceeds the limits of this scale, which is not very probable, it may be shifted so as to measure at several times the whole variation observed. The absolute height of the mercury is taken at the commencement of the experiment on a barometer, and we fix one of the extremities of the scale H at the summit of the mercury at this moment. The small branch of the syphon is furnished with a scale, in order also to observe the difference of the height of the mercury from the commencement to the end of the experiment. When the experiments require it, we give to the tube a length which exceeds much that of the common barometers, and it may be augmented sufficiently to indicate a pressure double that of the atmosphere.

The plate E has at C a stop-cock intended to give issue to the air of the apparatus when we wish to examine it; and this stop-cock is adjusted in such a manner that we may repeat these experiments as often as we judge it necessary in the course of an experiment, without fearing to change the nature, or even the state of compression, of the air of the manometer. For this effect, the stop-cock has above its collet at L (figures 1, 3, 4, and 5) two nut-screws, one internal and one external. On the latter is mounted a copper salver M, which we fill with distilled water: the glass tube N, graduated and furnished with a copper socket at O, is adjusted upon the internal screw, after having been also filled with distilled water: the extremity of its screw is furnished with a round of leather, which we compress. On opening the stop-cock the water of the tube is displaced by the air, which escapes from the manometer, and when we perceive that a sufficient quantity has entered into the tube, we shut the stop-cock. Upon unscrewing the tube, the volume of the air which has entered generally changes, and occupies a smaller or larger space, in proportion as it underwent in the manometer a pressure weaker or stronger than that of the atmosphere. But we remove the tube by plunging the finger into the water of the bason, and closing with its extremity the orifice of the tube, and we do do not measure the air until after we have determined with the usual precautions the temperature and pressure to which it is exposed.

We must only introduce in this manner into the manometer, a liquid, which most commonly does not disturb the results, and the influence of which we can always ascertain: if we were afraid, however, that it would interrupt the experiment, we might receive it into a vessel disposed for this purpose in the inside of the manometer.

Fig. 3 shows the various pieces just described, ready to be adjusted: fig. 5 is a section of these same pieces all adjusted.

We ought to take care, in the construction of this apparatus, to give the hole of the key of the stop-cock a diameter sufficiently large to admit of the easy flowing of the water of the tube, and it ought not to be less than twelve millimetres. In order that the air contained in this hole may be in the same circumstances with that which occupies the whole capacity of the manometer, we leave the stop-cock open during the experiments, as seen in fig. 1 and 2; we intercept the communication with the external air by means of a copper stopper Q (fig. 1 and 4) which has the same screw with the mounting of the divided tube, and which is also furnished with a rim of leather. In order to close it properly, it has at its surface a square cavity which is seen at *p*, into which we insert the stalk *r* of the same form which is at the extremity of the handle of the key T. We then only close the stop-cock at the moment when we wish to extract the air from the manometer.

LXXXII. *On the Barometer. By* Richard Walker, *Esq.*

To Mr. Tilloch.

Sir, Considering that I may not have been sufficiently explicit in my last paper, respecting the effects of the *difference of temperature* on the weather, I have been induced to offer the following remarks on that part of the subject.

A *warm temperature* of the air, at any degree of density of the atmosphere, will retain a greater portion of water in a state of *chemical combination*, than a *cold temperature* of the air at a similar degree of density of the atmosphere. Hence we may account for the almost constant *dry state* of the lower stratum of the atmosphere during the sum-

MER SEASON, and the almost constant *moist state* of the lower stratum of the atmosphere during the WINTER SEASON; the air, however, being sometimes sufficiently dense, as in the clear weather which accompanies a freezing atmosphere, to retain the water in a state of *chemical combination*, notwithstanding the diminution of temperature.

The same circumstance accounts likewise for the different states, with respect to *moisture and dryness*, of the middle seasons, viz. SPRING AND AUTUMN, accordingly as these participate in their nature more or less of either of the former seasons; observing that, *cæteris paribus*, there is more rain and misty weather during AUTUMN than SPRING, in consequence of the greater quantity of water which has been raised into the atmosphere during the SUMMER than the WINTER SEASON.

All the circumstances I have had occasion to mention, depending upon the *greater or less density*, and the *higher and lower degrees of temperature* of the atmosphere, are exemplified by the two following familiar experiments:

In the first instance, by means of pumping out of a glass receiver (containing air apparently dry and perfectly transparent) a certain portion of the air it contains, when the air being rarefied deposits a certain portion of the water it originally contained in *chemical combination* in a cloudy vapour, which, upon re-admission of the air, is re-absorbed; and in the second instance, by abstracting heat from a glass vessel containing atmospherical air, and again restoring the heat. The latter circumstance is likewise instanced, *naturally*, by what commonly happens in the course of a hot summer's day, particularly when the ground has become very moist by previous rain; the vapour *ascending* visibly in the morning, disappearing during the middle of the day, and *descending* visibly again in the evening*.

The variations of temperature in the atmosphere *independent of those which proceed from the direct influence of the sun*, arise from the conversion of water into vapour, which produces *cold*; and the condensation of vapour into water, which produces *heat*. Hence it commonly follows, that in proportion as the barometer *rises*, the thermometer *sinks*, and *vice versâ*, throughout the year; the direct influence of the sun in *clear weather* being abstracted†.

* At Lima, in Peru, it never rains; the moisture raised in the day time being restored again at night in the state of mist.

† In summer, during fair wearher, the nights, or rather the mornings before sun-rise, are cold, approaching even to frost.

Thunder

Thunder frequently follows a considerable duration of dry hot weather, both these circumstances being favourable to the collection and insulation of electric matter.

The extraordinary *elevation* of the barometer which sometimes happens, is said to arise from two currents of air, from opposite directions, meeting and accumulating over a particular spot; and the extraordinary *depression* of the barometer, from the circumstance of two currents of air setting out from any particular spot: in either case a commotion of the air is necessarily produced, whilst the equilibrium is restoring.

That the atmosphere, as well as the sea, is affected periodically in a small degree by the attraction of the moon, is well ascertained; but it does not appear that the weather is in the least influenced by any mechanical effect of the moon.

I was first led to the remark noticed in a former paper, respecting the difference of the weather during the *increase* and during the *wane* of the moon, by observing that eclipses of the moon were much seldomer obscured by a clouded atmosphere than eclipses of the sun; and subsequent observations of a general nature have somewhat confirmed me in the same opinion.

P. S. I omitted to mention, in my paper on the measurements of heights by the barometer, (Phil. Mag. for Oct. 1810, p. 278) that when the lower station in the barometer is *below* what is provided for in Table 2, p. 279, as is sometimes the case in *different gradations* of heights, the most accurate method will be, first to calculate the whole height, *assuming* 30 *inches of the barometer for the lower station;* and in like manner calculate the *lower portion only*, and then subtract the latter product from the former.

Oxford, Dec. 15, 1810. RD. WALKER.

LXXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On Dec. 6th, the reading of Mr. Davy's Bakerian Lecture was continued, and on the 13th concluded. In this part of the lecture Mr. Davy detailed a number of experiments, which he regarded as showing that when any metallic oxide is converted into the substance improperly called a muriate, but which is a binary combination of oxymuriatic gas and a metal, the oxygen produced is exactly that which had been absorbed by the metal: and he stated, that the proportions of oxygen or of oxymuriatic gas which combine with metals, are always definite; and that when two proportions combine, the one bears a simple ratio to the other.

Mr. Davy, inferring from the whole series of facts that oxymuriatic gas must be considered as a substance as yet undecompounded, and analogous in many of its properties to oxygen gas, but having stronger attractions for most inflammable bodies,—suggests the necessity of altering its name; which conveys so false an idea of its nature.

Conceiving it dangerous in the present improving state of science to adopt any names connected with theoretical arrangements, which may require alteration as knowledge advances,—he ventures to suggest for the consideration of chemical philosophers the name of *chlorice*, derived from its green colour; and he proposes to signify its compounds by the name of the basis, with a termination in *ine* or *ane*: thus hornsilver, improperly called muriate of silver, would be named argent*ane*; muriate of barytes, baryt*ine*, &c.

On the 13th and 20th, the Croonian Lecture on muscular motion, by —— Brodie, Esq. F.R.S. was read. The subjects introduced in this lecture were less numerous, and the discussion less varied, than usual on similar occasions; and very little or no reference was made to muscular action, the ingenious lecturer confining himself to a simple detail of the thermometrical effects on the animal body, in consequence of dividing the spinal marrow and afterwards inflating the lungs artificially with a pair of bellows, and continuing the circulation of the blood under such circumstances for nearly two hours. The subjects of operation were chiefly rabbits: and the author made a great number of experiments on these animals by dividing the spinal marrow and suffering them to die in this manner, noticing their temperature and that of the room at particular periods; or, after dividing the spinal marrow, inflating the lungs, and thus keeping up the circulation for an hour, and even an hour and a half; noting also the temperature of the heart, intestines, and rectum, at various times during the experiments. The result of the author's inquiries was, that animal heat does not appear to be produced, as generally supposed, by the action of the air on the lungs, and the circulation of the blood; as those animals whose lungs were inflated, and the circulation artificially continued, were always from one to three or four degrees colder in a certain time than those whose spinal marrow was divided and suffered

fered to die. Professor Davy suggested to the author, that the cold air thrown into the lungs (which produced the usual change in the colour of the blood) might contribute to this effect; and accordingly an experiment was made to obviate such consequence by means of a ligature: when it appeared, that in an hour and forty minutes the body in which the circulation was artificially continued after dividing the spinal marrow, was only one degree colder than that which died immediately. Mr. B.'s experiments seem to militate against the doctrine of the vitality of the blood; but they do little towards illustrating the fact, that tortoises can live and walk about long after having been deprived entirely of the brain, and even part of the spinal marrow.

On the evening of the 20th, part of a letter from Dr. Parry, of Bath, was read, on certain nervous affections; as convulsions, tremulous motions, and sudden startings or pulsations of what is vulgarly called the life blood; after which the society adjourned till January 10.

ROYAL SOCIETY OF EDINBURGH.

On Monday the 5th of November, the Royal Society of Edinburgh met for the first time in their new apartments in George-street, when Dr. Thomas Thomson read two papers, giving the account of the analyses of two new minerals from Greenland. To one of them he has given the name of allanite, and to the other sodalite. In the first he discovered a considerable portion of cerium, and in one analysis he detected a quantity of a metallic oxide perfectly new in its properties, for which he proposed the name of junonium. The other mineral, according to his investigation, affords 23 per cent. of soda and three of muriatic acid. By an analysis of Mr. Ekeberg, the same constituents were yielded in the proportions of 25 per cent. and six per cent.

At the next meeting on the 19th, a short communication was read, respecting a singular water-spout observed at Ramsgate.

On the 3d inst. a paper by Dr. Brewster was read, being a new demonstration of the fundamental properties of the lever.—Also a communication by Sir George Mackenzie, Bart. relative to the hot springs of Iceland; when Sir George exhibited some beautiful drawings and part of a series of magnificent specimens from that country, which he proposes to deposit in the cabinet of the Society: and at the last meeting, on the 17th, Sir George began a description of the minerals of Iceland, when he exhibited specimens from the district called the Guldbringe Syssel.

LXXXIV. *In-*

LXXXIV. *Intelligence and Miscellaneous Articles.*

DE LUC'S ELECTRIC COLUMN.

We have now to inform our readers, that the small pair of bells connected with the *electric column* invented by Mr. de Luc, which we have frequently noticed, were perceived to cease ringing for about ten minutes on the 4th of September; then (the apparatus remaining untouched) to begin again to ring by intervals, stopping perhaps half a second or more at a time: they stopped for several *days* after this, and began again, and at other times for *hours*; and on the 18th of November they were removed from the column, not having been heard that morning.

13th December, 1810.

Till the appearance of Dr. Adams's last edition of Morbid Poison, it was universally believed that the unhappy subjects of the Arabian leprosy are peculiarly salacious; an opinion as old as Aretæus, copied by most succeeding authors and contradicted by none. Dr. Adams, from actual observation, has proved the fallacy of this opinion. A melancholy case is now in St. Bartholomew's Hospital; a native of the Portuguese Brazils, about 30 years of age, without beard, and with all the other peculiarities remarked by the above accurate writer.

LECTURES.

Mr. Taunton's Spring Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, will commence on Saturday, January 19th, 1811, at Eight o'clock in the Evening precisely, and will be continued every Tuesday, Thursday, and Saturday, at the same hour, at the Theatre of Anatomy, Greville-Street, Hatton Garden.

Dr. Clutterbuck will begin his Spring Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday January 21st, at Ten o'clock in the Morning: viz. *Theory* and *Practice*, on Mondays, Wednesdays, and Fridays; and the *Materia Medica* and *Chemistry*, on Tuesdays, Thursdays, and Saturdays. Further particulars may be had at No. 1, Crescent, New Bridge-Street.

Dr. Adams's Course of Lectures on the Institutes and Practice of Medicine, will commence on Thursday the 10th Instant, at his House in Hatton Garden, precisely at Ten o'clock.

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For December 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o' clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Nov. 27	41	47°	42°	29·20	12	Fair
28	40	46	39	·01	21	Fair
29	36	43	37	28·98	0	Rain
30	35	42	34	29·25	17	Fair
Dec. 1	34	38	36	·50	12	Fair
2	32	36	31	·90	10	Fair
3	30	37	40	·85	0	Rain
4	42	44	44	·90	8	Fair
5	45	49	47	·89	10	Fair
6	47	50	44	·46	0	Rain
7	44	47	36	·30	6	Fair
8	37	42	30	·61	0	Rain
9	29	35	31	·87	10	Fair
10	35	40	36	·23	0	Rain
11	36	38	30	·80	5	Fair
12	35	46	42	·30	0	Rain
13	46	52	47	·85	10	Fair
14	48	48	43	·50	5	Stormy
15	40	46	36	·86	15	Fair
16	37	43	35	30·30	10	Cloudy
17	41	49	47	·08	7	Cloudy
18	48	46	45	29·40	0	Rain
19	41	40	36	·51	9	Fair
20	37	44	43	·62	0	Rain
21	43	46	42	·38	10	Fair
22	41	46	52	·60	9	Cloudy
23	52	47	43	·42	8	Fair
24	42	43	41	·45	4	Rain
25	49	49	46	·05	4	Stormy, and in the evening many vivid flashes of lightning.
26	44	47			25	Fair

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

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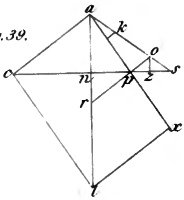
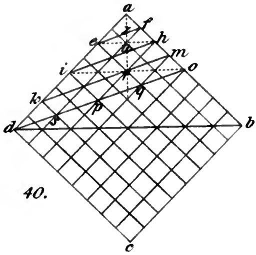
END OF THE THIRTY-SIXTH VOLUME.

Printed by Richard Taylor and Co., Shoe Lane, London.

HAÜY PLATE XII.

Fig. 39.





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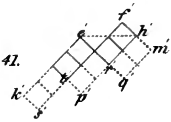

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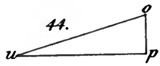

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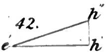

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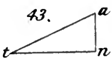

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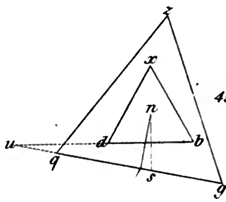

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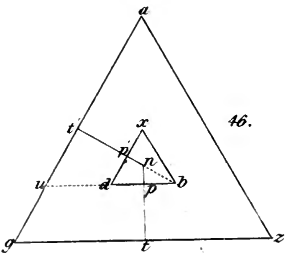

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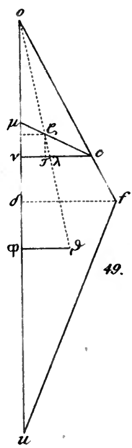

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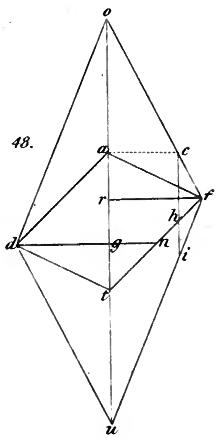
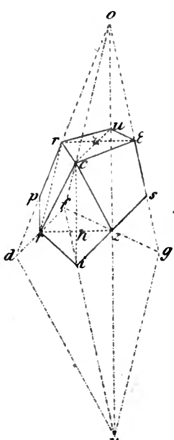


47.



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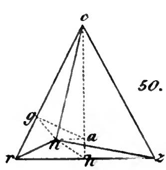

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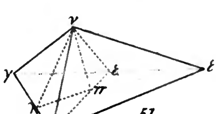


Lowry sculp.

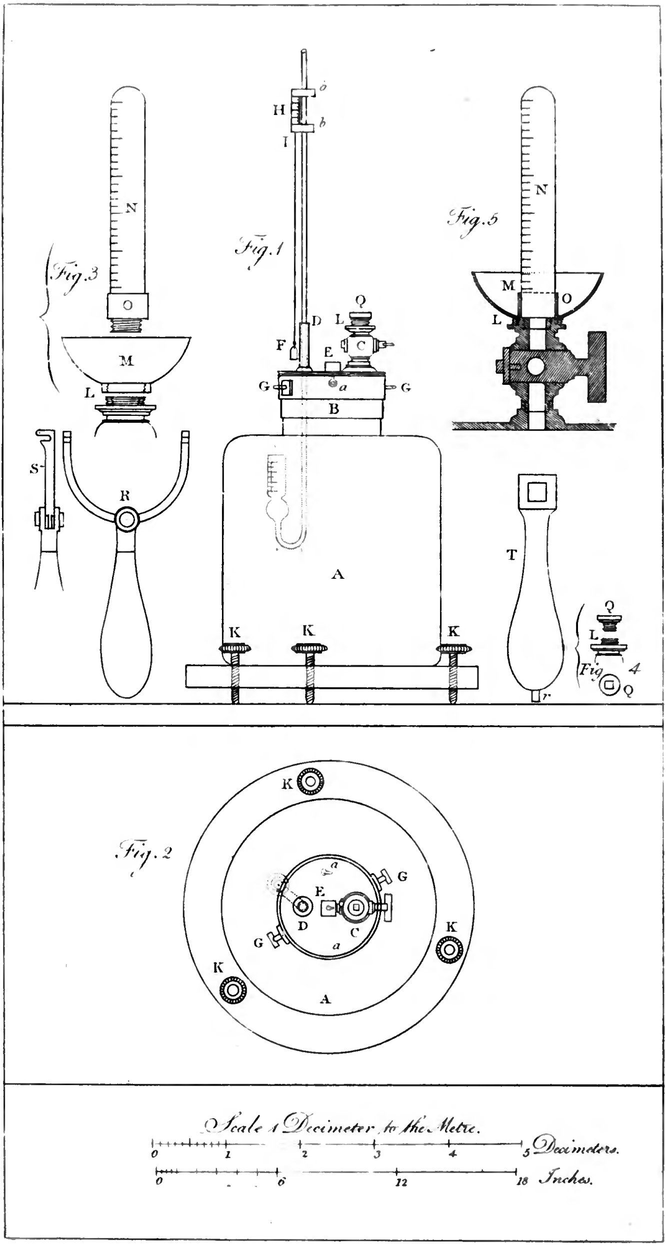
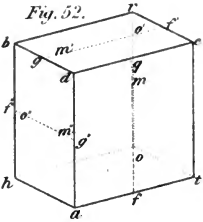


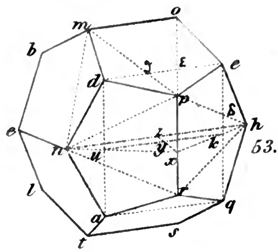
Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Scale 1 Decimeter to the Metre.
0 1 2 3 4 5 Decimeters.
0 6 12 18 Inches.

HAÜY.

PLATE XIII.

Fig. 52.





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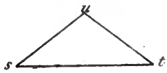

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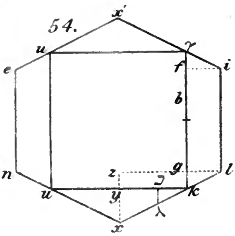
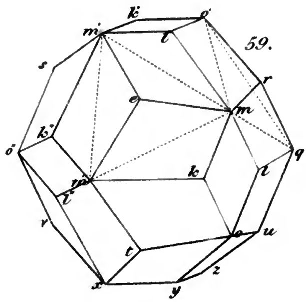
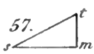




59.

57.



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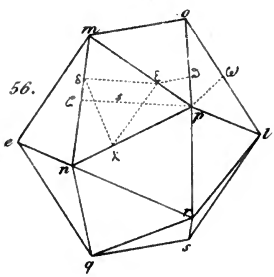
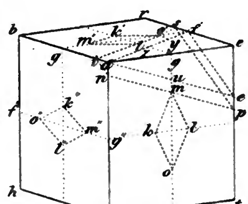
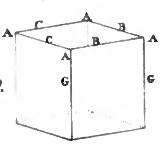




61.

60.



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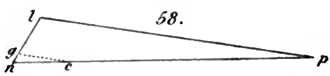

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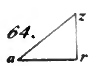

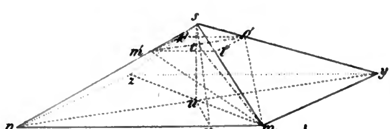

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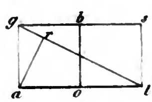


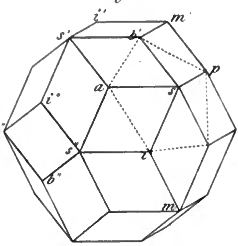
63.

Lowry sculp

HAÜY.

PLATE XIV.

Fig. 65.



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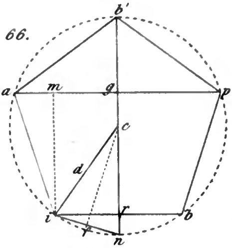

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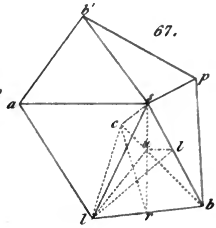

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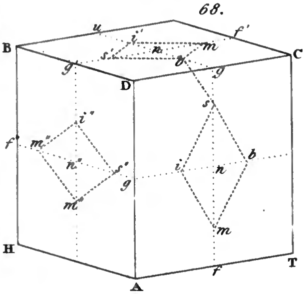

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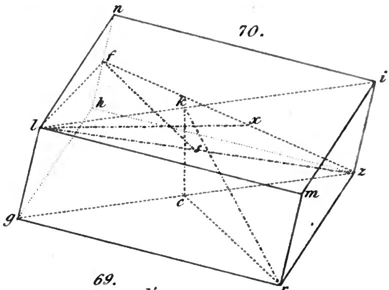

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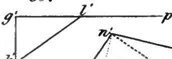

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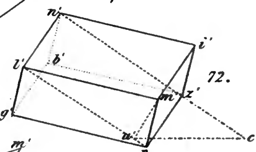

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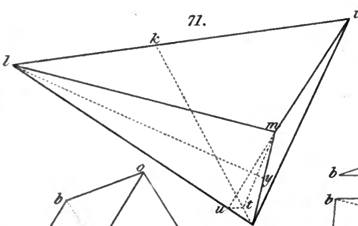

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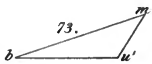

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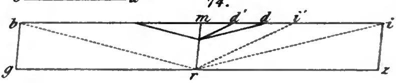

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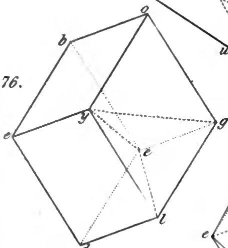

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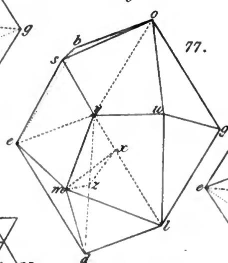

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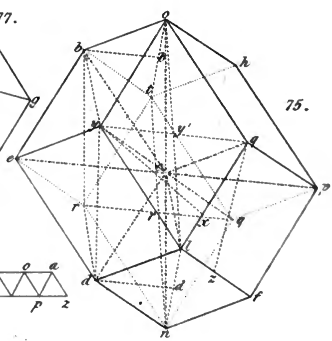

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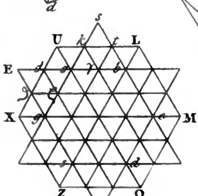

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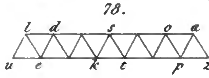


Lowry sculp.

An Equal Temperament Tuning Table, by Musicus Ignoramus, 1809.

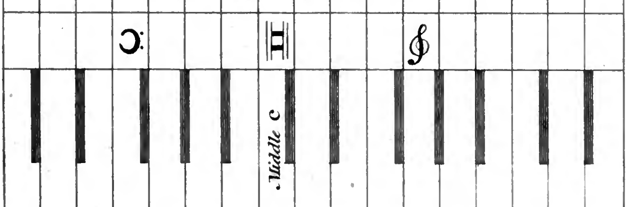
Sharpened Fifths downwards

15								×			f		×		C		
16						a♯			×		F						
17						A♯			×		×		a♯				
18						×♯			d♯			×	A♯				

Finis

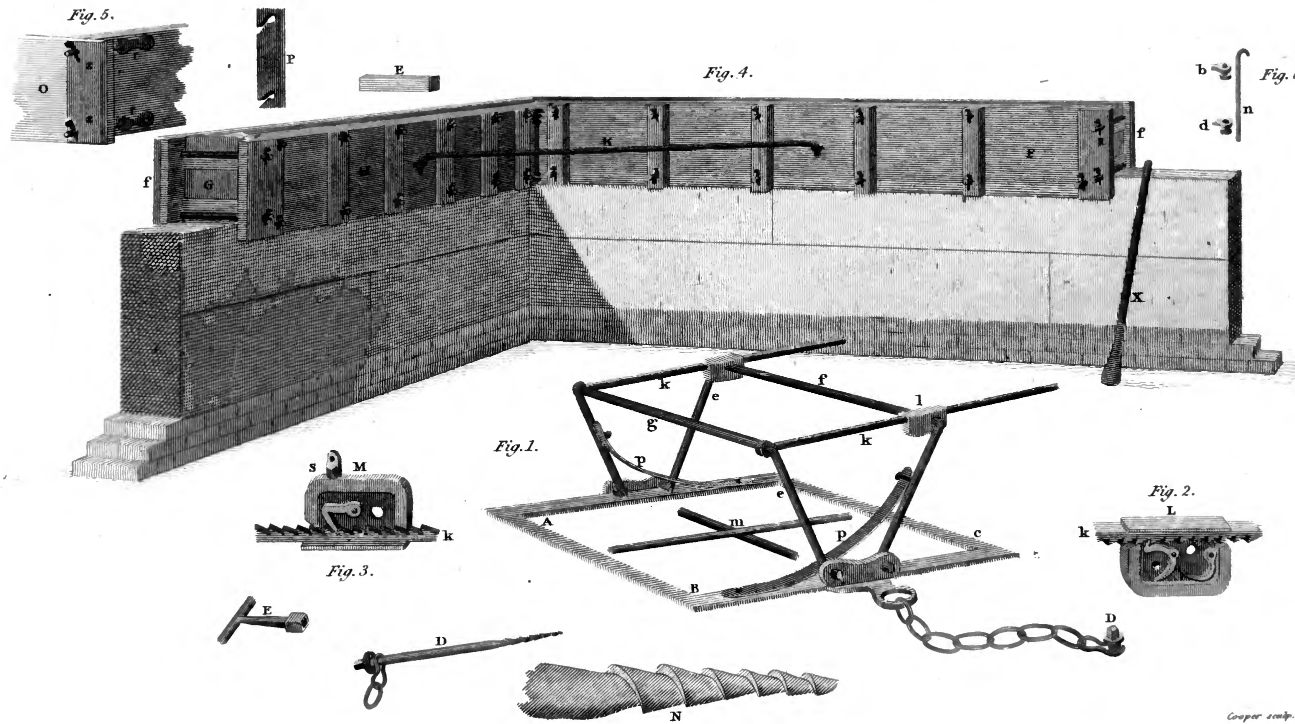
14					g♯		×			×		G♯					
13								c♯		×		g♯		×			
12				F♯		×		c♯			×♯						
11				f♯		×			×		F♯						
10							B		×		f♯			×			
9							b		×			×		B			
8										E		×		b			
7						A		×		e		×					
6						a			×				A				
5									D				a		×		
4					G				d			×					
3					g			×				G					
2								c				g			×		
1								c							C		

Flattened Fifths upwards

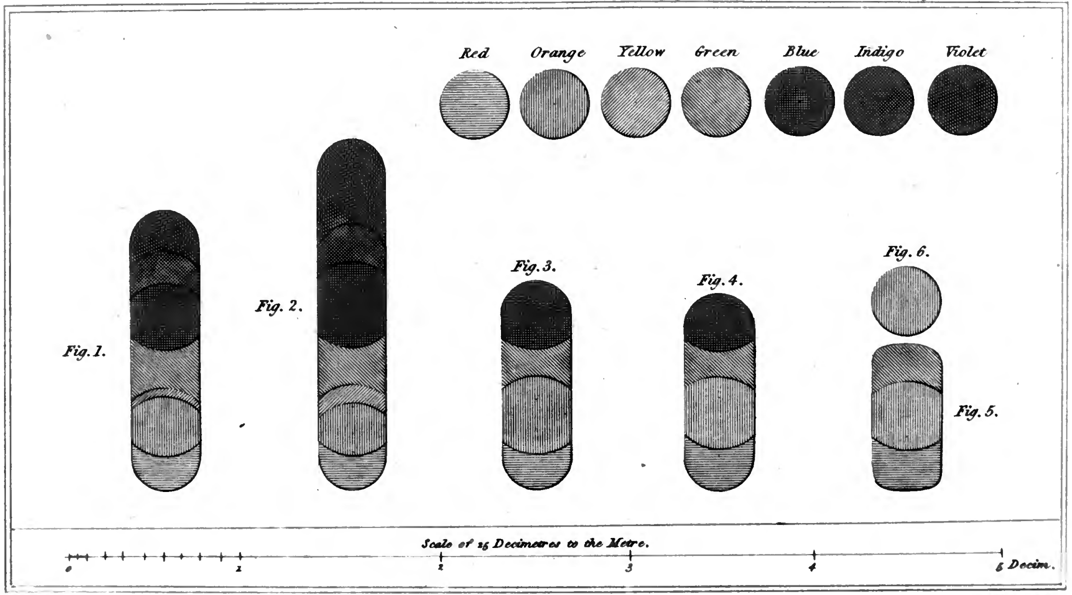


Steps of the process

Mr. Salmon's method of Building Pisé or Earthen Walls — & his Man Trap.

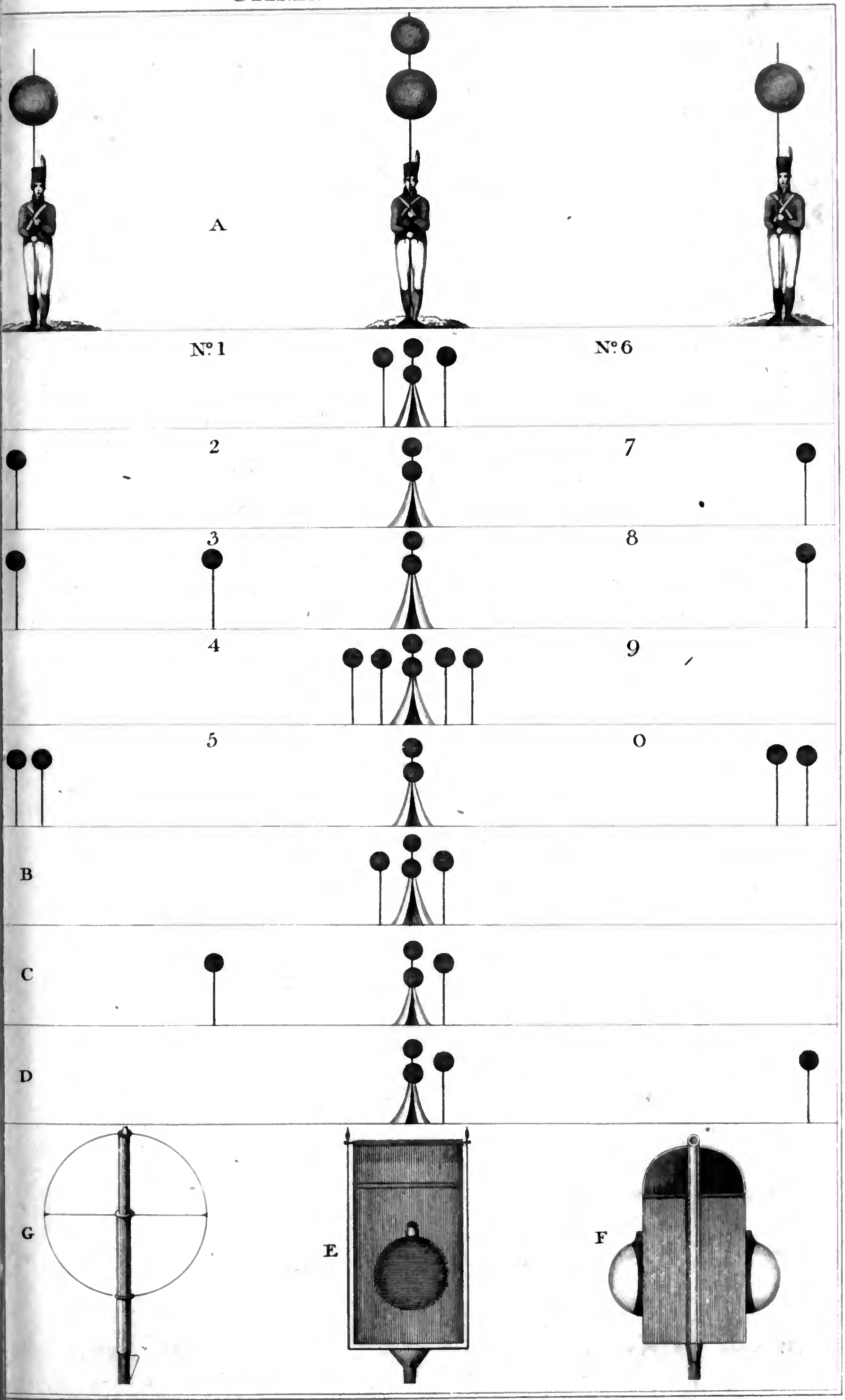


Cooper sculp.

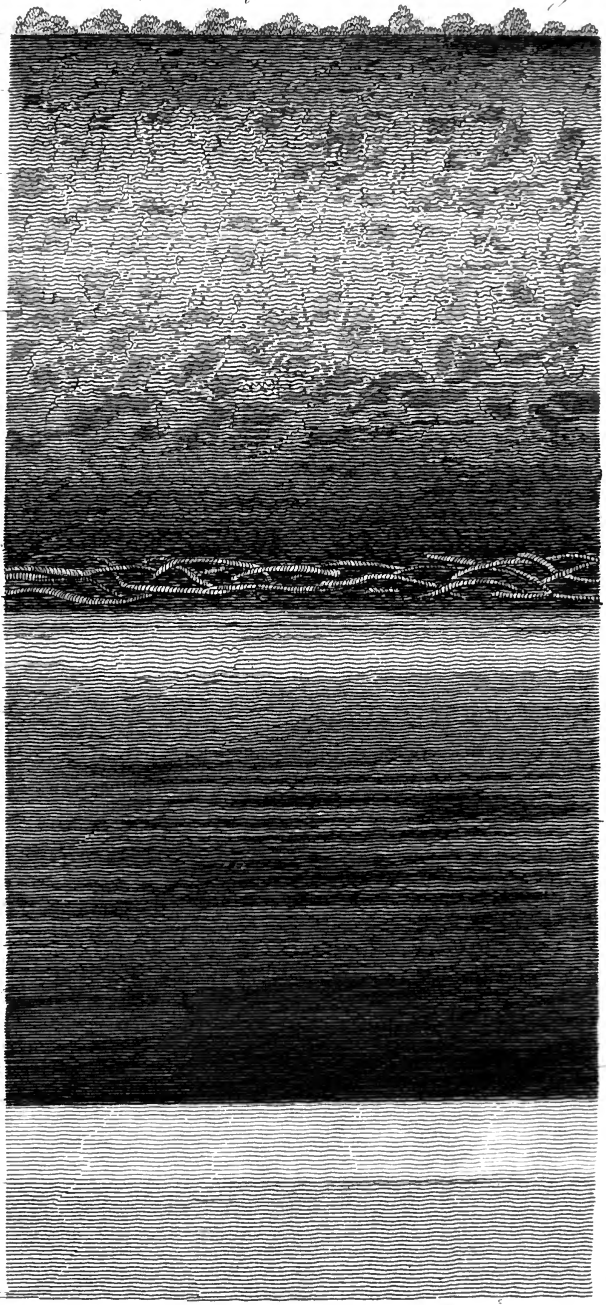


Cooper sculp.

CAMP TELEGRAPH.



Section of a Turf-bank in Timahoe Bog.



No. 1. 2 Feet.
No. 2. 3 Feet.
No. 3. 5 Feet.
No. 4. 8½ Feet.
No. 5. 3 Feet.
No. 6. 3 Feet.
No. 7. 10 Feet.
No. 8. 4 Feet.
No. 9. 3 Feet.
No. 10. 4 Feet.

No. 11. *Clay mixed with limestone Gravel. Depth unknown.*

A transverse Section of Lullymore Bog, part of the great Bog of Allen in Kings & Kildare Counties in Ireland, taken along the straight or Moss's Road, shewn in the Map published in the 1st Report to the House of Commons (20 June 1810) by Richard Griffith Jun.

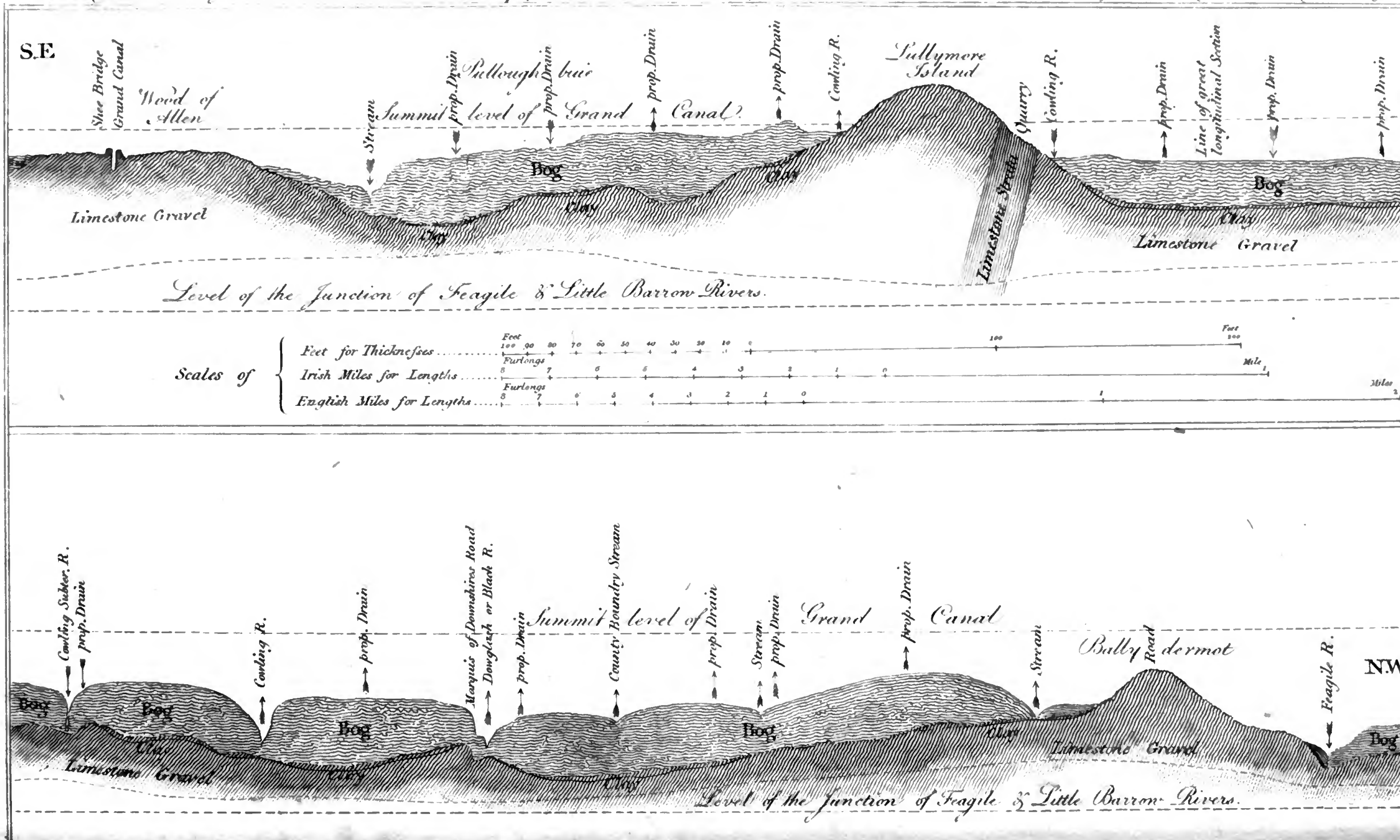


Fig. 1.

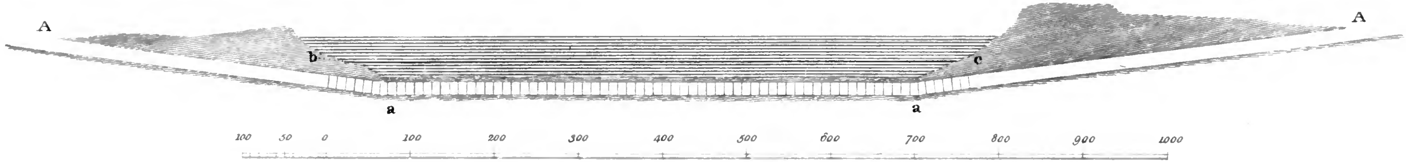


Fig. 2.

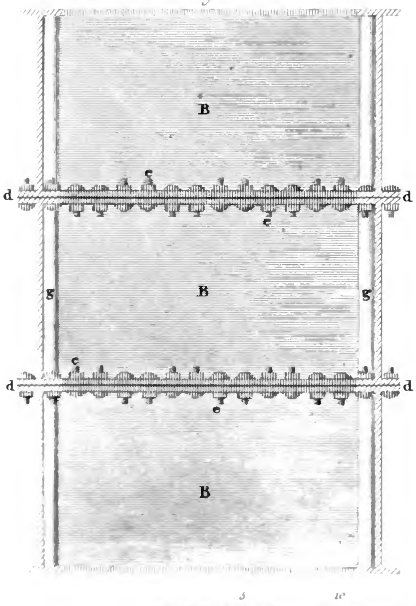


Fig. 4.

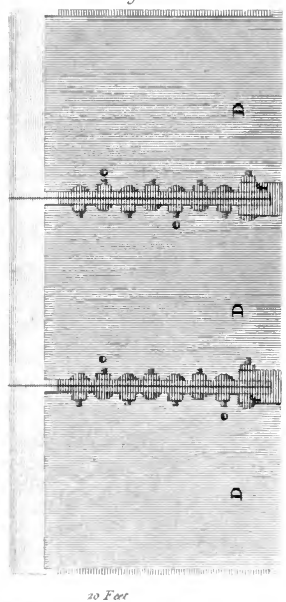
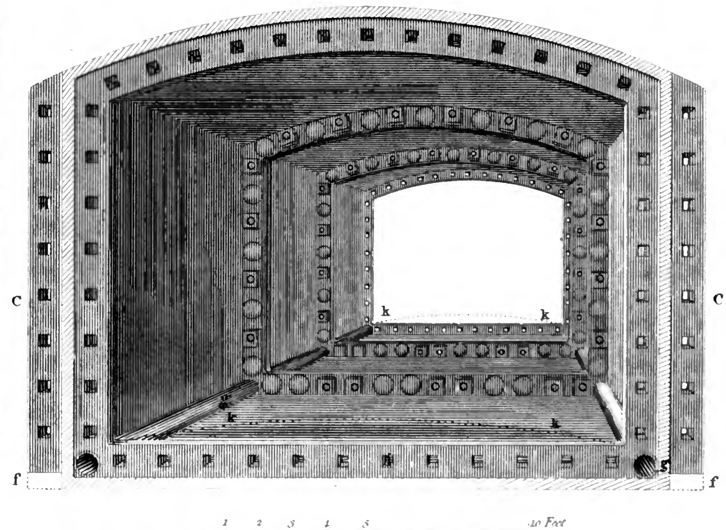


Fig. 3.



Design for a Cast Iron Tunnel to Cross the Thames.