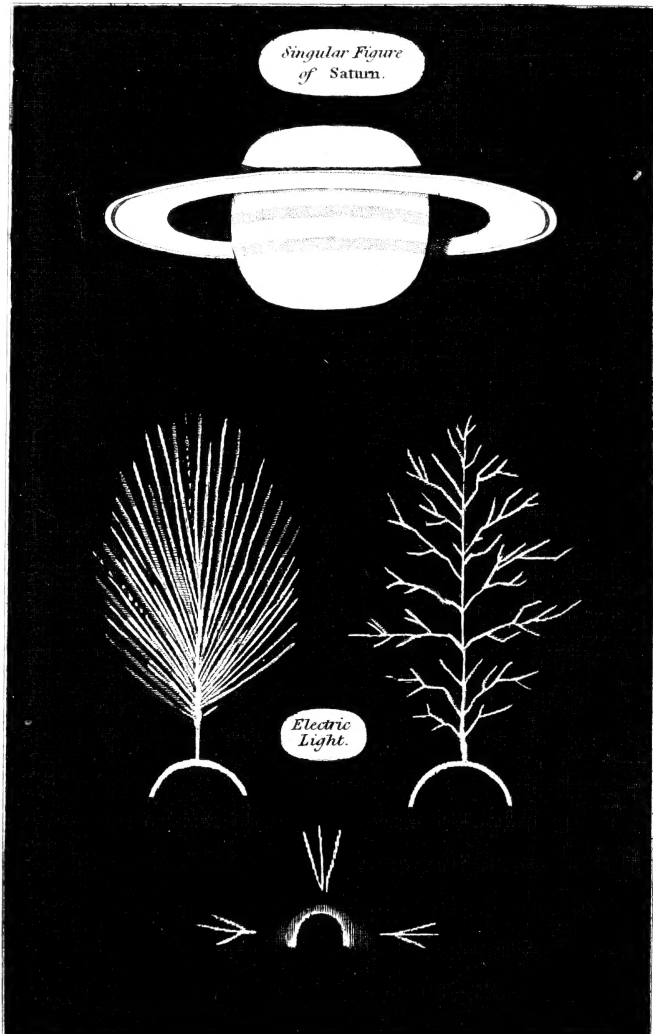

Fig. 1.

Fig. 1.

Fig. 2.

Fig. 3.

Mutlow Sc. Russell Cot.

A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

VOL. XIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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Fig. 1.

PREFACE.

THE Authors of Original Papers are, J. Gough, Esq.; Dr. Beddoes; H. B. K.; A Corespondent; J. S. Butt, Esq.; Mr. Richard Winter; H. Davy, Esq. F. R. S.; W. N.; Mr. Florian Jolly; Mr. R. Harrup; Mr. Alex. Crombie; Mr. James Stodart; A. F.; K. H. D.; Mr. J. W. Boswell; G. C.; W. Brande, Esq.; M. Cowan, Esq.; Mr. T. Northmore; Mr. J. Martin; T. Young, M. D. F.R.S.; Mr. J. Dalton; Dr. Okely; Mr. H. Steinhauer; J. Bostock, M. D.; A. T.; Amicus; A. B. C.

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And of English Memoirs abridged or extracted, William Herschel, L. L. D. F. R. S.; J. Horsburgh, Esq.; Henry Cavendish, F. R. S.; C. Hatchett, Esq.; Dr. Balfour; M. Flinders, Esq.; W. H. Wollaston, M. D. Sec. R. S.; Rev. W. Gregor; Mr. W. Shirreff; Rev. Dr. W. Richardson; Benjamin Smith Barton, M. D.; Dr. Holme; Sir James Hall, Bart. F. R. S.; Mr. B. Gibson; T. A. Knight, Esq. F. R. S.; Rt. Rev. Bishop Madison; Mr. B. Gibson; Mr. Thomas Earnshaw.

Soho Square, London, May 1, 1806.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JANUARY, 1806.

ARTICLE I.

On the Cause of Fairy Rings. *By* JOHN GOUGH, *Esq.*

To Mr. NICHOLSON,

SIR, *Middleshaw, December* 7, 1805.

YOU published in the first page of your ninth volume in octavo, a letter to me from the Rev. Jonathan Wilson, vicar of Biddulph, in which the appearance of a patch of ground recently blasted and torn up by lightning was described. The observations of this ingenious and accurate gentleman promised to throw light on the natural history of fairy-rings, provided they were continued, and in this expectation, I took the liberty in a note subjoined to the copy of his letter printed by you, to request his future remarks on the subject, drawn from repeated inspection of the place affected by the lightning. The following is an extract of a letter from Mr. Wilson, containing his observations relative to the subsequent appearances of the patch, with some thoughts which are certainly an improvement in one theory of fairy-rings, that has received the patronage of some writers. This letter is dated November 1, 1805, and after some prefatory matter, proceeds thus,

Introductory remarks.

Mr. Wilfon's remarks begin.

" In confequence of your efteemed favour of the 14th of Auguft, 1804, I went on the 2nd of September following, to view the place which the lightning had ftruck, being accompanied by the farmer of the grounds. The affected fpot was not then very eafy to be diftinguifhed, as the injured thiftles were generally overtopped at the time, but we had no doubt of its true fituation, upon finding the place where we formerly dug in purfuit of the imaginary ftone. Some dead grafs appeared, but it was confined to the fpace within which the roots had been plowed up by the electric fluid. The verdure was undoubtedly brighter about the hole, and the farmer was willing to attribute the flourifhing ftate of this circle of herbitage to the lightning; but for my part, I afcribed it to what had dropped from his cows, rather than any thing that had fallen from the clouds.

The place not eafy to be found.

Slight veftiges of the lightning.

These veftiges not permanent.

" I have not been able to perceive the leaft difference between the part ftruck, and the reft of the field, during the courfe of the prefent year; my obfervations muft therefore be acknowledged not to favour the hypothefis, which fuppofes fairy-rings to be formed at firft by lightning.

The explanation by grubs improved.

" I never faw a fairy-ring, and therefore may feem badly qualified to write on the fubject; but from what I have read, it appears to me, that the circle of decayed grafs is caufed by the innumerable grubs, which are faid to lay concealed under the ring among the roots of the herbitage; I alfo fuppofe, that the fungufes commonly feen on fairy-rings, give a preference to thefe circles on account of the abundance of dead vegetable matter to be found in them; amongft which various fpecies of fungi are known to grow. To this I may add, that the interior circle of dark green grafs is owing to the dung, and ploughing of thofe animals in the preceding year; and the reafon which compels thefe grubs or their offspring to pufh forward from the centre, feems to be this; every creature we know of has an averfion to working in its own excrement, or that of its own fpecies."

Reflections on thefe obfervations.

The obfervations of Mr. Wilfon, ftated above, feem to demonftrate, that a patch of herbage is not invariably converted into a fairy-ring by a powerful ftroke of lightning; confequently if electric difcharges from the atmofphere be the primary caufes of thefe circles, they require the affiftance of fome peculiarity in the foil to give permanency to the appearance.

ance. Moreover the circular figure of thefe phenomena embarraffes the electrical hypothefis with a fecond difficulty; for the tracts of difcoloured grafs, actually produced by lightning, are but feldom bent into rings, as they more frequently affume a zigzag, or elfe a ramified form, and are, I believe, of but fhort duration; which fhews, tbe roots of the herbage are not deftroyed, unlefs where the earth is torn up. The theory which attributes thefe circles of withered grafs to the running of a fungus, has little or no foundation; becaufe thefe imperfect-plants, generally fpeaking, attach themfelves to dead vegetables, confequently their prefence in fairy-rings is nothing more than an appearance which is fubfequent to the deftruction of the herbage upon them. As for the lively verdure of the grafs on the interior edges of thefe circles, I believe it may be explained upon general principles, without the agency of lightning or fungi. For if the herbage of a patch of ground be deftroyed root and branch by a caufe which does not remove the remains of it, the place will be covered, in procefs of time, with a frefh crop of plants, poffeffing fuperior luxuriancy and verdure. The caufes of this vigorous vegetation appear to be the following; the dead roots and ftems rot and manure the foil which produced them: this fource of fertility receives an additional fupply from a fucceffion of fungi, which grow and decay on the furface of the ground, as well as from the excrement and exuvia of the grubs, which take up their abode among the withered roots; laftly, the foil is rendered more porous by the decay of the vegetable remains, and thereby becomes more permeable to the air, which increafes its fertility not a little. The laft pofition feems to be confirmed by the circumftance of plants thriving better in unglazed, than in glazed flower pots. Tbe following facts may be adduced in corroboration of what has been here advanced. A fmall piece of ground was covered, in June, with common falt, which had been rubbed upon the corps of a drowned man; the herbage of this place died in a fhort time; but was fucceeded the next fummer by a new crop, the verdure of which diftinguifhed it for fome years from the furrounding grafs. As common falt is efteemed a manure, perhaps the following inftance will be called preferable to the former. Many woods in this country, efpecially thofe about Windermere, are cut

Difficulties of the electrical theory.

Fungi not the caufe of fairy rings.

Superior verdure of thefe rings explained.

Dead plants, &c. manure the ground.

The air increafes vegetation by acting on the roots.

Proofs of the preceding theory.

down about once in fourteen years, and converted into charcoal, for the uſe of the iron works. This is done by throwing the branches into large heaps; which are then covered with green turf, and ſet on fire. Theſe piles of wood continue burning for ſeveral days, in conſequence of which, the roots in the ground beneath them are completely charred; and the pit-ſtead, as the place is called, has no verdure left upon it. The loſs however is repaired in the courſe of a few years by nature herſelf, where art does not interfere, and the ſpot is clothed with a freſh coat of herbage, conſiſting of herbs remarkable for their ſize and flouriſhing appearance. This inſtance of vigorous vegetation on ground that has been completely burned, in all probability, is occaſioned by the texture of the ſoil; which is adapted to retain moiſture, and admit the air; unleſs we ſuppoſe the incorruptible ſubſtance of charcoal to afford a ſpecies of manure.

The preceding hints may perhaps incite ſome of your readers to ſtudy the natural hiſtory of fairy-rings with greater diligence, eſpecially to ſearch after the true cauſe which blaſts theſe circles; for when this is diſcovered, we ſhall be able to re-clothe them with ſuperior verdure, on rational principles.

I remain, &c.

JOHN GOUGH.

II.

Obſervations on the ſingular Figure of the Planet Saturn. By WILLIAM HERSCHEL, *LL. D. F. R. S.**

Examination of the ſtriking phenomena of the planet Saturn.

THERE is not perhaps another object in the heavens that preſents us with ſuch a variety of extraordinary phenomena as the planet Saturn: a magnificent globe, encompaſſed by a ſtupendous double ring: attended by ſeven ſatellites: ornamented with equatorial belts: compreſſed at the poles: turning upon its axis: mutually eclipſing its ring and ſatellites, and eclipſed by them: the moſt diſtant of the rings alſo turning upon its axis, and the ſame taking place with the fartheſt of the ſatellites: all the parts of the ſyſtem of Saturn occaſionally

* Philoſophical Tranſactions, 1805.

reflecting

reflecting light to each other: the rings and moons illuminating the nights of the Saturnian: the globe and satellites enlightening the dark parts of the rings: and the planet and rings throwing back the sun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

It must be confessed that a detail of circumstances like these, appears to leave hardly any room for addition, and yet the following observations will prove that there is a singularity left, which distinguishes the figure of Saturn from that of all the other planets.

It has already been mentioned on a former occasion, that so far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781 that it was flattened at the poles at least as much as Jupiter, I was insensibly diverted from a more critical attention to the rest of the figure. Prepossessed with its being spheroidical, I measured the equatorial and polar diameters in the year 1789, and supposed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally ascribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might still have remained, had not a late examination of the powers of my ten-feet telescope convinced me that I ought to rely with the greatest confidence upon the truth of its representations of the most minute objects I inspected.

Its body oblate.

Reasons why the greater peculiarities of its figure were overlooked;

The following observations, in which the singular figure of Saturn is fully investigated, contain many remarks on the rest of the appearances that may be seen when this beautiful planet is examined with attention; and though they are not immediately necessary to my present subject, I thought it right to retain them, as they show the degree of distinctness and precision of the action of the telescope, and the clearness of the atmosphere at the time of observation.

April 12, 1805. With a new 7-feet mirror of extraordinary distinctness, I examined the planet Saturn. The ring reflects more light than the body, and with a power of 570 the colour of the body becomes yellowish, while that of the ring remains more white. This gives us an opportunity to distinguish the ring

Very perfect observation, in which the circumference was seen to be flattened in four regions.

ring from the body, in that part where it croffes the difk, by means of the difference in the colour of the reflected light. I faw the quintuple belt, and the flattening of the body at the polar regions; I could alfo perceive the vacant fpace between the two rings.

Obfervations on Saturn by which its fingular figure is afcertained.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it feems not to begin till at a high latitude, and there to be more fudden than it is towards the poles of Jupiter. I have often made the fame obfervation before, but do not remember to have recorded it any where.

April 18; ten-feet reflector, power 300. The air is very favourable, and I fee the planet extremely well defined. The fhadow of the ring is very black in its extent over the difk fouth of the ring, where I fee it all the way with great diftinctnefs.

The ufual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be feen in the figure I have given in Plate I.; and alfo in a former reprefentation of the fame belts in 1794.*

The figure of the body of Saturn, as I fee it at prefent, is certainly different from the fpheroidical figure of Jupiter. The curvature is greateft in a high latitude.

I took a meafure of the fituation of the four points of the greateft curvature, with my angular micrometer, and power 527. When the crofs of the micrometer paffed through all the four points, the angle which gives the double latitude of two of the points, one being north the other fouth of the ring, or equator, was 93° 16′. The latitude therefore of the four points is 46° 38′; it is there the greateft curvature takes place. As neither of the crofs wires can be in the parallel, it makes the meafure fo difficult to take, that very great accuracy cannot be expected.

The moft northern belt comes up to the place where the ring of Saturn paffes behind the body, but the belt is bent in a contrary direction being concave to the north, on account of its croffing the body on the fide turned towards us, and the north pole being in view.

There is a very dark, but narrow fhadow of the body upon the following part of the ring, which as it were cuts off the ring from the body.

* See Phil. Tranf. for 1794, Table VI. page 32.

The

The ſhadow of the ring on the body, which I ſee ſouth of the ring, grows a little broader on both ſides near the margin of the diſk.

Obſervations on Saturn, by which its ſingular figure is aſcertained.

The diviſion between the two rings is dark, like the vacant ſpace, between the anſæ, but not black like the ſhadow I have deſcribed.

There are four ſatellites on the preceding ſide near the ring; the largeſt and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet teleſcope, both mirrors of which are very perfect. I ſaw all the phenomena as deſcribed laſt night, except the ſatellites, which had changed their ſituation; four of them being on the following ſide. This teleſcope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we ſee things at firſt ſight that would otherwiſe have eſcaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is ſomewhat like a ſquare or rather parellelogram, with the four corners rounded off deeply, but not ſo much as to bring it to a ſpheroid. I ſee it in perfection.

The four ſatellites that were laſt night on the preceding, are now on the following ſide, and are very bright.

I took a meaſure of the poſition of the four points of the greateſt curvature, and found it 91° 29′. This gives their latitude 45° 44′,5. I believe this meaſure to be pretty accurate. I ſet firſt the fixed thread to one of the lines, by keeping the north-preceding and ſouth-following two points in the thread; then adjuſted the other thread in the ſame manner to the ſouth-preceding and north-following points.

May 5, 1805. I directed my 20-feet teleſcope to Saturn, and, with a power of about 300, ſaw the planet perfectly well defined, the evening being remarkably clear. The ſhadow of the ring on the body is quite black. All the other phenomena are very diſtinct.

The figure of the planet is certainly not ſpheroidical, like that of Mars and Jupiter. The curvature is leſs on the equator and

Obfervations on Saturn, by which its fingular figure is afcertained.

and on the poles than at the latitude of about 45 degrees. The equatorial diameter is however confiderably greater than the polar.

In order to have the teftimony of all my inftruments, on the fubject of the ftructure of the planet Saturn, I had prepared the 40-feet reflector for obferving it in the meridian. I ufed a magnifying power of 360, and faw its form exactly as I had feen it in the 10 and 20-feet inftruments. The planet is flattened at the poles, but the fpheroid that would arife from this flattening is modified by fome other caufe, which I fuppofe to be the attraction of the ring. It refembles a parallelogram, one fide whereof is the equatorial, the other the polar diameter, with the four corners rounded off fo as to leave both the equatorial and polar regions flatter than they would be in the regular fpheroidical figure.

The planet Jupiter being by this time got up to a confiderable altitude, I viewed it alternately with Saturn in the 10-feet reflector, with a power of 500. The outlines of the figure of Saturn are as defcribed in the obfervation of the 40-feet telefcope; but thofe of Jupiter are fuch as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-feet telefcope of 24 inches aperture; and faw plainly that the former planet differs much in figure from the latter.

The temperature of the air is fo changeable that no large mirror can act well.

May 13. 10-feet reflector, power 300. The fhadow of the ring upon the body, and of the body upon the ring, are very black, and not of the dufky colour of the heavens about the planet, or of the fpace betwen the ring and planet, and between the two rings. The north-following part of the ring, clofe to the planet, is as it were cut off by the fhadow of the body; and the fhadow of the ring lies fouth of it, but clofe to the projection of the ring.

The planet is of the form defcribed in the obfervation of the 40-feet telefcope; I fee it fo diftinctly that there can be no doubt of it. By the appearance, I fhould think the points

points of the greateſt curvature not to be ſo far north as 45 degrees.

Obſervations on Saturn, by which its ſingular figure is aſcertained.

The evening being very calm and clear, I took a meaſure of their ſituation, which gives the latitude of the greateſt curvature 45° 21′. A ſecond meaſure gives 45° 41′

Jupiter being now at a conſiderable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is leſs than on Saturn.

I repeated theſe alternate obſervations many times, and the oftener I compared the two planets together, the more ſtriking was their different ſtructure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two meaſures of the diameter of the points of greateſt curvature. A mean of them gave 64,3 diviſions = 11″,98. After this, I took alſo two meaſures of the equatorial diameter, and a mean of them gave 60,5 diviſions = 11″,27; but the equatorial meaſures are probably too ſmall.

To judge by a view of the planet, I ſhould ſuppoſe the latitude of the greateſt curvature to be leſs than 45 degrees. The eye will alſo diſtinguiſh the difference in the three diameters of Saturn. That which paſſes through the points of the greateſt curvature is the largeſt; the equatorial the next, and the polar diameter is the ſmalleſt.

May 27. The evening being very favourable, I took again two meaſures of the diameter between the points of greateſt curvature, a mean of which was 63,8 diviſions = 11″,88. Two meaſures of the equatorial diameter gave 61,3 diviſions = 11″,44.

June 1. It occurred to me that a more accurate meaſure might be had of the latitude in which the greateſt curvature takes place, by ſetting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following meaſures were taken in this manner, and are more ſatisfactory than I had taken before. The firſt gave the latitude of the ſouth-preceding point of greateſt curvature 43° 26′; and the ſecond 43° 13′. A mean of the two will be 43° 20′.

June

Obfervations on Saturn, by which its fingular figure is afcertained.

June 2. I viewed Jupiter and Saturn alternately with a magnifying power of only 300, that the convexity of the eye-glafs might occafion no deception, and found the form of the two planets to differ in the manner that has been defcribed.

With 200 I faw the difference very plainly; and even with 160 it was fufficiently vifible to admit of no doubt. Thefe low powers fhow the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in paffing is leffened, we are more at liberty to fix our attention upon them.

I compared the telefcopic appearance of Saturn with a figure drawn by the meafures I have taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789;* and found that, in order to be a perfect refemblance, my figure required fome fmall reduction of the longeft diameter, fo as to bring it nearly to agree with the meafures taken the 27th of May. When I had made the neceffary alteration, my artificial Saturn was again compared with the telefcopic reprefentation of the planet, and I was then fatisfied that it had all the correctnefs of which a judgment of the eye is capable. An exact copy of it is given in Plate IX. The dimenfions of it in proportional parts are,

The diameter of the greateft curvature -	36
The equatorial diameter - - - - -	35
The polar diameter - - - - - -	32
Latitude of the longeft diameter -	43° 20.'

The foregoing obfervations of the figure of the body of Saturn will lead to fome intricate refearches, by which the quantity of matter in the ring, and its folidity, may be in fome meafure afcertained. They alfo afford a new inftance of the effect of gravitation on the figure of planets; for in the cafe of Saturn, we fhall have to confider the oppofite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been afcertained in fome of my former Papers.

* See Phil. Tranf. for 1790, page. 17.

Facts

III.

Facts and Obfervations on the medical Refpiration of gazeous Oxide of Azote. In a Letter from Dr. BEDDOES.

To Mr. NICHOLSON.

SIR,

DR. Pfaff's paper on refpiration * will probably draw the attention of the fcientific towards the gafeous oxide of azote, which has been too much neglected in a medical point of view. I was only forry to fee that he propofes to ufe it in melancholia. No combination of ideas can be more obvious than the application of an agent which has fo frequently proved exhilarating, and never yet been obferved to be followed by exhauftion where it did exhilarate, to a complaint, in which depreffion of fpirits is a ftriking circumftance. But I am apprehenfive that the firft thoughts of inexperience here (as fo often happens) will prove illufory, and that this project will not be followed by the expected advantage in many cafes of melancholia. For if it be true that there is no real diftinction between mania and melancholia, as far as the fenforium is concerned, and that the vivacity of ideas in melancholia anfwers to the violence of mufcular actions in mania, as I have endeavoured to fhew in my *Effays on Health*; is there not ground to apprehend that the actions of the brain, already too ftrong, will be increafed by this gas, or the difeafed contemplations rendered more intenfe?

Dr. Pfaff's experiments on refpiration.

Propofes gafeous oxide of azote in melancholy madnefs.

If there be any ftate of melancholia in which it may be of fervice, this will probably happen when the nervous fyftem is falling into debility, in confequence of having been kept too much on the ftretch.

Cautions againft its unguarded ufe, from theory;

But I do not here warn againft gafeous oxide from mere theory. The manager of a lunatic afylum near Briftol, refpectably known to the public, concurred with me fome years ago in the opinion which I expreffed to him concerning its probable advantage in melancholia; and a patient that had been under his care inhaled it fairly without benefit. The adminiftration was tried in two other cafes as fruitlefsly: Indeed

from experience.

Cafes.

* Philof. Journal. XII. 249.

I dif-

I diſcontinued it in one, from ſome indications of an aggravation of the ſymptoms. I was by this time alive to ſuſpicion, having thought much on the ſubject, and reaſoned myſelf into the idea that it would often do injury upon the above-mentioned principle. It has long been my opinion, and there are ſtriking obſervations on record to prove that hidrogen, hidro-carbonate, azotic, or carbonic acid gaſes, would be more likely to anſwer in active inſanity under whatever form. Theſe obſervations I ſhall take occaſion to quote hereafter.

Uſe in palſy of one kind.

The very firſt time I witneſſed the effects of gaſeous oxide on a perſon in health, I concluded that it would be a remedy in certain caſes of palſy. A patient who had emerged from apoplexy with the loſs of the power of one ſide of his body, was accordingly put under a courſe of the gas. The reſult completely anſwered expectation. The caſe was moſt carefully watched; and on withholding the gas, the ſymptoms repeatedly grew worſe, and *vice verſa.* After the patient's recovery, he was kept under inſpection for a conſiderable time, and did not relapſe. This has been confirmed by other reſults; and in palſy, where the brain is primarily affected, I expect that Dr. Pfaff will find either a cure or great relief to follow the uſe of this gas in a reſpectable proportion of caſes.

In another kind of palſy.

I have very fairly tried it in palſy apparently from cold, beginning at the extremities and creeping from muſcle to muſcle, without good or bad effect. There is a caſe of this kind, related by Dr. Kentiſh, with the patient's name, and corroborated by teſtimony ſuperior to all exception in *Conſiderations on factitious Airs (Johnſon)* in which a perfect cure was obtained from oxigen gas; and I have ſince learned by experiments carefully repeated before various philoſophical obſervers, that *in eſſential reſpects, oxigen gas and gaſeous oxide act in a very different, nay oppoſite manner upon the living fibre.*

Of oxigen,

Theſe experiments I hope to publiſh before midſummer.

From palſy, analogy led me to other caſes of debility. I fully tried gaſeous oxide in dropſy of the cheſt (anaſarca of the lungs), but without good or bad effect. I was much diſappointed, conceiving that in dropſy (at leaſt in one ſpecies) we have a paralytic ſtate of the lymphatics. But I have been ſince aſſured by a phyſician, that for ſome dropſies he has found

in dropſies;

found a remedy in this gas. There are dropfies which doubtlefs depend on excefs of exhalant action. Thefe are eafily diftinguifhed; and they require bleeding as much as pleurify.

In debility, arifing from refidence in hot climates and from intenfe application to bufinefs, I have known gafeous oxide completely fuccefsful after an infinity of remedies, Bath and other waters, had been tried in vain. In other ftates of debility.

The particulars of thefe cafes are alfo deftined for publication: But I refolved to wait for fome years after the ufe of the gas; for I have found that a fingle circumftance vitiates a large proportion of our medical records. Patients after an apparent recovery fall again into the fame complaint; and there are other confiderations, which I fhall for the prefent pafs over.

If Mr. Pfaff ufes gafeous oxide in palfy, he will probably fooner or later fee a phenomenon as extraordinary as any in galvanifm, and which after it has been defcribed by a philofopher of high reputation, will become equally celebrated. This is the inftantaneous reftoration of voluntary power over a limb deprived both of motion and feeling by palfy fucceeding to apoplexy, while the patient is refpiring gafeous oxide. This was witneffed in common with myfelf, by feveral refpectable perfons; and among others by fome of your philofophical acquaintance, if I do not miftake. It was in the cafe of Mr. G. a member of the laft parliament, who completely recovered: But as other means were afterwards adopted, I do not impute the refult to the gas, which however, when ufed alone, was vifibly of great fervice: for I have no idea of claiming for a remedy under fcrutiny any cure, if other powers have been called in at the fame time. Gafeous oxide has given voluntary power over palfied parts, while inhaled.

I tranfmit thefe obfervations to you, Sir, in preference to the Editor of any Medical Journal, becaufe I think them likely to meet the eye of Dr. Pfaff fooner in your Journal. I fhould be extremely forry that he fhould fet out wrong in his trials, becaufe the fault will be imputed to the power itfelf, and not to its mifapplication; and the difabled will ftill be left to languifh and be cut off, notwithftanding we have a remedy at hand. Firft wifhed for effect of this ftatement.

I have another reafon. I moft fincerely wifh any thing I could fay would haften the period, which *muft* arrive, when medical fcience fhall not be merely what the Germans call a A fecond.

Brod-

Brod-wissenschaft, or pursued only for a livelihood. If philosophical men without a profession would take it up, it is I think certain, that it must soon become both more efficient and more liberal. Any study is capable of interesting the feelings; and most surely that of the laws of the organic world is as much so as any other. Opportunities of anatomical, chemical, and *clinical* information are at hand. A person so prepared will, heaven knows, with ardour and industry soon acquire all that is useful in medical practice. Let him then, animated with no other motive than the pure desire of benefiting his fellow men, apply himself to the improvement of medicine.

Men of liberal curiosity exhorted to study medicine.

It is impossible that he should not succeed as fully as our Tennants, our Hatchetts, and Chenevix's have done in chemistry; for it is not its inherent difficulty, but collateral circumstances, that retard the progress of this art. Many apothecaries, for example, and old women in general, who are the great controulers of the destiny of physicians, would by no means allow the use of gaseous oxide in palsy, though the patient in the course both of nature and of ordinary medication be sure to die, and perhaps in a very miserable manner. But the philosophical cultivator of medicine, without troubling himself about the good opinion of the one or the other, would proceed on his career under the guidance of the collective light of science and of humanity.

Anecdote.

N. N. advanced in years, of a thickset stature, and with a short neck, shewed signs of palsy many years ago. The writer of these lines warned his friends of the danger. Concurring in this apprehension, Dr. Ingenhousz proposed to him to inhale oxigen gas, a practice familiar to that accurate philosopher, and by which he hoped the constitution might be recruited. The execution of the idea was deferred. Meanwhile the gaseous oxide was discovered to be respirable, and its power in palsy was to a degree ascertained. The writer now pressed the use of this gas with the utmost earnestness. The patient saw it taken by others: He himself consented to inhale it, when behold! the distress of a lady present, as excited by some apprehended imaginary bad consequences, put off the inhalation. The predicted paralytic seizure arrived: but there was ample time still for the use of the oxide. I proposed that another patient, situated as similarly as possible, should be sought; and that if he consented upon the credit

credit of the ſucceſsful exhibition, and upon my reſponſibility, to uſe the gas, the reſult ſhould determine as to its employment in the caſe firſt in queſtion. At the ſame time, I ſtated from the average courſe of paralytic attacks in general not immediately fatal, that a little apparent amendment would take place, and the ſtroke return with additional violence. My propoſal was acknowledged to be highly reaſonable; but that plan of routine treatment was followed which is ſo much more advantageous to the idle and unſcientific of our profeſſion than it is to the ſick, and the patient died of a return of his complaint. Such is probably the condition of thouſands of the diſeaſed at this moment! Rather than uſe a recently propoſed plan not in the Pharmacopœia, or ſeek a new one in analogy, we perſevere in painful or diſguſting means, from which, on the faith of long experience, no good of any ſort can be expected for the ſufferer. May the riſing generation of natural philoſophers exerciſe their talents and their benevolence in putting an end to ſo crying an evil!

I am, Dear Sir,

Reſpectfully your's,

THOMAS BEDDOES.

Clifton, Dec. 13, 1805.

P. S. A caſe in your Journal, where a gentleman accuſtomed to breathe gaſeous oxide for amuſement, experienced very diſagreeable feelings on one particular occaſion, ſeems to me clearly referable to hyſteria. Now the trials at the Pneumatic Inſtitution, as related in Mr. Davy's *Reſearches*, had clearly ſhewn that in the prediſpoſed, gaſeous oxide is a ſpecific for exciting an hyſteric paroxyſm. Perhaps in the individual whoſe caſe is related by himſelf in the Journal, no obvious prediſpoſition, either temporary or permanent, exiſted: Nothing to this purport is ſtated. But that the affection was ſimply hyſterical cannot I think be doubted by any one converſant both with hyſteria and the adminiſtration of gaſeous oxide. It ſeems to be ſtrongly marked by that idea of immediate danger, which is ſo common in hyſteria. Dr. Garnet very unneceſſarily, and, I believe, very miſtakenly, called up the whole Brunonian theory on the emergency. It led him, however, to give cordials; and they were proper. A tea-ſpoonful of ſal volatile, from

On effects of gaſ. ox. as ſtated in this Journal.

Their real nature.

from time to time, would probably have anfwered without the Brunonian theory. But it is certainly the bufinefs of the phyfician to avoid gafeous oxide in the hyfterical, as it is wine in thofe who labour under acute inflammation. If your correfpondent who related his own feelings could fpecify any caufe which might have rendered him nervous, or ftate the fact whether he was fo or not, it would give fatisfaction to the prefent writer, and perhaps alfo to future inquirers.

Caution regarding particular fubjects. Quære.

To interdict a remedy becaufe its ufe requires difcrimination, would, in many diforders, be leaving the fick to certain deftruction. I imagine that the outcry againft fuch means as gafeous oxide, will arife from thofe who daily ufe the moft hazardous remedies, and who are enabled to do it without reproach, becaufe they are put into a phial, and the patient and his friends never trouble themfelves about the nature of the articles which they are receiving into the ftomach.

Remark on dangerous diforders.

IV.

Abftract of Obfervations on a diurnal Variation of the Barometer between the Tropics. By J. HORSBURGH, *Efq. In a Letter to* HENRY CAVENDISH, *Efq. F. R. S.** *Read March* 14, 1805.

SIR, *Bombay, April*, 20, 1804.

WHEN I was in London at the conclufion of the year 1801, I had the pleafure of being introduced to you by my friend Mr. Dalrymple, at which time he prefented you with fome fheets of meteorological obfervations, with barometer and thermometer, made by me in India, and during a paffage from India to England.

Tropical variation of the barometer.

Being of opinion that few regifters of the barometer are kept at fea, efpecially in low latitudes, I have been induced to continue my obfervations fince I left England, judging that, even if they were found to be of no utility, they might at leaft be entertaining to you or other gentlemen, who have been making obfervations of a fimilar nature.

During my laft voyage I have employed two marine barometers, one made by Troughton, the other by Ramfden,

* Philofophical Tranfactions, 1805.

and

and a thermometer by Frazer. Thefe were placed expofed to a free current of air in a cabin, where the bafons of the barometers were 13 feet above the level of the fea.

Tropical variation of the barometer.

The hours at which the heights of the barometers, and thermometers were taken, *viz*. noon, 4 hours, 10 hours, 12 hours, 14 hours, and 19 hours, were chofen, becaufe at thefe times the mercury in the barometer had been perceived to be regularly ftationary between the tropics, by former obfervations made in India in 1800 and 1801. It was found that in fettled weather in the Indian feas, from 8 A M to noon, the mercury in the barometer was generally ftationary, and at the point of greateft elevation; after noon it began to fall, and continued falling till 4 afternoon, at which time it arrived at the loweft point of depreffion. From 4 or 5 P M the mercury rofe again, and continued rifing till about 9 or 10 P M, at which time it had again acquired its greateft point of elevation, and continued ftationary nearly till midnight; after which it began to fall, till at 4 A M it was again as low as it had been at 4 afternoon preceding; but from this time it rofe till 7 or 8 o'clock, when it reached the higheft point of elevation, and continued ftationary till noon.

Thus was the mercury obferved to be fubject to a regular elevation and depreffion twice in every 24 hours in fettled weather; and the loweft ftation was obferved to be at about 4 o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low ftation, but had a regular tendency to rife from thence till towards 8 in the morning and about 9 in the evening, and from thofe times continued ftationary till noon and midnight.

In unfettled blowing weather, efpecially at Bombay during the rains, thefe regular ebbings and flowings of the mercury could not be perceived; but a tendency to them was at fome times obfervable when the weather was more fettled.

In the fheets, which I formerly prefented to you, were evinced thefe elevations and depreffions twice every 24 hours within the tropics, in fteady weather, as had been obferved by Meff. Caffan and Peyroufe, by Dr. Balfour of Calcutta, and others. But fince my laft arrival in India, I have obferved that the atmofphere appears to produce a different effect on the barometer at *fea* from what it does on *fhore*.

 As

Tropical variation of the barometer.

As I am ignorant whether this phenomenon has been noticed by any perſon before, I will here give you an abſtract of my journal, ſhewing how the barometer has been influenced during the whole time ſince I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at ſea from what it is on ſhore, at thoſe places in India where the obſervations have been made.

The firſt ſheet begins with the obſervations made on board ſhip, in my voyage from London towards Bombay, in the months of April and May, 1802.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° N, longitude 20° W, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depreſſions every 24 hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° N to latitude 10° N, the difference of the high and low ſtations of the mercury in the barometers was not ſo great, as it was from latitude 10° N acroſs the equator, and from thence to latitude 25° S. Within theſe laſt-mentioned limits, the difference of high and low ſtations of the mercury in the barometers was very conſiderable, generally from five to nine hundred parts of an inch, both in the daily and nightly motions.

When we reached the latitude of 28° S, longitude 27° W, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its riſing and falling became irregular and fluctuating during our run from latitude 28° S, longitude 27° W, (moſtly between the parallels of 35° and 36° S,) until we were in latitude 27° S, and longitude 51° E, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly, during our run from the laſt-mentioned poſition, up the Madagaſcar Archipelago, acroſs the Equator, until our arrival at Bombay July 31ſt. 1802.

Auguſt 6th, 1802. When the barometers were placed on ſhore in Bombay, the mercury, for the firſt ſix days, appeared to have a ſmall tendency towards performing the equatropical motions, but not equally perceptible as when at ſea, the difference between the high and low ſtations of the mercury in the

the barometers being great to the day we entered the harbour of Bombay. From the 12th of Auguft to the 22d the mercury could not in general be obferved to have any inclination to perform the equatropical motions, although at times a very fmall tendency towards performing them might be perceived. Tropical variation of the barometer.

On the 23d of Auguft the barometers were taken from the fhore to the fhip. Immediately on leaving Bombay harbour, Auguft 26th, 1802, the mercury in the barometers performed the equatropical motions, and continued them with great uniformity, during our paffage down the Malabar coaft, acrofs the bay of Bengal, in the Strait of Malacca, and through the China Sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly ftationary during the 24 hours, except a fmall inclination at times towards the equatropical motions, but they were not near fo perceptible as at fea; this change taking place the day we got into the river.

During our ftay in China, the barometer on fhore, at Canton, had very little tendency towards the equatropical motions, throughout the months of October and November that we remained there. At times, while in China, a fmall inclination towards performing the equatropical motions appeared: but, as in Bombay, the difference of rife and fall was of fo fmall a quantity, as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river, the equatropical motions were inftantly performed by the mercury, and with great regularity continued during the whole of the paffage to Bombay, until our arrival in that harbour on the 11th of January, 1803.

On January 18th, the barometers were placed on fhore, and did not appear in the fmalleft degree fubject to the equatropical motions; although, with great regularity, they had been performed while at fea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on fhore, feemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly ftationary throughout the 24 hours. But about the latter part of March the mercury feemed to incline towards the equatropical mo-

 tions

Tropical variation of the borometer.

tions in a very ſmall degree; and, during the month of April, and to the 20th of May, this ſmall tendency of the mercury to perform the motions appeared at times, but was hardly diſcernible, the riſe and fall being of ſo ſmall a quantity. From the 18th of January to the 20th of May, the mercury in the barometers was in general ſtationary, except a very ſmall tendency towards the equatropical motions at times. At other times ſome change in the atmoſphere diſturbed the mercury from its ſtationary poſition; but this was ſeldom the caſe, as it was then the fair weather ſeaſon, or north-eaſt monſoon.

We ſailed from Bombay on the 23d of May, 1803. The inſtant we got out of the harbour, the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low ſtations was very conſiderable during the whole of the paſſage to China, excepting a few days in the eaſtern parts of Malacca Strait, where the land lay contiguous on each ſide of us; the difference between the high and low ſtations of the mercury was not then ſo great as in the open ſea. On clearing the Strait, and entering the China Sea, the equatropical motions were performed in greater quantity, and continued regular during our paſſage up the China Sea, until July 2, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceaſed.

From July 8th to September 7th, the barometers were placed on ſhore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a ſtationary poſition, except when influenced by changes of weather. After the barometers were taken from Canton to the ſhip, we were four days in getting clear of the river, in which time the mercury inclined to be ſtationary, excepting that a ſmall inclination towards the equatropical motions ſeemed to evince itſelf at times. But no ſooner had we cleared Canton river, September 13, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depreſſions every 24 hours, at equal intervals of time, (although we were near the land until the 15th September.) And the mercury, with great regularity, continued to perform

form the equatropical motions, from September 13, 1803, the day we cleared the river of Canton, until October 13, when we entered Sincapore Strait, excepting a small degree of irregularity, which affected the mercury on the 22d September, when it blew a gale on the coast of Ifsompa.

Tropical variation of the barometer.

October 13, 1803. On entering the Strait of Sincapore, which is about $3\frac{1}{2}$ leagues wide, the mercury in the barometers was then a little obstructed, and did not perform the equatropical motions, in the same quantity of rise and fall, as when we were in the China Sea. But on the following day, October 14, when we had passed the narrow part of the Strait, the mercury conformed to those motions with regularity until October 21, when we arrived in the harbour of Prince of Wales's Island; then a great retardation took place in the equatropical motions; for, during the time the ship remained in the harbour, from October 20 to November 5, 1803, the mercury in barometers seemed only in a small degree subject to them, the difference between the high and low stations of the mercury, being in general not more than half the quantity, that takes place in the open sea, or at a considerable distance from land. Where the ship lay at this time in the harbour, the land, on one side, was a full quarter of a mile distant, and on the other side about $1\frac{1}{2}$ mile.

On November 5, being clear of the harbour of Prince of Wales's Island, the equatropical motions were instantly performed by the mercury, in the usual quantity experienced at sea, which continued with uniformity until December 3. On this and the following day, the mercury fell considerably during our passage over the tails of the sands at the entrance of Hoogly river, in latitude 21° 06′ N; and on December 5, the day of the moon's last quarter, a gale of wind commenced from N N E, with much lightning and rain in the night. During the latter part of this day, the mercury began to rise, and there soon followed a change of settled weather. When we were in the lower part of the river, the mercury appeared to conform in a small degree to the equatropical motions; but when well up the river, at Diamond Harbour, the mercury inclined to be nearly stationary during the 24 hours, as has formerly been observed to happen in Canton river, Bombay harbour, &c.

On

Tropical variation of the barometer.

On January 13, 1804, after we had cleared the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the paſſage to Bombay, until our arrival there on February 12. The barometers being then placed on ſhore, the mercury inclined to a ſtationary poſition, without evincing any propenſity towards the equatropical motions from the 12th to the 18th February, 1804, as has been noticed in the foregoing deſcription, to happen frequently, on entering a harbour from ſea.

On February 18, 1804, the meteorological journal ceaſes, at which time it compriſes the obſervations of 22 months, having commenced April 6, 1802, in Margate Road.

I have taken the liberty of ſending you this abſtract from the journal, to exhibit the apparent difference of the mercury in the barometer at ſea, from what has been obſerved on ſhore, at thoſe places mentioned in the preceding deſcription. As I have not ſeen any account indicating the phenomenon, I thought it might be intereſting to you, or other gentlemen of the Royal Society to forward this imperfect abſtract, the journal itſelf being too cumberſome to ſend home at preſent. But as I am in expectation of returning to England by the ſhips from China next ſeaſon, I hope I ſhall be enabled to preſent you with the meteorological ſheets alluded to.

I am, &c.

J. HORSBURGH.

P. S. Since I wrote the foregoing abſtract, I have received a letter from my friend Mr. Dalrymple, intimating that a copy of the meteorological journal itſelf would be acceptable, which has induced me to tranſmit to him the original ſheets, with a requeſt to deliver them to you. I regret that I could not find leiſure time to make out a fair copy, to have ſent to you, in place of the original ſheets in their rough ſtate.

Bombay, June 1, 1804.

Second

V.

Second Communication on Artificial Tan. By CHARLES HATCHETT, *Esq. Abridged from the Philosophical Transactions for* 1805.

§ I.

THE artificial tan *, procured as described in the first communication (*see our Vol. XII. p.* 327), had been named tannin by Mr. Hatchett; but the objection having being made to this, that tannin was destroyed by the nitric acid, while the artificial tanning substance was actually formed by it, induced Mr. Hatchett to expunge the word *tannin* wherever it had been applied to the latter. It also induced the author to make the following experiments on the comparative effects produced by nitric acid on those substances which contain most tannin, and also some others in which a tanning substance has been produced, under circumstances in some respects different from those described. Name of the artificial tanning substance altered.

§ II.

Although it is not absolutely asserted that the tanning substance is indestructible by nitric acid, yet the following experiments prove, that to produce this effect must at least be the work of much time and difficulty.

1. Twenty grains of the artificial tan were dissolved in half an ounce of strong nitric acid, of the degree of 1.40; the solution distilled till the whole of the acid came over, which acid was returned back on the residuum, and the distillation repeated three times in this manner. Care was taken not to overheat the residuum; and then, when examined, did not appear to have suffered any alteration in its properties. Experiments to prove that the artificial tan is nearly indestructible by nitric acid.

* In several parts of the abridgement of Mr. Hatchett's papers, the artificial tanning substance has been called *the new tan* and *artificial tan*, and tanning matter *tan*, for the sake of brevity. It was thought necessary to mention this, as the name tan is usually appropriated to oak bark in a certain state; which, with singular impropriety, is that in which it contains least tanning matter, after having been used in the tanners' pits.—ABR.

2. Ten

Experiments continued.

2. Ten grains of the new tan, mixed with ten grains of white fugar, diffolved in half an ounce of nitric acid, was diftilled to drynefs. The refiduum was not changed by the gelatinous or other re-agents.

3. This experiment was the fame as the former, only that gum arabic was employed in place of fugar. The refult was the fame.

4. The precipitate from a folution of ifinglafs, with which the artificial tan had been mixed, was well wafhed with diftilled water and then dried. In this ftate it was digefted in ftrong nitric acid, by which a dark-brown folution was formed; which was evaporated to drynefs, and the fubftance, diffolved in boiling diftilled water, was examined by nitrate of iron, acetite of lead, muriate of tin, and folution of ifinglafs, with all of which it threw down copious precipitates, fimilar in all refpects to the artificial tan, which had not been fubjected to the procefs defcribed.

5. Some of the precipitate of ifinglafs by the new tan was diffolved in muriatic acid, and evaporated to drynefs: of this boiling diftilled water diffolved only a part; and the folution, of a dark beer colour, did not precipitate gelatine, though it acted on muriate of tin and fulphate of iron; for with the former it gave an afh-coloured precipitate, and with the latter a flight depofit of a reddifh-brown.

6. As boiling water diffolved only a part of the ifinglafs precipitate in the former experiment, the remainder was treated with nitric acid; after which, on being evaporated to drynefs, it was found to be completely foluble in water, and precipitated gelatine as copioufly as at firft.

7. Twenty grains of the new tan was diffolved in half an ounce of muriatic acid: The refiduum, after evaporation to drynefs, appeared in every refpect unchanged.

The author here makes the obfervation, mentioned at the conclufion of the former paper, relative to the folutions of the new tan not becoming mouldy like thofe of galls, fumach, and catechu, and feeming to be completely imputrefcible.

And having thus afcertained the unchangeable nature of this fubftance, he made the following comparative experiments on galls, fumach, Pegu cutch, kafcatti, common cutch, and oak bark.

8. Twenty

8. Twenty grains of powdered galls were diffolved in half an ounce of ftrong nitric acid: The refiduum from this folution evaporated to drynefs, and then diffolved in boiling water, did not produce the fmalleft effect on diffolved gelatine.

Experiments on galls, fumach, cutch, kafcatti, Pegu cutch, and oak bark.

The experiments on to No. 13. did not produce any tannin.

9. The refiduum of a ftrong infufion of galls, treated as No. 8.

10. Ifinglafs precipitated by infufion of galls, diffolved in ftrong nitric acid, and examined as No. 4.

11. Twenty grains of fumach diffolved in half an ounce of ftrong nitric acid, and treated as No. 8.

12. Twenty grains of Pegu cutch (which contains much mucilage) fubjected to a fimilar procefs, by which much oxalic acid was obtained.

13. Twenty grains of catechu, called kafcatti, treated fimilarly, had, together with the four foregoing experiments, all the fame refults as No. 8, not any of them fhewing any tannin.

14. Twenty grains of common catechu, diffolved in ftrong nitric acid, evaporated to drynefs, diffolved in water, and examined by ifinglafs, depofited a tenacious film infoluble in boiling water, evidently compofed of gelatine and tannin.

15. Twenty grains of oak bark treated in the fame way, depofited alfo an infoluble film on the fides and bottom of the veffel.

16. Infufions of galls, fumach, and oak wood, of equal ftrength, were mixed with nitric acid, in the proportion of half an ounce meafure of each to one drachm of the acid, and did not then render ifinglafs folution turbid.

But infufions prepared from oak bark and the artificial tan, and managed in the fame way, continued to precipitate the gelatine, until four drachms of the nitric acid had been added to each half ounce of the infufion.

Thefe refults fhew that artificial tan is the moft indeftructible, but that the other tanning fubftances have confiderable varieties in this refpect. The tannin of oak bark refifts nitric acid longer than that of galls, fumach, kafcutti, or Pegu cutch. This laft is replete with mucilage, and yields much oxalic acid, as before defcribed: it feems alfo to be the moft deftructible of all the kinds of catechu: From thefe facts the author was induced to add the fugar and gum to the artificial tan,

tan, to promote its deſtructibility; and expreſſes his belief that mucilage or gum renders the ſubſtances that contain it more deſtructible in the nitric acid, and in ſome caſes alſo prevents or impedes the formation of the tanning ſubſtance; which difference he thinks to be cauſed by the mucilage being in a ſtate of chemical combination in thoſe bodies.

§ III.

Experiments on the artificial tan.

A and B. When ſulphuric or muriatic acid was added to a ſolution of the new tan, it became turbid and depoſited a brown precipitate, which was ſoluble in boiling water, and was then capable of precipitating gelatine; in which particulars it reſembles the tannin of galls and other vegetable ſubſtances.

C. Carbonate of potaſh, added to a ſolution of the new tan, deepened the colour; the liquor became turbid, and depoſited a brown magma.

D. Five grains of dried artificial tan were diſſolved in half an ounce of ſtrong ammonia: the whole was then evaporated to dryneſs; and being diſſolved in water was found not to precipitate gelaten, unleſs a ſmall portion of muriatic acid was previouſly added.

E. Another portion diſſolved in ammonia was diſtilled: At firſt ammonia came over, and afterwards a yellow liquor, that had the odour of burned horn. The reſiduum was inſoluble in water, to which it only gave a ſlight yellow tinge.

On diſtillation it has an odour of burned horn, and yields ammonia.

F. The object of this experiment is to ſhew the ſtrange property of the new tan, of giving products analogous to animal matter (of which it yielded the odour in combuſtion on former trials), though prepared itſelf from vegetable ſubſtances. Some prepared from dry vegetable charcoal was diſtilled: Firſt a little water came over, then a little nitric acid, then a very ſmall portion of a yellowiſh liquor: The fire being then raiſed, the veſſels ſuddenly became filled with a white cloud, and ſo great a torrent of gas was almoſt exploſively produced as to overſet the jar: This gas, by its ſmell, appeared to be ammonia, and was formed into the cloud by the nitric acid vapour in the veſſels. The next jar of gas, which came ſlowly over, was carbonic acid, except a very ſmall part which ſeemed nitrogen gas. A bulky coal remained, that on incineration gave $1\frac{1}{2}$ grains aſhes, which conſiſted principally of lime.

G. Fifty

G. Fifty grains of this ſubſtance were diſſolved in four ounces of water and precipitated by iſinglaſs ſolution; eighty-one grains of which became thus combined with forty-ſix of the new tan. The remaining portion was not precipitated, and was therefore ſeparated on a filter and evaporated to dryneſs. It was a light brittle ſubſtance of a pale cinnamon colour, which, though compoſed of inodorous ſubſtances, had however a ſtrong ſmell itſelf of oak bark; which is remarked as a ſingular circumſtance; and this ſmell became ſtronger when the ſubſtance was put into water, in which it inſtantly diſſolved.

The ſolution was very bitter; acted but ſlightly on diſſolved iſinglaſs; produced a brown precipitate with ſulphate of iron, and with muriate of tin a black one; had no effect with nitrate of lime; but with acetite of lime gave a copious precipitate, of a pale brown colour. This ſubſtance appeared to be the tanning matter in the ſtate of extract.

§ IV.

Several unſucceſsful attempts were made to form the tanning matter by oxi-muriatic acid. It therefore appeared, that though a variety of it could be produced by the action of ſulphuric acid on reſinous ſubſtances, yet nitric acid was the moſt effective agent. Attempts to form tanning matter by oxi-muriatic acid unſucceſsful.

The author ſuſpecting that the new tan might be formed from bodies not abſolutely converted into coal, and not being able to get any touch-wood, which he firſt thought of trying for this purpoſe, made the following experiment with indigo, which he knew to contain much carbon. Artificial tan, it is ſuſpected, might be formed from ſubſtances not charred.

One hundred grains of indigo, with one ounce of nitric acid diluted with a double quantity of water, was (when the effervеſcence had ſubſided), placed in a ſand-bath for ſeveral days till evaporated to dryneſs. Experiments on indigo with this view.

The reſiduum, of an orange colour, was in great part diſſolved by three ounces of diſtilled water poured on it, and gave a ſolution of a deep yellow, and intenſely bitter; which, with the ſulphate of iron, depoſited a ſlight pale-yellow precipitate, and with nitrate of lime, a ſmall white precipitate, having the character of oxalate of lime: With muriate of tin a copious white precipitate, that changed to a yellowiſh-brown;

brown; and with acetite of lead a beautiful deep lemon-coloured precipitate, which may probably prove uſeful as a pigment.

Ammonia rendered the colour much deeper, and with it depoſited a large quantity of fine yellow ſpiculated cryſtals, which did not precipitate lime from its ſolutions. Their flavour was very bitter.

It produces tanning matter.

Laſtly, when this ſolution was added to diſſolved iſinglaſs, it became turbid, and depoſited a tough elaſtic inſoluble film, and poſſeſſed the characters of gelaten combined with tanning matter.

Almoſt all vegetable bodies yield tanning matter, when ſubjected to repeated diſtillations with nitric acid.

By this experiment the poſſibility of producing tanning matter from bodies not converted into coal was fully aſcertained; and the author has ſince diſcovered that though indigo yields this matter more readily than moſt other vegetable bodies, yet almoſt all produce it when ſubjected to repeated diſtillations with nitric acid.

Reſin yields it by this treatment.

A. The common reſin did not produce the tanning ſubſtance with nitric acid, but by the aid of ſulphuric acid, as before related; yet upon this nitric acid being repeatedly abſtracted from it, its ſolution in water formed a tough yellow inſoluble precipitate with diſſolved gelaten, ſimilar to that by ſolution of indigo, and with other re-agents produced the following effects.

With ſulphate of iron, after 12 hours, it produced a ſlight yellow precipitate. With nitrate of lime no effect. With muriate of tin, after 12 hours, a pale brown precipitate. And with acetite of lead a very abundant precipitate of a yellowiſh white colour.

On repeating this experiment, the author remarked that during each diſtillation nitrous gas was produced, while the acid which came over was weakened, which made the cauſe of the change in the properties of the reſin evident. The following are the reſults of experiments tried with other reſinous ſubſtances.

As do likewiſe ſtick lac,

B. Stick lac, treated as deſcribed, copiouſly precipitated gelaten.

—and balſam of Peru,

C. Balſam of Peru during the proceſs afforded ſome benzoic acid, and gelaten was precipitated by the aqueous ſolution.

Benzoin

Benzoin, after the fublimation of fome benzoic acid, yielded a refiduum, which yielded with water a pale yellow folution, of a very bitter flavour. —and benzoin,

This folution with fulphate of iron produced a flight pale yellow precipitate. With nitrate of lime, no effect. With muriate of tin in folution, a fmall quantity of brownifh white precipitate. With acetite of lead, a copious pale yellow precipitate. And with folution of ifinglafs, a denfe pale yellow infoluble precipitate.

E. Balfam of Tolu afforded benzoic acid, and the folution of the refiduum precipitated that of gelaten.

F. One hundred grains of dragon's blood in powder mixed with one ounce of nitric acid, evolved much gas; an ounce of water was then added; and the digeftion in a fand bath being continued, after it produced chaficcation on the dry yellow mafs that remained, a brilliant feather-like fublimate arofe, which weighed rather more than fix grains and had the afpect, odour, and properties of benzoic acid. —and dragon's blood,

The refiduum, of a brown colour, formed with water a gold coloured folution, which was not affected by nitrate of lime: But with fulphate of iron, and with muriate of tin it formed brownifh yellow precipitates; and with acetite of lead one of a lemon colour.

Gold was precipitated by it in the metallic ftate, and the containing glafs coloured purple; and with diffolved ifinglafs it produced a deep yellow infoluble depofit.

As dragon's blood fimply expofed to heat did not produce any benzoic acid, the author is inclined to believe, that in the firft experiment this acid was obtained as a product, and not as an educt.

G. Gum ammonia gave a brownifh yellow, bitter aftringent folution; which with fulphate of iron became of a darker colour, but produced no precipitate. —and gum ammonia,

With nitrate of lime, a flight precipitate. With muriate of tin and acetite of lead, copious yellow precipitates; and with gelaten a bright yellow infoluble depofit.

H. Affa fœtida yielded a folution which precipitated gelaten in a fimilar manner to that defcribed. —and affa fœtida.

I. Solutions of elemi, tacamahac, olibarum, fandarach, copaiba, maftich, myrrh, gambauge, and cacutchonc, although Solutions of elemi, tacamahac, &c. did not affect gelaten, though they precipitated the metallic folutions, did not affect gelaten; but poffibly might have done fo, if the procefs had been more frequently repeated.

—nor that of farcocol,

K. Sarcocol alfo produced fimilar refults.

—nor of gum Arabic,

L. Gum Arabic afforded oxalic acid but no tan.

—nor of tragacanth,

M. Tragacanth yielded much of faclatic acid, of oxalic, and of malic acid, but not the leaft tan.

—nor of manna.

N. Manna gave oxalic acid, part of which fublimed in the neck of the veffel.

Its refiduum formed a brown folution, which produced precipitates of the following colours: With fulphate of iron, a pale yellow; with muriate of tin, a pale brown; with acetite of lead, a brownifh white. From nitrate of lime, oxalate of lime was copioufly precipitated by it; but with ifinglafs folution no effect was produced.

Liquorice folution precipitates gelaten.

O. Nitric folution of liquorice yielded precipitates with fulphate of iron and muriate of tin, after twelve hours, flight brown. With acetite of lead, a brownifh red. With nitrate of lead, a brown. And with gelaten, one of a yellowifh brown, infoluble, and fimilar to other precipitates from it by tan.

Guiacum folution gave a flight precipitate with gelaten, which was foluble in water.

P. On guiacum nitric acid acted with great vehemence and fpeedily diffolved it: The refiduum was almoft totally foluble in water; and this folution produced effects on the metallic falts fimilar to thofe recited; but with gelaten formed a flight precipitate, which was fpeedily diffolved by boiling water. The remainder of the folution evaporated gave a large quantity of cryftalized oxalic acid; fo that in this refpect guiacum was fimilar to the gums, and unlike the refins.

§ V.

Experiments on feveral roafted vegetable fubftances, which do not affect gelaten.

As many vegetable fubftances when roafted yield a liquor by decoction, refembling folution of artificial tan, the author tried thofe fimilarly prepared, of dried peas, horfe beans, barley, and wheat flour, none of which gave any precipitate with gelaten.

Coffee gives a precipitate with it, which is again foluble in water.

The decoction of coffee alfo gave no precipitate till after feveral hours, and then one foluble in boiling water; but this might be occafioned, the author thinks, from want of fome particular nicety which may be required in roafting fuch bodies

5 fo

fo as to make them yield tan; which opinion was corroborated by experiments made by decoction of chicoree (probably endive) root, prepared in the fame manner, which produced a precipitate with gelaten after fome time, though not at firft, which was apparently diffolved in boiling water, but depofited again in its original ftate, on cooling. The author therefore is inclined to believe that the tanning fubftance is really developed in many vegetable matters by heat alone; but that a certain degree of heat, not eafy to determine is abfolutely neceffary for this effect.

A fmall quantity of nitric acid added to any of the decoctions juft mentioned, and evaporated to drynefs, produced a refiduum, having all the properties of the tan produced from coal.

Nitric acid added to thefe decoctions gives tanning properties to their refidue.

§ VI.

The production of a variety of the tanning fubftance before mentioned, by the action of fulphuric acid on the refins, amber, &c. fuggefted the following experiments on camphor; the refults of which tend to increafe the knowledge of its properties.

Experiments on Camphor with Sulphuric Acid.

The only facts hitherto related relative to the effects of fulphuric acid on camphor, are that a brown or reddifh brown folution is formed, from which water precipitates the camphor unchanged; but this only happens at a certain period of the operation; for if it be longer continued, the following effects will be produced.

A. One ounce of concentrated fulphuric acid was added to one hundred grains of camphor, which diffolved gradually, after firft becoming yellow; in about an hour, the liquor having progreffively changed to reddifh brown, brown, and at laft blackifh brown, much fulphureous acid gas was produced, and continued to encreafe during four hours, when the whole appeared a thick black liquid, having no other odour but that of fulphureous acid; after two days the production of the gas was much diminifhed; the containing alembic was then put in a fand bath, moderately hot, by which more fulphureous gas was obtained; but this foon abated; at the end of two days more, fix ounces of water was gradually added, by which the liquor

Experiments with fulphuric acid on camphor.

liquor changed to a reddifh-brown, a coagulum of the fame colour fubfided, the odour of fulphureous acid gas was immediately annulled, and was fucceeded by one which much refembled a mixture of oils of lavender and peppermint.

An odour yielded by it like oils of lavender and peppermint.

The whole was then diftilled gradually, when the water came over impregnated with the odour laft mentioned, accompanied by a yellowifh oil, which floated on the top, and was computed to amount to three grains.

B. When the whole of the water had come over, there was again a flight production of the fulphureous acid gas; two ounces of water were then added, and the diftillation continued (without the recurrence of the former odour) till a dry blackifh brown mafs remained; this mafs was well wafhed with warm diftilled water, by which nothing was extracted; but two ounces of alcohol digefted on it for 24 hours formed a very dark brown tincture.

The refiduum was digefted with two ounces more alcohol, and the procefs repeated till the alcohol ceafed to act.

The refiduum had now the appearance of a compact fort of coal in fmall fragments, which were well dried, and after being expofed to a low heat in a clofe veffel, weighed fifty-three grains.

C. From different portions of the alcohol folution, added together and diftilled in a water bath, a blackifh brown fubftance was obtained, which had the appearance of a refin or gum with a flight odour of caromel, and weighed 49 grains.

The products obtained from the 100 grains of camphor treated with fulphuric acid, were,—

Products from camphor and fulphuric acid.

	Grains.
A. An effential oil, having fomewhat of an odour of a mixture of lavender and peppermint, about	3
B. A compact and very hard fort of coal, in fmall fragments, - - - - -	53
C. A blackifh-brown fubftance, of a refinous appearance, - - - - - -	49
	105

The increafe of weight of five grains is attributed partly to water retained by the laft fubftance, and partly to oxigen united to the carbon.

The fubftance C had the following properties:

1. It

1. It was bitter and aſtringent, had the odour of caromel, and formed with water a dark-brown ſolution.

2. This ſolution produced very dark-brown precipitates with ſulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was precipitated by it in the metallic ſtate from its ſolution.

4. By ſolution of iſinglaſs the whole was precipitated; ſo that after four hours a colourleſs water only remained.

Subſtance C (from camphor) precipitates gelaten.

This precipitate was nearly black, and was inſoluble in boiling water: from whence, and its effects on ſkin, it was evidently a variety of tanning matter much reſembling that obtained from reſinous bodies by ſulphuric acid.

But this ſort of tan had leſs effect on ſkin than that procured from carbonaceous ſubſtances by nitric acid, and its precipitate from gelaten was more flocculent and leſs tenacious.

However, when a ſmall quantity of nitric acid was added to the ſolution of the ſubſtance obtained from camphor, and when the reſiduum, after evaporation to dryneſs, was diſſolved in water, a reddiſh-brown liquor was formed, which acted in every reſpect ſimilar to the tanning ſubſtance obtained from the varieties of coal by the nitric acid.

§ VII.

The three varieties of artificial tan.

From the experiments related, it appears that three varieties of the tanning ſubſtance may be formed.

1ſt. That produced by nitric acid with any carbonaceous ſubſtance, whether vegetable, animal, or mineral.

2d. That formed by diſtilling nitric acid from common reſin, indigo, dragon's blood, and various other ſubſtances.

3d. That which common reſin, elemi, aſſafœtida, camphor, &c. yield to alcohol, after they have been previouſly digeſted with ſulphuric acid.

Remarks on them.

On theſe products the author makes the following remarks:

The firſt variety is the moſt eaſily formed. From ſome experiments made purpoſely it appears, that, after making allowance for a ſmall quantity of moiſture and of nitric acid remaining, 100 grains of vegetable charcoal yield 116 of the dry tanning ſubſtance.

100 grains charcoal yield 116 tanning matter.

 From

Carbon the base of the tanning matter.

From the manner in which it is produced, carbon is evidently the base and predominating essential ingredient in this substance.

It also contains oxigen, hidrogen, and nitrogen.

From § III. experiment F, it also appears that the other component parts are oxigen, hidrogen, and nitrogen; for when the artificial tan was distilled, ammonia and carbonic acid were obtained, exclusive of a small portion of a yellow liquor that appeared to be of an oily nature, from being insoluble in water and alcohol.

It has an odour like animal matter when burned, and one of oak bark when precipitated as in G, § III.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, particularly those noticed in § III. experiment F; of its having the odour of animal substances when burned, though prepared from vegetable matter; and in experiment G, of the precipitate having the odour of oak bark, though the component materials were inodorous.

It resembles vegetable tannin in most properties.

But its most extraordinary properties are those in which it so nearly approaches the vegetable *tannin*, which it perfectly resembles in its solubility in water and in alcohol, in its action on gelaten and on skin, in its effects on the metallic solutions, on the alkalis, and on the earths.

Difference between it and tannin.

The sulphuric and muriatic acids also affect its solutions, as they do those of tannin; and the only marked difference between artificial tan and tannin is, that the former is produced by nitric acid, while the varieties of the latter are more or less destroyed by it; but here it must be remembered, that even the varieties of tannin do not accord in the degree of destructibility.

Second variety of artificial tan.

The second species of the tanning substance is obtained from a variety of vegetable bodies before recited, by digesting and distilling them with nitric acid. It is therefore not so readily prepared, and the quantity of it produced is less in proportion to the substance from which it is prepared.

Theory of its formation.

As resin and some other bodies do not afford it until they have been repeatedly treated with nitric acid, and as, during each operation, nitrous gas is produced, while the strength of the acid which comes over is diminished, the author thinks it almost certain that the tanning substance is formed in consequence of part of the oxigen of the nitric acid becoming combined with the hidrogen of the original body, so as to form water;

water; and the carbon being thus in ſome meaſure denuded, is rendered capable of being acted on gradually by the nitric acid, in a manner nearly ſimilar to what takes place when it has been previouſly converted into coal.

The precipitates of this tanning ſubſtance from gelaten are always pale or deep yellow, while thoſe formed by the firſt ſpecies are conſtantly brown; which induces the author to believe that the different colours of the precipitates depend on the ſtate of the carbon of the tannin.

Quantity of artificial tan leſs from reſins than from coal, accounted for.

The quantity of artificial tan obtained from reſin and other bodies, was always leſs than that from coal, or even from the ſame bodies previouſly converted to coal in the humid way by ſulphuric acid. The cauſe of this ſeems to be, that a number of other products are ſimultaneouſly formed with the tanning ſubſtance, all of which require more or leſs carbon as an ingredient; ſo that, according to the affinities which prevail, ſome bodies afford but little, and others none of it.

Its proportions from different ſubſtances.

The greateſt proportion of this ſubſtance was yielded by indigo, common reſin, and ſtick lac.

The quantity obtained from aſſafœtida and gum ammoniac was leſs.

Benzoic acid when formed counteracts the formation of the tan; and oxalic acid, when produced, prevents it entirely.

Benzoin, balſam of Tolu, balſam of Peru, and dragon's blood, were inferior to the former in this reſpect; ſo that the production of benzoic acid ſeemed to counteract the formation of the tanning ſubſtance. But oxalic acid, when formed in any conſiderable quantity, ſeemed abſolutely to prevent the formation of this ſubſtance: for gum arabic, tragacanth, manna, and guiacum, which produced oxalic acid in abundance, yielded no tanning matter.

Common liquorice ſeems to be an exception; but the author ſuppoſes that the ſmall quantity of tan produced by it, was formed by the action of the nitric acid on a portion of uncombined carbon, which being in a ſtate approaching to coal, is probably the cauſe of the blackneſs of the common liquorice.

Third variety of artificial tan.

The third variety of the tanning ſubſtance appears to be uniformly produced during a certain period of the proceſs; but by a long continuance of the digeſtion there is reaſon to think it is deſtroyed.

Subſtances, ſuch as gums, which yield much oxalic acid, do not apparently afford any of this tanning matter.

The

The energy of its action on gelaten and ſkin is certainly inferior to that of the firſt variety, into which however it may eaſily be converted by nitric acid.

From the mode of its formation, there does not appear to be any evidence of its containing nitrogen like the firſt and ſecond varieties, and perhaps the abſence of nitrogen may be the cauſe of its leſs powerful action.

Experiments of Meſſrs. Biggin, Prouſt, and Davy on tannin, noticed.

In the courſe of the communications on this ſubject, Mr. Hatchett notices the experiments on tannin by Mr. Biggin, the great contributions of M. Prouſt to the elucidation of its nature and properties, and the very great extenſion of, and valuable additions to the ſame, from the ingenious labours of Mr. Davy, particularly his diſcovery of the ſingular fact that terra japonica, or catechu, conſiſts principally of tannin.

Medicine, arts, &c. may derive great benefit from farther inveſtigations of gums, reſins, &c.

The author alſo greatly recommends the farther inveſtigation of the nature of the gums, reſins, balſams, and gum-reſins, by every poſſible method; and is of opinion, that medicine, arts, and manufactures may derive many advantages from it, and the myſterious proceſſes of vegetation probably receive conſiderable elucidation.

VI.

On carboniſed Turf. From a Report made to the Prefect of Police (at Paris) on the Methods employed for reducing it to this State. By MM. CALLIAS *and Co**.

The uſe of turf very ancient. It produces no deleterious effects.

THE uſe of turf for domeſtic fuel is of a very ancient date: Some of the moſt eminent men of ſcience have pronounced that it does not produce any deleterious effects. Without citing the examples of England (Ireland), Scotland, and Holland, where great quantities of it are conſumed, we will confine ourſelves to the uſe made of it in France, in the *(ci-devant)* provinces of Flanders, Artois, and Picardy.

Its uſe is now tolerated in Paris to relieve the ſcarcity of wood: the lime-burners, plaſter-bakers, brick-makers, and waſhers, make great uſe of it both in the city and its vicinity; and it has never been perceived that thoſe who lived within

* Sonini's Journal, Tom. II. p. 324.

the

the influence of its ſmoke, have experienced any bad effects from it. The commiſſioners (employed to make this report) obſerve, that the great volume of ſmoke which is diſengaged on the commencement of its combuſtion, is only cauſed by a great portion of water contained by the turf, which is expanded into ſteam by the heat; ſoon afterwards this ſmoke is combined with an acid analogous to that of vegetable ſubſtances, which, far from making the air deleterious, tends on the contrary to neutralize the vapours of infection which it may contain. It is true that ſometimes, for an inſtant, the turf in combuſtion exhaled an odour of empyreumatic oil, in the form of gaſeous vapours, but this odour is by no means injurious to the animal organiſation, but, on the contrary, is beneficial in nervous affections.

Its ſmoke is cauſed by contained water; with an acid vapour, which removes infection.

But if this odour is diſagreeable when the turf is burned in towns, villages, and private houſes, this complaint cannot take place when it is burned in the open air at a diſtance from all habitations, which will be effected by its previous carboniſation, as managed by MM. Callias and Co. therefore the company merit the public protection.

Charing prevents all the unpleaſant effects of its odour.

In 1785 the French Government took a great intereſt in what related to the carboniſation of turf, and granted 80,000 francs to a company to erect a furnace for this purpoſe on the ground of the Capuchins. The method of this company was that of extinguiſhing, but their plan did not ſucceed, and the works were abandoned.

Company at Paris for charing turf, aſſiſted by the government, did not ſucceed.

A new company tried, ſome time after, the ſame enterprize, at its own expence: the method of operating in cloſed veſſels was propoſed: the experiments made were on a great ſcale, and were attended with a ſucceſs that was certified by the commiſſioners of government: a memoir printed in 1790, by M. Morclot, contained theſe facts, with a ſtatement of the ſuperiority of turf-charcoal over that of wood. But the diſaſtrous events of the revolution put an unhappy end to this enterprize which promiſed ſo well.

Another company—promiſed well—but the revolution cauſed its failure.

At preſent MM. Callias and Co. offer to the public an additional ſpecies of fuel to that hitherto in uſe, a charcoal of a new proceſs, the materials of which are ſpread with profuſion over the territory of France, and the conſumption of which, being ſubſtituted for that of wood, will at the ſame time be an object of economy to individuals, and of incalculable advantage

Callias and Co. charr turf by a new proceſs.

The uſe of turf charcoal. It will be cheap, and prevent the destruction of the forests.

vantage to the management of the forests. Timber for the construction of houses and furniture, and timber for ship-building, daily increase in price, because they become more scarce. Some of the forests have become reduced, as the fresh growths in them do not keep pace with the destructive instrument that overturns them; some of them are entirely destroyed, and the ground converted into ornamental gardens; and thus each year, each month, each day, conducts us insensibly to a most alarming dearth of timber. Already the price of fire-wood is tripled, and Paris is on the eve of being deprived of a combustible which, as yet, has not been replaced to advantage.

The carbonization used by Callias is very perfect.

The commissioners compliment MM. Callias and Co. whose method of carbonisation is peculiar to themselves, and calculated conformably to the laws of combustion in its two first stages; that is to say, before the arrival of its third degree, or that of absolute combustion. MM. Callias and Co. by their method, direct the carbonisation at their pleasure and in an invariable manner; they are always sure of obtaining a perfect charcoal, without smoking-pieces, and without any risk of forming it into a pyrophorus, which sometimes happens in the carbonisation performed in closed vessels. Their manner of proceeding is also very economical; and what proves that they work with intelligence is, that they daily improve, and already are able to save ten hours out of 48 in each carbonisation.

Experiments made with Charcoal of Turf.

Turf-charcoal yields more heat than wood-charcoal.

1*st*. The charcoal of turf kindles a little slower than that of wood, but when it is once in compleat ignition, it throws out much more heat; its flame is also more elevated, and it yields no odour, except a very slight one of sulphur, which ceases when it is fully lighted.

Causes water to boil four times as speedily.

2. Charcoal of turf, in equal quantity with charcoal of wood, caused a given quantity of water to boil four times; while that of wood caused it only to boil once. The first is then superior to the second in a quadruple proportion.

3. To prove that turf charcoal emits more heat than wood charcoal, the following experiment was made.

It fused 11 oz. of gold in eight minutes; wood-charcoal did the same in sixteen

With turf charcoal, in a goldsmith's furnace, eleven ounces of gold were fused in eight minutes, which with wood charcoal was not performed in less than sixteen minutes. The gold lost nothing of its malleability in the fusion with the turf; but, on on the contrary, it was neceſſary to add ſome reductive flux, to that fuſed by the wood charcoal, in order to reſtore the malleability which it had loſt. minutes. The malleability of the gold was preſerved.

4. Iron made red-hot by charcoal of turf in a forge, became more malleable; which proved that it gave none of its carbon to the metals with which it came in contact. Iron heated by it becomes more malleable. It laſts longer than wood-charcoal.

5. Finally, Turf charcoal laſts longer in a ſtate of ignition than charcoal of wood, and its heat is conſtantly equal.

Concluſion.

1. The odour of turf in combuſtion is noways deleterious. This truth has been confirmed by the moſt diſtinguiſhed chemiſts; and is beſides proved by the conſtant uſe made of this fuel in the *ci-devant* provinces of Flanders, Artois, and Picardy. Its odour not at all unwholeſome.

2. It is deſirable that the carboniſation of turf may be encouraged, on account of the great advantages which may reſult from the uſe of this new ſpecies of charcoal, both for private conſumption and for large works. Its uſe ought to be encouraged,

But the greateſt matter in its favour is, that its uſe tends to diminiſh the felling of the foreſts, whoſe extenſion ought to be promoted by every means poſſible, and which nothing tends ſo much to deſtroy as the uſe of wood charcoal. and will preſerve the woods.

VII.

Account of the Cataracts and Canal of Troellhætta, in Sweden, (from a Work relative to them by Colonel SKIOELDEBRAND. *Publiſhed in one Volume Quarto, at Stockholm.)*

THE cataracts of Troellhætta produce one of the fineſt effects which nature affords in Europe. The river of Gothie is the only outlet of the vaſt lake of Wener, navigable through its whole extent. This river, which falls in the North ſea near Gothemberg, as ſoon as it departs from the lake, which is much more elevated than the ſea, rolls its waters with impetuoſity, and daſhes them againſt ſteep rocks, whoſe reſiſtance forms a ſucceſſion of cataracts, which without being individually very high, form altogether a moſt ſtriking object. The imagination is the more affected by this ſight, as the ſurrounding ſcenes are of a dark and melancholy character, conſiſting of grey rocks crowned The cataracts are formed by the river Gothie ſoon after its departure from lake Wener. They form a very ſtriking object.

crowned with ancient firs, and of frightful precipices, formed by the burſting of the locks and banks, which the fury of the water has overturned. Theſe laſt were conſtructed in the bed of the cataracts, in order to render the river navigable through its whole length; but this daring work of man could not reſiſt the reiterated efforts of nature, and therefore it was neceſſary to have recourſe to another plan.

Ruins of locks which had been formerly conſtructed in the bed of the cataracts.

The canal newly conſtructed paſſes by the ſide of the cataracts, and its bed is partly formed in the natural rock, and partly in a marſhy ſoil. It was began in 1794, and finiſhed at the end of ſix years, in 1800. Its breadth is 22 feet, and its depth ſix feet and an half. Its locks are eight in number, and its coſt amounted to the ſum of 59833 pounds ſterling, which was collected by ſubſcription. By means of this canal there is a continued navigation, without any interruption, from the province of Wermeland to Gothemberg. In 1802 the number of veſſels which had paſſed this canal amounted to 1380, which is at the rate of 1190 each year.

The canal paſſes by the ſide of the cataracts. Is partly cut in the rock. Its breadth, depth, number of locks, time of excavation, and coſt. The extent of its navigation, and number of veſſels which have paſſed.

VIII.

Letter from H. B. K. *on the Production of Nitrous Acid, and other Facts**.

To Mr. NICHOLSON,

SIR,

AS Mr. Accum has not anſwered my paper, he therefore knows of no experiments which ſhew the formation of the nitrous acid; but anxiouſly impreſſed with the ſubject, I have been performing ſome experiments, which I think will throw great light upon the cauſe of the nitrous acid appearing in electrical experiments.

Experiments announced. Carbonate of potaſh in water was galvanized, and emitted carbonic gas.

I paſſed the galvanic fluid through a watery ſolution of the carbonate of potaſh, made by diſtilled water, confined in a glaſs tube where no atmoſpherical air could have acceſs to it, and I found a great production of air come from the ſolution, which upon examination was pure carbonated air; and

The potaſh became capable of deflagration like nitre; and ſolution of ſilver ſhewed the preſence of muriatic acid.

* See our Journal, X. 105, 214, and XI. 105.

then

then examining the ſolution by dipping a piece of paper into it; upon its being dipped the paper ſhewed evident ſigns of nitre upon it, and when burned it detonated the ſame as nitre would have done; and alſo with the ſolution of ſilver the alkaline ſolution gave ſome faint indications of the marine acid being preſent in it. That the ſolution ſhould give indications of poſſeſſing both the nitrous and marine acids is not ſo ſurprizing; as we have the ſame products in firing oxigen and hidrogen gaſes, according to the foreign experiments, principally the nitrous acid, but with it a ſmall quantity of the marine acid.

Remark. Theſe two acids are found on the common detonation of oxigen and hidrogen.

I then filled the tube (after waſhing it clean) with pure diſtilled water, and ſent through it the galvanic fluid; and I obſerved a generation of airs, which, upon examination, appeared to be the airs uſually formed in theſe experiments, as they exploded.

Pure water galvanized gave theſe gaſes.

After this I filled the tube with a ſolution of pure potaſh in diſtilled water, and no air came from it upon galvaniſing it; if any, it was carbonated air: But upon examining the ſolution, it gave clearly the ſame indications of poſſeſſing the nitrous and marine acids, as the carbonated ſolution of potaſh did in the firſt experiment.

Pure potaſh and water gave no gas by galvanizing, but indicated the two acids of the firſt experiment.

Now exempt from all hypotheſes, let us examine theſe intereſting facts: The carbonated potaſh had its carbonic acid air expelled, clearly from the acid or acids; as we know that it could not part with its carbonic acid air, but from the action of a ſtronger acid. Alſo another more eſſential fact it proves to us, that the generation of the peculiar airs, what are called oxigen and hidrogen gaſes, are owing to the acids; for when the potaſh was in the water as to arreſt or attract them, there were neither of theſe airs produced; and upon examining the diſtilled water (in the experiment in which they were produced) after their production, there was no acid in the water, but it was pure diſtilled water. Therefore, beyond a doubt, the nitrous acid is eſſentially concerned in the production of theſe peculiar oxigen and hidrogen gaſes: indeed Mr. Cruickſhanks ſays, that upon fuſing theſe airs, he found in the reſiduum the nitrous acid.

Remarks. The carbonic acid was expelled from potaſh by a ſtronger acid produced. Whence it is inferred that the oxigen and hidrogen aroſe from theſe acids.

Theſe experiments were performed by two ſhort gold wires attached to each end of the galvanic pile. But upon placing a pretty long iron wire to the ſilver end of the pile inſtead of the

The conducting wires were gold.

the gold one, a little hydrogen gas was produced; even when the potash was mixed with the distilled water, though there was none when it was a gold wire.

I hope Mr. Nicholson you will not refuse the insertion of these interesting facts in your Journal, for I have made the relation of them as brief as possible that they might not occupy too much room *.

H. B. K.

London, August 15.

IX.

Report of M. Debuc's *Memoir on Acetic Acid, made by M. M.* Planche *and* Boullay, *by Order of the Society of Pharmacy at Paris* †.

M. Debuc repeated M. Badollier's process to obtain acetic acid from acetate of lead by sulphate of copper.

M. Debuc saw in the *Annales de Chimie*, No. 109, a method of M. Badollier, apothecary at Chartres, for obtaining acetic acid very readily from a mixture of equal parts of sulphate of copper and acetate of lead by a moderate heat.

Relying on this process M. Debuc made an exact mixture of two pounds of sulphate of copper and an equal quantity of acetate of lead, which he exposed in a distilling apparatus on a sand bath to a moderate fire, which he increased by degrees during the operation, which lasted for six hours: the product obtained was 26 ounces. It was given to a manufacturer, without examination, and being used in his business produced an effect entirely contrary to the acid extracted from crystals of copper.

The product used in manufacture produces an effect contrary to the common acetic acid.

The process again repeated, and its results carefully examined.

This circumstance determined M. Debuc to repeat the process as before, and examine its results carefully: In which he observed the mixture of the two salts to become pastey, which is easily explained by the difference of the concentration of the acid in the sulphate of copper, from that in the sulphate of lead. The products of this experiment were,

* I am extremely sorry that this communication was by mistake placed among papers already printed; which alone has caused the delay in its appearance.—W. N.

† Annales de Chimie, Tom. LIV. p. 145.

1. Four

1. Four ounces of water flightly acidulated.

Products of this process.

2. Four ounces of a liquor more acid than the firft, and which M. Debuc compares to good vinegar of *Saumur*.

3. Eighteen ounces of a very limpid liquor, with a lively and penetrating odour of acetic acid mixed with fulphurous acid.

The refidue is in layers of different colours.

The refidue, weighing 38 ounces, appeared to M. Debuc in different layers more or lefs red, according to their diftance from the bottom of the retort; and he found the upper part covered with a whiteifh powder, flightly inclined to a citron colour, in which he recognifed the prefence of fulphur.

Precipitates formed with the third product by different falts.

Barytes, the muriate of lime, and the acetate of lead formed immediately confiderable precipitates with the third product.

Sulphurous acid being produced, induces M. Debuc to fuppofe that acetic acid is fuper-oxigenated vinegar.

M. Debuc obferves that the decompofition of the acetate of lead by the fulphate of copper may be eafily explained; but that here there is a production of fulphurous acid, and a decompofition of the fulphuric acid from the abforption of its oxigen by the vinegar; which is a fingular phenomenon, that has no agreement with the affinities of the acidifying principle for the *acidifiable* and *falifiable* bafes; he leaves the explanation of this matter to more experienced chemifts, and only notices that the tranfportation of the oxigen of the fulphuric acid to another bafe, fuggefts the idea, *that acetic acid is fuperoxigenated vinegar*.

M. Debuc's method of freeing the third product from fulphurous and fulphuric acids.

M. Debuc fucceeded in freeing his third product from the fulphurous and fulphuric acids, by letting it remain for about 24 hours, on twelve grains of falt of tartar, and about two ounces of black oxide of manganefe pounded fine, and after that diftilling it flowly; by this rectification he obtained a pound of pure acetic acid of a lively and agreeable odour, and of about 10 degrees fpecific gravity; which is one degree lefs than that of radical vinegar well rectified, obtained from acetate of copper.

Acetic acid produced, one degree weaker than the common kind.

The author concludes from this,

1. That the product of two pounds of acetate of lead, treated with an equal quantity of fulphate of copper, is twenty-fix ounces; of which four ounces is acidulated water, an equal portion ftrong vinegar, and eighteen ounces acetic acid altered by the fulphurous and fulphuric acids.

M. Debuc concludes that the acetic acid produced only dif-

2. That the eighteen ounces, forming the third product, rectified as recited, does not differ from that drawn from cryftals of acetate of copper, but by its lefs denfity.

3. That

fers in strength from the common kind, and may be substituted for oximuriatic acid, in some cases to advantage.

3. That in many cases this acid may be substituted for oxi-muriatic acid, as an object of salubriety without possessing its inconveniencies.

The reporters repeat M. Badollier's process, with M. Debuc's modifications.

The reporters repeated the process of M. Badollier with the modifications advised by M. Debuc as follows.

They introduced a mixture of two pounds of sulphate of copper, and the same quantity of acetate of lead into a glass retort, placed it on a sand bath, and adjusted to it a tubulated receiver, which communicated with two bottles of a Wolf's apparatus; the first of which contained distilled water, and the second many pounds of lime water; from this last a tube was passed underneath a jar in an hydro-pneumatic apparatus; the retort was heated gradually to the end of the operation, which lasted more than 10 hours; and the following products were drawn from the receiver.

Products of their process from the receiver.

1. Eight ounces of a liquor similar to distilled vinegar, but with a less agreeable odour.

2. Ten ounces of a liquor with an unpleasing odour of acetic acid, more penetrating than the first, and not containing any trace of sulphurous or sulphuric acids.

3. Finally seven ounces of a liquor of great limpidity, with a very pungent odour of sulphurous acetic acid, and which did not precipitate muriate of barytes.

A considerable disengagement of an elastic fluid was observed, which became perceptible as soon as the retort began to run, and which lasted during the whole operation.

Carbonic acid gas evolved.

This gaseous fluid was absorbed almost totally by the lime water, forming with it a very abundant white precipitate; which, gathered on a filter, and dried, proved to be carbonate of lime: It weighed two hundred and fifty grains, which made the carbonic acid equal, according to the known proportions of this substance, to eighty-five grains; atmospheric acid alone passed under the jar mixed with some carbonic acid gas: no trace was perceived of hydrogen gas.

Many layers of different colours were found in the retort.

The first was of a beautiful green, surrouuded with a circle of yellowish white towards the sides.

The second, much more thick, was of a red colour, greatly like copper in very small particles.

The third was a mixture of sulphate of lead and of copper apparently in the metallic state.

The

The last larger, which occupied the bottom, of a black colour, and shining, was a mixture of sulphate of lead and of charcoal.

The lowest layer of the residue is a mixture of sulphate of lead and charcoal.

The same experiment with the same quantities of the salts, was repeated a second time, with the precaution of reducing the sulphate of copper by disiccation to $\frac{11}{16}$ of its weight. The product from this was preferable to the other.

The same process again repeated; drying the sulphate of copper, the produce is better.

The second and third products were mixed and rectified on carbonate of potash and oxide of manganese, with the precautions indicated by M. Debuc: This rectification produced an acetic acid of nearly the same specific gravity as that afforded by simple distillation from crystals of copper, but of a less strong odour, less agreeable, and besides mingled with sulphurous acid.

Second and third products rectified. A weaker and less agreeable acetic acid produced, mingled with sulphurous acid.

The reporters think that M. Debuc is deceived in his theory, "*that acetic acid is vinegar super-oxigenated by the oxigen of the sulphuric acid passing to the vegetable acid,*" for he has not considered,

Reasons why M. Debuc's theory is erroneous.

1. That the acetic acid is almost all obtained, before the sulphurous acid becomes perceptible.

2. That the metallic oxides, which are the basis of the salts employed, have less attraction than sulphur to oxigen.

3. That the disengagement of the carbonic acid is much more likely to explain the matter.

The considerable production of carbonic acid, and the presence of charcoal in the residue, surprised the reporters the more; as *MM. Boddolier and Darac* (the first in his notice of the preparation of acetic acid; the other in a memoir, in other respects very interesting, on the difference of acetous and acetic acids,) positively assert that in the operation related, "*there was no other gaseous production but that of part of the air contained in the vessels, especially no carbonic acid, and not an atom of charcoal in the residue.*

The production of carbonic acid and charcoal from the process, is contrary to the assertions of MM. Bodolier and Darac.

The result found by the reporters so different from that of *M. Darac,* in an experiment on which he supports his theory of the identity of the acetous and acetic acids, was so favourable to the theory of *M. Chaptal,* that they would have been induced to decide in favour of the opinion of the latter, if the following comparative experiments had not confirmed them in a contrary notion, and appeared to them one of those, of which M. Darac might most avail himself.

To

An experiment made, favourable to M. Darac's opinion.

To four ounces of pure concentrated radical vinegar (extracted from cryſtals of copper by heat alone) were added by degrees four ounces of ſemi-vitreous oxide of lead (litharge) in powder; which compleatly diſſolved in it by heat, there even remained an exceſs of acid, perceptible in the ſtrong odour of the ſolution. Being laid by to cool, it produced a very irregular cryſtalline maſs.

Four ounces of this maſs of acetic lead, mixed with an equal quantity of ſulphate of copper dried, were treated in a convenient apparatus. The acetic acid produced had an odour more penetrating and agreeable: but all the other phenomena were the ſame as with the acetate of lead; that is to ſay, there was an equal developement of carbonic and ſulphurous acids, and charcoal was found in the reſidue.

Which determined the reporters to conclude,

The reporters conclude that this acetic acid is always mixed with ſulphurous acid; of which it cannot be freed entirely by M. Debuc's proceſs; and never has ſo pleaſing an odour as the common kind.

1. That acetic acid formed by the diſtillation of a mixture of ſulphate of copper and acetate of lead, is always mixed with ſulphurous acid, which does not become perceptible till towards the end of the diſtillation.

2. That it cannot be compleatly deprived of this ſulphurous acid by the rectification propoſed by M. Debuc.

3. That the acid itſelf, totally deprived of the ſulphurous acid, is never of ſo lively and agreeable an odour, as that drawn from the cryſtals of the acetate of copper.

4. That it is preferable to dry the ſulphate of copper before it is uſed.

5. That MM. Boddolier and Darac, were miſtaken in ſuppoſing, that no carbonic acid was obtained in this operation.

The difference of acetous and acetic acids probably does not depend on the ſtate of their acidification.

6. Finally that the production of carbonic acid does not any more prove the decarboniſation of the acetous acid in becoming the acetic, than the ſulphurous acid proves the ſuperoxigenation of the vinegar; but on the contrary that it is allowable to conclude, that the difference of theſe two ſubſtances is not cauſed by their ſtate of acidification.

Account

X.

*Account of the Imperial Botanic Garden of Schœnbrunn, in the Vicinity of Vienna.**

IN 1753 the emperor, Francis the first, caused a portion of ground behind the garden of the castle of Schœnbrunn to be prepared for the cultivation of exotics, and of plants remarkable for their rarity or beauty. By the advice of the celebrated Van Swieten, the famous florist Adrien Steekhoven was invited to Schœnbrunn from Leyden, who caused many greenhouses to be constructed there, with a very large and beautiful hot-house, and various other buildings. At the same time Richard Vander Schot, of Delft, was named first gardener, and employed to convey to Vienna a great number of rare and exotic plants, brought up in different parts of Holland, and thus at the end of one year the garden was already rich in valuable plants.

The garden established in 1753, by Francis I.

Put under the care of Adrien Steckhoven,

—Vander Schot the first gardener —brings to it many exotics from Holland.

M. Jacquin, who was then at Vienna, went to visit the garden of Schœnbrunn, to class those plants which had not yet received a specific denomination; on which occasion he became known to the emperor, who proposed to him to travel at his expence on the continent of South America, and in the American islands, to enrich the garden with plants from the most distant countries. Accompanied by the gardener Van der Schot, he departed from Vienna in 1754; and in passing through Italy was joined by Jean Buonamici and Ferdinand Barculli, who were entrusted with the zoological part of the expedition, by which it was proposed to improve the royal menagerie, and the cabinet of natural history at the same time. After having visited the islands of Martinico, of Grenada, St. Vincent, St. Eustatia, St. Christopher, St. Martin, St. Bartholomew, Aruba, Cuba, Caracca, and Jamaica, he returned to Vienna in 1759. From August 1757 to the middle of 1759, M. Jacquin could do little for the advancement of Science, having been ill of a lientery for four months, of which he was at last cured at Jamaica. The war which then commenced between England and France, also deranged his travels.

M. Jacquin sent to America to collect plants.

Account of his proceedings in the West India islands.

* Magasin Encyclopédique, T. 6, p. 552.

vels. The veſſel in which he made his voyage was taken, and he was thus obliged to paſs a conſiderable time, againſt his will, at Montſerrat and the deſert iſland of Gonave.

The firſt cargo of plants ſhipped, —the ſecond cargo brought by Vander Schot.

In the month of Auguſt 1757 the firſt cargo of plants for the garden of Schœbrunn was ſhipped from Martinico, which arrived at Marſeilles. In the month of February, 1757, Vander Schot returned alſo from Martinico, and brought with him from the ſame iſland a great quantity of trees and ſhrubs. All this cargo arrived ſafe, except ſome ſpecimens of *Aleliconia*, which were attacked on the voyage by mice. The trees were of the height of a man, and of the thickneſs of an arm, and ſometimes more. The moſt of them had born fruit in their native ſoil; their tops had been cut off, and only ſome of the principal branches were permitted to remain about two feet in length; the ſhrubs remained in their natural ſtate. To remove thoſe trees from their native earth, a circular trench was dug round each, at a convenient diſtance, in ſuch manner that there might remain attached to their roots as great a maſs of the earth in which they grew as was poſſible. This maſs, which formed a ſort of ball, was entirely wrapped up in leaves of the *Muſa*, ſecured with cords made of the bark of the *hibiſcus tiliaceus*, in ſuch a manner that the earth could not fall out. A ſingle tree packed in this manner, weighed commonly an hundred and odd pounds. The balls of earth were moiſtened a little, with the neceſſary care, and ſuſpended in the air, where the vegetation ſoon became apparent.

Method of preparing the trees for carriage, —and of packing them.

Weight of a tree when packed 100lb.

Method of tranſporting the packages.

To prevent the earth from being detached from the roots on the way, all the packages were tranſported in barks to the port of St. Pierre, in Martinico; from hence they were ſhipped to Marſeilles, and from thence brought by ſea alſo to Leghorn, and from this port were carried by mules to Schœnbrunn. This was without exception the richeſt cargo of living plants which had ever been brought from the hot countries to Europe.

The third cargo of plants ſhipped, —the fourth, and the fifth.

In the month of Auguſt, 1756, Buonamici ſet off with the third cargo from St. Euſtatia to Leghorn. The fourth cargo departed towards the end of the ſame year. The fifth was ſhipped from Curacao for Amſterdam, and was accompanied by J. A. Veſuntin, who died in Germany of the dyſentery. This cargo was extremely rich in corals and other productions of the ſea, which ſtill form ſome of the moſt precious ornaments of of the Imperial Cabinet. In the ſame year, M. Jacquin ſent off the ſixth cargo, from the ſame iſland to Amſterdam. And finally, in January 1759, MM. Jacquin and Barculli departed with the ſeventh cargo from the Havannah, for Ferrol in Spain, and arrived at Vienna in the month of July. This laſt cargo was particularly rich in animals of every ſpecies.

The ſixth cargo ſhipped, —the ſeventh brought by MM. Jacquin and Barculli, 1759.

Thus in the ſpace of a few years the number of plants in the garden of Schœnbrunn was conſiderably encreaſed; for, beſides thoſe which had arrived from America, means were found to make many important acquiſitions in different other countries. In 1765, after the death of Francis the firſt, Maria Thereſa ordered the garden to be conducted on the ſame footing that it was before. In 1780, a little while before the death of this princeſs, it ſuffered a ſmall but irreparable loſs; the gardener, Van der Schot, then very aged, had been confined to his chamber for many weeks by an attack of the gout. Thoſe to whom the management of the plants was entruſted in that period acquitted themſelves with great negligence; in one of the coldeſt nights of that winter, the perſon who ſhould have taken care of the great hot-houſe forgot to keep up the fire. In the morning he thought to repair this neglect by heating it to an unuſual degree; but the ſudden tranſition from cold to heat killed a great number of fine plants, and among others all the cinnamon trees from Martinico, of which the trunks were as thick as a man's arm, the heads very large, and of the greateſt beauty; and alſo deſtroyed the plants *Creſcentia*, *Achras*, *Annona*, *Portlandia*, and a *Coccoloba Grandifolia*, which was 20 feet high, and whoſe leaves were of the ſize of two feet.

The garden receives plants from other countries.

Maria Thereſa patroniſes the inſtitution.

Many valuable plants deſtroyed by the negligence of the attendants with regard to the hot houſe.

This garden alſo ſuffered another loſs. A conſiderable collection ſent from the iſle of France by M. Gere, arrived at Trieſte entirely ſpoiled, the trees all dead, and the ſeeds improlific.

A cargo of plants from the Iſle of France ſpoiled.

At this time the emperor, Joſeph the ſecond, directed M. Jacquin and M. Born to propoſe to ſome men of abilities to undertake a voyage into remote countries. Profeſſor Marter was appointed the conductor of this expedition, and Doctor Stupiez was aſſociated with him, together with the gardeners Boor and Bredemeyer, and the painter Moll. This company of travellers quitted Vienna in the month of April, 1783, and arrived in September at Philadelphia. They travelled over Pennſylvania,

Prof. Marter and others ſent to collect in America in 1783.

Pennſylvania, Virginia and Carolina. M. Boor along with M. Schopf, made a journey into Florida, and from thence paſſed to the iſland of Providence. M. Bredemeyer returned from Carolina, and paſſing through England, arrived at Vienna in November, 1784, with ſeveral very beautiful plants. Boor, who during his ſtay at the Bahama iſlands had collected many rare plants, returned to Vienna in the month of September, 1785. But the painter Moll and Doctor Stupiez were ſeparated from their fellow travellers.

Several fine plants arrive at Vienna from thence, with M. Bredemeyer.

Bredemeyer ſent out again, —ſearches the iſlands and continent to the river Oronooko.

By the orders of the emperor, M. Bredemeyer, and the gardener Schucht, went towards the end of the year 1784 to rejoin the director of this expedition, M. Marter, who remained all this time in America; they paſſed over many of the great iſlands and a part of the continent as far as the mouth of the river Oronoco.

Many rare plants brought back by him and M. Marter in 1788.

In 1788 they returned by way of Amſterdam to Vienna, and brought back many rare and new plants. M. Marter alſo arrived the ſame year, by the way of London and Bruſſels with a new collection of plants.

M. Boor and Scholl ſent to the Iſle of France.

The emperor had not forgotten the loſs of the plants from the iſle of France, and commiſſioned M. Boor and the gardener Scholl to go there, and touch on their way at the Cape of Good Hope. In the month of May 1786, they arrived at the Cape with the Dutch veſſels; M. Boor remained there till 1787, and then departed by himſelf for the iſle of France and that of Bourbon. In the month of January, 1788, he returned to the Cape with 280 caſes full of rare plants; and on the 20th of July in the ſame year arrived at Vienna with a great number of magnificent vegetables; but as all the caſes could not be brought in the veſſel, the gardener remained at the Cape with the remainder. There has not ſince been any poſſibility of getting them to Vienna, as well as many other plants; and the gardener Scholl remains at the Cape from that time, from whence he has ſent from time to time ſeeds and roots. Beſides this increaſe to the garden, the number of plants was augmented in different manners. Thus, at the ſale of the garden of Schwenk at the Hague, the emperor cauſed all the rare plants to be bought; and likewiſe M. Jacquin, the ſon, when he was on his travels over a great part of Europe, ſent to Schœnbrunn many exotic plants which he found in other gardens.

M. Boor returns with many fine plants, —leaves ſome behind at the Cape with Scholl.

The plants of Schwenk bought.

M. Jacquin, the ſon, ſends many exotics from his travels.

The

The emperor Joſeph alſo enlarged the hot-houſes, and cauſed others to be built. In order to bring back Scholl the gardener to Vienna, with the plants which remained in his care at the Cape, the emperor Leopold, in 1791, ordered the gardener Bredemeyer and young Van der Schot, (the ſon of him who had been with M. Jacquin in the Eaſt Indies) to ſail to the Iſle of France, where Cere had collected many plants for the imperial garden, and during their return they were to touch at the Cape to take up all thoſe which remained with Scholl. The captain of the veſſel, in which the two gardeners had taken their paſſage put into Malaga; where they diſcovered in time that he had bad intentions with reſpect to them; which obliged them to return to Vienna without performing their commiſſion. After the death of the emperor Leopold, his ſucceſſor Francis the ſecond had an hot-houſe conſtructed, 235 feet long, for the plants of the Cape. A new garden was alſo eſtabliſhed, of which Doctor Hoſt was appointed inſpector, and in which were carefully cultivated all the plants which grew in the ſtates belonging to the houſe of Auſtria.

Emperor Joſeph enlarges the hot houſes and builds others.

Emperor Leopold orders Bredemeyer to the Cape for Scholl,—he is diſappointed of his paſſage.

Francis II. builds an hot houſe 255 feet long for Cape plants.

A new garden added for plants of the Auſtrian ſtates.

By theſe details may be ſeen with what care this juſtly celebrated garden was augmented from the reign of Francis the firſt, and all aſtoniſhment will ceaſe at the riches it contains, and which have furniſhed materials for different magnificent works on Botany, ſuch as the *Icones plantarum rariorum*, publiſhed by M. Jacquin, and above all, that which appeared a few years ago, under the title of *Plantarum rariorum Horti Cæſarei Schœnbrunnenſis deſcriptiones et Icones*, in two volumes folio, containing 150 coloured engravings.

Valuable Botanic publications from the garden of Schœnbrunn.

XI.

Letter from a Correſpondent on the Means of increaſing the Action of Sound on the Organs of ſuch as are partially deaf.

To Mr. NICHOLSON.

SIR

ALTHOUGH I am ſo deaf as not to be able to hear the beating of a watch, unleſs it be put cloſe to the ear, yet, if I place one end of a ſtick, or of a metal rod between my teeth, and

Sounds imperfectly heard rendered audible through a ſolid applied to the teeth.

and the other end upon the watch, at the distance of several feet, I can hear it very distinctly.

The hearing considerably assisted by pressing the external ear forward;

I know only two methods of alleviating the difficulty of hearing articulated sounds; one is by surrounding the ear, with the hand open, and pressing it forward, the fingers and thumb being fixed behind it; this expedient does more than might be supposed. Another method is, that of the application of a trumpet; which however, is of but little use, constructed as it is at present.

Or by a trumpet, though not considerably useful.

The discovery of any instrument to facilitate hearing, by being placed in the mouth (probably after the manner of a tobacco-pipe) would be of great importance to a numerous class of our fellow creatures, whose faculty of hearing is nearly sufficient for common conversation. If an instrument should be invented, which will do any thing at all in this way, our experience in regard to other inventions, encourages an expectation, that improvements will follow: means of assisting human sight have long been devised; little indeed has been done to assist defective hearing; it is however an object deserving of more attention than has been bestowed upon it. If you should be so good as to insert this in your Journal, I indulge a hope, that some of your ingenious correspondents, compassionating the unfortunate situation of those whose hearing is imperfect, may be led to attempt discoveries, the result of which may be of extensive utility. It is desirable to ascertain the best form and size of an ear trumpet, and what metal is to be preferred.

Probability that an instrument might be invented to act on the teeth or bones of the head so as to magnify sounds.

I am, Sir,

Your most obedient Servant,

A. B.

Reference to some disquisitions on sounds in the quarto series of this Journal.

P. S. On referring to my quarto edition of the Journal, vol. IV. page 383, I find something corresponding to my own observation. I shall be extremely obliged if your humanity should determine you to insert the above, as it may be a means of exciting investigation on a subject which is certainly of great consequence.

ANNOTATION—W. N.

Desultory remarks on the modification of sound by means of solid bodies.

A CONSIDERABLE mass of speculation concerning sound, and the means of encreasing its action on the organs of sense is to be found in my annotations on the experiments of Perrole, at p. 416 of vol. I. of the Quarto Series of this Journal. The excellen excellent papers of Mr. Gough, at page 66 and 160 of vol. X. of the prefent Octavo Series, concerning the augmentation of founds, and the fpeaking trumpet have added confiderably to our knowledge of this fubject. The memoir of Haffenfratz on the fame inftrument in our Ninth Volume, p. 283, and another, by the fame author, on the Propagation of Sound, at Vol. XI. p. 127, alfo deferve to be confulted. From the whole confideration of the facts it feems as if the fonorous vibration of the inftrument were of much more confequence than has hitherto been fufpected; and it feems not improbable, that a large furface expofed to the aerial pulfes of found, and having a tail of communication to be applied to the teeth, or inferted in the ear, might have confiderable effect. The ufe of the external ear, which has excited fo much difcuffion, may, perhaps, be of this kind. The experiment of Dr. Moyes (Philos. Journal, III. 57) of conveying found to great diftance through a ftring may be added to the other facts; and tends to fhew that the fonorous undulation does not require to be tranfmitted through fuch bodies as are the moft denfe, uniform, and elaftic. Leather, or felt, or pafteboard, or various other fimilar materials, are more frequently obferved to tremble in the hand at certain particular founds than many other denfe bodies.

XII.

Eafy and Correct Method of verifying the Portion of a Tranfit Inftrument. By J. S. BUTT, *Efq. Communicated by the Author.*

To Mr. NICHOLSON.

SIR, *Paragon, December* 8, 1805.

A SHORT note having appeared in Mr. Kelly's new edition of Spherics, defcribing my method of verifying the pofition of a tranfit inftrument, and thinking an account more in detail may not be unacceptable to your aftronomical readers, I am induced to trouble you with it, that you may, if you pleafe, infer it in your valuable Journal. Introductory note.

Your's refpectfully,

JAMES STRODE BUTT.

TO

The ufual method of adjufting a tranfit inftrument by a circumpolar ftar.

TO make the line of collimation move in the plane of the meridian, we are defired to obferve the tranfits of circumpolar ftars, and if the intervals between the times of their tranfits are equal, the tranfit inftrument moves in the place of the meridian: for the axis and line of collimation being previoufly adjufted, it muft pafs through the zenith; and if it divides the circle defcribed by any circumpolar ftar, into two equal parts, it muft pafs through the pole.

— requires the clock to keep time for at leaft 24 hours.

But here a difficulty arifes which is a probable alteration, or a want of uniformity in the rates of the clock or watch for fo long a period as twenty-four hours, or during that portion of time which the obferver may require to repeat his obfervations, fo as to be fatisfied.

Method independant of this rate and of previous right afcenfion, &c.

A method independant of the rate of a clock or watch for fo long a time, and alfo entirely of any other previous obfervations of right afcenfion, is a defideratum to practical aftronomers, and alfo to thofe who occafionally amufe themfelves by obferving time, and the rates of their chronometers, in their prefent improved ftate; but who may be unacquainted with aftronomical equations, of preceffion, nutation, &c.

Rule, obferve the tranfits of two different ftars, one above and the other below the pole; which differ only a fhort time, for example, only a few minutes: at any time afterwards repeat the obfervation upon the ftars when their fituations as to the pole are reverfed. If the difference in time be the fame the tranfit is duly placed, if not it muft be altered, &c.

Rule, Obferve the difference of tranfits of any two circumpolar ftars, that are fituated nearly in the fame azimuth, or vertical circle, the one above and the other below the pole: and whofe difference of right afcenfion is nearly 180°; (namely)

Obferve, The tranfit of α caffiop. above the pole, and immediately after it the tranfit of ε urfæ majoris below the pole, whofe difference of tranfit is not more than 15 minutes, and for fo fhort a time the clock or watch may be fafely depended on. Then invert the operation, and obferve the tranfit of α caffiop. below, and ε urfæ above the pole. If their difference of tranfits is the fame in both obfervations, the tranfit inftrument is accurately in the meridian; if not the error may be corrected by altering the pofition of the inftrument till their difference of tranfit is the fame in both obfervations.

Should the error be great it may be corrected nearly by any of the theorems now in ufe; (*vide* Wales on Time-Keepers) or half the difference may be fubftituted for the error, and by repeated approximation the tranfit inftrument may be accurately adjufted.

The

The advantage of this method is, that you rely upon the ſtars keeping $\frac{99}{100}$ of the time, which would otherwiſe be kept by the clock or watch; and it is of no conſequence whether the obſervations follow one another on the ſame day or week, provided the inſtrument is adjuſted to the ſame point of the horizon, previous to obſervation, for there is little or no difference in their preceſſion, &c. during an interval of a month.

Advantages. Moſt of the time is kept by the ſtars, and not by the clock.

Another advantage is, that the obſervations follow each other ſo ſoon, that you are not likely to be diſappointed by a change of weather; for each pair of obſervations is complete as far as it goes, which is not the caſe in the other method, which requiring an interval of twelve hours between each obſervation, a change of weather is more likely to take place.

The ſhort interval inſures againſt change of weather, &c.

A tranſit inſtrument is the baſis of aſtronomy, and whoever has the fixing of it ſhould conſider himſelf independant of every previous obſervation, and acting entirely upon principle, which is not the caſe where the adjuſtment is by previouſly obſerved right aſcenſion, and which require reducing to the day of obſervation; indeed nicely reduced right aſcenſions are not always in the hands of thoſe who may be wiſh to be in poſſeſſion of a ſimple and accurate method of placing a tranſit inſtrument preciſely in the meridian.

Other uſeful remarks.

This method was deviſed and uſed by me ſince 1794, but I have never read or heard of any one uſing the ſame.

J. S. B.

N. B. Proper ſtars in this Lat. are,

α Caſs. and ε Urſæ Majoris.
β Caſs. and δ Urſæ.
γ Caſs. and ϵ Urſæ.

Alſo the ſtars of Draco and Auriga.
Cepheus and Urſa.
Perſeus and Draco.

A large comet was diſcovered at the Royal Obſervatory Dec. 8, which paſſed the meridian at 6.h 24.m 7. mean time.

A large comet.

Obſerved right aſcenſion was - 353° 6′ 41″
Declination ſouth - - - - - 23° 41′ 8″

*** I have ſince heard that this comet was not again ſeen, but is ſuppoſed to have proceeded ſouthward.—N.

A Com-

XIII.

A Comparison of some Observations on the Diurnal Variations of the Barometer, made in Peyrouse's Voyage round the World, with those made at Calcutta by Dr. Balfour *.

Barometrical observations between the Tropics by Lamanon and by Dr. Balfour.

THE first of the observations here referred to were made by M. Lamanon, an ingenious naturalist who accompanied Peyrouse, and who has given an account of them, (see fourth volume of the *Voyage*, octavo edit.), in a letter to M. de Condorcet, dated St. Catharine, 5th November 1785. Dr. Balfour's Observations are in the *Asiatic Researches* for 1794, and a short account of them is also inserted in the fourth volume of the *Transactions, R. S. Edin.* Hist. p. 23.

M. Lamanon's observations were made in consequence of instructions from the Academy of Sciences, directing him to keep an exact account of the heights of the barometer in the vicinity of the equator at different hours of the day, with a view to discover, if possible, the quantity of the variation of that instrument, due to the action of the sun and moon, that quantity being there probably as its *maximum*, while the variations arising from other causes are at their *minimum*.

Lamanon used a marine Barometer of Nairne.

M. Lamanon was provided with one of Nairne's marine barometers, which, he says, was so little affected by the motion of the ship, that it might be depended on to the $\frac{1}{100}$ of an inch. In this barometer, he tells us, that from about the 11th degree of north latitude, he began to perceive a certain regular motion, so that the mercury stood highest about the middle of the day, from which time it descended till the evening, and rose again during the night. As they approached the equator, this became more distinctly perceptible; and on the 28th of September, the ship being then 1° 17′ north latitude, a series of observations was begun, and continued for every hour till the 1st of October, at 6 A. M. The following abstract shews the result of the observations on the 28th and 29th.

Regular diurnal change in lower Lats. than 11°. N.

Twenty eighth of September.

From 4 to 10 A. M.	Barometer rose	1*l.* $\frac{9}{10}$
From 10 A. M. to 4 P. M.	fell	1 $\frac{2}{10}$
From 4 to 10 P. M.	rose	0 $\frac{9}{10}$

* From the History of the Royal Society of Edinburgh, 1805.

Twenty

Twenty ninth of September.

From 10 (28th) to 4 A. M.	fell	1*l.* $\frac{3}{10}$
From 4 to 10 A. M.	rose	1 $\frac{5}{10}$
From 10 A. M. to 4 P. M	fell	1 $\frac{3}{10}$
From 4 to 10 P. M.	rose	1

The obfervation on the 30th were to the fame effect; and hence it is concluded that at the equator the flux and reflux of the atmofphere produces in the barometer a variation of about 1 line $\frac{2}{10}$ Englifh, correfponding, as M. Lamanon remarks, to a height in the atmofphere of nearly 100 feet. According to Bernouilli, the action of the fun and moon fhould produce a tide of feven feet, and according to Mr. de la Place, a tide not nearly fo great.

The effect is greater than might arife from the computed atmofpheric tides.

It fhould be obferved, that when thefe obfervations were made, the moon was in her laft quarter, and the fun a few degrees to the fouth of the equator. The latitude on the 28th was 50′ north, and 11′ north on the 29th; in the night between that and the 30th, the fhip croffed the line; and on the 30th at noon, the latitude was 42′ fouth: the longitude all this while between 17° 31′ and 18° 33′ weft of Paris, by the time-keeper; fo that the coaft of Africa, which was the neareft land, was diftant about 8° of a great circle, and the American continent about 19°.

Situation of the fun and moon, and place of the fhip.

She was *far out at fea.*

The agreement between thefe, and Dr. Balfour's obfervations at Calcutta is very remarkable. Dr. Balfour found that during the whole lunation, in which he obferved the barometer from half-hour to half-hour, the mercury conftantly fell from 10 at night to 6 in the morning; from 6 to 10 in the morning it rofe; from 10 in the morning to 6 at night it fell again; and laftly rofe from 6 to 10 at night. The *maximum* height is therefore at 10 at night and 10 in the morning, and the *minimum* at 6 at night and 6 in the morning. The only difference is, that in Mr. Lamanon's obfervations, the *minimum* is ftated to have happened about 4 inftead of 6. This, however, will not feem a very material difference, when it is remembered, that the inftant when any quantity attains either its greateft or its leaft ftate is not eafily afcertained with precifion. From the obfervations as detailed by M. Lamanon, the time of the *minimum* feems to anfwer fully as well to 5 as to 4; fo that the difference of the refults is in

Argreement between thefe Obfervations, and thofe of Dr. Balfour *at Calcutta.*

every

every view inconfiderable, and their coincidence on the whole, not a little fingular. The variations in Dr. Balfour's barometer between the neareft *maximum* and *maximum* is fometimes about $\frac{1}{10}$ of an inch, though in general confiderably lefs.

Whether the caufe which produces land and fea wiuds could produce the regular change.

In the abftract of Dr. Balfour's obfervations referred to above, it is remarked, that it feems not improbable that thefe variations of the barometer are connected with the reciprocations of the fea and land winds during the day and night. But whatever may have been formerly the probability of this fuppofition, it is entirely deftroyed by the obfervations of the French navigators. Thefe obfervations were made too far out at fea to leave room for fuppofing that the land winds had any influence on the phenomena to which they refer. It is at the fame time doubtful, whether thofe phenomena can be afcribed to the atmofpherical tides produced by the fun and moon, as the *ebbing* and *flowing* of the mercury in the barometer appears to have no dependence on the pofition of thofe luminaries relatively to one another, but happens, it would feem, conftantly at the fame hour, in all afpects of the moon and all feafons of the year. The fubject is well deferving of a fuller inveftigation. We fhould probably before now have had farther information refpecting it, if happily the able navigator above-named, and his brave affociates, had been deftined to revifit their native fhores. The cruel fate of an expedition fo well planned, and fo well appointed for the purpofes of fcience, will never ceafe to be matter of fincere regret.

—moft probably not.

—neither is it likely that it was caufed by tides in the air, as it does not follow the moon.

Annotation.—W. N.

Probability that the equi-tropical change is caufed by afcending and defcending currents in the atmofphere.

I have inferted the foregoing with a view, in fome meafure, to afford a comparifon with Mr. Horfburgh's paper on the fame fubject, at page 16. It is not without diffidence that I venture to propofe a conjecture on this fubject, which in fact requires more confideration than I can, at prefent, beftow on it. Its change feems to me to be governed by the afcent of the air which would take place immediately beneath the fun, if the earth were ftationary, and the furrounding defcent of the fame fluid, of which the circumftances and modifications are fo well

explained

explained by Prieur in his memoir on the morning and evening dew (at p. 86, vol. IV. of our quarto ſeries.) The conſiderations there detailed may be eaſily extended to ſhew alſo that the effects muſt be greatly altered, and, in moſt inſtances, obliterated by the vicinity of land; which even changes the regular trade winds into land and ſea breezes.

XIV.

Abſtract of a Memoir on the Direction and Velocity of the Motion of the Sun and Solar Syſtem. By Dr. HERSCHEL. *From the Philoſophical Tranſactions,* 1805. (A.)

THE learned author begins his paper by noticing Dr. Maſkelyne's table of the proper motions of 36 ſtars of the firſt magnitude, and conceives that if this table affords proof of motion in ſtars in our immediate neighbourhood, the changes of poſition in minute double ſtars, many of which are only to be ſeen by means of the beſt teleſcopes, likewiſe prove that motions are equally carried on in the remoteſt regions of ſpace. Proper motions of the fixed ſtars.

In 1783, the Doctor deduced from the proper motions of the ſtars, a motion of the ſun and ſolar ſyſtem towards λ hercules; and the opinion he then conceived has been much ſtrengthened by the conſiderations ſtated in the following pages. Should this doctrine be eſtabliſhed, many phenomena may be accounted for, which without it muſt remain inexplicable. Deduction of a proper motion of the ſun and its direction.

Though it was propoſed, by the admiſſion of a ſolar motion, to take away many of the proper motions of ſtars, by inveſting the ſun with a contrary one; our author admits that it will reveal a vaſtly greater number of concealed real motions than would be neceſſary to admit, were the ſun at reſt; and that the neceſſity for admitting its motion ought therefore to be well eſtabliſhed. Conſiderations in favour of ſuch a motion.

The motion of ſatellites round their primary planets, and of theſe round the ſun, ſuggeſts the idea of a revolution of the latter round ſome other unknown centre; nor are we without hypotheſes built upon this conjecture.* Reaſons from analogy of the planets, &c.

The poſſibility of a ſolar motion has been ſhown by the late

* See Syſtême du Monde de Lambert, p. 152, 158. Alſo Phil. Tranſ for the year 1783, p. 283.

Dr.

Dr. Wilfon, of Glafgow, upon theoretical principles; and its probability, from reafons of the fame nature, by M. de Lalande.

Probability that the fun has progreffive as well as rotatory motion,

The rotatory motion of the fun, from which the latter concludes a difplacing of the folar centre, indicates a motion of tranflation in fpace; for it is not very probable that the mechanical impreffion which gave the former, fhould not alfo occafion the latter. This however can be admitted only as a plaufible hypothefis, until we attain a knowledge of the caufe of the rotatory motion.

—and the variable ftars alfo.

This argument might be ftrengthened by clofely obferving the ftars which change their magnitudes periodically; for if thefe changes arife from a rotatory motion,* a real motion in fpace may be expected to attend it; and the multitude of thefe ftars is fo great, that their concurrent teftimony is defirable.

Three forts of motions of ftars.

But fetting afide theoretical arguments, the Doctor notices that as all parallactic motions indicate the obferver not to be at reft, it may be neceffary to explain three forts of motions, which will frequently be alluded to in the following difcuffion.

Parallactic, real, and apparent.

Suppofe the folar fyftem to move towards a certain part of the heavens, the ftars, to an inhabitant of the earth, will appear to move in an oppofite direction. Let $s\,p$ (*Pl.* II. *Fig.* 1.) reprefent the parallactic motion of a ftar; which, if the ftar have no real motion, will alfo be its apparent motion; but if it fhould have a real motion, which in the fame time that it could have gone from s to p, would have carried it from s to r, it will be feen to move along the diagonal $s\,a$; and $p\,a$, being parallel and equal to $s\,r$ will reprefent its real motion. The triangle $s\,p\,a$ is fuppofed to be formed in the concave of the heavens by three arches of great circles, the obferver being in the centre, and $s\,p$ reprefents the parallactic, $p\,a$ the real, and $s\,a$ the apparent motion of the ftar. The fituation and length of thefe arches in feconds of a degree will reprefent the direction and quantity of each motion; and calling the folar motion S, the diftance of the ftar from the fun d, and the fine of the ftar's diftance from the point towards which the fun is moving ϕ; the parallactic motion will be expreffed thus: $\frac{\phi.S}{r.d} = s\,p$.

The largeft ftars are moft fit to fhew the fun's motion.

A motion of the fun will occafion parallactic motions of the ftars, and *vice verfa*; but to afcertain if parallactic motions exift, fuch ftars fhould be examined as are moft vifibly affected

* See Phil. Tranf. for the year 1795, p. 68, and our Journal, XI. 271.

by

by folar motion; which points out the brighteft ftars as moft proper for the purpofe; for any ftar may have great real motion, but to have great parallactic motion it muft be in the neighbourhood of the fun.

Parallactic may be diftinguifhed from real motions by their directions: for, if a folar motion exift, all parallactic ones will tend to a point in oppofition to the direction of that motion; but real motions will be indifcriminately difperfed. The parallactic motions are directed to a point,

Under thefe diftinctions, the proper motions of the ftars, if the fun be not at reft, will be parallactic, or compofed of real and parallactic; the latter cafe conftituting the apparent motion of the ftar. and will combine with the real (angular).

Dr. H. next defcribes the meeting of the arches arifing from a calculation of the proper motions of the 36 ftars in Dr. Mafkelyne's catalogue on a celeftial globe, of which ten were made by ftars of the firft magnitude, about the conftellation Hercules; beyond thefe there was no appearance of any other than a promifcuous fituation of interfections.—Of the interfecting points, that towards which the fun moves is denominated the apex of its motion; and as the ftars will then have a parallactic motion towards the oppofite point, it has received the appellation of a parallactic centre. Deduction of the parallactic centre from obf. on many ftars.

Tabulated refults.

Interfecting points.	Right Afcenfion. °	′	″	Polar diftance North. °	′	″
1. Sirius and Arcturus, in the mouth of the Dragon - - -	255	39	50	36	41	34
2. Sirius and Capella, near the following hand of Hercules - -	275	9	32	64	21	48
3. Sirius and Lyra, between the hand and knee of Hercules - -	272	23	58	58	23	24
4. Sirius and Aldebaran, in the following leg of Hercules - -	263	25	38	44	39	47
5. Arcturus and Capella, N. of the preceding wing of the Swan -	290	0	58	32	7	23
6. Arcturus and Aldebaran, in the neck of the Dragon - - -	267	2	19	33	57	20
7. Arcturus and Procyon, in the preceding foot of Hercules -	235	3	13	46	21	34
8. Capella and Procyon, S. of the following hand of Hercules -	272	51	49	73	7	56
9. Lyra and Procyon, preceding the following fhoulder of Hercules	266	46	49	66	48	11
10. Aldebaran and Procyon, in the breaft of Hercules - -	260	1	29	60	59	34

As

Confirmation by other ſtars.

As a further confirmation that the parallactic motion may be perceived in the motion of the brighteſt ſtars, Dr. H. examined the interſections made by the proper motions of ſome large ſtars of the next order, with the arches in which the ſtars of the firſt magnitude move, and found 15 which gave ſimilar reſults with the former 10, in pointing out the ſame part of the heavens as a parallactic centre.

This reſult confirmed by double ſtars.

Changes in the poſition of double ſtars indicate the ſame reſult, and may therefore be more eligibly aſcribed to the effect of parallax, than to admit of ſeparate motions in different ſtars: for, if the alterations of the angle of poſition were owing to a motion of the largeſt ſtar in each ſet, ſuch motions muſt, in contradiction to probability, tend nearly to one particular part of the heavens. This argument derives its validity from the ſame ſource with the former, viz. the parallactic motions of at leaſt 28 more ſtars pointing out the ſame apex of a ſolar motion, by their direction to its oppoſite parallactic centre.

and by the harmony of the proper motions deduced from it.

The incongruous mixture of great velocity and extreme ſlowneſs in the proper motions of the ſtars of the ſame magnitude, is removed by the conſideration of parallax from the ſolar motion; and it will be ſeen that there is a general conſiſtency in their motions. The ſame obſervation is alſo applicable with reſpect to the ſidereal occultation of a ſmall ſtar in the Swan.

Inveſtigation of the direction of the ſun's proper motion.

Dr. H. concludes from the foregoing premiſes, that the expediency of admitting a ſolar motion will not be queſtioned, and proceeds to inveſtigate its direction. He begins by proving, that when the proper motions of two ſtars are given, an apex may be found, towards which if the ſun be ſuppoſed to move with a certain velocity, the two given motions may be reſolved into apparent changes ariſing from ſidereal parallax, the ſtars remaining perfectly at reſt. For we muſt not admit more motions than are ſufficient to account for the obſerved changes in the ſituation of the ſtars; and it would be wrong to have recourſe to the motions of two ſtars, when that of the ſun alone may be ſufficient to account for both; which conſideration would be a ſufficient inducement for fixing at once on the calculated apex as well as on the relative diſtances aſſigned to the two ſtars, could other proper motions be, with equal facility, reſolved into ſimilar parallactic appearances; but, when a third ſtar does not direct towards the

An apex or parallactic center is deduced from the apparent motions of two ſtars, ſuppoſed to have no real motion.

 ſame

fame apex as the former two, its apparent motion cannot be refolved by the effect of parallax alone; and this difficulty is further enhanced by the number of apices required to folve all proper motions into parallactic ones, increafing, not as the number of ftars admitted to have proper motions, but, when their fituation is favourable, as the fum of an arithmetical feries of numbers, beginning at 0, continued to as many terms as there are ftars admitted.

The author here propofes an illuftration of his fubject by confidering the three apices, or interfecting points, No. 1, 2, 5, in the foregoing table.

The diftance of Arcturus from the apex of the folar motion is found to be 47° 7′ 6″, and its parallactic motion, which is as the fine of that diftance 2.08718″, which is the apparent motion of Arcturus, as eftablifhed by obfervation.

Namely Arcturus and Sirius.

Admitting Sirius to be a very large ftar, at the diftance of 1.6809 from us, and computing its elongation from the apex of the folar motion at 138° 50′ 14.5″, its parallactic motion will be $\frac{\phi \,.\, S}{r \,:\, d} = s\,p = 1.11528''$, which alfo agrees with the apparent motion already afcertained by obfervation as the proper motion of Sirius.

The diftance of Capella from the apex of the folar motion is 80° 54′ 46″, and admitting the velocity of the fun towards the before given point, it will occafion a parallactic motion of Capella, in a direction 89° 54′ 48″ fouth-following its parallel, amounting to 2.8125″. Capella is here taken for a ftar of the firft magnitude, fuppofing its diftance from us to be equal to that of Arcturus.

Hence the parallactic motion of another, viz. Capella, is deduced:

By conftructing a triangle, the fides of which reprefent the three motions of every ftar, not at reft; one of the fides, reprefenting the apparent motion, will be equal to 0.4637″; the other fide, being the parallactic motion, 2.8125″; and the included angle 18° 19′ 27″, from which will be obtained the third fide, or the real motion of the ftar, 2.3757″. By the given fituation of this triangle with refpect to the parallel of declination of Capella, the angle of the real motion will be had, which is 86° 34′ 11″ north-following the parallel of this ftar. A compofition of the parallactic and real motions in the directions, will produce the annual apparent motion, as eftablifhed by obfervation.

and by refolving this into the apparent motion and another, this laft will be the real (angular) motion, (fuppofing the other ftars to have none.)

It is here obferved, that although the proper motion of a third

But it is not fit that all the real motion ſhould be aſcribed to Capella;

third ſtar is accounted for by retaining the ſame apex of the ſolar motion which explained the apparent motions of the other two, yet a great degree of real motion has been aſſigned to Capella, of which Arcturus and Sirius have been altogether deprived; which ſhews that the apex of ſolar motion muſt be ſo fixed as to be equally favourable to every ſtar proper for directing our choice. Hence a problem ariſes, for diſcovering a point whoſe ſituation among three given apices ſhall be ſuch as if the ſun's motion be directed towards it, there may be taken away the greateſt poſſible quantity of proper motion from the three given ſtars. The intricacy of this problem is, that by a change of the diſtance of the apex from any one of the ſtars, its parallactic motion, which is as the ſine of that diſtance, will be affected; ſo that it is not merely the alteration of the angle of direction which is concerned. From the ſolution of this problem, a much more complex one would ariſe, as three ſtars would certainly not be ſufficient to direct the preſent endeavour to find the beſt ſituation of an apex for the ſolar motion.

but the apex muſt be taken ſo as to leave the real motions as ſmall as poſſible.

Apparent motions of ſix bright ſtars

It was before ſhewn that the brighteſt ſtars are the moſt proper for demonſtrating the effect of parallax, and that in ſearching after the direction of the ſolar motion, the aim ſhould be to reduce the proper motions of the ſtars to their loweſt quantities. The ſix principal ſtars, whoſe interſecting arches have been given, when their proper motions in right aſcenſion and polar diſtance are brought into one direction, will have the following apparent motions:

tabulated.

Names of the Stars.	Direction of the apparent Motions.				Quantities of the apparent Motions, per Year
Sirius,	68°	49′	40.7″	South-preceding.	1.11528″
Arcturus,	55	29	42.0	Ditto.	2.08718
Capella,	71	35	22.4	South-following.	0.46374
Lyra,	56	20	57.3	North-following.	0.32435
Aldebaran,	76	29	37.3	South-following.	0.12341
Procyon,	50	2	24.5	South-preceding.	1.23941
				Sum of the apparent Motions,	5.35337

In

In ſeeking a ſolar motion, which requires the leaſt motion in the above ſix ſtars, let the line *p a*, *Fig*. 1, which repreſents the real motion, be brought into the ſituation *m a*, and the real motion required will then be at a minimum. If by the choice of an apex for the ſolar motion the angle at *s*, made by the lines *s p* and *s a*, can be leſſened, the quantity of real motion required to bring the ſtar from the parallactic line *s p m* to the obſerved poſition *a*, will alſo be diminiſhed.

Deduction of a ſolar motion that ſhall leave the real motions of theſe the leaſt poſſible.

It has already been ſhewn that when two ſtars only are given, the line *s p* may be made to coincide with the line *s a* of both ſtars, whereby their real motions are reduced to nothing; and that when three ſtars are concerned ſome real motion muſt be admitted in one of them. Now, ſince all parallactic motions are directed towards the ſame center, a ſingle line may repreſent the direction of the effect of the parallax. Therefore, let *s* P or *s* S, *Fig*. 2, ſtand for the direction of the parallactic motion of the ſtars; and as in the foregoing table we have the angles of the apparent motion of ſix ſtars, with the parallel of each, the direction of the line *s* P or *s* S muſt be computed with the parallels of the ſame ſtars, which may be done as ſoon as an apex for the ſolar motion is fixed upon. The difference between theſe angles and the former will give the ſeveral parallactic angles P *s a* or S *s a*, required for an inveſtigation of the leaſt quantity, *m a*, belonging to every ſtar.

A ſingle line may ſhew the direction of parallactic effect in two ſtars, &c.

The author exemplifies what he here lays down, by ſuppoſing the ſun to move towards λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted with the ſix ſtars already ſelected, he obtains the angles of the parallactic motion with the parallel, the difference between which and the former apparent angles with the parallel of each ſtar gives the angles of the apparent with the parallactic motion, as repreſented in *Fig*. 2. The lines *s a* repreſent the annual quantity of the apparent motions.

Computation, ſuppoſing the ſun to move towards λ Herculis.

When the ſituation of the laſt mentioned angles is regulated as in the figure alluded to, the ſeveral lines *m a* may be drawn perpendicular to S P, and by computation their quantity will be found to be—

Sirius	- -	0.65437″
Arcturus	- -	1.28784
Capella	- -	0.10887
Lyra	- - -	0.11281
Aldebaran	- -	0.01104
Procyon	- -	0.04998
Sum	- -	2.22491

The result of this investigation is, that by admitting a motion of the sun towards λ Herculis, the annual proper motions of the fix stars alluded to, of which the sum is 5.3537″, may be reduced to real motions of no more than 2.2249″.

A more favourable apex.

The author here observes, that although the precise place of the best apex is difficult to ascertain, a more favourable one than that above proposed may be obtained: for, by inspection of the figure which represents the quantities of real motion required, when λ Herculis is fixed upon, it will appear that by a regular method of approximation, the line S P may be turned into a situation, wherein all the angles of the apparent motion of the fix stars will be much reduced; and it is evident that the parallactic line S P should be turned more towards the line *s a*, representing the apparent motion of Sirius. He accordingly tries a point near the following knee of Hercules, whose right ascension in 270°. 15′. and north polar distance 54°. 45′, see *Fig.* 3, the quantities required for constructing which are found by the same method as already described in *Fig.* 2. By a calculation of the angles and the least quantities of real motion, according to this apex, it appeared that the annual motion of the fix stars was reduced to 1.4594″, which is 0.7655″ less than when the apex was λ Herculis.

Its situation.

Supposition that Sirius may be most affected by parallax, as brightest;

In the approximation to this point, it appeared, that when the line of the parallactic motion of Sirius was made to coincide with its apparent motion, a certain minimum might be easily obtained of the other parallactic motions. But as Sirius has not the greatest proper motion, the author conceived that another minimum, obtained from the line wherein Arcturus appears to move might be more accurate; as this star from its great proper motion may be more affected by the parallax arising from the motion of the sun. He therefore chose a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two stars which have the greatest motions.

or Arcturus, as having the greatest apparent motion.

" If

"If the principle of determining the direction of the solar motion by the stars which have the greatest proper motion, be admitted", observes the author, " the following apex must be extremely near the truth: for, an alteration of a few minutes in right ascension or polar distance either way, will immediately increase the required real motions of our stars. Its place is, right ascension 245° 52′ 30″, and north polar distance 40° 22′". The calculation is delineated in *Fig*. 4. The sum of the least quantities of real motion in this experiment is 0.95595″, less than the former by 0.50343″.

Apex on this last supposition.

In these calculations the author has proceeded upon the principle of obtaining the least possible quantity of real motion to ascertain the most favourable situation for a solar apex, and has proved that the sum of the observed proper motions of the six principal stars may be the result of a composition of two other motions; and that if the real motions were reduced to their smallest possible quantities, they would not exceed 0.9559.

If the nearest stars be most affected by parallax, their proper motions may also be more evident.

The Doctor, however, seems to think that these real motions may not be brought down to the low quantities mentioned; and proceeds to shew that this circumstance will not affect the arguments he has used for establishing the method he has adopted; for, although the great proper motions of Arcturus, Procyon, and Sirius, are strong indications of their being affected by parallax, it is not probable that the apparent changes of their situations should be entirely owing to solar motion; but that their own real motions would have a great share in them; and it is evident that in parallactic motions the distance of a star from the sun is of material consequence; and as this cannot be assumed at pleasure, we are not at liberty to make the parallactic motion sp, *Fig*. 1, equal to the line sm; hence it follows that the real motion of the star cannot be from m to a, but will be from p to a. If, however, $m\,a$ be a minimum, $p\,a$ when sp is given, will also be a minimum, and if all the $m\,a$'s in *Fig*. 4 be minima, the sp's will give the $p\,a$'s as small as possible; which is the point desired to be established.

Conclusion. Preferable situation of the apex.

In concluding Dr. H. observes, that as it is known that proper motions do exist, and as no solar motion can resolve them entirely into parallactic ones, we ought to prefer that direction of the motion of the sun which will take away most real motion, and this, as has been shewn, will be done when the right ascension of the Apex is 245° 52′ 30″, and its north polar distance 40° 22′.

XV.

New Experiments on the Solution of Sulphur in Alcohol, and the various Kinds of Ether. By M. FAVRE *.

Probability that ether would dissolve more sulphur than alcohol.

IN my first note on the solution of sulphur in alcohol, I announced an intention of examining the solvent power of the several ethers upon this combustible; which I had at that time been prevented from, by being obliged to leave Paris for Brussells, to take the office of apothecary to the military hospital. In the paper alluded to, I hinted that it was probable ethers would dissolve sulphur in greater quantity than alcohol; I had been led into this opinion by the results obtained from mixing this mineral with alcohol, at various degrees. I observed, as already stated, that the more alcohol was rectified, the more readily it dissolved sulphur; and *vice versa,* which difference I imagined to proceed from the greater quantum of hydrogen contained in highly rectified alcohol. Knowing ethers to contain less carbon and more hydrogen than alcohol, I had no doubt that they would dissolve a greater quantity of sulphur. The result of the several experiments, which I made under this impression, I am now about to detail: from which it will be perceived that I was not mistaken in my conjecture. I shall also subjoin the new experiments, which I made with alcohol, to ascertain the precise quanity of sulphur it is capable of dissolving, in order to compare the results with those obtained from ethers.

Preparation, &c. of the ethers.

The ethers I employed were prepared with much exactness, and according to the methods recommended by professor Fourcroy. I took care, in each experiment, to ascertain the specific gravity of the ether made use of, the quantity of sulphur dissolved by it, the various results obtained with or without the contact of the sun's rays, and the properties of sulphurated ether.

First and second Experiments.

Sulphuric ether by long digestion without heat took up nearly

In each of two six-ounce matrasses I put two drachms of the flowers of sulphur, prepared in the same manner as for

* Van Mons's Journ. de Chimie, Vol. VI.

the

the experiments mentioned in my firſt note, viz. nicely waſhed, and one ounce of rectified ſulphuric ether, whoſe weight was 0.7396. Having ſecured the mouths of the mattraſſes with luting, I put one in a very light place, and the other in a dark place. I ſhook them every day, and at the end of a month, filtered their contents. On examination the two ſulphurated ethers obtained by theſe operations, preſented the following characteriſtics:

one-thirteenth part of ſulphur in the light; and only one-ſeventeenth in the dark.

The colour of the ether expoſed to the light was ſcarcely changed; it had a powerful hydro-ſulphurous ſmell, and its taſte was diſagreeable, and likewiſe hydro-ſulphurous. Mixed with diſtilled water, it precipitated nothing; but I remarked that the water diſſolved a much leſs quantity of it than when pure. In proportion as the ether became volatiliſed, the ſulphur formed a whitiſh ſcum on the ſurface of the liquid, which at length was precipitated to the bottom of the glaſs in which the experiment was made. (I ſhall hereafter mention the quantity of this precipitate.) Put in contact with white metals, it deeply blackened them. (Care muſt be taken in this latter experiment to cloſe exactly the mouth of the veſſel in which the metals are placed in contact with ſulphuric ether, on account of the great tendency of ether to be converted into gas by its attraction of caloric from ſurrounding bodies.) When mixed with a ſolution of acetite of lead, it gave a pretty conſiderable black precipitate.

The ſulphurated ether prepared without light, poſſeſſed all the properties of the other, but in a leſs degree. It alſo was leſs impregnated with ſulphur: for, on a repetition of the experiment, and carefully weighing the products, I found that each ounce of the ether prepared in the light contained 38 grains of ſulphur; whilſt that prepared in the dark held only 29.

Third and fourth Experiments.

Having proceeded as above deſcribed, with nitric ether weighing 0.9088, I obtained an ether whoſe colour was in no degree changed; its ſmell and taſte, though hydro-ſulphurous, were not ſo powerful as thoſe of ſulphurated ſulphuric ether; mixed with diſtilled water, it preſented the ſame phenomena, but depoſited a leſs quantity of ſulphur. It diſcoloured white metals leſs forcibly than the preceding ether;

Nitric ether by the ſame treatment took up nearly one twenty-ſecond part of ſulphur in the light; and only twenty-fourth in the dark.

ether; and, in a word, it had all the qualities of fulphurated fulphuric ether, but in a lower degree. It likewife contained a lefs quantity of fulphur; the refult of the experiment made in the light being but 22 grains of precipitated fulphur; and 20 for that conducted in darknefs.

Fifth and fixth Experiments.

Muriatic ether took up one thirty-feventh in the light; and only one fifty-third in the dark.

With muriatic ether, weiging 0.7196, proceeding as already defcribed, and at the fame proportions, I obtained a fulphurated muriatic ether, poffeffing all the peculiarities above mentioned, but weaker. It contained only 13 grains of fulphur, when conducted in the light, and $9\frac{1}{8}$ grains wher managed in the dark.

Seventh and eighth Experiments.

Acetic ether took up very little fulphur.

Acetic ether weighing 0.8664, diffolved but a very fmall portion of fulphur, and its qualities were but flightly marked. It contained but three grains of fulphur in an ounce of ether, in the experiment made in the light, and about $1\frac{1}{2}$ grains in that made in the dark.

Ninth Experiment.

Solution of fulphur in alcohol was lefs charged than that of fulphuric ether.

Having made the foregoing experiments, I wifhed to afcertain the difference exifting between the feveral ethers and alcohol, in refpect to their capacity for diffolving fulphur: I therefore retraced the experiments I had formerly made with alcohol. To avoid the repetition of what has been already communicated in my firft effay, I fhall here merely ftate the quantity of fulphur I have been able to diffolve, either by fubmitting the mixture to a heat lefs than fufficient to caufe the alcohol to boil, or by expofing it to the light, or by placing it in a dark place. For thefe experiments I ufed alcohol of 43 degrees.

After digefting for 12 hours over a gentle fire an ounce of alcohol with two drachms of the flowers of fulphur, I obtained 23 grains of precipitate.

Tenth and eleventh Experiments.

On leaving fimilar mixtures, one expofed to the rays of the fun, and the other in a place impervious to the light, during a month, and proceeding as already defcribed. I obtained

tained 16 grains from the firſt mixture, and 13 from the ſecond.

After what has here been laid down, it is evident that ſulphuric ether diſſolved the greateſt quantity of ſulphur; for, after frequently repeating the experiment, I found the average to be 25 grains in an ounce. Nitric ether and alcohol at 43 degrees, diſſolved nearly in the ſame proportions; and acetic ether the leaſt of any. Recapitulation.

It has been long a deſideratum in medicine to diſcover a method of adminiſtering ſulphur in a ſtate of extreme diviſion, eſpecially in complaints of the lungs and diſeaſes of the ſkin. With this intent, phyſicians have recommended it to be diſſolved in eſſential oils, and to form what is known in pharmacy under the title of *balſams of ſulphur, terebinthinated, aniſated, &c.* Theſe medicaments have the diſadvantage of giving to the mixtures into which they enter an almoſt inſupportable taſte and ſmell of ſulphurated hydrogen. Sulphurated ether is free from this inconvenience; it may be eaſily mixed with other potions, to which it gives very little ſmell; and as the ſeparation of the ſulphur is only occaſioned by the evaporation of the ether, it may be eaſily prevented by keeping the mixture to which it is added cloſely corked. I have already adopted its uſe with ſucceſs, adminiſtered either upon ſugar, or with any appropriate vehicle: ſeveral phyſicians of my acquaintance, for whom I have prepared it, have likewiſe employed it with advantage: and I hope, ere long, to be able to flatter myſelf as having added an efficacious medicament to the art of healing. Sulphurated ether is a good medicine.

The ſulphurated ether may be alſo ſucceſsfully employed to detect the adulteration of wine with preparations of lead: in addition to the facility with which this ether precipitates the lead, in the form of a black ſulphur, it poſſeſſes the advantage of introducing nothing into the wine that can deceive as to its quality, which ſometimes happens even to thoſe who are accuſtomed to uſe the ſolution of ſulphur of potaſh. It may be uſed as a teſt for lead in wine.

I am now occupied in the cryſtallization of ſulphur diſſolved in ether, the reſult of which I ſhall loſe no time in laying before the public.

On

XVI.

On the Utility of ſcientific periodical Publications. In a Letter from Mr. RICHARD WINTER. *To which are added, ſome Experiments of Heat produced by a Blaſt of Air from Bellows.*

To Mr. NICHOLSON.

DEAR SIR,

Periodical works are of modern invention.

THE advantages derived from ſcientific periodic publications, are an acquiſition which former philoſophers were not poſſeſſed of; and it was not until the laſt century they were firſt inſtituted. The rapid progreſs of ſcience and information ſince that period, would be a ſufficient argument in favour of their decided utility, without any reference to ſyſtematic treatiſes publiſhed, of undoubted merit, and ſanctioned by univerſal approbation.

Advantages derived from ſcientific Journals.

To the active and ingenious mind in early life this mode of information is invaluable. Beſides furniſhing new ideas to the young ſtudent, they point out the preciſe ſtate of the different branches of human knowledge; they teach him the neceſſary caution for conducting experiments with vigour and accuracy, inſtead of drawing concluſions from a few inſulated analyſes, or imagining that his data are ſufficiently perfect for eſtabliſhing new ſyſtems. By reading theſe publications it is that he will enlarge his general conceptions, and will learn to emulate the various illuſtrious characters of all the enlightened countries of the world. In theſe treatiſes his views will not be confined to one object, but he will contemplate a ſcene continually varying. The phyſiology and phenomena of the animal and vegetable kingdoms; the actions and re-actions of the different elementary ſubſtances in nature, and their combinations with each other, will paſs in ſucceſſion under his obſervation.

The great phyſical laws which conſtitute and maintain the equilibrium of the world, are inſerted in reſpectable works of this nature as they are diſcovered and demonſtrated, while the errors of former philoſophers are detected and expoſed; by which means he has an opportunity of aſcertaining the value and correctneſs of thoſe works he may be already in poſſeſſion of.

To

To thofe who confult an Encyclopædia for fcientific matter, thefe publications are of indifpenfable utility, by continually pointing out the numerous improvements as they become public, and by that means the general fyftem of philofophical knowledge is kept to the level of the exifting ftate of difcovery.

To the mechanic a repofitory of this kind muft be highly ufeful, as the receptacle in which he may record his labours and improvements, and fecure to himfelf the well-earned fame of his difcoveries, at the fame time that he derives advantage from others following his example in their contributions to the general fund of fcience.

In fhort, there is no clafs of individuals but may profit from this method of extending ufeful knowledge. The fmall fum of feven-pence or eight-pence a week to any economical perfon is trifling, and there is no doubt but every enquirer will find fomething of which he may abridge himfelf, in order to become poffeffed of fuch an affemblage of facts and opinions. He is as it were making himfelf intimate with a clafs of men whofe names will be read with admiration by a grateful pofterity. It is only by familiarizing the mind with the fublime objects of fcience, and diffufing them over the face of the earth, that we can expect to eftablifh that fpirit of philanthropy and focial order, which is fo neceffary to the happinefs of the human race.

I will leave it to your judgment to abridge, or cancel the whole of this paper, as it would perhaps exclude more valuable fubjects.

I am, Sir,

With the greateft refpect,

Your very humble fervant,

RICHARD WINTER.

21, *Bolfover Street,*
Dec. 14, 1805.

The following experiments were made in order to afcertain whether a current of air projected upon a thermometer would increafe or diminifh the temperature. I made ufe of a pair of common bellows, the contents of which, when opened, were 95 cubic inches; the diameter of the end of the pipe was

The thermometer raifed by a blaft from bellows.

was $\frac{5}{17}$ths of an inch. The thermometer was adapted to Fahrenheit's fcale, and the refults of three experiments are exhibited in the following table:

		Number of Blafts.	Time of blowing.	Therm. rofe.
Exp. 1.	-	425	6 minutes.	4°
2.	-	222	3 ——	3.75
3.	-	217	3 ——	3.7

The current of air was directed againft the bulb of the thermometer. The diftance of the pipe out of which the air iffued, was half an inch from the bulb. The experiments were repeated with every caution poffible for twelve times, and always with the fame refults.

Mr. Dalton obferved (Philof. Journal, III. 160), that the thermometer fell on exhaufting the veffel in which it was placed, and rofe again on re-admitting the air. It is probable that the rifing of the thermometer in my experiments may be referred to the fame caufe, *viz.* the greater capacity of a vacuum for caloric than atmofpheric air.

XVII.

An Account of two interfecting Rainbows, feen at Dunglafs in Eaft Lothian in July laft, was communicated by Profeffor PLAYFAIR *.

Large rainbow where the fun was 2° high.

"AT Dunglafs, where I happened to be in the beginning of July laft, 1799, our attention was called one evening, a little before funfet, to a very large and beautiful rainbow, formed on a cloud which hung over the fea, and from which a fhower was falling at a confiderable diftance to the S. E. The fun was about 2° high, fo that the arch was not much lefs than a femicircle, with its higheft point elevated about 40°. At the point where the northern extremity of this arch touched the horizon, another arch feemed alfo to fpring from the fea, diverging from the former at an angle of 3° or 4°, on the fide toward the fun.

Another interfecting bow, over the fea.

* Edin. Tranf. 1805. Hiftory, p. 8.

This

This arch did not exceed 7° or 8° in length; it was of the same breadth with the principal bow; it had the colours in the same order, and nearly of the same brightnefs; or if any difference was difcernible, it was, that the tranfition from one colour to another was not made with fo much delicacy in the laft-mentioned rainbow as in the former.

It was a fhort portion,

We recollected that a phenomenon fimilar to this is defcribed in the *Philofophical Tranfactions*, as having been feen at Spithead, and that it is afcribed by the gentleman who obferved it to the reflection of the fun's rays from the furface of the fea, fo as to fall on the cloud where the rainbow was formed. This hypothefis feemed to agree exactly with the phenomenon now before us.

and was apparently formed by reflection of the fun's rays

The accidental rainbow, for fo it may be called, was feen only at the extremity where the principal arch rofe from the fea, and where of confequence, the fun's rays, reflected from the furface of the water, at that moment very fmooth, might fall on the drops of rain. The other parts of the cloud could not receive rays fo reflected, as the land intervened, and there, accordingly, no veftige of the accidental rainbow was obferved.

from the fmooth water.

The accidental rainbow lay, as was already faid, on the fide toward the fun, and this is agreeable to the hypothefis; for the rays that after reflection from the furface of the water fell on the drops of rain, muft have come as from a point as much depreffed below the horizon, as the fun was at that inftant elevated above it. The axis of the accidental rainbow muft therefore have made with the axis of the principal, an angle equal to twice the fun's elevation, and its center muft have been elevated by that fame quantity above the centre of the other, fo that if it had been complete, it would have been wholly between the principal rainbow and the fun.

Its center was above the horizon;

The only cirumftance in which the appearances did not perfectly correfpond with this hypothefis, was, that the two rainbows did not interfect one another in the horizon, but rather a little above it. This however, ought to have no great weight, as the reflected image of the fun cannot have prefented to the cloud a difk fo regular and well defined as the fun itfelf and the

but the interfection was not quite as low as the horizon.

the accidental rainbow muſt have ſomewhat participated of this indiſtinctneſs *.

The inclination of the two arcs computed,

When phenomena of this kind occur, it would afford a ſure means of trying the juſtneſs of the explanation, if the inclination of the two bows were obſerved, and alſo the ſun's altitude at the ſame time. Theſe two things are neceſſarily connected; for if we call I the angle of their interſection, E the elevation of the ſun, and S the angle ſubtended at the eye by the ſemidiameter of the rainbow, if complete, an angle which is conſtantly the ſame, and nearly equal to 42°, it is eaſy to infer from ſpherical trigonometry, that $\text{ſin } \frac{1}{2} \text{I} = \frac{\text{ſin E}}{\text{ſin S}}$.

and was a little more than the eſtimate.

Computing from this formula, the inclination of the two bows in the preſent inſtance comes out nearly 5°; ſomewhat greater than I was inclined to eſtimate it by the eye.

Phenomena of this kind can but rarely occur, as the neceſſary conditions will not often come together. The principal rainbow muſt be over the ſea; the ſea itſelf muſt extend ſomewhat on the ſide toward the ſun; it muſt be ſmooth and tranquil, and the ſun ſo low that the light reflected from the water may be conſiderable. Were it ever to happen that the accidental bow was completely formed; the effect could not fail to be very ſtriking.

* As the place of interſection will lie in a plane paſſing through the eye of the obſerver and parallel to the plane of reflection; does not this fact afford ground for a ſuſpicion that the reflection, at this low altitude, was made, not from the ſurface of the ſea, but from that of the ſtratum of vapour which occaſions looming, and has been ſo well treated of by Dr. Wollaſton and others, (ſee our Journal, VI. 46, and elſewhere), and that this ſtratum was higher farther out at ſea than near the coaſt?—N.

Notice

XVIII.

Notice of a Collection of Memoirs which have lately appeared at Paris, being Part of a Work on which the celebrated Lavoiſier was employed till the lamented Cloſe of his Life; with a Tranſlation of that Memoir, in which he claims the modern Theory of Chemiſtry as his own excluſive Diſcovery. Received from Mr. W. A. Cadell, at Paris.

To Mr. NICHOLSON,

SIR, *Paris, Oct.* 27, 1805.

I HAVE tranſlated the two following paſſages (pages 4 and 5) from a work which has lately appeared in two volumes octavo, entitled *Memoires de Chimie.* They will prove intereſting to the readers of your Journal. The firſt is the notice prefixed to the work by Madame Lavoiſier, (now counteſs of Rumford) it is written with the eloquence of real feeling, and I refer to it for an account of the nature of the work; the ſecond proves completely that the new theory of chemiſtry is due to M. Lavoſier alone. I alſo ſend you the titles of the papers of which the work is compoſed. I am, See introductory letter.

Your very humble ſervant,

W. A. CADELL.

CONTENTS OF THE TWO VOLUMES.

PART. I.—*General Views on Caloric: its Effects; the Manner of meaſuring it, and the Formation of Liquids and Fluids.*

1ſt mem. on caloric, by *Lavoiſier.* Mem. Ac. des Sci. 1777. Contents of the memoirs arranged by Lavoiſier.

2. On caloric, and the means of meaſuring its effects. *ib.* 1780. *Lavois. et Laplace.*

3. Supplement to the preceding. *Lavois. and Laplace.*

4. On ſome of the principal phenomena of chemiſtry.—*Seguin.* Soc. Philom. 1790.

5. On the natural zero. *Seguin.* Annal. de Chim. 1790.

6. On the effects of heat in dilating metals and glaſs, &c. *Laplace and Lavoiſier.*

7. On the paſſage of ſolids to a ſtate of liquidity by means of heat. *Lavoiſier.*

8. On the action of heat on liquids from their freezing point to that of their vaporization. *Lavoiſier.*

On

Contents of the memoirs arranged by Lavoisier.

9. On the combination of heat with different evaporable fubftances and the formation of feveral fluids. *Lavoifier*. Mem. Ac. Sci. 1777.

10. On the electricity that is abforbed by bodies that pafs to the ftate of vapour. *Lavoifier and Laplace*. M. A. S. 1781.

11. On the action of heat on fome aerial fluids from the freezing to the boiling point. *Guyton and Duvernois*.

12. On fome fubftances which are conftantly in the ftate of aerial fluid at the ordinary temperature and preffure of the atmofphere. *Lavoifier*.

13. Memoir on fome liqnids which can be obtained in an aerial form at a degree of heat a little above the mean temperature of the earth. M. A. S. 1777.

14. General views concerning the formation and conftitution of the atmofphere.

15. On the caufe of fome of the principal phenomena of meteorology.

PART II. *On the Decompofition of atmofpheric Air, its Analyfis and the Converfion of its Principles into the folid or liquid State.*

SECTION I. *On the Decompofition of Air by metalic Subftances and the Formation of Oxids.*

1. Memoir on the action of mercury upon atmofpheric air. *Lavoifier*. In pars. in M. A. S. 1777, p. 186.

2. On the decompofition of atmofpheric air by the oxidation of lead and tin performed by means of a burning glafs under a glafs receiver. *Lavoifier*. Opufc. Chim. chap. 6. pub. in 1773.

3. On the oxidation of tin in clofe veffels, &c. *Lavoifier*. Read in 1774.

4. On the decompofition of atmofpheric air by iron. *Lavois*.

5. Hiftorical details on the caufe of the augmentation of weight that metals acquire when heated with contact of air. *Lavoifier*. It is the paper of which I fend you the tranflation.

SECITON II. *on the Decompofition of Air by fimple inflammable Subftances which form Acids by their Combuftion.*

1. Memoir on the decompofition of air by phofphorus, and the formation of phofphoric acid. *Lavoifier*. Opufc. Chim.

2. Sup-

2. Supplement to the preceding paper. *Lavosier* M. A. S. 1777. Contents of the memoirs arranged by Lavoisier.

3. Proving that caloric disengaged from vital air during combustion is not possessed of weight susceptible of being estimated.

4. Process usually employed for obtaining phosphorus, phosphoric acid, and phosphoreous acid. *Seguin.*

5. Memoir on the combustion of phosphorus employed as an eudiometer. *Seguin.*

9. On the decomposition of air by sulphur, the formation of sulphureous and sulphuric acid, and the use of sulphurets in eudiometry. *Lavoisier.*

7. On the process employed in commerce to obtain the sulphureous and sulphuric acids. *Seguin.*

8. On the decomposition of air by charcoal and the formation of carbonic acid. *Lavoisier.* M. A. S. 1781.

2. On the formation of nitric acid by the immediate combination of azotic gas and vital air. *Seguin.*

10. On the eudiometer composed of nitrous gas. *Seguin.*

SECTION III. *On the Decomposition of Air by those simple inflammable Substances which do not form Acids by their Combustion.*

1. Mem. Account of the last experiments on the decomposition and recomposition of water. *Lavoisier* Journal Polytip. February, 1786.

2. Shewing that water is not a simple substance, but a binary combination of hydrogen and oxigen. *Lavoisier.* read in 1783.

3. Shewing by the decomposition of water that it is not a simple substance, and that there are several means of obtaining in abundance, the hydrogen gas, which is one of its elements. *Lavosier* M. A. S. 1781.

4. Report on the paper of Seguin, which treats of the combustion of hydrogen gas with vital air. *Lavoisier, Laplace, &c.*

5. On the combustion of hydrogen gas in close vessels. *Fourcroy, Vauquelin,* and *Seguin,* read in 1790.

PART IV. *On the principal Phenomena of the Animal Œconomy.*

1. Mem. Experiments on the respiration of animals, and the change which takes place in air in the lungs. M. A. S. 1777. p. 185.

2. The alterations that the air undergoes during reſpiration. *Lavoiſier*, read in 1785.

3. Memoir, report on a paper of Seguin's concerning reſpiration and animal heat. *Maquer and Fourcroy*.

4. On reſpiration and animal heat. *Seguin*.

5. On the reſpiration of animals. *Seguin and Lavoſier*, read in 1790.

Notice prefixed to the Work (by (Mad. Lavoiſier) counteſs of Rumford.

Intention of Lavoiſier to republiſh his memoirs,

In the year 1792 M. Lavoſier had formed the deſign of making a collection of all his memoirs which had been read at the academy during the twenty years preceding. This would have formed in ſome degree the hiſtory of modern chemiſtry.

In order to render this hiſtory more intereſting, and more complete he had propoſed to inſert the memoirs of thoſe, who having adopted his theory, had publiſhed experiments in ſupport of it.

in eight volumes.

This collection was to have been compriſed in about eight volumes.

All Europe is acquainted with the cauſe which prevented their completion.

Parts recovered.

The portion that have been recovered are, the firſt volume almoſt entire, the whole of the ſecond, and ſome ſheets of the fourth.

Several men of ſcience expreſſed a deſire for their publication: this was received with heſitation—it is difficult not to be under apprehenſions when we are intruſted with the power of publiſhing the unfiniſhed work of a man juſtly celebrated. When we have loſt the object of our affections and veneration, we ſhould employ an impartial criticiſm, in order to offer to the public thoſe of his works only which may augment his fame.

Madame Lavoiſier has printed them.

We ſhould have perſiſted, and theſe fragments would not have appeared, had they not contained a memoir of M. Lavoiſier (inſerted below page 5) in which he reclaims the modern theory of chemiſtry as belonging to himſelf, and ſtates the facts in ſupport of his claim.

It is conſequently a duty towards him to fix the opinion of men of ſcience concerning this point.

Lavoiſier was employed on this

Their indulgence is requeſted for the errors that may exiſt in ſome other parts of the collection. It will be granted when

5 they

they are informed that the greateſt part of the proof ſheets were reviſed in the laſt days of the author's life; and that whilſt he knew that his aſſaſſins were premeditating his death, M. Lavoſier, calm and intrepid, employed his laſt moments in a work which he conſidered as uſeful to ſcience, and gave a great example of that ſerenity which a virtuous and enlightened man can preſerve in the midſt of the moſt ſevere calamities. work in the laſt moments of his life.

PART II.—SECT. I. *Fifth Memoir*, (Tom. II. p. 78.)

Hiſtorical Details concerning the Augmentation of Weight which the Metals acquire when heated with Contact of Air. (By Lavoiſier.)

IT is not my object in this paper to give a compleat hiſtory of the different opinions that have been ſucceſſively adopted by the chemiſts and natural philoſophers on the cauſe of the augmentation of weight in metals expoſed to the action of heat; ſuch a hiſtory would only ſerve to ſhew how much the minds of men are ſuſceptible of being led aſtray when they give themſelves up to the ſpirit of theory, and how eaſily we are deceived by reaſoning, if it is not perpetually rectified by experiment. John Rey, a phyſician *(medecin)* little known is one of the firſt authors who has written on this ſubject; he lived in the beginning of the 17th century at Bugue in Perigord, and kept a correſpondence with the ſmall number of perſons who cultivated the ſciences at that time. Limit of this hiſtorical memoir. John Rey an early writer on combuſtion.

Neither Deſcartes nor Paſcal had yet appeared; the vacuum of Boyle, and that of Toricelli, the cauſe of the aſcent of liquids in tubes void of air, were unknown; experimental philoſophy did not exiſt; a profound darkneſs reigned in chemiſtry. Neverthelefs, J. Rey, in a work publiſhed in 1630, with a view of determining the cauſe of the augmentation of weight which takes place in lead and tin during their oxidation, diſplayed views ſo profound, ſo analogous to the facts which have been ſince confirmed by experiment, ſo conformable to the doctrines of ſaturation and affinity, that for a long time I could not help ſuſpecting that the eſſays of J. Rey had been compoſed at a much later period than that announced on the title page of the book. His philoſophy far exceeded that of his cotemporaries.

J. Rey, after having refuted ſucceſsfully, not by facts (for at that time the art of making experiments was in its infancy) but by concluſive reaſoning, the different cauſes to which the aug- He contends that metals gain weight from the air in oxidation.

augmentation of weight of metallic oxides might be attributed, expreſſes himſelf as follows in his 16th eſſay: "to this queſtion then, ſupported on the grounds already mentioned, I anſwer and maintain with confidence, that the increaſe of weight ariſes from the air of the veſſel, which is condenſed, rendered heavy, and adheſive, by the violent and long continued heat of the furnace; this air mixes itſelf with the calx (frequent agitation conducing) and attaches itſelf to the minuteſt molecules, in the ſame manner as water renders heavy ſand which is agitated with it, and moiſtens and adheres to the ſmalleſt grains.

He oppoſes other current opinions.

J. Rey combats in this work the opinion of Cardan *(lib. 5 de ſubtilitate)* on the augmentation of weight of metallic oxides; that of Scaliger, that of Cœſalpinus, who aſcribed this augmentation to a ſoot condenſed and reflected by the furnace, which ſoot, according to their opinion, fell down upon the metal. He ſhews likewiſe that the augmentation of weight proceeds neither from the veſſel, nor from any emanation of the charcoal, nor from the humidity of the air. It is difficult to conceive how J. Rey could attain to theſe concluſions by the force of reaſoning alone, without experiment, and ignorant as he was of many of the preliminary date.

His doctrines were not received by Boyle,

It appears that towards the end of the laſt century, when Boyle and ſome of his cotemporaries created the new ſcience of natural philoſophy, of which the ancients had not the ſlighteſt notion, the work of J. Rey was entirely forgotten.—Boyle, in his treatiſe on the weight of flame and of fire, publiſhed in 1670, that is 40 years after the publication of Rey's work, makes no mention of it; proceeding upon ſome illuſory experiments, he ſtill maintained at that time that the augmentation of weight which the metals acquire by their oxidation ariſes from the fixation of fire.

—nor by Lemery.

Lemery, who was an exact and ſcrupulous obſerver, embraced the ſame opinion: he attributes the oxidation of metals and their augmentation in weight which accompanies that operation, to the combination of igneous particles with the metal.

Opinion of Charras,

Charras, cotemporary of Lemery, aſcribed that augmentation to the acids of the wood and charcoal, which as he ſuppoſed penetrated the veſſels and entered into combination with the metals. Since that time the ſame acid of wood and charcoal

coal has re-appeared under the name of *acidum pingue*, igneous acid, and under ſeveral other denominations which it would be ſuperfluous to enumerate.

—and of Staahl,

Staahl could not be ignorant of the fact that metals expoſed to heat acquire an increaſe of weight; and yet he not only did not attempt to explain it, but alſo the ſyſtem under which he claſſed the whole of the chemical phenomena, and which after him has been ſo much extended, is abſolutely in contradiction with this capital fact.

Staahl ſuppoſed that the metals are compoſed of a metallic earth, and an inflammable principle, which he named phlogiſton; he pretended that they loſt this principle by their oxidation, and that they could not return to the metallic ſtate unleſs the phlogiſton they had loſt was reſtored to them.

Difficulties of the phlogiſtic ſyſtem.

It was difficult to imagine how the metals acquired weight, whilſt, according to Staahl's doctrine, they loſt a part of their ſubſtance; and on the other hand, how they diminiſhed in weight, when they recovered one of the principles which they had loſt; it was one of the chief difficulties that could be propoſed againſt the theory of Staahl, this difficulty however, has not hindered the theory from having a ſucceſs of limited duration.

Morveau's endeavours to remove them.

Guyton Morveau has made ſome unſucceſsful efforts to palliate this contradiction, in his diſſertation on this ſubject, under the title of *Degreſſions Academiques;* he ſuppoſes that phlogiſton is lighter than atmoſpheric air; and he concludes that all bodies that acquire phlogiſton ſhould loſe a part of their weight; that, on the contrary, thoſe which loſe phlogiſton ſhould augment in weight. This explanation would have been tenable, had the augmentation of weight acquired by the metallic oxides been equal only to the weight of the air diſplaced; or, which is the ſame thing, if it had diſappeared on weighing in vacuo; but the augmentation is much too conſiderable to admit of being attributed to that cauſe, ſince in ſome metals it exceeds one third of their weight. It is neceſſary then, either to give up the explanation of Guyton Morveau, or to ſuppoſe that phlogiſton has a negative gravity, a tendency to recede from the centre of the earth, a ſuppoſition incompatible with all the facts admitted by the diſciples of Staahl.

Hiſtory of the authors experiments.

Such was the ſtate of the ſcience when a ſet of experiments undertaken in 1772, upon the different kinds of air or gas

 which

which are difengaged in effervefcence, and a great number of other chemical operations, difcovered to me demonftratively the caufe of the augmentation of weight that the metals acquire when expofed to heat. At that time I was not acquainted with J. Rey's work upon the fubject, publifhed in 1630; and had I known it, I fhould have confidered his opinion in the light of a vague conjecture, which did honour to the genius of the author, but required the attention of chemifts in order to afcertain the truth of the opinion by experiment. I was young, I had newly entered the lifts of fcience, I was defirous of fame, and I thought it neceffary to take fome fteps to fecure to myfelf the property of my difcovery. At that time there exifted an habitual correfpondence between the men of fcience of France and thofe of England; there was a kind of rivality between the two nations, which gave importance to new experiments, and which fometimes was the caufe that the writers of the one or the other of the nations difputed the difcovery with the real author; confequently I thought it proper to depofit on the 1ft of November 1772, the following note in the hands of the fecretary of the Academy. This note was opened at the meeting of the 5th of May following, and mention of thefe circumftances marked at the top of the note. It was in the following terms:

He finds that fulphur and phofphorus gain weight by combuftion, and that the gain is from air abforbed. He infers that the phenomenon is general and difengages air from litharge on reducing it in clofed veffels.

"About eight days ago I difcovered that fulphur in burning, far from lofing, augments in weight; that is to fay, that from one pound of fulphur much more than one pound of vitriolic acid is obtained, without reckoning the humidity of the air; phofphorus prefents the fame phenomenon; this augmentation of weight arifes from a great quantity of air, which becomes fixed during the combuftion, and which combines with the vapours."

"This difcovery, which I confirmed by experiments which I regard as decifive, led me to think that what is obferved in the combuftion of fulphur and phofphorus might likewife take place with refpect to all the bodies which augment in weight by combuftion and by calcination, and I was perfuaded that the augmentation of weight in the calces of metals proceeded from the fame caufe. The experiment fully confirmed my conjectures; I operated the reduction of litharge in clofe veffels with Hales's apparatus, and I obferved that at the moment of the paffage of the calx into the metallic ftate, there was a difengage-

difengagement of air in confiderable quantity, and that this air formed a volume at leaft 1000 times greater than that of the litharge employed. As this difcovery appears to me one of the moft interefting which has been made fince Staahl, I thought it expedient to fecure to myfelf the property, by depofiting the prefent note in the hands of the fecretary of the academy, to remain fecret till the period when I fhall publifh my experiments."

(Signed) LAVOISIER.

Paris, 1ft November, 1772.

Comparing this firft note with that which I had depofited at the academy the 20th of October preceding on the combuftion of phofphorus, with the paper which I read at the academy of the public meeting of Eafter, 1773, and laftly with thofe that I have publifhed fucceffively, it is eafy to perceive that I had conceived fo early as the year 1772 the general idea of the theory of combuftion which I have fince publifhed.

Whence he vindicates his right to the modern theory of combuftion in 1772.

This theory which I have confiderably developed in 1777, and which almoft at that period I brought to the degree of perfection in which it is at prefent, was not begun to be taught by Fourcroy till the winter 1786-1787; it was not adopted by Guyton Morveau till a later period; and Berthollet wrote ftill in the language of the phlogiftic doctrine in 1785. This theory then is not, as I hear it called, the theory of the French chemifts; it is *mine*, and it is a property which I reclaim before the tribunal of my cotemporaries and of pofterity. Others undoubtedly have contributed to its perfection, but I hope that no one will difpute with me, all the theory of oxidation and combuftion; the analyfis and decompofition of air by metals and inflammable bodies; the theory of acidification; more accurate knowledge on the nature of a great many acids, and particularly the vegetable acids; the firft notions on the compofition of vegetable and animal fubftances; the theory of refpiration, in which Seguin co-operated with me; the prefent collection will prefent all the papers on which I found my claims; the reader will judge.

—which was not adopted by other chemifts till many years afterwards.

The claim fpecifically ftated.

ANNOTATION.—W. N.

It was my intention to have pointed out how far the earlier chemifts, as well as fome of the contemporaries of this deferv- edly

Notice of the early inventors

of the theory of combuftion.

edly celebrated philofopher, are intitled to rights which will greatly modify the unqualified claim he has made. I cannot now fay, whether Rey did, or did not *make experiments*, but whether he did or not, he certainly muft have founded his introductions upon facts; and between the obfervation of well eftablifhed facts, and the making of direct experiments there feems to be no effential difference. How it has happened that the great Robert Hooke, who had inveftigated the modern theory of combuftion in 1664 and publifhed it in an ample detail on his micrographia in 1675*, and John Mayow, who foon afterwards, or about the fame time eftablifhed the fame doctrine, and extended it to phyfiological refults, are overlooked by our author, appears to require fome difcuffion. I fhall take an early opportunity of refuming this fubject.

XIX.

On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Efq. F. R. S. Profeffor of Chemiftry in the Royal Inftitution †.

Acid of borax very ufeful in analyfis.

I HAVE found the boracic acid a very ufeful fubftance for bringing the conftituent parts of ftones containing a fixed alkali into folution.

It combines with earths by ignition and quits them to mineral acids.

Its attraction for the different fimple earths is confiderable at the heat of ignition, but the compounds that it forms with them are eafily decompofed by the mineral acids diffolved in water, and it is on this circumftance that the method of analyfis is founded.

Proceffes.

The proceffes are very fimple.

Pulverize the ftone and fufe with two parts boracic acid.

100 grains of the ftone to be examined in very fine powder, muft be fufed for about half an hour, at a ftrong red heat, in a crucible of platina or filver, with 200 grains of boracic acid.

Digeft with weak nitric acid.

An ounce and half of nitric acid, diluted with feven or eight times its quantity of water, muft be digefted upon the fufed mafs till the whole is decompofed.

Evaporate.

The fluid muft be evaporated till its quantity is reduced to an ounce and half or two ounces.

* Copied in our Journal quarto feries III. 479.

† Phil. Tranf. Part II. for 1805.

If

If the ftone contain filex, this earth will be feparated in the procefs of folution and evaporation; and it muft be collected upon a filter, and wafhed with diftilled water till the boracic acid and all the faline matter is feparated from it. Silex if prefent will feparate.

The fluid, mixed with the water that has paffed through the filter, muft be evaporated, till it is reduced to a convenient quantity, fuch as that of half a pint; when it muft be faturated with carbonate of ammonia, and boiled with an excefs of this falt, till all the materials that it contains, capable of being precipitated, have fallen to the bottom of the veffel. Precipitate the reft with carbonate of ammonia.

The folution muft then be feparated by the filter, and the earths and metallic oxides retained.

It muft be mixed with nitric acid till it taftes ftrongly four, and evaporated till the boracic acid appears free. Add nitric acid to the clear liquid.

The fluid muft be paffed through the filter, and fubjected to evaporation till it becomes dry; when, by expofure to a heat equal to 450° Farenheit, the nitrate of ammonia will be decompofed, and the nitrate of potafh or foda will remain in the veffel. Separate the boracic acid by evaporation. Decompofe the nitrate of ammonia by heat.

It will be unneceffary for me to defcribe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have ufed the common proceffes. I have feparated the alumine by folution of potafh, the lime by fulphuric acid, the oxide of iron by fuccinate of ammonia, the manganefe by hydrofulphuret of potafh, and the magnefia by pure foda.

XX.

Some Facts and Speculations on the luminous Phenomena of Electricity. W. N.

ABOUT eighteen years ago, I was confiderably occupied in experiments upon electricity, many of which were communicated in t1789, to the Royal Society, and were publifhed in the tranfactions. In the twenty-third fection of that communication, fome account is given of certain changes which take place in the luminous appearance of metallic balls when electrified; but the phenomena were not delineated, becaufe I referved them for another opportnnity. After fo long an interval Communication to the Royal Society on electricty.

terval of time, I now prefent them to the reader from my notes, and the fketch then made.

Three appearances of an electrified ball. It gives flafhes; is then luminous, and then gives flafhes of another kind.

Sept. 19, 1787. A fmall ball in the ftate of electricity called pofitive, threw out flafhes or ramified fparks; and when the intenfity was encreafed, the ball itfelf became luminous, at the fame time emitting the flafhes. When the electricity was ftill more ftrongly excited the flafhes ceafed, and a circle of light, extending about 45 degrees round the point fartheft from the ftem, was feen on the ball, and a ftrong wind proceeded from it.

Experiment with a ball $1\frac{1}{2}$ inch diameter.

A ball of one inch and a half diameter was ufed; and electricity communicated by means of a cylinder nine inches diameter, having its cufhion eight inches long. The excitation was ftrong enough, by flow turning with a fingle winch, to throw out large brufhes of light. When the rotation was quicker, the flafhes difappeared, and the circle of light was feen, having a bright fpeck moving irregularly round in its periphery.

Defcription.

Quicker turning threw out brufhes of light very different from the others: Thefe were lefs luminous in the branches; many ftarted out at once with a hoarfe found. They were greenifh at the point or furface of the ball, reddifh in the ftem, and ramified fooner. Half a dozen were fometimes feen flafhing out at once.

Experiment with a much fmaller ball. It became luminous, and acted like a point.

A ball of four tenths of an inch in diameter was ufed. Moderate excitation produced a denfe brufh of light about two inches in length. With ftronger electricity the brufh difappeared, and the upper half of the ball became luminous. When the excitation was ftill ftronger, more than half of the ball was luminous, as reprefented *Fig. 3, Plate I.* and fometimes a ramified flafh ftruck out from the top. Other flafhes were fometimes feen fideways when the electricity was ftrongeft of all; but this happened feldom.

The light was faint, and feemed to be about twice the diameter of the ball. It extended more than half way down, and fpread moft fideways.

A large ball $2\frac{1}{2}$ inch. diam.

When a larger ball of two and a half inch diameter was ufed, the brufhes of light flew out from three or four ftems together to the length of about fix or feven inches, making a hoarfe noife; but they could not be made to difappear, though they feemed now and then to ceafe for a moment when the turning was moft vigorous.

5 The

The next day, when the excitation was very nearly, but not quite, as ſtrong, it was obſerved that the order of theſe appearances could be effected by the affiſtance of a metallic point. Plentiful bruſhes were thrown out from a three inch ball, but they could not be made to diſappear. When a pointed wire or a ſmall metallic ball was preſented, the effects were as follow: Modification of the phenomena,

The point being at a great diſtance, the root of the bruſh had a luminous circle of lambent light round it on the ſurface of the ball. When the point was nearer, the bruſh diſappeared, and nothing was ſeen but an exceedingly bright ſpeck on the ſurface of the ball, which was ſometimes ſtationary and ſometimes moved about. When the point was ſtill nearer, the ſpeck threw out ramified ſparks of the ſecond kind, at the ſame time that a lambent luminous circle appeared. The ſpeck was never in the center of the circle, but moved at a diſtance round the circle, irregularly, ſometimes the one way and ſometimes the contrary, and was ſometimes ſtationary. by the vicinity of a point or ſmall ball.

Theſe two orders of bruſhes were entirely the ſame as thoſe of the day before. The luminous bruſh which firſt appeared had a ſtraight ſtem, then a broken or leſs luminous part, beyond which looſe cotton-looking fibres flew off in radial directions, as at *Fig.* 1, *Pl. I.* The latter ramified ſparks had a ſtraight central ſtem, out of which well defined branches iſſued nearly at right angles. They much more cloſely reſembled a tree bare of leaves. More particular deſcription of the luminous appearances.

The ſecond bruſh was not larger, but rather leſs in its dimenſions than the firſt.

When the ball of four-tenths of an inch was held at a certain diſtance from the two and a half inch ball, when electrified, the firſt kind of bruſh was ſeen on the ſide fartheſt from the ſmall ball, at the ſame time that the ſecond kind of ſpark or bruſh flew out towards the ſmall ball, and the lambent luminous appearance was ſeen on the ſurface. All the phenomena viſible at once.

Theſe are the general facts; but I have no doubt but they would preſent many modifications upon being repeated.

Theſe facts may ſerve to aſſiſt our meditations with regard to the nature of the electric ſpark. In a late paper by Mr. Biot, given at page 214 of our Vol. XII. the author makes an ingenious conjecture, that the light and heat in this phenomenon may have been produced by mechanical compreſſion Remarks on the electric ſpark.

 of

Whether Biot's theory of luminous condenfed air can be fupported.

of the atmofpheric air. Whether this fuppofition can be reconciled to the appearance of the fpark in oil, and to fome atmofpheric phenomena, in which we are told of luminous balls moving apparently with little velocity through the air, and particularly that flowly-moving artificial fire-ball, produced once, and only once, by Warltire, as narrated in Prieftley's work on air, may admit of queftion. When we confider that a particle of iron, cut off and fet on fire in the common action of ftriking a light, appears, from the vivacity of its combuftion, to be a body of confiderable magnitude, though the ufual quantity of metal would not form a ball of one thoufandth of an inch in diameter; when we confider the prodigious elevation of temperature indicated by the explofion of wires of all metals by the electric fhock, particularly in thofe beautiful and ftriking experiments which Van Marum has publifhed; and laftly, when we call to mind that a metallic chain lofes part of its weight every time a fhock is paffed through it, and that the fpark is never feen to pafs between incombuftible bodies—confiderable reafons will prefent themfelves in favour of a modified fuppofition, that the electric fpark may confift of, or be accompanied by, a portion of the body from which it proceeds.

Warltire's fire-ball.

Combuftion of flint and fteel,

requires a very minute portion of metal:

Electric temperature is extremely elevated:

All metals lofe by electricity, and the fpark paffes only between combuftible bodies.

Hence probably it was part of the body fet on fire.

Fire-Balls, &c. may be electric fparks;

Are not the atmofpheric fire-balls or luminous meteors, the fhooting ftars and the ftones which have fallen from the atmofphere, electric fparks upon a fcale of immenfe magnitude?

and the fpark a fire-ball.

If any luminous ball were to pafs with a fwift angular motion over the field of view, it would have the appearance of a line or ftreak of light. If it were to break in pieces many divergent ftreaks would be feen. May not the electric brufh be a phenomenon of this defcription on a fmall fcale?

Facts are more wanted than conjectures.

It would not be difficult to apply this fpeculation to the figures 1 and 2 before us; but as we are more in want of facts than of conjectures, and as it may be hoped that fome of my readers who have the means and the time will purfue this inveftigation, I fhall for the prefent conclude.

SCIENTIFIC

SCIENTIFIC NEWS.

Anatomical Cabinet.

THERE has appeared at Berlin, a complete defcription of the anatomical cabinet of M. Walter, which the king has purchafed, almoft a year ago, for the fum of 400,000 francs. This catalogue is compofed of fixty-two printed fheets. Anatomical cabinet.

Shower of Peas.

Dr. Hiem, of Berlin, has publifhed a note, in which he explains that the peas, which were faid to have fallen from the atmofphere in a fhower at Landfchut, in Silefia, were merely tubercles which are feparated from the roots of feveral plants. Thofe in queftion, according to the Doctor, were afforded by the roots of the aquatic plant *Ranunculus Ficaria*. He pretends that an enormous mafs of thefe tubercules may have been formed in certain cavities, whence they might be carried to a diftance by the whirl or eddy of ftrong wind. He fupports his opinion by the accounts of fhowers of this nature given by the celebrated Klaproth in his Journal of Chemiftry.—The Doctor concludes by remarking, that thefe tubercles contain a farinaceous fubftance equal in goodnefs to that of potatoes, and recommends an at tention to the ficaria for this purpofe. Shower of peas.

Univerfal Language.

THE Celtic academy, in a fitting of laft April, made proof of a new difcovery by one of its members; which gives the power of correfponding, and difcourfing, with men, whofe language is unknown, with expedition, without previous ftudy, any expence, the leaft trouble, or the fmalleft labour of the mind. The proof made at that fitting by twenty-five academicians, on the languages of Europe, afcertained, that by the aid of this invention, a man may travel any where without an interpreter, demand what he wants, difcourfe on whatever fubjects can intereft any fort of travellers, and even exprefs metaphyfical thoughts. It is intended to make this difcovery public at the return of the Emperor. Univerfal language.

The above account has appeared in ſeveral publications of credit, but it is probable the account is exaggerated in ſeveral reſpects.

Turkiſh Edict in Favour of Science.

Turkiſh edict in favour of ſcience.

THE Grand Seignor has conſtituted Prince Morouſi, by a diploma written with his own hand, director general of the hoſpitals of his empire, and inſpector of the ſchools of medicine, mathematics, and *belles lettres,* which his highneſs is engaged in founding with all poſſible diſpatch. This diploma is remarkable for the great praiſes of the ſciences made in it by the Grand Seignor; as they hitherto have been in no great favour with the Mahometans. In rendering juſtice to the ſkill of the Chriſtian phyſicians, who have ſtudied at the univerſities of Halle, of Padua, and of Montpelier, the Grand Seignour remarks with much truth, that theſe phyſicians, when brought into foreign countries, often commit great errors on account of the difference of the temperature of the climates; from whence he concludes that, in order to practiſe medicine well, it is neceſſary to ſtudy in the country where the profeſſion is to be exerciſed.

Coptic Manuſcripts.

Coptic manuſcript.

THE celebrated Daniſh antiquarian, M. Zoega, is daily occupied at Rome in completing his catalogue of Cophtic manuſcripts in the Borgheſe muſeum. He intends afterwards to publiſh a new topography of ancient Rome. It is probable this work will be printed in Germany, becauſe it will require numerous engravings, which no Italian bookſeller would chooſe to go to the expence of. It is, however, not believed that M. Zoega will occupy the profeſſor's chair, which has been granted him at the Univerſity of Kiel, as he is too much accuſtomed to the fine climate of Italy to leave it willingly.

A

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OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1806.

ARTICLE I.

On the Cause of Fairy Rings. In a Letter from Mr. FLORIAN-JOLLY.

To Mr. NICHOLSON.

Assembly-house, Laytonstone, Essex,
January 13.

SIR,

SEEING by the letter of Mr. Gough inserted in the last Number of your Journal, that the cause of fairy-rings is not yet agreed upon among naturalists, I beg leave to submit to their consideration a few facts which I had occasion to remark some years ago, during a summer residence in Hampshire. The phenomenon of fairy-rings

The park of Broadlands, Lord Palmerston's seat, near Rumsey, was divided into three principal inclosures, formed by hurdles only. One of these had been lately mowed; there were cattle grazing in the next; and the other, which had afforded winter and spring fodder to some horses kept at grass, was then left to grow for an autumn crop. *This* last exhibited an immense number of fairy rings, some perfectly circular, some forming irregular curves, and others nothing but small round produced in great numbers in one division of Broadlands park and none in other parts.

round patches: In all of thefe the grafs grew more luxuriant and of a deeper hue: No other fungus was to be found in any of them but the efculent mufhroom. In the part lately mowed, and in that where the cattle were grazing, there was not the leaft appearance of fairy-rings.

Another field abounding with them.

In the courfe of fubfequent perambulations, I obferved in a grafs field fituated on the top of the firft high ground upon the road from Rumfey to Salifbury, appearances nearly fimilar to thofe exhibited in the growing grafs of the park. There had been all fummer, and there were ftill horfes grazing in this field: The fairy-rings were numerous, but the grafs in the rings and patches, inftead of being more luxuriant, was completely dry and blafted, and there grew two or three different fungi, all of them of thofe forts which are reckoned noxious.

They were not produced by electricity,

That the fairy-rings at Broadlands were not the effect of electricity, appears to me beyond all doubt, fince one part only of the park exhibited them, while the reft of the contiguous grounds, divided from that part by nothing more than a row of hurdles, did not fhew any fuch appearance: otherwife it muft be contended, that the electrical phenomena might take place on one fide of the hurdles and never on the other, a predilection truly fingular, and, I fhould think, difficult to be accounted for.

but by the excrement of the horfes.

Another fact which I have repeatedly obferved fince that time, has led me to fufpect that the fairy-rings, their different appearances, and the various fpecies of fungi found in them, might be produced by no more uncommon caufe than the excrements of the horfes.

Argument from the appearances in hot-beds.

The hot-beds made of horfe-dung, which I have had feveral times in my garden, have generally produced in fucceffion the fame fungi which are to be found in the different ftates of the fairy-rings. Whilft the beds are yet new, the fungi are of the fame noxious fpecies as I faw in the dry blafted fairy-rings, but when they grow cooler and more matured, efculent mufhrooms begin to grow naturally, and although no fpawn was ever put in the bed.

I have alfo remarked, that horfe-dung produced in fome feafons an immenfe quantity of mufhrooms, and hardly any in others: This might perhaps be attributed to the different quality of the hay on which the horfes had fed; and this might explain why fairy-rings are to be found in fome paftures rather than in others.

That

That fairy-rings ſhould be produced by the excrements of horſes, may be illuſtrated by a very ſimple fact, which it is in the power of every perſon to obſerve. If you let fall ſome oil upon a marble ſlab, or ſome other liquid upon ſome ſubſtance that will imbibe it, you will ſee it gradually ſpread round in a more or leſs regular form; ſometimes aſſuming the appearance of a patch, and frequently continuing to flow from the center to the circumference, where it accumulates in a much greater proportion than in the inner part of the circle, taking thus the form of a ring. Experiment with oil, to illuſtrate the deductions.

This accumulation of the fluid at the circumference may be eaſily explained. As the fluid expands, the preſſure from the center becomes gradually leſs, till at laſt there is no ſufficient force to overcome the reſiſtance oppoſed by the dry parts of the ſolid ſubſtance which has imbibed it: yet, in conſequence of the firſt impulſe, the fluid will continue to flow from the center through the ſmall channels already opened, and will thus accumulate in greater quantities at the boundaries where its expanſive motion is ſtopped.

The excrements of horſes, diluted by the rains and imbibed in the ſoil, muſt have an effect ſimilar to that juſt deſcribed. This effect muſt, beſides, greatly depend upon the nature of the ſoil and the facility with which it is pervaded by the fluid; hence the conſtant appearance of fairy-rings in ſome paſture-grounds, while none are ever to be found in others. Application.

Should you, Sir, conſider theſe remarks, and the deductions which they have ſuggeſted to me, as likely to throw ſome light upon the cauſe of fairy-rings, you are welcome to make any uſe of them you may think proper.

I am, Sir,

Your obedient humble ſervant,

J. FLORIAN-JOLLY.

II.

Experiments on the Magnetiſm of ſlender Iron Wires. By JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR, *Middleſhaw, January* 9, 1806.

A general maxim in magnetiſm ſtated.

THE general phenomena of magnetiſm have given riſe to a maxim which ſhall be here ſtated in the words of a judicious writer on the ſubject. "The magnetiſm acquired by being placed within the influence or the ſphere of activity of a magnet in ſoft iron, laſts only while the iron continues in that ſituation; and when removed from the vicinity of the magnet, its magnetiſm vaniſhes immediately; but with hard iron, and eſpecially with ſteel, the caſe is quite different; for the harder the iron or ſteel is, the more permanent is the magnetiſm, which it acquires from the influence of a magnet." Cavallo on Magnetiſm, London, 1787, p. 30.

Remarks on this maxim.

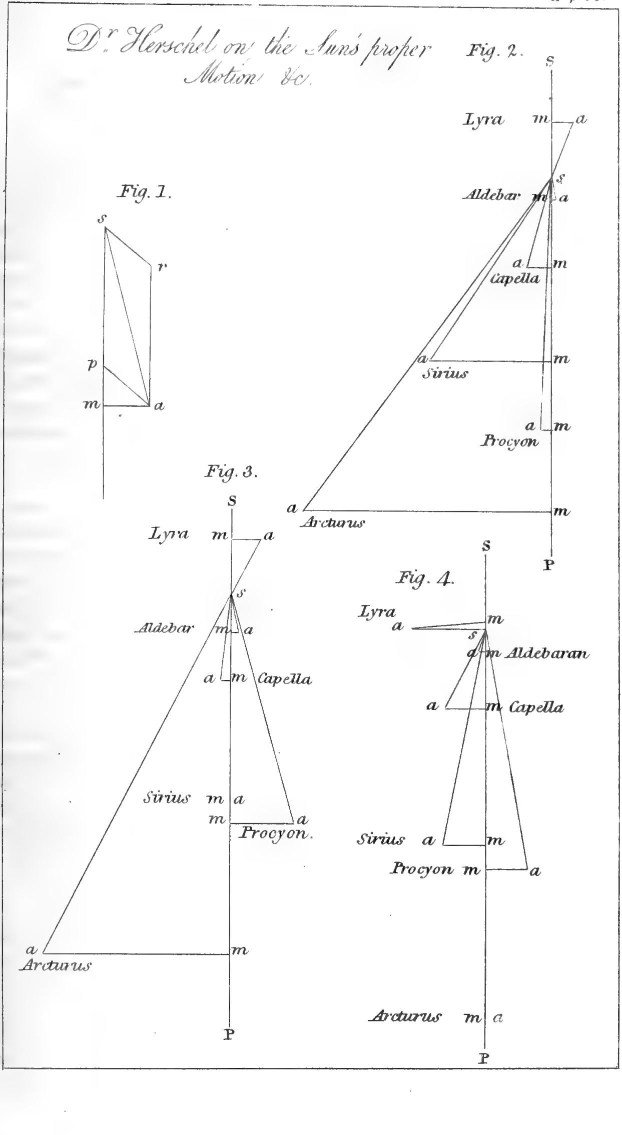
This propoſition is of great utility in the ſcience, for it explains a variety of relations betwixt the magnet and ferruginous bodies, but I have obſerved one phenomenon that appears inexplicable on the principle, and conſequently may be ſaid to offer one exception to the general propoſition. As my experiments on the ſubject are very eaſy, it ſeems adviſeable to deliver the leading circumſtances in the form of ſo many precepts becauſe this method will aſſiſt any one deſirous of purſuing the enquiry, to repeat them with eaſe.

An experiment conſiſtent with he maxim.

Experiment 1. Apply either pole of a ſtrong magnet to one end of a ſhort horizontal bar of clean ſoft iron, and a particle of iron equally ſoft to the other end. This particle will remain ſuſpended at the extremity of the bar until the magnet is withdrawn; but the removal of this power will diſſolve the connection ſubſiſting betwixt the two pieces of iron, and the particle will drop off immediately.

An experiment contradicting the maxim.

Exp. 2. The preceding experiment confirms the maxim ſtated above, when conducted according to the foregoing directions; but let it be repeated with the following alteration, and it will contradict the general propoſition. In place of the particle of ſoft iron, ſubſtitute a piece of iron wire of number 32 in the wire drawers ſcale, the weight of which may amount to two or three grains. The removal of the magnet will


Dr. Herschel on the Sun's proper Motion &c.
Fig. 1.
s
r
p
m
a
Fig. 2.
S
Lyra m a
s
Aldebar. m a
a m
Capella
a
Sirius
m
a m
Procyon
a
Arcturus
m
P
Fig. 3.
S
Lyra m a
s
Aldebar m a
a m Capella
Sirius m a
m a
Procyon.
a
Arcturus
m
P
Fig. 4.
S
Lyra
a m
s
a m Aldebaran
a m Capella
Sirius a m
Procyon m a
Arcturus m a
P

will not break the connection formed by its presence between the bar of soft iron and the wire; for the latter will remain attached to the end of the former, by the extremity which was first brought into contact with the iron; if the piece of wire be removed from the end of the bar, the magnetic connection may be revived by replacing it immediately. The same thing will happen if the wire be expeditiously transferred from the first bar to another rod of soft iron; but it loses its magnetism in the space of two or three seconds when kept at a distance from all ferruginous bodies which are capable of attracting it, and of being attracted by it. These facts prove wire of number 32 to be a magnet, the virtue of which is conditional, because its permanency depends on the presence of soft iron, and perhaps on no other circumstance; for the experiment may be repeated with success upon rusty wire of the same size, or on pieces which have been made red hot in the flame of a candle, or surrounded by sand in a crucible, in which situation they will cool much more gradually than when drawn singly through a flame.

Wires of low sizes not conditional magnet.

Exp. 3. This capacity of iron wire to preserve the magnetism imparted to it, as long as it remains in contact with a bar of the same metal, is a property confined to certain sizes; for let the first experiment be repeated with a small piece of numbers 18 or 17, not equal to half a grain in weight, and just as it comes from the hand of the workman, this piece will perform the part of a particle of soft hammered iron, that is it will drop from the end of the bar, to which it has been attached by the application of a magnet, to the opposite extremity, as soon as the magnetic influence ceases to act upon it: consequently the mere operation of drawing soft iron into wire, by forcing it through a conical hole too narrow for its present diameter, will not convert it into a conditional magnet.

The lowest size capable of conditional magnetism ascertained.

Amongst other experiments relating to the subject, I took the trouble to examine the quality of every size from 32 to 21, both inclusive; the 11 smallest wires, the extreams of which were 32 and 22, were all conditional magnets; that is, they all adhered to the bar of soft iron, to which they had been previously attached, after the removal of the magnet. Number 23 supported seven grains of lead including its own weight, without the assistance of the magnet; No. 24, $6\frac{1}{4}$ nearly; No. 32, $4\frac{1}{2}$; No. 22, no more than two grains.

As

As for number 21, it poffeffed the fimple properties of foft iron: for the fhorteft cylinder which could be taken from a rod of this fize by means of a cutting file, dropped from the end of the horizontal bar as foon as the magnet was withdrawn.

Remarks on Exp. 3d.

It is difficult to fay, which of the 11 wires mentioned above, had the magnetic virtue in the moft perfection, becaufe each piece differed in diameter from the reft; befides which, it is very well known, that a mafs of iron, of a weight and figure determinable by experiment only, is attracted by any particular magnet, more powerfully than any other mafs of the fame metal. But the preceding trials have difcovered one circumftance apparently of fome importance, for they fhew that wire is converted into a conditional magnet by its paffage through the 22 wordle, or wire drawers inftrument; and that the 23d operation brings this quality in it to perfection as far as we can judge from experiment.

I here only fpeak of wire drawn in Kendal, for I have been told, that the fame article manufactured in fome parts of Yorkfhire, has a much greater propenfity to become magnetical. This information was communicated to me by Mr. Morrice, a very intelligent fuperintendant of a manufacture of cards in this town; who moreover obferved, that wire of this defcription acquires a degree of magnetifm under the fhears, which induced him, when employed in working it, to fubftitute a brafs gauge for the common inftrument made of iron.

Conjectures relative to the caufe.

The magnetic property which commences with number 22, feems to be common to all the finer fizes, for I found it in the fmalleft wire I could procure, and which apparently did not exceed a ftrong human hair in thicknefs.

The foregoing experiments, befides proving that flender wires acquire a magnetifm which is permanent as long as they remain in contact with iron, alfo affords an exception to a fecond general maxim of the fcience, which afferts, that the permanency of communicated magnetifm depends on the hardnefs of the ferruginous body that receives it. This does not appear to be the cafe in experiment 2, in which wire of No. 32 did not lofe the faculty of being convertible into a conditional magnet after undergoing a red heat, a procefs that is well known to render wire very foft. I even repeated the experiment with the fame refult on all fizes betwixt 22

and

and 33, except 26; pieces of each ſort were heated both in the flame of a candle, and in ſand; all of which retained the faculty under conſideration after being treated in both ways. In reality, wires that had been thus ſoftened, ſeemed to be in the ſame condition with ſmall nails of caſt iron, conſidered as retainers of magnetiſm, though the latter are of a much harder quality; for a nail of the ſort called ſparrow-bills by ſhoe-makers exhibited the appearances deſcribed in the ſecond experiment, after being filed down to the thickneſs of a ſmall wire.

If then that kind of magnetiſm which I have ventured to call conditional do not depend on comparative hardneſs, to what cauſe is the phenomenon to be deſcribed? little can be offered on my part, beſides probable conjecture, in anſwer to this queſtion. The temperature of wire is conſiderably raiſed during its paſſage through the wordle; and may not we imagine with ſome ſhew of reaſon, that this encreaſe of temperature, joined to the ſubſequent contact of cold air, produces a new arrangement of the molecules conſtituting the wire which enables it to retain a portion of magnetiſm as long as it remains in contact with a ferruginous body? if this ſuppoſition be true, experiment proves the new arrangement to take place in the 22 wordle; when the ſlenderneſs of the wire will occaſion it to cool ſuddenly after paſſing through the inſtrument. The reality of ſuch changes in the texture of bodies which are not in a ſtate of fuſion, is admitted at preſent by experimental philoſophers. I may alſo quote in favour of this hypotheſis ſome valuable obſervations made by Gregory Watt, Eſq, on the various degrees of magnetiſm exhibited by the ſame bazaltic ſtone under different forms of cryſtallization; which obſervations may be ſeen in your Journal for February, 1805.

Any attempt to explain the permanent magnetiſm of ſmall wires during their connection with ſoft iron, and the loſs of this property which enſues when the connection is broken, appears to be ſuperfluous, becauſe the fact is evidently analogous to the well known method of adding ſtrength to a magnet by a gradual encreaſe of its load; for this operation, when judiciouſly conducted, gives a magnetic charge to a bar of ſteel already touched, which it cannot retain after the weight is removed.

I remain, &c.

JOHN GOUGH.

P. S. I neg-

P. S. I neglected to mention the following circumſtance in the body of the letter. The drawing inſtrument, or wordle, is made of ſteel; and is it not probable that this tool, poſſeſſing a ſlight degree of magnetiſm given to it by friction or otherwiſe, aſſiſts in producing the neceſſary arrangement, by acting upon heated and ſlender wires, while their molecules are in a violent motion from the preſſure of the inſtrument itſelf? This ſuppoſition has ſome claim to plauſibility; becauſe a weak magnet will impart a portion of the ſame virtue to a bar of tempered ſteel, the particles of which are in a ſtate of vibration; for a rod of this metal will acquire a degree of polarity, provided it be ſtruck on the end with a hammer when its axis lies parallel to the dipping-needle.

III.

Concerning the Differences in the magnetic Needle, on Board the Inveſtigator, ariſing from an Alteration in the Direction of the Ship's Head. By MATTHEW FLINDERS, *Eſq. Commander of his Majeſty's Ship Inveſtigator. From the Philoſophical Tranſactions,* 1805.

The magnetic needle is affected at ſea by the poſition of the ſhip's head:

WHILST ſurveying along the ſouth coaſt of New Holland, in 1801 and 1802, I obſerved a conſiderable difference in the direction of the magnetic needle, when there was no other apparent cauſe for it than that of the ſhip's head being in a different direction. This occaſioned much perplexity in laying down the bearings, and in allowing a proper variation upon them, and put me under the neceſſity of endeavouring to find out ſome method of correcting or allowing for theſe differences; for unleſs this could be done, many errors muſt unavoidably get admiſſion into the chart. I firſt removed two guns into the hold, which had ſtood near the compaſſes, and afterwards fixed the ſurveying compaſs exactly a-midſhips upon the binnacle, for at firſt it was occaſionally ſhifted to the weather ſide as the ſhip went about; but neither of theſe two arrangements produced any material effect in remedying the diſagreements.

ſcarcely from the iron on deck.

The following table contains the obſervations for the variation of the compaſs in which the differences are moſt remarkable, and from which I ſhall beg to point out ſuch inferences as I think may be drawn from them.

TABLE,

TABLE, ſhewing the Errors produced in the Magnetic Compaſs by the proper Magnetiſm of the Ship.

Time.	Latitude.	Longitude.	Number of compaſſes uſed.	Number of ſets of obſervations taken.	Place of the compaſs.	Suppoſed true variation.	Obſerved variation.	Ship's head.	Obſerver.
1801.									
Dec. AM	35° 5′ S	116° 28′ E	two	4 azimuths	binnacle	7° 0′ W	5° 59′ W	NW b. N	Commander
	Princeſs Royal Harbour		three	6 —	on ſhore	6 15	6 23	—	
	—	—	theodolite	1 —	—	—	6 8	—	
1802.									
Jan. 9, PM	34 1	121 20	one	2 —	binnacle	5 0	9 22	ESE	
16, AM	Gooſe Iſland Bay		one	2 —	—	4 0	0 54	W	
1803.									
May 20, AM	—	—	—	2 —	—	—	6 8	E	
1802.									
Jan. 18, PM	33 37	124 10	—	2 —	—	4 30	5 44	NNE	
20, PM	32 38	125 35	—	2 —	—	—	7 15	E b. N	
21, PM	32 30	125 48	—	2 —	—	—	4 45	S	
22, PM	32 30	126 7	—	2 —	—	4 15	6 13	NE b. E	
23, PM	32 21	126 33	—	1 amplitude	—	4 0	4 18	S b. E	
24, AM	32 5	128 15	—	2 azimuths	—	3 0	6 4	E b. N	
26, PM	32 15	128 15	three	6 —	—	—	3 7	S b. E	Lt. Flinders
30, AM	32 18	132 29	one	2 —	—	0 30	1 41	SSE	Commander
Feb. 4, AM	No. 4, bay in iſland e		—	2 —	—	0 15	2 23	Eaſterly	
5, AM	32 39	133 55	—	1 amplitude	—	—	1 56	E b. S	
6, AM	32 36	133 58	three	6 azimuths	—	—	1 0 E	NW	Lt. Flinders
16, PM	34 3	135 20	one	2 —	—	1 5 E	1 33 W	SE b. E	Commander
—, PM	34 5	135 24	—	1 amplitude	—	1 5	3 56 E	SW	

TABLE

TABLE, shewing the Errors produced in the Magnetic Compass by the proper Magnetism of the Ship.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.									
Feb. 18, AM	34° 50′ S	135° 32′ E	three	6 azimuths	binnacle	1° 12′ E	1° 12′E	S	Lt. Flinders
Mar. 1, PM	In No. 10, bay		theodolite	1 —	on shore	1 39	1 39	—	Commander
5, PM	—	—	one	2 —	binnacle	1 39	0 53	S b. E	
17, PM	34 12	137 20	—	1 amplitude	—	2 15	4 38	SW b. S	Commander
18, PM	34 23	137 36	one	2 azimuths	binnacle	2 15	0 35	SE	
21, AM	35 33	137 15	three	6 —	—	2 40 E	1 10	SE b. S	
23, AM	Kanguroo Island		two	4 —	—	2 58	6 31	SSW	
26, AM	35 10	137 41	one	1 amplitude	—	2 45	1 49	NE b. N	
27, AM	35 21	137 52	one	1 —	—	2 50	1 49	SSE	
April 6, AM	Kanguroo Island		—	2 azimuths	on shore	2 58	2 58	—	
10, AM	35 47	139 15	—	1 amplitude	binnacle	3 0	5 11	W b. S	
11, PM	35 53	139 26	—	2 azimuths	—	3 0	0 50	SE	
13, PM	36 45	140 5	—	2 —	—	3 30	1 25	SE b. S	
16, AM	37 55	139 55	—	{2 — 1 amplitude}	—	4 15	2 20	—	
17, PM	37 57	139 56	—	2 azimuths	—	4 15	2 2	NE	
22, AM	39 38	144 50	—	2 —	—	7 45	11 52	WSW	
26, PM	38 35	144 25	—	2 —	—	7 30	3 41	NE b. E	
—, AM	38 38	144 35	—	1 amplitude	—	7 30	6 48	NE b. N	

Note. All the compasses made use of on board the Investigator were of Walker's construction, one excepted, which was made by Adams, and used only on July 22, 1801.

It

It is apparent that ſome of the obſerved variations in the above table are 4° leſs and others 4° greater than the truth; and it may be remarked, that when this error is weſtward, the ſhip's head was eaſt, or nearly ſo, and when it was eaſtward the head was in the oppoſite direction. When the obſervations agree neareſt with what was taken on ſhore, or with what may be deemed the true variations the ſhip's head was nearly north or ſouth; and a minute inſpection of the table will favour the opinion, that the exceſs or diminution of the variation was generally in proportion as the ſhip's head inclined on either ſide from the magnetic meridian.

The errors were about 4° each way, and the north end of the needle deviated as if repelled by the ſhip's head, &c.

After I had well aſcertained the certainty of a difference in the compaſſes, ariſing from an alteration in the point ſteered, I judged it neceſſary, when I wanted a ſet of bearings from a point where we tacked the ſhip, to take one ſet juſt before and another immediately after that operation: ſome ſpecimens of theſe here follow.

Other obſervations.

1802.		*Head* ESE.		*Head* SW b. W.
April 13th,	Le Geographe Rocks -	N 55° to 71° E		
$11^h\ 32'$ AM	≡ point -	N 4 W	after tacking	N 9° W
—	Π point -	S 32 E	-	S 40 E.

		Head SE b. E.		*Head* W.
April 14th,	Π point rocky, inner part	N 39° E.	after tacking	N 30° E
$9^h\ 29'$ AM	——— projecting part	N 67 E	-	N 59 E
—	Furtheſt viſible extreme from deck -	S 51 E	-	S 55 E.

		Head ENE.		*Head* SW b. S.
April 15th,	Π, the weſtern part -	N 15° W.	after tacking	N 21° W
$11^h\ 50'$ AM	A peaked hummock -	N 19 E	-	N 15 E
—	Furtheſt extreme from deck	S 53 E	-	S 61 E
—	Centre of a naked ſandy patch -	E	-	E 5 N.

Variation per amplitude April 15, AM, taken with the ſurveying compaſs } 4° 8′ E, ſhip's head being S.

April

		Head E.	*Head* SW b. S.
April 15th, 5h PM	The peaked hummock	N 12° W.	after tacking N 18° W
	Former extreme, a projection	S 59 E	- S 64 E
—	Naked sandy patch, distant $3\frac{1}{2}$ -	N 33 E	- N 31 E.

Limits of error in observing bearings on ship-board.

From some little change of place after tacking the ship, and from the part whose bearing was set not being perhaps the individual spot in both instances, the difference between the separate bearings in any set will not be always the same: to these causes for error also may be added inacurracies in taking the angles arising from the motion of the ship and compass, from the view of the object being obstructed by the rigging, masts, or ship's upper works, and from too much haste to get the bearings before the ship's place was materially altered. Even in the Table of azimuths and amplitudes greater accuracy than one degree must not be looked for; and in ship-bearings two or even three degrees is not, I believe, too great an allowance for error, unless in very favourable circumstances.

They may amount to two or even three degrees.

Results similar to those first stated.

Without attending to small differences, it is evident that the bearings correspond with the observation in requiring a less east variation to be applied when the ship's head was easterly, and a greater when it was to the westward, in order to get at the true direction of the object *. When examining the north and

* As a specimen of the plan I followed in protracting such bearings as the above, take the set of April 15, A M, when the true variation appears to have been 4° E. On the first bearing the ship's head was six points on one side of the meridian, and on the second it was three points on the other side, the mean is one point and an half on the east side; now for this one point and an half I allow 1° of error, which, as it is on the east side of the meridian, and the variation is easterly, must be subtracted: the variation then to be allowed upon the mean between the bearings before and after tacking will be 3° E, from which the true bearings will stand as follows:

April

and eaft coafts of New Holland, I always endeavoured to take the angles on fhore with a Troughton's portable theodolite, and to obferve for the variation in the fame places, that all the errors might be done away or corrected; and as I was frequently fortunate enough to carry on my furveys in this manner for weeks together, inftances that might corroborate or contradict the preceding remarks are neither very numerous or pointed; the following are the moft remarkable:

April 15th, AM 11h 50′	Π weftern part - -	N 15° E
	A peaked hummock - - -	N 20 E
—	Furtheft extreme from deck -	S 54 E
—	Centre of a naked fandy patch -	E 0½ S.

In the fame manner upon fingle fets of bearings I was obliged to allow a variation different from what I fuppofed the true to be, unlefs the fhip's head was nearly north or fouth: but, that I might proceed as little upon conjecture as poffible, I always endeavoured to get obfervations for the variation when the fhip's head was in the fame direction as when I had taken or wifhed to take a particular fet of bearings, and I then allowed that variation exactly, whatever it was. The perplexity arifing from difagreements in bearings was by thefe means much alleviated, and happy agreements were frequently produced, when, without fuch corrections, there was nothing but difcord.

TABLE

TABLE of observed Variations of the Compass, and of the Influence of the Ship's Position upon them.

Time.	Latitude.	Longitude.	Number of compasses used.	Number of sets of observations taken.	Place of the compass.	Supposed true variation.	Observed variation.	Ship's head.	Observer.
1802.									
Aug. 5, PM	23° 51′ S	151° 42′ E	one	1 amplitude	binnacle	8° 0′ E	12° 7′ E	WSW	Commander
— AM	23 51	151 40	—	—	—	—	10 15	WNW	
12, PM	23 30	151 11	three	6 azimuths	—	—	6 50	SSE	Lt. Flinders
18, PM	23 23	151 16	one	2 —	—	7 45	7 52	W	Commander
31	22 23	150 38	two	4 —	—	7 30	4 49	E	
Sept. 6, AM	Upon Pier Head		theodolite	1 —	on shore	8 0	8 2	—	
Oct. 14, PM	20 44	150 42	one	1 amplitude	binnacle	7 0	6 40	SSE	Lt. Flinders
20, PM	19 22	148 40	—	1 —	—	6 0	5 39	S	Commander
21, AM	18 15	148 38	three	6 azimuths	—	—	5 42	N b. E	Lt. Flinders
Nov. 2, PM	10 30	142 32	one	2 —	—	4 0	3 32	E	Commander
7, AM	12 11	142 0	—	2 —	—	—	4 4	S	Lt. Flinders
9, PM	12 37	142 2	—	1 amplitude	—	—	5 24	W	Commander
1803.									
Jan. 3, PM	14 20	136 16	—	1 —	—	2 30	0 58	E	
7, PM	14 20	136 37	—	1 —	—	—	1 9	SE	
13, {PM AM}	13 38	137 20	—	2 —	—	3 0	3 47	Westerly	Lt. Flinders
14, AM	13 35	136 58	—	1 —	—	—	5 51	WSW	Commander
16, PM	In NW Bay (Gr. Eyl.)		theodolite	1 azimuth	on shore	—	3 6	—	
Feb. 3, AM	Arnhem S Bay		three	6 —	binnacle	2 20	2 26	NW b. W	
9, AM	—	—	theodolite	1 —	on shore	—	2 20	—	
Mar. 10, AM	11 5	134 15	one	2 —	binnacle	1 0	1 55	WNW	

In

In the latter of these observations, the differences arising from a change in the direction of the ship's head is less considerable than in the higher latitudes; indeed, on approaching the line of no variation upon the south coast, the differences in the variation were smaller than before and afterwards; but that these differences shall be greater in a large variation and smaller in a less, both places being equally distant from the magnetic pole, I will not venture to assert. The inferences that I think may be safely drawn from the above observations are as follows: 1st. That there was a difference in the direction of the magnetic needle on board the Investigator when the ship's head pointed to the east, and when it was directed westward. 2d. That this difference was easterly when the ship's head was pointed to the west, and westerly when it was east. 3d. That when the ship's head was north or south the needle took the same direction or nearly so that it would on shore; and shewed a variation from the true meridian, which was nearly the medium between what it showed when east and when west. 4th. That the error in variation was nearly proportionate to the number of points which the ship's head was from the north or south. Constant employment upon practice has not allowed me to become much acquainted with theories, but the little information I have upon the subject of magnetism has led me to form some notion concerning the cause of these differences, and although most probably vague and unscientific, I trust for the candour of the learned in submitting it, as well as the inferences above drawn, to their judgment.

General inferences. 1. The compass was found to deviate accordingly as the ship's head was easterly or westerly; the north end being farther from the ship's head than the truth. The deviation was proportional to the distance of the head from the N. or S.

1st. I suppose the attractive power of the different bodies in a ship, which are capable of affecting the compass, to be collected into something like a focal point or center of gravity, and that this point is nearly in the center of the ship where the shot are deposited, for here the greatest quantity of iron is collected together.

Theory proposed. That all the iron in the ship acts like one magnet,

2d. I suppose this point to be endued with the same kind of attraction as the pole of the hemisphere where the ship is; consequently, in New Holland the south end of the needle would be attracted by it and the north end repelled.

having a different polarity accordingly as the ship is near the N. or S. pole of the terrestrial magnetism.

3d. That the attractive power of this point is sufficiently strong in a ship of war to interfere with the action of the

 magnetic

magnetic poles upon a compafs placed upon or in the binnacle.

If thefe fuppofitions are confiftent with the laws of magnetifm, eftablifhed by experiments, I judge that they will account for all the differences above noticed; for the interference will neceffarily be moft perceptible upon a compafs when the attractive point is at right angles to the magnetic meridian, that is, when the fhip's head is eaft or weft, and will altogether vanifh or become imperceptible when the attractive point and meridian coincide, or when the fhip's head is north or fouth. That the power of this point fhould become lefs as the fhip increafes her diftance from the magnetic pole has not indeed entered into my fuppofitions; but it may probably be true, and is indeed almoft a neceffary confequence of the fecond fuppofition. If the above hypothefis, fo to call it, be true, it muft follow, that the differences in the variation of the magnetic needle, arifing from a change in the fhip's head, ought to be directly contrary to thofe before recited, when the fhip is on the north fide of the magnetic equator, for the north point of the needle fhould then be attracted, and the fouth end repelled. I have no obfervations which are very decifive upon this head, but thofe that were taken on board the Inveftigator feem to befpeak that as it is fo; they are as follow.

Inferences from the laft fuppofition:

that the effects fhould have a contrary direction in north latitude.

TABLE

TABLE of Obſervations to illuſtrate the foregoing Inferences.

Time.	Latitude.	Longitude.	Number of compaſſes uſed.	Number of ſets of obſervations taken.	Place of the compaſs.	Suppoſed true variation.	Obſerved variation.	Ship's head.	Obſerver.
1801. July 21, PM	Start Point in ſight to the NE		two	5 azimuths	binnacle	—	29° 34′ W	W	Mr. Thiſtle
—			one	1 amplitude	—	—	29 30	—	——
22, PM	49° 10′ N	5° 25′ W	two	4 azimuths	upon the booms in the middle of the ſhip	—	24 12	WNW	——
—, AM	48 15	6 45	one	1 amplitude		—	24 49	WSW	——
28, PM	38 1	14 20	five	10 azimuths		—	20 57	SW	——
—	—	—	—	11 —	binnacle	—	25 34	—	Commander
31, PM	Porto Santo in ſight to the NW		two	4 —	—	—	22 45	—	Mr. Thiſtle
—			—	4 —	booms	—	19 51	—	——
Aug. 24, AM	10 20	22 15	one	2 —	binnacle	—	12 45	SE b. S	Commander
29, AM	5 40	16 30	two	4 —	—	—	12 18	—	Lt. Flinders
Sept. 5, AM	2 15	14 00	—	3 —	—	—	14 54	WSW	Mr. Thiſtle

These obfervations, particularly thofe of July 28, feem to be decifive in fhowing that the variation is more wefterly when taken upon the binnacle of a fhip whofe head is weftward in north latitude, than when obferved in the center of the fhip, which is a ftrong confirmation of the fuppofitions before given; but the obfervations on the change of the fhip's head are too few to be fatisfactory. Almoft every fea officer can tell whether he has obferved the variation of the compafs to be greater when going down the Englifh Channel than when coming up it: and indeed it would be very eafy for a fhip lying in harbour to afcertain the point beyond controverfy.

Remarks and obfervations on the fame law.

Should this point be well eftablifhed, I think it would follow, that from a high fouth latitude where the differences are great on one fide, they are moft likely to decreafe gradually to the equator, and to increafe in the fame way to a high north latitude, where they are great on the other fide; thus the fmaller differences on the north coaft of New Holland will be accounted for. I fhall leave it to the learned on the fubject of magnetifm to compare the obfervations here given with thofe made by others in different parts of the earth, and to form from them an hypothefis that may embrace the whole of the phenomena: the opinion I have ventured to offer is merely the vague conjecture of one who does not profefs to underftand the fubject.

Inftances of the compafs being affected by local magnetifm.

Some account of the magnetifm of Pier Head, upon the eaft coaft of New Holland, may not perhaps be thought an unappropriate conclufion to this Paper. I was induced to attend to this from the following paffage in Hawkesworth, Vol, III. p. 126. "At fun-rife I went afhore," fays Captain Cook, "and climbing a confiderable hill," Pier Head, "I took a view of the coaft and the iflands that lie off it, with their bearings having an azimuth compafs with me for that purpofe; but I obferved that the needle differed very confiderably in its pofition, even to thirty degrees, in fome places more, in others lefs; and once I found it differ from itfelf no lefs than two points in the diftance of fourteen feet *. I took up fome

Pier Head.

Local deviation of the compafs as far as 50°.

* In a fet of angles taken near the head of Arnhem north bay, on the weft fide of the gulph of Carpentaria, I found the needle of the theodolite had been drawn 50° from its proper direction. The fhore confifted of grains of iron ore caked into a ftony mafs; and a piece of it, when applied to the needle, drew it fix or eight degrees

ſome of the looſe ſtones that lay upon the ground, and applied them to the needle, but they produced no effect; and I therefore concluded that there was iron ore in the hills, of which I had remarked other indications, both here and in the neighbouring parts."

The author's obſ. at Pier Head.

On landing at Pier Head I found the ſtones lying on the ſurface to be porphyry, of a dark bluiſh colour; but although I underſtand this ſpecies is uſually found to poſſeſs ſome magnetic power, a piece did not produce any ſenſible effect upon the needle of the theodolite when applied to it. In the following obſervations the theodolite always ſtood about four feet from the ground, that being nearly the length of its legs. I firſt took an extenſive ſet of bearings from the top of the hill, amongſt which were two ſtations whence Pier Head had been before ſet. The firſt, called Extenſive Mount, diſtant 34 miles, differed from its back bearing 4° 35′ to the right, and the ſecond, iſland *a*, diſtant 29½ miles, differed 4° 45′ the ſame way. I now moved the theodolite three yards to the weſtward, and the ſame two objects bore 2° 10′ to the right of their back bearing; on moving it three yards to the ſouth-eaſtward from the firſt place, they differed 2° to the left; and on moving the theodolite four yards to the northward, the ſame two objects bore 1° 10′ to the right of their back bearings. On the following morning I determined to try the magnetiſm more particularly. Taking the thedolite and dipping-needle, I landed upon the ſhore of the Head, whence the top of the hill bore N 50° W, about one-third of a mile. The variation of the

degrees from its direction, but it then ſwung back to its error of 50° where it was ſtationary. In Arnhem ſouth bay a ſmall piece of ſimilar ſtone drew the needle of the theodolite entirely round, yet the bearings taken in this place did not ſhow any diſagreement from the variation and bearings taken in the neighbouring places, where the ſtone did not produce any ſuch effect. In moſt places on ſhore, where I had occaſion to take angles, it was my practice to try the effect of a piece of the ſtone upon the theodolite, in order to detect the preſence of iron ore, as well as on account of my ſurvey. It commonly happened that no effect was apparent, but yet I could not truſt implicitly to the angles, (particularly on the main land,) unleſs obſervations for the variation were taken before the inſtrument was moved, or I had a back bearing of ſome ſtation where ſuch obſervations had been made.

The author's obf. at Pier Head.

theodolite in this place I obferved to be 8° 2′ E, and the inclination of the fouth end of the dipping needle 50° 50′, the needle ftood vertical when the face of the inftrument was S 2° E. I then took the following bearings: Extenfive Mount 108° 30′, the fame exactly as by back bearing. Double Peak 143° 30′; from hence I rowed round the Head, and landed on a rock, whence the top of the hill bore SSW one-fixth of a mile; Extenfive Mount bore 110° 14′, the inclination of the dipping-needle 50° 29′, and the needle ftood vertical when the inftrument faced S 3° E. Thus the difference was $1\frac{3}{4}$° in the horizontal, and $\frac{1}{2}$° in the vertical direction of the needle. Afcending the hill, I made the following obfervations on the top: Extenfive Mount 113° 50′, *a* ifland 133° 52′, Double Peak 148° 32′; the inclination of the needle was 53° 20′, and is ftood vertical at S 3° E. The differences here are 5° 10′ in the horizontal, and 2° 30′ in the vertical direction, from what the needle ftood at in the firft morning's place. On moving ten yards SSE, the bearings were, Extenfive Mount 108° 44′, Double Peak 143° 25′; the inclination was 52° 18′, and the needle was vertical when the inftrument faced S 5° W. In this 4th fet of obfervations, the horizontal direction of the needle is only a few minutes different from the firft place, but the vertical direction is 1° 28′. From the top of the hill I now moved twenty yards to the north-eaftward, when Extenfive Mount bore 110°, Double Peak 144° 42′; the inclination of the dipping needle was now 50° 35′, and it ftood vertical at S 3° W. Thus it appears that the polarity of the magnetic needle is moft interrupted at the top of the hill, both according to the theodolite and dipping-needle. Whether this may arife from fome particular magnetic fubftance lodged in the heart of the hill, or from the attractive powers of all the fubftances which compofe Pier Head being centered in a fimilar point to what I have fuppofed to take place with all the ferruginous bodies lodged within a fhip, I fhall not attempt to decide. The greater differences in the horizontal direction of the needle obferved by Captain Cook, might have arifen from his ufing a common azimuth compafs, which was probably not further elevated from the ground than to be placed on a ftone.

MATTHEW FLINDERS.

Ifle of France,
March 5th, 1804.

Letter

IV.

Letter from Mr. ROBERT HARRUP, *ſhewing that the Smut in Wheat exiſts in the Seed, and is greatly remedied by Lime ſteeping.*

To. Mr. NICHOLSON,

SIR, *Chobham, January* 7, 1806.

LITTLE converſant in agricultural affairs, I am yet to learn what enquiries have been made into the nature and cauſes of the diſeaſes of grain.

If the following communication on the diſeaſe of wheat, known by the name of *ſmut,* contains any thing new, or may lead to farther inveſtigation; an early inſertion of it will greatly oblige,

Sir,

Your obedient humble Servant,

R. HARRUP.

Different cauſes have been aſſigned for the production of ſmut; ſome ſuppoſing it to ariſe from too great an abundance of water ſhoots, others from intemperate ſeaſons. Cauſes uſually aſſigned for the ſmut.

A writer in a reſpectable publication ſtrenuouſly contends in favour of the latter opinion.

He informs us, that brine, pickling, liming, change of feed, and feed of one year old, and upwards, avail nothing. In cold wet ſummers, ſays he, the ſmut prevails notwithſtanding the uſe of every means which invention hath urged or ingenuity practiced. After a number of obſervations, he continues, "to ſum up the whole of this matter, it ſeems as certain as demonſtration can render it, that the ſmut is not owing to any defect or imperfection in the ſeed, but entirely to ſome corrupt-creating principle in the atmoſphere, in the blowing ſeaſon, which blights and deſtroys the grain in ſome ſhape or other, according to the time it has been blowing, when it is ſtruck with the blight." Intemperate weather: and that attention to the ſeed avails nothing.

On the contrary, it would appear from the accounts of thoſe who have the moſt frequent opportunities of making obſervations, that the primary cauſe of ſmut is in the ſeed. All the farmers I have converſed with on the ſubject, are decidedly Practical men uſually aſcribe it to the ſeed, decidedly of opinion, that ſmut in the ſeed will produce ſmut in the crop, unleſs certain means are uſed to prevent it. With this intention I have ſomewhere ſeen a variety of preparations recommended, in ſome of which arſenic was one of the ingredients. The farmers in this neighbourhood prepare their ſeed wheat in one or other of the following methods.

and uſe preparations.

Steeping in brine:

Formerly the wheat was immerſed about twelve hours in a ſtrong ſolution of common ſalt in water, and afterwards dried by mixing it with a ſufficient quantity of lime newly ſlaked.

wetting,

Of late years, in place of immerſing it, they pour a quantity of the ſaline ſolution over it on the floor, and after mixing the whole well together, dry it with lime as before.

or treatment with lime water only.

Another method which is now pretty generally adopted, is that in which no ſalt is uſed.

A quantity of boiling water is poured upon quick lime, and kept conſtantly ſtirred till the lime is reduced to powder, when it is immediately mixed with the grain. No great accuracy is uſed in aſcertaining the proportions; five or ſix pounds of lime, and three gallons of boiling water are about ſufficient to prepare five buſhels of wheat. In reaſoning *a priori*, one would be apt to ſuppoſe, that the vegetative powers of the grain would be materially injured by this boiling compoſition, but experience proves the contrary.

Experiment. Equal meaſures of ſound and ſmutty wheat were mixed. Half of this was ſteeped in brine for twelve hours, and half left unprepared.

Amidſt this diverſity of opinion on the cauſe of ſmut, I wiſhed to aſcertain the truth, if poſſible, by experiment. Accordingly, ſo early as December 1798, I mixed intimately together equal meaſures of ſound wheat and grains conſiſting entirely of ſmut. The heap was then divided into two equal parts; one of them was put into a ſaturated ſolution of ſalt in water for twelve hours, and then mixed with quick lime. The other part was ſubjected to no preparation whatever. Five or ſix days afterwards they were both planted in drills on a ſouth border, about nine or ten yards apart. Both parcels came up about the ſame time, and while in blade, no difference could be perceived. While the ear was yet inveloped by the blade, I cautiouſly opened ſeveral of both crops, and in ſome of that which had undergone no preparation, a conſiderable difference was obſervable. Some of the embrio grains were opened, and in place of a milky juice, they contained only a ſmall quantity of a whitiſh ſubſtance, in which, by

Both parcels were planted.

by the help of a common magnifier, I could readily diftinguifh from one, to three or four black fpecks in each. When rubbed between the fingers, a faint fmell of fmut was emitted. The ears which were examined, and had this appearance were marked, and afterwards proved to be fmut. When the crops came out in ear, it was eafy to diftinguifh the fmut from the wheat. At the time of blowing no bloffom whatever appeared on the fmut ears, and the weather proving tempeftuous at that time, the bloffom was frequently wafhed off the wheat ears by heavy fhowers, and as often renewed.

The unprepared feed gave an unhealthy product.

Smut eafily diftinguifhed.

Both pieces were cut at the ufual time, and upon a careful examination, *that which had been fubjected to no preparation confifted of nearly two-thirds of fmut ears, the remainder being tolerably good wheat. In that which had been prepared, not a fingle fmut-ball could be found.*

The prepared feed produced found grain, and the unprepared feed fmutty grain.

An accidental occurrence may be mentioned in corroboration of this experiment. Happening to pafs through a fmall field of wheat juft before the commencement of the harveft, I was ftruck with the unufual quantity of fmut in one part of it. On clofe examination, I found that this extraordinary crop of fmut ended abruptly in a line along one of the furrows. The other parts of the field had much the fame appearance of others in the neighbourhood; a few fmut ears fcattered through it. Upon enquiry I found, that the feed with which this field had been fown, running fhort, the piece fo abundant in fmut had been fown in feed which contained a confiderable quantity of fmut, and had undergone no preparation, only fprinkling it with a little flaked lime immediately before fowing.

An accidental occurrence of much fmut confined to a part of a field, which had been fown with unprepared feed.

The difeafe of fmut is entirely confined to the grain. The ftraw and every other part of the plant is found, and arrives at the natural fize. Smut ears are *ftaring*, and of a dirty whitifh colour, inclining to blue, at the time when healthy ears are of a bright yellow. Their odour is fœtid, and not inaptly compared to that of ftale lobfters. Part of an ear is not unfrequently found to contain fmut, while the other parts are filled with found wheat.

Smut affects the grain only.

Defcription of the fmut.

Difeafed grains have more the globular form than thofe of found wheat, which is perhaps the reafon why they are called fmut-balls. The fkin is fhrivelled and of a dirty brownifh

hue

hue, without any perforations which can be diſcovered by a high magnifying power. The whole of their contents, in a recent ſtate, are a blackiſh ſoft ſubſtance with a few ſhining ſpecks, which diſappear when dried.

The duſt of ſmut conſiſts of globules,

When kept ſome time in a dry place, this ſoft ſubſtance is in the form of a fine duſt or powder, of a dark brown colour when ſpread out on glaſs or talc. The microſcope ſhews each of theſe minute particles to be well formed globules, ſomewhat larger than the ſanguineous.

heavier than water:

They are ſpecifically heavier than water with which they readily mix but ſoon ſubſides, ſuffering no change by being kept in that fluid. In the beginning of September laſt, I infuſed ſome of the powder in water in a watch-glaſs. A few hours after I diſcovered by the microſcope, in a drop of the fluid a few animalculæ. Upon examination next day every drop of the liquor contained innumerable animalculæ, generally very minute but ſome a ſize larger. After ſtanding expoſed ſome days, the water evaporated, and an hour or two after the addition of freſh water every part ſwarmed with animalculæ, moving nimbly in all directions. While viewing them in the microſcope they ſuddenly became motionleſs owing to the evaporation of the drop of liquid; on adding a drop of freſh water, they inſtantly revived and began the ſame lively motion. A quantity of ſalt ſufficient to ſaturate the water was then added to the mixture. Upon examination about twenty hours afterwards, I was much ſurprized to find the animalculæ as numerous and lively as before the addition of the ſalt.

in which they produce animalcules,

which are not killed by ſalt;

The watch glaſs with its contents, after ſtanding neglected, on a ſhelf expoſed to the effluvia of a variety of drugs, till the latter end of November, was again filled with water, and placed near a fire, placing at the ſame time by it a ſimilar glaſs, containing ſmut powder and freſh water. They were both frequently examined for ſome days, but without diſcovering any animalculæ. My attention being called off by other avocations they remained unnoticed about eight days. The glaſs which contained the infuſion with ſimple water was quite dry, and only a ſmall quantity of fluid remained in the other. A drop being examined in the microſcope by a ſingle lens of a high magnifying power, was found

to

to fwarm with animalculæ. Both glaffes were now filled with frefh water, and placed under inverted jars. Being examined two days after, each of them fwarmed with lively animalculæ. While viewing them, *a fmall particle of lime water was added to the drop, which proved inftantly fatal,* at leaft all motion ceafed inftantaneoufly, and was not renewed.

Lime water killed the animalcules from fmut.

Among other inferences which may be drawn from the preceding facts and obfervations, are firft, that the caufe of fmut is in the feed, and that fmut produces fmut in the crop. At the fame time it is readily admitted, that certain feafons are more favourable to fmut than others, which can only be confidered as a fecondary caufe. 2. That lime ufed in the manner above mentioned, prevents fmut, if not entirely, at leaft fo far as not to prove injurious.

Inference. Smut is caufed by bad feed, and lime water prevents it.

Is fmut occafioned by animalculæ? Some of the foregoing facts feem ftrongly to favour the idea *.

V.

On the Difcovery of Palladium; with Obfervations on other Subftances found with Platina. By WILLIAM HYDE WOLLASTON, *M. D. Sec. R. S* †.

HAVING fome time fince purified a large quantity of platina by precipitation, I have had an opportunity of obferving various circumftances in the folution of this fingular mineral, that have not been noticed by others, and which, I think, cannot fail to be interefting to this Society.

The principal fubject of the prefent memoir is palladium.

* Mr. Nicholfon will readily perceive that the fubject is not near exhaufted. If future inveftigation fhould prefent any thing worthy of communication on the fubject, fhould Mr. N. deem fuch deferving a place in the Philofophical Journal, he has only to mention it in a marginal line.

Anfwer. The difeafes of corn form a fubject of fuch high importance, whether confidered in an economical or fcientific point of view, that I muft confider it a duty to pay the moft marked attention to whatever may tend to elucidate it.—N.

† Philof. Tranf. 1805, p. 316.

As

As I have already given an account * of one product obtained from that ore, which I confidered as a new metallic fubftance, and denominated Rhodium, I fhall on the prefent occafion confine myfelf principally to thofe proceffes by which I originally detected, and fubfequently obtained another metal, to which I gave the name of *Palladium*, from the planet that had been difcovered nearly at the fame time by Dr. Olbers.

In the courfe of my inquiries I have alfo examined the many impurities that are ufually mixed with the grains of platina, but I fhall not think it neceffary to defcribe minutely fubftances which have already been fully examined by others.

§ I. *Ore of Iridium.*

Ore of iridium, refembles that of platina, but is infoluble in nitro-muriatic acid; grains, harder to the file, not malleable and peculiar in their fracture:

I muft however notice one ore, that I find accompanies the ore of platina, but has paffed unobferved from its great refemblance to the grains of platina, and on that account is fcarcely to be diftinguifhed or feparated from them, excepting by folution of the platina; for the grains of which I fpeak are wholly infoluble in nitiro-muriatic acid. When tried by the file, they are harder than the grains of platina; under the hammer they are not in the leaft degree malleable; and in the fracture they appear to confift of laminæ poffeffing a peculiar luftre; fo that although the greater number of them cannot, as I have before obferved, be diftinguifhed from the grains of platina, the laminated ftructure fometimes occafions an external form by which they may be detected. With a view to be abfolutely certain that there exift grains in a natural ftate, which have not been detached by folution from the fubftance of the grains of platina, I have feparated from the mixed ore as many as enabled me to afcertain their general compofition.

much heavier than the grains of platina;

Their moft remarkable quality is their great fpecific gravity, which I have found to be as much as 19,5, while that of the crude grains of platina has not, in any experiment that I have made, exceeded 17,7. From this circumftance it might naturally be conjectured that they contain a greater quantity of platina than the grains in general; by analyfis, however, they do not appear to me to contain the fmalleft quantity of that metal, but

of which metal they contain none.

* See our Journal, IV. 107.

to

to be an ore consisting entirely of the metals that were found by Mr. Tennant in the black powder which is extricated by solution from the grains of platina, and which he has called Iridium and Osmium. But, since the specific gravity of these grains so much exceeds that of the powder, which by my experiments has appeared to be, at the utmost, 14,2. I have thought it might deserve inquiry whether their chemical composition is in any respect different. For this purpose I have selected a portion of them, and have requested Mr. Tennant to undertake a comparative examination, from whose well known skill in chemical inquiries, as well as peculiar knowledge of the subject, we have every reason to expect a complete analysis of this ore.

§ II. *Hyacinths.*

Very small hyacinths found among the platina grains;

Among those bodies which may be separated from the ore of platina, in consequence of their less specific gravity, by a current of water or of air, there may be discerned a small proportion of red crystals so minute, that 100 of the largest I could collect weighed scarcely $\frac{8}{10}$ of a grain. The quantity which I possess is consequently too small for chemical analysis; but their physical properties are such as correspond in every respect with those of the hyacinth. I was first led to compare them with that stone by their specific gravity, which I conjectured to be considerable from their accompanying other substances, that appear to have been collected together solely by reason of their superior weight.

Like the hyacinth, these crystals lose their colour immediately and entirely when heated; they also agree with it in their hardness, which is barely sufficient to scratch quartz, but is decidedly inferior to that of the topaz.

The principal varieties of their form may be very well understood by description.

Varieties of their forms;

1st. In its most simple state the crystal may be considered as a rectangular prism terminated by a quadrilateral obtuse pyramid, the sides of which sometimes arise direct from the sides of the prism; but,

2dly. The position of the pyramid is generally such that its sides arise from the angles of the prism. In this case the sides of the prism are hexagons.

3dly. It

3dly. It is more ufual for the prifm to have eight fides by truncation of each of its angles, and at each extremity eight additional furfaces occupying the place of the eight linear angles between the prifm and terminating pyramid of the fecond variety. The complete cryftal has then thirty-two fides.

4thly. The eight furfaces laft mentioned, as interpofed between the prifm and pyramid, are fometimes elongated into a complete acute pyramid having eight fides arifing from the angles of an octahedral prifm.

which prove the nature of the ftone.

The third form above defcribed, correfponds fo entirely with that given by the Abbé Haüy * as one of the forms of the hyacinth or jargon, that I have little reafon to regret my inability to obtain chemical evidence of the compofition of thefe cryftals.

Thofe, and other impurities, I ufually feparated, as far as was practicable, by mechanical means, previoufly to forming the folution of platina, which has been the principal object of my attention.

§ III. *Precipitation of Platina.*

Account of the treatment of platina. After the firft precipitation of it by fal ammoniac: another portion was thrown down by iron.

When a confiderable quantity of the ore has been diffolved, and I had obtained, in the form of a yellow triple falt, as much of the platina as could be precipitated by fal ammoniac, clean bars of iron were next immerfed in the folution for the purpofe of precipitating the remainder of the platina.

This is called the firft metallic precipitate, and was again diffolved as the firft ore had been, and again precipitated by fal ammoniac.

For diftinction it will be convenient to call this, which in fact confifts of various metals, the firft metallic precipitate.

The treatment of this precipitate differed in no refpect from that of the original ore. It was diffolved as before, and a portion of platina precipitated by fal ammoniac; but it was obfervable that the precipitate now obtained was not of fo pale a yellow as the preceding. Neverthelefs the impurity was in fo fmall quantity, that the platina reduced from it by heat did not differ difcernibly from that obtained from the pureft yellow precipitate.

* *Traité de Mineralogie*, Pl. XLI. fig. 17. *Journ. des Mines*, No. 26, fig. 9.

At

At this time I found it advantageous to neutralize the ſolution with ſoda, and to employ a ſolution of green ſulphate of iron for the precipitation of the gold, of which, I believe, a portion may always be obtained from the mixed ore; but I have obſerved in experiments upon any quantities of mere grains of crude platina carefully ſelected, that the ſmalleſt portion of gold could not be detected as a conſtituent part of the ore itſelf.

The ſolution was neutralized with ſoda; and gold, if preſent, precipitated by ſolution of green ſulphate of iron.

Bars of iron were ſubſequently employed as before for recovering the platina that remained diſſolved, together with thoſe ſubſtances which I have ſince found to accompany it.

A ſecond metallic precipitate was thrown down by iron.

The precipitate thus obtained, which I diſtinguiſh by the name of the ſecond metallic precipitate, was to appearance of a blacker colour than the former, and was a finer powder.

As I was not at firſt prepared to expect any new bodies, I proceeded to treat the ſecond precipitate, as the former, by ſolution and precipitation. But I ſoon obſerved appearances which I could not explain by ſuppoſition of the preſence of any known bodies, and was led to form conjectures of future diſcoveries, which ſubſequent inquiry has fully confirmed.

When I attempted to diſſolve this ſecond metallic precipitate in nitro-muriatic acid, I was ſurpriſed to find that a part of it reſiſted the action of that ſolvent, notwithſtanding any variations in the relative proportions or ſtrength of the acids employed to form the compound, and although the whole of this powder had certainly been twice completely diſſolved.

This was not all ſoluble in nitro-muriatic acid.

The ſolution formed in this caſe was of a peculiarly dark colour, and when I endeavoured to precipitate the platina from it by ſal ammoniac, the precipitate obtained was ſmall in quantity, and, inſtead of being yellow, was of a deep red colour, ariſing from an impurity which I did not at that time underſtand, but which we ſince know, from the experiments of Mr. Deſcotils, is occaſioned by the metal now called iridium.

This ſolution was very dark and its precipitation by ſal ammoniac was deep red, occaſioned by iridium.

The ſolution, inſtead of being rendered pale by the precipitation of the platina, retained its dark colour in conſequence of the other metals that remained in ſolution; but, as I had not then learned the means of ſeparating them from each other, and as the quantity of fluid which accumulated occaſioned me ſome inconvenience, I decompoſed it by iron, as in

Precipitation of of a third metallic precipitate by iron.

in the former inftances, and formed a third metallic precipitate, which could more commodioufly be referved for fubfequent examination.

Much of this being rhodium, was infoluble.

In this laft ftep I committed an error which afterwards occafioned me confiderable difficulty, for I found that a great part of this precipitate confifting of rhodium was unexpectedly rendered infoluble by this treatment, and refembled the refiduum of the fecond metallic precipitate abovementioned.

As I have already communicated to this fociety, in my Paper upon rhodium, the procefs by which I fubfequently avoided this difficulty, I fhall at prefent return to a previous ftage of my progrefs, and relate the means by which I firft obtained palladium in my attempts to analyze the fecond metallic precipitate.

§ IV. *Separation of Palladium.*

Separation of palladium. The fecond metallic precipitate contained lead, copper, and another metal precipitable by copper.

There was no difficulty in afcertaining the prefence of lead as one of the ingredients of this precipitate, by means of muriatic acid, which diffolved lead and iron and a fmall quantity of copper. It was equally eafy to obtain a larger portion of copper by dilute nitrous acid, with which it formed as ufual a blue folution. But when I endeavoured to extract the whole of the copper by a ftronger acid, it was evident, from the dark brown colour of the folution, that fome other metallic ingredient had alfo been diffolved. I at firft afcribed this colour to iron; but, when I confidered that this fubftance had been more flowly acted upon than copper, I relinquifhed that hypothefis, and endeavouring to precipitate a portion of it by a clean plate of copper, I obtained a black powder adhering to a furface of platina on which I had placed the folution. As this precipitate was foluble in nitric acid, it evidently confifted neither of gold nor platina; as the folution in that acid was of a red colour, the metal could not be either filver or mercury; and as the precipitation of it by copper excluded the fuppofition of all other known metals, I had reafon to fufpect the prefence of fome new body, but was not fully fatisfied of its exiftence until I attempted the precipitation of it by mercury.

It was feparated by agitating mercury with the folution with

For this purpofe I agitated a fmall quantity of mercury in the nitrous folution previoufly warmed, and obferved the mercury to acquire the confiftence of an amalgam. After this

amalgam

amalgam had been expofed to a red heat, there remained a white metal, which could not be fufed before the blowpipe. It gave a red folution as before in nitrous acid; it was not precipitated by fal ammoniac, or by nitre; but by pruffiate of potafh it gave a yellow or orange precipitate; and in the order of its affinities it was precipitated by mercury but not by filver.

which it formed an amalgam; and the mercury was driven of by heat. It was palladium.

Thefe are the properties by which I originally diftinguifhed palladium; and by the affiftance of thefe properties I obtained a fufficient quantity for inveftigating its nature more fully.

There were, however, various reafons which induced me to relinquifh the original procefs of folution in nitrous acid and precipitation by mercury; for although I found the metal thus obtained to be nearly pure, the neceffity of agitating the folution with the mercury was very tedious, and the wafte was alfo confiderable; for in the firft place it feemed that nitrous acid would not extract all the palladium from any quantity of the fecond metallic precipitate, neither would mercury reduce the whole of what was fo diffolved. I therefore fubftituted a procefs dependent on another of its properties, I had obferved that this metal differed from platina in not being precipitated from nitro-muriatic acid by nitre or by other falts containing potafh; for although a triple falt is thus formed, this falt is extremely foluble, while that of platina on the contrary requires a large quantity of water for its folution. On that account a compound *menftruum* confifting of nitrate of potafh diffolved in muriatic acid is unfit for the folution of platina, but diffolves palladium nearly as well as common nitro-muriatic acid in which there is no potafh prefent *.

The procefs with mercury was abandoned;

In five ounces of muriatic acid diluted with an equal quantity of water, I diffolved one ounce of nitre, and formed a folvent for palladium that poffeffes little power of acting on platina, fo that by digefting any quantity of the fecond metallic precipitate till there appeared to be no farther action, I procured a folution from which by due evaporation were formed cryftals of a triple falt, confifting of palladium combined with muriatic acid and potafh. Thefe are the cryftals which I have on a former

and a folvent confifting of muriatic acid, with nitre was ufed to the fecond precipitation from which it takes palladium but not platina.

The folution gave cryftals of triple falt of palladium potafh and muriatic acid.

* I have found that gold may alfo be diffolved with equal facility by the fame folvent, and nearly in the fame proportion. Ten grains of nitre added to a proper quantity of muriatic acid are fufficient for fixteen grains of either gold or palladium.

former occafion * mentioned as exhibiting a very fingular contraft of colours, being bright green when feen tranverfely, but red in the direction of their axis; the general afpect, however, of large cryftals is dark brown.

From the falt thus formed and purified by a fecond cryftallization, the metal may be precipitated nearly pure by iron or by zinc, or it may be rendered fo by fubfequent digeftion in muriatic acid.

§ V. *Reafons for thinking Palladium a fimple Metal.*

That palladium is a fimple metal appears from,

From the confideration of this falt alone I thought it highly probable that the fubftance combined in it with muriate of potafh was a fimple metal, for I know of *no inftance in chemiftry of a diftinctly cryftallized falt containing more than two bafes combined with one acid.* I neverthelefs endeavoured by a fuitable courfe of experiments to obviate all probable objections.

its forming a diftinctly cryftallized falt with bafes and an acid,

After examining by what acids it might be diffolved and by what reagents it might be precipitated, I combined it with various metals, with platina, with gold, with filver, with copper, and with lead; and when I had recovered it from its alloys fo formed, I afcertained that, after every mode of trial it ftill retained its characteriftic properties, being foluble in nitrous acid, and precipitable from thence by mercury, by green fulphate of iron, by muriate of tin, by pruffiate of potafh, by each of the pure alkalis, and hydrofulphurets.

its combinations with metals and feparation without change.

The precipitate obtained in each cafe was alfo found to be reducible by mere heat to a white metal, that, except in very fmall quantities, could not be fufed alone by the blowpipe, but could very readily be fufed with fulphur, with arfenic, or with phofphorus, and in all other refpects refembled the original metal.

and its precipitation is reducible by mere heat.

Qu. Whether it might confift of metal and a fixed acid?

The only hypothefis, on which I thought it poffible that I could be deceived, arofe from the recollection of the error, which fubfifted for a few years, refpecting the compound formerly called fiderite. It was poffible that fome metallic or other fixed acid might unite too intimately with either a known or an unknown metal to be feparated by the more common fimple affinities. I confequently made fuch attempts as appeared beft calculated to difunite a compound fo conftituted.

* Phil. Trans. 1804, p. 428.

Having

Having boiled the oxide with pure alkalis, and found it to be unaltered, I thought the affinities of lime or lead might be more likely to detect the presence of the phosphoric or of any known metallic acid; and accordingly I made various attempts by muriate and nitrate of lime, as well as by nitrate of lead, to effect a decomposition of the supposed compound. In the experiment on which I placed the greatest reliance, I poured liquid muriate of lime into a solution of palladium in nitromuriatic acid, and evaporated the mixture to dryness, intending thereby to expel any excess of acid that might have been left in the solution, and to render either phosphate of lime, or any compound of lime with a metallic acid, insoluble in water. The residuum however was very readily dissolved by water, and consisted merely of muriate of lime aad muriate of palladium, without any appearance of decomposition.

The oxide is not affected by boiling with alkalis; nor by pouring muriate of lime into its solution.

When I found all my endeavours directed to that end wholly unsuccessful, I no longer entertained any doubt of this substance being a new simple metal, and accordingly published a concise delineation of its character; but by not directing the attention of chemists to the substance from which it had been extracted, I reserved to myself an opportunity of examining more at leisure many anomalous phenomena, that had occurred to me in the analysis of platina, which I was at a loss to explain, until I had learned to distinguish those peculiarities, that I afterwards found to arise from the presence of rhodium.

Hence the discoverer was warranted in publishing it as a new metal.

§ VI. *Additional Properties of Palladium.*

In my former Paper on that subject I also added some observations upon the properties and origin of palladium, describing only such a mode of obtaining it from platina as should avoid the introduction of any unnecessary ingredient which might possibly be misinterpreted, and omitted one of the most distinguishing properties of palladium, by means of which it may be obtained with the utmost facility by any one who possesses a sufficient quantity of the ore of platina.

Method of easily separating palladium.

To a solution of crude platina, whether rendered neutral by evaporation of redundant acid, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper, or by iron, and also whether the platina has or has not been precipitated from the solution by sal ammoniac, it is merely necessary to add a solution of prussiate of mercury, for the

Prussiate of mercury added to the solution of crude platina, throws down the pure prussiate of palladium;

the precipitation of the palladium. Generally for a few feconds, and fometimes for a few minutes, there will be no appearance of any precipitate; but in a fhort time the whole folution becomes flightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowifh-white colour. This precipitate confifts wholly of pruffiate of palladium, and when heated will be found to yield that metal in a pure ftate, amounting to about four or five tenths *per cent.* upon the quantity of ore diffolved.

Heat difengages the pure metal which is not more than one two hund. part of the original ore.

The pruffiate of mercury is peculiarly adapted to the precipitation of palladium, exclufive of all other metals, on account of the great affinity of mercury for the pruffic acid, which in this cafe prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium that I have ftated by ufing more of the pruffiate of mercury, or to procure any precipitate by the fame means from a folution of pure platina.

More mercury does not augment the product.

The decompofition of muriate of palladium by pruffiate of mercury is not effected folely by the fuperior affinity of mercury for the muriatic acid, but is affifted alfo by the greater affinity of pruffic acid for palladium; for I have found that pruffiate of palladium may be formed by boiling a precipitated oxide of palladium in a folution of pruffiate of mercury.

The decompofition is by double affinity.

The pruffiate of mercury is confequently a teft by which the prefence of palladium may be detected in any of its folutions; but it may be worth obferving, that the precipitate obtained has not in all cafes the fame properties. In general, this compound is affected by heat fimilarly to other pruffiates, but when the palladium has been diffolved in nitrous acid and precipitated from a neutral folution by pruffiate of mercury, the precipitate thus formed has the property of detonating when heated. The noife is fimilar to that occafioned by firing an equal quantity of gunpowder, and accordingly the explofion is attended with no marks of violence unlefs occafioned by clofe confinement. The heat requifite for this purpofe is barely fufficient to melt bifmuth, confequently is about 500° of Fahrenheit. The light produced is proportionally feeble, and can only be feen in the abfence of all other light.

Pruffiate of mercury is the teft of palladium.

The precipitation from a nitrous folution detonates by low heat.

In

In endeavouring to dissolve a piece of palladium in strong colourless nitric acid for the purpose of forming the detonating prussiate, I found that, although the acid shortly acquired a red colour surrounding the metal, the action of the acid was extremely slow, and I was surprised to observe a fact that appears to me wholly singular: the metal was taken up without any extrication of nitrous gas; and this seemed to be the cause of the slow solution of this metal, as there was not that circulation of this fluid, which takes place in the solution of other metals until the acid is nearly saturated.

Palladium is very slowly acted on by nitric acid and *extricates no gas.*

As the want of production of gas appeared to retard the solution of palladium, I tried the effect of impregnating a quantity of the same acid previously with nitrous gas, and observed its action to be very considerably augmented, although the experiment was necessarily tried in the cold, because the gas would have been expelled by the application of heat.

Nitrous acid acts more strongly.

Beside those properties which are peculiar to palladium there are others, not less remarkable, which it possesses in common with platina. I have on a former occasion mentioned that these metals resemble each other in destroying the colour of a large quantity of gold. Their resemblance, however, in other properties is not less remarkable, more especially in the little power they possess of conducting heat, and in the small degree of expansion to which they are liable when heated.

For the purpose of making a comparison of the conducting power of different metals, I endeavoured to employ them in such a manner, that the same weight of each metal might expose the same extent of surface. With that view I selected pieces of silver, of copper, of palladium, and platina, which had been laminated so thin as to weigh each 10 grains to the square inch. Of these I cut slips $\frac{4}{10}$ of an inch in breadth, and four inches long; and having covered their surfaces with wax, I heated one extremity so as to be visibly red, and, observing the distance to which the wax was melted, I found that upon the silver it had melted as far as $3\frac{1}{4}$ inches: upon the copper $2\frac{1}{2}$ inches: but upon the palladium and upon the platina only one inch each: a difference sufficient to establish the peculiarity of these metals, although the conducting power cannot be said to be simply in proportion to those distances.

Conducting powers of palladium and platina as to heat, tried by the melting of wax upon them. The measure is not half those of silver and copper.

In order to form some estimate of the comparative rate of expansion of these metals, I rivetted together two thin plates of platina

Rate of expansion by heat, tried by rivetting bars together.

While ſteel expands through 12, platina will expand 9, and palladium 10.

platina and of palladium; and obſerving that the compound plate, when heated, became concave on the ſide of the platina, I aſcertained that the expanſion of palladium is in ſome degree the greater of the two. By a ſimilar mode of compariſon I found that palladium expands conſiderably leſs than ſteel by heat; ſo that if the expanſion of platina between the temperatures of freezing and boiling water be eſtimated at 9 parts in 10,000, while that of ſteel is known to be about 12, the expanſion of palladium will probably not be much more or leſs than 10, or one part in 1000 by the ſame difference of temperature.

It muſt, however, be acknowledged, that the method I have purſued is by no means ſufficient for determining the preciſe quantity of expanſion of any ſubſtance; but I have not been induced to beſtow much time on ſuch an inquiry, ſince the extreme ſcarcity of palladium precludes all chance of any practical utility to be derived from a more accurate inveſtigation.

VI.

Report made to the Athénée des Arts of Paris, by MM. RONDELET, BEAUVALLET, *and* DUCHESNE; *on the founding the Statue of* JOAN OF ARC *in Bronze, by a Way never before uſed for large Works, by MM.* ROUSSEAU *and* GENON, *under the Direction of M.* GOIS, *Statuary.**

Caſting in ſand uſed hitherto only for ſmall figures.

THE method of caſting in ſand hitherto has only been uſed for figures from 65 to 70 centimetres (about $2\frac{1}{4}$ feet) in heighth; while the ſtatue, which was to be formed, being of much larger dimenſions, ſhould of courſe be managed according to the method called the *great foundery*, on account of its being uſed for coloſſal ſtatues.

The great foundery known to the ancients, but was loſt.

At its revival large ſtatues were caſt in ſeparate parts.

This method of caſting was known to the ancients, who were even ſuperior to us in it; but this art was loſt with many others, and in the time of the Medicis large ſtatues were not formed at a ſingle caſting. The figures of Henry the Fourth and of Lewis the Thirteenth, which are ſeen at Paris, were

* Magazin Encyclopédique, T. I. p. 350.

placed

placed on horſes made previouſly, one for the ſtatue of Ferdinand, Grand Duke of Tuſcany, and the other for that of Henry the Second, King of France.

The ſtatue of Lewis the Fourteenth, in the Place de Vendome, is the firſt that was formed at a ſingle caſting ſince the revival of the art. It was ſuitable to ſo great a prince to permit his image to be made ſolely by a grand method; but Girarden and Keller, to whom the work was entruſted, then made their firſt attempts, which occaſioned many faults, ſuch as the caſting it too thick, which in uſeleſsly employing more metal, increaſed the difficulty of ſupporting the coloſſal figure; and ſuch alſo as uſing unneceſſary labour; but notwithſtanding all their precautions, the caſting did not ſucceed perfectly, and conſiderable repairs were obliged to be made in it.

That of Lewis XIV. the firſt caſt after this in a ſingle piece.

About the ſame time were erected the equeſtrian ſtatues of this prince, at Boufflers and at Lyons, by the ſame Girarden, at Rennes by Coizevox, at Montpelier by Mazeline and Utrels, and at Dijon by Le Hongre.

Various other ſtatues made in the ſame way.

After this, Le Moine had to found a ſtatue of Lewis the Fifteenth at Bourdeaux, which met with great accidents; but he had more ſucceſs with one at Rennes, which was a pedeſtrian ſtatue: Guibal alſo made one for Nancy. But this art did not attain to a great perfection till Boucharden was employed to conſtruct an equeſtrian ſtatue of Lewis the Fifteenth at Paris: The great care of M. Goor prevented any accident from happening to it, or to that alſo which was caſt at Reims by the ſame artiſt, from the model of Pigale. This founder had not the ſame ſucceſs when he formed the ſtatue of Frederic the Fifth at Copenhagen, from the model of Saly, which required great repairs. Finally, great improvements had been made in the art when the ſtatue of Peter the Great was founded at Peterſburg by Falconet, and neverthelefs he was obliged to refound a ſecond time the upper half of the ſtatue.

The art not very perfect before the ſtatue of Lewis XV. was caſt.

Great improvements made in the art when the ſtatue of Peter the Great was caſt.

The great diſadvantage of the method hitherto uſed, is its enormous expence and the great time it requires. It is true, that for works which are intended for duration economy is not the chief object; but if they can be performed equally well by M. Gois's method, at one-half the expence and in a fourth

Diſadvantages of the great foundery.

M. Gois's method ſuperior to it in various reſpects.

Reafons for believing that it would anfwer for the largeft works.

of the time, it certainly ought to be preferred. There is good reafon alfo to decide, that this method will do equally well for the largeft works; for, according to calculation, the largeft ftatue of this kind in France exceeds that of Joan of Arc by a much fmaller proportion than the latter exceeds that of the largeft ftatues ever before caft in the fame manner, which never weighed more than from 8 to 10 kilogrammes (from about 17 to 22 lb.) The ftatue of Joan of Arc weighed 600 kilogrammes, which is 60 times more; but that of Lewis the Fifteenth, which weighed 17,000 kilogrammes, was only twenty-eight times heavier than that of Joan of Arc.

But in order to judge better of the advantages of M. Gois's method, it fhall be defcribed at large, and an account given alfo of the method of molding by wax, or of the *grand foundery*, in order to compare them together.

M. Gois's ftatue of Joan of Arc exhibited and admired.

M. Gois having made a ftatue of Joan of Arc for a prize, exhibited it in public in the year 10, (1802.) The prefect of the department of the Loire faw it, and propofed to the city of Orleans to re-erect that monument to the glory of this heroine, which had been deftroyed in the anarchy of the revolution. It was accordingly ordered to be done. M. Gois being informed of this, went to Orleans and offered to make a caft in bronze from his ftatue, without precifely knowing whether it was that which the city required.

He is employed to make a caft in bronze from it for the city of Orleans.

An agreement was then made with M. Gois to complete the ftatue at a fixed price, in the courfe of about one year from the 5th Germinal, *An XI.* or before May 4, 1804.

Is induced to have the caft made in fand to fave time and expence.

M. Gois began to be alarmed at the enormous expence of the ufual method of cafting fuch ftatues, and at the great time it required, which he feared would prevent his performing the agreement. He knew that M. Rouffeau had made a caft from the groupe of Graces by Germain Pilon, with great fuccefs, by a different method; and though thefe figures were but 1,38 metres high, and his ftatue was more than two metres, ($6\frac{1}{2}$ feet) he went notwithftanding to confult this founder, who engaged for the fuccefs of the method, and promifed to employ in the bufinefs the fame workman who had caft the above groupe without having met with any accident: This laft confideration determined M. Gois to entruft the work to the *founders in fand.*

M. Rouffeau undertakes the work for him, who had before made a fine caft of the Graces; and employs the fame workman who caft them.

The

The firſt of Fructidor, *An XI.* the buſineſs was began; but as they commenced with the bas-reliefs, it was not till three months after that they undertook the work of the ſtatue.

They made uſe of the common ſand of the founders, which is argillaceous, and always kept a little moiſt. After having well raked it, ſeparated all the ſtones, and broken all the lumps that could be met with, they filled with it a caſe of 2,20 metres long, and one metre broad at the inſide, and 16 centimetres high; the thickneſs of the wood of the caſe was eight centimetres. The ſand was ſtrongly beaten with a rammer 10 centimetres broad and 60 long, and by this operation acquired ſufficient conſiſtence to be raiſed along with the caſe without any danger of running out.

The proceſs deſcribed for caſting Joan of Arc in ſand.

After this the ſtatue was placed upon the firſt caſe, which is called the *falſe mold,* becauſe it was to be afterwards replaced by another: the ſand was ſtirred up a little, to permit the moſt prominent parts to enter it; another caſe of the ſame ſize was then put over the firſt, and attached to it by four points of iron.

The true *concave mold* was then began, by modelling each part of the figure with the ſame kind of ſand. A workman of much addreſs and intelligence is requiſite for the diviſion and diſtribution of the different pieces which form the mold: he ſhould explain the motives which induced him to prefer one diſtribution to another: each piece ſhould have different ſections: care ſhould be taken to mold the parts which have a large and uniform ſurface in a ſingle piece, while the pieces muſt be multiplied for thoſe portions of the ſtatue which have many ſinuoſities and deep indentations.

Method of forming the mold.

This part of the operation requires the moſt care; for if it be performed with negligence, the extraction of the model would be attended with great difficulties; and if the workman employed is aukward, numerous faults will need reparation after the caſting, and probably great accidents may happen. It is but juſtice to ſay, that Genon, the workman on this ſtatue, ſhewed in his performance equal dexterity and knowledge.

To prevent the pieces of the mold from adhering to each other, care is taken to powder the parts of each which is finiſhed, with charcoal duſt incloſed in a bag, before a new piece is began. The workman having finiſhed the mold-

ings

ings of the contours of the figure, filled up the empty ſpaces between them and the caſe with ſand, which he firſt preſſed and forced together with his hands, afterwards beat it with the bat, and finally with a mallet; this compreſſion gives it ſuch a ſolidity that it appears like ſtone, or at leaſt like baked earth.

The ſame care was taken with each caſe as they were ſucceſſively added, to the number of ſeven; which compleating the top of the ſtatue, the whole was then reverſed in order to replace the loweſt caſe, which, as mentioned, was only a falſe mold; and then each part at the lower extremity was alſo modelled in ſame manner as the preceding.

The hollow mold being finiſhed, the caſes were taken aſunder, and each piece removed ſeparately to take out the ſtatue; then they were all placed in their proper order in the exterior mold, which may be compared to the cover uſed by thoſe who make plaſter-of-Paris caſts. Each piece would be well retained in its place by its irregular form; but it was ſtill farther faſtened by a little thin paſte made of flour, which was applied by a bruſh both to the pieces themſelves and the parts that adhered to the caſes. It was thought neceſſary to take a precaution more than what was uſual, through the apprehenſion that the paſte would not hold together thoſe large pieces as well as it did the ſmall pieces in leſſer works; they were therefore traverſed by long wires of iron, which entered into the cover or exterior mold.

The pieces of the mold cemented together with paſte, and ſecured by wires paſſing into the caſes.

This mold being thus entirely compleated, had only to be dried till the time of the caſting. A new mold was neceſſary to be made to caſt the core: the ſame pains were not taken with this as with the firſt, as it would be uſeleſs to do ſo. When this ſecond mold was finiſhed, a coat of modelling-clay was applied to its inſide, of the ſame thickneſs which was intended to be given to the bronze; and without waiting for its drying, it was cloſed and the core caſt in it, which was compoſed, as is uſual, of equal parts of plaſter of Paris and of brick duſt.

Method of forming the core.

Eight rods of iron having been placed at the inſide of the mold, afterwards projected from the core about 10 or 12 centimetres, which ſerved to place it with preciſion in the hollow mold.

Eight iron rods laid ſo as to project a few inches from the core, in order to place it in the mold.

2 But

But in the mean time the cafes had been placed one over the other, and the iron pins which connected them fitted to their places, taking care to divide them into two portions, which anfwered to the two cafes ufually employed by the founders; and which, inftead of the ufual thicknefs of five centimetres at moft, were, the one 48 and the other 64 centimetres thick.

The pieces of the mold dried.

In this ftate they were dried, by placing them round a brafier of kindled charcoal, the fire of which had the more power from the mold being divided into two portions, and empty.

The core alfo dried.

The core was likewife dried by placing it over a brafier of charcoal; the fame was alfo placed round it; and in eight hours the moifture was entirely evaporated. It was left to cool, and it was placed in one half of the mold; the fecond half of the mold was afterwards fitted on, and the whole compreffed together by iron preffes in the ufual manner.

The mold and core put together ready for cafting, and fecured by preffes.

After this there only remained to conftruct the bafon, *(l'echino)*, to fufe the metal, and make the caft. Thefe operations being the fame for both methods of founding, fhall be related, after firft as briefly as poffible defcribing the method of cafting by the great foundery in which wax is ufed.

Defcription of cafting by the great foundery.

The firft operation for the great foundery is to dig a trench proportionate to the fize of the figure to be caft, and to furround it with a wall to prevent the earth from tumbling in.

A mold formed from the ftatue in feparate pieces.

After the model is finifhed it is oiled, and a mold formed from it with plafter of Paris in the ufual way, and with the precautions before directed for molding in fand: In each piece of this mold rods of iron are inferted, by which they may be eafily lifted when the mold is taken afunder or put up; each of thefe pieces is numbered, that its proper place may be known.

A compofition of wax laid on to the infide of the mold, of the thicknefs intended for the bronze.

After this feveral layers are applied with a brufh to the infide of the pieces, of a compofition made of 7-tenths of yellow wax, 1-tenth of turpentine, 1-tenth of white pitch, and 1-tenth of hog's-lard, which is melted flowly to prevent its forming bubbles.

When the different layers form a thicknefs of three or four millimetres, (0.15 inch) cakes of wax are placed infide in thofe parts where the bronze fhould be of a greater thicknefs, and faftenings of fheet-brafs are inferted, which may take hold of the core and prevent the wax from falling off.

The

The core caſt in this mold, and the modelling of the wax finiſhed by the ſtatuary after it is taken out.

The mold is then fitted together, and the core caſt with quickneſs, that it may form an entire maſs, and not lie in layers: As ſoon as it is ſolid the model is taken aſunder, and the ſtatuary repairs the wax, takes off all the futures of the molds, rectifies the errors which may have occurred, and gives to the work all the perfection of which it is ſuſceptible.

The true mold formed over the wax; its compoſition.

After this the true mold is formed of materials capable of ſupporting the heat and the impulſe of the metal; to compoſe which three-ſixths of earth are mixed with one-ſixth of horſe-dung, and left to rot in a ditch for one winter: when this mixture is taken out, two-ſixths of broken crucibles, well pounded and paſſed through a ſieve, are added: the whole is tempered with urine and beat up on a ſtone: it is then what is called *potee*.

Formation of the true mold continued.

When it is to be uſed, a ſufficient quantity is taken and water enough added to it, to make it capable of being laid on with a bruſh; forty coats of it are then laid over the wax ſucceſſively, (care being taken that one coat is dry before another is laid on), which altogether form a thickneſs of five centimetres (2 inches.) The mold is then ſurrounded with flat bands of iron, which croſs each other like net-work; then, after rendering the *potee* thicker, by adding earth to it, and hair that has been well beaten, it is laid on over the former work with the fingers, until the mold has attained the thickneſs of twenty centimetres below and ſixteen above (6 and 8 inches); after which it is ſurrounded a ſecond time with bands of flat iron.

Preparations for, and method of melting out the wax.

After this a wall is built round it, the paſſages neceſſary for the fire conſtructed, and the intervening ſpaces are filled up with broken bricks: Then the fire is kindled in the paſſages moſt diſtant from the figure, and is gradually increaſed for nine days, and again diminiſhed for the ſame ſpace of time. On the ſecond day the wax begins to flow, and continues to do ſo for ten or twelve days; about half of it is loſt.

The mold buried, and the pipes and vents placed ready for caſting.

When the fire is extinguiſhed, the work is left ſome days to cool; then the broken bricks are removed, and before the mold is buried, a coat of plaſter, about half an inch thick, is put over it, which is called the *chemiſe*. Then they proceed to bury the mold, or incloſe it with earth, firſt taking care to ſtop all the ways through which the wax flowed, and to raiſe the

the pipes for the vents and for the entrance of the fufed metal. The earth ufed for enclofing the mold fhould be firft fkreened, and then laid on equally in the excavation. After each courfe is raifed to a thicknefs of thirty centimetres, (1 foot) it is beaten down till it is reduced to ten.

After this there only remains to build the bafon for the reception of the metal, called the *echino*.

Enumeration of the various advantages of cafting in fand, and difadvantages of the great foundery.

In enumerating the operations neceffary for the method of cafting in fand firft mentioned, the authors of the report ftate them to amount to ten; while thofe ufed for cafting in the large way laft recited, in which wax is ufed, amount to no lefs than twenty-eight, each of which they particularize; but as thefe operations may readily be counted from the relation already given, this catalogue is not inferted here. They alfo remark, that the laying on the wax on the pieces of the mold takes up much time, as does likewife the preparation of the *potee:* that in the firft method the circling with bands of iron is entirely avoided, and the building of the paffages for the fire, which are very expenfive: that likewife the molding and fetting up of the vaft number of pipes and vents is faved in it; and in the drying of the work the economifing of fuel is greatly in favour of the firft method, for in it thefe operations are performed in a fhort time with a very fmall fire, which in the other method require at leaft three weeks and a powerful heat: that in the repairing of the wax the ftatuary muft work with his own hands; and that in taking out the ftatue when caft, there is vaftly lefs trouble in the firft method.

The authors here defcribe the method of erecting the furnace for fufing the metal for the ftatue of Joan of Arc; but as it was conftructed to burn wood, which fuel is not ufed in our founderies, and as the defcription would be on other accounts of but little benefit to our artifts, it is omitted.

It is only neceffary to ftate, that the place which contained the fufed metal was at fuch an elevation, that, when the ftopper which retained it was driven in, it might flow freely into the *echino* through the paffage prepared for it.

The mold of the ftatue of Joan of Arc laid in the earth ready for cafting.

The mold for the ftatue was partly buried in the earth, fo as to allow a fall for the metal of eight centimetres (3 in.) from the hearth to the entrance of the pipes; and the authors obferve, that the trouble of burying the mold might be avoided by lay-

ing

ing it on its side, for which position they think it was sufficiently well prepared.

Parts of the statue cast separate from the rest.

The statue was all formed in one mold, except the skirts, *(pleinte)*, one arm, and the plumes of the helmet, which were placed in a separate case: This might have been dispensed with, but it was apprehended that, if they remained with the statue, they would have much encreased the difficulty of the work, by adding to the elevation of the figure.

1000 kilograms of metal fused.

Every thing being prepared for the casting, about 1000 kilograms (about 32 C.) of the metal was placed in the furnace, one half of which metal consisted of old cannon, a third of copper, and the rest of brass; and on the 8th of Germinal, *An XII*. (29th March, 1804), at eight o'clock in the afternoon, the metal, after five hours heating, being in compleat fusion, and the *echino* and the stoppers which closed the two passages for the metal being previously heated, M. Rousseau forced in the plug that retained the metal in the furnace; it flowed immediately into the *echino*; the stoppers were removed from the passages for the metal from thence, and in less than two minutes a little of the metal appeared at the vents, and shewed that the cast was completed.

The cast made by M. Rousseau: It succeeded compleatly.

The statue requires no repairs or additions.

On removing the sand it was found that no accident had happened but a slight flaw on the stomach of the figure; that the head was quite perfect; and that there had been no partial casting, or any part of the figure necessary to replace, which often happens in the other method.

The reporters recommend the *Athénée des Arts* to give medals to MM. Rousseau and Genon, and to make honourable mention of M. Gois.

The reporters conclude with high encomiums on the advantages of this method of casting, and recommend that medals be given to MM. Rousseau and Genon by the Athénée des Arts, in testimony of their merit; and that as the rules of the Society prevented this recompence from being granted to any of its members, honourable mention should be made of M. Gois.

Experiments

VII.

Experiments made at the Galvanic Society of Paris, by M. RIFFANT, *Director of the Nitre and Gunpowder Works, tending to prove that Muriatic Acid is not composed as announced by M. Pachiani* *.

As soon as the Galvanic Society knew that M. Pachiani, of Pisa, had announced, that he had obtained muriatic acid by depriving water of a portion of its oxigen, their first care was to engage in a course of experiments both by galvanism and electricity, to obtain, if it was possible, a confirmation of a discovery so important to the progress of science. The society had a letter communicated to them, which was addressed on the 9th of May, 1805, by M. Pachiani, to M. Pignotti; in which he recited the results which he had obtained, but without entering into any detail relative to the nature or order of his experiments; they only knew that he used the galvanic pile. They therefore determined to make their experiments with the same apparatus, in the manner which appeared to them the least likely to produce results liable to objections. Two of these experiments. which appeared principally worthy of attention, were conducted as follows:

The galvanic Society engage in experiments to determine the truth of Pachiani's position.

Experiment I.

A portion of a new glass tube was taken, three inches long, and 0.35 inch in diameter inside, one of the ends of this tube was closed at the lamp; to the other end was united a capillary tube bent in such a manner as to pass under a jar, and at equal distances from the junction of the capillary tube, two points were drawn out at the lamp in the thickness of the glass, by means of which two bits of gold wire of about 0.02 inch in diameter, and of the standard 0.976 of purity, were inserted in the tube, at a small distance from its lower extremity, and disposed so as not to touch each other, or bear against the sides of the tube; these points of the glass were then closed at the lamp. The tube and its capillary prolongation were filled with distilled water, whose

Description of the apparatus used in Exp. 1st.

* *Journal de Physique*, Tom. LXI. p. 281.

purity

purity had been proved by nitrate of filver; the whole was faftened by wax on a fmall piece of glafs, placed in the midft of an horizontal galvanic pile of fifty-two double fquare plates of 4.25 inches at each fide. Thefe plates were feparated by pieces of leather, which formed between each other divifions, which were filled with very pure fand, moiftened by a folution of muriate of foda. The capillary tube was paffed beneath the water of a ciftern, with its extremity under the mouth of a jar filled with water. The two wires of gold being made to communicate with the two poles of the pile, its activity was immediately exhibited by the difengagement of gas in chains of bubbles very apparent, parting from the extremity of each of the gold wires, but in a more confiderable quantity from that correfponding with the copper pole. This activity of difengagement of gas continued without interruption from the eighth of Thermidor, to the 23d of the fame month, on which day, after the pile being moiftened with folution of muriate of foda, it ftopped for fome time: it foon however recommenced, and always did fo after any fufpenfion. Its activity was immediately renewed by agitating the wires which communicated with the poles of the pile. The activity of the pile was conftantly greateft at four o'clock in the afternoon; and immediately afterwards it began to diminifh. On the 11th of Fructidor the apparatus was taken afunder, after continuing for thirty-four fucceffive days in continual action. The water was then diminifhed to half its original volume, but had not loft any of its limpidity.

The fand of the pile moiftened with folution of muriate of foda: gas immediately difengaged on its completion,

the activity of the difengagement of gas renewed by agitating the wires. This activity always greateft at four in the afternoon. The apparatus feparated after continuing in action thirty-four days.

The extremities of the gold wires, from whence the gas had proceeded, were oxided, but that moft perceptibly which communicated with the zinc pole of the pile. The whole gas obtained and collected during the experiment, was 793 cubic centimetres (1200 inches). The liquor remaining in the tube was carefully examined; it produced no fenfation of tafte on the tongue, had no action whatfoever on the tincture of turnfol and of fernambuc, and did not produce the leaft cloudinefs in the folution of nitrate of filver.

The water in the tube diminifhed one half, the gold wires oxided, that next the zinc moft, 793 cubic centimeters of gas collected. The remaining liquor in the tube has no tafte, has no action on turnfole, and is not affected by folution of nitrate of filver.

The gas produced by the action of the pile was then examined. After having introduced one meafure of it into the eudiometer of Fontana, an equal meafure of nitrous gas, made purpofely for the proof, was added; an abforption of feventy-

feventy-feven two hundredth parts took place in the volume of the two meafures.

The gas produced is tried by Fontana's eudiometer, an abforption of feventy-feven two-hundredths takes place.

In order to afcertain whether all the oxigen which the gas contained had entered into combination in this abforption, a fecond meafure of nitrous gas was introduced into the eudiometer; but the gas did not experience any diminution of its volume. The quantity of oxigen which the abforption produced by the introduction of the firft meafure of nitrous gas might indicate, was attempted to be valued by a comparifon with atmofpheric air effayed in the fame manner; for which purpofe one meafure of atmofpheric air was introduced, and an equal quantity of nitrous gas added; an abforption of fifty-five two hundredth parts took place in the volume of the two meafures. In confidering this abforption as the effect of the combination of nitrous gas with the volume of oxigen gas, correfponding to the 0,22 parts, which atmofpheric air contains, it may be concluded, that the abforption of feventy-feven two hundredth parts, produced with the gas of the pile, reprefented proportionally the combination of the fame quantity of nitrous gas with a little lefs than 0,31 parts of the oxigen. It was then obferved that the meafures of gas having been introduced feparately and fucceffively into the eudiometer, it might happen that they were not fufficiently well mixed, and that confequently the abforption was not as compleat as it might be. It was thought that it might be better to pafs the gafes in feparate meafures under a jar, and then to introduce the whole volume together into the eudiometer. The former experiments having been repeated in this manner, an abforption took place between the gas of the pile and the nitrous gas, of ninety-two two hundredth parts in the volume of the two gafes, in place of feventy-feven refulting from the fame proof, by the firft method; and with the atmofpheric air and the nitrous gas the abforption was fixty-eight two hundredth parts inftead of fifty-five. There refults then from this, according to the fame ratio of the 0,22 parts of oxigen contained in the atmofpheric air, a proportional indication of about 0,30 parts of this gas contained in that of the pile. It was again proved with the eudiometer of Volta, by introducing a fingle meafure into it, through which the electric fpark was made to pafs; the proof was afterwards repeated fucceffively.

The quantity of oxigen contained in the gas valued at 0,31 parts.

The proportion of the oxigen after more exact trials is more accurately valued at 0,30 of the gas.

The gas farther proved by Volta's eudiometer gives the fame refult of 0,30 oxigen.

fucceffively, on two, three, and four meafures, and always the abforption refulting from the inflammation by the electric fpark, gave the fame indication of about 0,30 parts of oxigen.

Experiment II.

Apparatus ufed in fecond experiment defcribed. The fand is moiftened with river water containing 1 fixtieth nitric acid. The gold wire is diffolved and precipitated,

Two grammes (31 gr.) of diftilled water were put into a glafs tube bent into the form of a fyphon; two wires of gold of commerce of about 0,008 inch in diameter, paffing into the water at about 0,024 inch diftance from each other, were inclofed in this tube; the tube was then placed upon an horizontal pile of fifty double plates, of about $3\frac{1}{4}$ inches in each fide. The intervals between them were filled up with dry fand, and then moiftened with river water acidulated with about $\frac{1}{60}$ of nitric acid. The wires of gold having been placed in communication with the two poles of the pile, the water in the tube affumed in the firft day a reddifh brown tint at the fide of the copper pole, and the wire which paffed to that part was covered with a coat of oxide of gold of a deep brown colour. The wire which communicated with the zinc pole did not affume the fame tint; the gold of the wire was diffolved by degrees, and was precipitated together with a part of the filver. This precipitate exhibited with a magnifying lens, over almoft the whole length of the tube, cryftals in needles. The wire correfponding to the zinc pole was entirely deprived of the gold which covered it, and then only confifted of a thread of filver of extreme tenuity. But very little gas was difcharged from either extremities of the wires. The water was not diminifhed a fiftieth part of its volume.

very little gas produced, the water in the tube is not diminifhed 1-fiftieth.

The pile continued in action forty days. Indicates by the electrometer an intenfity of 840°. The remaining liquor in the tube fhews no trace of acidity by any of the reagents, and has a metallic tafte.

The pile continued in activity 40 days from the 28th of Meffidor to the 8th of Fructidor. It indicated then on the laft day by the electrometer (fimplified by one of the Members of the Galvanic Society, from that conftructed in Germany, defcribed in the *Journal de Phyfique* for the month Meffidor, an. 13,) an intenfity of 840 degrees. The liquor remaining did not afford, by any of the different reagents, the leaft trace of oxidity; a metallic tafte was alone perceptible in it.

The galvanic fociety, in examining chiefly the refults of the firft experiment, as correfponding more particularly with the fact announced by M. Pachiani, have confidered, that in

allowing

allowing for the fmall quantity of oxigen which had caufed the oxidation of the extremities of the gold wires, the whole quantity of the oxigen contained by the gas of the pile may be valued in a very near approximation, at 0,31 of its volume; and it is very nearly in this fame proportion that oxigen gas enters into the formation of water; it was thought that it might thence be concluded, that the whole effect of the galvanic pile, during the entire continuation of the experiment, had been the decompofition of a part of the water employed, and the feparation, in their ftate of purity, of the oxigen and hidrogen gafes which compofed it. The Society then are of opinion, that M. Pachiani was deceived as to the nature of the acid which he announced that he obtained, and that this acid * might have been produced by fome animal or vegetable fubftance employed in the apparatus. The Society does not hefitate to declare, that with the apparatus which they ufed in preference, (as being the moft fimple, and the leaft liable to the influence of other matters,) they do not think it poffible to effect any thing by the action of the galvanic pile, but a decompofition more or lefs great, of the water ufed in the experiment.

The Society think the action of their pile only decompofed the water into oxigen and hydrogen gafes, and that Pachiani is miftaken, and that the acid found in his experiment was produced by other means than thofe he announced, and that it is not poffible to effect anythi by the galvanic pile in water but its decompofition.

VIII.

Account of an Ancient Geographical Tablet in the Mufeum of Cardinal Borgia, from † a Memoir prefented to the Academy of Gottingen, by PROFESSOR HEEVEN.

IN the Mufeum of Cardinal Borgia there is depofited an ancient geographical tablet, from which an engraving has been

The tablet was found in the mufeum of Cardinal Borgia.

* M. Giobert, in Van Mon's Journal, pretends, that the acids and falts ufed in the pile *circulate along the wires*, and pafs into any liquor into which they are conducted; which does not appear probable.—B.

The galvanic apparatus ufed in thefe experiments is called a pile through the whole of the French paper, though from its horizontal pofition the appellation does not feem very proper. *Trans.*

† This memoir is entitled, "explicatio planiglobii orbis terrarum faciem exhibens, ante medium fec. xv. fumma arte confecti; agitantur fimul de hiftoria mapparum geographicarum recte inftruenda confilia.

The design is two feet in diameter, and done in coloured enamel,

limits of the countries not marked,

accounts inserted in it of various remarkable things.

been made, one of the impressions of which is in the possession of the author of the Memoir. This remarkable monument is not a chart drawn by the pen, but a round tablet, of which the design occupies a space about two feet in diameter, on which the hemisphere, known at that time, is represented in coloured enamel, like a round surface. The countries and the places are marked by their proper names, but the limits of the countries are not traced; the mountains, the rivers, the people, and all the things remarkable (as the animals, the battles, the caravans, the bazars, the camps, the wandering tribes, &c.) are represented and explained on it by inscriptions in the latin tongue, but written in German characters. It may be conceived from this first view how interesting this monument is, and also with what art it is executed, so that it is impossible to suppose, that it was made for the use of a private person. Its date is not mentioned, but it may be determined with certainty that it was constructed in the first half of the fifteenth century. In reality, the most recent event marked on it is the victory of Tamerlane over Bajazet in 1402; there is no mention of the taking of Constantinople, or the least trace of any of the discoveries of the Portuguese. Of the geographical charts known at present, that of *Marino Sanudo* at the commencement of the fourteenth century is the only one certainly more ancient; but that of *Andre Bianco* of 1436, which *Formaleoni* has made known, is very nearly of the same time as this monument. No general source of information can be discovered by which the author of the tablet has been assisted. It is not made according to documents from Ptolemy; it more follows those of the Arabians, especially with regard to Africa; of the names which are found in the works of *Marco Paolo,* and the other more ancient travellers in Asia, only some are seen on that part of the world. The extent of Europe is represented as much greater than that of Africa, and at least as large as that of Asia. The following are some of the most remarkable particulars of it: Sweden is set down under the name of *Magna Gothia,* and Denmark is wanting. In Prussia, the seat of the wars of the Teutonic order with the Lithuanians, represented with this inscription: *Hic sunt confinia paganorum et christianorum, qui in Prusia adinvicem continuo bellant.* It may be perceived by this, that the Lithuanians were therein considered

Why supposed to have been made in the first half of the fifteenth century.

It is the oldest geographical design, except the chart of Sanudo.

Sweden called Magna Gothia in it, Denmark omitted, Lithuanians called Pagans, ſidered as Pagans, although chriſtianity was introduced among them before this period. Ruſſia appears under the denomination of Tartary, and near the Caſpian Sea and the ſea of Aſof, are repreſented the famous Bazars of thoſe times. England and Scotland appear at the border, but there is no more room for Ireland. Africa exhibits none of the diſcoveries of the Portugueſe, but the northern half of it was known to the author as far as Soudan. He names not only the villages along the coaſt, but he moreover knows that the inhabitants of mount Atlas, the people of Barbary are at war with the Saracens. Near theſe mountains is inſcribed, *In illis montanis habitant plures principes et reges, et habitant continuo in tentoriis, et præliantur continuo contra Saracenos, et contra juxta caſtra et civitates* *. In Egypt the junction of the grand caravan of Mecca is marked, and not only the names of the deſerts of ſand are inſerted, but thoſe alſo of the places moſt important to commerce, as Tagaza, Ganuſia, &c.

Ruſſia called Tartary in it. Places of the Bazors repreſented.

It contains none of the diſcoveries of the Portugueſe in Africa.

The junction of the Caravans of Mecca noted in Egypt.

The kingdom of Preſter John extends in Nubia *ab oſtio gandis* (Cape Gardeſan) *usque ad fluvium auri*. Bianco likewiſe ſets down the kingdom of Preſter John in Africa in the ſame manner, ſo that the Portugueſe are not the firſt who have thus deſcribed it. Aſia does not preſent fewer ſingularities. In Aſia Minor the camps of the Tartars are repreſented; *Tartaria reges maxima, qua Tartari cum ſuis jumentis et bobus excurrunt, civitatem ex multis tentoriis et carutes ſituant*. India is divided into *India ſuperior*, where the body of St. Thomas is found, and many chriſtian kingdoms, and *India interior, in qua Cathai civitas et magnificanis Imperatoris Tartarorum ſedes*. China is likewiſe inſerted in it, and its capital Cambalck (Cambalu Pekin) is alſo named. On the frontiers of little Bucharia at Organti, (Urgang)

Extent of Preſter John's kingdom ſet down.

Camps of the Tartars in Aſia Minor marked.

the diviſions of India, and poſition of Cathai inſerted, and China with its capital Cambalk (Pekin.)

* The Latin of the inſcriptions in this paper is not very correct, *continuo* is uſed in them for *ſemper*, and *juxta* for *vicina*, the word *ſituant* is alſo improper, and ſome others, but theſe circumſtances perhaps only mark more ſtrongly the authenticity of the account. In the inſcription relative to India interior, a ſmall alteration has been made from the memoir in this tranſlation. The word *magnificanis* has been formed from *magni canis* in the memoir, which being ſo printed, evidently was an error.—B.

The route of the caravans to Cathai; the country of Gog and Magog; and the site of Paradise.

de Organti ad Carthagium vadunt cameli in quatuor mensibus; the caravans going and returning to Cathai are represented. On the eastern border the country of *Gog* and *Magog* is set down, and finally *locus deliciarum* or paradise.

IX.

Analysis of Birdlime. By M. Buillon Lagrange.*

Sect. I.

The Origin and Preparation of Birdlime.

Various opinions on the nature of birdlime;

THE substance known by the name and appellation of *birdlime*, has been classed among the immediate productions of vegetables.

M. Fourcroy,

M. Fourcroy was the first person who considered this matter to be glutinous: he has described it as a species in his "*Systeme des Connoissances Chimiques*," Vol. VIII. p. 306.

Birdlime, according to this chemist, may be made of the berries of mistletoe, or of the tender bark of the holly, and several other kinds of trees, macerated in water. Although this substance appears to have been hitherto not examined with sufficient accuracy, many qualities have been discovered in it analogous to those of gluten,

Excepting a few chemical properties, mentioned in my "*Manuel d'un Cours de Chimie*," third edition, I have never found in any work the least elucidation of the nature of this singular substance.

M. Chaptal,

M. Chaptal, in his "*Elemens de Chimie*," speaks only of its preparation. As the method prescribed by this chemist is nothing different from that in the "*Materia Medica*" of Geoffroy, and in the "*Dictionary*" of Valmont de Bomare, I shall quote the article itself.

Geoffroy, Valmont de Bomare.

Ancient mode of preparing it, by boiling and pounding the berries of mistletoe;

"The ancients made use of the berries of the mistletoe of oak in the preparation of birdlime. The berries being first boiled in water, were pounded, and the hot water was then poured off, in order to carry away the seeds and rhind. At present birdlime made of the bark of holly is preferred. The middle bark is made choice of, as being the most tender and

* Annales de Chimie, Vol. LVI.

green:

green: this is placed in a pit to rot, after which it is pounded in mortars until it becomes a paſte, and is then waſhed and cleanſed with water. This ſubſtance has been conſidered as diſcuſſive and emollient, when applied outwardly."

at preſent it is made from holly bark rotted and pounded.

It is already known that the miſtletoe of oak is employed in ſeveral pharmaceutical preparations; as, the univerſal water, the antiſpaſmodic powder, Guttet's powder, &c.

Miſtletoe of oak uſed in pharmacy.

In England, according to Geoffroy, birdlime is made of the bark of holly. He ſays, the bark is boiled in water ſeven or eight hours, till it becomes ſoft and tender. This is laid in maſſes in the earth, and covered with ſtones, placing one layer over another—the water having been previouſly drained from the bark. In this ſtate it is left to ferment and rot, during a fortnight or three weeks, in which time it changes to a kind of mucilage. It is then taken from the pit, pounded in mortars till reduced to a paſte, waſhed in river water, and kneaded till freed from all extraneous matters. The paſte is left in earthen veſſels during four or five days, to ferment and purify itſelf. It is afterwards put into proper veſſels, and thus becomes an article of commerce.

English mode of making birdlime: Bark of holly is boiled, rotted, pounded, waſhed, and kneaded.

This mode of preparation is not univerſally followed, as every county has its peculiar way: there are even thoſe who make a ſecret of the proceſs.

At Nogent-le-Rotrou birdlime is manufactured by cutting in ſmall pieces the ſecond bark of the holly, fermenting them in a cool place during a fortnight, and then boiling them in water, which is afterwards evaporated.

Method purſued at Nogent-le-Rotrou; firſt rotting and then boiling:

At Commerci and its environs birdlime is obtained from ſeveral ſhrubs, as the holly, the wild vine *(viburnum lantana, Lin.)* and the miſtletoe of every ſpecies.

At Commerci.

The beſt is that made from the prickly holly, which is greeniſh; that obtained from the *viburnum lantana* is of a yellowiſh tint. In uſing this latter, the epidermus is rejected, and only the ſecond bark employed.

Beſt ſort from the prickly holly.

The birdlime which I uſed in my experiments was made from the ſecond bark of holly; and on comparing it carefully with ſome which had been ſent me from Commerci, I found there was no apparent difference between them. I thought this precaution eſſential to obtaining greater preciſion in my analyſis. It is well known that the birdlime of commerce is never in a pure ſtate; it is frequently a compoſition of vege-

table

Birdlime of commerce frequently adulterated.

table and animal matter; sometimes it is even adulterated with turpentine, oil, vinegar, &c. It was therefore necessary that I should be certain as to its purity; and by the following mode, which I adopted, I obtained birdlime of the very best quality.

The author's preparation of birdlime.

Having procured a sufficient quantity of the second bark of holly, I bruised it well, and boiled it in water for four or five hours: the water being poured off, I deposited the bark in pits, in earthen pans, where it remained till rotten, or at least till it became viscous, moistening it from time to time with a little water. When it had obtained a proper degree of fermentation, it was cleansed, by washing, from all heterogeneous matters.

Sect. II.

Chemical and physical Characteristics of Birdlime.

Characteristics.

Birdlime is of a greenish colour, and of a sour flavour: it is gluey, stringy, and tenacious. Its smell resembles that of linseed oil.

It becomes dry and brittle by exposure to the air;

Spread on a glass plate, and exposed for some time to the action of air and light, it dries, and becomes brown in colour, being no longer viscous. When quite dry, it may be reduced to powder, in which state it is totally divested of its adhesive qualities, and only recovers them on the addition of water.

but viscid again when wetted.

Birdlime reddens tincture of turnsole.

By moderate heat it fuses.

When gently heated in a porcelain vessel it melts, but does not become very liquid; it swells in bubbles, which float upon the surface. This kind of fusion produces small black grains, which render the birdlime grumous: it produces a smell very similar to that obtained from animal oils, on raising their temperature.

If this fusion be continued for some time, the birdlime assumes a brownish colour; but recovers its proper characteristics on cooling.

Strong heat inflames it;

Placed on red-hot coals, it burns with a brisk flame, and creates a great deal of smoke.

Heated in a crucible of platina, it takes fire when the crucible is red-hot; produces a lively flame, which rises to about two decimetres, accompanied with a considerable quantity of smoke,

ſmoke, which eaſily attaches to the chimney: this combuſtion always takes place, although the crucible be taken out of the fire.

A whitiſh reſidue is left, which is very alkaline and partly ſoluble in water. Re-agents demonſtrate the preſence of ſulphate and muriate of potaſh.

leaving an alkaline reſidue.

That part which water could not take up, on being put into muriatic acid was diſſolved with ſome effervеſcence.

Experiments on the reſidue.

This liquid is copiouſly precipitated by the oxalate of ammonia; pruſſiate of potaſh gives a blue precipitate. That produced by ammonia is of a paſty conſiſtence, partly ſoluble in cauſtic potaſh: whence may be inferred that the reſidue, independent of the ſalts ſoluble by water, is compoſed of the carbonates of lime and of alumine, with a ſmall portion of iron.

It contains ſoluble ſalt and carbonate of lime, and alumine and iron.

Water has very little influence on birdlime. On boiling, the matter does not completely diſſolve, but acquires merely a ſmall increaſe of fluidity, which it loſes in cooling, and reſumes its primitive conſiſtency.

Water has little action on birdlime;

This water obtains no colour; its flavour is at firſt inſipid, afterwards ſour, and it reddens tincture of turnſole.

Evaporated to the conſiſtency of a ſyrup, it becomes coloured, with a mucilaginous appearance, which may be ſeparated by alcohol.

The action of water, therefore, is confined to the ſolution of a mucilaginous ſubſtance, with a ſmall portion of the extractive matter.

It takes up ſome mucilage.

It is not thus with cauſtic potaſh. Its concentrated ſolution forms at once with birdlime a whitiſh magma, which turns brown on evaporation, with a ſeparation of ammonia.

Cauſtic potaſh combines with birdlime and gives out ammonia.

This compoſition is leſs viſcid; it acquires a great degree of hardneſs by expoſure to the air; and its ſmell and taſte are ſimilar to thoſe of ſoap.

It is chiefly ſoluble in water and alcohol, there remaining but a few vegetable dregs. Theſe ſolutions are affected by ſtrong acids; but theſe kinds of decompoſitions preſent no new phenomena to thoſe obtained with a ſolution of ſoap.

The compound has the habitudes of ſoap.

The moſt feeble acids ſoften birdlime, and partly diſſolve it: when concentrated, they act in a different manner.

Weak acids ſoften and partly diſſolve birdlime.

Sulphuric acid renders it black and charry: by adding powdered lime, ſo as to form a thick magma, a ſeparation of acetic

Sulphuric acid charrs it.

tic acid and ammonia is procured. There can be no doubt but that in addition to the acetic acid naturally present in birdlime, more is produced by the action of the sulphuric acid.

Hot nitric acid decomposes it, and forms a kind of wax:

Nitric acid, whilst cold, has very little power over birdlime: but on increasing its temperature, the mixture turns yellow, dissolves, and as evaporation advances swells considerably, leaving at last a hard brittle mass. This mass, when a second time submitted to the action of nitric acid, is dissolved, a part being converted into malic and oxalic acids. By continuing the evaporation a yellow matter is obtained, easily friable, yielding to the pressure of the fingers like wax, with a kind of elasticity, and melting by means of a gentle heat.

which forms soap with potash.

Potash combines with this matter, changes its colour from yellow to brown, and forms perfect soap.

Alcohol partially dissolves it.

Alcohol partially dissolves it, and becomes yellowish; its transparency is diminished by the addition of water.

On evaporating the alcohol to dryness, there remains a yellow matter divested of the greasy appearance, which yields a sweet odour in burning.

Cold muriatic acid has little action on birdlime.

Cold muriatic acid has no action upon birdlime: when heated it turns it black.

Oxigenated m. acid alters birdlime considerably.

Oxigenated muriatic acid operates differently. Either by mixing the gas with the water containing the liquid birdlime, or by shaking it in a bottle with the acid in a very concentrated state, the following phenomena were equally observed:

The birdlime quickly lost its colour, and became white; it was no longer viscid, but divided into hard compact portions, containing in their centre a quantity of birdlime, which the oxigenating principle had not reached. This non-oxigenation may be attributed to the difficulty there is of preserving this substance in its liquefied state in hot water, whereby the operation of the acid is confined to its exterior surface.

Characteristics of oxigenated birdlime.

The characteristics of oxigenated birdlime are—

1. It is capable of being reduced to powder.

2. It is insoluble in water, even when heated.

3. It does not become liquefied at a high temperature.

4. It will not turn yellow, nor will it form a resin with nitric acid.

Acetous

Acetous acid softens birdlime and dissolves a certain quantity; the liquor acquires a yellow colour; its taste is insipid. Acetous acid softens birdlime, &c.

Carbonate of potash produces no precipitate; evaporation gives a resinous residuum, which cannot, however, be brought to a state of perfect dryness.

Certain metallic oxides are easily reduced on being heated with birdlime. Birdlime reduces metallic oxides.

Semi-vitreous oxide of lead assumes a grey colour, dissolves, and forms with the birdlime an emplastic mass. Semi vitreous oxide of lead incorporates with birdlime.

Alcohol at 40 degrees, and boiling, dissolves birdlime so long as it is kept hot; it is clear, and of a transparent yellow colour; but in proportion as the liquor cools, it becomes turbid. Boiling alcohol dissolves birdlime,

A yellow matter may be separated by filtering, which is much softer than the original mass; melts in a moderate heat, diffusing a smell analogous to that of wax, of which it seems to possess all the qualities. and lets fall a kind of wax by cooling;

The filtered liquor is bitter, nauseous, and acid; precipitating in water, and leaving on evaporation a substance similar to resin. retaining a resin.

Sulphuric ether may be considered as the true solvent of birdlime: its action on this substance is rapid, first dividing it, and then dissolving it nearly *in toto*, there remaining only a few vegetable dregs. The liquor acquires a greenish yellow colour, and strongly reddens turnsole. On adding a little water the mixture thickens, and the ether swims on the top; but if a sufficient quantity of water be poured in to dissolve the ether, a body of oil is formed on the surface, bearing a considerable analogy to that of lintseed: this may be converted, by the assistance of the semi-vitreous oxide of lead, into an emplastic mass. Sulphuric ether the true solvent of birdlime. Water separates oil from the solution.

By evaporating the solution of birdlime in ether, a greasy substance is obtained, of a yellow colour, and of the softness of wax. Ether by evaporation gives a greasy substance.

Conclusion.

From the foregoing observations it will be perceived how little analogy exists between birdlime and gluten.

A simple comparison will be sufficient to designate the place it ought to occupy among vegetable productions. General properties of birdlime.

Birdlime

Birdlime is viscid, elastic, dries a little in the air, by exposure to which it becomes brown; but is not rendered brittle and irrecoverable like gluten.

It melts in the fire, swells, and burns with a vivid blaze; but does not diffuse that animal odour which is to be observed in gluten.

Water will not dissolve birdlime; it merely imbibes the mucilage, the extractive matter, and the acetic acid.

Alcalies dissolve it; when concentrated, they convert it into soap.

Dilute acids soften and partially dissolve birdlime.

Concentrated sulphuric acid renders it black and charry.

Nitric acid turns its colour to yellow, converting the substance partly to malic and oxalic acids, and partly to resin and wax.

Oxigenated muriatic acid renders it white and solid, constituting *oxigenated birdlime.*

Alcohol exerts but little action upon birdlime; it dissolves the resin and destroys the acid.

Lastly, sulphuric ether dissolves it entirely.

Recapitulation of the points wherein it differs from gluten.

Birdlime, therefore, differs from gluten,

1st. In the acetous acid which exists in it.

2d. In being very slightly animalized.

3d. In the mucilage and extractive matter which may be obtained from it.

4th. In the great quantity of resin which may be obtained from it by means of nitric acid.

5th. In its solubility in ether.

X.

Method of purifying Oil. By M. CURAUDEAU.*

The purification of oil consists in its clarification.

THE purification of oils for combustion consists solely in their clarification: It is only since Argand's lamps have come into common use that this subject has received much attention.

There are many processes for the purification of oils, but all are not equally good; and those who sell purified oil make a secret of the method of purification.

* From Cours complet d'Agriculture, Tome XII.

However,

However, as the art of purifying oils ought to be known by those who manufacture them, the processes, which are considered the most economical and simple, shall be here mentioned; by which information they will be able to obtain that profit which those now make who follow this species of industry after them.

The process for the purification of oil by sulphuric acid, which follows, is little different from that published by Thenard. Process by sulphuric acid described.

To one hundred parts of rape oil one part of sulphuric acid is to be added, diluted with six times its weight of water; the mixture should be strongly agitated, and as soon as this is completely finished, it is left still till the oil becomes clear; when it is perfectly clear the purification is effected. Sulphuric acid and water added to the oil; the mixture strongly agitated, and left to settle.

There remains at the bottom of the vessel an acid liquor somewhat coloured: the oil is to be separated from the sediment; and in order to be certain that no acid is retained by the oil, some ounces of powdered chalk is to be added; the mixture should then be shaken, and the oil again left quiet to settle. The oil separated from the sediment; powdered chalk added; agitated again, and left to settle.

The action of the sulphuric acid in this process consists in depriving the oil of all its humidity, although it is itself mixed with water, and in separating from it a mucoso-extractive substance, the presence of which diminishes the energy of the combustion of the oil, covers the wick with charcoal, and produces much smoke: It is then on the abstraction of these principles foreign to the oil, that its quality of giving a good light depends. The acid in this process separates the mucoso-extractive matter from the oil, which injures its combustion.

Another Method.

The next process to be described has been followed by some manufacturers, who have had good success with it. Process by flour and water.

To one hundred parts of rape-seed oil ten parts of water are to be put, to which one part of wheaten flour has been added; the mixture is to be well agitated, and then to be heated until all the water added has been evaporated, or, more properly, until the oil has ceased to have any union with the substances which it held in suspension: In this state it becomes purified; and at the end of twenty-four hours it is very clear, and does not differ at all in quality from that prepared by sulphuric acid. Flour and water added to the oil; the whole well agitated, and heated till a separation of the contained matters ensues; in 24 hours after this it is clear.

In

The heat should be applied gradually, and should not exceed 80° Reaumur.

In the practice of this last process, care should be taken to heat the oil gradually, and not to raise its temperature above 80 degrees of Reaumurs thermometer. (212 Fahrenheit) This heat is sufficient to effect the coction of the flour, and of the mucoso-extractive matter contained by the oil; a greater degree of heat would colour the oil, and deprive it of the appearance most favourable to its sale.

M. Curaudeau led to this process by observing the separation of white sauce into two substances when too much done.

M. Curaudeau was led to this process by an observation, which every one may likewise make. It is well known that the sauce called melted butter, when too much boiled is separated into two parts, one which is thick and occupies the bottom of the vessel, while the other part is clear and floats above the first: The lower substance is the caseous part of the butter united to the flour that has been added to the sauce, and which the action of the fire has separated from the oil; The upper substance is the butter deprived of all foreign matter; and in this state it may be called *purified butter*.

XI.

On a peculiar Fluctuation of the River Dordogne, called the Mascaret. By M. LAGRAVE SORBIE *.

The mascaret takes place only when the waters are low.

THE peculiar movement of the waters of the River Dordogne, which is called the *Mascaret*, takes place twice each day in the summer time, *when the waters are low*, which is an essential condition. A similar motion also takes place on the river Amazons according to the report of M. de la Condamine, when it is named the Pororoca; the same is also perceived at the Orcades, off the north of Scotland, according this author: and M. Sorbie has seen accounts in the publications of some voyagers of its likewise occuring in some of the rivers of Hudson's bay, and also in the Mississipi.

The Pororoca of the Amazons similar to it, a like occurrence at the Orcades, and in the rivers of Hudsons bay, and in the Mississipi.

It is not surprising that this phenomenon does not happen in all rivers; it is not always seen even in the Dordogne. From the most exact observation, if the summer is not dry, and that the waters are not low to a certain degree the Mascaret does not appear, It rarely occurs in winter; it however

* Journal de Physique, LXI. 286.

sometimes

ſometimes takes place during very hard froſts, when the cold has diminiſhed the waters by the formation of much ice; but this happens very ſeldom, and has never been more than three times, in ſeveral ages.

There is a *maximum* of depreſſion in the waters neceſſary to its appearance; Wherefore the mariners in the neighbourhood of Bourdeaux are in the habit of talking of it ſomewhat in this manner, " The waters have fallen ſo much, the tide will encreaſe to day to ſuch a height, we ſhall have a Maſcaret", and they load their barks accordingly, and take precautions to avoid it. The manœuvres of theſe mariners have cauſed ſome naturaliſts in the vicinity of Bourdeaux to obſerve long ſince, that this phenomenon muſt depend on a natural cauſe, connected with the bed of the river, ſince theſe men can foretel, without being ſcarcely ever miſtaken, by the depreſſion of the water, whether the Maſcaret ſhall appear or not, although ſometimes it has not appeared before for ſome years, becauſe the rains have prevented the waters from diminiſhing to the neceſſary degree.

The mariners foretel the Maſcaret from the lowneſs of the river,

No one has yet tried to explain the cauſe of this ſingular fact, not even M. Condamine, or if there be any works on the ſubject, they are unknown to the author though his ſtudies have been particularly directed in the line where ſuch information might occur, and he has read much. In order to enable others to account for the facts, he mentions thoſe which relate to it ſuch as he has himſelf ſeen, and ſuch as he has been told have been witneſſed for ſeveral ages.

no account of the cauſe of the Maſcaret yet publiſhed.

In the ſummer, or, more properly ſpeaking, when the waters are low, there appears at a little diſtance from the junction of the Dordogne with the Garonne, or at Bec d'Ambes, an accumulation of water, like a promontory, on the ſhore, which is from the thickneſs of a ton to that of a ſmall houſe, and which rolls along with ſuch velocity that no horſe, whatever might be his ſpeed, could keep up with it. It follows the direction of the ſhore, and makes a moſt frightful noiſe. The horſes and oxen, which feed in the meadows near the river, run away with their utmoſt ſpeed exhibiting the greateſt terror; ſo much ſo that they remain trembling a long time after, and cannot be driven back but with much difficulty. The ducks and geeſe have alſo been ſeen to precipitate themſelves into the reeds at its approach, with the greateſt ſpeed and af-

It conſiſts of an accumulation of water, which appears firſt at Bec d'Ambes,

and ruſhes up the river with great velocity, and a frightful noiſe, terrifies the cattle, and the water fowl,

fright

fright, and lie flat there, without being able to come out. Hard bodies, which lie in the way of the Mascaret are struck by it with such force, that the piers, built for the use of the vessels, along the shore are demolished, and some of the stones which compose them, although very large, are driven away more than fifty paces; the strongest trees are torn up by the roots, the barks which it meets are not only sunk, but broken asunder, especially if they are near the shore, or have any hard body lying beneath them. From the place called St. Andre (See the lower part of Plate IV.) on the river, the Mascaret forms itself into waves which half its breadth as far up as Caverne; there it disappears for a short time, to appear again between Asque and Lile like a promontory, and then returns into the form of waves as far on as Tersac; at Tersac it regains its first appearance, which it only quits at Vayne; from Vayne it proceeds along the bank as far as Fronsac, the house of M. de Richelieu; from Fronsac it occupies the whole breadth of the river, passes with a terrifying noise before the village of Libourne, throws the road for vessels belonging to this village into confusion, and afterwards appears at Genisac-les Reaux and at Peyresite with but very little force. The whole passes in the course of seven or eight leagues.

overturns the piers along the river, and drives the large stones which compose them, more than fifty paces off, tears up large trees by the roots, and sinks and breaks vessels in pieces. Above St. Andre it appears in waves, above Asque is seen in its original form; in waves again above Lile, at Tersac it regains its first appearance; at Fronsac it occupies the whole breadth of the river, passes before Lisbourne with a terrible noise, and ceases at Peyresite.

The following is the account, which M. la Condamine gives of the *Pororoca* of the river Amazons, the comparison of the effects of which with those of the Mascaret will tend to establish the theory of these phenomena.

Account of the Pororoca on the Amazons:

In his voyage to the river Amazons, page 193, he relates, that " between Macapa and Cape-Port, where the channel of the river is most confined by the islands, and especially opposite the mouth of the Arawary, which joins the Amazons on the north side, the flowing of the sea exhibits a singular phenomenon. During the three days next the full of the new moons, the times of the highest tides, the sea, instead of taking almost six hours to rise arrives at its greatest heighth in one or two minutes; it may be conceived that this does not happen quietly; there is heard at a league distance a terrible noise, which announces the *Pororoca*, which is the name that the Indians of these parts give to this frightful flood. In proportion as it approaches the noise encreases, and soon an accumulation of water, like a promontory, appears from 12 to 15 feet high; after that another is seen, then a third, and sometimes a fourth, which follow

its noise heard at a league distance. It advances in several waves, each twelve or fifteen feet high,

2 each

each other closely, and which occupy the whole breadth of the channel. These waves advance with a prodigious rapidity, break and overturn every thing which opposes them. I have seen in some places a large extent of land carried away, great trees torn up by the roots, and ravages of all kinds committed; every where that they pass the banks are swept clean; the canoes, the pirogues, and even the barks can only escape their fury, by anchoring in deep water. After having examined this phenomenon with attention in different places, I have always remarked that it only takes place, when the rising flood is engaged in a narrow channel, or meets in its way with a bank of sand, or a shallow place, which occasions an obstacle to it; that it was in those places alone that this impetuous and irregular movement of the waters commenced, and that it ceased a little beyond the bank, when the channel became deeper, or grew considerably wider. It is said that something similar to this happens at the isles of the Orcades, at the north of Scotland, and at the entrance of the Garonne, (it should be the Dordogne), in the vicinity of Bourdeaux, where the effects of these tides, is called a *Mascaret*."

which rush forward with great rapidity, and overturn every thing which opposes them, carry away large portions of the land, and tear up trees by the roots.

It only occurs in narrow channels, over sand banks, or shallow places.

It ceased where the channel became deeper or wider.

It appears from what has been cited from M. Condamine, that the effects of the Pororoca are almost the same as those of the Mascaret. Nevertheless there is a marked difference between them in this respect, that on the Dordogne, two kinds of floods take place, one which extends over the whole river, and is similar to that which M. Condamine has observed, and the other which ranges along the shore, rolling more over the deposits which the waters have left, than in the water itself. He says positively in page 194, that " at one or two leagues a frightful noise is heard, which announces the Pororoca; as it approaches the noise encreases; and soon an accumulation of water appears from 12 to 15 feet high, and then another that follows, which occupies the whole breadth of the channel". On the Dordogne the Mascaret rises with great noise, sometimes along the coast in an elongated accumulation, and sometimes in the form of frightful waves, which extend over the whole river; when it follows the shore it only appears in the re-entring angles, and on the sand banks, as is described in the sketch of the plan of the river, which accompanies this account, and which takes in the whole extent where the effects of the Mascaret are perceived. The parts covered with small

Difference between the Mascaret and Pororoca.

further particulars of the Mascaret.

Description of the sketch of the course of the

Mascaret in the river.

small points, indicate the sand banks where the Mascaret always commences; the parts occupied by small lines, are the places where the waves occupy the whole breadth of the river. The dotted parts indicate the re-entring angles, where the sand banks are found which are deposited by the counter-current. It is here principally that the Mascaret rolls with all its fury over the mud of the river. On the banks the salient angles, are the places where the Mascaret quits the shore, occupies the whole river, and runs upwards, accompanied by many considerable waves, which succeed each other, till another re-entring angle occurs, where it again resumes its first form.

It is thus that these who dwell in the vicinity of Bourdeaux witness without emotion twice each day, when the waters are low, so extraordinary a phenomenon, without any one thinking of examining into the cause of it, or even of communicating the particulars to naturalists.

The tide is the primary cause of the Mascaret.

The primary cause of this rising of the water is the same as that of the tide in all rivers; and if the Mascaret occurs on very few rivers, it is because their beds are not formed in a manner necessary to produce it, and have not the same disposition as the Gironde and Dordogne: they have either too little or too great a current; their waters are not sufficiently low, or when they are, the tide does not continue long enough; finally the re-entring and salient angles are not such as they ought to be. M. Sorbie thinks he could tell before hand whether any river would be liable to such effects, from the form of its plan and the disposition of its bottom; and is of opinion that the cause why more rivers are not subject to the Mascaret, depends entirely on the shape of their beds, and not on any particularity in their tides. The physical cause of that on the Dordogne appears very simple, M. de la Condamine says, that on the Amazons it is always at the narrow parts where it is observed. The cause is not the same on the Dordogne, for there is no narrow parts in almost its whole course: it is nearly every where very rapid, and of small depth, as all those rivers are which have much current. It forms, as may be seen in the plan, many turns and windings; and has few isles: but at each angle a bank of sand is deposited: It descends, notwithstanding these windings, almost from the east to the north-west. As far as Bec d'Ambes, where it unites with the Garonne which is much more powerful than it, and they form together that beautiful arm of the sea, called the

The course of the Dordogne described, to account for the Mascaret, the Gironde. The two rivers then descend together from Bec d'Ambes to the sea in a direction from the east to the north-west. All the waters which arrive from the arm of the sea or from the river, advance in a straight line with abundance into the mouth of the Dordogne, instead of mounting up the Garonne, which runs almost north and south as far up as Bourdeaux. The greatest part of the waters which are advancing to the Garonne, ought then, when the current has taken its course, to run up the Dordogne at the beginning of the flood, since its velocity does not allow them time to turn up the Garonne; and thus the water which ought to go to the Garonne, running up the Dordogne, form by their abundance, this effect which Condamine recites: He says that "the tides, which usually take six hours to rise, arrive at their full height in one or two minutes". But on the Dordogne, the tides never come to their highest level in near so short a time, even when the waters are lowest; but in one or two minutes they encrease considerably; which encrease is probably caused by the waves which arrive almost instantly; and the flood raising their masses of water above their natural level, leaves them there to augment the water in the bed of the river in proportion to their bulk.] After the Mascaret has passed, the waters of both rivers encrease in the same gradual manner as those of all other rivers.

supposed to be caused by the waters on their way to the Garonne, taking the straighter course up the Dordogne.

M. Sorbie likewise thinks, after all, that the tide of the Gironde may be the cause of the Mascaret on the Dordogne, for it pours its waters into the mouth of the Dordogne in almost a right line; this arm of the sea being at least six times larger and deeper than the Dordogne, ought at the flood to carry up such an abundance of water, as could not enter into the bed of this river without occasioning the accumulation of waters described. The physical cause then of the Mascaret is the considerable mass of water which arrives from the Gironde into the mouth of the Dordogne, and the small depth of this river; since it is known that in rainy seasons, and when the river is a little encreased in size, this circumstance never takes place.

It may also be caused by the tide of the Gironde rushing in a right line into the mouth of the Dordogne, and by the shallowness of this river,

M. Sorbie remarks in conclusion that the facts related shew evidently that the flowing and ebbing of the tides of rivers are different from those of the sea; that the ebbing and flowing of rivers, are only secondary effects of the tides of the sea; that is to say, that the waters of the sea only form a dam to those

remarks on the tides of rivers, supposed to be caused by the tide of the sea forming a dam across their course.

those of the rivers, and that the rivers form by the abundance of their waters, those rapid flood-tides which are observed on the great rivers, such as those of the river Amazons, which ascend from 5 to 100 leagues, those of the Senegal, which advance almost as far up, and those of other rivers almost equally considerable. M. Sorbie thinks that the Mascaret, or the Pororoca, have altogether the same cause as the flood-tide of rivers, and though some slight secondary effects occur, such as those related, that all arise from the same physical cause.

XII.

Description of a secret Lock of ten thousand Combinations. W. N.

Disquisition upon locks. The common lock.

THE common lock usually consists of a bolt, which requires a particular instrument, called the key, to push it backward and forward; and in order that this bolt may be inaccessible to violation, certain impediments or obstacles, usually called wards, are interposed between the key-hole and the bolt, which make it difficult to open the lock by any general or common process. The general process for picking a lock, of which the key has not been seen, consists in operating upon the bolt by a small bended instrument or wire; or else by endeavouring to discover the position of the wards by an unperforated key, on the face of which some soft or plastic matter is lodged. And when this situation is once discovered, it is not difficult to file away so much of the key as shall allow it to pass, or else to select, out of a number of skeleton keys, one, of which the form shall admit of its passing through the lock. There are many locks so situated, as for example in the vestries of churches and other little frequented places, as to admit of this slow operation; but it must at the same time be allowed, that the English market presents locks of a number of different constructions, which can neither be picked nor analyzed by the process here mentioned. Nothing is more common, however, than for keys to be entrusted out of the hands of the possessor, or to be hung up, or casually laid down or mislaid. In these circumstances, their figure may be taken

Bolt, key, wards, pick-locks, skeleton-keys.

with

with wax, like the impreſſion of a ſeal, or more ſpeedily by indentation upon a piece of moiſtened paper, or by various other means; and it muſt be admitted, that very little ſkill is required to enlarge the openings of a common key, ſo as to make it paſs the wards of a ſuperior lock.

Secret locks; their ſtructure and imperfection.

Theſe neceſſary and unavoidable imperfections of common locks, have long ago led to the introduction of ſecret locks, which are ſo conſtructed as to require ſome particular manipulation in opening them; ſuch as that the key ſhould be turned twice round, or that it ſhould be turned through a certain ſpace in one direction, and then back again; or that it ſhould act upon ſome delicately reſiſting piece, very likely to be diſregarded by an uninſtructed poſſeſſor of the key; or that a number of viſible parts ſhould be placed in ſome determined order, before the common proceſs of opening, either with or without a key, can take place. Upon all theſe contrivances one general remark may be made, namely, that the poſſeſſor muſt always in perſon open his own lock; for if this be to be done by the mere practice of a ſecret without a key, his cabinet becomes for ever open to him who, by communication or otherwiſe, ſhall poſſeſs that ſecret; and if a key be uſed, his lock, as to that perſon, becomes as ſubject to violation as a common lock.

Methods of violating them.

In the mechanical conſideration of a ſecret lock, we may ſuppoſe the conſtruction to be entirely unknown to him who is deſirous of opening it. In this, according to the experience and ſagacity of the operator, the difficulties will be greater or leſs, and a very ſhallow contrivance may occaſionally preſent a greater obſtacle than a much more elaborate ſtructure. But if we ſuppoſe the ſyſtem of the lock to be known, but the particular conditions of opening it to be ſecret, the examiner will then take for his guide the probable circumſtance that the re-action of the parts may feel conſiderably different, when they are duly placed for opening, than when their ſituation is ſuch as to prevent that effect. By this clue, and by careful examination, moſt of theſe locks may be opened; and it is remarkable, that the better the workmanſhip the more eaſy it is in general to make the intended diſcovery.

Conditions of a perfect lock enumerated.

The following are the conditions which appear to me to be neceſſary in a lock of the moſt perfect kind:

1. That

1. That certain parts of the lock ſhould be variable in poſition through a great number of combinations, one only of which ſhall allow the lock to be opened or ſhut.

2. That this laſt mentioned combination ſhould be variable at the pleaſure of the poſſeſſor.

3. That it ſhall not be poſſible, after the lock is cloſed and the combination diſturbed, for any one, not even the maker of the lock, to diſcover by any examination what may be the proper ſituations of the parts required to open the lock.

4. That trials of this nature ſhall not be capable of injuring the work.

5. That it ſhall require no key;

6. And be as eaſily opened in the dark as in the light.

Theſe conditions are in ſome reſpects liable to the inconveniences already mentioned. I would therefore add the following conditions:

7. That the opening and ſhutting ſhould be done by a proceſs as ſimple as that of a common lock.

8. That it ſhould open without a key, or with one, at pleaſure.

9. That the key-hole be concealed, defended, or inacceſſible.

10. That the key may be uſed by a ſtranger without his knowing or being able to diſcover the adopted combination.

11. That the key be capable of adjuſtment to all the variations of the lock, and yet be ſimple.

12. That the lock ſhould not be liable to be taken off and examined, whether the receptacle be open or ſhut, except by one who knows the adopted combination.

Deſcription of a new lock of combination.

In meditating upon this mechanical problem, I have thought of various conſtructions, but have not yet matured one in which all the above conditions are complied with. The lock delineated in *Plate III*. poſſeſſes the firſt ſix requiſites. *Fig*. 1. repreſents the plate of the lock, of which the other ſide is ſeen at *Fig*. 4. In this laſt figure the middle piece is a handle or knob, repreſented *Fig*. 6, which, when turned, ſerves to ſhoot the double bolt *i k*, *Fig*. 1, by any common connection. In the actual lock this bolt is carried backward and forward by a pin ſtanding out of *Fig*. 2, ſoon to be deſcribed. The other four circles in *Fig*. 4, are handles, repreſented in *Fig*. 5, which ſerve ferve to move the four wheels feen in *Fig.* 1. Thefe wheels have twelve teeth each, and are faftened by center-fcrews, each upon a flat wheel of the fame tooth; but having only ten notches actually cut, as is feen in the right hand upper corner, where one of the upper wheels is taken off, and is fhewn at *Fig.* 3. Thefe upper wheels have their toothed part confiderably higher than the interior or flat part; fo that they would be contrate wheels if the teeth were cut quite through. But this is not the cafe, except with two of the notches, as may be feen in the two lower wheels more particularly, and alfo in the others. The upper wheels have alfo two of the notches between the teeth ftopped up, as is fhewn in *Fig.* 3; by which contrivance there are but ten fituations for fcrewing each wheel upon its correfpondent under wheel; and thefe fituations are rendered precife, and all relative motion between the two correfpondent wheels prevented by a fmall ftud feen in the uncovered wheel, *Fig.* 1, which fits into one of the notches of the upper wheel when put in its place. The upper wheel has a number on each tooth from 1 to 9 and 0, which are of ufe for placing this ftud. The four under wheels are held in their fituations by four fpring-catches, which allow them to be turned, in one direction only, by means of their knobs or handles; and when any wheel is thus turned round, the finger and thumb will feel the ftroke of the lever, as it fucceffively falls into each notch, until the lever comes to reft upon the fmooth part. This very palpable indication then fhews when to begin to count, calling the firft hold or ftroke of the catch 1; the fecond 2; the third 3, &c.; and the lock is fo conftructed, that when the top wheel of any of the four couple is put on with any number oppofite the ftud, the fame number counted by the catch will place the upper wheel in fuch a fituation, as that its notches, which pafs clear through, will lie in a circle defcribed from the center or axis upon which the great handle turns. And therefore, when each of thefe wheels is put in its place, and the numbers known (and regiftered, or put in the memory by fome artificial affociation, fuch as of the date of the year taken either backwards or forwards, &c.) it is only needful to move each of the four knobs till its catch has paffed the fmooth part, with a number of ftrokes anfwering to its adjuftment, and the circle indicated by broken fhaded lines in *Fig.* 1, will be capable of paffing through

Defcription of a new lock of combination.

Defcription of a new lock of combination.

through the open fpaces of every one of the wheels. *Fig.* 2. reprefents a contrate wheel, having its irregular portions A, B, C, D, &c. ftanding up above its plane. Thefe portions are parts of a circle equal to that denoted by the broken fhaded parts in *Fig.* 1. The contrate wheel is to be placed in *Fig.* 1. with its face turned down; and being there fcrewed with its center to the central handle, it ferves to open and fhut the bolt, which it can only do when the four wheels are in fuch a fituation as to allow the circular edge-parts of *Fig.* 2. to pafs clear through their notches. If any one or more of thofe wheels be turned fo as not to correfpond with its number, it will be impoffible to turn the handle, becaufe every attempt to do fo will caufe one of the parts of *Fig.* 2. to ftop in one of the notches of the wheels through which it cannot pafs. The method of opening the lock will therefore confift in fetting each wheel to its known number.

As the proper fituation of each wheel is only one out of ten, it is nine to one againft any operator upon this lock, that he fhall not fet the firft wheel right, fuppofing all the others in their due pofitions; but it is true that he may try all round, and will come to the right place at laft. If two only of the wheels were deranged, it would be eighty-one to one that he fhould not fet them both right; and he would be deprived of any trial round a fingle wheel, becaufe the other wheel would always hold againft him, and prevent his knowing when the open notch of the wheel under trial prefented itfelf. Three wheels deranged would make the odds 729 to one, and the four would make the odds 6561. In the plate the combinations are faid to be ten thoufand, from an overfight in taking the ratio of ten to one inftead of nine to one. But this is a matter of no confequence as to the principles of the lock, becaufe the number of teeth or number of wheels are capable of variation. If a fifth wheel were added to this lock, the odds would amount to 59049.

As the quantity cut from *Fig.* 2. is not more than was neceffary for the clear rotation of the wheels when the lock is fhut, this piece, when in every other pofition, prevents the other wheels from being turned at all.

Letter

XIII.

Letter from Mr. Alex. Crombie, *concerning the Caledonian Literary Society at Aberdeen.*

To Mr. NICHOLSON.

SIR,

THE want of ſocieties for ſcientific and literary improvement, has been long felt in many conſiderable towns in Scotland, and I believe in none more than in Aberdeen.

Great utility of ſocieties for ſcientific and literary improvement.

The utility of ſuch inſtitutions being ſo generally acknowledged, it is truly a matter of ſurprize to find ſo few of them in this kingdom, eſpecially when the facility of forming them is conſidered. Any attempt, however ſmall, to promote the intereſts of literature, and to diffuſe moral, political, or philoſophical knowledge among men of all ranks, will ever meet with the marked approbation of the ſincere wellwiſher to his country; and I am perſuaded you will receive peculiar ſatisfaction in being able to communicate to the public the feebleſt efforts which may be at any time directed to ſo important and deſirable an object.

Reference to a letter in a former Journal.

In your Journal for December laſt, a *traveller* has expreſſed his ſurprize to find no antiquarian or literary ſociety, or ſubſcription library, at Aberdeen; and I agree with his remark, that thoſe who know the reſpectability of the place, cannot fail to be aſtoniſhed at it. To account for ſo ſingular a fact would perhaps be deemed preſumptuous. I have too much reſpect for my fellow-citizens to attribute it to a want of taſte, but I cannot help blaming thoſe amongſt us who are qualified for ſupporting ſuch inſtitutions, for their want of attention in this reſpect.

The Profeſſors of both Univerſities certainly unite talents with influence and reſpectability,—It were to be wiſhed that they and other literary characters in town, had more concern for the improvement of the community at large, and would make ſuitable efforts to promote it.

The Athenæum and circulating library.

It would be doing injuſtice to the liberality of the proprietors of the Athenæum and circulating library, to deny theſe inſtitutions their reſpective merits and advantages. But I apprehend that neither of them is ſufficient to ſupply the *de-*

ſideratum

ſideratum mentioned by your correſpondent. The firſt is principally calculated for the commercial part of the inhabitants, and thoſe who have time to *lounge*; the ſecond, although compriſing much uſeful reading, is ſometimes defective in the ſelection of the books, and affords little opportunity for the union of literary exertions.

Conſideration in favour of a proprietory aſſociation.

A ſociety whoſe books are the property of the individual ſubſcribers, is far better adapted, not only for advancing knowledge and bringing uſeful talents into notice, but alſo for giving a favourable bias to the purſuits of ingenious young men of all deſcriptions, to whom ſuch a ſociety is at all times acceſſible, from the ſmall expence attending it. People become more ſolidly concerned in promoting the ſucceſs of any ſcheme, in proportion as their perſonal intereſts are interwoven with it; and we may therefore conclude, that a man will take more pleaſure, and perhaps derive more profit, from reading a book which he conſiders as his own property, than one only lent him for a time.

Subſcription-library eſtabliſhed Feb. 1805,

Impreſſed with theſe conſiderations, a few perſons in Aberdeen inſtituted a ſubſcription-library upon the 22d February, 1805, under the title of the *Caledonian Literary Society*. Beſides embracing all the periodical publications of merit in Great Britain, our ſtock is enriched with a ſelection of the moſt approved books, either preſented by the members or purchaſed from the Society's funds: Which Society has *already* increaſed to upwards of 100 members, and the liſt is daily augmenting in number and reſpectability.

at a very moderate expence.

It is worthy of remark, that the trifling ſum of ſix ſhillings *per annum* is only required from each ſubſcriber to *The Caledonian Literary Society*. So inconſiderable an expence, contraſted with the great variety of uſeful and entertaining knowledge to be derived from it, muſt form a very powerful recommendation in its favour.

We have been informed with pleaſure, that many perſons in Glaſgow, who are not members of the Society eſtabliſhed there, have contributed liberally to its ſupport by giving books —an example worthy of the imitation of others.

A Philoſophical Society in contemplation.

It is alſo in contemplation to inſtitute a Philoſophical Society, on a plan ſimilar to thoſe of London, Edinburgh, &c. for the purpoſe of receiving occaſional diſſertations on a variety of literary

literary and other subjects, to be deposited as the property, or, entered into the books of the society; and afterwards published in such manner as the society may direct.

Should any of the friends of science in Inverness, Banff, Peterhead, or other places, be desirous of establishing similar institutions, we will most chearfully furnish them with a copy of our plan and regulations.

We have a sincere wish to see every encouragement given to undertakings so laudable and beneficial, and have with this view made the present communication, to give publicity to ours through the medium of your excellent Journal. The insertion of the above will oblige, Sir,

With respect,

Your humble servant,

ALEX. CROMBIE, *Pres.*

Aberdeen, January 2, 1806.

XIV.

Letter from Mr. James Stodart, *in Answer to a Question concerning the Effect of the Nitrous Oxide, proposed by Dr. Beddoes.*

To Mr. NICHOLSON.

Dear Sir,

Qu. Whether Mr. Stodart was nervously affected previous to his feeling inconvenience from nitrous oxide.

Dr. Beddoes, in a paper on the medical effect of respiring the nitrous oxide, published in the last number of your Journal, refers to an account I formerly gave of some unpleasant and rather alarming sensations experienced after inhaling that gas. He attributes the whole to hysteria or nervous affection; at the same time signifying a wish that I would state whether or not that was really the case. In answer to this I have only to observe, that if any such predisposition to hysteria did exist, it was wholly unknown to me. My general state of health was as usual; nor had any thing occurred particularly to affect the mind. I had often inhaled the nitrous oxide under circumstances in every respect similar (at least as far as I can judge) and till that time, so far from experiencing any thing like

Reply: that he was not.

like debility, the very contrary effect was produced; namely, found and undisturbed sleep in the night, followed by strength and increased chearfulness on the following morning.

Expectation that the nitrous oxide may prove eminently useful, &c.

I very sincerely hope the medical application of this extraordinary agent, directed as it is by the very able hand of Dr. Beddoes, may prove as important and useful in medicine as it is interesting and curious in philosophy.

I have not yet heard of its being tried in cases of suspended animation; it appears to be an experiment well worth making. The subject is perhaps worthy of the attention of the Humane Society. I am with respect,

Dear Sir,

Your's sincerely,

JAMES STODART.

Strand, January 22, 1806

XV.

Description of a Statical Lamp, which maintains a Supply of Oil to the Burner from a Reservoir, placed so low as to occasion no Interception of Light. By A. F.

To Mr. NICHOLSON.

SIR,

Description of a new statical lamp.

I SEND you a sketch of an overflowing lamp, of which the construction will be easily deduced from the figure. Its advantages are, that the flame is supplied from below, and the light is not intercepted, but falls on all surrounding objects as directly as that of a candle. The upper part of A (see Plate IV.) contains the usual apparatus of a lamp, either according to Argand's construction or any other; and the column or tube which supplies the oil may be no longer than that supply and the conditions of the structure may demand. The vase below contains the oil, which is poured in, when needful, at the top of the column, by a funnel or otherwise. The circle round B, C, represents a globular (or cylindrical) vessel, having no communication with the vase except through a neck a neck or pipe D, proceeding downwards nearly to its bottom; but there is a communication with the external air, through a perforation (represented by a small shaded circle near B) which prevents the atmosphere from interrupting the intended action. The lightly shaded semicircle B represents an hemispherical solid capable of revolving on an horizontal axis, so as to hang downwards and fill the lower half of the globe, when no fluid is present; or it can be raised up by floatage into any other position, according to the quantity and density of any fluid that may be poured in.

Description of a new statical lamp.

Let us now suppose the vessel C to contain any fluid not more than half its capacity, and that the revolving piece B is of such a weight as to be of half the specific gravity of that fluid: it may then be easily understood that the piece B will settle into such a situation as that part of it shall be immersed in the fluid and support it in the vessel, exactly to the height of its axis. For the part of the solid, immersed on one side, is exactly equal to the space above the fluid in that situation, on the other side; and the greater part of B which is on one side of the perpendicular will exceed the smaller part on the other side, by exactly double that quantity. Consequently the immersed part of the solid will be pressed down by twice its own weight; and this is exactly equal to the weight of fluid which it displaces; whence the body and the fluid will be in equilibrio. Let us now suppose the fluid to be brine, at the specific gravity of 12, which may be poured in either at the top or at the side hole, and that oil of the specific gravity of 9 be then poured upon it; and it is manifest that the oil will press the dense fluid upwards into C, as represented in the figure, and that when C is half filled, the oil will stand at an elevation above the axis equal to one half more than the height of the dense fluid, measured from its surface where the oil presses upon it. And, when this adjustment is once made, by putting in the proper quantity of dense fluid, if any of the oil be taken out, or consumed by burning, the pressure will be less, and the dense fluid will rise within the vase. But this rise will not be attended with any depression in the vessel C, because the level will be kept up by the revolving piece B, and consequently the oil itself will be prevented from falling as much as it would have done if this contrivance had not been applied.

I do

Description of a new statical lamp.

I do not disguise the consideration, that as the oil diminishes, the distances between the upper and lower surfaces of the dense fluid must diminish, and a proportional difference or subsidence in the surface of the oil must take place. The proper remedy for this appears to be that the lower surface should be made as large as convenience will allow; that its rise and fall may be less.

With regard to the disposition and form of the spaces which are to contain the oil, it is only needful to observe that they may all be made small or narrow, except that which is alternately to be occupied by the oil, and the dense fluid. If the height of the dense fluid be 12 inches, the lamp may stand 18 or 20 inches high, using salt water as above mentioned.

There are various practical objections to mercury; but if this fluid were to be used, the oil might be raised ten times as high, or the apparatus, if required, might be constructed with a less distance between the surfaces.*

I am, Sir,

Your constant Reader,

A. F.

XVI.

Letter from a Correspondent rectifying some Particulars of Misinformation respecting the Fishery of the North of Scotland.

To Mr. NICHOLSON.

SIR,

WHEN any important information is communicated to the public, we have a right to expect that it should be given with extreme accuracy; or at least where any doubts exist, with such a degree of diffidence and modesty, as may leave room for avoiding misrepresentation or falshood.

* The contrivance for keeping a fluid at its level by a semi-circular revolving solid was invented by Robert Hooke. See Birch's History of the Royal Society. A. F. has ingeniously adopted it to a lamp which casts no shadow. Hooke's lamp is nearly as faulty as the common fountain lamp in this respect. N.

I wish

I wish *an Enquirer,* in your Journal for December last, had attended to this, before making what I conceive to be a hasty, ill-founded statement, respecting *certain instances of wasteful negligence in some of our fisheries in the north of Scotland,* which it is my duty at present to controvert.—He states:

Erroneous information respecting the fisheries on the north of Scotland.

1st, "That the fishermen of Aberdeen, Banff, Peterhead, &c. never think of carrying their fish along the coast southward, which they might do to Leith in twenty-four hours; or with a good brisk wind to Berwick-upon-Tweed, or even Newcastle-upon-Tyne; but when their respective towns are supplied, *they throw the remainder upon the dunghill for manure!!*"

A fact so improbable as the above, would indeed, require no ordinary share of proof to gain credit to it, and I have the satisfaction to assure you that it is entirely without foundation. The truth is, the number of hands employed in the fisheries in the north of Scotland are so few, and the encouragement given to enterprize and speculation in this important source of national wealth so small, that no more fish is caught than what supplies the neighbouring towns. But even admitting that more were caught, and that we could vend at Leith, Berwick-upon-Tweed, or Newcastle-upon-Tyne, is it not to be supposed that fishers of places nearest to these towns could afford to greatly undersell us?

From various causes this source of wealth is neglected.

When the dog-fish (*squalus catulus,* L.) appear on the coast, our fishers catch a great number of them and dry them for their own private use (for none but themselves and the lower classes of people would use them) and likewise for the benefit of the oil, which they yield in great abundance, and the skin, which is used for smoothing the surface of wood. After they are drained of the oil which they contain, besides keeping a sufficient number for use, they throw the remainder on their dunghills, which produces a *valuable* manure. And no doubt your correspondent may have mistaken these for any other kind of fish.

The dog-fish caught for food, for oil, for its skin, and for manure.

He next observes; "That at Arbroath, another custom, equally as extravagant in its kind prevails, and of which I have been a witness; the crab fishery is so productive, that after boiling them, the bodies of the crabs are thrown away, and the *large claws only* brought to table."

It is

The claws of crabs only are fold at Arbroath, but the bodies are not thrown away.

It is indeed, generally the cafe here, and in every other fifhing town, that the fifhers for the moft part retain the bodies of the crabs, and only difpofe of the claws in the public markets: but that the former are thrown away, is by no means true in almoft any inftance; for the fifhermen find them of far more value in baiting their hooks, than what they could get for them otherwife. Indeed, if it were not for this purpofe, it is believed, few or no crabs would be caught at all.

Much profit might be derived by a company if eftablifhed at Aberdeen for exporting white fifh.

Having thus endeavoured to vindicate our fifhers from the charge of wafteful negligence, which none who know them will think them guilty of; I cannot conclude without expreffing my furprize that no company has yet been eftablifhed at Aberdeen for exporting white-fifh. It is obvious from its excellent fituation, and advantages, that very handfome profits could be cleared, if fuch an undertaking were once fet on foot, and well conducted; equal, if not fuperior to the falmon fifhing, which it is well known has been greatly the means of enriching this place.

If you deem the above obfervations worthy a place in your ufeful Journal you will oblige,

SIR,

Yours refpectfully,

A. L.

Aberdeen, January 3, 1806.

XVII.

Obfervations and Enquiries concerning the Heat of Air blown from Bellows. By K. H. D.

To Mr. NICHOLSON,

SIR,

Paffage from Dr. Black's lectures.

I BEG leave to mention a paffage in Dr. Black's Lectures on the Elements of Chemiftry, publifhed by Profeffor Robifon, which occurs at page 88, Vol. I.

The author is fpeaking of the communication of heat, and has, in the former part of the page accounted for the apparent coldnefs of a ftream of air, by its preventing the accumulation of heat around our bodies, by its impulſe and rapid ſucceſſion, both cooling our clothes faſter, and carrying away the warm air that was intangled in them. The Doctor ſays, "the ſenſation of coldneſs, therefore, produced by wind, or agitated air, is ſo much ſtronger than that produced by equally cold air in a ſtagnating ſtate, that we are often perſuaded the agitated air is actually colder, until we examine it by the thermometer; and Dr. Boerhaave thought the deception ſo ſtrong, that he contrived an experiment to remove it completely (Boerhaave Elementa Chemiæ.) He ſuſpended a thermometer in the air of a large room for ſome time, and noting the degree to which it pointed, he then directed againſt the bulb of it a ſtream of air impelled by a large bellows in the ſame room;—that ſtream of air would certainly feel to a perſon who oppoſed any part of his body to it, conſiderably colder than the reſt of the air in the ſame room; *but the thermometer is not in the leaſt affected by it.* And it would be eaſy to exhibit another experiment to ſhew, that agitated air is not made colder by agitation. A piece of ice, for example, being ſuſpended in the air of a warm room, and blown upon by bellows, inſtead of being thereby kept the more cool, as our hand would be, and preſerved the longer from being totally melted, would certainly be melted ſo much the faſter, than when the air is allowed to ſtagnate in ſome meaſure around it."

that agitation of the air, though it cools heated bodies, does not render the air colder.

nor hotter, though it melts ice.

I take the liberty of troubling you with this in conſequence of a communication from your ingenious correſpondent, Mr. Richard Winter, publiſhed in the laſt Number of your excellent Journal, where his experiment on the effect produced on a thermometer by a blaſt of air from a pair of bellows, directly contradicts Dr. Black's aſſertion, that "the thermometer is not in the leaſt affected by it."

M. Winter found the air from bellows gave out heat.

That there is great truth in Dr. Black's general ſtatement of the fact, of a blaſt of air cooling a body warmer than itſelf, by affording a continued ſeries of freſh ſurfaces to carry off the caloric, I have no doubt, and that it ſhould have an equal effect in warming a body colder than itſelf, ſeems equally evident, or by ſupplying the colder body with caloric. But in the caſe of the thermometer being raiſed four degrees, (as ſtated in Mr. Winter's experiments) we are not told that it was of a temperature lower than that of the air of the room

Queſtions reſpecting theſe facts.

room. How then, Sir, are we to reconcile the refult of your correfpondent's experiment with Dr. Black's affertion, mentioned above?—Are we to fuppofe the blaft of air to have actually acquired an increafe of temperature, and if fo, how has it acquired it? I hope your correfpondent (fhould this ever reach his ears) will not imagine I doubt the accuracy of his experiment; my only object is, the clearing up a circumftance, which at prefent is to me at leaft, not by any means fatisfactorily accounted for. To whom then can I better apply, than to you, if indeed I may venture to hope you may think the object worthy of your confideration? Whether that fhall prove the cafe or not, I muft always feel (in common with thoufands of others) the benefit you confer on the fcientific world, by the eafy means of communication of knowledge to the public, which your Journal affords,

I have the honour to be,

Sir,

Your obedient Servant,

K. H. D.

Tunbridge,
January 19, 1806.

P. S. I do not underftand how the fuppofed greater capacity of a vacuum for caloric explains the facts, whether of the rife of the mercury in the thermometer, or the melting of the ice.

Obfervations on the preceding Letter, by W. N.

It is defirable that the experiments fhould be repeated.

WHEN a queftion arifes concerning the difagreement of facts, the procefs obvioufly indicated is to repeat the experiments; in order that it may be feen what circumftances may have tended to produce miftake, or what may have been the real difference between operations fuppofed to be the fame. Though I have not had an opportunity of doing this, I have neverthelefs thought it proper to make a few remarks.

Agitation enables a fluid to gain the common temperature more fpeedily.

When a body is immerfed in the air, or in any other fluid differing from itfelf in temperature, the body will acquire the common temperature more fpeedily (that is to fay, it will be heated or cooled more quickly) by agitating the fluid, than if it were left undifturbed;—and this for the plain reafon, that more of the particles at the original temperature will come into int contact with it in the latter than in the former case. These remarks support and explain the facts noticed by Dr. Black and Boerhaave. Agitation of the air is merely supposed, and not that it shall be either condensed or rarified. Many facts concur to shew, that the capacities of elastic fluids for heat are encreased by rarefaction, and diminished by condensation; proofs of which we have by experiments in the air-pump and condenser, and in the late experiments of explosions produced in the chamber of the condensing syringe. If we attend to this law, we must infer that the air in a pair of bellows, being suddenly compressed by a force perhaps equal to one twentieth of an atmosphere or more, will acquire an increase of temperature; and if in this disposition to give out heat, it be made to rush against the ball of a thermometer, it will heat the mercury, and cause it to rise in the tube. Now, in order to reconcile both the results of Mr. Winter, and of Boerhaave to truth, we must recollect that bellows, like the unfortunate traveller in Esop's Fables, can blow hot and cold at the same time. If the thermometer be held very near the aperture, the warm air will heat the mercury; but if it be held at a greater distance, where the warm air has become plentifully mixed with cold, the effect of its temperature may be altogether inconsiderable, while that of the agitation continues to be effective: that is, to say, the thermometer if already at the common temperature, will neither rise nor fall; if it be already hot the steam will cool it; or if cool the steam will heat it. Thus it is, to return to our traveller, that we breathe upon our fingers held close to our mouth when we mean to warm them; but when we wish to produce cold, we hold the subject at a distance, and blow at it.

Air is heated by condensation.

Whence the rear blast of bellows will be hot, and the remoter will act only by its motion.

As the thermometer falls in the pneumatic vacuum, I suppose there may be some mistake in the postscript.

XVIII.

Account of the Performance of the patent Ship Economy at Sea, in a Voyage to the West India Islands, and of some Improvement in the Tackle aboard, proved of great Utility. By Mr. J. WHITLEY BOSWELL.

To Mr. NICHOLSON.

DEAR SIR,

Description of the ship's construction has been published in a former number of this work.

AS in a former number of your Journal *, you favoured me by inserting a description of the construction of the ship Economy, built according to my patent, I hope you will also admit the following account of her performance at sea, and of some other matters; of considerable utility to naval concerns.

The subject proper for the Journal as containing an account of an experiment in the arts important to the nation, and on a great scale.

Your Journal is principally devoted to the furtherance of the most useful of all knowledge, that of experiments in Philosophy and the Arts. And to a nation which like this depends on its shipping for most of the many advantages it enjoys over the rest of the world, what experiments can be more important, or ought to be more interesting, than those which concern this subject?

The experiment which has been made on this occasion is entitled to a farther superiority over other usual experiments, an account of the large sum of money required for conducting it, which altogether rather exceeded 5000*l.* and on this occasion it is but justice to mention the spirit with which Wm. Lushington, Esq. of this City, and Richard Griffith, Esq. of Dublin have came forward to assist in making this experiment, whose property the ship principally is, (my share of it being comparatively small to theirs); to those gentlemen this country is chiefly indebted for proving a matter of great utility to its naval concerns, and which sooner or later must be of the greatest advantage to it, when the plan comes into use, though the spirit of the times may defer this period until it shall cease to be of any benefit to us, and others may reap the profit of these gentlemens public spirit and my labour and study; but as I waited till I should have the proof of actual experiment to add, to that of a theory (which though founded on un-

Gentlemen who have assisted in this experiment.

The plan must be of great use to the nation when adopted,

* Vol. IX. p. 166.

2 erring

erring principles, and of which each part had been often proved in detail before, it could not be expected to convince those whose pressure of business, or want of taste for such studies, deprived of time, or inclination, or made it too great a labour to attend to its demonstration in any other way) I shall hope now, (that my exertions to bring this plan of ship-building into the notice it deserves, when its sufficiency, strength and security is,) supported by actual and severe proof, will meet with a fair and candid consideration, from the direction of our navy, and those whose commercial pursuits lead to employ vessels of great burden.

hopes that the success of the experiment will accelerate this period.

The chief advantage of this method of ship-building is, that it enables the builder to use timber of much less cost, and vastly more easy to procure, with strength and stability superior to the old method, in proportion to the quantity of timber, and to dispense with knee timber entirely.

Economical advantages of this method of ship-building, cheaper timber, more easily procured, may be used in it.

In a national point of view this method is still of greater benefit; for as it admits of timber of fifty years growth to supply the place of that of one hundred, not only the forest lands may be made to produce timber for double the number of ships for our navy in a given time, but private gentlemen would be also induced to plant more timber for this purpose, from the superior profit they could in this case make of their plantations, and the hope it would give them of being able to receive the fruits of their labour during their own lives, which at present can only be expected to be reaped by their grand children.

Its national advantages, the forests could supply double the quantity of the timber wanted in this plan in a given time, more timber would be planted if it was adopted; oaks of fifty years, have more serviceable timber in proportion than those of 100, and occupy only one fourth the ground.

An oak of fifty years growth has also a much greater quantity of serviceable timber in it, in proportion to its age, than one of an hundred years, and four times the number of them at least can stand and flourish at one time at the same extent of ground; so that the public would be benefited by the adoption of the plan every way; for while timber would thus be rendered more plenty, those who prepared it for market would also obtain a greater profit.

Hitherto the price of timber for the navy has been attempted to be kept down by arbitrary regulations, which tended to encrease its scarcity; at last, notwithstanding every effort, the price and scarcity have encreased so much that our government have been forced to the expedient of partly relying on a foreign country for the continuation of the navy; and to depend

The scarcity of ship timber has compelled this country to build ships of war in Russia; danger of this expedient.

depend on the dock yards of Ruſſia for the bulwark of the Britiſh nation, for the defence of its liberties, and of its political exiſtence, and this at a time when our crafty and implacable foe has got poſſeſſion of nearly all the foreſts of the reſt of Europe, and is making the moſt prodigious exertions to out-number our navy.

Should induce the trial of the patent plan, in doing which there is no riſk, as its ſufficiency has been proved.

If my plan of ſhip-building tends in ſo great a degree to diminiſh thoſe difficulties, and even dangers, as is ſtated above, is it not worthy of a trial at leaſt, even if ſome riſk was run in that trial? but when no riſk is run, when the plan has been proved, the moſt ſcrupulous economiſt of the public wealth can ſtart no objection to that trial of it in the navy, that the public neceſſity for ſome expedient to ſupply timber for its uſe ſo loudly calls for.

No public money required to make experiments, they have been already made at the expence of the owners of the ſhip.

We aſk no drafts on the public ſource to try experiments on the ſubject, theſe have been already compleatly made at our own expence, and all we demand is our country to condeſcend to reap the fruit of our exertions; if ſhe does, we ſhall rely on her generoſity to recompence us, convinced that ſhe will have ample proof that we have deſerved it; but ſhould this not be the caſe, we will not reſt contented with having diſcharged our duty, in doing the moſt we could to ſerve her; which if we ſhould be ſo happy as to effect, we will never regret our trouble or coſt.

The performance of the ſhip at ſea cannot be miſtated on account of its publicity.

Having thus ſtated the claims which the ſubject has to public attention, I ſhall proceed to relate the performance of the ſhip at ſea, which, as ſhe failed in company with a large convoy both out and home, is a matter of too public a nature to admit any miſtatement I might wiſh to make, which God knows is far from my deſire.

On the 22d of Auguſt, 1804, The patent ſhip Economy weighed anchor off Graveſend, with but a ſmall cargo aboard, as is uſual for ſhips outward bound to her deſtination, and ſet ſail on her voyage to Trinidad and Grenada; and on the 14th October following arrived at Grenada; her performance on this voyage is beſt ſtated in her Captain's own words, in the following extract from a letter to Wm. Luſhington, Eſq. London.

SIR, *Grenada, Oct.* 15th, 1804.

The Captain's letter relative to the voyage out.

I have the pleaſure to inform you of the ſhip Economy's ſafe arrival here yeſterday evening. We had a fine paſſage, and

and had but one gale of wind: The ſhip performs as well as it is poſſible for a ſhip; is remarkable eaſy at ſea, ſteers and ſails well, and is perfectly tight. In the gale of wind the Epervier man of war ſprung her foremaſt; the Robert Aylward ditto; a brig, Maſter or brig named Swinger, loſt both topmaſts and parted convoy in lat. 14. 30 N. Our ſhip behaved extremely well and never ſtrained a rope yarn.

Ship ſteers and ſails well, is remarkably eaſy at ſea, perfectly tight in a heavy gale, in which other ſhips ſuffer much, ſhe meets no accident;

(*Signed*) ALEXANDER SMITH.

From the period of this letter ſhe remained at the Weſt India iſlands until the 23d of July 1805; being detained there the greateſt part of that time by the arrival of the French fleet, which was afterwards chaſed back to Europe by the gallant and ever to be regretted Lord Nelſon; from the 23d. of July, when ſhe ſailed for England, to the 29th of Oct. when ſhe caſt anchor off Portſmouth on the Mother-bank, ſhe experienced a ſeries of ſevere weather and violent gales of wind, in which ſome of the fleet with which ſhe returned foundered, and others were obliged to bear away for America for ſhelter. The remarkable bad paſſage home of the Leeward iſland fleet, of which ſhe was one, is too well known to need much deſcription: all ſeamen muſt be ſenſible that three months toſſing on the Atlantic ocean in ſuch hard weather, beating up againſt contrary winds, to a veſſel as deeply laden with ſugar as the ſkrews could compreſs it into her, muſt have been a moſt ſevere trial, and that if ſhe had a ſingle weak part, or defective principle in her conſtruction, it muſt have given out in that time: but while moſt of the other ſhips of the fleet met with more or leſs damage both to themſelves and their cargoes, ſhe bore through all without the ſmalleſt accident, and brought home her ſugar perfectly dry and ſafe; which was not compleatly diſcharged until Jan. 1806 (on account of her detention at Portſmouth, through contrary winds from whence ſhe ſhe did not get to London before the 27th. of Nov. on which day ſhe hauled into the Weſt India dock,) or this account would have been made public before. A further proof of the ſtability of her frame work, is her taking the ground with a full cargo on board without any accident, as may be ſeen more particularly in the following account of her performance home which I received from her Captain.

delayed at the Weſt Indies by the French fleet, returns home, experiences violent gales, and a tedious paſſage of three months, ſome ſhips of the fleet founder through ſeverity of weather. Why this ſevere trial is a ſufficient proof of her ſtability, when deeply laden,

took the ground without injury at Trinadad.

DEAR

DEAR SIR, January 17, 1806.

"IT is with pleasure that I have leisure to inform you of the performance of the patent ship Economy, during the voyage under my command.

Captains account of the voyage home, In a violent gale the ship performs extremely well, and is a good sea boat: two ships founder in this storm, one abandoned, another large ship rendered unmanageable, and taken in tow, and several others much damaged.

"On the fifth and sixth of September last, latitude 37, 34 N. we experienced a very heavy gale of wind, with an heavy cross sea, occasioned by the wind shifting to different points of the compass suddenly, and blowing with extreme violence: during the whole of the gale, the Economy behaved as well as I ever experienced a ship to do, and much better than could have been expected for so small a ship; in fine, she is as good a sea boat as ever put keel in salt water. During the gale, two ships, it is supposed, foundered; after the gale one was abandoned as not tenable, should another gale of wind come on: the Prince of Wales, a ship of 300 tons, had every thing washed from her deck: The Princess of Wales, a ship of the same size, broke her rudder, and was left in tow of the Hyæna sloop of war. Several other ships met with considerable damage, which proved undeniably the violence of the wind. Notwithstanding the lumbered state of the Economy, we lost nothing off deck, and I don't think there was a ship, large or small in the fleet, that made better weather: she did not sail so fast coming home as going out, but that is easily accounted for, when we consider she was not coppered, and was out fifteen months on a wooden sheathing, with barnacles as long as your finger on her, and the bottom resembling a rock; and was besides laden as deep as she could stow. She works and steers amazingly well. I would not wish to change her if she had been larger, but being only 200 tons, she is too small both for my interest and the West India trade.

The Economy meets no accident, and is very weatherly: tho' deep laden; has a foul bottom which impedes her sailing, she works and steers well.

"The ship has been perfectly tight all the voyage, although we had a very tempestuous passage, and likewise ran her on shore, *sugar loaded*, under the batteries at Trinadad, to prevent her falling into the hands of the French, as we supposed, where we lay for twenty-four hours, until we discovered that it was Nelson's fleet. In my opinion she is one of the strongest ships in the river Thames of her size.

She remains perfectly tight after the severe passage, though run aground at Trinadad with a full cargo of sugar;

"The new iron slings and other iron work on the yards exceed my most sanguine expectation, I have seen the ship covered with flashes of lightning when at Trinadad, and never experienced

is a very remarkable strong ship,

experienced the leaſt injury from ſo much iron being about the yards, owing to the precaution which I took of ſerving the iron work and paying it with pitch, which I think ſerved as a non-conductor. I have a higher opinion of iron work than ever I had, and think the iron rigging in the plan we uſed to talk about while the ſhip was building, would anſwer to admiration, and might be the means of preſerving the maſts of men of war, when in action, as being leſs liable to be cut with ſhot. When I can manage it, I mean to rig the mizen maſt of a ſhip wholly with iron, to give it a trial. When I examine the bottom, I will give you my opinion of the pieces of ſheathing ſteeped in your preparation to prevent the worms from deſtroying the bottom. The large rollers which you had let in beneath the hawſe holes for the cables to work on, were of very great benefit, and I think ſaved us the labour of two men in weighing anchor, they alſo prevented the wear of the cables very much, and were greatly liked by the ſailors, as making the purchaſe more lively,

Of the iron ſlings to her yards, and captain's high opinion of the uſe of iron work in rigging.

State of experiment on part of the ſheathing will be attended to,

great uſe of the large rollers for the cables.

Your's very ſincerely,

ALEX. SMITH."

The iron ſlings which Captain Smith mentions, were on a plan of his own, and different from thoſe uſed in men of war, in not requiring above three or four feet of chain for each yard, and ſerved merely to ſuſpend the yards from the point of the tops; which method greatly ſaved the wear of the maſts, and permitted the yards to work more freely. Iron ſtraps were alſo uſed to moſt of the blocks inſtead of hemp.

Further account of the iron ſlings,

The rollers for the cables were about fourteen inches long and eleven in diameter, and worked on iron gudgeons about two inches in diameter, in braſs ſockets. The rollers which have been hitherto uſed for this purpoſe, were generally much too ſmall, ſeldom exceeding the diameter of the cable; which diminiſhed ſize both increaſes the friction and injures the cable, from the ſmallneſs of the nip which they occaſion; or, in other words, from the acuteneſs of the angle at which the cable is forced to bend in paſſing over them.

and of the cable rollers,

In concluding this account I beg leave to mention, that I could, in building another ſhip, greatly diminiſh the ſpace neceſſary for the tranſverſe forms uſed in my plan, by ſetting them farther aſunder, and forming them of iron, which method

Iron tranſverſe frames may be uſed in future in this place, to ſave room.

—and the fore and aft ribs fcarped in a more economical method.

method is fpecified in my patent, and that I could alfo make a great faving in the timber ufed in the fore and aft ribs, by a method of fcarping them, alfo within the limits of my fpecification. Experience has fince convinced me of the fuperiority of both thefe methods, of which I had fome doubt when I built the Economy, or they fhould have been ufed in her.

It may feem paradoxical to affert that iron is oftentimes cheaper than wood in fhip building, when it can be ufed: but a plain proof of this exifts in the bow of the Economy, of which the three lower breaft-hooks are iron of confiderable fubftance, and yet coft lefs individually than any of the wooden ones above them, though thefe are of no extraordinary girth, or of much curvature.

The Economy will remain a few weeks at the London Docks for infpection.

The Economy will be a few weeks in the London Docks, where fhe has now moved, for the infpection of the public, and where all gentlemen who are interefted in fhipping concerns may fee her conftruction; and thofe who examined her previous to her failing, may convince themfelves that I have exaggerated nothing, as to the found ftate in which fhe has returned from her tempeftuous voyage.

Dear Sir,

Your very humble fervant,

J. WHITLEY BOSWELL.

XIX.

Experiments on the Torpedo. By Meffrs. HUMBOLDT *and* GAY LUSSAC. *Extracted from a Letter of M. Humboldt to M. Berthollet; dated Rome, 15 Fructidor,* Year 13 (Sept. 2, 1805.)*

THE curious theory with which Volta has enriched the fcience of natural philofophy, on the fubject of electric fifh having been received as authentic by many naturalifts, renders the phenomenon of the Torpedo worthy of farther inveftigation. You know, my dear friend, what was our impatience to procure thefe fifh, and will perhaps be furprifed that fo much time fhould elapfe without having heard from us on the

* Annales de Chimie, Vol. LVI.

subject,

subject. At Genoa, we perceived some; but we were then without our instruments. At Civita Vecchia we sought them in vain. But during our stay at Naples we frequently procured some very large and lively ones. In this letter you will find detailed the experiments made by M. Gay-Lussac and myself on the powers of this fish (*Raja-torpeda* of Linneus). M. de Buch, a German mineralogist, well acquainted with all the branches of physical science, was witness to our proceedings. I send you the results, giving simple facts, unmixed with theoretical speculations. Our experiments were chiefly directed towards the discovery of that state of the torpedo when it was least capable of exerting its power upon the human frame. This power has been generally described as electrical; but the sensation produced by it is materially different from that caused by the discharge of a Leyden phial.— Having no other book by us besides the work wherein Aldini * combines the researches of Geoffroy with those of Spallanzani and Galvani, it is not to be expected that we should compare our experiments with those which may have been previously made by other philosophers.

The torpedo found at Genoa and Naples, but not at Civita Vecchia.

The shock of the torpedo feels different from that of electricity.

1. Though the strength of the torpedo is far inferior to that of the gymnotus, it is equally capable of causing painful sensations. A person much accustomed to electric shocks, can hardly sustain that of a lively torpedo of four decimeters (16 inches) in length. The animal acts under water, and it is only when it loses strength that the fluid impedes its action. In this case, M. Gay Lussac observed that the shock is not perceptible till the fish is raised above the surface.

Powers of the torpedo inferior to those of the gymnotus of S. America. Shock of the torpedo more violent than that of electricity. It acts under the water,

2. I observed, when in South America, that the gymnotus gives the most violent shocks, without any exterior movement of the eyes, the head, or the fins: it appeared as tranquil as a person when passing from one idea to another, or from one sensation to another. Not so the torpedo: We observed a convulsive movement of the pectoral fins, each time it gave a shock, which was more or less violent according as the surface was larger or smaller wherein the contact took place.

—and seems to use more effort than the gymnotus.

3. The powers of the torpedo and gymnotus cannot be excited at pleasure, as we should discharge a Leyden phial or a conductor.

Shocks from the torpedo and gymnotus cannot be obtained but by irritating the animal.

* Memoires sur la Torpille, dans l'Essai sur le Galvanism, Vol. II. p. 61.

conductor. A shock is not always felt on touching an electric fish; it must be irritated before it will give the shock. This action depends on the will of the animal, whose electric powers perhaps, are not kept constantly charged; yet it can recover them with wonderful celerity, as it is capable of giving a long succession of shocks.

The shock obtained by a mere touch with the finger,

4. The shock is felt (the animal being disposed to give it) as well on touching with one finger a single surface of the electric organs, as on applying the two hands to the two surfaces, the upper and under, at once. In both cases it is immaterial whether the person applying his finger or his two hands, be insulated or not.

—but the contact must be direct. Metals seem to be non-conductors of the shock of the torpedo.

5. When an isolated person touches the torpedo with a single finger, it is indispensible that the contact be immediate, as no shock will be felt if a conducting body (of metal for example) be interposed between the finger and the organ of the fish.— For this reason, the animal may be touched with impunity by means of a key, or any other instrument of metal.

6. M. Gay-Lussac having made this important observation, we placed a torpedo on a metal dish, with which the inferior surface of its organs were in contact. The hand which supported this dish experienced no shock, whilst another isolated person irritated the animal, whose convulsive movement of the pectoral fins indicated a most violent emission of the electric fluid.

Experiments which shew that they conduct.

7. When on the contrary, a person held the torpedo in a metal dish in his left hand (as in the preceding experiment), and with his right touched the superior surface of the electric organ, he experienced a smart shock in both arms at the same moment.

8. The same was felt, on placing the fish between two metal plates, whose edges were not in contact with each other, and applying the two hands at once above and below them.

9. But if the edges of the metal plates be suffered to touch each other, no shock will be felt in either arm. The communication between the two surfaces of the organs is, in this case, formed by the plates; and the new connection arising from the contact of the two hands with the plates is without effect.

The organs of the torpedo have no influence on the electrometer.

10. The most sensible electrometer manifested no electrical tension in the organs of the torpedo; in whatever way it was applied, it was not in the least affected; neither, on directing it

it towards the organs, nor in insulating the fish, covering it with a metallic plate, and making a communication between this plate, by means of a conducting thread, and the condenser of Volta, was there any indication (as with the gymnotus) that the animal affected the electric intensity of surrounding bodies.

11. As electric fish, when healthy, exercise their powers as forcibly beneath the water as in the open air, we were led to examine the conducting properties of this fluid. Several persons formed a chain of hands between the superior and inferior surfaces of the organs of the torpedo: the shock was not felt until they had wetted their hands. The action was not intercepted when two persons supported the torpedo with their right hands; and instead of holding each other's left hand, they each plunged a metallic rod into water placed upon an isolated body.

Examination of the conducting powers of water.

12. By substituting flame in lieu of water, the communication was destroyed, until the rods touched each other in the flame.

Flame does not conduct the shock.

13. It must, however, be observed, that in water, as in air, the shock was not perceptible without an immediate contact with the body of the electric fish; the least possible intervention of the water prevented it. This fact is the more remarkable, as it is known that in galvanic experiments, where the frog is immersed in water, it is sufficient to direct the silver forceps towards the muscles to cause a contraction, though a body of water be interposed, equal to one or two millimetres in thickness, or about one-twentieth of an inch.

No shock can be obtained without immediate contact with the fish.

These, my dear friend, are the principal observations which we have made on the torpedo. The experiments, No. 4 and 10, prove that the electric organs of these animals are not susceptible of any intensity or excess of charge. Their action may rather be compared to that of a combination of Leyden phials, than to the conductor of Volta. Without communication no shock could be felt; and having experienced the power of the gymnotus through very dry cords, I imagine, that where I have been affected by this powerful animal without direct contact, it had been occasioned by some deficiency in my insulated state. If the torpedo act by poles, that is by an electric equilibrium which possesses a tendency to replenish itself, experiments 5 and 6 seem to prove that these poles exist near each

Organs of the torpedo not susceptible of any excess of charge.

Doubt whether the shock of the gymnotus can be felt without actual contact with it.

Torpedo supposed to act by an electric equilibrium, the opposite state being very near.

each other, on the ſame ſurface of the organ. The ſhock is felt on merely touching the ſurface with the finger. A plate interpoſed between the hand and the organ, (*Exp.* 6,) re-eſtabliſhes the equilibrium, and the hand which ſuſtains the plate is not affected, becauſe it is placed beyond the current. But if we ſuppoſe an heterogeneous number of poles upon each ſurface of the organ, whence does it ariſe, that, in covering theſe ſurfaces with two metal plates, whoſe edges do not touch each other, and placing the hands on theſe plates, the equilibrium ſhould be found in the arms? Why, it may be aſked, does not the poſitive electricity of the inferior ſurface ſeek at the moment of exploſion the negative electricity of the next or neareſt pole, but rather ſeek it in the ſuperior ſurface of the electric organ? Perhaps theſe difficulties may not be inſurmountable; yet the theory of theſe *vital actions* well deſerves attentive reſearch. Geoffroy has proved that thornbacks, who give no ſigns of electricity, are furniſhed with organs analogous to thoſe of the torpedo. The leaſt injury on the brain of the torpedo deſtroys its electric powers. The nerves are no doubt concerned chiefly in theſe phenomena; and the phyſiologiſt who ſhould admit the power of vital actions, might with ſucceſs oppoſe the theory of the naturaliſt, who would endeavour to explain all by the contact of the albumino-gelatinous pulp of the nervous laminæ wherewith nature has endowed the organs of the torpedo.

Objections to this notion.

Conſiderations of theory.

SCIENTIFIC NEWS.

Prizes propoſed by the Univerſity and Academy of Wilna, in June, 1805.

CLASS OF SCIENCE AND MEDICINE.

Firſt Prize.

To determine whether ſaccharine ſecretions take place in other organs beſides thoſe affected in diabetes mellitus.

BESIDES the diabetes mellitus of the authors on medicine, are there any other diſorders peculiar to man, which, according to experiments well aſcertained, produce in different organs a ſecretion ſimilar to ſugar, ſufficiently abundant to finally occaſion conſumption? And what are theſe diſorders?

5 In

In a note on this fubject it is recommended to examine for faccharine matter, the fluid fubftance of colliquative fweats; that produced in the *fluxus cœliacus*, and in the pituitous confumption from lungs, which after death are not ulcerated; and the milk of women afflicted with the *gallactirhœa*.

Second Prize.

What are the true characters and the caufes of the malady, which although not exclufively appertaining to Poland, is however called the *Plica Polonica?* Are there any means of curing this difeafe more fuccefsful than thofe hitherto employed? and what are thefe means?

To afcertain the true caufe and cure of the Plica Polonica.

Third Prize.

What are the principal maladies of vegetables? And what is the true analogy between them and thofe of animals?

Relative to the diforders of vegetables.

CLASS OF NATURAL PHILOSOPHY AND MATHEMATICS.

Prize.

Suppofe a canal, through which a certain quantity of water m, flows in a given number of feconds, through a tranfverfe fection of a given depth and breadth, terminated by the two banks: If on this fection a dam is conftructed, at the top of which an opening is made for the water to pafs, of given dimenfions; it is demanded according to what law the water, elevated by the obftacle which the dam prefents, will be forced to rife not only at the dam, but backwards along the canal.

The law is demanded, by which water rifes in a canal behind a dam, at the top of which a given opening is made.

Formulæ are required fufficiently general, to be applied not only to the quantity of water m, but to any other $m+x$. Experience not exactly agreeing with the theory, the neceffary corrections muft be made to the formulæ, and proofs given from facts and obfervations, fhewing how nearly they approach the truth.

CLASS OF MORAL AND POLITICAL SCIENCES.

Prize.

As the fciences of natural philofophy and mathematics make daily advances, and are enriched with new difcoveries, it is demanded—

Qu. why moral ſciences do not make the ſame progreſs as the phyſical? —If they can be farther improved? —What are the bounds to their perfectibility? —What are the beſt methods to attain this point?

1ſt. Why the ſame does not take place in the moral ſciences?

2d. Whether among the different branches of theſe ſciences, there be any capable of a farther degree of perfection? And what theſe are?

3d. To what degree are they of this nature? And what are the limits to their farther improvement?

4th. What are the moſt proper methods to advance the moral ſciences to this boundary of perfection?

It is deſired that the diſcuſſion of this ſubject may be conducted ſo as to preſent reſults, which may contribute to the perfection of that theory of Legiſlation, which is moſt conformable to the nature of man.

Second Prize.

Tenets of Adam Smith and Dr. Queſnay?

To determine (by making an analyſis of political economy) what are the points in which the leading notions of Adam Smith and Doctor Queſnay agree, and in what they differ, or are oppoſite?

This examination muſt neceſſarily produce reſults uſeful to the progreſs of political economy.

Amount of prizes, and laſt days for receiving memoirs.

The prize for each of theſe queſtions is 100 golden ducats of Holland (46*l.* 5*s.*); and the laſt day for the reception of memoirs on medical ſubjects, the 31ſt Auguſt, 1807; and for the others, the ſame day and month in 1806.

Conditions to be obſerved by the Candidates.

To each memoir ſent in muſt be attached a ſeparate and ſealed note, containing the title of the work, and the name and addreſs of the author: This note will only be opened by the Univerſity if the work ſhall obtain the prize.

Memoirs to be written in Latin, French, or Poliſh.

The memoirs muſt be written legibly, in Latin, French, or Poliſh languages. The packet ſhould be addreſſed to the Rector of the Univerſity of Wilna, and addreſſed to one of the bankers of that city, MM. Keyſer or Karner, that it may go free. The Rector will give a receipt to theſe bankers.

The Univerſity ſhall not be obliged to return either the memoirs or the drawings ſent; but the authors will always be permitted to take copies of them.

Conditions relative to property, copy right, &c. of the memoirs.

The Univerſity engages not to print any of the works ſent them, without permiſſion of the authors; but the authors may, at any time, print them if they think proper.

The

The diſtribution of prizes ſhould take place before the termination of the years in which they are to be determined. The prizes adjudged ſhall be publiſhed in the gazette. Time of diſtributing prizes.

The author ſhall receive his prize from the adminiſtrative committee of the Imperial Univerſity of Wilna, either in perſon or by deputy. The prize will be at his option, either a gold medal or 100 golden ducats of Holland.

The Profeſſors and honorary members of the Univerſity of Wilna, cannot be candidates for the prizes. Profeſſors of Wilna not to be candidates.

Revived Precipitates from alkaline Solutions of metallic Oxides.

M. Klaproth, a little before his deceaſe, diſcovered that the ſolution of the metallic oxides in the alkalis, are as eaſily precipitated in their metallic ſtate, by the other metals ſoluble in the ſame alkalis, as are the acid ſolutions of theſe metals by phoſphorus: He has made a very ingenious application of this proceſs to the analyſis of tin ores, according to the method which is deſcribed in his *Beitraege:* In this operation tungſtein is ſeparated from tungſtate of ammonia, by the addition of zinc, in the form of black flakes. Alkaline ſolutions of metallic oxides precipitated, in the metallic ſtate, by other metals.

Experiments on falling Bodies, by M. Benzenberg.

M. Benzenberg, profeſſor of phyſic and aſtronomy at Duſſeldorp, publiſhed, ſome months ago, twenty-eight experiments made with balls well turned and poliſhed, which were made to fall from a height of 262 French feet: At a medium they produced five lines of deviation towards the eaſt, according to the determination of the plumb-line, and the theory gives four lines ſix tenths. Theſe experiments were made in the coal-mines of Schebuſch; they are an additional proof, if it were neceſſary, of the rotatory movement of the earth, of which no one now doubts. The laſt experiments, made at Bologna by M. Guglielmini, gave nearly the ſame reſults. A falling body deviates four lines ſix tenths eaſt, after paſſing through 262 feet.

Geography.

Great pains are taking in the conſtruction of an accurate map of Holland: The ſame precautions have been uſed in this buſineſs as in the meaſurement of the degree of the meridian. M. de Zach has publiſhed in his Journal the chart of the triangles which have been completed: They are joined to Map of Holland.

thoſe

thofe which M. Delambre made for the great meridian; and the diftance from Dunkirk to Montreal has been taken for the firft bafe. When the triangles are finifhed, a bafe will be meafured towards the north, to ferve for the verification of the work. The Batavian republic have entrufted the direction of this map to Colonel Krayenhoff.

Chart of the White Sea, by General Kautauzoff.

Some months ago there appeared at Peterfburgh a very fine hydrographical chart of the White Sea, of which General Kautouzoff is the author: Many naval officers have worked under his direction for four years, in collecting the materials neceffary to compofe this chart. The coafts of the White Sea, of its gulphs, and of a part of the Northern Ocean, have been laid down trigonometrically. The depths have been carefully founded; and fix of the principal points of the coaft have been determined by aftronomical obfervations.

M. Lartigue's map of America in relief.

M. Lartigue having been engaged for thirty years in conftructing, at the marine depot (of Paris), a large and beautiful map of America in relief, has at length completed it. It is faid that the mountains, and the iflands, and the tints of the fea, are all exhibited in a manner moft capable of interefting thofe who make geography their ftudy.

Expedition of Capt. Lewis up the Miffouri.

Several months ago, Captain Lewis, in America, undertook to afcend the river Miffouri, in fearch of a paffage to the South Sea. Very interefting intelligence may be foon expected from this expedition.

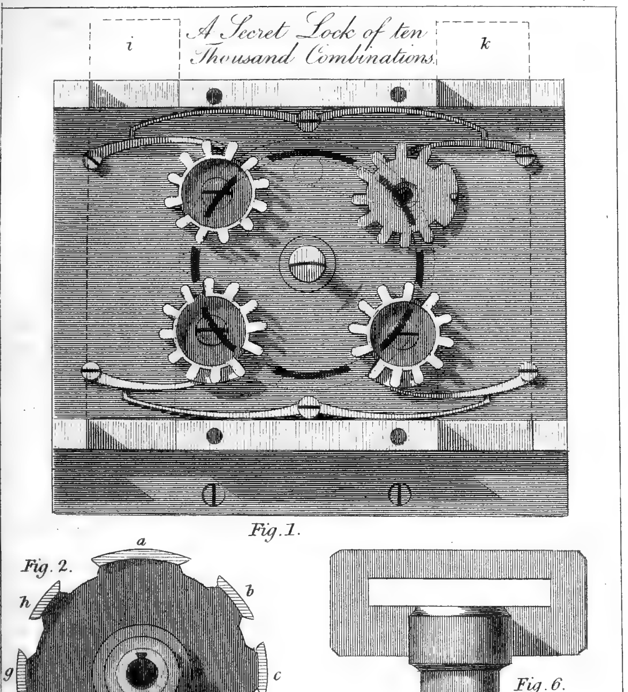
Survey of France.

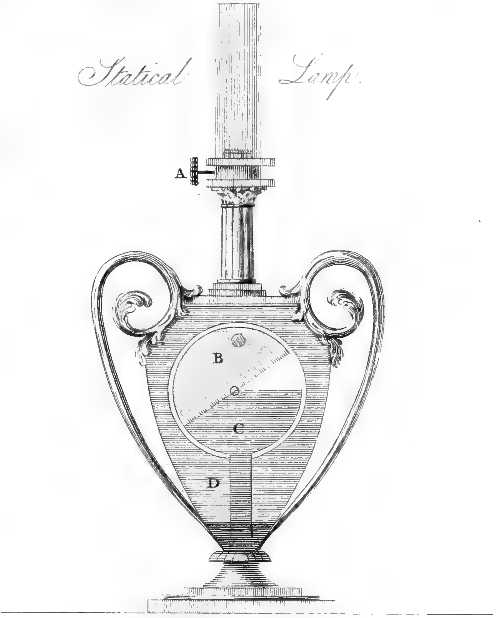
The work of the government furvey, or *cadaftre*, of France, has proceeded with activity; 2000 perfons are employed in it in the 108 departments.

Effect of Heat on Magnetifm.

Magnetifm deftroyed at 700° of heat.

M. Coulomb has publifhed an interefting memoir on the effect of heat on magnetifm. At 200 degrees of heat, two-fifths of it are deftroyed, and the whole at 700 degrees.


A Secret Lock of ten Thousand Combinations.
i
k
Fig. 1.
Fig. 2.
a
b
c
d
e
f
g
h
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.


Statical Lamp.
A
B
C
D


Brane
Moulon
Genisac
Vayre
St Pardon
Caverne
Lile
Girone
Tersac
Asque
Liboinne
Fronsac
Riv. de Perigueux
St Andre
Gironde

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1806.

ARTICLE I.

Experiments on the Temperature of Water ſurrounded by freezing Mixtures. In a Letter from JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR, *Middleſhaw, Jan.* 29, 1806.

MANY philoſophers have turned their attention to the dilatation obſervable in water when cooled below 40 or 41 degrees of Fahrenheit's ſcale, and alſo to the no leſs ſingular fact of water retaining its fluidity for a conſiderable time when expoſed to a freezing mixture, without being agitated. But one circumſtance, relating to the latter phenomenon, appears to have eſcaped the notice of them all; which in all probability will prove of ſome importance to both enquiries.

Expanſion of water in cooling below 41 deg.

We know from common experience, that when a hotter and colder body come into contact, the former will loſe and the latter acquire heat, until they arrive at an equality of temperature. The frequent opportunities every one has of making this obſervation have authoriſed it to paſs for a general rule; hence it has been concluded, that water in a ſtate of reſt may be cooled many degrees below the freezing point,

Explained by a developement of the conjecture that ice may be formed in minute cryſtals at that temperature.

and ſtill remain fluid. For my part, I adopted the maxim without heſitation, until the peruſal of Dr. Hope's paper, given in the ſupplement to your laſt volume, led me to reaſon in the following manner on the ſubject.

When water is expoſed to a freezing mixture, thoſe particles of it which are in contact with the ſides of the veſſel, are ſoon reduced to a temperature lower than the point of congelation; in conſequence of this, they are probably converted into minute icicles, which impart a quantity of caloric at the moment of their formation to the ſurrounding water, thereby preventing its temperature from ſinking below 32°. Theſe inviſible bodies afterwards begin to aſcend ſlowly on account of the diminution in their ſpecific gravity; and while they riſe towards the ſurface of the water, other particles will approach the ſides of the veſſel in ſucceſſion, and undergo a ſimilar transformation. This proceſs would evidently increaſe the volume of the water without reducing its temperature, ſuppoſing it to be ice-cold at the commencement of the experiment; for the hypotheſis reſts on the ſuppoſition that water freezes as ſoon as it is cooled below the 32nd degree of Fahrenheit's ſcale. This gradual increaſe of bulk will explain the appearances deſcribed by my friend Mr. Dalton, who found that thermometers filled with water continued to riſe when expoſed to freezing mixtures, until the encloſed water congealed ſuddenly, and frequently burſt his inſtruments. The reaſon why agitation accelerated the congelation of water thus circumſtanced appears to be this: When the inviſible icicles become very numerous, the leaſt motion carries them in crowds againſt the ſides of the veſſels; where the ſmall quantity of water contained amongſt them cryſtallizes immediately, and cements the whole into a film, adhering to the inſide of the cup. This theory, or hypotheſis, call it whatever you think proper, evidently requires water to be what it really is, namely, a bad conductor of heat; and after forming it, I proceeded to examine the merits of it experimentally, in the following manner.

Experiment. Water at 32° was cooled by a ſurrounding mixture, and became congealed at the ſides by ſtirring, having never ſunk below 32°.

Experiment 1ſt. A ſmall thermometer was ſuſpended at the lower end of a wire, which could be moved in a vertical direction, through a hole in a horizontal bar of wood, fixed over a table for the purpoſe; a veſſel, containing a freezing mixture, of the temperature of 21°, was next placed with

5 its

its centre under the wire; and the bowl of a wine glafs, filled with two ounces of ice-cold water, being then properly placed in the mixture, the thermometer which ftood at 32°, was immediately let down into the water, where it remained ftationary for the fpace of feven minutes. A wire, cooled to the freezing point was now introduced into the glafs, and the water agitated with it; upon which a thick coating of ice formed on the infide of the veffel; but no marks of congelation were obfervable on the wire or thermometer.

Exp. 2. The fame apparatus being ufed, with a mixture, having the low temperature of 6°, the glafs was filled with water of 58°, in which the thermometer fell to 32° in $7\frac{3}{4}$ minutes, by a ftop watch; at which point it remained ftationary five minutes longer. The glafs was then taken out of the mixture, and the water being agitated, lined the upper part of it for about two-thirds of its depth from the brim, with a porous covering of ice, but the remaining part of it was free from all incruftation.

Exp. 2. Water at 58° was cooled by an intenfe freezing mixture. It was brought to ftationary 32° and funk no lower. When taken out and fhaken the top froze.

I will ventnre to infer from the two preceding paragraphs, that we have all been under a miftake in concluding that water may be cooled when at reft many degrees below 32° of Fahrenheit, without congealing; at the fame time we are certain, that it will preferve its fluidity, when judicioufly expofed to great degrees of cold, and dilate at the fame time, as Mr. Dalton has proved. Now as the heat never falls below 32° in thefe experiments, the expanfion of the water in Mr. Dalton's thermometers, placed in a freezing mixture, cannot be afcribed to a lofs of temperature, but muft be owing to fome other caufe, probably to that which has been affigned above. As for agitation, the firft experiment feems to fhew its office to confift in bringing the water, crowded with minute icicles, into contact with parts of the veffel much colder than itfelf, where it is concreted into ice.

Hence water cannot be cooled and remain fluid at temperatures below 32°, as generally fuppofed. The water thermometer muft expand in cooling by fome other caufe.

Exp. 3. To examine this part of the fubject with more care, I formed a cup of caoutchouc, the capacity of which for caloric greatly exceeds that of glafs; or, I believe, that of moft other fubftances. Two ounces of water, a little warmer than melting fnow being poured into this cup, it was placed in a mixture of the temperature of 15°, where it remained eight minutes without giving the leaft indication of a tendency to freeze. The cup was now removed from the mixture,

Exp. 3. Repetition of Exp. 2 very ftrikingly in a cup of caoutchouc.

mixture, and gently ſhaken; upon which long icicles formed in an inſtant, projecting into the water in all directions, from the caoutchouc to which they adhered. This experiment, I have no doubt, might be made a very beautiful one by a dexterous operator, who is in the habit of exhibiting natural appearances to public aſſemblies.

After diſcovering that water will dilate without any change of temperature from warm to colder, at 32°, I began to imagine that the whole variation of expanſion under 41°, might be explained on the ſame principle, becauſe I believe all the experiments relating to the ſubject, have been made in a cooling medium, not warmer than melting ſnow.

Water expands by cooling between 41 and 32 deg. or begins to cryſtalize at the upper term.

In order to try the merits of this opinion, with an inſtrument larger than a common thermometer, I filled a four-ounce phial with water, and fixed an open tube into it, by means of a perforated cork and cement; but this apparatus proved my ſuſpicion to be falſe. For the place of the water being marked on the tube when the temperature was 41°, my bulky thermometer roſe immediately upon being plunged into water of 34°. This fact proves, that water expands by a loſs of temperature between 41° and 32°; or elſe, that this fluid begins to cryſtalize at the upper term; in conſequence of which the lower term, or 32°, is not, properly ſpeaking, the commencement of congelation, but the point at which the cryſtals of water begin to concrete into maſſes by aggregation.

I remain, &c.

JOHN GOUGH.

II.

Account of the Art and Inſtruments uſed for boring and blaſting Rocks; with Improvements. In a Letter from G. C.

To Mr. NICHOLSON.

SIR, *Briſtol, Jan.* 21, 1806.

Improvements in blaſting mentioned.

BY way of appendix to Mr. Cloſe's remarks on the uſe of ſand in ſtemming mines in hard rocks, and his uſeful improvement of the pricker, by making it of copper inſtead of iron, allow me to add two other improvements in the art of blaſting ſtone,

ſtone, which my own experience has proved to diminiſh conſiderably the expence of gunpowder, while one of them, at the ſame time, removes all danger from imperfect priming.

I ſhall alſo, with your permiſſion, as many of your correſpondents muſt neceſſarily be ignorant of the conſtruction of the tools, give you a deſcription of thoſe now in uſe at the village of Shipham, in Somerſetſhire, a village wholly compoſed of men, women and children, who mine after lead ore, calamine, and ochre, chiefly in a lime-ſtone rock; a numerous band of ſome of the ſtouteſt beings in England.

Deſcription of the art as practiſed in the lime ſtone rocks in Somerſetſhire.

Theſe men ſtill uſe the iron pricker, becauſe an accident ſeldom or ever happens to them; owing, I believe, in a great meaſure to their ſtemming with ſpar, and their habit of turning and looſening the inſtrument at every half inch they fill.

The iron pricker uſed without bad effect with ſpar for ſtemming.

The tools they uſe are theſe, *Plate* V.

A. A round bar of iron, bevilled off at one end, of 18 inches long, and of the diameter of half an inch.

B. A ditto, of 24 inches, to follow when the hole in the ſtone is about 12 inches deep.

C. A rod, with a loop for the finger, 25 inches long; at the bottom of which is a round flat plate of iron to draw out the pounded ſtone occaſionally.

D. A pricker, 24 inches long, with a loop alſo, uſed to preſerve a paſſage to inſert the priming ſtraw, while the hole is rammed or ſtemmed with E, the iron rammer, 20 inches long, and which, ſix inches or more from the end, is formed into a conical groove, very open at bottom, in order to enable the miner to ram round the pricker, and alſo that by its ſharpneſs at the end it may the eaſier break to duſt the pieces of ſpar dropped in as faſt as wanted.

Tools and implements deſcribed. A hole eighteen or twenty inches deep and half an inch in diameter is cut by repeated ſtrokes of a chiſſel, the edge of which lies in the diameter of the hole and is ſhifted round between ſtroke and ſtroke.

F. A hammer with a handle and ſtrap, about five inches long; the iron head weighing about four or five pounds, according to the ſtrength of the operator; for ſome have them of ſix or ſeven.

They alſo have by them a bottle of water, to pour occaſionally into the hole, for the wetter it is the faſter the work goes on.

At every ſtroke of the hammer, the miner turns his chiſſel, by which means he works the bottom of the mine in a regular circle, and is enabled to keep his perforation true.

The work is performed wet, and the chipped ſtone ſcooped out with an inſtrument.

When arrived at the depth of 18 or 19 inches, he cleans, and, The charge of powder is an ounce (which is too much). An iron wire called the pricker is put down in the hole cloſe to the ſide, and ſmall pieces of ſpar are dropped in, which are ſlightly rammed and afterwards more firmly. The whole being full, the pricker is drawn, and a wheat ſtraw filled with gunpowder is put down in place of the pricker.

and, as well as he can, dries his mine; then inſerts his charge of gunpowder, often amounting to the unneceſſary quantity of an ounce, and dropping the pricker to the bottom, with its ſide touching the ſide of the mine, he begins by dropping into it ſome lumps of ſpar; and after he has filled up about an inch, begins pounding it round the pricker with his rammer and hammer; tapping gently at firſt, but ſoon beginning to ram very hard, all the while frequently turning and looſening the circular pricker.

When the hole is quite filled, he draws it, by giving ſome gentle ſtrokes on the chiſſel that he has now paſſed through the loop to draw it with.

He then takes the upper joint of a wheat ſtraw, the ſmalleſt he can get, and having ſtopped the fine end with clay, if it has no knot; he afterwards places the other end, cut off very bevel and ſharp, between his ſecond and third finger of the left hand, cloſe to where the fingers join the palm, forming his hand into a kind of baſon, to keep off the wind, and drawing the open end of the ſtraw ſo low between the fingers that he can but juſt prevent it from dropping on the ground; when pouring a ſmall quantity of gunpowder on the orifice, and tapping with his other hand on the ſtraw below, to ſhake it, it ſpeedily is filled.

This ſtraw muſt be 19 inches long for a hole of 18 at leaſt, and a little ſhaved away at the bottom, but not cut open of courſe.

Fire is given by a piece of touch-wood,

When thruſt down to the powder the train is compleat, and our operator laſtly lights a piece of touch-wood, and places it ſo that when all on fire, it ſhall communicate to the train; after which he withdraws to a place out of the line of exploſion, and waits its effect.

—which occaſions loſs of time by its failure and danger, when too rapid.

And here in blowing a well, I found that much time was loſt; for not only does the wind occaſionally blow away the touch-wood before it is all inflamed, but frequently the damp extinguiſhes it. I alſo found there was danger to the workman if it went off too ſoon, which the wind ſometimes occaſions, or his companion is too ſlow in haling him up; and we likewiſe found that when they worked by the day, and *we* found powder, they uſed an immoderate quantity.

Improvements. If a cork be thruſt down the hole or well pre-

To remedy theſe two great evils, I purſued the following plan: the firſt of which was ſuggeſted to me by an ingenious neighbour and both had the deſired effect.

The

vious to ftemming, fo as to leave one inch of windage, lefs powder will be requifite.

The firft experiment I tried was upon a fingle block of limeftone, of about two ton weight. I charged the mine with only the common charge of a mufket, as at K, over which I drove a cork, as at H, leaving one inch, or thereabout, as at I, over which I rammed fpar, as at G, up to the furface of the rock.

The German fungus or amadou is fteadier and more certain than touchwood. The ftraw may be thruft through a hole in this fungus,

I then made a flit in my ftraw train, as at L, and paffed through it, as through a loop, a cut of the German afh-tree fungus; but not liking that, as endangering the lofs of the priming powder, I cut the flit in the fungus, as at N, paffing the ftraw through the flit, and cutting a fmall notch on one fide of the ftraw, as at O. When it was flid down to it, being elaftic, it clofed there, and filled the notch.

—which burns flowly, but with certainty, and is not blown out by the wind.

This match burns flow but fure, and no wind can extinguifh it. A great advantage, as I have frequently witneffed, in making the new and beautiful towing path on both fides the Avon, from Briftol. One hundred men lofe from ten minutes to twenty and more while getting out of the way during the blowing of a mine near the fpot they were levelling, and all owing to the flow burning of the touch-wood match, or the wind blowing it afide.

Account of the fungus.

This German match is, I fancy, pretty well known; it is merely the fungus of the afh-tree, macerated and hammered until it becomes as flexible as a piece of buff leather, and has been called the German match, I believe, from its general ufe on the upper Rhine, where, by its means, habitual fmokers of tobacco can light their pipes in the open air, whatever may be the weather; and as a piece which fcarcely weighs four grains is fufficient to light without danger, the largeft mines, while the article is by no means dear, and always fafe, inextinguifhable, and regular in its burning, nothing can be more ufeful to the practical miner.

Stemming with loofe fand will not (it is thought) fucceed in mines upon a large fcale.

With refpect to fand (which I fee recommended in a Dublin paper of only laft week, as a new difcovery in ftemming), it will not always fucceed, efpecially in thofe great mines of the Clifton blafters, where, often 15 or 16lb. of powder are ufed at a time; but I fhould think if ftopped with a ftiff clay, it would greatly encreafe the refiftance, efpecially if fufficient windage was left over the powder.

The author's experiments were fully fuccefsful.

The experiment I firft tried, as defcribed above, on that plan, tore to pieces, and threw four pieces of my rock to a great

great height, ſhaking and leaving fit for loading, a good cart load in all; while, but for a wall at hand, my Shipham miner, as uſual, deſpiſing novelties, would probably have been wounded, having been with difficulty perſuaded to take that cover at four yards diſtance.

Thus, Sir, I have ſtated what I take to be improvements in this valuable art, and if they afford you or your readers any gratification, I ſhall not regret the trouble of putting them on paper, being always, Sir,

Your grateful reader,

G. C.

III.

Deſcription of a new Parallel Rule, exempt from lateral Deviation; invented by Mr. J. W. BOSWELL; *with an Account of the Imperfections of thoſe already made for the ſame Purpoſe.*

To Mr. NICHOLSON.

DEAR SIR,

Inconveniencies from the lateral deviation of the common parallel rule.

THE common parallel rule of four pieces has been long found inconvenient, on account of the lateral deviation of the moving piece, which cauſes a neceſſity of ſhifting the poſition of the whole rule frequently, when many parallel lines are to be drawn; that, beſides the loſs of time which it occaſions, tends alſo to produce error in the parallelіſm of theſe lines. For this reaſon it is in no reſpect ſuperior to the more ſimple apparatus for the ſame purpoſe, formed by a triangular plane of wood or metal, moved along a common rule; and as this latter is more ſteady, and ſerves for other purpoſes in drawing, it is preferred by ſeveral.

It is not ſuperior to the triangular plane and rule.

Parallel rules as yet contrived to operate without ſide deviation are ſubject to inaccuracy.

Many inſtruments have been contrived to draw parallel lines without being ſubject to the imperfections here ſtated; but all, that I know of, are more or leſs deficient in correctneſs, from requiring an exactneſs in their formation hardly attainable, or from extreme tendency to have this perfection deranged when attained.

The parallel rule with ſliding joints difficult to make exact, and

The parallel rule with croſſing connectors, and two ſliding joints, is ſubject to both the above inconveniences. The leaſt play

play in the ſlides, or deviation in the grooves in which they are to move, muſt alter the parallelifm of the lines drawn by it; and however exact it may be at firſt, the natural wear attendant on its uſe muſt demonſtrably produce theſe imperfections; to which may be added, that the nicety of workmanſhip which it requires, and its complicated form, muſt of courſe render it expenſive.

becomes inaccurate from wear.

The inſtrument formed by a rule ſupported by two ſmall wheels fixed to the ſame axis, which axis is placed ſo as to be parallel to the edge of the rule, is liable to be imperfect, from any difference in the diameters of the two wheels, or ſlight inaccuracy in the poſition of the axis.

This rule is alſo very liable to ſlip on the paper, and is rendered incorrect in its effects by any unevenneſs in the ſurface over which it is moved.

That moving upon two wheels fixed to one axis, has the ſame defects, and is liable to inaccuracy from the unevenneſs of the paper.

The parallel rule, mentioned in your ninth volume, page 212, requires an exact proportion in the length of each of its parts; and as theſe are all of different meaſures, would be liable to error in the firſt formation, on this account; and however exactly made, would, after a little wear, ſoon deviate, on account of the play which this would produce in the joints; the connectors alſo between the two rules, paſſing from different extremities, and leaving long ſpaces beyond the points of ſupport, would thereby occaſion any play in the joints to produce a greater deviation from parallelifm in the lines drawn.

That formed by ſeveral unequal joints difficult to make exact, and liable to become very incorrect in wear from its long projections beyond the points of ſupport.

The apparatus for producing parallel lines formed by the drawing board and normal ſquare, can hardly with propriety be claſſed among the inſtruments here treated of; whatever its accuracy may be, its cumbrous form, and the time required for faſtening the paper to it, render it for many purpoſes very inconvenient.

The drawing board and normal ſquare is cumbrous, and waſtes time.

Theſe conſiderations induced me, about the time when the account of the parallel rule, given in your ninth volume was publiſhed, to conſider how a parallel rule might be conſtructed not liable to ſide deviation, and as free as poſſible from the defects of the others above ſtated. The inſtrument which then occurred to me as the beſt calculated for this purpoſe, I ſhall now deſcribe; and as I have often examined it ſince, if it poſſeſſed any material defect, it is probable it would have become manifeſt before this; in which caſe I ſhould not have brought it forward to public notice.

My

Defcription of Mr. Bofwell's parallel rule to prevent lateral deviation.

My inftrument for drawing parallel lines without fide deviation, is formed of three rulers, laid parallel to each other, connected by two pair of moveable pieces, all of equal length, and parallel to each other; thefe pieces, where they meet on the middle rule, have their extremities formed into portions of toothed wheels, which lock into each other, as may be feen in the figure: the effect of thefe fegments of wheels thus acting in each other, is, that all the lateral motion is transferred to the middle rule, while the external rules move only in an oppofite and parallel direction.

The contrivance to prevent lateral deviation cannot affect its accuracy. Its fupport at each end makes it fteady. It is eafily made exact.

This inftrument will not be liable to the incorrectnefs of thofe before defcribed, for the following reafons: 1ft. The toothed fegments being in no way concerned in producing the parallelifm of the inftrument, its accuracy of parallelifm cannot be at all affected by any trifling incorrectnefs of formation in their parts. 2nd. All the connecting pieces being of equal length, can be formed with more certain accuracy. 3d. The connecting pieces paffing from the fame extremities of the external rules, give them a fteady fupport. For thefe reafons, in my opinion, it poffeffes all the fteadinefs and facility of formation of the common parallel rule, while it effectually prevents the fide deviation, to which the latter is liable.

It might be made with but one pair of toothed fegments, but two pair make it look more uniform.

It is not abfolutely neceffary to have more than one pair of the connecting pieces made with toothed fegments; but as thefe fegments are eafily formed in the clock makers engine for cutting teeth in wheels, it can add little to the expence to make the two pair in this manner, as fhewn in the figure, and will make the inftrument look more uniform.

The middle rule fhould be made thinner than the others to prevent friction.

The middle rule fhould alfo be made a little thinner than the others, to prevent friction on the paper in its lateral movement when in ufe.

Novelty of the inftrument confifts in its toothed fegments.

Reafons for fuppofing this invention to be new.

In the defcription of this inftrument, it will be obferved, that the novelty of it confifts in the application of the toothed fegments of the wheels to the ufe mentioned; which I cannot find has ever been before ufed for this purpofe; and I think it highly probable it has not, as, befides its not being known to gentlemen, whom I have confulted on this head, moft likely to be acquainted with fuch matters, the fimplicity of the contrivance would probably have brought it into extenfive ufe, if it had been ever known at any former period.

I mention this only to fhew that, before I claim the priority of invention, I have taken fome pains to inveftigate my pretenfions;

tenſions; which I think is incumbent on every man to do on ſuch occaſions: for, however fair the claim may be of invention, if a thing is well known to have been before done, it at leaſt produces an aukward ſenſation to the claimer; for which reaſon, thoſe who accuſe others of doing this, ſhould be the more cautious, that their accuſation is fair in all its parts; for oftentimes an external reſemblance may ſubſiſt between two contrivances, as between my inſtrument and the triple parallel ruler, and yet a ſmall addition render their effects eſſentially different; thus the triple parallel ruler admits of ſide deviation, while my parallel ruler effectually prevents it.

A ſmall addition to an inſtrument ſometimes renders its effects eſſentially different.

My motive for publiſhing the account of this inſtrument is principally becauſe I think it a duty incumbent on every man, who has contrived any thing that may be of uſe to the world, to make it known as extenſively as poſſible, which it certainly will be by appearing in your Journal.

The inſtrument from which the figure was drawn was made according to my directions, by Mr. Banks, inſtrument maker, No. 441 in the Strand, and anſwers the purpoſe perfectly well; of courſe any gentlemen who deſire to uſe parallel rulers of this kind, may have them accurately made at the ſame place.

This inſtrument made by Mr. Banks, 441, Strand.

I requeſt the favour of your permitting the inſertion, at the end of this communication, of the indication of ſome typographical errors, made in my paper relative to the performance at ſea of the ſhip Economy, in your laſt number; and which I am the more anxious to have rectified, as ſome of them entirely alter the ſenſe of the paſſages where they occur.

Typographical errors in Mr. Boſwell's laſt paper relative to the ſhip Economy.

Page 175, line 2, eraſe *it* before *could*; line 6, transfer the bracket to before *when* in the next line; line 8, for *is* read *are*; eraſe the comma after *is*; and transfer the bracket to after *proof*; line 9, 10, for *direction* read *directors*; page 176, line 15, for *ſource* read *purſe*; line 21, eraſe *not* before *reſt*; page 179, line 22, for *point* read *front*; line 37, for *forms* read *frames*; page 180, line 3, for *ſcarping* read *ſcarfing*.

Some errors of the preſs are alſo apparent in the ſide notes, but I ſhall not trouble you by pointing them out, as they can be rectified by the meaning of the paſſages to which they are added. I am, Dear Sir,

Your very reſpectful humble ſervant,

J. W. BOSWELL.

Reference

Reference to the Figure. Plate V. Fig. 2.

A A,A A. The external parallel rules, B B the central rule, C D,C D the connecting pieces, D D the fegments of toothed wheels in which the connecting pieces terminate, which by their action on each other prevent fide deviation in A A,A A.

IV.

Letter from an ENQUIRER, *on the Wafte of Fifh afferted to be made on the Scottifh Coaft. In Reply to A. L.*

To Mr. NICHOLSON,

SIR, *London, Feb. 7, 1806.*

Proper fpirit of inquiry and publication.

I AGREE moft cordially with your Correfpondent A. L. of Aberdeen, in page 168, with regard to the accuracy of important information when communicated to the public, and that when doubts exift, it fhould be given with fo much modefty and diffidence, as to fhew that the communicator is not certain of his fubject. Of the ftatement I made refpecting thofe inftances of wafteful negligence in fome fifheries of the north of Scotland, I am not the firft; the refpectable author of the ftatiftical account of the parifh of Peterhead, the Rev. Dr. Moir, has afferted the fame, limited to that parifh that I did. In the 16th Vol. p. 550 of that work, he fays, " turbot (I believe the holybut of the London market) is now caught frequently, and in great perfection. Thirty years ago they were feldom ufed here, *frequently caft into the dunghill*, or *left to wafte on the fea beach*, they at prefent fell from four pence to one fhilling each, and are rifing every day in price;" in the preceding page of that volume we are informed, that " the greateft part of the *cods' founds*, in this parifh, are *permitted to remain and rot on the fea beach*, or, *are caft into the dunghill*, though the ufe and value of them as an article of food and delicacy at table have been known here for many years," and yet in the following paragraph the Doctor tells his readers, " that the crews of the fhips have been fent from this town to Barryhead, *to preferve the founds*, tongues, and palates of the cod caught *there*, and the owners have always

Scotch fifheries.

found

found a ready market for them!" for myfelf, Sir, before I even hinted through your Journal, at thefe ftrong affirmations, I made it my bufinefs to enquire of fome friends at Aberdeen, of the truth or falfehood of fuch affertions; deeming it then, as that gentleman does at prefent, an improbable ftatement; under thefe circumftances I cannot confider the communication you did me the favour to infert, as militating either againft your correfpondents rule of examination, or my own habitual fcepticifm:—that gentleman, in recommending accuracy of ftatement, ought not to have forgotten it himfelf; he will eafily fee that *Aberdeen* is not mentioned by me as being at all concerned in this wafte of fuftenance. My little note to you has rouzed the attention of A. L.—Is it wandering too much into

"*the fairy regions of romance,*"

to hope that the fubject may obtain ftill farther notice? and continue to do fo until it be made productive of *all the advantages it is capable?* in that cafe, fuppofing defective information in my firft notice of it (and I prefume A. L. will allow I had fome authority for my opinion, and that he himfelf has not been completely accurate) my errors will be eventually attended with good. Your correfpondent well knows, that the affertion of Dr. Johnfon about the fcarcity of trees in Scotland, has had the happieft effects. How far that gentleman's queftion about the vend at the towns I mentioned, may be anfwered in the affirmative or otherwife, I have not yet fufficiently informed myfelf; but certainly under the circumftances I conceive to be true, thofe markets would be preferable to fuch wafte. The men employed may look with confidence for a fpeedy fale; and, if I am not very much miftaken, thefe towns are fupplied from the Yorkfhire boats, the wind therefore which brings the one fet of boats, would impede, if not totally hinder the other. Far am I from wifhing to throw any obftacles in the way of fo excellent a plan, as that for a fociety for exporting white fifh from Aberdeen, but where would your correfpondent fend them to? "*is it not to be fuppofed, that fifhers of the places neareft to fuch towns could greatly underfell them?*" this queftion is not a greater difficulty in the fcheme fuggefted by me of bringing the fifh to Leith, Berwick, or Newcaftle, than it is to that of A. L. The fact is, that by giving that queftion weight, competition, in every bufinefs

Good effects of inquiry and public remark.

bufinefs would be undone; for my own part, I do not think it any objection to either of our fchemes; nay, our plans confidered ferioufly, are nearly alike. I fuggefted, rather I believe in the form of a query than otherwife, the propriety of bringing fifh where I know a demand exifts; but that gentleman oppofes my fuggeftion with the above queftion, and then propofes a plan on a fimilar, but more comprehenfive fcale! Allow me merely to correct at prefent one more error A. L. has, *unintentionally, I am certain,* committed in the laft paragraph of his vindication of the Arbroath fifhers (of whofe methods, and *probable* wafte, I hope foon to obtain a *correct* account,) It is *not* the cafe even for the moft part in *every* large fifhing town, that the fifhermen " retain the bodies of the crabs, and fell the large claws only. A lift of fome of the towns in which that cuftom prevails, would, doubtlefs oblige many of your readers. If I am correct in my opinion of A. L. all *his* attempts are for the fpread of ufeful knowledge; his candour will fuggeft the propriety of viewing mine alfo in a favourable light.

I am, Sir,

Your's and A. L's,

Friend and Servant,

AN ENQUIRER.

Notice of a Publication of Importance intended by the Literary and Antiquarian Society of Perth.

SIR,

IT is with great pleafure I inform you that the very refpectable Literary and Antiquarian Society of Perth, intend giving a felection of their valuable papers to the public; it is much to be lamented that they have delayed doing this fo long, as many of their manufcripts throw a very extenfive light on the antiquities of that part of the ifland.

I am, Sir,

Your's truly,

N. L.

To Mr. Nicholfon,

Newcaftle-upon-Tyne, Jan. 28, 1806.

Letter

Letter concerning a Library established at Aberdeen. From a TRAVELLER.

SIR, *York Hotel, Bridge Street, Blackfriars.*

I AM extremely glad to find, that there is a subscription library established in Aberdeen. I am astonished, however, to be informed from Mr. Crombie's paper, that none of the very learned Professors in that part of the country are engaged in the undertaking! perhaps if the subscription was raised to one guinea per annum, much more good might be effected, and those gentlemen would not then scruple to join themselves; their freedom as to pecuniary motives is well known. I hope for the sake of the general diffusion of knowledge, to find myself equally mistaken with regard to Banff, Peterhead, and Inverness. The society at Aberdeen, though young, seems to be conducted with great liberality, as appears evident from their offer of assistance to the places above mentioned, or any other that may be now forming rules; is it too much for a friend to the spread of useful knowledge to suggest to them an extension of the benefits of their association, to those gentlemen who are members of similar societies, whilst in Aberdeen, on condition of a return of such civilities, should any of their members be where such libraries are? a rule of this kind you Sir, have mentioned with applause in a former number,

Your's, &c.

A TRAVELLER.

February 7, 1806.

V.

A Chemical and Medical Examination of the Gizzards of White Fowls compared with Gelatine, together with an Exposition of the Characteristics of the latter when oxigenated, By. M. BOUILLON LAGRANGE*.

IT has long been understood, that the gizzards of white poultry possesses certain medicinal qualities. The use made of it by many physicians may justify some reliance upon the virtues

Gizzards of fowls medically used.

* *Annales de Chimie*, Vol. LV.

virtues attributed to it; but no one, I believe, has hitherto thought of analyſing this ſubſtance.

It occurred to me, that it would be uſeful to the art of healing, were a few chemical facts added to the knowledge already poſſeſſed of the medical uſes of gizzard, particularly after reading in the "*Journal d'Economie Rurale and Domeſtique, ou Bibliothèque des Propriétaires ruraux, Pluvoiſe, an* 12;" a letter, wherein is announced the ſucceſs obtained by its uſe in agues. As this letter contains the details of the preparation, and adminiſtration of this remedy, I ſhall tranſcribe it at length.

"*Amiens, 25 Frimaire.*

Letter reſpecting it. Recommended as a febrifuge by the French government.

"YOU mention animal gelatine as a febrifuge, I will inform you of a more ſimple and leſs expenſive remedy. I know not by what fatality this great ſpecific has been neglected, notwithſtanding it was publiſhed by government full forty years ago, and in ſpite of its efficacy, of which I have had long experience; for, of about a thouſand caſes, in which during that period, I have adopted its uſe, I can atteſt the cure of eight-tenths.

"I have reſided at Montpellier during fifty-ſix years; the climate of the place and its environs is mild and ſalubrious; but the inhabitants along the coaſt are ſubject to agues, on account of the vicinity of the Mediterranean, and of ſtagnant pools, M. de St. Prieſt, intendant of this province, publiſhed the order of government relative to the remedy above alluded to.

Preſcription.

"*Remedy.*—This remedy conſiſts of the gizzard of fowls, dried and pulverized.

The gizzards are waſhed, dried, and pulverized.

"*Preparation.*—Take the gizzard of white poultry, as fowls, turkies, &c. (I never made uſe of thoſe of black fowls, as pigeons, ducks, &c.) open them, and clear away the gravel they contain; having ſlightly waſhed them, let them be put on a ſtring and hanged in the ſun, or up a chimney to dry, after which they muſt be reduced to powder, ſifted, and kept in a bottle cloſely corked.

"*Doſe.*—The doſe is about a drachm for adults, and from half a dram to a ſcruple for children.

The doſe is one drachm taken in wine.

"*Mode of taking.*—Mix the proper quantity of the powder in a glaſs or half-glaſs of good old white wine, and let the patient

patient swallow it about half an hour before the fit comes on, or on the appearance of the precursory symptoms of the fever. This being thrice repeated, it rarely happens that the disorder returns.

" *Regimen.*—A wholesome regimen is all that is necessary during the administration of this medicine, but the patient should carefully avoid exposure to moisture or cold, particularly in the feet."

The foregoing details lead us naturally to the following observation:

Should this substance be considered as gelatine, and possessing the same property of being a febrifuge, as stated by M. Seguin; or should we rather acknowledge it to be possessed of those particular virtues which have been attributed to it by several emient physicians? M. Pia, an old apothecary of Paris, assured me, that full thirty or forty years ago, the powdered gizzard of poultry was recommended in all obstructions of the urinary passage, in complaints of the bladder occasioned by slimy matter, as well as in all nephritic pains. Questions whether it be gelatine.

" The efficacy of this remedy has long ago established its use;" and the writer adds, " that during my practice in pharmacy, I have prepared large quantities of it; so much were physicians and their patients satisfied with its operation." Its efficacy,

His method of preparing the gizzards was to choose those of young fowls, and particularly of pullets: after cleansing, rubbing, washing, and wiping them carefully, he strung them, and left them to dry on hurdles between sheets of paper, assisted by the gentle heat of a stove, and not in the sun, which, according to M. Pia, would have spoiled them. they should not be dried in the sun.

When the gizzards were properly dried, they became friable, almost transparent, and exhibited on being broken a vitreous appearance. Vitreous appearance.

The powder obtained was of a whitish grey ash-colour, yielding in the mouth a kind of mucilage, and possessing a slightly salt and bitter taste. Powder ash-coloured, mucilaginous and saltish.

This powder was administered twice a day, (morning and evening) in doses of twenty-four to thirty-six grains, in a glass of the infusion of pellitory of the wall; of bearberries *(uva ursi)* or of lintseed sweetened with syrup. Dose.

Effects. The efficacy of this remedy as a diuretic and aperient, was ſo much relied on, that the afflicted even omitted the infuſion and took it in pure water with a little ſugar.

Arguments in its favour. The long experience which has been had of the ſalutary effects of gizzard as a febrifuge, diuretic, aperient, &c. and the publicity which the government, doubtleſs not upon light grounds, has given to this remedy, are authorities in its favour; and it muſt therefore be an acceptable labour to the phyſician, to furniſh him with new lights upon an object ſo eſſentially intereſting to humanity. This is the motive by which I have been induced to ſubmit the following experiments to the ſociety of medicine.

Inquiries as to its compoſition and uſe. As gizzard has a great analogy to gelatine, I endeavoured to diſcover their ſimilitude. If gelatine be really a febrifuge, gizzard ſhould be ſo likewiſe, particularly as it contains, when freſh, a large portion of that ſubſtance; but whence does it derive its power as a diuretic, aperient, &c.? does it poſſeſs it in common with gelatine? I cannot tell. Or, have the ſaline parts of its compoſition this double property? of this alſo I am ignorant; for practice has not yet aſcertained whether the anti-febrile quality ſhould be aſcribed to the acidulous ſalts rather than to the ſubſtances with which they are combined.

Experiments on recent gizzard. A freſh gizzard preſented the following phenomena.

A. The water wherein this ſubſtance had been boiled acquired a yellowiſh white colour, and flakes were depoſited in cooling; it had a taſte rather inſipid than ſweet.

It reddened the tincture of turnſol.

B. Lime water, and water of barytes produced in this liquor an abundant precipitate, partially ſoluble by nitric and muriatic acids.

C. Ammonia cauſed a leſs degree of precipitation.

D. Oxigenated muriatic acid ſeparated with flakes from the liquor.

E. Cauſtic potaſh, either ſolid or liquid, acted upon gizzard in the ſame manner as upon muſcular fleſh.

When ground together, ammonia was diſengaged from the gizzard; it became ſoft, of a reddiſh colour, and ſoluble in water. If this liquor be evaporated, it will depoſit fibres in cooling. Alcohol, by deſtroying the potaſh, ſeparated a flaky ſubſtance, ſoluble in water.

This

This aqueous ſolution gave a precipitate on the addition of lime-water, or muriate of lime or of barytes, as well as of ſome acids. The precipitate obtained by lime-water may be rediſſolved by the addition of more water, which proves that the mixture had not become truly ſaponaceous, but that the potaſh had merely diſſolved the animal matter.

F. The action of certain metallic ſolutions on the liquor of freſh gizzard was more or leſs perceptible, according to the facility with which the metal communicated its oxigen to the animal matter.

Nitrates of mercury and of ſilver, for example, were decompoſed, but the precipitates obtained by the action of theſe ſalts upon gelatine and the extract, quickly turned black, particularly that of mercury, and they were no longer ſoluble in nitric acid. The oxides had, therefore communicated a part of their oxigen to the gelatine and the extractive matter, which were thus united to the mercury, now approaching a metalline ſtate.

Oxigenated muriate of mercury was not decompoſed in this manner. The circumſtances, in fact, were no longer alike: the exceſs of oxigen which it contains ſufficing to oxigenate the two ſubſtances. Here the precipitate was very little coloured, and the metallic ſalt was only reſtored to the ſtate of mild mercurial muriate.

Some other metallic ſolutions produced in the liquor of freſh gizzard only gelatinous flakes; ſuch are the acetate of lead, and the ſulphate of copper and iron.

G. Aqueous tincture of nutgall changed the liquor into a kind of jelly.

I have thought theſe experiments ſufficient for demonſtrating the nature of thoſe ſubſtances which were capable of ſolution in water; yet as gizzard is not adminiſtered in its freſh ſtate, but undergoes a proceſs which might cauſe a variation in the foregoing reſults, I again examined it in this latter point of view. Experiments on dried gizzard.

In drying the gizzard, I followed the preſcription already cited of M. Pia, and obtained a ſubſtance exactly anſwering his deſcription.

A. Reduced to powder, its taſte was inſipid, yet partaking ſtrongly of an animal flavour; its colour was a whitiſh grey.

 B. The

Experiments on dried gizzard.

B. The aqueous decoction took a light yellow tint, and fmelled like chicken broth.

It reddened the tincture of turnfol.

C. Lime-water and water of barytes caufed the fame kind of precipitate as in the decoction of frefh gizzard.

D. Oxalate of ammonia proved the prefence of lime.

E. Oxigenated muriatic acid feparated white flakes.

F. Nitric acid had a violent effect upon the dry gizzard; at a mild temperature it diffolved it completely.

Nitric acid at eighteen degrees excited a flight effervefcence, and by gradually increafing its temperature, a feparation was perceived of azotic gas, then of nitrous gas, and of carbonic acid gas.

The liquor left in the retort was evaporated, in the expectation of obtaining cryftals; but on cooling, none appeared. The evaporation was then continued, the refult of which was a yellowifh glutinous matter, tenacious, and of an exceffively bitter and acrid tafte.

Water imbibed the acid, and prefented all the characters of the decoction of apples.

G. Metallic folutions prefented nothing particular, as in the experiments upon frefh gizzard, except that antimonial tartrite of potafh was decompofed, forming in the decoction a white precipitate.

H. Aqueous infufion of nut-gall produced a lefs copious precipitate in this experiment, than it had with that upon frefh gizzard.

I. Dry and friable gizzard was digefted in alcohol; but the liquor was fcarcely coloured, even with the affiftance of caloric.

This alcoholic tincture reddened that of turnfol, and gave precipitates with lime-water and water of barytes, as alfo with nitrate of filver; a proof that the alcohol has diffolved only the faline particles.

L. The incineration of gizzard left a refidue of a faline and alcaline tafte. Paper tinged by curcuma became of a deep brown.

This refidue was partly foluble in water. The liquor contained fulphate, muriate, and carbonate of potafh.

The part not foluble, on being fubmitted to the action of muriatic acid, difcovered carbonate of lime, phofphate of lime, and a fmall portion of iron.

2. Hence

Hence it refults, that the greater part of the falts contained in gizzard, is the acid phofphate of lime; the prefence of muriate and fulphate of potafh is alfo obfervable.

Thefe falts are not only united with gelatine, but alfo with a fmall quantity of extractive matter. It fhould feem that the latter fubftance, and perhaps the gelatine, is oxigenated by the deficcation of the gizzard; for in this ftate they are lefs foluble in water.

Wifhing to afcertain the difference between pure gelatine, and that which had been oxigenated, I made experiments upon the former, of which the following is the refult.

Pure gelatine acquires different properties, according to the means employed in its oxigenation. Experiments on gelatin,

Of the metallic oxides, fome freely communicate their oxigen to gelatine, as the oxide of red-lead, and the red oxide of mercury; but the gelatine was combined with a part of the oxide, and could not again be feparated completely from it. In treating gelatine with the red oxide of mercury, a part of the oxide was reftored to its metallic ftate, and the remainder affumed a reddifh brown colour. with metallic oxides.

Superoxigenated muriate of potafh heated with gelatine, caufed no alteration in its nature. And other means of oxygenation.

Oxigen gas combined with it but flowly, and in fmall quantity. After being for a confiderable time fubmitted to the action of this gas, the gelatine only fuffered a change of colour; it became whitifh, but its characteriftics are ftill the fame.

Oxigenated muriatic acid prefented the following phenomena.

On pouring oxigenated muriatic acid gas into diffolved gelatine, a whitifh thick fcum appeared on the furface, of a moderate thicknefs, the under fide of which gradually changed colour, and became milky. The white filaments which fwam in the liquor, together with the fcum which floated on the furface, were feparated by filtering, and wafhed in cold and warm water till the water ceafed to redden tincture of turnfol. The fubftance thus prepared prefented the following characteriftics:

1. It was capable of extenfion equally with gluten, and was of a white colour. Properties of oxigenated gelatin.

2. It

2. It was very light, and ſwam upon water.

3. When well waſhed, it retained little or no flavour,

4. Left expoſed to the air, it dried, and fell to duſt.

5. It did not redden the tincture of turnſol.

6. It was ſcarcely at all ſoluble in warm water. On boiling it a length of time, in a ſufficient quantity of water, it was reduced to an infinite number of particles, ſo minute as to be hardly perceptible; but as the heat was lowered, they re-united in a maſs as before the boiling.

7. Heated nitric and acetic acids diſſolved this ſubſtance; but it was precipitated in its original form, by refrigeration.

8. Trituration with cauſtic potaſh produced a ſeparation of ammoniac.

It differs from albumen.

This matter, it will be perceived, is neither gelatine nor albumen, ſince its properties are wholly different.

The gelatin of gizzard is probably oxigenated.

It appears probable, that the gelatine in gizzard acquires by drying, properties analogous to thoſe above deſcribed; which, with the changes obſerved in the extractive matter already mentioned, would certainly render dried gizzard leſs ſoluble in water.

We have no means of aſcertaining, for want of a proper object of compariſon, whether this difference be eſſential to the efficacy of gizzard; and I know not if freſh gizzard has ever been adopted in medical practice. I could only wiſh to aſcertain if its febrifuge quality exiſt in the oxigenated gelatine, in the extractive matter, or in the acid ſalt. Indeed, on comparing the quantity of gelatine adminiſtered to patients, according to M. Seguin, with the doſe of powdered gizzard, above-deſcribed, a great difference will be obſerved; and yet according to thoſe who have made uſe of it, a ſmall doſe of powdered gizzard is ſufficient to check the fever.

The compariſon which I have made of gelatine with gizzard is ſufficient to eſtabliſh a material diſtinction between them.

Experiments on gelatine.

Pure gelatine poſſeſſes a weak inſipid flavour; does not redden tincture of turnſol; is mucous and gluey between the fingers; aſſumes in the fire a concrete, ſolid, and tranſparemt appearance; and is ſoluble in boiling water.

Solution of barytes or of lime mixed with that of gelatine, cauſes a precipitation of phoſphate of lime.

Sulphates

Sulphates of copper or tin, and acetate of lead, experience no decompofition. Experiments on gelatine.

Nitrates of mercury and filver are decompofed, but the precipitates are much lefs copious than thofe produced with the decoction of gizzards

Solution of tartrite of antimony only thickened the liquor.

Alcohol likewife has but little power over gelatine. The precipitates obtained by means of the water of lime or of barytes, as well as that by nitrate of filver, are fcarcely perceptible.

The decoction of frefh gizzard when fuitably evaporated, leaves a coloured gelatinous matter, foluble in water, which reddens tincture of turnfol; gives copious precipitates with lime-water and water of barytes; decompofes fulphates of iron and copper, acetate of lead, muriate of tin, tartrite of antimonial potafh, and nitrates of mercury and filver; the precipitates refulting from thefe decompofitions are generally too confiderable to be attributed folely to the gelatine.

Dried and powdered gizzard poffeffes characteriftics ftill more diftinct from thofe of pure gelatine, whence I conclude that the latter fubftance has a different operation.

I leave practitioners to decide on the advantages which the medical art may derive from gizzard; it is for them to decide whether much confidence is to be placed in the notice inferted in the Journal d'Economique. And if it fhall appear that the medieal ufe of this material has been attended with fuccefs; it will perhaps be proper to attend particularly to other fubftances which have not hitherto been fuppofed to poffefs any febrifuge virtue; fuch as the falts with excefs of acid, the oxigenated extractive and even oxigenated gelatine.

On

VI.

On Pirite found in France by M. Cocq, Commiſſary of Gunpowder and Saltpetre Works at Clermont-Ferrant, with an Analyſis of this Subſtance. By J. J. DRAPPIER, *Teacher of Chemiſtry at the Polytechnic School.**

Cryſtals of pirite found in the diſtrict of Puy de Dome,

M. COCQ found the cryſtals of pirite, in a porous grey porphyry, with a baſe of feldſpath, and containing cryſtals of quartz, forming a part of that chain of primitive mountains which ſupport the volcanoes of the diſtrict of Puy-de-Dome. Theſe cryſtals of pirite ſeparate from the rock, and leave in the porphyry an impreſſion perfectly ſmooth.

—and on the way to Menat.

He alſo found at the village of St. Avit, and in the vicinity of Pont-Gibaud, a ſubſtance which appeared to be pirite; in both ſituations it was ſo indeterminate as to render it impoſſible to pronounce exactly on its nature. But in returning to Menat, at twelve leagues to the north of Clermont, he perceived the granites reſuming the ſame appearance of thoſe which he had obſerved near Saint Avit and Pont-Gibaud, ſometimes the colour of the feldſpath inclined to purple, and oftentimes this ſubſtance appearing alone in the maſs of the granite, exhibited a beautiful purple.

The grey porous granite appeared again at intervals, with the appearance of the cryſtals obſerved in the ſame rock near St. Avit and Pont-Gibaud; at laſt, after a great many ſearches, he found the pirite well defined, and aſſuming a character much more determinate than that of Scheenberg.

Its Phyſical Characteriſtics.

Each cryſtal is a priſm of twelve faces, of a blackiſh or greeniſh brown, with a ſmooth ſurface.

Its colour is a greeniſh or blackiſh brown. Its form is a regular hexhedral priſm, of which all the lateral edges are truncated, which conſtitutes it a priſm of twelve faces. Sometimes the priſm has alſo a ſmall face at each of the angles of its baſe, which has not hitherto been remarked in the pirite of Saxony.

The ſurface of the cryſtals is ſmooth, and a little brilliant: in its interior, the pirite is dull, containing at times ſome particles of mica.

* Journal des Mines, Vol. XVII. p. 207.

Its

Its fracture is unequal, with a fine grain, approaching to a splintery fracture.

Its fracture is unequal, with a fine grain.

It admits of being scraped by a knife, and yields a dust of a bright grey colour; it is tender, and does not adhere to the tongue, though it is a little unctuous to the touch.

Yields to the knife, does not adhere to the tongue, and is a little unctuous to the touch.

Besides the size of its crystals, their faces, the substances to which they are found attached, added to the characters described, establish the identity of this mineral with the pirite of Saxony.

The crystals found in Auvergne are more perfect than those of Scheenberg; they exhibit no alteration, and the purity of their form removes all doubt of there being any necessity to class this substance as a new species.

The external characters of these crystals leave no doubt of their identity with the pirite of Saxony.

Analysis by M. Drappier.

The pirite of France, separated carefully from its bed, and reduced to a fine powder, is attacked and discoloured by muriatic acid. This acid dissolves the oxide of iron, the colouring principle, and a portion of the alumine: but as it leaves a considerable residue, on which it appears to have no action, M. Drappier thought the method of analysis should be changed: he then took 100 parts of this substance, and kept it at a red heat in a crucible of platina for half an hour; after it was cooled, there was a loss of seven parts. The remaining 93 parts were heated in the crucible for three quarters of an hour, with three times their weight of caustic potash, purified by alcohol. The fused mass, detached from the crucible by distilled water, dissolved entirely in muriatic acid. The solution evaporated almost to dryness, and then diluted with a fresh quantity of water, let fall a white precipitate, having all the characters of silex. This precipitate washed carefully and well dried, formed 0,46 of the substance submitted to experiment.

Analysis of the French pirite.

One hundred parts lose 7 by heating.

The remainder fused with potash dissolves in muriatic acid.

The solution evaporated and treated with water deposits 46 parts precipitate.

The remainder of the muriatic solution was decomposed by caustic potash. It immediately formed a precipitate, which soon dissolved again in the excess of alkali, with the exception of $2\frac{1}{2}$ parts of oxide of iron.

The residue treated with caustic potash leaves $2\frac{1}{2}$ parts iron oxide.

The alkaline solution saturated by an acid, deposited 42 parts of an earth, which had all the properties of alumine. All these precipitates, before they were weighed, were washed carefully, and heated to redness in a crucible of platina.

The alkaline solution saturated by an acid deposits 42 parts alumine.

Analysis

Analysis of the Pirites of France compared with that of Saxony

Analysis tabulated.

	Pirite of France.	Pirite of Saxony analysed by Klaproth.
Silex - - - - -	46,00 - - -	29,50
Alumine - - -	42,00 - - -	63,75
Oxide of iron -	2,50 - - -	6,75
Loss by calcination	7,00	
Loss - - - -	2,50	
	100,00	100,00

Remarks on the difference of the pirite of France from that of Saxony.

M. Drappier thinks, on comparing his analysis with that made by M. Klaproth, that it may be concluded, supposing there was no error in either analysis, that either the pirite of France is not the same substance as that of Saxony, or that minerals having the same external characters, and especially the same form, may vary both in their chemical properties, and in the proportions of their constituent principles. M. Klaproth says that acids have no action on the pirite of Saxony, that he found much difficulty in operating on it by potash, and that, in order to separate its parts, he was obliged to treat it twice with this alkali. The same chemist appears not to have found any water in this substance. This difference, it is true, may be explained, if it is considered that the pirite of Saxony contains more alumine, and that it adheres to the tongue, while that of France has not this property, probably on account of the water which it contains.

VII.

Experiments, shewing, contrary to the Assertions of Morichini, that the Enamel of Teeth does not contain Fluoric Acid. In a Letter from WM. BRANDE, *Esq.*

To Mr. NICHOLSON.

SIR,

Gay Lussac on fluoric acid in animal substances.

HAVING seen in one of the last numbers of the *Annales de Chimie*, an article entitled, "Lettre de Monsieur Gay-Lussac a Monsieur Berthollet, sur le presence de l'acide fluorique dans les substances animales," &c. I was surprised to find that a chemist at Rome, of the name of Morichini, had discovered fluoric

fluoric acid united to lime in the enamel of human teeth. The extraordinary refults of thefe refearches, induced me to repeat them; but before I mention the experiments from which I have drawn conclufions different from thofe of the abovementioned chemift, it may perhaps be proper to quote that part of Gay-Luffac's letter which relates to the prefent fubject:

—and faid by Morichini to exift in the enamel of teeth.

" M. Morichini having detached fome of the enamel from human teeth, fuppofed that it might bear fome refemblance in its compofition to the enamel of the foffil teeth of an elephant, in which, on a former occafion, he had detached fluoric acid; he therefore fubjected it to analyfis, and perceived, to his great fatisfaction, that it contained a large proportion of fluoric acid.

Quotation to that effect.

To render thefe experiments more conclufive, he fubmitted portions of the two fpecies of enamel, viz. that of the foffil, and human teeth, and likewife fluat of lime, to the action of fulphuric acid, and found that the laft of thefe three fubftances yielded fluoric acid in the greateft abundance, and that the enamel of foffil teeth yielded fomewhat more than that of human teeth; but Morichini remarks, that this difference is merely owing to the prefence of animal matter in the two kinds of enamel, and that the difengagement of the acid from the fluat may be retarded, by adding a little gelatine to that fubftance, after it has been calcined, and then drying the compound. He moreover obferves that the vapours which fulphuric acid difengaged from any of thefe three fubftances, had the property of acting on glafs, of depofiting a filiceous film on water, and other properties, which it is fcarcely neceffary to mention.

Morichini fays that the enamel of recent teeth afforded fluoric acid as well as that of foffil teeth;

and that the fulphuric acid difengaged vapours that corrode glafs, &c.

According to Morichini's experiments, one hundred parts of the enamel of human teeth contain 30 parts of animal fubftance, and 22 parts of fluat and phofphate of lime, with fome magnefia, alumine, and carbonic acid. He has not yet been able to feparate the fluoric and phofporic acids from each other, but thinks that the proportion of the latter muft be extremely minute. M. Morichini has alfo obferved that the enamel of the foffil teeth of the elephant differs from that of human teeth, in containing a fmaller proportion of animal fubftance and phofphoric acid; but he thinks that the phofphoric acid which he found in the enamel of human teeth may have been derived from

Component parts of enamel of teeth according to Morichini.

from a portion of the bony part from which the enamel is feparated with great difficulty. But the moft interefting and unexpected refult is, that fluoric acid exifts in animal fubftances: a difcovery of the greateft importance. Thefe experiments oppofe the prefent opinion concerning the compofition of enamel, for Mr. Hatchett in his analyfis of this fubftance has only detected phofphate of lime.

Morichini profeffes to have proved my facts by repeated experiments.

The refult of Mr. Hatchett's experiments, together with thofe which were fubfequently publifhed by Mr. Joffe, in the Annales de Chimie, Tom XLIII. rendered it neceffary for M. Morichini to fubmit his opinion to accurate inveftigation, and after having made a numerous feries of experiments on the fubject, he obferves, that he cannot entertain a doubt, that the enamel of human teeth confifts chiefly of fluat of lime.

General remarks by Gay-Luffac.

After fome obfervations on the compofition of ivory, M. Gay-Luffac concludes this part of his letter, by obferving that there is an immenfe field laid open in that part of chemiftry which relates to animal fubftances, if it were merely to fearch for fluoric acid. Morichini has undertaken an inveftigation of the fubject: but fo much remains to be done, that the exertions of many chemifts will be requifite."

The author's experiments fhew the contrary.

I fhall now relate fome experiments, which will fhew that fluoric acid does not exift in the enamel of human teeth, but that this fubftance confifts chiefly of phofphate of lime, as originally ftated by Mr. Hatchett.*

Enamel of human teeth was ignited, pulverized, and fubjected to fulphuric acid. The fumes did not corrode glafs.

One hundred grains of the enamel of human teeth, detached from what is ufually termed the bony part, but which appears to confift of a fubftance of the nature of ivory, were kept for a few minutes in a red heat, and then pulverifed. The enamel, thus reduced to powder, was put into a platina crucible, in which a piece of a glafs rod was placed horizontally in fuch a manner as to be about an inch and a half above the enamel. Half an ounce of fulphuric acid was then added, and the crucible being covered with a clean plate of glafs, the heat of a lamp was applied, and diftillation carried on for half an hour. During the procefs, white fuffocating fumes were extricated; but on removing the glafs which clofed the top of the crucible, neither this, nor the rod below it were in the leaft acted upon; which certainly would have happened, had

* Vide Phil. Tranf. 1799, p. 328.

any

any fluoric acid been present. Finding this, therefore, to be the case, I proceeded as follows:—Fifty grains of the same enamel were introduced into a small glass retort, and a little sulphuric acid being added, distillation was carried on nearly to dryness, but in such a manner, that the gaseous products might be received over mercury. A small quantity of sulphuric acid gas was disengaged, and what remained in the retort, consisted, as far as I could ascertain, of a mixture of sulphate of lime, phosphoric acid, and a small portion of sulphur, arising from a decomposition of a small part of the sulphuric acid by the animal matter, existing in the enamel.

neither was there any extrication of fluoric acid by adding sulph. acid to enamel and distilling over mercury.

I have the honour to be,

Sir,

Your most obedient servant,

WILLIAM BRANDE.

Arlington Street,
Feb. 15, 1806.

VIII.

A Memoir on taking the Levels of the whole Surface of France. By P. S. GIRARD, *Chief Engineer of Bridges and Highways, &c.**

IF the surface of the earth were formed by the revolution of a curve round its axis, it would be sufficient, in order to determine the respective positions of different points upon it, to measure their distances from the intersection of that surface made by the plane of the equator and any particular or assumed meridian.

Thus geographers, considering the earth as perfectly spherical, have determined the position of any given place by the conjunction of two co-ordinates, one of which is the arc of the meridian, comprised between the place and the equator, and the other an arc of the circle parallel to the equator, comprised between the place and any assigned meridian.

The method by which geographers determine the position of a place.

* Journal des Mines, Vol. XVII. p. 297.

As

As these two co-ordinates intersect each other at right angles, it is apparent that the method of geographers, for determining the position of any place on the earth, is the same as that by which the position of a point on a plane is commonly determined.

is not exact, on account of the inequality of the earth's surface.

But this process, which would completely answer the views of geographers, if the terrestrial sphere were regular, ceases to be exact when the irregularities and protuberances are considered, with which the surface of this spheroid is covered.

This true position of a place is in a line perpendicular to that assigned by geographers.

The position of any place depends in reality, according to this hypothesis, on a third co-ordinate, which is supposed to be drawn perpendicular to the point of intersection of the two others.

This third co-ordinate ought to be taken vertically over the place of which the position is to be determined, and its measure reckoned from the place itself to its arrival at an imaginary surface, produced by the revolution of a known curve round the axis of the earth.

The level of the sea affords a spherical surface, from whence to measure those perpendiculars.

But it is known, that if our globe were surrounded by a fluid mass, all other force being supposed to be absent but that of terrestrial gravity, the surface of this fluid mass would be that of a spherical solid, of which the mean surface of the sea, in its actual state, represents a part: It appears then convenient to choose, for the third co-ordinate here mentioned, that portion of a vertical line passing through any place, which is comprised between that place and the mean surface of the sea, supposed to penetrate the globe and to be extended beneath the continent.

This is the best, though not the only method:

We have said that the choice of this line would be convenient; because, in reality, the position of a point on the terrestrial surface may be determined by adopting any other system of co-ordinates; for example, by fixing the position of this point, by three planes mutually intersecting at right angles;

But it is the most simple, and is besides generally adopted.

but, besides the advantage of greater simplicity in the expression of the circular co-ordinates, they have moreover, that of being generally adopted; for the geographical charts, hitherto prepared, may be considered as the projection of the continents and islands on the mean surface of the sea; so that there only remains, in order to render geography perfect, to add to the latitude and longitude of all the places on the earth, the vertical

The true position of a place determined by annexing its vertical height which they are elevated above the ſurface of the ocean. vertical elevation above the ſea to its latitude and longitude.

The object of this memoir is to indicate the means of determining this vertical heighth, by their particular application to the territory of France. Management proper for aſcertaining theſe poſitions ſhewn in the inſtance of France.

It is evident, that all the operations neceſſary for this determination, may be reduced to a ſeries of levels made in determinate directions.

Nature itſelf has pointed out theſe directions, by the lines of *greateſt declivity*, which the large rivers, and thoſe which flow into them, form on the ſurface of the earth.

Thus, France being divided into five principal baſons, by the Rhine, the Seine, the Loire, the Gironde, and the Rhone,—the levels of the courſe of theſe rivers, from their ſources, or from their entrance into France, to their terminations in the ocean, would form the firſt baſis of the work propoſed to be undertaken. The levels of the five great rivers of France would form the baſis of this operation for that country.

After having aſcertained this firſt baſis of the general operation, the levels of the ſtreams by which the great rivers are ſupplied, ſhould be next taken, and theſe ſtreams ſhould be conſidered without any regard to thoſe of the third order, by which they are themſelves maintained. The levels of the ſtreams which ſupply thoſe great rivers ſhould be next taken.

At the ſame time, the levels of the rivers of the ſecond rank, which fall into the two ſeas, ſhould be taken; ſuch as the Eſcaut, the Somme, the Orne, the Vilaine, the Charente, the Adour, the Herault, &c. The levels of the rivers of the ſecond rank taken at the ſame time as thoſe of the firſt.

The declivities of the beds of the ſecondary rivers being known, thoſe of the rivers of the third, fourth, and fifth orders, &c. ſhould be determined ſucceſſively, according to ſpecial inſtructions which ſhould be given for this purpoſe. The levels of rivers of the third, fourth, and fifth order, taken.

By thus claſſing the operations relative to the general levels of France, and by arranging their reſults in order, as they were obtained, all the data would be ſoon collected, which were neceſſary for tracing the elevation of its territory on a geographical chart already prepared.

This tracing of the elevations would be effected, by joining all the points on one level by the ſame line. Elevation of the ſurface of France to be expreſſed in the rough, by joining all the points on one level by the ſame line.

Theſe *lines of levels* might be ſuppoſed to be elevated perpendicularly, one above the other, by a determinate ſpace, conformable to the ſcale of the chart on which they were traced.

It is evident, that thefe lines would reprefent the borders of the coafts of the fea, if it was fuppofed that its mean level fhould be elevated fucceffively to the fame heighths which they reprefented.

M. Triel prepared the fketch of a map on this plan.

It was according to this idea that M. Dupain Triel prepared a phyfical chart, mentioned by M. Lacroix, member of the National Inftitute, in his introduction to Pinkerton's Geography; a chart which, from the defect of materials neceffary for its conftruction, prefented only the fketch of a work, the extent of which would require for its perfection an union of means, which could not be at the difpofal of any particular individual.*

The order has been pointed out in which this work ought to be executed, and we fhall now examine how it fhould be performed.

The courfes of the great rivers fhould be divided into portions, and the levels of each taken by obfervers at the fame time.

The bed of each of the great rivers muft be divided into a certain number of portions, and each portion fhould be levelled by obfervers, who fhould operate at the fame time.

Thefe obfervers fhould place accounts of their operations at each extremity of the portions of the bafons with which they were charged; and as the levels of the fecondary rivers fhould be connected with thofe of the principal rivers, it would be neceffary alfo to place accounts of the operations at the mouth of each of the influent ftreams.

The levels fhould be taken on the banks of the rivers, without any regard to the furface of the water. If it were thought ufeful to determine the declivity of this furface, it would be eafy to afcertain it, by levels taken at the fame time with the others, at certain diftances from each other.

The refults to be collected into a general fyftem.

When the different obfervers have completed their refpective obfervations, the refults muft be collected, to form the feries of levels of one of the beds. And in the fame manner the levels of all the reft fhould be obtained.

After this, a general fyftem fhould be formed from thofe particular levels, by connecting together the different beds, by operations directed from one to the other, according to thofe lines which would afford the greateft facility.

* Compare this with Mr. Churchill's plan, at p. 224 of our XIth. Vol.—T.

There

There only remains to determine to what agents Government should entruſt the performance of the general levelling of France, in order to have it executed with the greateſt exactneſs, ſpeed, and economy.

The engineers of bridges and highways, already placed in the different departments, where this operation ſhould be performed, are evidently the only perſons to whom it could be confided, ſo as to fulfil thoſe three conditions.

The engineers of bridges and highways would be the moſt proper perſons to employ in this work.

In fact, the execution of all projects relative to the eſtabliſhment of communications by land or by water, require, that the elevation in relief of the country, through which the works ſhould be carried, ſhould be known. The theory and practice of levelling form an eſſential part of the inſtruction given to the engineers of bridges and highways; and greater reliance may be placed on the exactneſs of the reſults which they might furniſh, becauſe the uſe of the inſtruments neceſſary to this operation, is more familiar to them.

On the other hand, there are none of thoſe engineers who could not dedicate ſome days of the ſummer to taking the levels of that portion of ſuch great rivers, or ſtreams, as ſhall traverſe his diſtrict; and as it is eaſy to take the levels of four or five kilometres (about three Engliſh miles) each day, eſpecially when the line to be levelled is previouſly determined by the direction of the river or current of water, it is certain, that the engineers of the bridges and highways might collect, in a very ſhort period, very minutely detailed materials for a phyſical chart of France.

Laſtly, theſe materials would be collected by them with the leaſt poſſible expence, becauſe Government would neither have to ſupport the coſt of extraordinary journies, nor the purchaſe of inſtruments, as the engineers are already, by the very nature of their employments, diſperſed over the ſeveral diſtricts where it would be neceſſary to operate, and are, at the ſame time, provided with the different inſtruments required for this purpoſe.

They could perform it without expence to the nation.

It may alſo be added, that the taking the general levels of France appears to be, with the more propriety, a work that ſhould be performed by the engineers of bridges and highways, as they would be the firſt to profit by this operation in putting their projects into execution.

It would tend much to their own benefit to have it effected.

Suppoſe then that the engineers of bridges and highways were charged with the performance of this work, let us conſider how, after ſome years, the exactneſs of the reſults, which they had collected, could be ſufficiently aſcertained.

Let us take, for example, the bed of the Loire, whoſe courſe is of great extent.

The chief engineers of the departments of the Upper Loire, of the Loire, of the Saone and Loire, of the Nievre, of the Loiret, of the Loire and Cher, of the Indre and Loire, of the Mayenne and Loire, and of the Lower Loire, would be ordered to furniſh, during the year, the levels of that part of the courſe of the Loire which traverſed their reſpective departments.

According to the new organization of the ſervice of bridges and highways, theſe nine departments require twenty-two engineers, in the diſtrict of each of whom would be found a portion of the work to be performed.

The total extent of the Loire is about ninety muriameters (about 550 miles), which being divided among twenty-two obſervers, would give to each of them little more than forty kilometers (about twenty-five miles) of levels to execute.

Levels of the Loire might be taken by the 22 engineers in that diſtrict, in one ſeaſon.

There is reaſon to believe, from experience, that the twenty-two engineers employed on the courſe of the river, would finiſh, in leſs than one ſeaſon, the levels of the whole river.

The ſame thing may be affirmed of the engineers placed in the departments traverſed by the Rhine, the Seine, the Gironde, and the Rhone. It appears then beyond a doubt, that, at the end of the firſt year, the chief part of this phyſical chart could be completed, to which the farther details might be afterwards added.

Levels of France, when taken, might be afterwards verified by the newly appointed engineers.

Whatever care may be beſtowed in taking levels, their verification is always an uſeful operation. That of the general levels of France might be made as often, and in whatever circumſtances it ſhould be judged neceſſary. It would be ſufficient for this purpoſe, to direct the newly appointed engineers to repeat, in the departments to which they might be ſent, the obſervations of their predeceſſors; which, beſides the advantage of confirming or correcting the reſults already obtained, would give an opportunity to the new engineers of acquiring, in perſon, a knowledge of the elevation of their reſpective diſtricts in relief.

The

The facility and promptitude with which the engineers of bridges and highways might execute this work, will be apparent, if it be recollected that, at the time when the major part of the great roads in France were formed, and when a general ſyſtem of internal communications was deſired to be eſtabliſhed, M. de Trudaine, aſſiſted by M. Perronnet, cauſed plans to be taken of all the principal roads, from their commencement to the frontiers. There was joined to the plan of the road properly ſo called, that of the country bordering on it, to the diſtance of three or four hundred yards at each ſide; a work which evidently required more time than ſimply taking the levels of a determined line, ſuch as we propoſe; and yet the engineers of bridges and highways, or their pupils, employed in taking thoſe itinerary plans, completed from five to ſix leagues of them each month. Facility of this work.

The general utility of the operation, of which a ſketch is here given, will ſooner or later determine ſome of the nations of Europe to undertake it. France, on whoſe territory has been lately executed ſome of the fineſt geological operations which were ever performed, and where, for the firſt time, a ſyſtem of univerſal meaſure has been eſtabliſhed on an invariable baſis, ſeems to be particularly called on, to give, on this occaſion, the firſt example of a work, which, by completing the natural geography of countries, will furniſh new facts to geology, and to thoſe different parts of natural hiſtory which depend on it. Geological operations already performed in France, urged as a motive to commence this work; which would complete the natural geography of countries.

IX.

Obſervations and Experiments on the Compoſition of Water, and other Elementary Doctrines. By H. B. K.

To Mr. NICHOLSON.

SIR,

As two papers have appeared in your Journal, both of which militate againſt the reſult of my experiment, and as Mr. Accum has been concerned in one of them, I therefore think it incumbent on me to anſwer them.

 I was

The gafes obtained from water by galvanifm, fmelled of nitrous gas after explofion, and gave nitre with potafh.

I was glad to fee my experiments in your Journal, as it has fo extenfive a circulation. I fhall now give you the analytical part to confirm my former experiments; as by them I had, I hope, given ftrong evidence, that acids are neceffary in forming gafes. Having collected a great quantity of the gafes produced by the galvanic pile, I introduced them into a ftrong glafs tube, clofed at one end, the other end I afterwards clofed, having previoufly introduced to the gafes a fmall quantity of a folution of potafh: through this tube the electric fpark was made to pafs, it having fmall openings to admit the wires of communication. Upon their combuftion, the fmell of the nitrous acid vapour appeared, both from its colour and fmell; and the tube being moved up and down, fo as to allow the vapour and the folution of potafh to come in contact; the folution being examined fome fhort time after, it gave evident and unequivocal figns of the nitrate of potafh.

On Pacchioni and Riffant's experiments.

I fee in your Journal, Mr. Riffant's experiments, in anfwer to Mr. Pacchiani's paper. Indeed, in reading Mr. P.'s experiments, nothing could appear more vague and wild than that water, by having oxigen, the fuppofed acidifying principle, taken from it, fhould become a ftrong mineral acid. Mr. Riffant's fecond experiment directly contradicts my experiment, on the fuppofition that water is a compound body; but if examined upon my fuppofition, that the acids are neceffary in forming the gafes, and that the water is only neceffary in forming the water of compofition, I hope I fhall be able to prove that his experiment confirms my opinion. There were very little of gafes formed by this experiment, and the wires were very much calcined: now this calcination was from the acid, or acids, I proved by repeating the very fame experiments; but only inftead of diftilled water, I ufed a folution of potafh, and inftead of the wires being calcined, they were not fenfibly acted upon, and the potafh became nitrated.—Now, Mr. Nicholfon, I (ferioufly and ardently) call upon your numerous readers to perform this experiment, which I think muft be decifive.

Againft the doctrine of the compofition of water: it is urged that the gafes obtained in galvanifm vary from different caufes.

I can but fmile at the French chemifts, in making the proportion of the gafes fo exactly to tally with their opinion of the compofition of water; but I have in my experiments found very different refults; the kind of gafes depending a good deal upon the wires ufed, the different metals, their length, and the

the different liquors between the plates of the pile; all of which had a fenfible effect upon the gafes, both upon the quantity produced, and their kind; the calcinable wires when long producing the moft inflammable kinds, and the lefs calcinable metals the more of the oxigen kind, and the longer the wires the more in volume were the gafes.

Remarks on Mr. Northmore's experiments.

Mr. Northmore, in your Journal, endeavours to prove the formation of the nitric acid from the compreffion of gafes.—Upon inveftigation, his experiments will, I think, be found very vague and inconclufive: that gafes from active compreffion will produce both heat and water, has been long known. The firft experiment was in condenfing hidrogen, oxigen and nitrogen gafes, two pints of each. He fays they produced "white floating vapours, probably the gafeous oxide;" but in experiment the feventh, he obferves, "the hydrogen produced white clouds at firft, *quære* ammonia." So without any chemical examination of thefe white clouds, they are at firft fuppofed to be the gafeous oxide, and afterwards ammonia, juft according as his theory dictates to him. In the fifth experiment, he fays, "and the refult was only a fmell of gafeous oxide of nitrogen, a few yellowifh fumes." Here then the gafeous oxide produces a yellowifh colour, though in the firft experiment it was a white colour.

The acid produced was, from the fame vague opinion, fuppofed to be the nitric; but this he endeavours to examine in the next experiment; firft by a good teft, in expofing it to lime water; and he fays, "Some yellow particles were feen floating upon the lime water; thefe particles probably arofe from the refinous fubftance ufed in faftening on the cap of the receiver being diffolved by the nitrous gas formed during condenfation. Here then was the lime water affected. I fay with confidence, thefe flocculi in the lime water were from the carbonic acid produced, and why they appeared yellow was from their being feen through the gafes, being clouded with an orange colour, which, as he obferves, they put on when they were condenfed.

That acids are neceffary in forming oxigen gas, I hope appears very clear from my experiments; therefore when it forms combuftion with inflammable bodies, it is rational to fuppofe that an acid will appear upon its decompofition. If the combuftion is active, as in the French experiments in condenfing oxigen

Remarks on Mr. Northmore's experiments.

oxigen and hidrogen gafes, the heat produced is fo active as to make an explofion of the gafes; but if a flow combuftion, it will leave the oxigenized acid in a gafeous ftate, as carbonic acid gas, which, I fuppofe, was the cafe in Mr. Northmore's experiments. His next experiment of examining the acid: He compreffed the gafes upon two fcruples of the folution of potafh; he fays, " there was fcarce enough acidity to tinge the edge of the teft paper; of courfe, I could not effect the formation of the nitrate of potafh." But always to affign fome reafon for the failure, he fays, " This quantity (of gafes) was hardly fufficient for the receiver's capacity;" but there was the fame quantity in this experiment as in the others; nay, in the next experiment (the fixth) there was identically the *fame quantity and in the fame proportions;* and in this fifth experiment, he found fo little acid, as he fays, " Scarce enough acidity to tinge the edge of the teft paper; of courfe I could not effect the formation of the nitrate of potafh." Now upon the fuppofition that the carbonic acid was formed, it would unite with the potafh, and therefore the mixture would be lefs faturated with it: But if the acid was fo ftrong as he fpeaks of in the fixth experiment, from the very fame procefs, as he fays, " Which moifture was ftrongly acid to the tafte, coloured litmus, and when very much diluted with water, acted upon filver." Now if Mr. Northmore will confult the writings of chemifts, (Dr. Black's lectures, for inftance); in Vol. II. the doctor fays, " that the nitric acid requires a little water to reduce it to the ftrength of aqua fortis; in order to act upon filver, therefore, in this experiment, the acid muft have been in the concentrated ftate of the nitric acid, as it required water to be diluted to make it act upon filver; but probably Mr. N. does not know that water impregnated with hidrogen gas will colour filver; which I fuppofe to have been the cafe here.

This reafoning muft appear to be moft extraordinary: this vaft quantity of nitric acid produced was even to penetrate into the cap of the receiver; but very unfortunately for this fuppofition, chemifts are of opinion that acids will not diffolve refins. Mr. Hatchett has promoted their operation upon each other by ufing the ftrong nitric acid; but this was a difficult and tedious procefs, not during the tranfitory action of a little time, by compreffion; and where the refin was fo concealed;

cealed; ſo that the acid could not get to act upon it, being placed within the cap of the receiver; therefore the ſmall quantity of weak acid formed in Mr. N.'s experiment could not rationally be ſuppoſed to have penetrated to it, even if it was in a high concentrated ſtate; but it muſt have been much diluted with water, as there was water alſo produced in this experiment: Alſo, if it was in this high concentrated ſtate, and in that abundance as to enter into all the crevices, it would eaſily have been detected, and his fifth experiment was for this purpoſe, but it failed; he could find no nitrous acid.

Remarks on Mr. Northmore's experiments.

In experiment the ſeventh, he ſuppoſed he had formed ammonia, and he ſays in this very experiment, "Some vapour was generated, which was, as uſual, ſtrongly acid." How comes it that this acid, which was ſuppoſed to find out the reſin, ſo perfectly concealed, could not find out the ammonia, which was formed along with it in the proceſs, and ſo univerſally diſperſed as to form white clouds.

The third experiment: "Two pints of carbonic acid and two of hydrogen was ſubjected to condenſation. The reſult was a watery vapour, and a gas of rather offenſive ſmell." This compreſſed gas I found to be ſimilar to Mr. Cruikſhanks's gaſeous oxide of carbon from the acid air and the phlogiſtic air ſaturating each other.

Mr. Northmore apologiſes for giving theſe experiments "until he had brought them to a greater degree of perfection," but at the concluſion he alſo ſays, "Beſides the above, I have made various other experiments with different gaſes, &c." But as he ſays nothing more of theſe *imperfect experiments*, there are no hopes of his correcting them; he appears to have exhauſted his reſearch, and we have ſeen with what ſucceſs. There appears ſuch an ardent deſire to ſupport the Lavoiſierian theory: but if it has always failed from the experiments of Lavoiſier himſelf, Mr. Cavendiſh and others, I am afraid we have little to expect from theſe new ſupporters. I might make many other obſervations, but theſe will, I preſume, be thought enough.

It will be expected in contradicting Mr. N.'s experiments I ſhould make ſome of my own; I muſt own my apparatus was not ſo good as his; yet I hope ſufficient to prove my opinions. I had the barrel of a large blunderbuſs, and ſtopped its priming hole, and having filled it either with ſand or diſtilled

Experiments of compreſſion of the gaſes.

diftilled water, I then tied to its mouth a bladder filled with the different gafes I wanted to comprefs. Upon pouring out the fand or water into the bladder, the gafes entered the barrel, and then having a ftrong iron ram-rod made perfectly air-tight, it was forced down upon the gafes by a long iron lever, by which means I was capable of making a ftronger condenfation than reducing them to one fifth of the volume. The refults of thefe experiments were, that all the different gafes, by being compreffed, gave out heat and moifture: The hydrogen gas, the greateft proportion of moifture to its fpecific gravity. That when oxigen and hydrogen gafes were compreffed, there was an acid which produced flocculi in lime water; and that nitrogen gas was not neceffary to the production of the acid, but rather retarded its production. The nitrogen gas obtained by the nitric acid and animal fubftances ought not to be ufed in thefe experiments, as it is partly acid of itfelf; but the nitrogen of the atmofphere ought to be made ufe of, being previoufly paffed through lime water.

They gave out heat and moifture.

Oxigen and hydrogen gave acid.

Mr. Nicholfon, I have condenfed this communication as much as poffible, in order that it might not occupy too much room in your Journal. I am

Your's, &c.

H. B. K.

London, February, 15, 1806.

X.

On the Conftruction of the Sails of Ships and Veffels. By MALCOLM COWAN, *Efq. Captain in the Royal Navy.* *

The fails of fhips have been long without improvement.

IT appears from the conftruction of the fails of fhips and veffels, belonging to every nation, that it is a fubject no one has hitherto taken much pains to inveftigate; but the maritime world have been content to ufe them, as they found them, every one following the beaten track of his predeceffors, without examination.

* Extracted from an effay, by the author, who has letters patent for the fails.

That

That the ſails of ſhips have been hitherto ſo conſtructed by all European nations, ſo as to be only managed with great labour and danger; and that when managed with the greateſt ſkill, they are very far from being of that utility which they ought to poſſeſs, and are capable of having, is inconteſtible.

They are worked with difficulty and danger.

Ships are driven on ſhore every winter, which might, with proper ſails, have eſcaped every danger. The loſs of one ſail, in many ſituations, is followed by the inevitable loſs of the ſhip and crew. Sails are often ſplit in hauling up to reef, and it may be neceſſary to reef a ſail that is worn, to preſerve it from ſplitting; hence the neceſſity of the ſails being conſtructed to reef without ſtarting tack or ſheet.

Many ſhips have been loſt by not having time, or drift, to haul their courſes up, to reef them on the yard, by which they riſk their ſplitting; a circumſtance which alone muſt convince the ſeamen of the utility of having ſails that can be reefed without taking their effect off the ſhip.

Many dangers may be avoided, by carrying ſail with ſafety to the maſts and yards. A ſhip can carry top gallant ſails that reef at the foot, with ſafety, when other ſhips muſt furl theirs; an evident advantage in many ſituations.

Advantages of ſails which are reefed at the foot inſtead of the head.

The top ſails of ſhips, with one or two reefs at the foot, can be reefed in a minute by one ſeaman at each lower yard arm, while they remain ſet with the top gallant ſails over them, by only ſettling the hallyards; by which a ſhip in ſqually weather, on many occaſions, would have a great advantage, particularly in chace, &c. or when caught by a ſudden ſhift of wind on a lee ſhore, or obliged to haul ſuddenly to the wind from ſailing large.

The facility with which ſails that reef at the foot, can at all times be managed, would enable ſhips to make quicker voyages, and prevent them often, when weakly manned, from detaining fleets; by the difficulty and danger of carrying ſail, being entirely removed, muſt enable merchant ſhips to be navigated with fewer hands, which would be a conſiderable ſaving of expence, and a great advantage in time of war in particular, when men are ſo ſcarce.

The ſeams ought to be horizontal.

If the ſails were made with horizontal cloths and ſeams, the ſails would ſtand better, particularly in a gale of wind; as the ſtrongeſt direction of the cloths and ſeams would be oppoſed to the greateſt force of the wind, which acts horizontally;

tally; and ſhould the ſail ſplit in that direction, it would ſtill remain full, and be leſs liable to blow away altogether, which is generally the caſe when a ſail ſplits in a vertical direction. Storm ſtay ſails ſet purpoſely with the cloths horizontal, have proved this beyond a doubt.

Many ſeamen are loſt every winter, by falling overboard from the yards while reefing the ſails, as it is more dangerous and requires longer time to perform in a gale of wind, than furling the ſails, which is not ſo often neceſſary as reefing.

Other advantages from improvements in ſails, &c.

Ships may ſometimes avoid a lee ſhore, by carrying a timely preſs of ſail, and when in that perilous ſituation, in a gale of wind, the ſafety of the ſhip may ſolely depend on the ſails being kept ſet; though it may be neceſſary to reduce them, either to ſave them, or eaſe the ſhip. The common ſails require to be hauled up, to be reefed, at the riſk of ſplitting them, at a time perhaps, when the ſhip is in imminent danger, from the want of ſea room; and the beſt ſeamen of the crew muſt be ſent on the yards when they poſſibly may be much wanted on deck.

Whole fleets are often caught by a ſudden ſhift of wind, of a lee ſhore, thrown into confuſion, and obliged immediately to reef their ſails, at the ſame time the ſhips may require the whole of their crews on deck, to attend the working of the ſhip, to keep clear of each other; particularly when it happens in the night time, with the wind ſqually and variable.

When ſhips from foreign voyages, enter the Engliſh or Iriſh channels, in the winter time, when the days are ſhort, and the nights long, with weak or diſabled crews, or men not accuſtomed to cold or froſt, ſuch as Laſcars, Negroes, &c. it is with the greateſt difficulty they can be prevailed on to go aloft; but ſhould they get on a lee ſhore, which all ſhips are liable to, and with a helpleſs crew, nothing can exceed the horror of their ſituation, ſhould they not be able to proportion their ſail to the wind in time to ſave the ſhip.

Naval improvements are of great importance to the ſtate.

To facilitate the working of ſhips, by the moſt approved means, is an object of greater conſequence to a maritime nation than many are aware of, even in a commercial point of view. The little alteration that has been made in ſhipping for many years paſt, ſhews with what indifference attempts at improvements have been regarded, many of which have been

been tried, proved, and neglected, while others have failed from the unavoidable expence, neceſſarily attending all experiments on a large ſcale, which require repetitions to bring to perfection; or from partial intereſts or prejudices, being oppoſed with ſucceſs (which not unfrequently happens) to improvements of general advantage. And many are apt to ſuppoſe that particular arts and ſciences are brought to the higheſt degree of perfection they are capable of, though experience every day convinces us to the contrary.

The largeſt ſhips might be much more eaſily navigated, if the improvements on capſterns, windlaſſes, blocks, hawſe-holes, &c. were univerſally adopted from the great reduction of the friction.

The following explanation will be eaſily underſtood by thoſe who are acquainted with the conſtruction of a ſhip, See Plate VI.

The courſes and top gallant ſails are to be reefed from the deck, and the top ſails by one man at each lower yard arm.

Deſcription of the improved ſails.

A. The after-part of the ſails.

B. A ſtrong band on the after-part of the ſails, ſewed on at the upper part only, and roped at the lower part.

C. The long clews of the courſe, formed by the bight of the leech rope and rope of the reef band with thimbles, ſeized in above the tack blocks, for laſhing the lower clews to.

D. The tacks and ſheets fitted to the upper clews of the courſes with thimbles above the tack blocks.

E. The buntlines, brought up through the thimbles H, on the foot ropes of the ſails, and bent to the cringles I, on the ropes of the reef bands.

F. A ſmall rope or gaſket, rove, occaſionally as a reef line, through eyelet holes, under the reef bands, and made faſt to the middle ſail, for confining the ſail when reefed, in the wake of the reef bands.

G. Thimbles in the clews and earings.

K. Thimbles on the foot rope with the earings rove through them.

L. The reef tackle pendants, paſſing through thimbles in the clews and leech of the top-ſail, and brought up and bent to the cringles above the upper reef band.

M. A boom tackle or burton hooked to the reef pendants.

N. The crow-foot legs to the top gallant buntline.

 N. B.

N. B. The reef bands are fewed by the upper part, to the after part of the fails, to prevent the rope from girting the fails, when the whole fail is fet.

The rope of the reef band of the courfe, is the fame fize as the common foot rope, and the foot rope muft be in proportion to the rate of the fhip: for the firft rates, $3\frac{1}{4}$, or 4 inch; fecond rates $3\frac{1}{2}$; third rates, 3 inch rope: as the quantity of fail below the reef band does not require fo ftrong a foot rope, as when the whole fail depended on it.

The rope of the reef bands of the top fails, fhould be fmaller than the leech ropes, as the foot of the fail will be confiderably ftrengthened, when reefed.

Thefe fails are not fo heavy as the common ones; a 74 gun fhip's courfe is reduced in weight about 200lbs. as the points, bands, and eyelet holes of the old reefs are not required, nor any additional geer.

Men of war will find one reef at the foot of the top fails, very ufeful in chace in fqually weather, or when obliged to haul fuddenly on a wind, &c.

Merchant fhips will only require two reefs in the top fails, as the fquareft part of the fail is taken off, by reefing at the foot inftead of the head, but more reefs may be added if neceffary.

Inftructions for reefing and fetting the fails.

When the courfes are to be reefed, caft off the lower clews, from the thimbles in the upper clews, haul up the flack fail by the buntlines. and haul tort the reef line, one part at a time, from the middle of the fail, towards the clews, and make it faft round the upper clews, fo as to confine the lower clews.

To fet the fail, reeve a few turns of the lafhing for the clews, and haul them down, overhauling the reef line, and buntlines.

To reef the top fails, fend a man up to each lower yard arm, fettle the hallyards, and haul the fail down by the reef tackles, and pafs the turns of the earings, through the thimbles in the earing cringles, and on the foot of the rope, and make them faft. Hoift the fail tort up, haul through the flack of the buntlines, and haul tort the reef lines on each fide towards the clews, and make faft,

The top gallant fails are reefed from the deck, by the clew lines, and a fingle buntline with a crow-foot.

The

The buntlines and reef line will confine the flack fail, when reefed, clofe up the wake of the reef bands; and the buntlines will only require to be kept hand tort, as is ufual, to prevent them from chafing the fail.

The flack fail of the roof of the top fail, will be kept extended tort acrofs the foot, by the reef pendants paffing through cringles in the leech.

The ends of the clewlines may likewife pafs through cringles, in the leech of the top gallant fails if neceffary.

The reef lines, if neceffary, may be in feparate pieces, made faft in the middle and quarters of the fail.

XI.

Experiments on condenfed Gafes. *By* T. NORTHMORE.

To Mr. NICHOLSON,

SIR,

I NOW take the liberty of prefenting you with a continuation of my experiments upon the condenfation of the gafes, but firft beg leave to make one obfervation, viz. that the quantity of gas faid to be injected in each experiment, cannot (particularly in the preceding article) always be depended upon; for its tendency to efcape is fo conftant and powerful, as frequently to elude every effort of mine to prevent it, and if it can find no other exit, it will fometimes efcape by the fide of the pifton of the forcing pump. In the preceding experiments I have endeavoured as much as poffible to obviate this evil, but not always with the fuccefs that I could wifh. Experiments on condenfed gafes.

Repeating the eighth experiment mentioned in my former letter, (fee Vol. XII. p. 372-3) viz. the condenfation of nitrogen upon lime*, in order to difcover the caufe of the lofs of colour in the nitrogen, I perceived that this arofe from its fixation, and a nitrate of lime was the refult. This experiment, on account of the elafticity of nitrogen previous to its change of habitude, requires fome caution; for one of my beft receivers, three-eighths of an inch thick, was Nitrogen condenfed upon lime, produced nitrate.

* Your marginal note fays erronoeufly *lime-water*.

fhivered

shivered in pieces with a violent explosion, after I had set it aside to see the effect of time upon the compressed gas.

Nitrogen and gaseous oxide of carbon condensed, gave nitrous acid, &c.

Experiment 9. Upwards of a pint of nitrogen was condensed, and upon this I pumped one pint of gaseous oxide of carbon. The colour of the nitrogen was destroyed; nitrous acid was formed; and upon collecting the liberated gaseous oxide, it burnt not unlike alcohol. The two gases together were at first highly elastic.

Explosions attributed to nitrogen.

From the facility with which nitrogen becomes united and fixed in various bodies, and from its expansive force when liberated from that state, I know not whether I am sufficiently warranted in suggesting an opinion, that the explosive force of various compounds may in a great measure be attributed to the sudden liberation of this fixed gas. To this cause I partly attribute the fulminating silver of Berthollet; the fulminating gold, and various nitrates; and the detonation which accompanies the decomposition of ammoniac by oxigenated muriatic acid gas.

Attempt to fire phosphorus by condensed air.

Exp. 10. Having been unsuccessful in my endeavours to inflame phosphorus by the compression of atmospheric air, (see *Exp.* 4.) I now tried oxigen, but with little better effect. The phosphorus appeared to be somewhat discoloured, and I thought had a tendency to liquify, as it does when put upon a heated plate of iron. Indeed I have no doubt that some heat is generated by the condensation of air, since the thermometer rises upon external application to the receiver.

Oxigenated muriatic acid gas gave a yellow and highly volatile fluid by condensation.

Exp. 11. Upon the compression of nearly two pints of oxigenated muriatic acid gas in a receiver two and a quarter cubic inches capacity, it speedily became converted into a yellow *fluid*, of such extreme volatility under the common pressure of the atmosphere, that it instantly evaporates upon opening the screw of the receiver. I need not add, that this fluid, so highly concentrated, is of a most insupportable pungency. When atmospheric air was pumped into the empty receiver, it was speedily filled with dense white fumes. There was a trifling residue of a yellowish substance left after the evaporation, which probably arose from a small portion of the oil and grease used in the machine, mixed with some of the concentrated gas; it yielded to sulphuric ether, and destroyed vegetable colours.

This

This gas is very injurious to the machine, and on that account difficult to work.

Exp. 12. Upon half a pint of oxigen was injected one pint of oxigenated muriatic acid gas. The refult was a thicker fubftance which did not fo foon evaporate, and a yellowifh mafs was left behind. Oxigenated muriatic acid and oxigen afforded a thicker fluid.

Exp. 13. Upon half a pint of nitrogen was injected one pint of oxy-muriatic gas. The refult was a ftill thicker fubftance, and the yellow colour deeper, nor did it appear to act fo powerfully upon vegetable colours. Much of the greafe of the machine was carried down in both thefe laft experiments, which formed part of the yellow refidue, and yielded only to ether. Oxigenated muriatic gas and nitrogen.

Exp. 14. Having condenfed about a pint of carbonic acid, the receiver very unexpectedly burft with violence. This circumftance I attribute to the vicinity of the furnace, and I mention it to guard others againft ftanding too near a fire in thefe experiments; nor perhaps may it be ufelefs to add another precaution, that of ufing goggles, or at leaft a thick plate of glafs when examining the refults. Receiver burft, caution.

I now took a new receiver of three cubic inches of capacity, and pumped in one pint of carbonic acid, and upon this rather more than a pint of oxigenated muriatic acid gas. Carbonic acid, and oxigenated muriatic acid.

The union produced a light fap-green colour, but no fluid, though as ufual the oil of the machine had retained enough efficacy to deftroy vegetable colours.

Exp. 15. Upon rather more than a pint of hidrogen, which was highly elaftic, were compreffed two pints of the oxigenated muriatic gas. The refult was a light yellow-green colour, and no fluid. Some fmoke or vapour feemed to iffue out of the receiver upon turning the fcrew, and the gas was highly deftructive of colouring matter. Oxigenated muriatic acid gas on hidrogen.

Exp. 16. I now proceeded to the muriatic acid gas, and upon the condenfation of a fmall quantity of it, a beautiful green coloured fubftance adhered to the fide of the receiver, which had all the qualities of muriatic acid; but upon a large quantity, four pints, being condenfed, the refult was a yellowifh-green glutinous fubftance, which does not evaporate, but is inftantly abforbed by a few drops of water; it is of a highly pungent quality, being the effence of muriatic acid. As this gas eafily becomes fluid, there is little or no elafticity Muriatic acid gas eafily made fluid by condenfation.

elasticity, so that any quantity may be condensed without danger. My method of collecting this, and other gases which are absorbable by water, is by means of an exhausted florence flask (and in some cases an empty bladder) connected by a stop-cock with the extremity of the retort.

An idea here occurs to me, that the facility of fixation which is the property of the compressed muriatic, oxy-muriatic, and some other gases, may be made of some utility to the arts, since by previously pouring in a little water, or other fluid into the receiver, an acid may be obtained of almost any degree of concentration.

Sulphureous acid gas condensed by pressure.

Exp. 17. Having collected about a pint and a half of sulphureous acid gas, I proceeded to condense it in the three cubic inch receiver, but after a very few pumps the forcing piston became immoveable, being completely choked by the operation of the gas. A sufficient quantity however had been compressed to form vapour, and a thick slimy fluid of a dark yellow colour began to trickle down the sides of the receiver, which immediately evaporated with the most suffocating odour upon the removal of the pressure. This experiment corroborates the affirmation of Monge and Clouet, mentioned in Accum's chemistry, vol. I. p. 319. viz, that " by extreme artificial cold, and a strong pressure exerted at the same time, they rendered sulphureous acid gas fluid. From the injury which this gas does to the machine, it will be very difficult to perform any experiments upon its elective attractions with the other gases.

I remain, Sir,

Your obedient humble Servant,

T. NORTHMORE.

Devonshire Street, Portland Place,
Feb. 15, 1806.

On

XII.

On the Probability that Muriatic Acid is composed of Oxigen and Hidrogen. In a Letter from Mr. J. Martin.

To Mr. NICHOLSON,

SIR,

LATE experiments in galvanism have furnished sufficient grounds to suspect, that the muriatic acid is an oxide of hidrogen, and I have been somewhat strengthened in this supposition by the well known fact, that hidrogen gas is always liberated upon effecting a solution of tin in muriatic acid: this phenomenon has been accounted for, by supposing the water which held the muriatic acid in solution to be decomposed; its oxigen seizing the metal which thereby became disposed to be taken up by the acid and the hidrogen, the other constituent part of the water being liberated under the form of gas: however plausible this hypothesis might seem, I did not think it perfectly satisfactory, for if the acid consisted of oxigen and hidrogen, part of the oxigen might unite to the metal to render it fit to be dissolved by the remaining acid, and its hidrogen of course given out under the gaseous form, in this case no decomposition of the water would take place, or at least these phenomena might happen without that decomposition.

Facts induced in favour of the position, that muriatic acid may be an oxide of hidrogen.

To clear up these doubts I procured an earthern tube into which was introduced some iron wire; the tube was made to traverse a furnace; to the one end was luted a bent tube, brought under the shelf of a pneumatic trough, and to the other was adapted a tubulated retort, containing some muriate of soda carefully freed from its water of crystallization When I supposed the iron wire was sufficiently ignited, I affused some dense sulphuric acid over the muriate of soda; as soon as the atmospheric air which the vessel contained was nearly expelled, hidrogen gas was liberated from the other extremity of the tube in considerable quantities, mixed however with a small portion of muriatic acid gas; after the operation had been suffered to go on some time, the apparatus was taken to pieces, and crystals of muriate of iron were found in the tube. May we

Experiment. Muriatic acid gas disengaged from decripetated sea salt by sulphuric acid, was passed over ignited iron. Hidrogen was liberated.

It is inferred that this came from the acid.

we not from this experiment be ſufficiently authorized to conclude, that muriatic acid is compoſed of oxigen and hidrogen, and that hidrogen gas is liberated in conſequence of part of the oxigen of the acid uniting to the metal to prediſpoſe it to unite to the remaining acid?

It is to be remarked, that the hidrogen gas was liberated in ſuch abundance as to do away every idea, that it might proceed from any water which the gas accidently held in ſolution.

Should you deem theſe obſervations of ſufficient value, an inſertion of them in your valuable journal will greatly oblige,

Sir,

Your moſt obedient,

and moſt humble Servant,

J. MARTIN.

Crown-Court, Old Bread Street,
February 20, 1806.

XIII.

Subſtance of a Memoir read before the Society of Emulation, at Amiens, by Meſſrs. Reynard *and* Facquer, *on the foul Air of Oil Ciſterns* *.

Fatal effects of the foul air of an oil ciſtern.

M. ACHILLE POULAIN, ſoap-maker at Amiens, and one of his workmen having been killed by the foul air of an oil ciſtern, into which the latter had fallen in an attempt to cleanſe it, and the former in endeavouring to ſave the man's life, Meſſrs. Reynard and Facquer were induced to make an analyſis of the deleterious vapour which had cauſed this melancholy accident.

Dimenſions of the ciſtern.

The ciſtern meaſured about twelve feet in every direction. Its mouth is ſecured with a ſmall cover which completely excludes the external air.

Appearance of the oil.

The vegetable oil, of which only a ſmall quantity at a time had been depoſited in this ciſtern, was thick, viſcid, and even in ſome places gelatinous, yielding a ſtrong rancid effluvium.

* Annales de Chimie, Vol. LVI.

A lighted

A lighted candle on being let down into the ciftern, was inftantly extinguifhed.

The furface of lime-water, when included for a few minutes in a broad veffel, was flightly tinged with prifmatic colours.

To obtain the gas for experiments, bottles filled with water were lowered into the cifterns, and emptied at various depths.

On the gas obtained from about two feet below the mouth of the ciftern, the following experiments were tried: Obfervations on the gas.

1. A cylindrical veffel being filled with the gas, kept in contact with lime-water, during fifteen days, with frequent fhaking, caufed a fmall diminution in the bulk of the gas.

2. The fame experiment repeated with ammoniac offered a fimilar refult.

Thefe two experiments denote the prefence of carbonic acid gas.

3. The gas remaining from the two former experiments, when put in contact with liquid hydrogenated fulphuret of potafh, underwent an abforption of eight centimes; which muft have been oxigen.

The gas taken within a foot of the bottom of the ciftern afforded fimilar refults, only the proportion of carbonic acid gas was greater. That which remained after the effect of reagents was azote, as the following phenomena prove. It contained carbonic acid.

1. A lighted candle was extinguifhed by immerfion in the gas at the upper part of the cylindric veffel; but it remained burning if the veffel was previoufly opened for a few feconds.

2. The veffel when reverfed loft none of the gas contained in it; and the light was extinguifhed when introduced.

3. The luminous combuftion of phofphorus in oxigen gas (the formation of nitric acid with this gas and oxigen gas not having been tried) was confidered a pofitive proof of its nature.

This noxious gas was found to contain, Analyfis.

Upper Part,		Lower Part,	
Azotic gas - -	86	Azotic gas - -	80
Oxigenated gas - -	8	Oxigenated gas - -	6
Carbonic acid gas -	6	Carbonic acid gas -	14
	100		100

 The

Chemical agency insufficient to deſtroy the foul air.

The nature of this gas does not admit of purification by lime or ammonia. Theſe indeed deſtroy the carbonic acid, but have no influence on the azote.

Mechanical means more effectual.

Mechanical means are the only methods by which any conſiderable quantity of this air can be ſpeedily removed; ſuch as the firing of gun-powder, the uſe of ventilators, &c.

Deſtructive effects of confined air cauſed by the preſence of azotic gas.

The reſult of this analyſis is rather ſurpriſing, as, inſtead of a ſuperabundance of carbonic acid gas, which was ſuppoſed to be the cauſe of the deſtructive effects of this confined air, azotic gas has been found—a gas lighter than atmoſpheric air.

Theory.

The theory of this reſult ſeems to be, that the oil having deprived the encloſed air of its oxigen, leaves only the azotic gas at liberty.

XIV.

Extract from a Memoir, by Meſſrs. FOURCROY *and* VAUQUELIN, *on the Phenomena obſerved in, and the Reſults obtained from Animal Matter, when acted upon by Nitric Acid. Read at the National Inſtitute, by* A. LAUGIER.*

Berthollet's experiments on azote,

THE exiſtence of azote in animal ſubſtances has been determined by the experiments of M. Berthollet, and the diſengagement of this principle, when treated with nitric acid, is among the moſt uſeful of modern diſcoveries in chemiſtry.

—repeated.

Meſſrs. Fourcroy and Vauquelin, on repeating theſe experiments on muſcular fibre, have added ſome intereſting reſults to this valuable fact.

The following is a ſummary of their experiments, and of the reſults which they obtained.

Nitrous acid with muſcular fleſh gave azote and ſome carbonic acid.

SECT. 1. A mixture of 150 grammes of muſcular fleſh, with an equal quantity of nitric acid, at 32 degrees, and water, put into a mattraſs, and heated till it boiled gently, gave 96 cubic inches of gas, containing nine-tenths of azote, and one-tenth of carbonic acid.

The reſiduum contained fibrous matter, yellow liquor, and a greaſy ſubſtance.

The reſiduum conſiſted of, 1, Matter which had not loſt its original fibrous formation; 2, a yellowiſh liquor; 3, a greaſy ſubſtance, of a yellow colour, which floated on the ſurface of the liquor.

* *Annales de Chimie*, Vol. LVI. p. 37.

After

After ſeparating the greaſe, and filtring the liquor, the reſidue was ſubmitted to the following experiments.

To boiling water it gave a yellow colour, and the property of reddening vegetable blues: After waſhing in ſeveral waters, it continued to turn the colour, though it ceaſed to give acidity. Waſhing rendered its colour deeper than at firſt; and when diffuſed in a little water, it ſtill reddened paper of turnſol.

Its ſolution in alcalis was of a deep blood colour. It was precipitated by acids in yellow flakes.

This matter feels fat and pitchy; has a rancid ſmell, and very bitter taſte. The fibrous matter reſembles fat.

The fuſion and ſwelling which it undergoes when placed on hot coals, the greaſy vapour, and fetid colour, produced by this operation; the ſmall quantity of coal which it leaves, ſhews its reſemblance to fat ſubſtances, notwithſtanding its acidity.

Sect. 2. On a cloſer inveſtigation of the yellow matter, the following characteriſtics and properties were obſerved:

It ſo ſaturated alcalis as nearly to maſk their properties.— It ſaturates alkalis. Its combinations with potaſh and ammonia lathered like ſoap and water, and are not decompoſed by carbonic acid, but precipitated the ſolutions of mercury and lead in yellowiſh white flakes.

The yellow matter decompoſed alcaline carbonates, in the Decompoſes carbonate. cold, with effervеſcence, and likewiſe the acetate of potaſh, with the aſſiſtance of water, and a gentle heat.

The authors of the memoir next made uſe of alcohol, and It is a yellow acid and fatty matter. found that the yellow matter was compoſed of a ſmall quantity of fat, which was taken up by the alcohol; and of an acid, which, on account of its colour, they denominated "*yellow acid.*" This acid, when deprived of its fat, which occaſions an alteration in its properties, was of a deeper colour, more readily reddened the paper of turnſol, did not melt in the ſame manner as before, nor exhale the ſame rancid ſmell, but fetid and ammoniacal vapours.

The yellow acid is diſſolved in the fat, to which it com- The yellow liquor is an animal acid, conſiſting of azote, hydrogen, carbon and oxygen. municated acidity and rancidneſs. It combined with ammonia, and deprived it of its ſmell; and by diſtillation it yielded all the products of animal ſubſtances. Its conſtituent principles, therefore, are azote, hydrogen, carbon, and oxigen; and it muſt be placed among animal acids.

Sect. 3.

Sect. 3. The combination of yellow acid and fat, on being again ſubmitted to the action of nitric acid, at a temperature of about 50 degrees, underwent no remarkable alteration.—Its colour changed from yellow to white; its ſpecific gravity was diminiſhed, as was likewiſe its bulk; but without any motion or efferveſcence in the acid. Blue colours were deeply reddened by it; it diſſolved, as before, in the ley of potaſh, to which it communicated an orange-red colour, and had an extremely acrid taſte. The action of nitric acid upon this yellow matter ſeems confined to giving it properties which make it approximate to an oily ſtate, without deſtroying its original acid character.

Experiments on the nitric acid wherein the muſcular fleſh had been decompoſed.

Sect. 4. It was of importance that the nitric acid with which the muſcular fleſh had been decompoſed, ſhould be examined. Its yellow colour reſembled that of the ſolution of chromate of potaſh. When ſaturated with carbonate of potaſh, the liquor at firſt acquired an orange colour, afterwards it became turbid, and depoſited a ſmall quantity of orange-red powder. On diſtillation, this mixture afforded a clear liquid, void of colour, of a rancid ſmell, containing a little ammonia, probably formed by the nitric acid. What remained in the retort, was of a blackiſh brown colour, but it was not farther examined.

A colourleſs liquor, having the ſame taſte and ſmell, was afterwards obtained by diſtillation of another portion of the nitric acid uſed in the decompoſition of the muſcular fleſh. The liquor remaining in the retort became yellow by concentration, and its re-action upon nitric acid was quickly perceived in a copious emiſſion of red vapours. When reduced to 40 grammes, flattiſh cryſtals were formed in a thick mother-water, whoſe tenacity was ſimilar to that of the ſolution of gum.

This mother-water poſſeſſed an acid bitter taſte, and on the addition of a little cauſtic potaſh, became of a blood-red colour: mixed with alcohol, it depoſited a white flaky ſediment, which afterwards formed itſelf into fine ſemi-tranſparent grains, of a pleaſant acid flavour.

Five decigrammes of this ſalt, on being calcined, left 21 centigrammes of yellowiſh very light reſiduum, which effer-veſced and were diſſolved in nitric acid, and on being evaporated produced cryſtals of ſulphate of lime and nitrate of potaſh.

This

This saline precipitate, obtained by means of alcohol, was ascertained to be a mixture of sulphate of lime and acidulous oxalate of potash.

The mother-water, after precipitation with alcohol, gave a second precipitate with lime-water, consisting of oxalate of lime. After this double operation with alcohol and lime-water, the mother-water, on being gradually evaporated, became converted into the brown viscid syrup, of a bitter taste, like that of walnut shells. This being mixed with a good quantity of alcohol, coagulated, and threw down a plentiful precipitate of white matter. This matter was very pure malate of lime, the alcohol having retained the yellow acrid substance.

The learned authors of the memoir, of which we have given this detailed extract, conclude from the facts above stated, Conclusions.

1. That the muscles contain potash, lime, and sulphuric acid, or perhaps sulphur burned by nitric acid.

2. That a portion of the muscular fibre, or rather the cellular membrane with which it is enveloped, was converted by the action of the nitric acid into oxalic acid and malic acid.

The alcohol employed in the separation of the malate of lime, held in solution, 1, A small portion of nitrate of lime; 2, A very bitter red-brown matter, possessing the flavour of walnut rhinds, of which more will be said hereafter; 3. A small quantity of that detonating matter already found in indigo: it was in this case obtained by concentrating the alcoholic solution, and separating it by the addition of carbonate of potash, in the form of granulated crystals, very inflammable, and very detonating.

SECT. 5. The importance of the results obtained from the foregoing analysis will be readily understood; particularly if a comparison be made of the knowledge hitherto possessed, with the extensive notions here opened to the view, of an object so interesting in the consequences which may be drawn from it, in the applications which may be made to the animal economy, and which, as will be shewn, leaves scarcely any thing more to be desired. Importance of the foregoing analysis.

The disengagement of azotic gas, the formation of carbonic acid, of fat, of oxalic acid, and of a bitter substance, constitute the whole that was known respecting the treatment of animal Discoveries added to what was formerly known on this subject.

animal ſubſtances by nitric acid; to this is now added the diſcovery, 1, Of a yellow inſipid matter, of little ſolubility, though acid, and which immediately ſucceeds the fleſhy fibre; 2, Of another yellow matter, bitter, more ſoluble, and equally acid, which remains diſſolved in the nitric liquor; 3, Of an inflammable, detonating ſubſtance, which is alſo retained in ſolution; 4, and laſtly, of the formation of malic acid.

It appears, and is the opinion of Meſſrs. Fourcroy and Vauquelin, that the yellow and nearly inſoluble matter is the firſt degree of change produced upon the muſcular fibre; it paſſes quickly to the ſecond degree of alteration and of acidity, whoſe product is the more ſoluble yellow matter: this, by a third degree of alteration is ſucceeded by the inflammable detonating ſubſtance, being the third and laſt term of the decompoſing action of nitric acid. The authors of this memoir attribute the ſucceſſive formation of theſe three compounds to the ſubtraction of part of the azote, and of a more conſiderable portion of the hydrogen: by this means the proportions of their elements are changed, and there remains an exceſs of carbon and of oxigen, which produces the ſtate of fat and acidity already noticed. As to the proportion of the conſtituent principles of theſe three compounds, it is a problem of too remote a nature for its ſolution to be readily diſcovered.

Acidity of the yellow ſubſtance not cauſed by nitric acid.

Meſſrs. F. and V. examined if the acidity of the yellow ſubſtances might in any meaſure ariſe from nitric acid; but, after a careful inveſtigation, they were ſatisfied that it was in no degree preſent.

Formation of oxalic and malic acids.

The formation of oxalic and malic acids belongs to the white mucous ſcales of the cellular membrane. Comparative experiments of the effects of nitric acid on the white membranaceous organs, which furniſhed plenty of theſe acids, and very little of the fat yellow matter, led the authors to this concluſion.

Sect. 6. A few inſulated facts, which hitherto have ſcarcely appeared to be ſuſceptible of any uſeful application, ſeem to unite with thoſe preſented by this analyſis; and the learned chemiſts, to whom we are indebted for it, have not omitted to connect them with the other facts. Such are thoſe which are obtained by examining the bilious concretions in certain animals; thoſe in the gall-bladder of the ox and elephant; and the analogy which appears to exiſt between bile, the

the colour of the ſkin in perſons afflicted with the jaundice, and alſo their urine, and the yellow ſubſtance treated of in this memoir. Analogy of the yellow matter to bile, jaundice, &c.

New experiments made with a view to confirm theſe ſuſpicions obtained the moſt happy reſults. The red matter of bilious concretions, when ſeparated from the bitter green matter with which it is combined, diſplayed ſimilar properties with the firſt yellow matter obtained from muſcles acted upon by nitric acid. Bilious concretions.

From the urine of a young man troubled with a ſlight jaundice, they obtained a red ſubſtance, whoſe identity with the matter formed by muſcles and nitric acid was remarkable. To obtain this, they evaporated the urine to the conſiſtency of honey, and treated the reſiduum with alcohol: this contained, beſides much of uree, ſal-ammoniac, and acetate of ſoda, of which the patient made uſe, the red ſubſtance they ſought for. It was found in the urine of an icteric ſubject.

From theſe experiments, made with ſkill and ability, may we not conclude with the authors, that the jaundice is occaſioned by a ſuperabundance of this matter introduced to the cutaneous abſorbent ſyſtem; that this is what gives a yellow colour to bile and bilious calculi, which diſplay, on analyſis, the ſame properties; and that the yellow acid is diſperſed throughout the animal economy, either by the oxigenation of the muſcular fibre, or of the ſanguineous fibrine, from which it is formed? Jaundice occaſioned by a ſuperabundance of the yellow acid; which alſo cauſes the yellow colour of bile, &c.

Neither can we avoid admitting a ſtriking analogy between this yellow acid matter, and the acid found in fat after long expoſure to the air, or that has contracted a yellow hue through diſeaſe, and fat treated with nitric acid to form oxigenated pomatum. Reſemblance of the yellow acid and rancid matter of fat.

It muſt be confeſſed that theſe conjectures aſſume much probability, when we conſider that the acetate of ſoda, alcaline carbonates, and yolks of eggs, are the remedies beſt adapted for the cure of the jaundice, and form alſo the beſt chemical ſolvents of the yellow acid, or of the acid and fat matter, which ſo evidently characteriſe the jaundice. Other facts.

After what has been ſaid, it muſt no longer be imagined that the hope of tracing the cauſe of morbific affections, is altogether chimerical: nor that diſcoveries in chemiſtry, and attentive Chemical reſearches not to be neglected by phyſicians.

reſearches

researches respecting animal matter, will not enlighten the physician on the nature of diseases, and the means of curing them.

XV.

Remarks relative to Dr. HERSCHEL'S *Figure of Saturn.* *By* AN OBSERVER.

To Mr. NICHOLSON.

SIR,

Singular circumstance that Dr. Herschel's figure of Saturn had not been before observed.

ON reading in your Journal, Observations on the singular Figure of the Planet Saturn, by Dr. Herschel, from the Philosophical Transactions; when I saw the engraving of the figure, as described by the Doctor, resembling a parallelogram, one side whereof is the equatorial and the other the polar diameter, with the four corners rounded off, so as to leave both the equatorial and polar regions flatter than they would be in a regular spheroidical figure; I was surprised to find, on enquiry, that so remarkable a figure had not been noticed before by other astronomers, whose telescopes were supposed to define objects very correctly, with powers considerably exceeding 160 times, by which power the Doctor could distinguish Saturn from the spheroidical figure of Jupiter.

Former obs. of the Doctor did not shew it.

In the year 1776, the Doctor relates he perceived the body of Saturn was not exactly round, and in 1781, that it was flattened at the poles, at least as much as Jupiter. In 1789, the Doctor being then prepossessed with its being spheroidical, he measured the equatorial and polar diameters, and supposed there could be no other particularity to remark in the figure of the planet.

It is evident, from the Doctor's former observations of Saturn and Jupiter, that the visible difference in their figures was not, before last year, observed so distinctly, owing to the superior excellence of his 10-feet telescope of two feet aperture, but that, when observed, he afterwards found the other telescopes gave a similar disparity.

Q. whether there was no deception in the telescopes.

As the figures given by former astronomers, and even by the Doctor himself, of both Jupiter and Saturn, were spheroidal,

dal, it may be requiſite, before any intricate reſearches are attempted (as mentioned by the Doctor at the end of the communication), to be well aſſured that his teleſcopes have defined the figures of the planets accurately, which at preſent admits of a doubt, and which may be cleared up about the time of the next oppoſition of the Sun and Saturn, in April next.

The following may prove the neceſſity of ſuch an enquiry:

Experiment. An object ſeen by oblique reflection from a ſpherical mirror, is rendered oblong.

Place a circular or ſpherical figure before a concave mirror, which mirror muſt be ſo inclined, that when the object is above the head of the obſerver, it may be ſeen, by reflection, in the center of the mirror *: If ſeen within the focus, the object will be repreſented oval in a vertical direction, and when beyond the focus, in a horizontal; which figure will be more and more oval as the angle is enlarged.

Your's,

AN OBSERVER.

XVI.

Experiments on a Mineral Subſtance formerly ſuppoſed to be Zeolite; with ſome Remarks on two Species of Uran-glimmer. By the Rev. WILLIAM GREGOR.†

Deſcription and analyſis of a mineral from Cornwall.

THIS mineral is raiſed in a mine called Stenna Gwyn, in the pariſh of St. Stephen's, in Branwell, in the county of Cornwall; the principal production of which is the compound ſulphuret of tin, copper, and iron.

Deſcription.

Two ſpecies of this mineral are found, aſſuming a marked difference in external character.

The firſt and moſt common one conſiſts of an aſſemblage of minute cryſtals, which are attached to quartz cryſtals, in tufts, which diverge from the point of adherence, as from a centre. Theſe tufts vary, as to the number of cryſtals, of which they are

* If the object is ſmall, it may be enlarged by a concave eye-glaſs.

† Phil. Tranſ. 1805.

Description and analysis of a mineral from Cornwall.

are compofed, and are light and delicate in the forms which they affume, or they are grouped together according to a variety of degrees of proximity and compactnefs. Sometimes they fill the whole cavity of a ftone, with little or no interruption; in other fpecimens they are feen partially fpreading over the fides and pointed pyramids of quartz cryftals.

In fome cafes thefe grouped tufts adhere very pertinaceoufly to the ftone which bears them; in others, they are eafily feparable, in comparatively large pieces, from the quartz, the impreffed form of which the pieces thus feparated retain. The furface of thefe, which was in immediate contact with the quartz, exhibits the feveral minute cryftals of which the mafs confifts, matted together in various directions.

Thefe cryftalline affemblages are, in general, white; a nearer infpection of the individual cryftals proves that they are tranfparent. Sometimes they are ftained of a yellowifh hue by ochry water.

The fize of thefe cryftals varies confiderably in different fpecimens. Sometimes they affume the appearance of a white powder raifed up in fmall heaps, upon the furface of the ftone, to which they adhere. In other fpecimens they refemble a tender down. And the larger fort varies, in relative fize, in the proportion, perhaps, in which a human hair, horfe-hair, and a hog's briftle, feverally differ from each other in magnitude. They feldom exceed a quarter of an inch in length. The figure of thefe cryftals is not eafily afcertainable, on account of their minutenefs. By the help of a very powerful microfcope, they appear to confift of four-fided prifms; where thefe are broken off, the fection exhibits a rhomboidal, approaching indeed to an elliptical figure, from the circumftance of the angles of the prifm being worn away; but that the prifm itfelf is rhomboidal, cannot be inferred from hence, unlefs we could be certified, that the fection were at right angles with the axis of it.

Imbedded amongft thefe cryftals two fpecies of cryftalline laminæ are frequently difcoverable; the one confifting of parallelopipedon plates with truncated angles, applied to each other, of a green colour of various tints, from the emerald to the apple-green: the other fpecies, confifting of an affemblage of fquare plates, which vary in thicknefs. The angles of the feveral fquare laminæ, which are applied to each other, are not

not always coincident. They are of a bright wax yellow. The fides of the largeft of thefe fquare laminæ is about a quarter of an inch. This laft fpecies is frequently found adhering to the fides of quartz cryftals, in the cavities of granite. Defcription and analyfis of a mineral from Cornwall.

The other fpecies of this mineral confifts of an affemblage of cryftals clofely compacted together in the form of mammillary protuberances, in general, of the fize of fmall peas, intimately connected with each other. A ftratum of thefe about $\frac{1}{8}$ of an inch thick, is fpread upon a layer of quartz, in the cavities or fiffures of a fpecies of compact granite. The ftriæ of which thefe mamillæ confift, diverge from a centre, like zeolite. Some of the individual ftriæ, in fome cafes, overtop their fellows, in thefe globular affemblages, and evidently affume, on their projecting points, a cryftallized form.

A.

(1.) The detached cryftals of the former fpecies are eafily reduced to powder, of a brilliant whitenefs. At the temperature 56° of Fahrenheit, its fpecific gravity was found to be 2,22.

(2.) The hardnefs of the more compact fpecies is fufficient to fcratch calcareous fpar. At the temperature 55°, its fpecific gravity was 2,253. It does not imbibe water.

(3.) Some of the cryftals expofed, on charcoal, to the flame of the blowpipe fuddenly and ftrongly driven upon them, decrepitate: if they are gradually expofed to the flame they grow opaque, and become more light and tender: but they fhow no figns of fufion under the ftrongeft heat.

(4.) The phofphate of foda and ammonia takes up a piece of this mineral without effervefcence, but it fwims about the fufed globule, unaltered. Borax diffolves a fragment of a cryftal, and the globule remains tranfparent.

(5.) Some of this mineral, reduced to a fine powder, was mixed with about half its weight of pounded quartz, and kneaded with water into a ball: but as foon as the mafs became dry, all cohefion was deftroyed, and it fell into powder.

(6.) Sulphuric acid, poured upon fome of it, caufed no effervefcence, nor was there any perceptible vapour extricated.

(7.) Some of the pulverized cryftals were put into a crucible of platina, and fulphuric acid was poured upon them. The

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crucible was covered with a piece of glafs, and placed in warm fand. On examination of the crucible and its contents, after fome time, it appeared that the greater part of the mineral had been diffolved, but the furface of the glafs cover was not in the leaft affected.

(8.) Some of the cryftals were introduced into a fmall glafs retort, to which a receiver was adapted. The retort was expofed to the heat of a charcoal fire. A fluid diftilled over into the receiver, which had a peculiar empyreumatic fmell. It changed litmus-paper to a faint red. It produced no change in a folution of nitrate of filver; but it caufed a white precipitate in a folution of nitrate of mercury. I attributed thefe phænomena, at the time, to a fmall bit of the feather with which I had fwept the powder into the retort, and which, I thought, had fallen into it. A flight whitifh cruft was alfo produced in the neck of the retort, but the fmallnefs of the quantity did not admit of examination.

(9.) Some of this mineral, expofed to a red heat for about ten minutes, loft in weight at the rate of $25\frac{5}{6}$ *per cent.* Another portion, expofed to a ftronger heat for more than an hour, loft $30\frac{3}{4}$ *per cent.* This operation was performed in a crucible of platina; the cover of which gave fome indications as if a flight portion of the finer parts had been volatilized.

Some of the compact fpecies, after expofure to a red heat for one hour, experienced a diminution in weight of 30 *per cent.*

(10.) The fulphuric, muriatic, and nitric acids, aided by a long digefting heat, effect nearly a complete folution of this fubftance. The quantity of the undiffolved refiduum is diminifhed in proportion to the purity of the mineral employed.

(11.) The nitrate of filver, as well as the muriate of barytes, produce no change in the folution of this fubftance in nitric acid.

(12.) The folutions of this fubftance in muriatic and nitric acids, cannot be brought to cryftallize.

B.

(1.) I felected fome of the cryftals of this fubftance, as free as it was poffible from extraneous matter. 50 grains grofsly pounded were expofed, in a platina crucible, to a red heat for one

one hour. They weighed, *whilst still warm*, 35$\frac{7}{8}$ grains, which is a loss of 28$\frac{1}{4}$ *per cent.* 25 grains of the same parcel, from which I had taken the former, exposed to a heat of longer continuance and greater intensity, were diminished in weight, at the rate of 30$\frac{1}{4}$ *per cent.* Description and analysis of a mineral from Cornwall.

(2.) The powder still preserved its pure whiteness. It was transferred into a matrass, and nitric acid poured upon it, which soon began to act upon it. The matrass was placed, for many hours, in a digesting heat. A solution of the whole of the substance, except a small portion, was effected. I added a few drops of muriatic acid, and continued the digestion.

(3.) The acid was now diluted with distilled water, and poured off from the residuum, which consisted partly of a fine spongy earth, and partly of fragments of quartz. It was caught on a filter and sufficiently edulcorated. The last portion of edulcorating water dropped through the filter of an opalish hue.

The residuum, dried and exposed to a red heat, for ten minutes, $= \frac{3}{16}$ of a grain, $\frac{1}{16}$ of which consisted of fragments of quartz, $\frac{1}{32}$ was found to be silica, and $\frac{3}{32}$ alumina.

C.

(1.) The clear solution and edulcorating water were poured into a large matrass and boiled, and whilst boiling, the contents were precipitated, in white flakes, by ammonia.

(2.) When the ammonia had ceased to produce any further precipitate, the clear fluid was decanted, and assayed with carbonate of ammonia. But its transparency was not in the least disturbed.

(3.) This clear fluid, together with the edulcorating water, with which the subsided precipitate had been washed, was gradually evaporated. When its volume was considerably diminished, a separation of a spongy earth took place, more copiously than I had reason to expect, and the quantity of it was still further increased by a few drops of ammonia. This earth, thus separated, was sufficiently edulcorated, and added to the former precipitate.

(4.) The fluid was again evaporated, and at last transferred to a crucible of platina, and the salt reduced to a dry state: on redissolving this salt in distilled water, a minute portion of

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earthy matter was feparated, which, after edulcoration, was added to the reft. The fluid from which it had been feparated, and the edulcorating water, were again evaporated to drynefs, and the ammoniacal falt expelled by heat, in a platina crucible.

(5.) After the crucible had been made red hot, it was examined. I difcovered on the bottom of it, fome traces of earthy matter, and fome fpots, which had a glaffy appearance. Water boiled upon it, diffolved nothing; from which circumftance, the abfence of both of the fixed alkaline falts may be inferred. Neither did nitric acid produce any alteration. A few drops of fulphuric acid effected a folution of the fubftance, which adhered to the bottom of the crucible. Ammonia precipitated from it a fmall quantity of earth, which was transferred to the reft, and the fulphate of ammonia and edulcorating water were again evaporated and expelled by heat. A few fpots of the fame glazing ftill appeared. I had obferved the fame phenomenon in a former experiment: but in that, as well as in the prefent inftance, the fubftance was in too fmall a quantity to become the fubject of experiment.

D.

(1.) Upon the precipitate (C 1), and the earths collected at different times, whilft they were in a moift ftate, I poured a folution of potafh in alcohol mixed with diftilled water; in a fhort time, the greater part of it was diffolved.

The clear folution was decanted, and the undiffolved fediment was transferred to a bafon of pure filver, and boiled with a folution of potafh.

(2.) When the potafh ceafed to act upon it, it was diluted with diftilled water and decanted from a brown powder, which had fubfided. This powder edulcorated, dried, and ignited weighed $\frac{7}{16}$ of a grain; $\frac{1}{4}$ of a grain was alumina, $\frac{3}{32}$ filica, and $\frac{3}{32}$ oxide of iron.

E.

(1.) The folution effected by potafh was decompofed and rediffolved by muriatic acid, and the contents of the folution were precipitated by ammonia. The fubfided precipitate was edulcorated.

(2.)

(2.) The fluid and the edulcorating water were evaporated to drynefs, and rediffolved in diftilled water. Here again, to my furprife, a feparation took place of a white earth, more abundant than is ufual in cafes where ammonia is employed as a precipitant.

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(3.) This earth and the precipitate were edulcorated with diftilled water, until it ceafed to affect a folution of nitrite of mercury. Collected, dried, and ignited, for one hour it weighed *whilft ftill warm* 32 $\frac{8}{16}$.

F.

(1.) This earth was placed in a crucible of platina, and repeatedly moiftened with fulphuric acid, which was abftracted from it in the fand bath; diftilled water affected the folution of the whole, except a white powder which weighed, after ignition, $2\frac{7}{32}$ grains. It was proved to be filica.

(2.) This folution was now mixed with fome acetat of potafh and gradually evaporated; large and regular cryftals of alum were from time to time formed. A fmall portion of filica which weighed after ignition $\frac{1}{32}$ of grain was depofited; fome fulphate of lime alfo made its appearance, which wafhed with diluted alcohol and dried in a low heat $= \frac{7}{16}$ of a grain.

(3.) A portion of the fluid remained which neither the addition of potafh nor the lapfe of many weeks could induce to cryftallize. Sufpecting that it might contain glucine, I precipitated the contents by carbonate of ammonia, added to excefs, and fhook the mixture repeatedly and ftrongly. The precipitated earth was collected and the fluid boiled, but it was found to contain nothing but a minute portion of alumina.

(4.) The edulcorated earth was rediffolved in fulphuric acid, except $\frac{5}{8}$ of a grain of ignited filica.

The folution was mixed with a little potafh, and gradually evaporated. Sulphate of lime was feparated at feveral times and after long intervals, which fufficiently wafhed and dried in a low heat $= \frac{9}{32}$ .Some filica alfo feparated, but too minute in quantity to be afcertained by weight. The remaining fluid at length cryftallized into regularly formed alum.

(5.) The whole, therefore of the 32 $\frac{8}{16}$ (E. 3.) confifted of alumina except $2\frac{7}{8}$ of filica, and the lime contained in $\frac{23}{32}$ of fulphate of lime, which may be eftimated about $\frac{3}{16}$ of a grain; the alumina, therefore, $= 29$; the alumina in B. and D. $= \frac{12}{32}$

 the

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the filica in B, D, and F, $= 3\frac{1}{16}$; the oxide of iron (D.) $= \frac{3}{32}$, and lime F, $\frac{3}{16}$; the volatile parts of this fubftance $= 15\frac{3}{8}$ in the 50 grains employed.

The fum total of thefe is - - - -	$47\frac{1}{16}$
Lofs - - - - - - - -	$2\frac{15}{16}$
	50

I have fubjected thefe cryftals, as well as the harder fpeices of this mineral, to analyfis by means of direct folution in fulphuric acid, and have found in each cafe the fame fixed ingredients, viz. alumina, a fmall portion of filica, and a very minute quantity of lime. Both thefe latter ingredients are, I think, effential to the compofition of this foffil, as I have always difcovered them in the pureft fpecimens. In this mode of analyfis I experienced the fame difficulty and tedioufnefs of delay in bringing the laft portions of the folution to cryftallize into alum. This anomalous circumftance I have reafon to attribute to a particular combination, which takes place between the fulphate of alumina and lime, filica, and potafh. In my examination of the compact fpecies there was no appearance of the fulphate of lime until the laft; and in every experiment, previoufly to the frefh appearance of cryftals of alum that had been long delayed, filica and fulphate of lime were depofited.

I forbear entering into any further details concerning my former experiments on this curious foffil, as I have reafon to think that it will ftill require a more particular and minute examination, on account of another ingredient which eluded my notice, and which may poffibly impart to its peculiar character. The fcarcity of it has been hitherto a great bar to my experiments; I fhall record, however, a few facts which I have lately obferved, in the hope that at a future time I may be able to refume my examination of it.

I was induced to pay more attention to the volatile ingredients of this fubftance *. With this view, I introduced fome

* Mr. Humphry Davy, whofe well known fkill and fagacity have probably rendered the refearches of another perfon fuperfluous, had, I found, been engaged in the analyfis of a mineral which is thought to be identical with the fubject of thefe obfervations. He informed me that he had obferved a peculiar fmell, and acid properties in the water diftilled from the fubftance which he examined,

of the cryſtals into a ſmall retort, adapted a receiver unto it, and expoſed the retort to a charcoal fire. The neck of the retort was ſoon covered with moiſture, which paſſed into the receiver; and I obſerved a white cruſt gradually forming in the arch and neck of the retort.

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On examination of the fluid in the receiver, it was found to have the ſame empyreumatic ſmell that I had obſerved before. It reſembles very much the ſmell which that fluid is found to have which is diſtilled from the white cruſt that ſurrounds flint as a nucleus.

It changed litmus paper to a faint reddiſh hue. It produced no change on a ſolution of nitrate of ſilver, and ſcarcely a perceptible one, on that of nitrate of mercury.

The cruſt formed in the neck of the retort conſiſted of thin ſcales, which after the veſſel had been dried, were diſpoſed to ſeparate from the glaſs in ſome places, but in others they firmly adhered unto it. They were opaque, like white enamel, and reflected the colours of the rainbow. A portion of this ſubſtance expoſed to the flame of the blow-pipe upon charcoal turned at firſt black, and then melted into a globule, that exhibited ſomewhat of a metallic ſplendor which ſoon grew dull. This ſubſtance is ſoluble in water; on evaporation of it, it aſſumes, at the edges of the fluid, a ſaline appearance, which, as the moiſture evaporates, becomes earthy, opake, and white. Some of the ſolution changed litmus paper to a faint red. Lime and ſtrontian waters produce in it white clouds, which a drop of nitric acid removes. Muriats of lime and barytes produce no change in it. Nitrate and acetate of barytes diſturb its tranſparency, the effect produced by the latter is more evident. Nitrate of ſilver produces no effect, but nitrates of mercury and lead cauſe copious precipitates, which are white and ſoluble in nitric acid. Phoſphate of ammonia and ſoda produced a white precipitate. Oxalate, tartrite, and pruſſiate of potaſh did not affect it, nor did ſulphate of ſoda. Ammonia was dropped into it, but the fluid preſerved its tranſparency. But carbonate of ammonia inſtantly cauſed a white precipitate, which was not rediſſolved by an exceſs of the precipitant; upon ſome of this ſubſided precipitate a concentrated ſolution of potaſh was poured and ſhaken with it, but it was not

 ſenſibly

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fenfibly diminifhed. But if after edulcoration it be diffolved in nitric acid, and potafh be added, no precipitate is produced.

Carbonate of potafh caufes a white precipitate when dropped into the aqueous folution of the fcaly fublimate.

The fupernatant fluid was poured off and gradually evaporated, but it became repeatedly turbid, nor could I by means either of the filter or alcohol prevent a recurrence of the fame effect. Nearly the fame refult takes place when carbonate of ammonia is ufed as the precipitant.

Some of the white fcales were moiftened with fulphuric acid. No vapour arofe.

Some of the precipitate obtained by means of carbonate of potafh from the watery folution of this fubftance, was, after fufficient edulcoration, diffolved in fulphuric acid; the folution, on due evaporation, produced permanent cryftals, fome of which refembled alum, but others feemed to differ from it in external character. Ammonia decompofed the folution of them in water, and a few drops of liquid potafh diffolved the precipitated earth. The quantity was too fmall for further experiment.

If diftilled water be poured into the retort and boiled in it, fo as to diffolve what adheres to the neck and cavity of it, a further folution is effected, but differing in fome meafure from the folution of the fublimate collected from the neck of the veffel. This latter folution is found to contain lead. If nitric or muriatic acid be poured into the retort, fo as to diffolve what *ftill* remains adhering to it, the prefence of lead becomes more evident. Whence does this metal arife? I have reafon to believe that it arifes from the glafs retort, which is corroded by the acid of the foffil extricated by heat. But what acid is it? It does not feem to be either the phofphoric or fluoric acids, the latter of which became the firft object of my fufpicion.

The opinion which Mr. Davy fuggefted to me feems more probable, that it is of vegetable origin. Oxalic acid, on the authority of Bergman, may be volatilized; yet fome of its properties are very extraordinary and do not accord with this idea.

I decompofed the watery folution of the fcales by nitrate of lead, and after a fufficient edulcoration of the fubfided precipitate

tate, I dropped upon it fome fulphuric acid. No fumes were perceptible. The fulphate of lead was feparated by the filter, and the clear fluid, which paffed through it, was gradually evaporated; fmall cryftallizations were formed, the figure of which I could not afcertain; fome of them were expofed to the flame of the blowpipe in a gold fpoon; they did not burn to coal, nor give out any empyreumatic fmell nor fufe, but they affumed an earthy appearance *. Defcription and analyfis of a mineral from Cornwall.

Uran-glimmer.

I fhall add a few defultory remarks upon the yellow and green cryftals, which frequently accompany the foffil.

I confidered them to be the two fpecies of uran-glimmer which had been examined by the celebrated Klaproth.

The yellow cubic cryftals are light. Their fpecific gravity, taken at temperature 45° Fahrenheit, was 2,19.

Expofed to the flame of the blowpipe on charcoal, they decrepitate violently. A piece of this fubftance is taken up by phofphate of ammonia and foda, without effervefcence, and communicates a light emerald-green colour to the fufed globule.

By expofure to a red heat, this fubftance lofes nearly a third part of its weight. It then becomes of a braffy colour.

It is foluble in the nitric and muriatic acids; but I could procure no cryftallized falt from the folution of either of them.

By evaporation to drynefs, and rediffolving the mafs, fome filica is feparated.

A

(1.) A certain quantity of the yellow cryftals were diffolved in nitric acid. Muriatic and fulphuric acids fucceffively dropped into the folution produced no fenfible change. The contents of the folution were precipitated by ammonia,

* I fubjected fome of the Barnftaple mineral, with which Mr. Rafhleigh kindly furnifhed me out of his cabinet, to experiment, with a view of afcertaining whether it would produce the fame volatilized faline cruft, as the ftenna gwyn foffil, and I found that it did.

in

Defcription and analyfis of a mineral from Cornwall.

in white clots, mixed with fome of a yellowifh hue. Ammonia, added in excefs, betrayed no fign of the prefence of copper.

(2.) The ammonia, on evaporation, was found to have held a portion of the mineral in folution. A frefh portion of ammonia diffolved more, but in a lefs quantity, at each fucceeding affufion of it.

(3.) The precipitate, which had refifted the ammonia, was boiled in a filver crucible, with a folution of potafh in alcohol, diluted with diftilled water, and a confiderable portion of the fubftance was diffolved by it: the potafh and the ammonia had diffolved rather more than half of the fixed ingredients of it.

(4.) The edulcorated refiduum, which was of a dirty yellow colour, was transferred to a crucible of platina, and moiftened with fulphuric acid, which was abftracted from it, in the fand-bath. The brownifh-gray mafs was elixated with diftilled water, which diffolved nearly the whole of it. The refiduum confifted of a white heavy powder, which, tried in different ways, was found to be *fulphate of lead.*

(5.) The folution effected by fulphuric acid was greenifh. On evaporation, a falt was produced, of uncommon brilliancy, refembling fcales of mica, or filver leaf. Thefe diminifhed in quantity at every frefh folution and evaporation, and at laft they could not be reproduced; but a confufed cryftallized mafs remained. How far the platina crucible may have contributed to this phenomenon I cannot afcertain.

(6.) The folution of the faline mafs was precipitated by potafh, of a dark brown colour. The potafh held nothing in folution. I rediffolved the precipitate in nitric acid, and precipitated the folution by ammonia, of a bright yellow colour, peculiar to the *oxide of uranium*, with which it agreed in other properties.

(7.) What was diffolved by ammonia (2.) amounted to nearly $\frac{1}{8}$ part of the fixed ingredients. It was white, inclining to afh-colour. It tinged phofphate of foda and ammonia of a light green. It was foluble in fulphuric acid, except a few gelatinous flakes. The folution was greenifh; gradually evaporated, it fhot into a number of minute ftellated cryftallizations, which were circular, and confifted of rays diverging from a centre. They were, in general, colourlefs: a few of them

them were tinged of a smoke-colour. They soon became deliquescent. Upon evaporation, the same crystallizations were produced. After a time, some detached, regular, and permanent crystals were formed, which were colourless. Their figure I could not accurately ascertain. They were exposed to a red heat in a platina crucible. No ammoniacal vapour was perceptible. The crystals melted into opaque globules: some of these were transferred to a small glass, and distilled water was poured upon them. No solution took place apparently: on shaking the glass, the globules fell to pieces into gelatinous flakes, which were white. Some of the supernatant fluid was tried with muriate of barytes, which produced a cloud. But neither ammonia nor prussiate of potash caused any change in it. It is soluble also in nitric acid: the solution formed a confused crystallized mass, which soon became deliquescent. Zinc, immersed in it, caused the separation of white gelatinous flakes. Iron caused no change. Ammonia and potash threw down white precipitates, a portion of which were redissolved. The carbonates of soda, potash, and ammonia produced white precipitates. Prussiate of potash threw down the contents of the solution in distinct flakes, of the colour of mahogany; and the solution of galls in alcohol caused a light yellow powder to subside. It is soluble also in muriatic acid; the solution is a very dilute green. It requires an excess of acid to hold the substance in solution: which, after a time, deposits crystalline grains of a yellowish colour, which require a large quantity of water to dissolve them.

Description and analysis of a mineral from Cornwall.

Acetic acid does not dissolve this powder.

(8.) What was dissolved by potash (3.) was of an isabella colour; it was tried with nitric, muriatic, and sulphuric acids, neither of which could dissolve the whole of it. What resisted the two former acids was found to be silica. That which remained undissolved by the latter, was silica and sulphate of lead. Evaporation of the latter solution, betrayed also the presence of lime, in the state of sulphate. The nitric and muriatic solutions, on evaporation, deposited nitrate and muriate of lead; and sulphuric acid dropped into them produced a small quantity of sulphate of lime.

The nitrate and muriate of lead were decomposed by sulphuric acid, and the lead reduced on charcoal.

Ammonia

Defcription and anslyfis of a mineral from Coruwall.

Ammonia precipitated what remained in thefe folutions, and rediffolved a part of the precipitates, which agreed in properties with that fubftance before mentioned (2.); the remainder was of a brighter yellow. But I could not bring the folution of it in nitric acid to cryftallize.

B.

(1.) Some of the yellow cryftals, which had not the flighteft appearance of being contaminated with extraneous matter, were diffolved in fulphuric acid. *Silica was feparated;* and the prefence of *lime* and *lead* proved by the appearance of their refpective *fulphates.*

(2.) If fulphate of ammonia is dropped into a folution of this mineral in nitric or muriatic acids, no change takes place, *immediately.* But on evaporation, a yellowifh cruft is depofited, which is infoluble in water. A folution of carbonate of foda in water, boiled on it, becomes yellowifh-brown, and the greater part of it is diffolved. The refiduum, which is white, is reduced on charcoal to a globule of lead. What the carbonate of foda had diffolved was found to be *oxide of uranium.* Sulphuric acid *alone,* does not produce this depofited cruft.

(3.) Some perfectly pure cryftals were diffolved in muriatic acid. Some filica was feparated. A few drops of fulphuric acid were dropped into the folution, which produced no immediate change: on evaporation a white powder feparated, which confifted in part of fulphate of lime. The remainder, expofed to the flame of the blowpipe, was reduced to globules of lead.

The folution was decompofed by ammonia, which rediffolved a part of the precipitate; and, after edulcoration, the precipitate was diffolved by nitric acid, and precipitated again by ammonia, which held a lefs quantity in folution. The edulcorated precipitate was now boiled with a folution of carbonate of foda, which diffolved a large portion of it. The folution was yellowifh-brown, and contained oxide of uranium. What was undiffolved by the carbonate of foda was diffolved in fulphuric acid, and feemed to be the fame fubftance as that which the ammonia held in folution, A. (2.)

The

The fcarcity of this beautiful mineral has precluded me from operating on fuch a fufficient quantity, as a regular and rigid analyfis required.

Defcription and analyfis of a mineral from Cornwall.

The fubftance, which is held in folution by ammonia, has fome peculiar properties that feem to diftinguifh it from uranium. And if this mineral be the uran-glimmer, I have certainly detected the oxide of lead, lime, and filica in it, which have not hitherto been confidered as ingredients of that foffil. The green cryftals differ in no refpect from the yellow, except in containing a little of the oxide of copper.

XVII.

Examination of different Methods of feparating Nickel from Cobalt. By M. C. F. Bucholz.*

THE want of nickel and cobalt in a ftate of purity induced M. Bucholz, to make experiments himfelf on the means of procuring them, and to repeat thofe of others.

M. Bucholz repeats Hermftadt's method.

A. The able chemift Hermftadt propofed to feparate oxide of cobalt and oxide of nickel, by diffolving the nitrate or fulphate of cobalt, impregnated with nickel, in ammonia; and expofing the folution to a fingle evaporation. This M. Bucholz tried in the following manner, for the reverfed purpofe.

One oz. cobalt ore diffolved in nitric acid. Depofits 3 drams of arfenic. The folution filtered and diluted depofits a little bifmuth oxide. Cauftic ammonia added. The undiffolved refidue is arfeniate of cobalt, with oxides of bifmuth and iron.

1. An ounce of cobalt ore *(cobalt fpeife)* was diffolved with heat in four ounces of nitric acid of the fpecific gravity 1,220. and mixed with an equal quantity of water; which produced a refidue of three drams of oxide of arfenic, in the form of fmall cryftals. When the folution mixed with half the quantity of water, coloured of a dull green, had been filtered and diluted with a great quantity of water, it depofited a little of the oxide of bifmuth. Cauftic ammonia was then mized with it to excefs, until no farther apparent folution took place of the precipitate obtained. That which was not diffolved, of a dull reddifh white, was a compofition of arfeniate of cobalt with a little of the oxide of bifmuth, and the oxide of iron.

* Bucholz, &c. Journal of Chem. III. p. 2.

The

By evaporation oxides of cobalt and nickel are precipitated.

The solution being filtered, appeared of a beautiful blue, it was then evaporated at a gentle heat, by which about two drams of a bright green precipitate were obtained; which proved to be oxide of nickel, united to oxide of cobalt. The filtered liquor being then afterwards evaporated at the heat of a stove, deposited still an oxide of the same quality.

The saline mass obtained, re-dissolved, filtered, and boiled with caustic potash produces pure oxide of nickel.

The saline mass of ammoniacal nitrate of nickel, of a deep green colour, which had been obtained by the evaporation, was re-dissolved, filtered, and kept in ebullition with an excess of caustic potash, until the evaporation of the ammonia was compleated, by means of which a dram and half of oxide of nickel was separated, which did not appear to contain any more oxide of cobalt.

Sulphuric acid tried.

2. As the separation was not effected very well nor with much facility by the former method, the effect of sulphuric acid was tried. For this purpose, an equal quantity of water was poured on the oxide obtained as before, and sulphuric acid added till all was dissolved by the aid of heat. It then evidently gave out an odour similar to that of oximuriatic acid, although there was not any muriatic acid used. A like phenomenon, on a similar occasion, was before observed by the author (which is mentioned in the first section, page 18, of *Deitrage zur erweiterung*, for 1799.) The solution was then treated with ammonia as before, until the whole was almost dissolved. The residue, which was oxide of cobalt with a little oxide of nickel, had the colour of verdigris. When the solution was evaporated at a gradual fire, and separated by filtration from the precipitate, of which the greatest part was oxide of cobalt, it was submitted to spontaneous evaporation: It then crystalized without any farther separation, partly into prismatic crystals in groups, and of a green colour, and partly into crusts united together, and blue at the edges; the essay of the oxides procured by potash from the solution of the crystals, as well as from the mother water, shewed that they contained cobalt almost in equal proportions.

Gives an odour of oximuriatic acid.

Treated with ammonia deposits oxide of cobalt.

The solution crystalised.

The crystals contain cobalt and nickel.

The last experiment repeated on a larger scale.

3. Mr. Bucholz repeated the former experiments on a larger scale, in hope to obtain a better crystallization, and operated on eight ounces of cobalt ore, from which the first crystals, of a blueish green, obtained by a process similar to that last recited, and which weighed about five ounces, were again dissolved in 32 ounces of boiling water: This solution was evaporated

porated till a pellicle was formed, and, after being filtered, was left near a ftove, that it might cool flowly and cryftallize. At the end of 48 hours, the greateft part of the falt was cryftallized in beautiful tetrahedral rhomboidal pyramids, fhort, and of a yellow green, of which the lateral faces formed angles of 115 and of 65 degrees, often with one extremity truncated, and always with an angle of 132 degrees towards its terminating face. This refult proves that this falt forms more readily into regular cryftals by cooling than by flow evaporation. All the cryftals were then collected, wafhed with water, and again diffolved, and the nickel feparated by boiling the folution with potafh till the ammonia was difengaged.

Produces fine cryftals in tetrahedral rhomboidal pyramids.

The cryftals diffolved and the nickel feparated as before.

4. As well to free this oxide from carbonic acid as to judge if it had been purified from cobalt, it was diffolved in nitric acid and treated with pure ammonia in the fame manner as has been defcribed. The liquor of a fine blue colour, (and from which a refidue of five grains, which feemed to be an oxide of cobalt, had been feparated by filtration), was evaporated to drynefs. After another folution then made, it depofited an oxide of a beautiful bright green, which, after being wafhed and dried, weighed half an ounce. The liquor, which paffed the filter, was analyfed by pure carbonate of potafh at the heat of boiling water, which then produced 170 grains of oxide of nickel, of a pale green, united to carbonic acid; a little of it was diffolved in muriatic acid, and fome of the folution fpread upon paper. On heating it afterwards, the tint became yellow, and inclined but very little to a green. But the oxide of nickel, which feparated fpontaneoufly during the evaporation, was diffolved in difengaging much oximuriatic acid; fpread on paper, it exhibited the colour, when heated, of a fympathetic ink of cobalt highly faturated; from whence it follows that it was more rich in cobalt than that procured from the precipitation.

The oxide obtained diffolved in nitric acid and treated with ammonia, evaporated and re-diffolved depofits a green oxide.

The filtered liquor yields by potafh 170 grs. oxide of nickel.

Which contains lefs cobalt than the fpontaneous precipitate.

The oxides collected in thofe two ways, diffolved in nitric and fulphuric acids, after becoming grey, (which the author fuppofed to be occafioned by the nickel diffolving firft, and at leaft the greateft part of the cobalt remaining to the laft, but which opinion was not confirmed by other experiments made on this fubject.) Thefe oxides made lightly red in the fire, changed their colour to a dark grey, and then, as well as on

The oxides diffolve in nitric and fulphuric acids.

Give out nitrous acid by fire, and by fulphuric acid.

on the addition of ſulphuric acid, a diſengagement of nitrous acid took place from the reſidue obtained by evaporation, which was alſo cauſed by the addition of an alkaline lixivium: With ammonia the ſame effects were produced which have been before mentioned.

Sulphites and nitrates of ammoniacal nickel always contain cobalt.

The reſults of the foregoing experiments are:—The ſulphates and nitrates of ammoniacal nickel ſeparated from cobalt ore, retain always ſome cobalt in their compoſition, and it is impoſſible from the method of Hermſtadt modified in the preceding manner, to obtain an oxide of nickel without a mixture of cobalt.

The oxide of nickel remaining in the ſalt after evaporation contains very little cobalt.

b. By partially decompoſing the ammoniacal nitrate of cobalt by evaporation, an oxide of nickel is obtained, very rich in cobalt, which contains nitric acid; and the oxide of nickel which remains undecompoſed in this ſalt, retains a very ſmall quantity of cobalt.

Dr. Schnaubert's method of obtaining pure oxide of nickel.

B. Doctor Schnaubert has publiſhed (in Tromſdorf's Journal of Pharmacy, vol. II. p. 66) a method of obtaining the oxide of nickel pure: Which conſiſts in diſſolving the metal of nickel mixed with cobalt, or its oxide ſeparated from other ſubſtances, in nitric acid, in precipitating it by the carbonate of potaſh, and in heating it to a white heat, after waſhing and drying it. In this manner he always procured a yellow oxide, on which he cauſed very ſtrong ſulphuric acid to boil; which gave him a ſolution of oxide of nickel of a graſs green, while the oxide of cobalt appeared in the form of a yellow reſidue. He proves the purity of the ſulphate of nickel prepared in this manner, by the property which ammonia has of precipitating it of a bright green, and when added to exceſs, of re-diſſolving it with a beautiful deep blue colour; but this argument appears inſufficient to thoſe who know that oxide of nickel, although mixed with many hundredth parts of cobalt, does not, however, experience any perceptible change in the colour of its precipitates, nor in its ammoniacal ſolutions.

His teſt of its purity defective.

He has not mentioned his proof that the other oxide obtained was cobalt, —nor the degree of heat to be uſed, —nor the ſtrength of the ſulphuric acid employed.

Beſides the omiſſion of indicating the means by which he was convinced that the oxide, which was the reſidue of the ſulphuric acid ſolution, was really an oxide of cobalt, with the vague precept of heating the oxide acquired, without the leaſt direction relative to the degree of the fire, and the uncertainty which he leaves of the degree of ſtrength of the ſulphuric

ſulphuric acid which he uſed, altogether throw doubts on the exactneſs of the proceſs indicated, which the following experiments may elucidate.

Experiments on Dr. Schnaubert's proceſs.

1. A portion of the carbonic oxide of nickel, A 4, was expoſed during an hour in a ſtrong fire to a red heat approaching white heat. The oxide while hot was of a browniſh yellow; after cooling it aſſumed a grey colour inclining to yellow, but not yellow. The oxide obtained by the evaporation A 4, having been treated in the ſame manner was ſtill a little more grey than the preceding. The carbonic oxide of nickel was placed again for half an hour in a white heat; while hot it was yellow inclining to brown, but when cool, it was grey inclining to browniſh yellow.

2. Thirty grains of this oxide made red (hot), were put for ſome hours to digeſt, with ninety grains of pure ſulphuric acid of the ſpecific gravity 1,860. Being then heated, the maſs immediately ſwelled up with an exploſive noiſe, and exhibited a yellow ſubſtance inclining to a green; by means of ebullition with half an ounce of water it was diſſolved, except about a grain of a yellowiſh-grey powder, which proved to be an oxide of nickel mixed with cobalt and a little dirt. Thirty-five grains of oxide of nickel, (obtained by heating briſkly to redneſs 60 grains of ammonical nitrate of nickel prepared by evaporation), afforded the ſame reſult, and the ſame phenomena, on being treated in the ſame manner: The ſame oxide being heated for half an hour to whiteneſs, uſing the bellows at the ſame time, did not afford a yellow maſs, but one of a yellowiſh grey inclining to a green, which had the ſame effect with ſulphuric acid that has been already related.

The experiment repeated with diluted ſulphuric acid.

3. The experiment was again repeated with diluted ſulphuric acid; 160 grains of ammonical oxide of nickel, which had been precipitated from many ſolutions were expoſed for half an hour to the moſt violent white heat, under the operation of the bellows, after which they weighed 75 grains. This ſubſtance was of a greeniſh yellow here and there, and of a blueiſh grey where it touched the crucible; being broken it produced a black grey powder. It was mixed with a dram of ſulphuric acid diluted with five drams of water; at that inſtant there was a rapid diſengagement of gas, and on heating the mixture it evidently gave out hydrogen gas. After a ſuffi-

cient

cient ebullition, water was added, and the solution decanted off clear. The residue was treated again with weak sulphuric acid, and then gave a residue of ten grains which was by no means oxide of cobalt, but oxide of nickel mixed with cobalt, as its solutions in the acids and in ammonia proved. The two preceeding solutions were each separately analysed by pure potash, and the precipitate was besides heated with an excess of potash, and then washed and dried. At the proof each of the precipitates afforded cobalt, which was always most pure in that of the first solution; for the solution in muriatic acid, laid on paper, and heated, inclined perceptibly to a yellow, while the precipitate of the second solution produced a stain of a clear and pure green. It is strange that the first solution afforded more oxi-muriatic acid than the second.

The residue oxide of nickel mixed with oxide of cobalt.

Each of the precipitates of the foregoing solutions afford cobalt.

These experiments, and others made by the author, but not related, prove,

A. That the oxide of nickel heated either slightly or violently does not assume a yellow colour; and if this colour was observed by M. Schnaubert, it must have been caused by some substances which entered into the composition of the oxide, or perhaps by the mixture of a little arsenic.

The experiments prove that nickel oxide does not become yellow; cause of the mistake of Dr. Schnaubert on this point.

B. That it is impossible by M. Schnaubert's method, to obtain an oxide of nickel exempt from cobalt; since it does not even effect a separation of the two oxides so far as to be perceptible to the eye.

It is not possible to obtain pure nickel in his way.

C. M. Bucholz hints here at several experiments he made with a view to find an acid which would form an insoluble salt with one of the oxides, and one easy of solution with the other, but which, as they did not succeed, he does not mention; and as the method proposed by Mr. Lehman (in the *Cadmiologia,* part II, page 110) of fusing fifteen or twenty times, to a commencement of vitrification, a mixture of nickel and cobalt, in order to scorify all the cobalt, would be too troublesome and expensive, as would that also indicated by Bergman (*Opuscul, Physic, et chem,* Vol. II. p. 246—249) of repeating the fusion three or four times with from 8 to 12 times the quantity of pure nitre. The process indicated A 4, (consisting of a partial decomposition of the ammonical nitrate of nickel), alone remained to be repeated. For this purpose oxide

M. Lehman's method too troublesome and expensive, and M. Bergman's also,

the process A 4 repeated.

oxide of nickel, (which was ſeparated from the triple ſalt, not diſſolved at the firſt evaporation, by carbonate of potaſh, was treated repeatedly, (in ſuch a manner) that after diſſolving it in nitric acid, recourſe, was had to the uſe of ammonia and evaporation as before deſcribed. In this method was obtained, entirely free from cobalt, an oxide ſeparated by potaſh from the triple ſalt, which had been rediſſolved after evaporation, and which oxide had the properties mentioned in the memoir printed in the ſecond volume of the Annales de Chimie.

The oxide which was ſeparated by evaporation from the ammoniacal nitrate of nickel, was in the laſt operation entirely freed from cobalt; it only contained a ſtill, as has been obſerved, a little nitric acid. The oxide of nickel, which, after having been laid bare by evaporation, ſtill contains cobalt, may naturally undergo the ſame operation over again.

The oxide entirely freed from cobalt in the laſt operation.

This method may be made uſe of untill one more expeditious is diſcovered by farther experiments, ſince it does not occaſion any conſiderable expence, for by potaſh, the evaporation of the ammonical nitrate of nickel may be effected in a retort, and alſo the ſubſequent decompoſition of the triple ſalt, and thus the ammonia may be ſeparated for other uſes; in like manner, in works on a great ſcale, a part of the nitre may be recovered from the laſt operation, by the evaporation of the water in which the ſubſtance has been waſhed.

This method recommended for the preſent, the ammonia may be ſaved during the proceſs.

and the nitre recovered.

XVIII

Sugar prepared from Beets. By M. Hermbstadt.*

THE method of M. Achard for extracting ſugar from beets, was ſo expenſive, that it was of no advantage for common uſe. M. Hermbſtadt, of Berlin, has practiſed another method, which is eaſily performed, and affords hopes of rendering this ſugar cheaper than that from the ſugar-cane; which is as follows:

After having bruiſed the beets in a mortar, M. Hermbſtadt ſubmits them to the operation of a preſs, to extract the juice from them; which is then placed in veſſels, and clarified with lime in the ſame manner as cane-ſugar.

The expreſſed juice of the beet is clarified by lime,

* Sonnini's Journal, Tom. II. p. 331.

When

When this operation is finifhed, the liquor is evaporated to the confiftence of a fyrup: It is then left to cool, and a courfe fugar is obtained, of a dark-brown colour: At the bottom of the veffel a fyrup remains, which may be ufed for domeftic purpofes.

and then evaporated to a fyrup. A coarfe fugar is produced on cooling.

From 100 pounds of the coarfe fugar, eighty pounds of well cryftallized fugar are obtained by the firft refining, which fugar is not at all inferior in quality or whitenefs to that of the cane. The whole operation may be completed in two days.

From 100 lb. of this coarfe fugar, 80 lb. of refined fugar may be obtained.

The particular fpecies of beet which M. Hermbftadt ufed in his experiments, is not mentioned; but it is moft probable that this chemift made ufe of the common field beet, known in Germany by the name of *mangel wortzel*, the culture of which is fpread through many cantons of Germany. This variety, however, contains lefs fugar than all the other fpecies of beets; and, for this reafon, M. Sonnini is of opinion, that if fugar can be obtained from beets with profit and economy, more fuccefs would be obtained by fubmitting to the operations defcribed, the fmall red beet, called in France that of *Caftlenaudery*, which is the fweeteft of all.

It is probable M. Hermbftadt ufed the common field beet, or root of fcarcity.

A better produce might be obtained from the fmall red beet of Caftlenaudery.

XIX.

Method of ftacking Turnips, to preferve them through the Winter. By Mr. John Shirreff, *of Captain Head, near Haddington, N. Britain.**

Rapa folo molli et ære humidulo lætantur.

Preservation of turnips through the winter.

SATISFIED, from obfervation and experience, that turnips are the foundation of the beft hufbandry on almoft all foils and fituations in the arable diftricts of Great Britain; and that this crop fhould always be drawn, except from blowing fands, or light moorifh foil, on both of which it fhould always be in part confumed on the ground with fheep; convinced alfo, that turnips, if poffible, fhould be off all foils, and the land

* Soc. Arts, 1805. The premium of 30 guineas was awarded for this method.

ploughed

ploughed up before the middle of December, at the lateſt, to ſecure the ſucceeding corn crop, and graſſes, or clovers, with either of which every field that carried a turnip crop the preceding ſeaſon, ſhould, in almoſt every caſe, be ſown down; and impreſſed with the many high advantages attending this practice, as ſoon as my pea and bean ſtubbles are ploughed up, and ſown with wheat, my turnips are begun to be drawn, and ſtacked up for uſe during the following winter and ſpring. If the diſtance of the turnip-field from the homeſtead does not exceed a quarter of a mile, two double horſe carts only are employed, and more in proportion to the diſtance of the turnip field, or number of hands you may be able to command to carry on the work. One clever driver is ſufficient for two carts, and two for three carts, &c. one cart being always in the field loading or loaded. On being brought home, the turnips are inſtantly tumbled out at the ſtack; which is done with great facility, from the conſtruction of the carts in this diſtrict, which to convenience and ſtrength likewiſe add lightneſs, to enable horſes to move at a ſmart pace with them when empty. The turnips tumbled out of the cart, are trimmed of their leaves, and cleaned of any earth that may adhere to them, by women, &c. before being put into the ſtack. Old table-knives do very well for the purpoſe, and the leaves ſhould be cut off cloſe to the root; the back of the knife being uſed for removing any pieces of ſoil that may ſtick on the turnip.

Preservation of turnips through the winter.

Women, &c. trim the turnips, and put them into ſtrong coarſe wicker baſkets, to be carried forward by a man, who hands them to another, who lays them into or on the ſtack. The ground on which the turnips are placed ought to be dry bottomed. If that is not the ſort of ſoil where you find it moſt convenient to make your ſtack, a quantity of boulders may be put on, regularly ſpread over the ſpace, to the thickneſs of at leaſt eighteen inches. My corn-rick yard, being dry ground, has been uſed as the place for keeping my turnips in. The ſtacks have been made about ten feet wide, by driving a row of ſtakes into the ground parallel to the wall of the yard, which ſerves inſtead of another row. The wall is only about five feet and a half high, and the ſtakes are driven to the ſame height. The inſide of the wall and ſtakes are lined with compact bunches, or ſheaves of wheat-

Preservation of turnips through the winter.

straw, about ten inches in diameter, placed horizontally on the ground or boulders, and introduced, as wanted, during the operation of stacking. A tire of the largest turnips are placed one above another, on the inside of the bundles of straw, more particularly on the side guarded by the stakes, till the pile reaches the height of five feet from the ground, or from the boulders, if it has been found necessary to spread any over the ground. The inner part of the stack is at the same time gradually made up with turnips put in promiscuously; along which a plank is laid, and occasionally shifted as the pile rises, for the man who builds the stack to stand on without bruising the turnips with his shoes. When the pile of turnips is reared, in the manner described, to the height of above five feet, it is gradually contracted inwards, on both sides, at an angle of about forty-five degrees, like the roof of a barn; the largest turnips being still piled on the outside, till the roof is so far completed. The stack is every day so far finished in height as it is extended in length, and is covered with wheat straw thatch, roped down with twisted bands of oat-straw before evening, to secure the stacked turnips from rain that may fall during the night. The thatch is laid on a foot thick, and secured in the same simple, effectual manner, that corn-ricks are covered in Northumberland, Berwickshire, and the Lothians; with this difference only, that the straw is four times as thick laid on the turnip as on the corn, to exclude cold as well as wet; and that there is a rail of wood stretched, hanging horizontally at the tops of the wall and stakes, to fix the straw ropes to, which secure the thatch on the stack. The end of the stack is every night covered with bundles of wheat-straw, which are removed next day, or when building recommences.

Three men are employed in the field to load and dispatch the carts, occasionally assisting four women who draw the turnips, striking off the top root with a strong heavy knife, leaving the turnips on the tops of the drills as drawn and chopped, with the leaves all in one direction, to be readily laid hold of by the men who lift them up to the cart. The horses pass along in the space between the two rows or drills of the turnips, which may be drawn: and, being at thirty inches apart, and the extremities of the wheels about five feet from each other, it is evident a wheel runs in the middle of of each ſpace between the contiguous drills, without injuring the turnip, whether drawn or not. When the cart is about to turn, after being loaded, the men move the turnips to make room for the horſes, putting them into the cart as part of the load.

Preſervation of turnips through the winter.

Expences of drawing, carting, trimming, ſtacking, covering, &c. a ſtatute acre of good turnip,—at the diſtance of not more than a quarter of a mile from the ſtack.

	£.	s.	d.
Two double-horſe carts, and one man -	0	16	0
Two men loading, drawing, building, &c. -	0	8	4
Seven women drawing and trimming - -	0	4	1
Two girls trimming - - - - -	0	1	0
Four ditto and boys ditto - - - - -	0	1	8
Twiſting ropes, drawing thatch, thatching, waſte of thatch, ſtakes, &c. ſay - - -	0	3	6
	1	14	7

The above is a fair average of the expence of ſecuring ſomewhat more than twelve and a quarter ſtatute acres laſt ſeaſon, which was all I drew; and one field of two acres, one rood, thirty-three perches, was ſo far diſtant as to require three carts, and two drivers. That field, however, was firſt drawn, and the weather being fine and moderate, more work was done in proportion to the length of the day, which was alſo longer. Women and children cannot, indeed, exert themſelves with ſpirit, in raw cold weather. October is perhaps the beſt month to draw in. It is a queſtion with me, whether the average of the acres that are under turnip in the iſland, if the weight exceeds twenty-four tons, does not coſt more, merely for drawing and carting only. When it is conſidered that this operation is performed often in cold, froſty, and ſtormy weather, and that frequently much ſnow may be to be removed before the turnip can be ſeen. If no ſnow has fallen before the froſt ſets in, the turnips muſt be hoed up with inſtruments for the purpoſe. Many are cut, and much left in the ground of the lower part of the root. After all this labour, what is obtained is frequently no better than a lump of ice, environed with earth, frozen ſo firmly to its ſurface, that nothing but thawing in

 cold

Preservation of turnips through the winter.

cold water can ever render it fit to be touched by the mouth of any animal whatever.

Admitting, however, the expence of drawing and carting to be the ſame, all that can be ſtated as extraordinary expence is the coſt of trimming and ſacking, which amounts to 11s. 3d. an acre. On the other hand, we have the advantage of having fine freſh clean turnips, always ſecure and at command, to carry on feeding and breeding ſtock; at the ſame time that all loſs by rotting in the ſpring months is prevented, which is frequently thirty and even fifty per cent. on all the crop that remains in the field, after the firſt of February. Above all, the practice of drawing and ſtacking before winter, by admitting of early ploughing to mellow the ſoil, ſecures a valuable corn, and ſucceeding clover crops. When all theſe circumſtances are maturely weighed, the expence of eleven ſhillings and three-pence will, to every enlightened agriculturiſt, appear but trifling to obtain ſuch very valuable advantages. The writer of this little eſſay has had the ſatisfaction of having excellent crops after his turnips, this ſeaſon; while almoſt every other crop in the neighbourhood was indifferent; and ſome on rich dry loams, high rented, by being ſown in the months of April and May, on the ſpring ploughing, after turnips eaten off with ſheep, were ſo miſerable, as evidently to pay *nothing* after expences of labour, ſeed, and reaping. The young clovers too, ſown with theſe crops, have almoſt entirely periſhed from want of moiſture. The loſs of the crop and clover ſeed is not all: the ſyſtem ſuffers a derangement, the conſequences of which none but practical men can calculate.

One thing remains to be noticed, which is, that twenty-ſix young cattle, cows, and yearling calves, were kept nearly three weeks on the turnip trimmings, with oat-ſtraw along with them, to their improvement; and that many more might have been kept, had they been provided in time. A quantity of good manure was made: and, eſtimating all advantages ariſing from the conſumption of the leaves in this way, at no more than 3d. a head per night, for the keep of each beaſt, the amount will exceed the expence of trimming and ſtacking the whole crop of turnips on twelve acres and a quarter.—The leaves that remain on turnips after Chriſtmas, are either unfit to be eaten, or waſted by the froſts.

T. SHIRREFF.

XX.

Account of ſome Specimens of Baſaltes from the northern Coaſt of Antrim. By the Rev. Dr. WILLIAM RICHARDSON.*

THE Reverend Dr. William Richardſon, late F. T. C. D. having ſent to Dr. Hope a collection of ſpecimens from the northern coaſt of Antrim, with a catalogue and obſervations, the ſpecimens were exhibited, and the obſervations were read in the Royal Society, March 1803. Remarks on the baſaltes of the coaſt of Antrim.

Siliceous Baſalt.

Dr. Richardſon diſcovered the foſſil to which he gives this name, in the peninſula of Portruſh, four or five years ago. It abounds alſo in the Skerry iſlands, a reef of rocky iſlots extending from the northern point of Portruſh-head for about a mile eaſtward. A ſmall part of every one of thoſe iſlots is formed of this ſtone, while the remainder conſiſts of coarſe baſalt, ſimilar in all reſpects to that on the eaſt ſide of the above-mentioned peninſula. It is met with in one or two other places.

This ſtone is arranged in ſtrata, from ten to twenty inches thick, all ſteadily parallel to one another, and every ſtratum, as far as can be obſerved, preſerving an uniform thickneſs through its whole extent. When theſe ſtrata are quarried into, they appear to be conſtructed of large priſms, generally pentagonal, which when broken divide into ſmaller priſms. This internal priſmatic conſtruction frequently gives an irregular or ſhivery appearance to the fracture, which however is often conchoidal, and the grain as uniform as in the Giant's Cauſeway baſaltes.

The beds of this foſſil are remarkable for containing marine exuviæ in great abundance, particularly impreſſions of *cornua ammonis*, The flat ſhells and impreſſions contained in theſe ſtones, are ſteadily parallel to each other, and perpendicular to the axis of the priſms. It muſt be obſerved, that the priſmatic conſtruction is never interrupted by the ſhells diſperſed through it; the planes which ſeparate the priſms paſſing equally through the ſhells and the ſtone itſelf.

* Edinburgh Tranſ. Vol. V.

The

Remarks on the basaltes of the coast of Antrim.

The grain of this stone passes by insensible shades from a high degree of fineness, until it become undistinguishable from that of the common columnar basaltes.

The name of Siliceous Basalt, which Dr. Richardson employs, was first given to this fossil by Mr. Pictet of Geneva, when he visited Portrush, in a tour through Ireland two years ago. He considered it as a variety of basalt, containing a greater proportion of *silica* than usual.

The strata of siliceous basalt, both at Portrush and the Skerry islands, generally alternate with strata of equal thickness of a coarse-grained basalt of a grey colour. The materials of the strata grow into each other, so as to form one solid mass, from which it is easy to quarry pieces in the confine of the two strata, with a part of each adhering; but the coarse basalt, as it approaches very near to the fine, always abates somewhat of its coarseness; yet the line of demarcation is left completely distinct.

(The conclusion in our next.)

SCIENTIFIC NEWS.

Almanack printed at Constantinople.

Almanack printed at Constantinople.

FOR the first time an almanack has been printed at Constantinople, under the direction of Abdorahman. The printing-office was established in 1716, by *Said* (who had heen at Paris with his father, the ambassador), and by Ibrahim, an Hungarian: Achmet the Third patronized them, and they printed many books; but an almanack was never before printed.

Observatory at Bavaria.

Bavarian observatory.

The Elector of Bavaria, a few months before the arrival of the French armies, caused an observatory to be erected in the neighbourhood of Munich. The situation chosen for its construction, takes in an extensive horizon. Professor Seyfer, a celebrated astronomer of Gottingen, was nominated director of this establishment.

Establishments

Eſtabliſhments for Natural Philoſophy in the Ukraine.

The rich land-owners in the Ukraine and Volhinia, have contributed largely for the eſtabliſhment of Lyceums for teaching natural philoſophy, at Krzeminico, and at Winnica. The library and philoſophical apparatus of the King of Poland, have been purchaſed for this purpoſe. M. Sniadecki has received a ſum equal to 500*l.* to purchaſe teleſcopes and clocks; and no expence is to be ſpared in properly furniſhing the obſervatories with inſtruments. Eſtabliſhment for natural philoſophy in the Ukraine.

Obſervatory at Moſkow.

M. Goldbach, an able aſtronomer of Leipſic, has been nominated profeſſor of the univerſity of Moſkow, with a ſalary equal to 250*l.* He is to have the direction of the conſtruction of a new obſervatory, to furniſh it with inſtruments, to make regular obſervations, and to inſtruct ſome young men in practical aſtronomy who have been previouſly inſtructed in the preparatory ſciences, and to give a courſe of lectures in theoretical aſtronomy in one of the halls of the Univerſity. Obſervatory at Moſkow.

They poſſeſs many of Cary's teleſcopes, of different powers; an excellent aſtronomical clock; a chronometer, made by Arnold; a portable circle, of one foot diameter; and, it was reported, had ordered one of three feet diameter from the ſucceſſor of Mr. Ramſden: Thus M. Goldbach will be provided with every inſtrument neceſſary and uſeful to aſtronomy, at the obſervatory of Moſkow.

M. Goldbach has taken the opportunity of his journey, to determine the poſition of ſome towns; among others that of Riga, $1^h\ 27'.0$, and $56^\circ\ 57'.8$.

At the ſame time that M. Goldbach is engaged with the aſtronomical eſtabliſhment at Moſkow, MM. Schubert and Wiſniewſki are employed at the obſervatory of Peterſburgh; and there is reaſon to expect a ſeries of obſervations from that part of the world.

Solar Tables.

A ſet of tables of the ſun, compoſed by M. Delambre, have been printed at Paris, in which there are many new equations, Solar Tables.

tions, and of which all the elementary parts have been verified by new obſervations. A ſet of tables of the moon's motion are alſo to be printed, and when they are completed, thoſe of the planets will follow.

Bequeſt of Erneſt the Second relative to his Obſervatory.

Erneſt the Second's bequeſt to his obſervatory.

Erneſt the Second, late Duke of Saxe-Gotha, was remarkably attached to aſtronomical ſtudies. He made obſervations and calculations himſelf, aſſiſted in compoſing books on the ſubject, and furniſhed the funds for their publication. He enabled M. Zach to meaſure a degree of the meridian in Germany, and defrayed the expences from his private purſe; ſo that he united to the merit of a connoiſſeur in the ſcience, that of an author, a patron, a man of ſcience, and of a generous prince.

He left in his will a ſum equal to about 1330*l.* to form a fund for the maintenance of the obſervatory of Seeberg, near Gotha, which was built out of his own private eſtate; and ordered his ſucceſſor to erect no other monument to his fame, but the careful ſupport of this eſtabliſhment.

Baron de Zach, who has given a copy of the will in his Journal, adds, "That he can aſſure the lovers of ſcience, that the will of the father will not only be fulfilled, but ſurpaſſed by his ſucceſſor, the preſent Duke Emilius Leopold Auguſtus, who has already ſhewn the moſt marked proofs of his attachment to the ſciences.

"In a codicil to the will the Duke repeated, 'I forbid expreſsly the elevation of any monument to my memory, or even an epitaph, or any monument at or near my tomb.'"

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1806.

ARTICLE I.

Letter from T. YOUNG, *M. D. F. R. S. &c. claiming the Lamp defcribed in our laft Number, and demanding an Explanation from the anonymous Communicator.*

To Mr. NICHOLSON.

SIR,

I WAS much furprized on feeing, a few days ago, the figure of a lamp contained in the fourth plate of your Number for February laft. I truft you will be convinced, upon infpection of the figure which I now fend you, and which was engraved before Chriftmas, that your correfpondent A. F. muft have copied his lamp from that which is here reprefented; and I am fure you will think I have a right to demand a public explanation of the manner in which he procured a fight of a plate not yet publifhed, and of the motives which induced him to make fo unjuftifiable a ufe of it. I fhall referve the complete explanation of this lamp for the work to which the plate belongs, which has been long in the prefs, and which will foon be ready for publication; I fhall only obferve that Concerning the invention of a Lamp.

 it

it is in a great meaſure free from the inconvenience which A. F. has attributed to it, (p. 168) and that the " ſmall ſhaded circle" is not a " perforation," but a weight attached to the counterpoiſe,

I am, Sir,

Your very obedient Servant,

THOMAS YOUNG.

Welbec Street,
March 15, 1806.

II.

On the Tendency of Elaſtic Fluids to Diffuſion through each other. By JOHN DALTON *.

Mixed elaſtic fluids of different denſities do not ſeparate;

IN an early period of pneumatic chemiſtry it was diſcovered that elaſtic fluids of different ſpecific gravities being once diffuſed through each other, do not of themſelves ſeparate, by long ſtanding, in ſuch manner as that the heavieſt is found in the loweſt place; but on the contrary, remain in a ſtate of uniform and equal diffuſion.

but will they mix without agitation.

Dr. Prieſtley has given us a ſection on this ſubject (vid. Experiments and Obſervations, &c. abridged. Vol. II. p. 441) in which he has proved the fact above-mentioned in a ſatisfactory manner; and every one's experience ſince, as far as I know, has coincided with his concluſions. He has not offered any conjecture concerning the cauſe of this deviation from the law obſerved by inelaſtic fluids; but he ſuggeſts that

Dr. Prieſtley thinks not.

" if two kinds of air of very different ſpecific gravities, were put into the ſame veſſel, with very great care, without the leaſt agitation that might mix or blend them together, they might continue ſeparate, as with the ſame care *wine* and *water* may be made to do."

The determination of this point, which ſeems at firſt view but a trivial one, is of conſiderable importance; as from it we may obtain a ſtriking trait, either of the agreement or diſagreement of elaſtic and inelaſtic fluids in their mutual action on each other.

* Mancheſter Memoirs, Vol. I. New Series.

It

It is, therefore, the ſubject of the following experiments to aſcertain whether two elaſtic fluids brought into contact, could intermix with each other, independently of agitation. The reſult ſeems to give it in the affirmative beyond a doubt, contrary to the ſuggeſtion of Dr. Prieſtley; and eſtabliſhes this remarkable fact, *that a lighter elaſtic fluid cannot* reſt *upon a heavier*, as is the caſe with liquids; but, they are conſtantly active in diffuſing themſelves through each other till an equilibrium is effected, and that without any regard to their ſpecific gravity, except ſo far as it accelerates or retards the effect, according to circumſtances. Inquiry by experiment which ſhews the contrary.

The only apparatus found neceſſary was a few phials, and tubes with perforated corks; the tube moſtly uſed was one ten inches long, and of $\frac{1}{20}$ inch bore; in ſome caſes a tube of 30 inches in length and $\frac{1}{3}$ inch bore was uſed; the phials held the gaſes that were ſubjects of experiment and the tube formed the connection. In all caſes, the heavier gas was in the *under* phial, and the two were placed in a perpendicular poſition, and ſuffered to remain ſo during the experiment in a ſtate of reſt; thus circumſtanced it is evident that the effect of agitation was ſufficiently guarded againſt; for, a tube almoſt capillary and ten inches long, could not be inſtrumental in progagating an intermixture from a momentary commotion at the commencement of each experiment. Apparatus.

FIRST CLASS.

Carbonic Acid Gas, with Atmoſpheric Air, Hydrogenous, Azotic and Nitrous Gaſes.

1. A pint phial filled with carbonic acid gas, the 30 inch tube and an ounce phial, the tube and ſmall vial being filled with common air, were uſed at firſt. In one hour the ſmall phial was removed, and had acquired no ſenſible quantity of acid gas, as appeared from agitating lime water in it. In three hours it had the acid gas in great plenty, inſtantly making lime water milky. After this it was repeatedly removed in the ſpace of half an hour, and never failed to exhibit ſigns of the acid gas. Things remaining juſt the ſame, the upper phial was filled with the different gaſes mentioned above repeatedly, and in half an hour there was always found acid ſufficient to make the phial $\frac{1}{2}$ filled with lime water quite milky. Carbonic acid gas with lighter gaſes.

milky. There was not any perceptible difference whatever gas was in the upper phial *.

SECOND CLASS.

Hydrogenous Gas with Atmofpheric Air and Oxigenous Gas.

Hydrogen, with atmofpheric air and oxigen.

1. Two fix ounce phials were connected by the tube of a tobacco pipe, three inches long, the upper containing hydrogenous gas, the lower atmofpheric air: after ftanding two hours, the lower phial was examined; the mixed gafes it contained made fix explofions in a fmall phial. The gas in the upper alfo exploded.

2. Two four ounce phials connected with the ten inch fmall tube ftood two days, having common air and hydrogen gas. Upon examination the upper was found to be $\frac{1}{3}$ common air by the teft of nitrous gas. The gas in the under exploded fmartly; that in the upper moderately with a lambent flame.

3. Two one ounce phials were connected by the ten inch tube, containing common air and hydrogenous gas; in three hours and a half the upper was about $\frac{1}{3}$ common air and the under $\frac{2}{3}$; the former exploded faintly; the latter fmartly.

4. Two one ounce phials were connected as above; the under containing gas about $\frac{3}{4}$ oxygenous, the upper hydrogenous: In three hours the latter was $\frac{1}{5}$ oxygenous, and the former about $\frac{1}{2}$; the upper exploded violently, the under, moderately.

5. Two one ounce phials were again connected, the lower having atmofpheric air, the upper hydrogenous gas; they ftood fifteen hours, and were then examined; the upper gave 1.67 with nitrous gas, the under 1.66.—Hence it is evident that an equilibrium had taken place, or the two gafes were uniformly diffufed through each other in both phials.

THIRD CLASS.

Nitrous Gas, with Oxigenous Gas, Atmofpheric Air, Hydrogenous and Azotic Gafes.

Nitrous gas with oxigenous, atmofpheric hydrogenous, and azote.

The refults of the preceding experiments upon gafes that have no known affinity for each other, were conformable to

* The fmall tube of ten inches was then ufed and a phial of common air; in one hour much acid gas had come through, as appeared by lime water.

what

what *à priori*, I had conceived; for, according to my hypothesis, every gas diffuses itself equably through any given space that may be assigned to it, and no other gas being in its way can *prevent*, though it may considerably *retard* this diffusion. But in some of the following experiments, in which the two gases are known to have a chemical affinity for each other, I expected different results from what are found; perhaps without sufficient reason. For, chemical union cannot take place till the particles are brought into contiguity; and the elastic force which sets them in motion appears, from the above experiments, to be a principle diametrically opposite to affinity. That circulation of elastic fluids, therefore, which we have now before us, cannot be *accelerated* by their having a chemical affinity for each other. Another circumstance deserves explanation;—when nitrous and oxygenous gas are in the two phials, the residuary gases after the experiment are nearly as pure as before; because those portions of them that meet in the tube, form nitrous acid vapour, which is absorbed by the moisture in the phials, and therefore does not contaminate either gas.

Nitrous gas with oxygenous, atmospheric hydrogenous, and azote.

1. Two one ounce phials were connected with the small tube, the under containing nitrous gas, the upper atmospheric air; after three hours, the upper phial was taken off when a quantity of air was perceived to enter, as was expected; the air in the upper phial was scarcely distinguishable from what it was at first; that in the under phial was still so much nitrous as to require its own bulk of common air to saturate it.

2. The above experiment was repeated, and the upper phial drawn off when the whole was under water, in order to prevent communication with the atmosphere: about $\frac{1}{8}$ of an ounce of water entered the phials, to compensate the diminution. Remaining air in the upper phial was a very little worse than common air, it being of the standard 1,47 when the former was 1,44. The gas in the under phial was still nitrous and nearly of the same purity as at first; for three parts of it required four of atmospheric air to saturate them.

3. Nitrous gas and one $\frac{2}{3}$ oxygenous were tried in the same way: after four hours, the apparatus was taken down under water. The upper phial was $\frac{2}{3}$ filled with water, and the gas

gas in it was partly driven down the tube into the other phial, by which, and the previous procefs, the nitrous gas was completely faturated and nothing but azotic with a fmall portion of oxigenous were found in the under phial: the remaining gas in the upper phial was ftill $\frac{1}{2}$ oxygenous.

4. Nitrous gas and hydrogenous: in three hours the upper phial was $\frac{1}{4}$ nitrous, and of courfe the under muft have a like part of hydrogen.

5. Nitrous gas and azotic: after three hours the upper phial was $\frac{1}{5}$ nitrous.

In the two laft experiments, the quantity of nitrous gas in the upper phial was lefs than might be expected; but the tube was at firft filled with common air, and fome muft enter on connecting the apparatus, which is fufficient to account for the refults.

FOURTH CLASS.

Azotic Gas, with Mixtures containing Oxigenous Gas.

Azote with oxygenous compounds.

1. Azotic gas and one $\frac{2}{3}$ oxygenous: after ftanding three hours the upper phial was of the ftandard 1.78, or about $\frac{1}{14}$ oxygenous.

2. Azotic gas with atmofpheric air: after ftanding three hours: the upper phial was not fenfibly diminifhed by nitrous gas; the under phial, however, had loft two per cent, or $\frac{1}{10}$ of its oxigen. The reafon of this was, that the azotic gas in this experiment having been juft made for it from nitrous gas, this laft had not been completely faturated with atmofpheric air, and hence had feized upon all the oxygen afcending into the upper phial.

Having now related all the experiments I made of any importance to the fubject, it will be proper to add, for the fake of thofe that may wifh to repeat fome of them, that great care muft be taken to keep the infide of the tube dry; for if a drop of water interpofe between the two gafes, I have found that it effectually prevents the intercourfe: glafs tubes fhould therefore be ufed, that one may be fatisfied on this head, as the obftruction will then be vifible.

I fhall make no further comments on the above experiments, by way of explanation: becaufe to thofe who underftand my hypothefis of elaftic fluids, they need none: and I think it would be in vain to attempt an explanation any other way.

I can

I cannot however, on this occasion, avoid adverting to some experiments of Dr. Priestley, which few modern philosophers can be unacquainted with: I mean those relating to the seeming conversion of water into air. (Vid. Philos. Transact. vol. 73, page 414,—or his Expts. abridged, vol. 2, page 407.) He found that unglazed earthern retorts containing a little moisture, when heated, admitted the external air to pass through their pores at the same time that aqueous vapour passed through the pores the contrary way or outward; and that this last circumstance was *necessary* to the air's entrance. The retorts are air-tight, so far as that blowing into them discovers no pores; but when subjected to a greater pressure, as that of the atmosphere, or even one much short of it, they are not able to prevent the passage of elastic fluids. The fact of air passing into the retort through its pores, and vapour out of them at the same time, are elegantly and most convincingly shewn by Dr. Priestley's experiments, in which he used the apparatus represented in plate 7, fig. 1, of the edition above referred to. The Doctor confesses his explanation of these remarkable facts is very inadequate; and no wonder, for it is impossible for him or any other to explain them on the commonly received principles of elastic fluids. But we will hear what he says on the subject:—"At present it is my opinion, that the agent in this case is that principle which we call *attraction* of *cohesion*, or that power by which water is raised in capillary tubes. But in what manner it acts in this case I am far from being able to explain. Much less can I imagine how *air* should pass one way and *vapour* the other, in the same pores, and how the transmission of the one should be necessary to the transmission of the other.—I am satisfied, however, that it is by means of such pores as air may be forced through, that this curious process is performed; because the experiment never succeeds but in such vessels as, by the air-pump at least, appear to be porous, though in all such."

The remarkable experiment of Priestley, of air entering earthen retorts while water passed out in a vapor.

Dr. Priestley's explanation or conjectures.

The truth is, these facts so difficult to explain are exactly similar to those which are the subject of this memoir: only instead of a *great number* of pores we have *one* of sensible magnitude, (the bore of the tube.) Let the porous retort have the same elastic fluid within and without, in the one case; and the two phials contain the same elastic fluid in the other, then

The fact is, that the steam and air mix by means of the pores.

then no tranfmiffion is obfervable in either; but if the retort have common air, or any other gas, without, and aqueous vapour, or any other elaftic fluid, except the outfide one, within; then the motion in and out commences, juft as with the phials in fimilar circumftances. In fact this laft obfervation has fince been verified by Dr. Prieftley himfelf, of which an account is given in No. 2, of the American Philofophical Tranfactions, vol. 5. After alluding to his experiments abovementioned, he obferves, " Since that time I have extended and diverfified the experiments, and have obferved, that what was done by *air* and *water*, will be done by *any two kinds of air*, and whether they have affinity to one another or not, that this takes place in circumftances of which I was not at all apprized before, and fuch as experimenters ought to be acquainted with, in order to prevent miftakes of confiderable confequence."

—and the fame happens in any two gafes.

The facts ftated above, taken altogether, appear to me to form as decifive evidence *for* that of elaftic fluids which I maintain, and *againft* the one commonly received, as any phyfical principle which has ever been deemed a fubject of difpute, can adduce.

III.

On the Horizontal Moon. By Dr. Okely. *In a Letter from Mr.* H. Steinhauer.

To Mr. NICHOLSON.

SIR, *Fulnuk, March* 1, 1806.

ABOUT the beginning of laft year, I had the pleafure, in compliance with your obliging letter to fend you impreffions of the Egyptian Scarabacus, which I hope came fafe to hand. Your kindnefs in inferting my trivial remarks upon the fame in your valuable Journal, encourage me to fubmit the following fhort effay, upon a fubject which has employed the ingenuity of feveral of your correfpondents, which I received from my friend Dr. Okely, of Wyke, near Hallifax, in confequence of fome converfations occafioned by the perufal of your work.

If

If you think it worthy a place in your collection, it will be confidered as an additional obligation conferred on,

SIR,

Your obedient fervant,

H. STEINHAUER.

Obfervations on the feemingly enlarged apparent Diameters of the Sun and Moon, when viewed in or near the Horizon.

Every one who views the fun or moon, when they are in the horizon, thinks that they appear larger than when they are feen in any more elevated part of the heavens. And aftronomers know, that the diftance of the fame fixed ftars is apparently greater when feen near the horizon, than when they are more elevated. But it is likewife well known to aftronomers, that the apparent magnitudes of the fun and moon, as well as the apparent diftances of any given fixed ftars, as meafured by the micrometer, are the fame in that part of the heavens which is near the horizon, as, in the fame circumftances, they are found to be in any other part, except that the moon, being really perceptibly farther from an obferver, placed on the earth's furface, when fhe appears in the horizon, than when fhe appears in the zenith, is found to have a fmaller apparent magnitude, agreeing with the caufes to which it is known to be owing. The firft mentioned phenomena muft therefore belong to the head of *optical deceptions.* Let us enquire from what fource this deception arifes.

General fact ftated that the heavenly bodies feem larger at low altitudes.

I am not the firft by whom the fource was fought for in the apparent flatnefs of the fky; but I differ as far as I know, from all others in my manner of connecting one appearance with the other.

In order to explain my idea of the matter, I fhall firft attempt to fhew that the flattened appearance of the vifible heavens is not an illufion, but a reality; or in other words, that an obferver placed on the earth is really at a greater diftance from a point of the fky, fituate in the horizon, than from a point fituate in the zenith.

Explanation of the flattened appearance of the heavens.

This will appear in the cleareft manner if we endeavour to give an anfwer to the two following queftions: *What is he fky?* and *Where is the fky?*

The fky is a real object, and offers a flattened concavity.

By the fky, I mean that blue concave fuperficies, within which every obferver on the furface of the earth finds himfelf placed

placed. What is this? It is certainly ſomething real and material, or elſe it would not appear coloured. For bodies, to appear coloured, muſt have parts of ſome determinate magnitude.

Where doth it exiſt? Not in thoſe immenſely diſtant parts of ſpace, where the heavenly bodies revolve. For if thoſe ſpaces contained any bodies of a determinate magnitude, and conſequently of a determinate denſity, the heavenly bodies could not continue through ages to revolve in the ſame periodic times; their momentum would be diminiſhed by reſiſtance, and the periodic times of their revolution would change. The blue ſky therefore cannot be placed beyond the atmoſphere of our earth. The ſmalleſt parts of bodies, that are coloured are blue, and the blue ſky is therefore either the atmoſphere itſelf or the ſmalleſt and moſt elevated vapours aſcending in it, or both together. The heavenly bodies ſhine through it, and therefore it cannot be opaque; it is itſelf of a blue colour, and therefore is not perfectly tranſparent.

Though we are ignorant of the exact height of the atmoſphere, yet we may take it for granted, that it does not extend as far as the moon, and therefore that the diſtance of its fartheſt points from the centre of the earth has a finite ratio to the ſemi-diameter of the earth. That the ratio is probably leſs than 2 : 1.

If therefore the blue heavens which ſurround the earth, and are concentric with it, have a ſemidiameter not double that of the earth, their horizontal points as viewed from the earth, muſt be farther from us than any that are nearer the zenith.

For let A C F (*Plate* VII. *Fig.* 1.) repreſent a great circle of the earth, and A C be its radius, and let the circle D B E G repreſent a great circle of the atmoſphere drawn with a radius A B not $= 2.$ A C, the line C D $>$ B C; H C $>$ B C.

—which is not always alike.

I was led to this ſolution of the flattened appearance of the heavens, by obſerving that, when the ſky is uniformly overcaſt with clouds, the concave ſuperficies appears conſiderably flatter than when the ſky is ſerene. In the former caſe, the two concentric circles in the figure approach nearer to each other, the clouds being nearer to the earth than the ſky is, and the ratio of D E to C B muſt of courſe increaſe.

Explanation from a diagram of the ſky, that

But to proceed. When any bodies ſituated behind a ſemitranſparent ſcreen are ſeen through it, they will appear to be fixed

fixed in the ſcreen at the points of interſection, which lines drawn from every point of the bodies to the eye of the obſerver, make with the ſcreen. Now ſuch a ſemi-tranſparent ſcreen, the blue ſkies interpoſe between the heavenly bodies and our eyes. They will therefore appear to be fixed in the ſky, at the above-mentioned points of interſection.

the heavenly bodies muſt appear largeſt in the horizon.

But if lines D C, H C, I C, B C, be drawn ſo that the angles at C are equal, they may be conſidered as coming from the extreme points of bodies which ſubtend equal angles of viſion, or which have the ſame apparent magnitude. The angle D C H may be conſidered as repreſenting the angle of viſion which the ſun ſubtends at the horizon. The equal angle H C I, the angle ſubtended by the ſame body in a more elevated ſituation. But $DH > HI$. Thus the heavenly bodies muſt appear enlarged in their vertical diameters, when in the horizon; and the ſame may be ſhewn of any other diameter They will therefore appear uniformly enlarged; which was the thing to be explained.

W. OKELY.

IV.

Account of ſome Specimens of Baſaltes from the northern Coaſt of Antrim. By the Rev. Dr. WILLIAM RICHARDSON.

(Concluded from Page 273.)

THE peninſula of Portruſh lies about ſix miles to the weſt of the Giant's Cauſeway, and on its eaſtern ſurface alone preſents theſe ſtrata.

Remarks on the baſaltes of the coaſt of Antrim.

In the ſpace of about 700 yards, it exhibits in miniature thoſe changes and interruptions of the ſtrata, which occur on the large ſcale along the northern baſaltic coaſt of Ireland. At the place where it emerges from the ſtrand, there firſt occurs a maſs compoſed of ſtrata of the coarſe and ſiliceous baſalt, placed over each other alternately; this is ſucceeded by an accumulation of regular ſtrata of the coarſe baſalt alone. A ſecond alternation, and a ſecond accumulation of the coarſe-grained ſtrata, come in order, and extend to the well called Tubber Wherry. Here commences an accumulation of many ſtrata of the ſiliceous

Remarks on the basaltes of the coast of Antrim.

ceous basalt alone, which stretches along the shore for about 100 yards, and then changes into a third alternation, which continues to the little boat-harbour, called *Port-in-too*, near which the siliceous basalt disappears. Over this stretch, notwithstanding the frequent change in the arrangement of the strata, the thickness of each stratum, of both species, remains pretty nearly the same, and the position of them all steadily so, viz. with a considerable dip to E. N. E.

The west side of the peninsula, though only about 400 yards distant, consists entirely of coarse basalt. It shows a bolder face, and is formed of rude massive pillars, from 60 to 80 feet long.

"I am aware," says Dr. Richardson, "that several mineralogists deny the shell-bearing stone to be basalt, while others contend strenuously that it is. I will not venture to decide on the question, but must remark, that I have never met with it but contiguous to basalt, and so solidly united to this last, that the continuity of the whole mass was uninterrupted. The grain of the stone graduates, as has been already remarked, into that of the common basaltes; and the arrangement of it and that of the basalt, with which it is so much mixed at Portrush and the Skerry island, is exactly the same; the strata of each scarcely differing in thickness, and not at all in inclination. The strata of both kinds break into prisms, and the surfaces, where accessible, exhibit the appearance of causeways, differing only in this, that in the siliceous basalt, the pentagon is the prevalent figure, and in the coarse basalt, the quadrangle. The fusibility of both stones is also nearly the same; the shells in the siliceous basalt are calcined in the fire, and many more are then discovered which had before escaped the eye *."

Whinstone

* Dr. Richardson observes, that some mineralogists deny that this fossil is basalt. Several of the members present when this paper was read, some of whom had examined the stone in its native place, were of that number. It was remarked, that though certain portions of the strata of this fossil bore much resemblance to some species of basalt, by far the greater part of the mass bore no resemblance whatever to any.

It was also stated, that the substance of the coarse-grained, undisputed basalt, which lies between the strata of this stone, does not contain any vestiges of marine animals; That veins often issue from

Whinstone Dikes on the Coast of Antrim.

Remarks on the basaltes of the coast of Antrim.

Dr. Richardson describes some particulars in the construction of the whinstone dikes on the coast of Antrim, which appear singular, and deserving of attention. These dikes, he says, are uniformly formed of large massive prisms laid horizontally, which are always divisible into smaller prisms that are likewise horizontal. To prevent confusion, he calls the first of these *component prisms*, and the second, or smaller ones into which the others break, *constituent prisms*.

The component prisms are sometimes of enormous size, and, in the same dike are nearly equal; the constituent prisms are small, (the sides about an inch long), and neatly formed.

The dike which traverses the Giant's Causeway, differs from those on other parts of the coast, by having no component prisms. It resembles a plain wall, of which the parts shiver under the hammer into very neat constituent prisms, In the dike at Seaport the same thing is observed; the prismatic structure does not penetrate two inches from its edge; the whole interior seems an amorphous mass.

The specimens of this latter dike, sent to Dr. Hope, exhibit its continuity with the adjacent basaltic rock which it traverses, and also the continuity of the fine basalt of its edge with the *granular* stone which composes the middle of the dike.

The dike of *Port-coan* is a very solid mass, composed of stones apparently round, and imbedded in a basaltic paste, or indurated mortar. The round stones are formed of concentric spheres, like the coats of an onion; they exceed a foot in diameter, and, together with the mortar by which they are united, they form a very compact and highly indurated rock.

Besides these large dikes, Dr. Richardson remarks, that veins from half an inch to an inch and a half thick, often cut the basaltic strata on that coast in all directions. The materials of these veins are never the same with the contiguous basalt,

from the beds of this real basalt, and pervade the supposed siliceous species; some of them connecting together the separate beds of the real basalt; others dying away in slender ramifications; as they rise through the interposed stratum. In no instance is this reversed: The veins never proceed from what is called the Siliceous Basalt. It was farther observed, that both the fracture and external surface of this stone exhibit a stratified structure, in many instances, which never happens in the true basaltes.

but

Remarks on the basaltes of the coast of Antrim.

but are generally finer. At Portrush is a large vein, and near it a smaller vein, not an inch thick, which, proceeding from below, terminates in the solid rock before it reaches the surface.

Miscellaneons Observations.

Some of the specimens in Dr. Richardson's catalogue are from a quarry in a mass of basalt at Ballylugan, two miles south of Portrush. This basalt contains small cavities in its interior; many of them full of fresh water, which gushes out when the stone is broken by the hammer, as if it had been in a state of compression. The stone is so hard, and flies so in pieces, that Dr. Richardson has not been able to collect any of the water for the purpose of analysis.

The face of the quarry in which this variety of the basalt is found is about 15 feet high, and is cut into a stratum, the thickness of which is not yet ascertained. The rock is entirely columnar, the pillars somewhat smaller than those of the Giant's Causeway, less perfect, not articulated, sometimes bent, and variously inclined. The sides and the interior of the pillars are full of cavities. In consequence of the observations of Dr. Hamilton and Mr. Whitehurst respecting the porous texture of the air or bladder holes of the basaltes of the Causeway and its vicinity, Dr. Richardson has examined a great variety; but in no instance, except this of Ballylugan, has he found cavities, in the interior of the basaltic rocks on this coast, though they are frequent on the surface exposed to the air.

The last variety of whinstone enumerated by Dr. Richardson is the Ochrous, which makes, as he says, a conspicuous figure in the stupenduous precipices along the coast of Antrim. It is disposed in extensive strata of every thickness, from an inch to twenty-four feet, and varies in colour, from a bright minium to a dull ferruginous brown.

Three remarks are made by Dr. Richardson, that are undoubtedly of importance, and show that this stone is merely basalt in a certain state of decomposition.

1. The ochrous strata are extensive; they remain always parallel to the basalt strata which they separate; they unite to the basalt without interrupting its solidity; the change from the one

one to the other is fudden, and the lines of dematkation are diftinct. The ochrous ftone is never found but contiguous to other bafalt.

2. The fubftances imbedded in the ochrous rock, and in bafalts, are exactly the fame; calcareous fpar, zeolite, chalcedony, &c.

3. Among the varieties which this rock prefents, there may be found every intermediate ftage between found bafalt and perfect ochre. The change is often partial, beginning with veins and flender ramifications.

V.

On the Abforption of Gafes by Water and other Liquids. By JOHN DALTON.*

1. IF a quantity of pure water be boiled rapidly for a fhort time in a veffel with a narrow aperture, or if it be fubjected to the air-pump, the air exhaufted from the receiver containing the water, and then be brifkly agitated for fome time, very nearly the whole of any gas the water may contain, will be extricated from it.

Air or gas is extricated from water by boiling and agitation in vacuo.

2. If a quantity of water thus freed from air be agitated in any kind of gas, not chemically uniting with water, it will abforb its bulk of the gas, or otherwife a part of it equal to fome one of the following fractions, namely, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c. thefe being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c. or $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, $\frac{1}{4^3}$, &c. the fame gas always being abforbed in the fame proportion, as exibited in the following table:—It muft be underftood that the quantity of gas is to be meafured *at the preffure and temperature with which the impregnation is effected.*

The volume of every gas abforbed by water is conftant, and is either equal to the bulk or to the cube of a reciprocal of that bulk,

—equal preffures and temperatures being fuppofed.

* Manchester Mem. N. S. Vol. I.

Bulk

Table of quantities.

Bulk abforbed, the bulk of water being unity. $\frac{1}{1^3} = 1$	Carbonic acid gas, fulphuretted hydrogen, nitrous oxide.*
$\frac{1}{2^3} = \frac{1}{8}$	Olefiant gas, of the Dutch chemifts.
$\frac{1}{3^3} = \frac{1}{27}$	Oxygenous gas, nitrous gas,† carburretted hydrogen gas, from ftagnant water.
$\frac{1}{4^3} = \frac{1}{64}$	Azotic gas, hydrogenous gas, carbonic oxide.
$\frac{1}{5^3} = \frac{1}{125}$	None difcovered.

3. The gas thus abforbed may be recovered from the water the fame in quantity and quality as it entered, by the means pointed out in the firft article.

Water abforbs any gas in the fame quantity, whether it contain another gas or not.

4. If a quantity of water free from air be agitated with a mixture of two or more gafes (fuch as atmofpheaic air) the water will abforb portions of each gas the fame as if they were prefented to it feparately in their proper denfity.

Ex. gr. Atmofpheric air, confifting of 79 parts azotic gas, and 21 parts oxygenous gas, *per cent.*

Water abforbs $\frac{1}{64}$ of $\frac{79}{100}$, azotic gas = 1.234

——— $\frac{1}{27}$ of $\frac{21}{100}$, oxygen gas = .778

Sum, *per cent.* 2.012

* According to Mr. William Henry's experiments, water does not imbibe quite its bulk of nitrous oxide; in one or two inftances with me it has come very near it: The apparent deviation of this gas, may be owing to the difficulty of afcertaining the exact degree of its impurity.

† About $\frac{1}{20}$ of nitrous gas is ufually abforbed; and $\frac{1}{27}$ is recoverable: This difference is owing to the refiduum of oxygen in the water, each meafure of which takes $3\frac{1}{2}$ of nitrous gas to faturate it, when in water. Perhaps it may be found that nitrous gas ufually contains a fmall portion of nitrous oxide.

5. If

5. If water impregnated with any one gas (as hydrogenous) be agitated with another gas *equally* abſorbable (as azotic) there will *apparently* be no abſorption of the latter gas; just as much gas being found after agitation as was introduced to the water; but upon examination the reſiduary gas will be found a *mixture* of the two, and the parts of each, in the water, will be exactly proportional to thoſe out of the water.

If water and gaſes be agitated in confinement, a mixture will take place of the gaſes in and out of the water, &c.

6. If water impregnated with any one gas be agitated with another gas leſs or more abſorbable; there will *apparently* be an increaſe or diminution of the latter; but upon examination the reſiduary gas will be found a *mixture* of the two, and the proportions agreeable to article 4.

7. If a quantity of water in a phial having a ground ſtopper very accurately adapted, be agitated with any gas, or mixture of gaſes, till the due ſhare has entered the water; then, if the ſtopper be ſecured, the phial may be expoſed to any variation of *temperature*, without diſturbing the equilibrium: That is, the quantity of gas in the water will remain the ſame whether it be expoſed to heat or cold, if the ſtopper be air-tight.

Temperature does not affect ſuch incloſed fluids.

N. B. The phial ought not to be near full of water, and the temperature ſhould be between 32° and 212°.

8. If water be impregnated with one gas (as oxygenous), and another gas, having an affinity for the former (as nitrous), be agitated along with it; the abſorption of the latter gas will be greater, by the quantity neceſſary to ſaturate the former, than it would have been if the water had been free from gas.*

Gaſes which are diſpoſed to combine.

9. Moſt liquids free from viſcidity, ſuch as acids, alcohol, liquid ſulphurets, and ſaline ſolutions in water, abſorb the ſame quantity of gaſes as pure water; except they have an affinity for the gas, ſuch as ſulphurets for oxygen, &c.

The abſorption by other liquids is the ſame as by water.

The preceding articles contain the principal facts neceſſary to eſtabliſh the theory of abſorption: Thoſe that follow are of a ſubordinate nature, and partly deducible as corrollaries to them.

* One part of oxygenous gas requires 3.4 of nitrous gas to ſaturate it in water. It is agreeable to this that the rapid mixture of oxygenous and nitrous gas over a broad ſurface of water, occaſions a greater diminution than otherwiſe. In fact, the *nitrous* acid is formed this way; whereas, when water is not preſent, the *nitric* acid is formed, which requires juſt half the quantity of nitrous gas, as I have lately aſcertained.

Natural waters or rain contain the due ſhare of atmoſ. air; but corrupt water has leſs or no oxygen.

10. Pure diſtilled water, rain and ſpring water uſually contain nearly their due ſhare of atmoſpheric air: if not, they quickly acquire that ſhare by agitation in it, and loſe any other gas they may be impregnated with. It is remarkable, however, that water by ſtagnation, in certain circumſtances, loſes part or all of its oxygen, notwithſtanding its conſtant expoſition to the atmoſphere. This I have uniformly found to be the caſe in my large wooden pneumatic trough, containing about eight gallons, or $1\frac{1}{3}$ cubic foot of water. Whenever this is repleniſhed with tolerably pure rain water, it contains its ſhare of atmoſpheric air; but in proceſs of time it becomes deficient of oxygen: In three months the whole ſurface has been covered with a pellicle, and no oxygenous gas whatever was found in the water. It was grown offenſive, but not extremely ſo; it had not been contaminated with any material portion of metallic or ſulphureous mixtures, or any other article to which the effect could be aſcribed.* The quantity of azotic gas is not materially diminiſhed by ſtagnation, if at all.—Theſe circumſtances, not being duly noticed, have been the ſource of great diverſity in the reſults of different philoſophers upon the quantity and quality of atmoſpheric air in water. By article 4, it appears that atmoſpheric air expelled from water ought to have 38 per cent. oxygen; whereas by this article air may be expelled from water that ſhall contain from 38 to 0 per cent. of oxygen. The diſappearance of oxygenous gas in water, I preſume, muſt be owing to ſome impurities in the water which combine with the oxygen. Pure rain water that had ſtood more than a year in an earthenware bottle had loſt none of its oxygen.

Why water by agitation abſorbs moſt oxygen from air.

11. If water free from air be agitated with a ſmall portion of atmoſpheric air (as $\frac{1}{15}$ of its bulk) the reſiduum of ſuch air will have proportionally leſs oxygen that the original: If we take $\frac{1}{15}$, as above, then the reſiduum will have only 17 per cent. oxygen; agreeably to the principle eſtabliſhed in article 4. This circumſtance accounts for the obſervations made by Dr. Prieſtley, and Mr. William Henry, that water abſorbs oxygen in preference to azote.

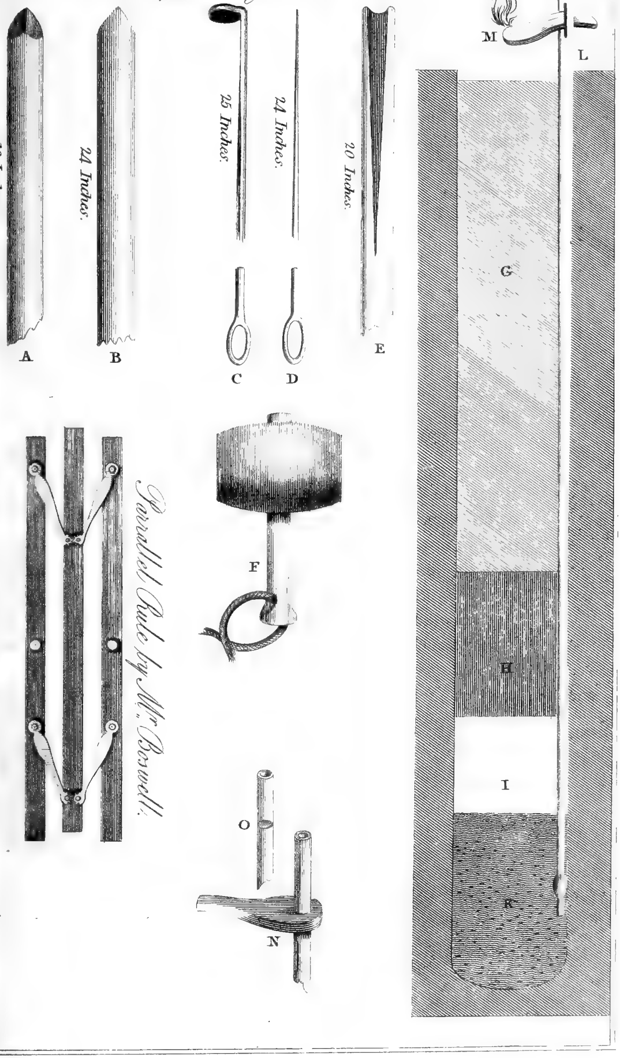
Diſappearance of gas by agitation under a jar.

12. If a tall glaſs veſſel, containing a ſmall portion of gas be inverted into a deep trough of water, and the gas thus confined by the glaſs and the water be briſkly agitated, it will gradually diſappear.

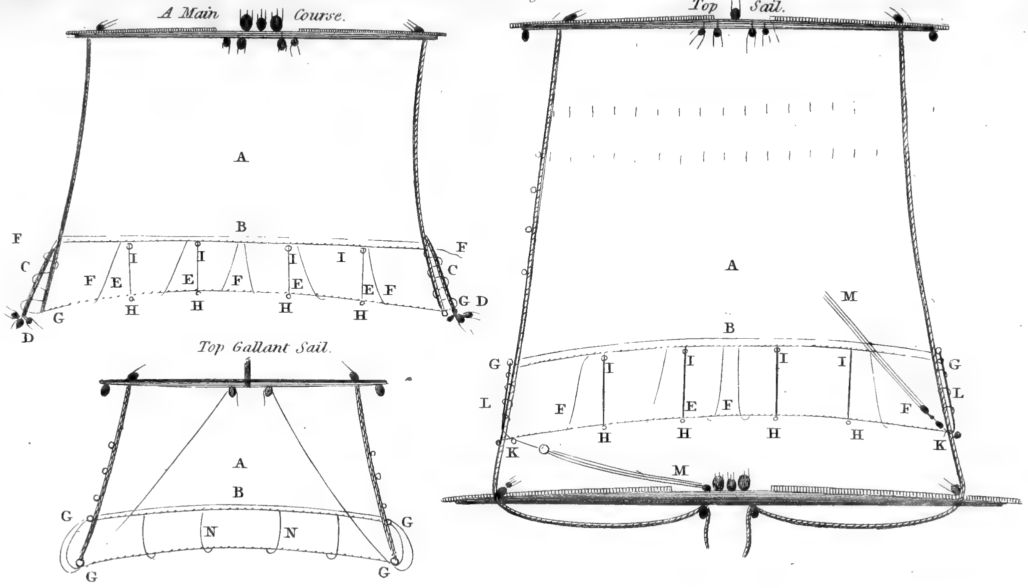
* It was drawn from a leaden ciſtern.

It

Method of blasting Rocks.



Improved Sails by Capt. Cowan.



It is a wonder that Dr. Prieftley, who feems to have been the firft to notice this fact, fhould have made any difficulty of it;—the lofs of gas has evidently a mechanical caufe; the agitation divides the air into an infinite number of minute bubbles, which may be feen pervading the whole water; thefe are fucceffively driven out from under the margin of the glafs into the trough, and fo efcape.

13. If old ftagnant water be in the trough, in the laft experiment, and atmofpheric air be the fubject, the oxygenous gas will very foon be almoft wholly extracted, and leave a refiduum of azotic gas; but if the water be fully impregnated with atmofpheric air at the beginning, the refiduary gas examined at any time will be pure atmofpheric air. Old ftagnant water.

14. If any gas not containing either azotic or oxygenous gas, be agitated over water containing atmofpheric air, the refiduum will be found to contain both azotic and oxygenous gas. Agitation of gas over common water gives out oxygen and azote by mixture.

15. Let a quantity of water contain equal portions of any two or more unequally abforbable gafes: For inftance, azotic gas, oxygenous gas, and carbonic acid gas; then, let the water be boiled or fubjected to the air-pump, and it will be found that unequal portions of the gafes will be expelled. The azotic will be the greateft part, the oxygenous next, and the carbonic acid will be the leaft. For, the previous impregnation being fuch as is due to atmofpheres of the following relative forces nearly: The efcape of any gas from water by removing the preffure will be greater the lefs abforbable.

Azotic - - -	21 inch. of mercury.
Oxygenous - -	9
Carbonic acid -	$\frac{1}{3}$

confequently, when thofe forces are removed, the refiliency of the azotic gas will be the greateft, and that of the carbonic acid the leaft; the last will even be fo fmall as not to overcome the cohefion of the water without violent agitation.

Remarks on the Authority of the preceding Facts.

In order to give the chain of facts as diftinct as poffible, I have not hitherto mentioned by whom or in what manner they were afcertained. Remarks on the laws of abforption of gafes by denfe fluids, &c.

The fact mentioned in the firft article has been long known; a doubt, however, remained refpecting the quantity of air ftill

Remarks on the laws of absorption of gases by dense fluids, &c.

left in water after ebullition and the operation of the air-pump. The subsequent articles will, I apprehend, have placed this in a clearer point of view.

In determining the quantity of gases absorbed, I had the result of Mr. William Henry's experience on the subject before me, an account of which has been published in the Philosophical Transactions for 1803. By the reciprocal communications since, we have been enabled to bring the results of our experiments to a near agreement; as the quantities he has given in his appendix to that paper nearly accord with those I have stated in the second article. In my experiments with the less absorbable gases, or those of the 2d, 3d, and 4th classes, I used a phial holding 2700 grains of water, having a very accurately ground stopper; in those with the more absorbable of the first class, I used an eudiometer tube, properly graduated, and of aperture so as to be covered with the end of a finger. This was filled with the gas and a small portion expelled by introducing a solid body under water; the quantity being noticed by the quantity of water that entered on withdrawing the solid body, the finger was applied to the end and the water within agitated; then removing the finger for a moment under water, an additional quantity of water entered, and the agitation was repeated till no more water would enter, when the quantity and quality of the residuary gas was examined. In fact, water could never be made to take its bulk of any gas by this procedure; but if it took $\frac{9}{10}$, or any other part, and the residuary gas was $\frac{9}{10}$ pure, then it was inferred that water would take its bulk of that gas. The principle was the same in using the phial; only a small quantity of the gas was admitted, and the agitation was longer.

There are two very important facts contained in the second article. The first is, that the quantity of gas absorbed is as the density or pressure. This was discovered by Mr. William Henry, before either he or I had formed any theory on the subject.

The other is, that the density of the gas in the water has a special relation to that out of the water, the distance of the particles within being always some multiple of that without: Thus, in the case of carbonic acid, &c. the distance within and without is the same, or the gas within the water is of the same density as without; in olefiant gas the distance of the particles

particles in the water is twice that without; in oxygenous gas, &c. the diftance is juft three times as great within as without; and in azotic, &c. it is four times. This fact was the refult of my own enquiry. The former of thefe, I think, decides the effect to be mechanical; and the latter feems to point to the principle on which the equilibrium is adjufted. **Remarks on the laws of abforption of gafes by denfe fluids, &c.**

The facts noticed in the 4th, 5th and 6th articles, were inveftigated *à priori* from the mechanical hypothefis, and the notion of the diftinct agency of elaftic fluids when mixed together. The refults were found entirely to agree with both, or as nearly as could be expected from experiments of fuch nature.

The facts mentioned in the 7th article are of great importance in a theoretic view; for, if the quantity of gas abforbed depend upon mechanical principles, it cannot be affected by temperature in confined air, as the mechanical effect of the external and internal air are alike increafed by heat, and the denfity not at all affected in thofe circumftances. I have tried the experiments in a confiderable variety of temperature without perceiving any deviation from the principle. It deferves further attention.

If water be, as pointed out by this effay, a mere receptacle of gafes, it cannot affect their affinities: hence what is obferved in the 8th article is too obvious to need explanation.—And if we find the abforption of gafes to arife not from a chemical but a mechanical caufe, it may be expected that all liquids having an equal fluidity with water, will abforb like portions of gas. In feveral liquids I have tried, no perceptible difference has been found; but this deferves further inveftigation.

After what has been obferved, it feems unneceffary to add any explanation of the 10th and following articles.

Theory of the Abforption of Gafes by Water, &c.

From the facts developed in the preceding articles, the following theory of the abforption of gafes by water feems deducible.

1. All gafes that enter into water and other liquids by means of preffure, and are wholly difengaged again by the removal of that preffure, are *mechanically* mixed with the liquid, and not *chemically* combined with it.

2. Gafes

Remarks on the laws of absorption of gases by dense fluids, &c.

2. Gases so mixed with water, &c. retain their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum.

3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect.

4. When water has absorbed its bulk of carbonic acid gas, &c. the gas does not press on the water at all, but presses on the containing vessel just as if no water were in. When water has absorbed its proper quantity of oxygenous gas, &c. that is, $\frac{1}{27}$ of its bulk, the exterior gas presses on the surface of the water with $\frac{26}{27}$ of its force, and on the internal gas with $\frac{1}{27}$ of its force, which force presses upon the containing vessel, and not on the water. With azotic and hydrogenous gas the proportions are $\frac{63}{64}$ and $\frac{1}{64}$ respectively. When water contains no gas, its surface must support the whole pressure of any gas admitted to it, till the gas has, in part, forced its way into the water.

5. A particle of gas pressing on the surface of water is analogous to a single shot pressing upon the summit of a square pile of them. As the shot distributes its pressure equally amongst all the individuals forming the lowest stratum of the pile, so the particle of gas distributes its pressure equally amongst every successive horizontal stratum of particles of water downwards till it reaches the sphere of influence of another particle of gas. For instance; let any gas press with a given force on the surface of water, and let the distance of the particles of gas from each other be to those of water as 10 to 1; then each particle of gas must divide its force equally amongst 100 particles of water, as follows:—It exerts its immediate force upon 4 particles of water; those 4 press upon 9, the 9 upon 16, and so on according to the order of square numbers, till 100 particles of water have the force distributed amongst them; and in the same stratum each square of 100, having its incumbent particle of gas, the water below this stratum is uniformly pressed by the gas, and consequently has not its equilibrium disturbed by that pressure.

6. When water has absorbed $\frac{1}{27}$ of its bulk of any gas, the stratum of gas on the surface of the water presses with $\frac{26}{27}$ of its

its force on the water, in the manner pointed out in the laſt article, and with $\frac{1}{27}$ of its force on the uppermoſt ſtratum of gas in the water: The diſtance of the two ſtrata of gas muſt be nearly 27 times the diſtance of the particles in the incumbent atmoſphere, and 9 times the diſtance of the particles in the water. This comparatively great diſtance of the inner and outer atmoſphere ariſes from the great repulſive power of the latter, on account of its ſuperior denſity, or its preſenting 9 particles of ſurface to the other 1. When $\frac{1}{64}$ is abſorbed, the diſtance of the atmoſpheres becomes 64 times the diſtance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal ſtrata of gas in and out of water, will ſufficiently illuſtrate theſe poſitions.

Remarks on the laws of abſorption of gaſes by denſe fluids, &c.

7. An equilibrium between the outer and inner atmoſpheres can be eſtabliſhed in no other circumſtance than that of the diſtance of the particles of one atmoſphere being the ſame or ſome multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this caſe the diſtance of the inner and outer atmoſpheres is ſuch as to make the perpendicular force of each particle of the former on thoſe particles of the latter that are immediately ſubject to its influence, phyſically ſpeaking, equal; and the ſame may be obſerved of the ſmall lateral force.

8. The greateſt difficulty attending the mechanical hypotheſis, ariſes from different gaſes obſerving different laws.—Why does water not admit its bulk of every kind of gas alike? This queſtion I have duly conſidered, and though I am not yet able to ſatisfy myſelf completely, I am nearly perſuaded that the circumſtance depends upon the weight and number of the ultimate particles of the ſeveral gaſes: thoſe whoſe particles are lighteſt and ſingle being leaſt abſorbable, and the others more, according as they increaſe in weight and complexity.* An enquiry into the relative weights of the ultimate particles of bodies, is a ſubject, as far as I know, entirely new: I have lately been proſecuting this enquiry with remarkable ſucceſs. The principle cannot be entered upon in this paper; but I ſhall juſt ſubjoin the reſults, as far as they appear to be aſcertained by my experiments.

* Subſequent experience renders this conjecture leſs probable.

Table

Weights of the particles of bodies.

Table of the relative weights of the ultimate particles of gaseous and other bodies.

Hydrogen	1
Azote	4.2
Carbon	4.3
Ammonia	5.2
Oxygen	5.5
Water	6.5
Phosphorus	7.2
Phosphuretted hydrogen	8.2
Nitrous gas	9.3
Ether	9.6
Gaseous oxide of carbon	9.8
Nitrous oxide	13.7
Sulphur	14.4
Nitric acid	15.2
Sulphuretted hydrogen	15.4
Carbonic acid	15.3
Alcohol	15.1
Sulphureous acid	19.9
Sulphuric acid	25.4
Carburetted hydrogen from stagnated water	6.3
Olefiant gas	5.3

VI.

On the supposed fascinating Power of the Rattle-snake. With a remarkable Indian Tradition upon which it is probable the early European Settlers founded their popular Tales. From the Philadelphia Medical and Physical Journal, by BENJAMIN SMITH BARTON, *M. D.*

Fascinating power of the rattle-snake described by Fabricius.

ALMOST all amphibious animals (says Professor Fabricius,) the tortoise excepted, live by preying upon other animals. But being destitute of strength and swiftness, nature has given, at least to some of them (according to the testimony of many and creditable writers;) the peculiar faculty of forcing other animals to throw themselves into their open jaws. Kalm, the Swede, and the American Smith Barton, assert of the American

American ſerpents, that if they fix their fiery, glaring eyes upon any animal, ſuch as a ſquirrel, or a bird, within a certain diſtance, they entirely loſe the power of eſcaping, but throw themſelves, ſlowly, irreſiſtibly, into the extended jaws of the ſnake. And if any thing diſturbs the ſnake, ſo that it withdraws its eyes but for one moment, they eſcape with the utmoſt precipitation.

We obſerve (continues this learned naturaliſt) ſomething ſimilar to this in our common, tardy, thick, and fat toads, which frequently ſit under little ſtones and buſhes, having their mouths wide open, into which flies, bees, and other inſects, are drawn in the ſame manner. All the theories that have hitherto been offered to explain theſe appearances appear to me both unnatural and improbable. Indeed, I cannot but doubt the reality of the fact itſelf, until we ſhall receive further obſervations and diſcoveries relative to it.

J. C. FABRICII, &c.
Reſultate Natur-Hiſtoriſcher Vorleſungen, *p.* 267, 268. *Kiel:* 1804.

It will be evident to any one, who has peruſed, with attention, my two publications * on the ſuppoſed faſcinating faculty of the rattle-ſnake, and other American ſerpents, that Mr. Fabricius has by no means fully comprehended my peculiar theory. I have not adopted the hypotheſis of the very reſpectable Kalm, with whoſe name mine is mentioned by the Daniſh Profeſſor. On the contrary, I have endeavoured to ſhow, and I flatter myſelf that I have very ſatisfactorily ſhown, that there is no ſolid foundation for the vulgar, and very generally-received opinion, that ſerpents are endued with the faculty of faſcinating, or charming, other animals. Annotation by Dr. Barton.

B. S. B.

The following very curious tradition of ſome of our Indians, relative to ſerpents, is worthy of publication in this place. A *part* of the tradition has already been publiſhed in my Narrative.

* A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-ſnake, and other American Serpents. Philadelphia: 1796.—Supplement to a Memoir, &c. Philadelphia, 1800.—Or ſee Philoſ. Journal, Vols. VII. and VIII.

Supplement

Supplement to a Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-ſnake, and other American Serpents.

'Having queſtioned Indians, a number of times, with reſpect to ſnakes having the power of charming, and always being anſwered in the negative, I was at length deſired (ſays my friend, Mr. John Heckewelder) to give the reaſon the white people had for believing ſuch a thing, which not being ſatisfactory, Pemaholend * declared: "The rattle-ſnake obtains its food merely by ſlyneſs, and a perſevering patience. It knoweth as well where to watch for its prey as a cat does, and ſucceeds as well. It has, and retains its hunting grounds. In ſpring, when the warm weather ſets in, and the woods ſeem alive with the ſmaller animals, it leaves its den. It will croſs a river, and go a mile and further from its den, to the place it intends to ſpend the ſummer; and in fall, when all the young animals bred this ſeaſon are become ſtrong and active, ſo that they are no more ſo eaſily overtaken or caught, it directs its courſe back again, to its den, the ſame as a hunter does to his camp.

The rattle-ſnake catches its prey by craft and addreſs.

Indian tradition.

"The white-people," continued Pemaholend, "probably have taken the idea of this ſnake having the power of charming from a tradition of ours (the Indians) which our forefathers have handed down to us, from many hundred years back, and long before ever the white people came into this country. Then (they tell us) there *was* ſuch a ſnake, and a rattle-ſnake too, but then there was only *this one* ſnake which had this power, and he was afterwards deſtroyed; and ſince that time it hath never been ſaid that any other of the kind had made its appearance."

American native tradition about a rattle-ſnake.

'At my requeſt, Pemaholend related the tradition, and in the following words. "Our forefathers have told us, that at a ſmall lake, or large pond, not a great diſtance from where, as is believed, now the great city *Quequenàku* (Philadelphia) is built, there dwelt a rattle-ſnake, whoſe length and thickneſs exceeded that of the thickeſt and longeſt tree in the woods. This ſnake was very deſtructive, not only in deſtroying ſo much game, but in devouring ſo many Indians: for when he was hungry, he only looked round, and whatever he

* An aged and much reſpected Delaware-Indian.

ſaw, whether Indian, deer, turkey, or even geeſe flying, he only held his head that way, opening his mouth wide, and drawing breath in the manner we do, and nothing could prevent ſuch living creature entering his jaws. It is even ſaid, that a whole flock of geeſe, flying at a great diſtance, have been drawn into his mouth, at one time; * and it was well known among the Indians, that of all the hunters or travellers, who paſſed that way, very few eſcaped him. American native tradition about a rattle-ſnake.

"The Indians well knew when he was hungry, for then he grew angry, and blew with his mouth, which ſounded like thunder: for his breath was ſo powerful, that all the trees, however large, would bend, and even ſometimes break down before him. There being no proſpect of ever killing him with arrows, on account of the barrenneſs of the land far round the lake, into which he would always retire, after ſatisfying his hunger, a great council of the nation was called together, and the queſtion put, *Where are the Mannittoes of the nation? Are they no more? Shall the whole of the nation be deſtroyed by a Mannitto-Snake?* At length, two young men, endowed with *Mannittoie powers*, offered their ſervices, and declared, that unleſs the Mannittoie power of the ſnake exceeded theirs, they ſhould ſucceed; but they would, at all events, make an attempt. They then bid farewell to the aſſembly and their friends, dived into the river, from whence they proceeded under the water to a place oppoſite the Mennúppeek (lake, or large pond) where this ſnake dwelt. They made an opening under ground, from the river to the centre of the pond, by which the pond was drained, and became perfectly dry. After returning again, the ſame way they had come, they found the ſnake in great uneaſineſs, and on dry ground. Taking then the advantage of the dry weather, and the graſs far around the ſnake being dry, they ſet fire to the graſs, at a diſtance, and around the ſnake, by which means he was burnt

* It is curious, at leaſt, to compare this part of the Indian tradition with what Metrodorus, as cited by Pliny, relates of certain Aſiatic ſerpents. Theſe, he ſays, by means of their breath, attracted birds, however high they were, or however quick their flight. "Metrodorus, circa Rhyndacum amnem in Ponto, ut ſupervolantes quamvis alte perniciterque, alites hauſtu raptas abſorbeant." Plin. Hiſt. Nat. lib. viii. cap. 14.

to

American native tradition about a rattle-fnake.

to death.* Thus (continued Pemaholend) was the monfter killed by two mannitto men of the nation: for, you muft know, in thofe days, we had fuch men among us, who could live as well in the water as on land."

'Converfing one day with a Monfy (advanced in years) on ancient times, on the migration of the Indians, &c. he, in order to convince me (fays Mr. Heckewelder) what the Indians once were, mentioned the killing of the big fnake, the hiftory of which, according to his relation, differing only in the following points:

"*a.* He did not think it had been a rattle-fnake, but underftood the old men, from whom he had heard it fo often related (when he was young), that it was a fnake of a peculiar kind, and had feet; and that never fince had a fnake of this kind appeared:

"*b.* That he was not fure as to the place where this fnake kept; believed it had been higher up the country, and kept in a wide and deep place of the river, and in the country of the Munfees (or Minfy) and was killed by a Mannitto Munfee:

"*c.* That after the nation had met in council, and the above queftions put, a Munfee man of no character, nor feemingly of any confequence to the nation, faid and declared, that he had *Mannittoie Powers;* could and would deftroy the monfter, prefcribing the ceremonies the affembly were to obferve during the expedition. That he then made a very ftrong arrow, or fpear, fharp at both ends; and being equipped, took leave of the affembly—plunged into the river, and dived under water, until he arrived within a fmall diftance of the place where the fnake lay, or floated, bafking in the fun. Here he afcended to the furface, and calling out to the fnake to receive him, he opened his mouth wide, and drew him in, when, however, in an inftant, the fnake was ftabbed by him through both his fides, with the fpear, which wounded him fo deadly, that he gave a whirl, and being under great pain, difcharged his excrements, and with the fame this hero, who then

* Even this part of the Indian tradition feems to be borrowed from the old world. See a curious relation of the capture of an enormous ferpent in *The Life of Sethos, as taken from private memoirs of the Egyptians.* Vol. i. p. 125—147. London, 1737.

then ſwam again to ſhore, announcing his victory, and congratulating the aſſembly on the deliverance of the nation.

"Thus (continued the old Munſee) were the Indians of thoſe days *Mannittoes*. Nothing could reſiſt them. They knew nothing of drowning. Our firſt Parents have ſprung from the bottom of a lake."

VII.

A Deſcription of a Property of Caoutchouc, or Indian Rubber; With ſome Reflections on the Cauſe of the Elaſticity of this Subſtance. In a Letter to Dr. HOLME.*

SIR, *Middleſhaw, near Kendal, Nov.* 26, 1802.

THE ſubſtance called Caoutchouc, or Indian Rubber, poſſeſſes a ſingular property; which, I believe, has never been taken notice of in print, at leaſt by any Engliſh writer; the preſent letter contains my experiments and reflections on the ſubject; and ſhould they appear to deſerve the attention of your philoſophical friends, I am certain you will take the trouble of communicating the paper to the Literary and Philoſophical Society of Mancheſter.

Caoutchouc more pliant by heat,

The property I am about to deſcribe depends on the temperature of the Caoutchouc, which is uſed in the experiment; for heat increaſes the pliancy of the ſubſtance, and cold, on the contrary, renders it more rigid: ſo that when a ſlip of this reſin has been ſufficiently warmed, it may be extended to more than twice its natural length, by a moderate force applied to its extremities, after which it will recover its original dimenſions in a moment, provided one of the ends of it be let go as ſoon as it has been ſtretched. This diſpoſition of the ſubſtance may be produced by a degree of temperature leſs than the heat of the blood; it is therefore neceſſary to prepare a ſlip of it, by ſteeping it for a few minutes in warm water, or by holding it ſomewhat longer in the fiſt; either of theſe precautions makes the reſin pliant, and fits it for the exriment; which is performed in the following manner.

* Mancheſter Mem. N. S. Vol. I.

I made

and lefs denfe.

I made a piece of Caoutchouc a little heavier than an equal bulk of water, the temperature of which was 45 degrees: the veffel containing the refin and water was then placed on the fire; and when the contents of it were heated to 130 degrees, the Caoutchouc floated on the furface.

It becomes cold by fudden drawing out, and hot by contraction.

Exp. 1. Hold one end of the flip, thus prepared, between the thumb and fore-finger of each hand; bring the middle of the piece into flight contact with the edges of the lips;* taking care to keep it ftraight at the time, but not to ftretch it much beyond its natural length: after taking thefe preparatory fteps, extend the flip fuddenly; and you will immediately perceive a fenfation of warmth in that part of the mouth which touches it, arifing from an augmentation of temperature in the Caoutchouc: for this refin evidently grows warmer the further it is extended; and the edges of the lips poffefs a high degree of fenfibility, which enables them to difcover thefe changes with greater facility than other parts of the body. The increafe of temperature, which is perceived upon extending a piece of Caoutchouc, may be deftroyed in an inftant, by permitting the flip to contract again; which it will do quickly by virtue of its own fpring, as oft as the ftretching forth ceafes to act as foon as it has been fully exerted. Perhaps it will be faid, that the preceding experiment is conducted in a negligent manner; that a perfon, who wifhes for accuracy, will not truft his own fenfe of feeling in inquiries of this defcription, but will contrive to employ a thermometer in the bufinefs. Should the objection be ftarted, the anfwer to it is obvious; for the experiment in its prefent ftate demonftrates the reality of a fingular fact; by convincing that fenfe, which is the only direct judge in the cafe, that the temperature of a piece of Caoutchouc may be changed, by compelling it to change its dimenfions. The ufe of a thermometer determines the relative magnitudes of thefe variations, by referring the queftion of temperature to the eye; experiments of this fort are therefore of a mathematical nature, and afford a kind of knowledge with which we have nothing to do at prefent; for we are not inquiring after proportions, but endeavouring to efta-

* This effect was firft noticed in 1784, at Mr. Kirwan's meetings in Newman ftreet, and Dr. Crawford afcribed it to change of capacity fimilar to what he fuppofed to take place in a nail by hammering.—N.

blifh

blifh the certainty of a fact, which may affift in difcovering the reafon of the uncommon elafticity obfervable in Caoutchouc. My effay or letter appears to be running into a long digreffion; the fubject muft therefore be refumed, and it will not be improper to premife the following fimple experiment, in the prefent ftate of the inquiry; becaufe it feems capable of affording no inconfiderable degree of infight into the plan which nature purfues in producing the phenomenon in queftion.

Exp. 2. If one end of a flip of Caoutchouc be faftened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical pofition; the thong will be found to become fhorter with heat and longer with cold. The proceffes of heating, cooling, and meafuring bodies are fo well known, that I need not enter into the minuter parts of the experiment; it will be proper, however, to add, that an increafe of temperature diminifhes the fpecific gravity of the Indian Rubber, and a lofs of heat occafions a contrary effect in it; as I have proved experimentally. The knowledge of the latter fact leads me to conclude, apparently on reafonable grounds, that the pores or interftices of Caoutchouc are enlarged by heat, and diminifhed by cold; confequently when a flip of this fubftance which remains extended by a weight, or the application of force, happens to contract from an acceffion of temperature, the capacity of its pores, taken feparately or collectively, is augmented by the change that takes place in the figure of the thong. Now if the exiftence of caloric be admitted, it will follow from the preceding arguments, that the phenomenon under confideration is occafioned by the alternate abforption and emiffion of the calorific fluid, in the fame manner that ropes, the blades of Fuci, as well as many more bodies, are obliged to contract and extend themfelves, by the alternate abforption and emiffion of water.—You will perceive by the tenour of the foregoing obfervations, that my theory of this cafe of elafticity is perfectly mechanical; in fact, the explanation of it depends upon the mutual attraction of Caloric and Caoutchouc; the former of which penetrates the latter, and pervades every part of it with the greateft eafe and expedition; by which the refin is compelled to accommodate its pores to that portion of the Calorific fluid which is due to its whole mafs, at

Caoutchouc when ftretched expands by heat and contracts by cold.

Theory. That this fubftance is affected by caloric, as ropes are by water;

at any particular degree of temperature. In order to apply the laſt remark to the phenomenon under conſideration I may obſerve, that if a force be exerted on a piece of Caoutchouc to alter the dimenſions of its pores, the mutual attraction mentioned above will reſiſt the effort. But the eaſe with which this ſubſtance may be made to change its figure, and the retractile power which it poſſeſſes on theſe occaſions, ſhew that its conſtituent particles move freely amongſt themſelves: but where there is motion, there is void ſpace; conſequently Caoutchouc abounds with innumerable pores or interſtices, the magnitudes of which are variable, becauſe the ſpecific gravity of the reſin becomes leſs with heat, and greater with cold. Now if the dimenſions of the pores in a piece of Caoutchouc can be leſſened, without taking away part of the matter of heat, which it contains at the time; this new arrangement in the internal ſtructure of the ſlip will leſſen its capacity for the matter of heat, and conſequently augment its temperature. But the warmth of ſuch a ſlip is increaſed by ſtretching it, according to the firſt experiment; the pores of it are therefore diminiſhed: and the effort, which it exerts at the time, ariſes from the mutual attraction of the Caoutchouc and Caloric; which attraction cauſes an endeavour to enlarge the interſtices of the former for the reception of the latter; hence it happens that the thong contracts longitudinally, according to the ſecond experiment, and the redundant caloric is abſorbed in the courſe of this operation, which again reduces the temperature. The preceding explanation agrees very well with the phenomenon, as it is ſtated in the beginning of this letter; and the theory receives additional confirmation from the following facts.

that its capacity may be mechanically altered and the caloric extruded, &c.

Exp. 3. If a thong of Caoutchouc be ſtretched in water warmer than itſelf, it retains its elaſticity unimpaired; on the contrary, if the experiment be made in water colder than itſelf, it loſes part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of ſpring, and the heat will immediately make it contract briſkly. The foregoing circumſtances may be conſidered as proving, that the elaſticity of Caoutchouc is not a conſtitutional quality of the ſubſtance, but a contingent effect, ariſing from the loſs of equilibrium between the portion of caloric, which the reſin

Overſtretched Caoutchouc does not completely recover itſelf in the cold; but heat reſtores its elaſticity:

whence the nature of its elaſticity is deduced, &c.

rèsin happens to contain at any moment, and its capacity to receive that fluid at the same instant. The object of the present letter is to demonstrate, that the faculty of this body to absorb the calorific principle, may be lessened, by forcibly diminishing the magnitudes of its pores; and this essential point of the theory may be confirmed by experiment: for the specific gravity of a slip of Caoutchouc is increased, by keeping it extended, while it is weighed in water.

JOHN GOUGH.

VIII.

Observations on the training of Pugilists, Wrestlers, Jockies, and others, who give themselves up to Athletic Exercises; with some Queries for discovering the Principles thereof, and the Process of training Running Horses, &c. with a View of ascertaining whether the same can furnish any Hints serviceable to the Human Species. *

PROFESSIONAL men are ready to acknowledge, that prevention is better than cure; and the best informed ingenuously admit, that organic diseases, once confirmed, are beyond the reach of their art. As organic diseases generally proceed from slow and gradual changes, they may certainly be prevented by temperance and labour; by activity of body, and contentment of mind. In regard to the common metaphysical expressions, "of the exhausting of the excitability; of the wearing of the parts; of the attrition of our fluids, in circulation, against the solids; of the abrasion of the solids by fric- General considerations on organic diseases.

* The subsequent queries and observations have been circulated by Sir John Sinclair, with a view to obtain information concerning the effects of diet and exercise on the human frame, from a class of practical experimentalists, whom the pride of science has hitherto overlooked. The philosophical manner in which this branch of diætatic medicine is here considered, appears to render it a fit object for insertion in a Journal conducted on the plan of the present. In promoting the circulation of this paper, we have no doubt that we are coinciding with the plan of the author, by extending his means of information: Any communications tending to throw further light on the subject, will be acceptable. W. N.

tion; of the debility produced by the moſt natural powers ſupporting life, namely, the waſte of ſubſtance created by that exerciſe and labour, for which we ſeem peculiarly deſtined,"—all theſe expreſſions are extremely ſuſpicious. The ſpeculator is always to be ſuſpected, when, forſaking plain direct facts, he involves his want of meaning, and conſcious ignorance, in learned words, or metaphor.

It is uſually ſuppoſed that bones, and other ſolid parts are permanent; but they are ſucceſſively replaced, like the fluids.

Theſe metaphorical expreſſions have originated in a perſuaſion, that the bones, cartilages, muſcles, and other ſolid parts, being once formed, *are permanent*, becauſe the identity of the individual is permanent; and that being once formed, and always retaining one ſhape, their actual component parts muſt continue the ſame. Nothing in philoſophy is farther from the truth. There are experiments to demonſtrate, that every part and particle of the firmeſt bones, is ſucceſſively abſorbed and depoſited again *. The ſolids of the body, whatever their form or texture, are inceſſantly renewed. The whole body is a perpetual ſecretion, and the bones and their ligaments, the muſcles and their tendons, all the finer and all the more flexible parts of the body, are as continually renewed, and as properly a ſecretion, as the ſaliva that flows from the mouth, or the moiſture that bedews the ſurface. The health of all the parts, and their ſoundneſs of ſtructure, depends on this perpetual abſorption, and perpetual renovation; and exerciſe, by promoting at once abſorption and ſecretion, promotes life, without hurrying it; renovates all the parts and organs, and preſerves them apt and fit for every office.

Nutrition is a general proceſs.

Nutrition belongs not to the ſtomach alone, which but prepares the food, and converts it into chyle, but to the veſſels by which it is circulated, and appropriated to the nutrition of parts, which of courſe is performed by every petty artery of the body.

Many general rules are raſh and dangerous.

In nothing ſhould we be more anxiouſly careful, than, in laying down rules, which muſt affect the health of thouſands; and whenever we proceed on doctrines, unſupported by fact,

* This has been aſcertained by giving madder to growing animals, eſpecially pigs and fowls, among their food. It is found that the madder tinges the bones, layer after layer, with a red colour; and by the deepneſs of the tinge, demonſtrates the ſucceſſion in which the particles of the bone are abſorbed and depoſited. This is, I believe, the concluſion which phyſiologiſts have formed.

wherever

wherever we divert mankind from thofe amufements and labours to which nature excites us, we fhould proceed with particular caution. We read in books, that life and the body are but as a given quantity of living energy and living materials, to be expended and ufed with difcretion and economy; and that the fum of excitability, which is born with the child, is expended towards the clofe of life. The doctrine of abrafion alfo intimates, that our folids are perpetually wafting, and that it is by the diminution of moifture,—the aridity of folids, the fcantinefs of fluids, and the flow induration of the folid parts; that the body becomes fhrunk, emaciated, ftiff, and motionlefs, before it finks into the grave. And, rafh as the doctrine feems, it has been boldly afferted, that "to live with as little food, and as little exercife as poffible, is the fureft means to preferve the body, and to live long." To live with as little food, and as little exercife as poffible, would make a man little better than a mere grafshopper. A man living thus, would be a voluntary prifoner, wan, colourlefs, flefhlefs, bloodlefs, having no fpeculation in his eyes, no marrow in his bones; his complexion would declare him what he was. This fyftem practifed, either in infancy, in the prime of manhood, or in the decline of life, would abridge it. Afcetics are a proof, not of the length of life, which temperance infures, but of the premature old age which abftinence brings upon us. The fqualid look, the hollow cheek, the matted hair, the emaciated body, only prove how much, by fuch criminal felf-denial, the body fuffers, with but little profit to the powers of the mind. Let us then take care that our philofophy be not too fevere; for men may run into real danger, if we take from them every fair indulgence, or divert them from following the dictates of nature. The faireft livers, who have not abufed, but have enjoyed their ftrength and health, have in general enjoyed them longeft.

The doctrine of abrafion or wearing out has been abfurdly applied.

There are habits which feem to be natural to, and congenial with, the feveral periods of life. The child fhould merely fuck, fleep, and vegetate. The boy fhould ramble wild and unconftrained, little oppreffed with tafks or ftudies, and nourifhed with abundance of fimple food. The youth fhould be temperate, fober, active. The old man quiet, fedate, felf-indulgent; fhould have long fleep, delicate food, rich wines, and agreeable temperature; little labour, and a cheerful mind.

Natural habits of infancy, youth, &c. &c.

Nature aſſigns us vigour, ſpirit, enterpriſe, and foreſight in the early part of life, to treaſure up the needful indulgences for age. Parents are careful of our firſt infancy; we ourſelves ought to provide for our latter childhood.

Conſiderations reſpecting the functions of the ſkin;—

The moſt intelligent profeſſional men have an opinion concerning the functions of the ſkin, conſonant with that of the vulgar; and more refined, only from their aſſigning a general cauſe for thoſe effects, of which all of us are conſcious. The ſkin is not regarded merely as an organ of ſecretion, deſtined for draining off ſuperfluous moiſture, or ſaline particles, from the general maſs of fluids, but as a ſurface of more active circulation, which ſolicits the blood to the very extremities of the veſſels, and thus contributes to ſupport and complete the circulation of the blood, and to nouriſh the parts within. The ſkin is regarded as connected, in a peculiar manner, with all the parts of the cellular ſubſtance, interpoſed betwixt the muſcles, and involving the blood veſſels. The ſtate of the ſkin indicates the condition of that cellular ſubſtance, whoſe office it is to conduct the blood-veſſels to all parts, eſpecially to the muſcular fleſh, and to nouriſh the parts; and while the circulation of the ſkin is lively and active, that of the involved parts can never flag. The condition of the bowels, and of the ſkin, are the firſt and moſt natural points for the phyſician to attend to. It is by regulating theſe, that he regulates the pulſe; by ſtimulating or ſoothing them, that he raiſes or depreſſes the vital actions; and it is matter of common obſervation, that in animals, a good ſkin is the criterion of health, and the dryneſs of the ſkin, the forming of ſcabs or eruptions upon it, and the clapping of the hair, (as it is called by thoſe who have the care of ſtock), are the firſt and ſureſt ſigns of approaching diſeaſe.

—and the inteſtines.

The lungs and their office.

Next to the free circulation of the blood through all the body, terminating in the ſurface, that of the free tranſit of the blood through the lungs, is eſſential to health.

The oxydation or chemical change produced by air upon the blood, is eſſential to its vital properties. A free and powerful reſpiration is moſt eſſential to a freſh colour of the face, to lively ſpirits, and cheerful feelings, and to the healthy and vigorous actions of the body. "It is my breathing hour of the day," ſays Hamlet to Oſric. It is a princely thing to ſet apart hours for exerciſes; and there is little doubt, that if all

thoſe

those, who linger away their hours in luxurious and indolent relaxations, were to assign a regular portion of their time to the hardy and manly exercises of walking, riding, fencing, &c. and would take *their breathing hour*, they would breathe long and well.

The art of training men to athletic exercises is wonderfully effective.

These reflections naturally arise upon considering the almost incredible perfection, to which those, whose profession it is to train men to athletic exercises, have brought their respective arts. By certain processes, they improve the breath, the strength, and the courage of those they take in hand, so as to enable them to run thirty, or walk a hundred miles, in a given space of time; to excel in wrestling; or to challenge a professed boxer. Would it not then be a most important addition to the facts we already know concerning the means of improving strength, and ensuring long life, if authentic information could be procured from those districts where athletic exercises prevail, what are esteemed the best and surest processes for training men for foot-races, trials of strength in wrestling or boxing matches, or for raising the strength and courage of game-cocks, or improving the wind, strength, and speed of running horses to their highest pitch.*

Some account of the methods,

Those who give themselves out as skilful in this art, attend to the state of the bowels, the skin, and the lungs. They use such means as reduce the cellular or fatty substance, and invigorate the muscular fibres. When they take a man in training for any feat of this kind, he is not oiled and suppled as the ancient athletics were; for as their common modes of life were hardy and active, they needed no other preparation: but he is sweated, purged, and dieted, and then put upon trial. He is purged with very drastic purges, to reduce his grossness. He is made to walk out under a load of clothes; his walks are regularly increased, and a certain number of times a-week; he is laid between two feather-beds; sweat promoted by drinks; his limbs taken from between the feather-beds, successively, and rubbed very roughly. After enduring for many

* Though not immediately connected with the object of this paper, it may not be improper to suggest, that it would be of great importance, if medical gentlemen, whether in the army or navy, who have been on service, were also to point out the various circumstances which tended to support, or to abate, the strength and courage of the soldier or the sailor.

hours

hours this ſtate of ſuffocation, he is comforted with a draught of ale or wine. The purges and ſweatings are repeated, according to the groſſneſs of his habit, and from time to time his trainer, (regarding him no otherwiſe than he would a running horſe, under the like diſcipline,) takes him out, and makes trial of his wind and ſtrength, and does not ceaſe till he has made him as lank as a greyhound, and almoſt as fleet.

and the great increaſe of force acquired by the human frame.

A man, even in the beſt of ordinary health, becomes giddy and breathleſs when he ſtrikes; and ſick and pale on receiving a few blows. He is thence unable to bear any unuſual exertion, and by inference prone to diſeaſe. If, by extenuating the fat, emptying in the cellular ſubſtance, hardening the muſcular fibres, and improving the breath, a man of the ordinary frame may be made to fight for one hour, with the utmoſt exertion of ſtrength and courage; the inquiry which I have already ſuggeſted muſt be of the higheſt uſe. For were this new train of facts regularly laid before profeſſional men, and were they enabled thus to judge of the influence which the methods of theſe practical philoſophers have on regulating the functions of breathing, perſpiration and digeſtion; it would be drawing into the province of ſcience, an art connected moſt particularly with the means of prolonging life, and hitherto known and practiſed only by a few inſulated individuals, of courſe imperfectly known, and of too limited uſe.

Theſe facts are probably of great value to the ſcience of prolonging life.

The art ſeems to be modern.

I queſtion whether the athletics of old uſed ſimilar means; whether they were equally ſucceſsful; whether there ever were, in any climate, age, or country, more hardy or powerful frames than thoſe of our Engliſh pugiliſts. In Cooke's voyage, we are told of the marked inferiority of the Engliſh ſailors, in wreſtling or boxing, to the naked ſun-burnt heroes of the South Sea Iſlands. But an Engliſh ſailor, though full of ſpirit and vigour, is as clumſy as a clown, and could not even row againſt an inhabitant of the Sandwich Iſlands. An Engliſh bricklayer, blackſmith, or drayman, however, who liked the ſport, and was practiſed in balancing and ſtriking, might have challenged the whole of the tawny nation.

Queries.

With a view of collecting ſuch important information, I am very anxious that the following queries ſhould be propoſed to thoſe who profeſs the art of training pugiliſts, wreſtlers, and runners of foot-races, by ſuch intelligent men as have the opportunity of converſing with them.

1. By

1. By what criterions or tefts, they judge of the mufcular ftrength, or wind, or other qualities of thofe who feek to put themfelves under training. What is the earlieft, and what is the lateft age they would attempt to train? Tefts of ftrength &c.?

2. How they judge of the length of time that may be required for bringing a man into good plight, vigorous health, and free breathing; and what period of preparation is ufually required for running a match? Time required to train?

3. What purges they ufe; and in what fucceffion; and by what rules do they adminifter them; and how do they judge of their effects? Is the purging only preparatory, or is it regularly continued? Is it meant, by this procefs to reduce the plethoric ftate of the fyftem, (on the idea that there is too great a quantity of blood,) or is it fimply defigned to put the bowels in the moft favourable condition, for eafy and good digeftion? Is the reducing the actual fize of the belly, neceffary to more free and perfect breathing*? Purges, treatment, their object, &c.?

4. Is the diet rich or fimple; of animal food, or of vegetable; in great quantity, or fparing; is it increafed gradually, or diminifhed gradually? What meals have they in the day; and at what hours; one or more; frequent feeding, in fmall and fixed portions, or full and fubftantial meals? What kinds of flefh or meat is reckoned the beft; whether beef, mutton, veal, pork, lamb, or fowl? Are any kinds of fifh allowed? What quality of food is moft conducive to ftrength? What quantity is neceffary for maintaining the fyftem in its moft perfect ftate of vigour? Do they feed much in the intermediate days of the purges? Is abftinence required when they take their phyfic? Diet?

5. What kinds of liquors are reckoned beft? Whether wine, ale, water, fpirits, &c.? Whether given hot or cold; in what quantities; and when ought they to be given? Liquors?

* The effects of taking up a running horfe from idlenefs and foft pafture, to hard food and regular exercife, is attended with this peculiar effect, that while the animal becomes lank, fleek, and gloffy, while he gets fire in his eye, and a new vigour in his limbs, and wind and fpeed, his belly, (fwollen with coarfe indigeftible food, eaten in great profufion,) is drawn into half its fize. May we not then prefume from this analogy, that the ftate of the belly has a remarkable effect upon the wind.

Intention of the perfpirations, how excited, &c.?

6. Are the very violent perfpirations into which they throw their patients, defigned to reduce the fyftem, to extenuate the fat, to leffen that quantity of blood, the excefs of which makes us giddy or fhort breathed; or is it merely defigned to produce a new condition of the fkin, more favourable to health and mufcular vigour; to produce a fharper appetite; a greater demand for food; and a quicker nourifhment, or a greater nutrition from a more flender diet? Is the fweat at firft produced by exercife, and only continued by the perfon, when trained, being put between feather beds, and encouraged by drinks; or is it produced by force of fweating drugs, or violent heats, or by continued friction? At what hours are the perfpirations brought on? How is the pupil treated when the fweat is over? What becomes of the fkin of a fat man, when, by the procefs, he is reduced in fize, and rendered lean? Does it hang loofe, or is it tight? Has it any effect upon the bones?

Exercife and treatment?

7. What hours of exercife do they require of their pupils during the day? At what hours do they fend them out in the morning? How long do they continue abroad? Are they loaded with clothes after the body is reduced, and becomes limber, and thin and mufcular; or only while the fweating procefs continues? Are they fed before they go abroad, or when they return? What trials are made of their ftrength? When is a man known to be up to his full ftrength and breath in training? At what hours do they go to bed? What fleep are they allowed? What indifpofitions are they fubject to during training? Are there any circumftances by which the procefs may be interrupted; or any circumftances, in confequence of which, it muft fometimes be abandoned?

Subfequent effects of training?

8. What is the ftate of the health, after they give up training? Are they fubject to any complaints; and what are they? How long does the acquired excefs of ftrength continue?

What part of training moft effectual? Whether it be permanent, temporary, curative, &c.

9. It is moft interefting to learn, on which part of this procefs, the purging, the fweating, the exercife, or the feeding, they moft depend; and whether it procures a permanent increafe of vigour, eafily maintained by fuitable diet and exercifes, or only a temporary excitement, calculated for the particular occafion? Alfo, whether perfons have ever thought of undergoing this procefs, not for the purpofe of running matches, but to recover health; with what fuccefs this

this has been done, and whether it is to be recommended for gout, corpulency, aſthma, nervous diſorder, or other maladies, as likely to be of ſervice?

Theſe are queſtions, of the importance of which, thoſe who are beſt able to anſwer, may not be fully aware. But nothing which ſo ſuddenly changes the powers, and the very form and character of the body, from groſs to lean, from weakneſs to vigorous health, from a breathleſs and bloated carcaſe, to one active and untiring, can ever be unimportant, either to the art of phyſic in general, or to that branch of it more immediately connected with inquiries regarding health and longevity. The art muſt be of importance.

The queries to be put regarding jockies, running-horſes, or game-cocks, may be to the following effect:

1. *Jockies.*

1. What is the proceſs uſed in training them, and reducing their weight? Queries reſpecting Jockies.

2. What effect has it upon their health and ſtrength?

3. What effect has it upon their mind, in regard to courage, quickneſs, &c.

4. How long do theſe effects continue?

5. After being reduced, do they quickly get fat again, or do they continue long in the ſtate to which they were brought?

6. Are jockies, accuſtomed to be thus treated, healthy and long lived?

2. *Running Horſes.*

1. What are the principal objects to be attended to in regard to running-horſes? Do their perfections depend upon parentage, and whether moſt upon the male or the female? Is it neceſſary that the mare ſhould have gone her full time, to bring a perfect foal? Is the gradual growth of the foal eſſential? Is there a great difference, in regard to natural conſtitution, between horſes of the ſame parentage? What kind of form is in general preferred? Do you prefer great or ſmall bones? Which ſex is preferable for ſpeed, and which for ſtrength? Running horſes?

2. What is the beſt age for beginning to train horſes for the turf? Are they firſt put upon graſs? What is the effect of ſoft

ſoft meat? When ſhould they be put on hard meat? What are the effects thereof? Is it neceſſary to purge them frequently? Have the purges any tendency to weaken them? What food is reckoned the moſt nouriſhing? How often are they fed? What drinks are given them, and how often? Whether hot or cold? Is it neceſſary to keep their ſkin perfectly clean, and how? Is it neceſſary to make them perſpire much? What exerciſe is given them? How is the training completed?

3. After the training is completed, can the perfections thereby obtained be eaſily kept up? Does the proceſs effect merely a temporary change, or does it laſt during life? Are running horſes as long lived as others, or do they ſoon wear out?

3. *Game-Cocks.*

Game-cocks. 1. Does the ſuperiority of game-cocks depend upon parentage? Which is of moſt importance, the male or the female? Is it of any conſequence that the cock ſhould arrive rather gradually at maturity? Is there a great difference, in point of ſtrength and conſtitution, in game cocks of the ſame parentage? Do you prefer great or ſmall bones?

2. When do you begin to feed the young cocks? What diet and drink do you give them, and what is the proceſs by which they are brought to the greateſt poſſible height of ſtrength and ſpirit?

3. When the game-cocks are thus trained, how long do the effects thereof laſt? Are they temporary or permanent? Do game-cocks thus trained live ſhorter or longer than others of the ſame ſpecies?

4. What drugs are given to fighting-cocks immediately before the main begins? Is it not uſual, by giving them ſaffron, (or ſome drug which has the ſame effect with opium, as uſed among the Janiſaries, or brandy among the French ſoldiery,) to excite an unnatural and ſhort-lived courage? What are the effects of ſuch drugs? and how do they manage the feeding up to this point, ſo as to take advantage of this momentary excitement?

IX. *On*

IX.

On the Dangers encountered in travelling over Downs, occasioned by Quicksands, which are frequently found on the Sea Coast; with an Indication of the Means of avoiding them. By M. BIEMONTIER, *Inspector-General of Bridges and Roads.**

Quickfands formed by bodies of fand transported by the wind into pools of water, where they form vaulted cavities till the hollow of the pool is filled up.

AFTER heavy and continued rains, there are formed at the edge of the fea-downs, fmall pools, or collections of water, frequently of feveral feet in depth. Strong winds diflodge portions of fand from the general mafs, and tranfport them to a diftance; which falling in fhowers on the clayey and fheltered furface of thefe pools, defcend gradually, and remain as it were in equilibrium in the midft of the water, fo as to form an infinity of little vaulted cavities. Thefe arches fuftain others, which are again furmounted in a fimilar manner, till at length the mafs rifes, fometimes to feveral feet above the level of the water; the furface becomes white and dry, and the fnare lies perfectly concealed. Whoever walks over this ftructure deftroys the whole, the arches give way, and the intruder is immerfed fometimes to his waift; but his alarm is ufually greater than the real danger; for if he were buried even up to the neck, he might eafily extricate himfelf, only by retaining fufficient prefence of mind not to ftruggle, but to move flowly and deliberately; want of attention to this might hazard his deftruction.

The furface is dry and fcarcely differs in appearance from the contiguous ground, but it gives way when trod upon.

Management to avoid danger.

When the equilibrium of the maffes of fand is deftroyed, they naturally fall into heaps, and it is only neceffary that time fhould be allowed for this to take place. When this has happened, the perfon immerfed fhould gently lift up one leg, and remain in that pofition till the fand has formed a fufficient bottom to fupport his raifed foot; the other leg fhould then be lifted up with the fame precautions; and thus fucceffively, till he rifes to the furface. In the mean time, the water which had been confined in the hollows of the fand will have alfo rifen, forming a pond (three or four inches deep) through which the adventurer may pafs in perfect fafety.

* Bibliotheque Phyfico-Economique, &c. de Sonini, November, 1805, page 186.

Cows,

Animals when immerfed ufe the fame method;

Cows, dogs, and other animals who frequent downs, and chance to fall into thefe quickfands, either through inftinct or experience, make ufe of this method to regain their freedom; provided, however, they be not too deeply immerfed to retain the free ufe of their fhoulder joints, otherwife they cannot be extricated without affiftance. I experienced this twice in one day; my horfe fank to above the breaft-leather, and although he was very ftrong, his efforts to extricate himfelf were unavailing, till we had removed fo much of the fand as impeded the action of thofe joints.

but they are feldom caught in quickfands.

It rarely happens that animals accuftomed to live on downs are caught in thefe fnares, which they are aware of, and know how to avoid.

Inftance.

I attempted, but in vain, to force another horfe with the whip and fpur into a quickfand; his owner, who acted as guide, affured me, that I fhould not fucceed, although there was no other indication of the fpot than a flat furface, flightly wrinkled. By thefe marks the traveller may generally detect the concealed pitfall; but he may always avoid them by tracing the footfteps of the cattle, when vifible, or by walking a few fathoms above the bottom of the declivity, or on the fummit of the down.

Another kind of quickfand,

Another kind of quickfand is fometimes met with on the fea-fhore, between high and low water mark, which it is proper fhould be here taken notice of. This is fometimes the effect of rain, but more commonly of the fea, when forced by wind and tempeft beyond its ufual limits, which being generally more elevated than the diftant land, the waters thus impelled forward are prevented from returning to their ancient bed; they therefore after forming in a body, drain away through the earth they have inundated, or brought with them, and form excavations beneath, large or fmall, deep or fhallow, according to circumftances.

formed by the waters drained through the earth.

I ought not to omit a fingular fact which paffed under my own obfervation, and which feems to prove, (as I have already ftated) that animals frequenting thefe plains, and living near the borders of the fea, employ combined means, acquired undoubtedly by experience, to extricate themfelves from thefe cavities, wherein they muft inevitably perifh, did they, as it appears natural they fhould, attempt to efcape by recoiling or by flight.

 Traverfing

Traverfing the plain of Arcachon, after a violent tempeft, which had been accompanied with heavy rains, we thought it prudent to get off our horfes and lead them by the bridle. One of the horfes who was left to himfelf, immediately quitted the company; and was retiring from the fhore, but being compelled to return by the application of the whip, he went upon the quickfand, which probably he had attempted to avoid by defertion; but the moment he felt the earth giving way, he crouched down, or rather threw himfelf precipitately on his fide. The ground quickly fank beneath and round about him; the water furmounted the fand; the horfe was only wetted to the crupper, and we efcaped with no other damage than the lofs of our ftock of bread, which being foaked in the falt water was rendered unfit to be eaten. Singular incident.

It may be received as matter of fact, that a man who fhould experience a fimilar misfortune, could not do better than to extend himfelf in the like manner, nearly in the attitude of a fwimmer, when he throws himfelf into the water. It is fcarcely neceffary to explain the fuperior advantage of this method; a plate of lead, of fome thoufands of weight, and feveral feet in breadth, if caft flat into any liquid body, would reach the bottom no quicker than the fluid could efcape to make way for it; if a fimilar body were to fall upon a quickfand, it would fhake every part of it, but would prevent the fand or earth from rifing, while the firm furrounding earth would confine it laterally: the ruins of the arched vaults would replace the waters which had been liberated from their fubterraneous confinement; folid heaps would then neceffarily be formed towards the centre, and the incumbent body would remain at the furface, or at leaft it would not be fwallowed up. Travellers when caught in this fnare fhould throw themfelves down.

Thefe quickfands are generally denoted by fmall ftreams, below which, when practicable, there is no danger in paffing. Quickfands are denoted by rills of water.

X. Extract

X.

Extract from a Memoir by Messrs. FOURCROY *and* VAUQUELIN, *on the Guano, or Natural Manure, of the small Islands of the South Sea, near the Coast of Peru. Read at the French National Institute, by* A. LAUGIER.*

M. Humboldt the first who gave an account of the Guano.

AMONG the multitude of subjects worthy the attention of the naturalist, which the philosophical Humboldt observed and collected during his travels, the Guano is not the least considerable, from the interest which it excites. This celebrated naturalist, by making us acquainted with this singular matter, one of the principal resources of agriculture in the countries he visited, has given confirmation to a discovery made by the authors of this memoir, about the time of his return. Reading their memoir on the existence of uric acid in the excrements of birds, it occurred to him that the Guano of the islets on the coast of Peru, which are frequented by great numbers of birds, might possibly be of the same nature. It remained for chemical investigation to examine how far this conjecture was well founded; and Messrs. Fourcroy and Vauquelin undertook the analysis of this matter. The following is the result of their labours, with this view, extracted from the Memoirs of the National Institute.

Memoir by Messrs. Fourcroy and Vauquelin on the excrements of birds, suggested the notion that the Guano was derived from the same origin.

Before I enter upon a detail of the experiments made upon Guano, in order to ascertain its nature, it may not be irrelevant to the subject to transcribe what M. Humboldt himself says of this substance in a note sent to the authors of this memoir.

Extract from M. Humboldt's note.

Guano found on certain small islands,

"The Guano is found in abundance in the South Sea, in the Chinche islands, near Pisco; and also on the more southern coasts and islets of Ilo, Iza, and Arica. The inhabitants of Chancay, who make Guano an object of their commerce, go to and return from the Chinche islands once in 20 days.—Each vessel contains from 1500 to 2000 cubic feet. A vanega sells at Chancay for 14 livres, and at Arica for 15 livres, Tournois.

—in beds 50 or 60 feet thick.

"Guano is dug from beds 50 or 60 feet thick; where it is worked like the bog-ore of iron. The islets where it is found

* Annales de Chimie, Vol. LVI. p. 258.

are

are frequented by a multitude of birds, particularly of the species of Ardea and Phœnicopterus, who rooſt there every night: but the excrements of theſe birds have hardly formed, in three centuries, a layer of four or five lines in depth. Is then the Guano the effect of ſome convulſion of the globe, like pit-coal and foſſil wood? The fertility of the naturally ſterile ſoil of Peru is derived from the Guano, which has become a material article of commerce. Fifty little veſſels, called *Guaneras*, are conſtantly employed in fetching this manure, for the ſupply of the coaſt. Its effluvium may be ſmelled at the diſtance of a quarter of a league. The ſailors accuſtomed to this ſmell of ammonia, feel no inconvenience from it; but we could not approach it without being affected with continued fits of ſneezing.

The place frequented by vaſt numbers of birds.

Sterile ſoil of Peru made fruitful by the Guano.

Veſſels employed to collect it.

It has a ſtrong odour of ammonia.

"Maize is the particular vegetable for which Guano forms an excellent manure. The Spaniards learned its uſe of the Indians: If too much be thrown upon the maize, the root is burned and deſtroyed. Guano is too acidifiable, and is therefore a manure containing hydruret of azote; whilſt all other manures are rather hydrurets of carbon."

Maize particularly benefited by Guano as a manure.

Guano is of a dirty yellow colour, rather inſipid to the taſte, but poſſeſſing a powerful odour, partaking of caſtor and of valerian. It turns black in the fire, and exhales a white ſmoke of an ammoniacal ſmell.

Its appearance.

Its ſolubility in water, particularly with potaſh, determined the operators as to the method they ſhould purſue in its analyſis. They treated it ſucceſſively with water, with potaſh, and with muriatic acid; each of which methods preſented many phenomena, as related in the following part of this paper, diveſted of the particular details of proceſs, which are too extenſive for an extract.

Partly ſoluble in water.

Ten grammes of this matter, after being repeatedly waſhed with large quantities of boiling water, were reduced to $5\frac{7}{10}$ grammes. The water had obtained a red colour, which it communicated to paper ſtained with turnſole.

The ſolution is acid.

In diſtillation, the water yielded ammonia during the whole operation. Twenty-four hours afterwards, it had depoſited a dirty yellow powder, poſſeſſing very little flavour, but with an odour of caſtor: On the ſurface was a cryſtalline pellicle, of the ſame colour with the depoſition.

The water yielded ammonia by diſtillation, and depoſited a yellow powder with a ſmell of caſtor.

The

The liquor, filtered and again evaporated, till reduced to 3 grammes, on cooling again, depofited a fawn-coloured powder, fimilar to the former, but in lefs quantity.

The powder, and the mother-water, which had held it in folution, were feparately examined.

Examination of the powder.

The powder offered the following properties:—It is a concrete and pulverulent fubftance, of a brilliant cryftalline afpect, and of a dull yellow colour. Before the blow-pipe it is confumed entirely away, yielding a flight empyreumatic odour of ammonia and pruffic acid. It is very little foluble in cold water; but abundantly fo in warm water, to which it communicates its yellowifh colour. This folution, though taftelefs, ftrongly reddens the tincture of turnfole, precipitates folutions of acetate of lead, and of nitrate of filver and mercury, in coloured flakes, which are readily and completely rediffolved by nitric acid.

This matter inftantaneoufly diffolves in an alkaline ley, which it tinges of a deep brown colour, exhaling a pungent fmell of ammonia. Sulphuric acid poured into the concentrated alkaline folution, throws down a very thick whitifh precipitate, and difengages a brifk odour, refembling that of weak acetic acid.

It is an acidulous falt, compofed of animal acid, ammonia, and lime.

The learned authors of this memoir conclude from their experiments, that this powder is an acidulous falt, compofed of animal acid, ammonia, and a little lime. In fact, very weak nitric acid, wherein this falt had been macerated in order to difengage the acid it contained, from its bafes, yielded, on evaporation, copious ammoniacal vapours, by the addition of potafh, and unequivocal figns of the prefence of lime, by the addition of oxalic acid.

Analyfis of the powder when deprived of its ammonia and lime.

When thus deprived of its ammonia and lime, this matter is lefs coloured and lefs foluble than before. Its folution in boiling water depofits pretty hard and brilliant cryftals, and more deeply reddens turnfole paper. It combines readily, and without any ammoniacal vapour, with potafh, from which all the acids again feparate it. Heat turns it black; and it burns, without leaving any refiduum, with an odour of ammonia and of pruffic acid. A neutral combination of it with ammonia will not precipitate the folution of fulphate of alumine, as is done by homftic acid.

From

From thefe facts it appears evident, 1, that the matter taken up by the boiling water from Guano is an acid, partly faturated with ammonia and a little lime; 2, that this acid is an animal product, becaufe it yields ammonia and pruffic acid, when decompofed by fire; 3, that the fame acid, according to all the known properties, muft be uric acid, fimilar to that contained in the excrements of aquatic birds; 4, that it forms about one fourth part of the Guano.

The acid of Guano is uric acid, forming about $\frac{1}{4}$ of the whole.

The mother-water which depofited the powder, whofe qualities have been juft examined, is very acid; potafh caufes a copious difengagement of ammonia: It contains, therefore, an ammoniacal falt. Nitrate of barytes and of filver announce the prefence of muriatic and fulphuric falts; which are precipitated in white flakes by lime-water, and are re-diffolved, though with difficulty, in muriatic acid.

Analyfis of the mother-water which depofited the powder. It contains an ammoniacal falt,

This precipitate caufed by lime water, is evidently formed of two falts, both foluble in acids without effervefcence; one eafily, and without the affiftance of heat, the other with difficulty, even with the aid of heat; the former refifts calcination, the latter is decompofed by fire, and afterwards diffolves in acids with effervefcence. The firft is phofphate of lime, the fecond oxalate of lime.

—and phofphate and oxalate of lime.

Meffrs. Fourcroy and Vauquelin wifhed to feparate thefe two falts, without their undergoing any alteration; and with this view they made ufe of weak nitric acid, which diffolved the phofphate of lime, and left the oxalate untouched. The latter falt, on being treated with a folution of carbonate of potafh, yielded a precipitate that diffolved with effervefcence in nitric acid: This folution difplayed all the properties of nitrate of lime. The acid feparated from the lime was taken up by the potafh: in fact, the liquor poffeffed the characters of oxalate of potafh; it precipitated with lime-water, a very divided powder, with fulphate of lime, in flakes, which would not readily unite; and with all the metallic folutions capable of precipitation by oxalic acid. Sulphate of alumine caufed no precipitate, as it would have done with honiftate of potafh.

The potafh found in the mother-water, after its precipitation by lime-water, and the difengagement of ammonia, caufed by the addition of potafh to the mother-water, prior to its decompofition by lime-water, fufficiently fhew that thefe two alkalis faturate the acids contained in the mother-water of Guano;

The mother-water contains oxalates, phofphates, fulphates, and muriates of potafh, and of ammonia.

Guano; and that the mother-water certainly contains oxalates phofphates, fulphates, and muriates of potafh, and of ammonia.

The Guano left from the firft wafhing, —contains uric acid.

The five grammes and feven-tenths, left after the wafhing of the ten grammes originally taken for analyfis, were treated with cauftic potafh, which took up eight-tenths. This alkaline folution contained only uric acid, and a fmall portion of fat matter.

Phofphate of lime, iron, and carbonate of lime;

The 4.9 grammes left by the cauftic potafh, were treated with muriatic acid: the product was phofphate of lime, iron, and an atom of carbonate of lime.

—and left quartzofe and ferruginous fand.

After thefe applications of water, of cauftic potafh, and of muriatic acid, there remained of the 10 grammes of Guano, only 3.1 grammes of matter, compofed of quartzofe and ferruginous fands.

Recapitulation of component parts.

From the foregoing interefting analyfis, it appears that the manure of the iflets of the South Sea is formed of,—

1. Uric acid to the amount of $\frac{1}{4}$ of the whole compound; partly faturated with ammonia and lime:

2. Oxalic acid, partly faturated with ammonia and potafh:

3. Phofphoric acid, combined with the fame bafes and with lime:

4. Small quantities of fulphate and muriates of potafh and ammonia:

5. A fmall portion of fat matter:

6. Sand, partly quartzofe and partly ferruginous.

Remarks.

The exiftence of Guano in places frequented by vaft numbers of birds, and the identity of its nature with that of the excrements of aquatic birds, neceffarily throw confiderable light on the origin of this matter.

The analyfis proves how well founded was the ingenious comparifon of the learned naturalift, to whom we are indebted for our knowledge of this fubftance, no lefs interefting to us than ufeful to the inhabitants of Peru. It confirms the important difcovery made by the refearches of Meffrs. Fourcroy and Vauquelin. In a word, this analyfis poffeffes the advantage of proving a well-known maxim, that the fciences mutually enrich and enlighten each other with the light they poffefs; and it affords a new occafion to remark that among the fciences, there are perhaps none which have fo immediate and fo neceffary a connection as Chemiftry and Natural Hiftory.

XI. *No.*

XI.

Note on a Varniſh for glazing Cups. By M. Parmentier *.

M. BOMPOIX, chief apothecary to the French Military Hoſpital at Genoa, having ſent me ſome coffee-cups of a remarkable lightneſs, and glazed with a varniſh which is held in great repute, perhaps only becauſe its preparation is kept a ſecret in that country; I requeſted him to uſe his utmoſt endeavours to procure me the receipt. He obtained it through the medium of one of his pupils, who learned the ſecret from the artificer at the manufactory, and had made from his preſcription a varniſh in every reſpect equal to that in queſtion: Account of very light coffee-cups finely varniſhed.

It conſiſted of lintſeed oil 1 ½ lbs.; amber 1lb.; litharge in powder, minium in powder, ceruſe in powder, each, 5 oz. Receipt for the varniſh.

Boil the lintſeed oil in an unglazed earthen veſſel, and tie the litharge, minium, and ceruſe in a linen bag, which is to be ſuſpended in the oil whilſt boiling, ſo that it may not touch the bottom of the veſſel. When the oil begins to turn brown, take out the bag, and put in a clove of garlic, cleared of the ſkin; continue the boiling; and when the garlic is dried away, put in another and another, to the amount of ſix or ſeven. In the mean time, the amber is to be melted in another unglazed veſſel, according to the method hereafter preſcribed; and when the oil has been ſufficiently boiled, the fuſed amber is to be poured into it.

To melt the Amber.

Take two ounces of lintſeed oil, to ſoften the amber and to aſſiſt its fuſion by a very briſk fire, and when the amber is melted, add the lintſeed oil, and boil the whole about two minutes. The fluid muſt then be ſtrained through a coarſe cloth, and when cold put into a bottle well corked, to prevent it from drying. Fuſion of the amber.

Method of uſing the Varniſh.

Let the piece intended to be varniſhed be firſt well poliſhed, and then apply the varniſh in the following manner: Application of the varniſh.

* Annales de Chimie, Vol. LVI. p. 254.

 Mix

Mix lamp black with varnifh and a little turpentine, with a hair pencil, and lay one coat on the piece; when this is dry, lay on another, and repeat the procefs till four coats have been laid on, taking care to let each dry before the application of the next. When the laft is dry, put the piece into a ftove or oven to complete the drying, and then polifh it with pumice and Tripoli powder.

Method of preparing the Piece intended to be varnifhed.

Manner of making the wooden cups.

Make the cups of hazel, alder, or cherry-tree, which are preferable to other woods for this ufe, becaufe they are porous when perfectly dry, and do not warp. Form them according to fancy, and dry them in an oven. The work muft be polifhed as if it were complete; and afterwards lay on the varnifh as already prefcribed.

Red varnifh.

If it fhould be wifhed to give a red ground to the article, mix a little minium, or rather cinabar, with the varnifh. Any other colour may in like manner be mixed with it, as may beft pleafe the fancy of the operator.

XII.

Account of a Series of Experiments, fhewing the Effects of Compreffion in modifying the Action of Heat. By* Sir James Hall, *Bart. F. R. S. Edinburgh.*

Section I.

Ancient Revolutions of the Mineral Kingdom.—Vain Attempts to explain them.—Dependance of Geology on Chemiftry.—Importance of the Carbonate of Lime.—Dr Black's *Difcovery of Carbonic Acid fubverted the former theories depending on Fire, but gave Birth to that of Dr.* Hutton.—*Progrefs of the Author's Ideas with Regard to that Theory.—Experiments with Heat and Compreffion, fuggefted to Dr.* Hutton *in* 1790.—*Undertaken by the Author in* 1798.—*Speculations on which his Hopes of Succefs were founded.*

Violent revolutions of the furface of the globe.

WHOEVER has attended to the ftructure of rocks and mountains, muft be convinced, that our globe has not always exifted

* The highly interefting experiments of Sir James Hall upon the effects of heat modified by compreffion, were communicated to the

existed in its present state; but that every part of its mass, so far at least as our observations reach, has been agitated and subverted by the most violent revolutions.

Geological systems imperfect.

Facts leading to such striking conclusions, however imperfectly observed, could not fail to awaken curiosity, and give rise to a desire of tracing the history, and of investigating the causes, of such stupendous events; and various attempts were made in this way, but with little success; for while discoveries of the utmost importance and accuracy were made in astronomy and natural philosophy, the systems produced by the Geologists were so fanciful and puerile, as scarcely to deserve a serious refutation.

because chemical knowledge was in its infancy.

One principal cause of this failure seems to have lain in the very imperfect state of chemistry, which has only of late years begun to deserve the name of a science. While chemistry was in its infancy, it was impossible that geology should make any progress; since several of the most important circumstances to be accounted for by this latter science, are admitted on all hands to depend upon principles of the former. The consolidation of loose sand into strata of solid rock; the crystalline arrangement of substances accompanying those strata, and blended with them in various modes, are circumstances of a

the Royal Society of Edinburgh in August 1804, and were transmitted to our Journal by the author in the following month. They appear in Vol. IX. page 98. That concise narrative could not but strongly excite the curiosity of philosophers and geologists, and direct their earnest expectations to a fuller detail. In the last session, June 3, 1805, a very ample communication was made, which has been printed with five quarto plates, very beautifully engraved by Lizars, from designs by Sir James. I cannot but consider it as one of those high marks of approbation, with which the Philosophical Journal has been honoured from time to time, that the author has again directed his attention to this periodical work, as the vehicle through which his discoveries should be more extensively circulated. With this view he has not only favoured me with the memoir as soon as completed, but has liberally taken upon himself the expence of engraving the plates for the Journal in the same superior style. By this means the numbers containing his memoir will be enriched with ten additional plates besides those usually given:—for I shall with great satisfaction follow the steps of the worthy baronet by presenting the additional expences of paper and print to the reader without charge. W. N.

chemical

chemical nature, which all thofe who have attempted to frame theories of the earth have endeavoured by chemical reafonings to reconcile to their hypothefes.

Fire and water adduced as the agents in two theories.

Fire and *water*, the only agents in nature by which ftony fubftances are produced, under our obfervation, were employed by contending fects of geologifts, to explain all the phenomena of the mineral kingdom.

Water has little agency on minerals.

But the known properties of water are quite repugnant to the belief of its univerfal influence, fince a very great proportion of the fubftances under confideration are infoluble, or nearly fo, in that fluid; and fince, if they were all extremely foluble, the quantity of water which is known to exift, or that could poffibly exift in our planet, would be far too fmall to accomplifh the office affigned to it in the Neptunian theory*. On the other hand, the known properties of fire are no lefs inadequate to the purpofe; for, various fubftances which frequently occur in the mineral kingdom, feem, by their prefence, to preclude its fuppofed agency; fince experiment fhews, that, in our fires, they are totally changed or deftroyed.

Common fire does not explain the facts.

Hence both theories were doubtful.

Under fuch circumftances, the advocates of either element were enabled, very fuccefsfully, to refute the opinions of their adverfaries, though they could but feebly defend their own: and, owing, perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both fyftems maintained a certain degree of credit; and writers on geology indulged themfelves, with a fort of impunity, in a ftyle of unphilofophical reafoning, which would not have been tolerated in other fciences.

Carbonate of lime is of extenfive importance,

Of all mineral fubftances, the *carbonate of lime* is unqueftionably the moft important in a general view. As limeftone or marble, it conftitutes a very confiderable part of the folid mafs of many countries; and, in the form of veins and nodules of fpar, pervades every fpecies of ftone. Its hiftory is thus interwoven in fuch a manner with that of the mineral kingdom at large, that the fate of any geological theory muft very much depend upon its fuccefsful application to the various conditions of this fubftance. But, till Dr. Black, by his

* *Illuftrations of the Huttonian Theory*, by Mr. Profeffor Playfair, § 430.

2 difcovery

discovery of carbonic acid, explained the chemical nature of the carbonate, no rational theory could be formed, of the chemical revolutions which it has undoubtedly undergone.

This discovery was, in the first instance, hostile to the supposed action of fire; for the decomposition of limestone by fire in every common kiln being thus proved, it seemed absurd to ascribe to that same agent the formation of limestone, or of any mass containing it.

seems not producible by heat.

The contemplation of this difficulty led Dr. Hutton to view the action of fire in a manner peculiar to himself, and thus to form a geological theory, by which, in my opinion, he has furnished the world with the true solution of one of the most interesting problems that has ever engaged the attention of men of science.

Dr. Hutton's theory.

He supposed,

I. That heat has acted, at some remote period, on all rocks.

II. That during the action of heat, all these rocks (even such as now appear at the surface) lay covered by a superincumbent mass, of great weight and strength.

That rocks have undergone heat under strong pressure.

III. That in consequence of the combined action of heat and pressure, effects were produced different from those of heat on common occasions; in particular, that the carbonate of lime was reduced to a state of fusion, more or less complete, without any calcination.

The essential and characteristic principle of his theory is thus comprised in the word *compression;* and by one bold hypothesis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for those circumstances in which minerals are found to differ from the usual products of our furnaces.

This system, however, involves so many suppositions, apparently in contradiction to common experience, which meet us on the very threshold, that most men have hitherto been deterred from the investigation of its principles, and only a few individuals have justly appreciated its merits. It was long before I belonged to the latter class; for I must own, that, on reading Dr. Hutton's first geological publication, I was induced to reject his system entirely, and should probably have continued still to do so, with the great majority of the world, but for my habits of intimacy with the author; the vivacity and

Singular contrast of the perspicuity of Dr. Hutton's conversation, and the obscurity of his writings.

and perſpicuity of whoſe converſation formed a ſtriking contraſt to the obſcurity of his writings. I was induced by that charm, and by the numerous original facts which his ſyſtem had led him to obſerve, to liſten to his arguments, in favour of opinions which I then looked upon as viſionary. I thus derived from his converſation the ſame advantage which the world has lately done from the publication of Mr. Playfair's *Illuſtrations;* and, experienced the ſame influence which is now exerted by that work, on the minds of our moſt eminent men of ſcience.

The author's progreſs in the Doctor's theory.

After three years of almoſt daily warfare with Dr. Hutton, on the ſubject of his theory, I began to view his fundamental principles with leſs and leſs repugnance. There is a period, I believe, in all ſcientific inveſtigations, when the conjectures of genius ceaſe to appear extravagant; and when we balance the fertility of a principle, in explaining the phenomena of nature, againſt its improbability as an hypotheſis: The partial view which we then obtain of truth, is perhaps the moſt attractive of any, and moſt powerfully ſtimulates the exertions of an active mind. The miſt which obſcured ſome objects diſſipates by degrees, and allows them to appear in their true colours; at the ſame time, a diſtant proſpect opens to our view, of ſcenes unſuſpected before.

He propoſes experimental confirmation,

Entering now ſeriouſly into the train of reaſoning followed by Dr. Hutton, I conceived that the chemical effects aſcribed by him to compreſſion, ought, in the firſt place, to be inveſtigated; for, unleſs ſome good reaſon were given us for believing that heat would be modified by preſſure, in the manner alledged, it would avail us little to know that they had acted together. He reſted his belief of this influence on analogy; and on the ſatisfactory ſolution of all the phenomena furniſhed by this ſuppoſition. It occurred to me, however, that this principle was ſuſceptible of being eſtabliſhed in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this propoſal, on account of the immenſity of the natural agents, whoſe operations he ſuppoſed to lie far beyond the reach of our imitation; and he ſeemed to imagine, that any ſuch attempt muſt undoubtedly fail, and thus throw diſcredit on opinions already ſufficiently eſtabliſhed, as he conceived, on other principles. I was far, however,

rejected by the Doctor.

from

from being convinced by these arguments; for, without being able to prove that any artificial compression to which we could expose the carbonate, would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might possibly perform all that was hypothetically assumed in the Huttonian theory. On the other hand, I considered myself as bound, in practice, to pay deference to his opinion, in a field which he had already so nobly occupied; and abstained, during the remainder of his life, from the prosecution of some experiments with compression, which I had begun in 1790.

Experimental investigation undertaken.

In 1798, I resumed the subject with eagerness, being still of opinion that the chemical law which forms the basis of the Huttonian theory, ought, in the first place, to be investigated experimentally; all my subsequent reflections and observations having tended to confirm my idea of the importance of this pursuit, without in any degree rendering me more apprehensive as to the result.

Order of the present treatise.

In the arrangement of the following paper, I shall first confine myself to the investigation of the chemical effects of heat and compression, reserving to the concluding part the application of my results to Geology. I shall then appeal to the volcanoes, and shall endeavour to vindicate the laws of action assumed in the Huttonian theory, by shewing, that lavas, previous to their eruptions, are subject to similar laws; and that the volcanoes, by their subterranean and submarine exertions, must produce, in our times, results similar to those ascribed, in that theory, to the former action of fire.

In comparing the Huttonian operations with those of the volcanoes, I shall avail myself of some facts, brought to light in the course of the following investigations, by which a precise limit is assigned to the intensity of the heat, and to the force of compression, required to fulfil the conditions of Dr. Hutton's hypothesis: For, according to him, the power of those agents was very great, but quite indefinite; it was therefore impossible to compare their supposed effects in any precise manner with the phenomena of nature.

Argument respecting carbonate of lime.

My attention was almost exclusively confined to the carbonate of lime, about which I reasoned as follows: The carbonic acid, when uncombined with any other substance, exists naturally in a gaſeous form, at the common temperature of our atmoſphere; but when in union with lime, its volatility is repreſſed, in that ſame temperature, by the chemical force of the earthy ſubſtance, which retains it in a ſolid form. When the temperature is raiſed to a full red-heat, the acid acquires a volatility by which that force is overcome, it eſcapes from the lime, and aſſumes its gaſeous form. It is evident, that were the attractive force of the lime increaſed, or the volatility of the acid diminiſhed by any means, the compound would be enabled to bear a higher heat without decompoſition, than it can in the preſent ſtate of things. Now, preſſure muſt produce an effect of this kind; for when a mechanical force oppoſes the expanſion of the acid, its volatility muſt, to a certain degree, be diminiſhed. Under preſſure, then, the carbonate may be expected to remain unchanged in a heat, by which, in the open air, it would have been calcined. But experiment alone can teach us what compreſſing force is requiſite to enable it to reſiſt any given elevation of temperature; and what is to be the reſult of ſuch an operation. Some of the compounds of lime with acids are fuſible, others refractory; the carbonate, when conſtrained by preſſure to endure a proper heat, may be as fuſible as the muriate.

Preſſure muſt oppoſe the expanſion and eſcape of the acid, and enable it to ſupport a ſtronger heat.

One circumſtance, derived from the Huttonian Theory, induced me to hope, that the carbonate was eaſily fuſible, and indicated a preciſe point, under which that fuſion ought to be expected. Nothing is more common than to meet with nodules of calcareous ſpar incloſed in whinſtone; and we ſuppoſe, according to the Huttonian theory, that the whin and the ſpar had been liquid together; the two fluids keeping ſeparate, like oil and water. It is natural, at the junction of theſe two, to look for indications of their relative fuſibilities; and we find, accordingly, that the termination of the ſpar is generally globular and ſmooth; which ſeems to prove, that, when the whin became ſolid, the ſpar was ſtill in a liquid ſtate; for had the ſpar congealed firſt, the tendency which it ſhews, on all occaſions of freedom, to ſhoot out into prominent cryſtals, would have made it dart into the liquid whin, according to the peculiar forms of its cryſtallization; as has happened with the various ſubſtances contained in whin, much more refractory than itſelf, namely, augite, felſpar, &c.; all of which having congealed in the liquid whin, have aſſumed their peculiar forms

Probability that the carbonate might not be of difficult fuſion.

Facts which indicate its melting heat.

forms with perfect regularity. From this I concluded, that when the whin congealed, which must have happened about 28° or 30° of Wedgwood, the spar was still liquid. I therefore expected, if I could compel the carbonate to bear a heat of 28° without decomposition, that it would enter into fusion. The sequel will shew that this conjecture was not without foundation.

The experiments introduced.

I shall now enter upon the description of those experiments, the result of which I had the honour to lay before this Society on the 30th of August last (1804); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various particulars, to steer between the opposite faults of prolixity and barrenness. My object shall be to describe, as shortly as possible, all the methods followed, so as to enable any chemist to repeat the experiments; and to dwell particularly on such circumstances only as seem to lead to conclusions of importance.

The result being already known, I consider the account I am about to give of the execution of these experiments, as addressed to those who take a particular interest in the progress of chemical operations: in the eyes of such gentlemen, I trust, that none of the details into which I must enter, will appear superfluous.

Section II.

Principle of Execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the Muzzle.—Method with the fusible Metal.—Remarkable Effects of its Expansion.—Necessity of introducing Air.—Results obtained.

The author's early contrivances for confining elastic substances at high temperatures.

When I first undertook to make experiments with heat acting under compression, I employed myself in contriving various devices of screws, of bolts, and of lids, so adjusted, I hoped, as to confine all elastic substances; and perhaps some of them might have answered. But I laid aside all such devices, in favour of one which occurred to me in January 1798; which, by its simplicity, was of easy application in all cases, and accomplished all that could be done by any device, since it secured perfect strength and tightness to the utmost that the vessels employed could bear, whether formed of metallic or earthy substance. The device depends upon the following general view; If we take a hollow tube or barrel (AD *Pl.* ix. fig.

The method adopted ultimately was to include the ſubject in an iron barrel, and cloſe the aperture by fuſion.

fig. 1.)* cloſed at one end, and open at the other, of one foot or more in length; it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if neceſſary, expoſed to extreme cold. If, then, the ſubſtance which we mean to ſubject to the combined action of heat and preſſure be introduced into the breech or cloſed end of the barrel (CD), and if the middle part be filled with ſome refractory ſubſtance, leaving a ſmall empty ſpace at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the ſubject of experiment, is kept cool, and thus cloſe the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of ſealing-wax. Things being then reverſed, and the breech put into the furnace, a heat of any required intenſity may be applied to the ſubject of experiment, now in a ſtate of conſtraint.

First experiment with the muzzle plugged and welded,

My firſt application of this ſcheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very ſtrongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previouſly rammed into a cartridge of paper or paſteboard, in order to protect it from the iron, by which, in ſome former trials, the ſubject of experiment had been contaminated throughout during the action of heat. I then rammed the reſt of the barrel full of pounded clay previouſly baked in a ſtrong heat, and I had the muzzle cloſed like the breech, by a plug of iron welded upon it in a common forge; the reſt of the barrel being kept cold during this operation, by means of wet cloths. The breech of the barrel was then put horizontally into a common muffle, heated to about 25° of Wedgwood. To the muzzle a rope was fixed, in ſuch a manner, that the barrel could be withdrawn without danger from an exploſion *. I likewiſe, about this time cloſed the muzzle of

in another inſtance ſoldered.

* This plate will be given in No. 54, being the ſupplement to the preſent volume.

† On one occaſion, the importance of this precaution was ſtrongly felt. Having inadvertently introduced a conſiderable quantity of moiſture into a welded barrel, an exploſion took place, before the heat had riſen to redneſs, by which, part of the barrel was ſpread out to a flat plate, and the furnace was blown to pieces. Dr. Kennedy, who happened to be preſent on this occaſion, obſerved, that notwithſtanding this accident, the time might come when we ſhould employ water in theſe experiments to aſſiſt the force

of the barrel, by means of a plug, fixed by folder only; which method had this peculiar advantage, that I could fhut and open the barrel without having recourfe to a workman. In thefe trials, though many barrels yielded to the expanfive force, others refifted it, and afforded fome refults that were in the higheft degree encouraging, and even fatisfactory, could they have been obtained with certainty on repetition of the procefs. In many of them, chalk, or common limeftone previoufly pulverifed, was agglutinated into a ftony mafs, which required a fmart blow of a hammer to break it, and felt under the knife like a common limeftone; at the fame time, the fubftance, when thrown into nitric acid, diffolved entirely with violent effervefcence. Satisfactory refults.

In one of thefe experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been ufed to fill the barrel, was ftained black throughout, to the diftance of two-thirds of the length of the barrel from its breech. This circumftance is of importance, by fhewing, that though all is tight at the muzzle, a protrufion may take place along the barrel, greatly to the detriment of complete compreffion: and, at the fame time, it illuftrates what has happened occafionally in nature, where the bituminous matter feems to have been driven by fuperior local heat, from one part of a coaly bed, though retained in others, under the fame compreffion. The bitumen fo driven off being found, in other cafes, to pervade and tinge beds of flate and of fandftone. Volatile matter may be driven from one to another part of a clofed barrel.

I was employed in this purfuit in fpring 1800, when an event of importance interrupted my experiments for about a year. But I refumed them in March 1801, with many new plans of execution, and with confiderable addition to my apparatus.

In the courfe of my firft trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemifts, that a certain compofition of Experiments in which the fufible metal was ufed as the plug.

force of compreffion. I have fince made great ufe of this valuable fuggeftion: but he fcarcely lived, alas! to fee its application; for my firft fuccefs in this way took place during his laft illnefs.—I have been expofed to no rifk in any other experiment with iron barrels; matters being fo arranged, that the ftrain againft them has only commenced in a red heat, in which the metal has been fo far foftened, as to yield by laceration like a piece of leather.

different

different metals*, produces a ſubſtance ſo fuſible, as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and diſpatch, might be gained in theſe experiments, by ſubſtituting this metal for the baked clay above mentioned: That after introducing the carbonate into the breech of the barrel, the fuſible metal, in a liquid ſtate, might be poured in, ſo as to fill the barrel to its brim: That when the metal had cooled and become ſolid, the breech might, as before, be introduced into a muffle, and expoſed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fuſible metal being melted but what lay at the breech, the reſt, continuing in a ſolid ſtate, would effectually confine the carbonic acid: That after the action of ſtrong heat had ceaſed, and after all had been allowed to cool completely, the fuſible metal might be removed entirely from the barrel, by means of a heat little above that of boiling water, and far too low to occaſion any decompoſition of the carbonate by calcination, though acting upon it in freedom; and then, that the ſubject of experiment might, as before, be taken out of the barrel.

Advantages of this method.

This ſcheme, with various modifications and additions, which practice has ſuggeſted, forms the baſis of moſt of the following methods.

A ſtriking phenomenon. When the barrel was completely filled with fuſible metal only, and the cloſed end of the iron expoſed to heat in a muffle, the greater expanſion of the fluid forced it through the texture of the iron in very fine wire reſembling wool.

In the firſt trial, a ſtriking phenomenon occurred, which gave riſe to the moſt important of theſe modifications. Having filled a gun-barrel with the fuſible metal, without any carbonate; and having placed the breech in a muffle, I was ſurpriſed to ſee, as the heat approached to redneſs, the liquid metal exuding through the iron in innumerable minute drops, diſperſed all round the barrel. As the heat advanced, this exudation increaſed, till at laſt the metal flowed out in continued ſtreams, and the barrel was quite deſtroyed. On ſeveral occaſions of the ſame kind, the fuſible metal, being forced through ſome very minute aperture in the barrel, ſpouted from it to the diſtance of ſeveral yards, depoſiting upon any ſubſtance oppoſed to the ſtream, a beautiful aſſemblage of fine wire, exactly in the form of wool. I immediately underſtood that the phenomenon was produced by the ſuperior expanſion of the liquid over the ſolid metal, in con-

* Eight parts of biſmuth, five of lead, and three of tin.

ſequence

ſequence of which, the fuſible metal was driven through the iron as water was driven through ſilver * by mechanical percuſſion in the Florentine experiment. It occurred to me, that this might be prevented by confining along with the fuſible metal a ſmall quantity of air, which, by yielding a little to the expanſion of the liquid, would ſave the barrel. This remedy was found to anſwer completely, and was applied, in all the experiments made at this time †.

Remedy. A ſmall quantity of air was left in the barrel.

I now propoſed, in order to keep the carbonate clean, to incloſe it in a ſmall veſſel; and to obviate the difficulty of removing the reſult at the concluſion of the experiment, I further propoſed to connect that veſſel with an iron ramrod, longer than the barrel, by which it could be introduced or withdrawn at pleaſure.

The carbonate was incloſed in a ſmall ſeparate veſſel.

* *Eſſays of Natural Experiments made in the Academie del Cimento*, tranſlated by Waller, London, 1684, page 117. The ſame in Muſſchenbroek's Latin Tranſlation, Ludg. Bat. 1731, p. 63.

† I found it a matter of much difficulty to aſcertain the proper quantity of air which ought to be thus incloſed. When the quantity was too great, the reſult was injured by diminution of elaſticity, as I ſhall have occaſion fully to ſhew hereafter. When too ſmall, or when, by any accident, the whole of this included air was allowed to eſcape, the barrel was deſtroyed.

I hoped to aſcertain the bulk of air neceſſary to give liberty to the expanſion of the liquid metal, by meaſuring the actual quantity expelled by known heats from an open barrel filled with it. But I was ſurpriſed to find, that the quantity thus diſcharged, exceeded in bulk that of the air which, in the ſame heats, I had confined along with the carbonate and fuſible metal in many ſucceſsful experiments. As the expanſion of the liquid does not ſeem capable of ſenſible diminution by an oppoſing force, this fact can only be accounted for by a diſtenſion of the barrel. In theſe experiments, then, the expanſive force of the carbonic acid, of the included air, and of the fuſible metal, acted in combination againſt the barrel, and were yielded to in part by the diſtenſion of the barrel, and by the condenſation of the included air. My object was to increaſe the force of this mutual action, by diminiſhing the quantity of air, and by other devices to be mentioned hereafter. Where ſo many forces were concerned, the laws of whoſe variations were unknown, much preciſion could not be expected, nor is it wonderful, that in attempting to carry the compreſſing force to the utmoſt, I ſhould have deſtroyed barrels innumerable.

A ſmall

Defcription of this apparatus.

A fmall tube of glafs,* or of Reaumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length, (fig. 2, A) was half filled with pounded carbonate of lime, rammed as hard as poffible; the other half of the tube being filled with pounded filex, or with whatever occurred as moft likely to prevent the intrufion of the fufible metal in its liquid and penetrating ftate. This tube fo filled, was placed in a frame or cradle of iron (*d f k h*, figs. 3, 4, 5, and 6), fixed to the end (*m*) of a ram-rod (*m n*). The cradle was from fix to three inches in length, and as much in diameter as a gun-barrel would admit with eafe. It was compofed of two circular plates of iron, (*d e f g* and *h i k l*, feen edgewife in the figures), placed at right-angles to the ram-rod, one of thefe plates (*d e f g*) being fixed to it by the centre (*m*). Thefe plates were connected together by four ribs or flattened wires of iron (*d h*, *e i*, *f k*, and *g l*,) which formed the cradle into which the tube (A), containing the carbonate, was introduced by thrufting the adjacent ribs afunder. Along with the tube juft mentioned, was introduced another tube (B), of iron or porcelain, filled only with air. Likewife, in the cradle, a pyrometer † piece (C) was placed in contact with (A) the tube con-

* I have fince conftantly ufed tubes of common porcelain, finding glafs much too fufible for this purpofe.

† The pyrometer-pieces ufed in thefe experiments were made under my own eye. Neceffity compelled me to undertake this laborious and difficult work, in which I have already fo far fucceeded as to obtain a fet of pieces, which, though far from complete, anfwer my purpofe tolerably well. I had lately an opportunity of comparing my fet with that of Mr. Wedgwood, at various temperatures, in furnaces of great fize and fteadinefs. The refult has proved, that my pieces agree as well with each other as his, though with my fet each temperature is indicated by a different degree of the fcale. I have thus been enabled to conftruct a table, by which my obfervations have been corrected, fo that the temperatures mentioned in this paper are fuch as would have been indicated by Mr. Wedgwood's pieces. By Mr. Wedgwood's pieces, I mean thofe of the only fet which has been fold to the public, and by which the melting heat of pure filver is indicated at the 22d degree. I am well aware, that the late Mr. Wedgwood, in his Table of Fufibilities, has ftated that fufion as taking place

containing the carbonate. Thefe articles generally occupied the whole cradle; when any fpace remained, it was filled up by a piece of chalk dreffed for the purpofe. (Fig. 4, reprefents the cradle filled, as juft defcribed).

Things being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fufible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, fo that the carbonate was placed very near the breech, (as reprefented in fig. 5, the fufible metal ftanding at *o*). The air-tube (B) being placed fo as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though fome of it might be driven off by the heat, fo as to efcape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held faft. The barrel being then filled to the brim with fufible metal, the apparatus was ready for the application of heat to the breech, (as fhewn in fig. 6.) Plate X.

Method of ufing the fame,

In the experiments made at this time, I ufed a fquare brick furnace, (figs. 7 and 8, having a muffle (*r s*) traverfing it horizontally and open at both ends. This muffle being fupported in the middle by a very flender prop, was expofed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hotteft part, and the end next the muzzle projecting beyond the furnace, and furrounded with cloths which were drenched with water from time to time. (This arrangement is fhewn in fig. 7.) In this fituation, the fufible metal furrounding the cradle being melted, the air contained in the air-tube would of courfe feek the higheft pofition, and its firft place in the air-tube would be occupied by fufible metal. (In fig. 6, the new pofition of the air is fhewn at *p q*).

the furnace and muffle arrangement, &c.

At the conclufion of the experiment, the metal was generally removed by placing the barrel in the tranfverfe muffle, with its muzzle pointing a little downwards, and fo that the heat was applied firft to the muzzle, and then to the reft of the barrel in fucceffion. (This operation is fhewn in fig. 8.) In

Method of difengaging the contents after experiment.

place at the 28th degree; but I am convinced that his obfervations muft have been made with fome fet different from that which was afterwards fold.

ſome of the firſt of theſe experiments, I looſened the cradle, by plunging the barrel into heated brine, or a ſtrong ſolution of muriate of lime; which laſt bears a temperature of 250° of Fahrenheit before it boils. For this purpoſe, I uſed a pan three inches in diameter, and three feet deep, having a flat baſon at top to receive the liquid when it boiled over. The method anſwered, but was troubleſome, and I laid it aſide. I have had occaſion, lately, however, to reſume it in ſome experiments in which it was of conſequence to open the barrel with the leaſt poſſible heat *.

By theſe methods I made a great number of experiments, with reſults that were highly intereſting in that ſtage of the buſineſs, though their importance is ſo much diminiſhed by the ſubſequent progreſs of the inveſtigation, that I think it proper to mention but very few of them.

Calcareous ſpar converted into hard denſe marble by heat of 33° Wedgwood.

On the 31ſt of March, 1801, I rammed forty grains of pounded chalk into a tube of green bottle-glaſs, and placed it in the cradle as above deſcribed. A pyrometer in the muffle along with the barrel indicated 33°. The barrel was expoſed to heat during ſeventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one ſolid maſs, which had viſibly ſhrunk in bulk, the ſpace thus left within the tube being accurately filled with metal, which plated the carbonate all over without penetrating it in the leaſt, ſo that the metal was eaſily removed. The weight was reduced from forty to thirty-ſix grains. The ſubſtance was very hard, and reſiſted the knife better than any reſult of the kind previouſly obtained; its fracture was cryſtalline, bearing a reſemblance to white ſaline marble; and its thin edges had a decided ſemitranſparency, a circumſtance firſt obſerved in this reſult.

Calcareous ſpar rendered cryſtalline with rhomboidal fracture by heat 23°.

On the 3d of March of the ſame year, I made a ſimilar experiment, in which a pyrometer-piece was placed within

* In many of the following experiments, lead was uſed in place of the fuſible metal, and often with ſucceſs; but I loſt many good reſults in this way: for the heat required to liquefy the lead approaches ſo near to redneſs, that it is difficult to diſengage the cradle without applying a temperature by which the carbonate is injured. I have found it anſwer well, to ſurround the cradle and a few inches of the rod with fuſible metal, and to fill the reſt of the barrel with lead.

the

the barrel, and another in the muffle; they agreed in indicating 23°. The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have loft $3\frac{1}{2}$ grains. A thin rim, lefs than the 20th of an inch in thicknefs, of whitifh matter, appeared on the outfide of the mafs. In other refpects, the carbonate was in a very perfect ftate; it was of a yellowifh colour, and had a decided femitranfparency and faline fracture. But what renders this refult of the greateft value, is, that on breaking the mafs, a fpace of more than the tenth of an inch fquare, was found to be completely cryftallized, having acquired the rhomboidal fracture of calcareous fpar. It was white and opaque, and prefented to the view three fets of parallel plates which are feen under three different angles. This fubftance, owing to partial calcination and fubfequent abforption of moifture, had loft all appearance of its remarkable properties in fome weeks after its production; but this appearance has fince been reftored, by a frefh fracture, and the fpecimen is now well preferved by being hermetically inclofed,

(To be continued.)

XIII.

On the Ufe of the Sutures in the Skulls of Animals. By Mr. B. GIBSON *.

THE full ufe of the fingular junction of the bones of the fkull, which is called future, has, from the earlieft periods of anatomy and furgery, attracted the attention and eluded the refearches of the phyfiologift. To this remarkable feature in ofteogny, in a great meafure peculiar to a certain period of life, many ufes have been attributed. Some of thefe are totally erroneous; fuch as that for allowing the tranfpiration of moifture, to keep the brain cool and fit for thinking; for giving a more ftrict adhefion of the *dura mater* to the inner furface of the fkull; for admitting a more free communication by blood-veffels between the external and internal parts of the head; or for affording interftices, that the bones may be

Conjectures on the ufe of the futures in the fkulls of animals.

* Manchefter Memoirs, N. S. Vol. I. 39.

pufhed

pushed asunder by the growth of the brain, lest that organ should be cramped in its growth, in consequence of the comparatively slow growth of the bones of the skull.

Other supposed uses.

Other uses attributed to the sutures are merely slight advantages derived from their structure, which are enjoyed in early infancy, or till adult life, but gradually cease after that period. Thus at the time of birth the loose union of the bones of the skull accommodates the shape of the head to the figure of the different parts of the cavity through which it passes. At adult age, when the sutures are fully formed, they may occasionally check the progress (if I may be allowed the expression) of a fracture nearly spent;—or vibrations, communicated to the bones of the skull, will be propagated with less force to the brain, in consequence of the bones being separated at the sutures. It is, however, abundantly evident, that these are not the main purposes for which the sutures are formed; otherwise they would not begin to be obliterated at a period of life when they would perform these offices more usefully than ever. Consistent with this remark we shall find, that the true purpose for which they are formed, and the particular process with which they are connected, is fully completed before their obliteration takes place.

The cartilage between bones destined to be united, disappears at last.

When we take a view of the mode of junction between many bones, and parts of bones in the human body, which do not admit of motion, we find that with little exception they all agree in this particular; that sooner or later the cartilage or periosteum which once was interposed is obliterated, and these different portions, or entire bones, coalesce.

Instances in the ribs and other bones.

The separate portions, which originally compose the vertebræ, are early in thus uniting: after these the sides of the lower jaw; at a later period the epiphysis of a cylindrical bone is united to its body: and still later the bones of the skull usually coalesce, and the sutures are obliterated. Other bones, as those of the face, which have no motion and sustain little weight, are irregular in this respect; sometimes uniting, but generally remaining distinct, to the end of a long life.

Manner in which the osseous system is completed, &c.

The original formation of the osseous system in several distinct pieces, respects principally its speedy ossification at an early period of life, and its future convenient extension, till it has arrived at its full growth; and we may consider it as a general principle, that where two parts of *one* bone are separated from each other by an intervening cartilage, or *two* distinct bones merely by periosteum, at that part osseous materials are added to increase their length or extend their superfices. This we shall find takes place, whether the junction be effected by comparatively smooth surfaces, as between the body of a bone and its epiphysis; or between the bones of the skull by jagged sutures. Hence it appears that the bones of the body generally are increased in length or extent, not by a uniform extension of the whole substance, but by an addition of bony matter in some particular part.

Cylindrical bones are lengththened by additions at each end.

Thus the body of a cylindrical bone is lengthened by addition to each end. This we might conclude would be the case, from considering the part in which its ossification commences: as this commences in a middle point and proceeds to each extremity, it is natural to suppose that its growth still goes on in the same direction, or continues at the extremities. That this is the case we know, not by reasoning alone, but by a direct experiment. Mr. Hunter sunk two small pieces of lead in the middle of the tibia, or shin bone of a pig, and measured accurately the distance between them: on examining the animal some time afterwards, it appeared, that though the bone had increased considerably in length, the pieces of lead still remained at the same distance from each other that they were before. From this experiment we learn, that a cylindrical bone is not extended in its middle, but is lengthened by addition to its extremities, where the body of the bone is joined to its epiphysis; the chief intention of the epiphysis being to allow the intervention of a vascular organ, which may conveniently deposit bony materials, without interfering with the joint itself.

The same process appears to take place at the edges of flat bones.

As cylindrical bones are lengthened at their extreme parts, we are led by analogy to conclude, that the same general plan is pursued in the extension of the flat bones of the body: and although we have no direct experiment by which this has been proved, there are circumstances which leave little doubt but they are extended by addition to their edges. Thus to take the parietal bone as an example; as ossification begins in a central point and extends towards the circumference, it is probable that to the completion of the process, it continues to go on in the same direction; and the same circumstance taking place

place in every bone of the cranium, it is probable that even after the whole of the brain is incafed in bone, the addition is ftill made at the edge of each, and that the general enlargement originates where they are all mutually joined by the futures. Of this procefs I had a very ftriking illuftration fome years ago. In a young fubject, from what caufe I know not, the depofition of offeous matter had been fuddenly increafed a fhort time before death. It was in different ftages of progrefs, but had taken place in all the bones of the body which I preferved; in fome partially, in others generally. In all, the new offeous matter was elevated above the level of the bone upon which it was placed. In fome parts of the parietal bones it was only in its commencement, and put on the appearance of a net-work, fimilar to that which may be obferved in the fame bones at an early period of their formation. In other parts the mefhes of the net-work were more or lefs filled up; in others again completely, fo as to put on the uniform appearance of folid bone. The fame reticulated appearance was evident on the edges of all the bones of the fkull, where they form the futures, and at the extremities of the cylindrical bones, between the body and epiphyfis. The fame appearance of increafed depofition was feen on the furface of the cylindrical bones, with this difference, that the mefhes were not circular, but oblong fquares; fo as to put on more of the ftriated appearance. In fome parts, the newly fecreted bone was eafily feparable from the general mafs, and formed a thin layer externally, affording one of the beft proofs I have met with, of the increafe of cylindrical bones in thicknefs by depofition externally, whilft a correfponding internal abforption goes on. From the ftriking fimilarity of appearance on the furfaces and edges of the bones, we may fafely conclude, that the fame procefs of depofition was going on in both, and may thence infer, that the bones of the fkull are increafed in extent bythe depofition of offeous matter at their edges, or where they are joined to each other by future. This fact points out to us, in a great meafure, the real ufe of this peculiar mode of junction.

Inftance in the fkull.

In order that the bones of the fkull may be increafed in extent, it is neceffary that they fhould be retained at a certain diftance from each other; that the periofteum with its veffels

The ferrated edges give firmnefs, &c.

2 may

may paſs down between them, free from compreſſion and ſecrete the oſſeous matter. At the ſame time, the thin bones compoſing the upper part of the ſkull, reſting as an arch upon its baſis, muſt be united together ſo firmly, as not to be ſeparated by common degrees of violence. For this purpoſe, projecting points from the external ſurface of each bone, are reciprocally received into correſponding nitches; which only penetrate through one half of the thickneſs of the ſkull, and form an irregular kind of dovetailing.

Two advantages ariſe from this ſtructure, being ſuperficial, and confined to the external table of the ſkull. The projecting points from each ſide, reſting upon the ſolid ſurface of the internal table of the oppoſite bone, can reſiſt more effectually any violence, which might tend to force the bones inwards; and the internal part of the ſkull preſents, by this means, a ſmooth ſurface to the coverings of the brain; for internally no appearance of a jagged ſuture is ſeen.

Thus the ſutures unite the bones, and admit the vaſcular organ requiſite for their growth.

From this view of the ſubject we ſee, that the ſutures of the human ſkull, by their peculiar formation, at once unite the bones together, and ſo far ſeparate them, as to allow the interpoſition of a vaſcular organ by which their ſuperficies is gradually increaſed to its greateſt extent *. This explanation of

* Since this paper was written in the year 1800, I have found, that a ſimilar opinion was publiſhed by Profeſſor Soemmerring in 1794, in his valuable work, "De corporis humani fabrica." To him, therefore, any credit which may belong to the primary ſuggeſtion of this uſe of the ſutures is due. As his opinion, however, has been little noticed by anatomiſts generally, and is placed in a clearer point of view by the facts which ſuggeſted this further explanation of it to me, it has not been thought improper to give this eſſay a place in theſe Memoirs. But whilſt the reader will ſee, by the following quotation, the near reſemblance between the opinion of Profeſſor Soemmerring and that which I have brought forward, I hope the character of plagiariſt or compiler will not be attributed to me.

"Uſus horum ſic ſeſe habentium terminorum oſſa cranii inter bene liquet.

"Incrementum ambitus calvariæ levant, ni enim inter oſſa capitis mox poſt partum ſuturæ interponerentur, hæc creſcere non poſſent, niſi aliâ ratione natura rem inſtitueret. Tali igitur modo incrementum calvariæ cum incremento reliquorum oſſium convenit; initio enim ſuturis, vel potius lineis cartilaginoſis oſſa iis of the ufe of futures comprehends and accounts for thofe concomitant circumftances, which were confidered by older anatomifts as their real ufe; and, as far as I can fee, is not contradicted by any fact connected with them.

Other remarks and inferences.

If it be afked, for inftance, why at the futures there is a ftronger adhefion of the *dura mater* internally and *periofteum* externally than in other parts of the fkull? the anfwer is, that thefe membranes with their veffels are continued into the futures, to form conjointly the fecretory organ, by which the bones are extended.

If it be afked, why there is a greater vafcularity or an appearance of blood-veffels paffing through the futures? it is perfectly confiftent with this opinion to anfwer, that the increafe of blood goes to this fecretory organ, for the purpofe of the extenfion of the bones.

Why the futures are obliterated, &c.

The explanation here offered accounts alfo for the general obliteration of the futures after a certain period of life; for the bones having then arrived at their full fize, the organ for the fecretion of offeous matter is no longer needed; it fhrinks and is abforbed, and the bones gradually coalefce; by which a further advantage is derived, that of an acceffion of ftrength to the cranium at large.

iis locis conglutinantur, verum tamen non nifi in embrionibus ad fonticulos, ut aiunt, hæc linea notabili latitudine, obfervatur. Offibus enim capitis hic locorum cerebro crefcente, placide quafi deductus, cartilago augetur, latior evafura, nifi priftina pars fimul in os mutaretur, inde offa calvariæ, eodem modo, quo offa longa deductis epiphyfibus, vel quod unum idemque eft, marginibus crefcere, liquet, etfi in offibus, longis futura epiphyfes inter et diaphyfin non crifpetur.

"Quo junior igitur infans, eo minus crifpa et implexa futura, vel ut rectius loquar, linea cartilaginofa augufta, offa jungens, obfervatur. Quum vero aucta ætate offa, crefente cerebro, deducuntur, eorumque, craffitudo adpofita cum internæ, tum externæ potiffimum tabulæ, (internæ enim incrementum citius abfolutum videtur) maffa offea, augetur, non poteft non effe, quin hæc crifpa futuræ forma, quum quidem nafci cœpit, externâ in fuperficie tamdiu, augeatur, donec tandem ipfa ea quam maxime impediat, quo minus cerebrum calvariam ulterius deducere poffit, quod pubertatis tempore accidit. Rariffime hæc offificatio ad ætatem virilem ufque detinetur."—Soemmerring de corporis Humani Fabrica, page 212.

If

If any additional argument be neceſſary in ſupport of this opinion, I may alſo notice the ſtriking analogy which ſubſiſts between the ſeparation of one bone of the ſkull from another by a ſuture; and that ſeparation which exiſts between the body of a cylindrical bone and its epiphyſis. They each remain only for a certain length of time; each allows the interpoſition of a ſecretory organ; and both begin to be obliterated when the bones with which they are connected have completed their growth, and their continuance is no longer neceſſary.

XIV.

On the Reproduction of Buds. By THOMAS ANDREW KNIGHT, *Eſq. F. R. S.**

MY DEAR SIR,

EVERY tree in the ordinary courſe of its growth generates, in each ſeaſon, thoſe buds which expand in the ſucceeding ſpring; and the buds thus generated, contain, in many inſtances, the whole of the leaves which appear in the following ſummer. But if theſe buds be deſtroyed during the winter or early part of the ſpring, other buds, in many ſpecies of trees, are generated, which in every reſpect perform the office of thoſe which previouſly exiſted, except that they never afford fruit or bloſſoms. This reproduction of buds has not eſcaped the notice of naturaliſts; but it does not appear to have been aſcertained by them from which, amongſt the various ſubſtances of the tree, the buds derive their origin.

If the generated buds of a ſeaſon be deſtroyed, others are produced.

Du Hamel conceived that reproduced buds ſprang from pre-organized germs; but the exiſtence of ſuch germs has not, in any inſtance, been proved, and it is well known that the roots, and trunk, and branches of many ſpecies of trees will, under proper management, afford buds from every part of their ſurfaces; and therefore, if this hypotheſis be well founded, many millions of ſuch germs muſt be annually generated in every large tree; not one of which in the ordinary courſe of nature will come into action: and as nature, amidſt all its exu-

Du Hamel's opinion that theſe laſt are from pre-organized germs.

Objection.

* Phil. Tranſ. 1805.

berance,

berance, does not abound in ufelefs productions, the opinions of this illuftrious phyfiologift are, in this cafe, probably erroneous.

Suppofition that they are afforded by the bark.

Other naturalifts have fuppofed the buds, when reproduced, to fpring from the plexus of veffels which conftitutes the internal bark; and this opinion is, I believe, much entertained by modern botanifts: it neverthelefs appears to be unfounded, as the facts I fhall proceed to ftate will evince.

Inftance to the contrary in fea cale. Internal buds.

If the fruit-ftalks of the fea cale *(crambe maritima)* be cut off near the ground in the fpring, the medullary fubftance, within that part of the ftalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the fucceeding winter. The fides of this cup confift of a woody fubftance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum, as the white wood of the oak or elm: and from the interior part of this fubftance, within the cup, I have frequently obferved new buds to be generated in the enfuing fpring. It is fufficiently obvious that the buds in this cafe do not fpring from the bark; but it is not equally evident that they might not have fprung from fome remains of the medulla.

Potatoes afford buds at the cut furface,

In the autumn of 1802, I difcovered that the potatoe poffeffed a fimilar power of reproducing its buds. Some plants of this fpecies had been fet, rather late in the preceding fpring, in very dry ground, where, through want of moifture, they vegetated very feebly; and the portions of the old roots remained found and entire till the fucceeding autumn. Being then moiftened by rain, many fmall tubers were generated on the furfaces made by the knife in dividing the roots into cuttings; and the buds of thefe, in many inftances, elongated into runners, which gave exiftence to other tubers, fome of which I had the pleafure to fend to you.

—and therefore not from the bark.

I have in a former paper remarked, that the potatoe confifts of four diftinct fubftances, the epidermis, the true fkin, the bark, and its internal fubftance, which, from its mode of formation, and fubfequent office, I have fuppofed to be alburnous: there is alfo in the young tubes a tranfparent line through the centre, which is probably its medulla. The buds and runners fprang from the fubftance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the

the furface in contact with the bark. It muft, however, be admitted, that the internal fubftance of the potatoe correfponds more nearly with our ideas of a medullary than of an alburnous fubftance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of thefe plants are not generated by the cortical fubftance of the root: and I fhall proceed to relate fome experiments on the apple, and pear, and plumb-tree, which I conceive to prove that the reproduced buds of thofe plants do not fpring from the medulla.

Other inftances of fruit trees in which reproduced buds appeared to fpring from the alburnum.

Having raifed from feeds a very confiderable number of plants of each of thefe fpecies in 1802, I partly difengaged them from the foil in the autumn, by digging round each plant, which was then raifed about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the feed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained expofed to the air and light. In the beginning of April, I obferved many fmall elevated points on the bark of thefe roots, and, removing the whole of the cortical fubftance, I found that the elevations were occafioned by fmall protuberances on the furface of the alburnum. As the fpring advanced, many minute red points appeared to perforate the bark: thefe foon affumed the character of buds, and produced fhoots, in every refpect fimilar to thofe which would have fprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I fhall not venture to decide; but I am much difpofed to believe that, like thofe of the potatoe, they fprang from the alburnous fubftance folely.

They do not originate from the medulla.

The fpace, however, in the annual root, between the medulla and the bark is very fmall; and therefore it may be contended that the buds in thefe inftances may have originated from the medulla. I therefore thought it neceffary to repeat fimilar experiments on the roots and trunks of old trees, and by thefe the buds were reproduced precifely in the fame manner as the annual roots: and therefore, conceiving myfelf to have proved in a former Memoir,* that the fubftance which has

* Phil. Tranf. of 1803.

been

been called the medullary procefs does not originate from the medulla, I muft conclude that reproduced buds do not fpring from that fubftance.

Remarks on the manner in which this procefs of nature is probably effected.

I have remarked, in a paper which you did me the honour to lay before the Royal Society in the commencement of the prefent year, that the alburnous tubes at their termination upwards invariably join the central veffels, and that thefe veffels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give exiftence to new buds and leaves. It is alfo evident, from the facility with which the rifing fap is transferred from one fide of a wounded tree to the other, that the alburnous tubes poffefs lateral as well as terminal orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may poffefs the power to generate central veffels; which veffels evidently feed, if they do not give exiftence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither fpring from the medulla nor the bark, I am much inclined to believe that they are generated by central veffels which fpring from the lateral orifices of the alburnous tubes. The practicability of propagating fome plants from their leaves may feem to ftand in oppofition to this hypothefis; but the central veffel is always a component part of the leaf, and from it the bud and young plant probably originate.

Attempt to difcover the fame power in feeds.

I expected to difcover in feeds a fimilar power to regenerate their buds; for the cotyledons of thefe, though diffimilar in organization, execute the office of the alburnum, and contain a fimilar refervoir of nutriment, and at once fupply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been decifive, owing to the difficulty of afcertaining the number of buds previoufly exifting within the feed. Few, if any, feeds, I have reafon to believe, contain lefs than three buds, one only of which, except in cafes of accident, germinates; and fome feeds appear to contain a much greater number. The feed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the feeds, like the buds of the horfe-cheftnut, contain all the leaves and apparently all the buds of the fucceeding year: and I have never been able to fatisfy myfelf that all the buds were eradicated without

having

having deftroyed the bafe of the plumule, in which the power of reproducing buds probably refides, if fuch power exifts.

Nature appears to have denied to annual and biennial plants (at leaft to thofe which have been the fubjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but neverthelefs fome biennials poffefs, under peculiar circumftances, a very fingular refource, when all their buds have been deftroyed. A turnip, bred between the Englifh and Swedifh variety, from which I had cut off the greater part of its fruit-ftalks, and of which all the buds had been deftroyed, remained fome weeks in an apparently dormant ftate; after which the firft feed in each pod germinated, and burfting the feed-veffel, feemed to execute the office of a bud and leaves to the parent plant, during the fhort remaining term of its exiftence, when its preternatual foliage perifhed with it. Whether this property be poffeffed by other biennial plants in common with the turnip, or not, I am not at prefent in poffeffion of facts to decide, not having made precifely the fame experiment on any other plant.

Annual and biennial plants have not this power.

I will take this opportunity to correct an inference that I have drawn in a former paper,* which the facts (though quite correctly ftated) do not, on fubfequent repetition of the experiment, appear to juftify. I have ftated, that when a perpendicular fhoot of the vine was inverted to a depending pofition, and a portion of its bark between two circular incifions round the ftem removed, much more new wood was generated on the lower lip of the wound become uppermoft by the inverted pofition of the branch, than on the oppofite lip, which would not have happened had the branch continued to grow erect; and I have inferred that this effect was produced by fap which had defcended by gravitation from the leaves above. But the branch was, as I have there ftated, employed as a layer, and the matter which would have accumulated on the oppofite lip of the wound had been employed in the formation of roots, a circumftance which at that time efcaped my attention. The effects of gravitation on the motion of the defcending fap, and confequent growth of plants, are, I am well fatisfied, from a great variety of experiments, very great; but it will be very difficult to difcover any method by which the extent

Correction of a former inference.

* Phil. Tranf. of 1803.

of

of its operation can be accurately afcertained. For the veffels which convey and impel * the true fap, or fluid from which the new wood appears to be generated, pafs immediately from the leaf-ftalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unlefs it had been extravafated, could have defcended to the part from which the bark was taken off in the experiment I have defcribed. I am not fenfible that in the different papers which I have had the honour to addrefs to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of fupporting.

I am, &c.

THO[s]. ANDREW KNIGHT.

Elton, May 12, 1805.

XV.

Experiments on the Gafeous Oxide of Azote, by a Society of Amateurs at Touloufe. Publifhed by M. P. DISPAN, *Profeffor of Chemiftry in the College of that City.*†

Difagreement of former experiments on the oxide of azote.

THE motive for the following experiments was the very different, and even contradictory refults, which have been publifhed of former effects. The experiments were tried upon more than a dozen perfons, and in fome cafes repeated two or three times; the fenfations which each experienced were written down at the moment, by the reporter, from whofe memorandums the fubfequent obfervations are drawn.

Preparation of the nitrate of ammonia.

The nitrate of ammonia ufed for the experiment was indiftinctly cryftallized, but was quite neutral. Its tafte was very pungent, with a flight odour. It had been formed by the faturating very pure nitric acid with ammoniacal gas obtained by diftilling fal ammoniac with the common potafh of commerce.

Procefs for obtaining the gafeous oxide of azote.

About one hectogramme (1545 grains) of this falt was put into a fmall retort, and placed on a fand-bath, where the falt

* Phil. Tranf. of 1804.

† Annales de Chimie, Vol. LVI. p. 243.

melted

melted and boiled for a ſhort time without yielding any gas; at length, the retort became filled with a white vapour, which quickly diſappeared; the gas was then rapidly diſengaged, and was caught in bladders. By degrees the diſengagement became more and more ſlow, and when the operation was ended, ſcarcely any thing remained in the retort.

Another experiment was made with a larger retort, and three hectogrammes (10 oz. troy) of the ſalt, from which was obtained gas ſufficient to fill eight bladders. This operation proceeded in a ſimilar manner with the former; except that as the retort cooled, a red vapour aroſe within it, which it was aſcertained by experiment, contained no nitrous gas. The ſame proceſs on a larger ſcale.

Effects of Gaſeous Oxide of Azote when breathed into the Lungs.

All who have taſted or inhaled this gas, agree in deſcribing its flavour as ſtrongly ſaccharine, and remaining upon the organs of ſome perſons during the whole day after receiving it. M. Diſpan obſerved in it an after-taſte of nitre; but acknowledges that it was the laſt collected gas which he taſted.—M. de M***, perhaps under a ſimilar impreſſion, ſays he perceived in it a ſtyptic quality. The gas has a ſaccharine taſte.

The method of reſpiring this gas was by means of a bladder with a ſtop-cock in it, applied to the mouth; the noſtrils being cloſed, and the lungs as much as poſſible emptied. The gas was reſpired.

No. 1. The firſt perſon upon whom the experiment was tried, ſwooned at the third inſpiration, and remained ſenſeleſs about five minutes, when he recovered, but with a ſenſation of great fatigue. He recollected to have experienced only a ſudden faintneſs, attended with a tingling at the temples.

No. 2. M. de M*** obſerved a ſaccharine and ſtyptic taſte, and experienced a ſenſe of great dilatation, accompanied with heat in the breaſt; his veins ſwelled, and his pulſe was quickened: ſurrounding objects ſeemed to revolve round him. But he thought he could have borne a ſtronger doſe; the bladder not being large enough for his lungs.

No. 3 experienced a ſaccharine taſte on the firſt inſpiration; but became inſenſible to thoſe which ſucceeded. His lungs were forcibly dilated with great heat. When the bladder was removed, he appeared very comfortable, but could not refrain from violent burſts of involuntary laughter.

No.

No. 4 had the fame faccharine tafte with the preceding, and retained the impreffion from ten o'clock in the morning till after midnight. He experienced vertigoes, and his legs trembled under him during the remainder of the day.

No. 5, the fame faccharine tafte. On quitting the bladder, he had a dizzinefs of fight, which was fucceeded by a fenfation of great pleafure throughout the body. His legs were weakened.

No. 6. Saccharine flavour throughout the day; tingling in the ears; legs tottering, and the ftomach oppreffed. All that he experienced was rather painful than agreeable.

Receiving the gas from a bladder, had no influence on the refult of the experiment.

In order to afcertain what influence the mode of breathing from a bladder might have on the foregoing refults, the parties were requefted to infpire common air in the fame manner.—They were all mechanically fatigued by it, and nothing more.

Oxigen gas differed from common air only by a fmall increafe of the heat of the lungs.

The bladders were next filled with oxygen gas, and applied as before to the fame perfons, who found only a flight difference between it and common air, confifting in an augmentation of the heat of the lungs.

Conclufion.

The fingular effects above defcribed, can, therefore, and ought only to be afcribed to the gafeous oxide of azote.

Other experiments.

Another meeting of the fociety was held for repeating the experiments more at large, on the refpiration of gafeous oxide of azote.

Defcription of the apparatus with upwards of 2lbs. of the falt.

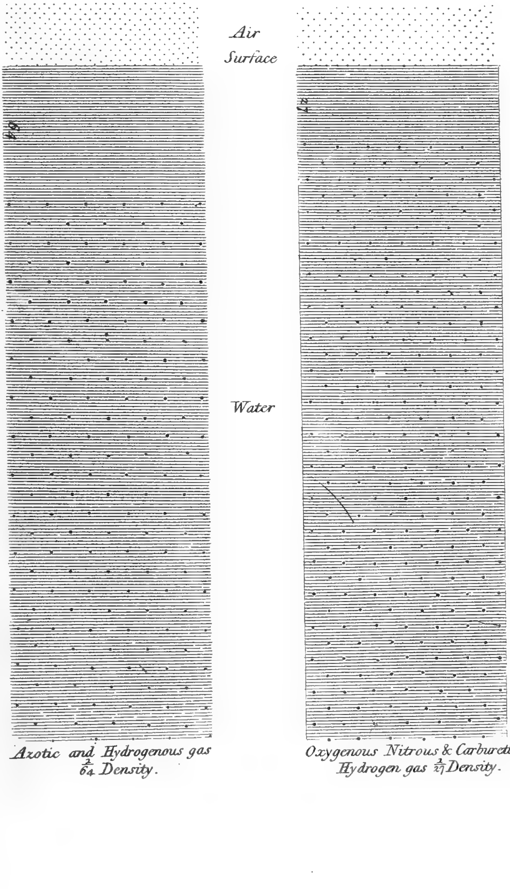
Eight hectogrammes ($27\frac{1}{2}$ oz. troy) of nitrate of ammonia, prepared as before, were put into a retort, with its neck fitted to a double-bodied receiver, from whence, by means of a tube of welter, the gas paffed into an inverted veffel over water. The retort was placed on a fand-bath.

Particulars of the procefs.

As foon as the heat affected the retort, the falt melted; and nearly at the fame moment, fparkling vapours arofe in the retort, but in very fmall quantity. The air which the heat expelled from the veffels had a nitrous odour; but this as well as the vapours gradually diminifhed, and as the procefs continued they difappeared altogether; they were fucceeded by a lively fmell of pruffic acid. At length the retort became filled with white vapours, and the gafeous oxide of azote began to pafs over. The difengagement foon became fo abundant that it was judged proper to draw out the fire; but afterwards, on replacing the coals, the gas, which in the interval had diminifhed,

Profile View of Air in Water.

by Mr. Dalton.



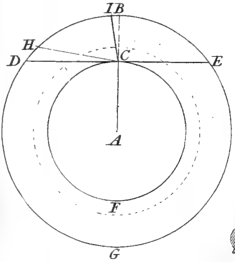
Azotic and Hydrogenous gas $\frac{1}{64}$ Density.

Oxygenous Nitrous & Carburetted Hydrogen gas $\frac{1}{27}$ Density.

View of a square Pile of Shot &c.

The lower globes are to represent particles of water: the top globe represents a particle of air resting on 4 particles of water.

Fig. 1.



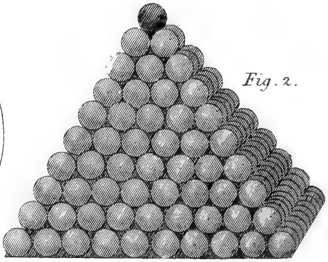


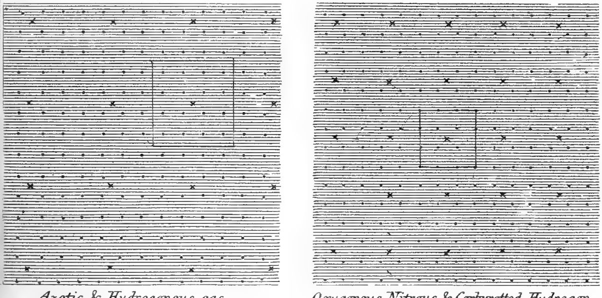
Fig. 2.

Horizontal View of particles of Air in Water. by Mr. Dalton.

Incumbent particles are marked ·

Absorbed particles . . . . ×

Fig. 3.



Azotic & Hydrogenous gas Distance of Particles 4 to 1.

Oxygenous Nitrous & Carburetted Hydrogen Gas. —— Distance of Particles 3 to 1.

nished, was again so rapidly developed that the luting of the vessels began to give way. But notwithstanding the loss which this occasioned, the disengagement continued extremely rapid in the receiver for at least a quarter of an hour.

M. Dispan supposes, that if the luting had not given way, an explosion would have taken place, as has happened to others in this process.

Danger of explosion.

He next proceeds to state the effects of the respiration of this gas.

Twelve persons underwent the experiment, and on many it was repeated. He observes that most of them had inhaled the gas of the former operation, where two out of seven experienced pleasing sensations; but on this second occasion, not one felt pleasure; on the contrary, they all felt pain, and many suffered extremely.

The effects produced by the last gas were more powerful than the former.

One person stamped with his foot the whole time of the breathing: when the bladder was removed, he recovered from the profound stupor into which he had been plunged, and complained of a pain in the back part of his head, as if he had received a violent blow from a dagger: he could not be prevailed on to make another trial. The other persons in general were affected with vertigoes and dizziness of sight, succeeded in some by involuntary convulsive fits of laughter.

M. Dispan tried the effects of this gas on himself, which he thus describes:—

M. Dispan's description of the effect of the gas upon himself.

"At the first inspiration, I emptied the bladder, and my mouth was instantaneously filled with a saccharine flavour, which extended into my lungs and inflated them. I emptied and filled them again; but on the third attempt, my ears were filled with a tingling noise, and I dropped the bladder. I did not, however, become altogether insensible, but remained in a kind of benumbed astonishment, rolling my eyes about without fixing them on any particular object: I was then suddenly seized with convulsive laughing fits, such as I never in my life before experienced. In a few seconds this propensity to laugh stopped suddenly, and I no longer felt any unpleasant symptom."

Two others on whom the gas was tried, experienced only a convulsive movement of some of the muscles of the face; but were in the course of the day attacked with violent diarrhœa.

Effects on two other persons.

Difficulty of reducing the effects of this gas to any general fyftem.

M. Difpan thinks it will be very difficult to reduce the effects of gafeous oxide of azote to any general fyftem, as they vary fo confiderably in their operations upon different individuals, and, what is more fingular, even upon the fame perfon.

M. D. concludes his paper with an account of an experiment to afcertain the effect of gafeous oxide of azote upon animals.

Experiments on a bird immerfed in gafeous oxide of azote.

He placed a greenfinch in a veffel of fufficient dimenfions, and filled it with gafeous oxide of azote. At firft, the bird feemed to fuffer no inconvenience; but he foon gradually clofed his eyes, and dropped gently on his fide, as if afleep. On being reftored to the pure air, he refumed his feet, without attempting to fly away. About an hour afterwards he was fubjected to a fecond trial, and having been fuffered to remain longer in the veffel, he was taken out quite dead.

M. Difpan thinks it very remarkable that the bird fhould make no effort to efcape, and that he fhould manifeft no convulfive fymptoms, fuch as take place in experiments with other gafes.

XVI.

Obfervations on the Mammoth, or American Elephant, by which it is proved to have been an herbivorous Animal. In a Letter from the Right Reverend Bifhop MADISON.*

Difcovery of a mammoth having vegetable remains in its ftomach.

ONE of thofe facts has lately occurred, which the naturalift knows beft how to appreciate, and which I therefore take a pleafure in communicating to you. It is now no longer a queftion, whether the Mammoth was a herbivorous or carnivorous animal. Human induftry has revealed a fecret, which the bofom of the earth had, in vain, attempted to conceal.— In digging a well, near a Salt-Lick, in Wythe-county, Virginia, after penetrating about five feet and a half from the furface, the labourers ftruck upon the ftomach of a mammoth. The contents were in a ftate of perfect prefervation, confifting

* To Benjamin Smith Barton, M. D. editor of the Philadelphia Medical and Phyfical Journal, from which (vol. II.) it is taken.

of

of half masticated reeds, twigs, and grass, or leaves. There could be no deception; the substances were designated by obvious characters which could not be mistaken, and of which every one could judge; besides, the bones of the animal lay around, and added a silent, but sure confirmation. The whole rested upon a lime-stone rock. I have not seen, as yet, any part of those contents; for, though I was within two days' journey of the place where they were found, I was so well satisfied with the narration of gentlemen who had seen them, and upon whose veracity, as well as accuracy, I could rely, that I thought the journey unnecessary; especially as I took measures to ensure the transmission of a sufficient quantity of the contents, together with all the bones, to Williamsburgh. When the contents arrive, a part shall be forwarded to you. I hope to form a complete skeleton of this vast animal, having given directions to spare no labour, in digging up every bone.

We should not be surprised, that these substances should be thus preserved, when we recollect the state of the rhinoceros, mentioned by Pallas. Blumenbach, in his *Manuel d'Histoire Naturelle*, vol. II. p. 398, (traduit par Artaud), has a note, which is very applicable to the present subject. He says, "Quelquefois on trouve encore des pièces animales qui ont conservé, sans alteration, leurs parties *molles;* mais, cependant, comme elles se trouvent aussi enfouies dans la terre par la suite de ces grandes catastrophes des temps antérieurs, on doit les ranger parmi les corps pétrifiés, dans le sens le plus étendu. Je citerai, par exemple, le rhinoceros deterré près de Wiloi, en Sibérie, qui offroit encore des restes tres-reconnoissables, même ayant encore l'odeur animal de muscles, de chair, de peau, & de poils. Pallas l'a décrit trés-exactement dans les Nov. Comment. Petropolit., tome 13. p. 585." Remarks on the preservation of those bodies,

Whether this first kind of petrifaction, of which Blumenbach speaks, and which he calls *simplement calcinés,* has been the cause of the preservation of these substances, or whether it be the effect of the marine salt, with which the earth, where they were buried, has been constantly charged, must be left to future investigation. I pretend not to decide. Had they been buried deep in the earth, that circumstance alone might have prevented a decomposition; but the depth of five or six feet seems insufficient to arrest that chemical action, which —probably from marine salt.

 changes

changes the appearances of organized bodies. The fact, however, is decifive, as to the principal queftion. It has fummoned the difcordant opinions of philofophers before a tribunal, from which there is no appeal.

Williamfburgh, October 6th, 1805.

Note on the preceding Paper. By the Editor.

Facts by Mr. Nevil on long preferved vegetable bodies.

Mr. Francis Nevil, in his account of the elephantine teeth that were difcovered in the north of Ireland, early in the eighteenth century, has mentioned fome facts relative to the long prefervation of vegetable matters, which feem worthy of our notice in this place: and the more fo, as this gentleman's paper feems not to have excited any attention among the modern writers on the exuviæ of animals found in countries in which the living animals themfelves are no longer feen. Some extravagant conjectures are mixed with Mr. Nevil's account: but thefe do not, in the leaft, invalidate the truth of what he fays, relative to the bed upon which the Irifh elephant was laid.

His narrative.

"The place (fays he) where this monfter lay, was thus prepared, which makes me believe it had been buried, or that it had lain there fince the deluge. It was about four feet under ground, with a little rifing above the fuperficies of the earth, which was a plain under the foot of a hill, and about thirty yards from the brook * or thereabout. The bed whereon it lay had been laid with fern, with that fort of rufhes here called fprits, and with bufhes intermixed. Under this was a ftiff blue clay on which the teeth and bones were found: above this was firft a mixture of yellow clay and fand much of the fame colour; under that a fine white fandy clay, which was next to the bed: the bed was for the moft part a foot thick, and in fome places thicker, with a moifture clear through it; it lay fad and clofe, and cut much like turf, and would divide into flakes, thicker or thinner as you would; and in every layer the feed of the rufhes was as frefh as if new pulled, fo that it was in the height of feed-time that thofe bones were laid there. The branches of the fern, in every

* "A fmall brook that parts the counties of Cavan and Monaghan."

lay

lay as we opened them, were very diftinguifhable, as were the feeds of the rufhes and the tops of the boughs. The whole matter fmelt very four as it was dug, and tracing it I found it 34 feet long and about 20 or 22 feet broad."—" I forgot to mention that there was a great many nut-fhells found about the bed, perhaps thofe might have been on the bufhes which compofed part of the bed *."

XVII.

Obfervations on the Danger of ufing Earthen-ware or Pottery of a bad Quality. By M. POIDEVIN *of Rouen* †.

PURE white argil forms the body of the fineft pottery which bears the name of porcelain; clays lefs pure, and coloured more or lefs with iron, ferve to form the ftone ware, or hard earthen-ware, and the common or foft fort, which differs from the other, in not experiencing a commencement of fufion at their furface in baking, like porcelain or ftone ware.

Different kinds of pottery.

This badly prepared common earthen-ware is the kind which is occafionally attended with danger in its ufe, and is the fubject of this paper.

Earthen-ware.

Common brown ware.

The bifcuit of brown earthen-ware is prepared from a ferruginous clay; that of white earthen-ware is compofed of a mixture of ferruginous clay, of another clay containing much filicious fand, a little lime, and finally of a porous clay, which renders it lefs compact, and gives it whitenefs after baking.

Nature not always affording thefe earths in the fame ftate of combination, occafions differences in the bifcuit, when it becomes fubjected to the heat: other differences alfo arife in the action of the enamel on the bifcuit. If the earth be too ferruginous, or too much mixed with filicious particles, the enamel, during the baking, acts as a flux on the bifcuit, foftens it, and occafions the pieces to lofe their fhape.

Differences in the ware from the quality the materia

If the earth is too porous it abforbs the enamel and remains

* A Natural Hiftory of Ireland, in three parts, by Dr. Gerrard Boate, Thomas Molineux, M. D. F. R. S. and others. Pages 128—130. Dublin: 1755.

† Annales de Chemie, T. 55.

rough,

rough, and as it were dried. If it contains too much lime, it throws off the enamel, which falls from it in ſcales inſtead of adhering to it.

Compoſition of the enamel or glaze.

On the other hand, the white enamel is compoſed of ſilicious ſand, a little lime, lead and tin oxides, and ſome flux, ground together with water in mills. The brown ſort is compoſed of the ſame materials, with the addition of manganeſe and perigord ſtone *.

Cauſes which occaſion variations in the glaze.

The greater or leſs fuſibility of the ſand; the greater or leſs purity of the lead, of the tin, and of the ſaline ſubſtances employed as fluxes; the different degrees of heat which the mixture receives in the glazing; the variations of the fineneſs given to the glazing materials by the action of the mill, are ſo many circumſtances which cauſe changes in the enamel in its ſtate of fuſion on the pieces, relative to the ſtate in which it finds the biſcuit and to the fuſible layer, with which this laſt is covered.

Pottery.

Brown pottery.

The body of the brown pottery is a red clay, more or leſs ferruginous and compact according to the places where it is procured.

Yellow pottery.

The common or yellow pottery is made of a white clay, which contains a little lime and magneſia, and a conſiderable quantity of ſilicious ſand, which may be generally eſteemed a fourth of the maſs.

Glaze for brown ware.

The glazing of the brown pottery is formed with a mixture of ſilicious ſand, yellow or red oxide of lead, and manganeſe pulveriſed together.

Glaze for yellow ware.

That of the yellow earthen-ware is compoſed of a mixture of ſilicious ſand, and red oxide of lead, which, during its baking vitrifies at its ſurface, and forms a yellow glazing more or leſs tranſparent. To this mixture is commonly added, in France, a little oxide of magnaneſe in powder, more or leſs fine, without grinding them together. This is called the *grain*, becauſe it fuſes more difficultly than the other materials, without mixing with them, and by that means forms ſtreaks, ſpots, or brown ſpecks, according to the coarſeneſs of the powder itſelf.

Mottled ſtreaks in foreign ware.

Cloudy tinges in the glaze.

In ſome manufactories they mix oxide of copper with the common glazing, to give it a green colour, and in others they form deſigns on the pieces, with oxide of copper, which pro-

* A black ſtone or compact manganeſe. T.

duces

duces a green, with oxide of iron, which caufes a red, or with oxide of manganefe, which gives a brown.

Great imperfections are produced in pottery, from the injudicious ufe of glazing over earths of an unfuitable nature; and this is more remarkable when the earths are not fo well prepared for their glazings as they are for thofe of the finer wares. The articles of common pottery are lefs carefully prepared both in their materials and baking. This laft is ufually performed at a fingle operation, and with lefs fire. Caufes of imperfection in pottery.

The means of producing good pottery and earthen-ware, confift in carefully chufing the earths for forming the body: in producing an exact coincidence of expanfion by heat between them, and the vitrifiable glaze with which they are to be covered, and in baking them by a proper degree of fire, produced from combuftibles not capable of changing the nature of the glazing. Cautions requifite to infure its goodnefs.

The neglect of thefe attentions occafion defects in the manufactured articles, which are either unfightly and nothing more, or both unfightly and dangerous.

The unfightly defects which are found in ill conditioned pottery or earthen-ware, are, *fcaling*; the *dropping* or *drops*; *fmoke*; *drying* of the ware, and *flaws* or *cracks*. Defects or deformities enumerated.

The fcaling is the appellation ufed when the glazing of a piece detaches itfelf in fcales, by the action of moift air, or on the leaft touch, and leaves the bifcuit uncovered. Scaling of the glaze.

The *dropping* or *drops* take place when the moifture of the fuel having ftruck the pieces during the baking, the enamel is collected in *drops* on the furface, and remains vitrified in that form, inftead of being equally fpread. Dropping or drops.

The fmoky appearance happens when a piece has not been purified by a clear flame, but remains blackened or ftained. Smoky tinge.

The drying happens when the pieces are, as it were, roafted in the firing, and come out rough from the abforption of the enamel into their fubftance. Drying.

The flaws happen, when the earth or the bifcuit, having a different pyrometrical expanfibility from that of the enamel; the body contracts in cooling more than the glaze which is therefore fplit, or which is divided into an infinite number of fmall parts, fometimes not perceptible to the eye when the pieces are new, but which become very vifible, when the goods have imbibed any greafy fubftance in ufing. Flaws.

All thefe defects, though difagreeable to the eye, have really, The coarfe pottery is moft defective.

really, with regard to the ware itſelf, only the inconvenience of a dirty appearance, provided the biſcuit is always compact, and well baked. But it is different in the common pottery in which the dropping, the ſcaling, and the flaws produce more injurious defects. As the earth is more porous and leſs baked in thoſe, the liquids preſerved in them enter into the pores where they become altered and decompoſed, and produce ſulphurated hydrogen, which injures every thing kept in them.

Cavities or pits from bad firing.

The moſt noxious defects in pottery are the cavities or pits, and the underbaking. The pits are roughneſſes or hollow bubbles which are found on thoſe pieces, whoſe enamel being injured by rubbing, or being too little acted on by the fire, has not been fuſed into a vitreous ſubſtance. In theſe the metallic oxides are in a ſtate capable of doing injury, being ſtill ſoluble in fat or acid ſubſtances.

Underbaking or imperfect fuſion of the glaze.

The underbaking occaſions one of the moſt dangerous defects in pottery; the pieces thus affected have not had ſufficient heat to cauſe the enamel to do more than agglutinate together, and in ſome caſes it even ſtill remains in powder. It is therefore capable of being divided, and taken up by all the liquids with which it may come in contact.

It is eaſy to ſhew the danger to which the public muſt be expoſed in buying thoſe articles at a low price which are called waſte or refuſe and which ought to be carefully thrown away. In vain may it be ſaid that they are uſed daily without any immediate miſchief happening; from the injury being more concealed, it is no leſs deſtructive. It is known that lead and its oxides act inſenſibly on the organs of digeſtion, eſpecially when taken in ſmall quantities: They do not, however, leſs certainly cauſe, at length, emaciation, cholics, convulſions, ſometimes of all parts of the body, with obſtinate diarrhœas; and the wretched people who uſe ſuch veſſels become the victims of their own ignorance, and of the imprudent avarice of the manufacturer.

It would be to the honour of enlightened manufacturers, not to offer to the public pieces which have imperfections beyond a certain degree, and to make this ſacrifice to the good of national commerce, eſpecially as they can avoid the loſs by a greater attention to their materials.

XVIII. *Extract*

XVIII.

Extract of a Letter from M. JOHN MICHAEL HAUSSMANN, to M. BERTHOLLET, *on the Existence of intermediate Terms of Oxidation.* *

I THINK there are sufficient grounds for admitting, with you, that there exist, in the oxidation of many metallic bodies, intermediate degrees between the *minimum* and the *maximum*. Existence of intermediate degrees of oxidation of metals.

The first example I shall cite, is, that of a *minimum* oxide of tin, precipitated from the muriatic solution, and dissolved in an excess of caustic potash; a metallic alkaline solution which I have before noticed in my Observations on the Red Dye of Adrianople, inserted in the "*Annales de Chimie*," and also in a Memoir on the coloured Oxides of Tin, inserted in the "*Journal de Physique*." Oxide of tin.

By avoiding any dilution of the muriate of tin, and using a very concentrated solution of caustic potash, the mixture disengages much caloric, part of the tin is precipitated in the metalline state, whilst the remainder is held in solution in an intermediate state of oxidation. This alkaline solution is so disoxidant, that it changes the yellow oxide of gold, fixed on cotton, by means of ammonia, to a grey; whilst a similar yellow pattern underwent no change of colour on being steeped in the simple liquor of caustic potash. A like alteration took place on dipping a cotton cloth, which had been previously stained with the solution of gold, and well dried in the alkaline solution of tin, which also produced the same effect on pouring into it the pure solution of gold diluted with water. Experiment. Muriate of tin is in part precipitated metallic and the rest suspended with intermediate oxidation.

This change of the yellow colour of oxide of gold by the alkaline solution of tin, is not the only proof of an intermediate state of oxidation; this liquor possesses besides, a property of destroying the blackish-brown colour of the oxide of manganese stained upon cotton by an alkaline precipitant. Other proofs of that intermediate state.

All these changes are more rapidly produced, if, prior to the precipitation and solution in the caustic potash liquor, the muriatic solution of tin be diluted with six or eight parts of water, in which case there is no sensible disengagement of

* Annales de Chimie, Vol. LVI. 5.

caloric

caloric, and no tin is precipitated in the metalline ſtate. This ſolution, whoſe oxidation approaches the degree of *minimum*, for the moſt part retains an aqueous tranſparency, without any precipitation of oxide; even when long expoſed to the atmoſpheric air, it does not loſe the property of changing the yellow oxide of gold to a grey colour, or of deſtroying the blackiſh brown tint of oxide of manganeſe, when fixed upon cotton.

Oxide of manganeſe.

The oxide of manganeſe is capable of various degrees of oxidation; if a piece of cotton cloth be dipped in the tranſparent ſolution of ſulphate of manganeſe, it will, when dry, retain its original whiteneſs; but on their dipping the ſame cloth in the liquor of carbonated or cauſtic potaſh, it will, after waſhing and expoſure to the atmoſpheric air, be coloured brown; which colour will acquire a deeper ſhade, approximating to black, on being ſteeped for a time in an oxigenated alkaline, muriatic liquor. The oxigenated alkaline liquor, on being for any length of time ſubmitted to the action of the brown precipitate of manganeſe, inſtead of the rag ſteeped therein (which is to diſſolve by means of an increaſed oxidation) will aſſume a purple colour, of greater or leſs tranſparency as the time of their union has been longer or ſhorter.

Other oxides.

There ſeems reaſon, generally, to expect particular reſults from ſubmitting any of the metallic oxides to the action of this oxigenated muriatic alkaline liquor; which might, perhaps, be a means of giving them acid properties, and at the ſame time of proving the gradual oxidation of many metals; this is the more obſervable in white oxide of lead, which becomes gradually coloured by long expoſure to the oxigenated liquor, and being frequently ſtirred.

Muriatic and nitro-muriatic ſolutions of tin, though colourleſs in themſelves, acquire by admixture a vinous tint.

Muriatic and nitro-muriatic ſolutions of tin, well diluted with water, have an aqueous tranſparency, when properly made; but if the two be mixed together, they acquire a fine vinous colour, ſimilar to that of Malaga; this can only ariſe from the oxigen of the nitro-muriatic being in part communicated to the muriatic ſolution of tin.

The addition of ſolution of gold produces a purple dye;

If a ſolution of gold with great exceſs of acid, and diluted with from 130 to 160 parts of water, be gradually poured into the above mixture, ſtirring it all the time, the intenſity of the colour will be increaſed, till at length the liquor becomes

comes of a beautiful purple hue, in which all kinds of goods may be dyed; this may be changed to the tint of peach or lilac blossoms, by increasing the proportions of the nitro muriatic solution; or, on the other hand, by causing the muriatic solutions of tin to preponderate; shades of grey will be obtained, deeper or paler in colour, according to the quantity of the solution added. Care must, however, be taken, in the latter experiment, that too great a proportion of the muriatic liquor of tin be not used; for by depriving the oxide of gold of too much of its oxigen, it might be too much disoxided and precipitated. The precipitate caused by such an accident is not altogether void of oxigen, which prevents its gilding cold silver, as do the ashes of burned cloth impregnated with the solution of gold. The degree to which the preservation of the tincture of gold may be carried, must depend on the proportions of the two solutions of tin, their being more or less surcharged with acids, and the quality of the solution of gold, wherein also there should be a very great excess of acid.

capable of being changed to the bloom of peaches or lilac;

or even to grey, by proportioning the quantity of the two solutions of tin.

The precipitate of the oxide of gold will not gild silver, without the assistance of heat.

The purple tincture of gold, though of the most perfect transparency, is decomposed by exposure to a strong heat, and throws down what is known by the name of "*Purple of Cassius,*" whose beauty depends on the quantity of nitro-muriatic solution of tin made use of. The latter, however, if mixed alone with the solution of gold, without the presence of muriate of tin, produces no alteration of colour, and, if the mixture be not too much weakened with water, is a very long time before it gives a precipitate.

Purple powder of Cassius.

The purple tincture of gold, is, properly speaking, nothing more than the powder of cassius, held in solution by means of the oxigen of the nitro-muriatic liquor of tin; and there is every reason to believe, that in the powder of cassius, the oxide of gold is in some way combined with the oxide of tin, which, by transmitting to it its own origin, during its fixation upon porcelain, prevents it, I think, from returning to its metallic state. I find a difficulty in subscribing to the opinion of Dr. Richter, of Berlin, who, in a memoir (which I have not read) attempts to prove, by mathematical demonstration, that the crimson-coloured gold on porcelain is in the metallic state.

Dr. Richter's opinion of the crimson-coloured gold upon porcelain.

The purple tincture of gold might be advantageously employed in dying silks, without greatly enhancing the price. The

Purple tincture of gold surpasses all others for dying silks.

The colour obtained from it furpaffes all others in duration, fince nothing lefs than combuftion can deftroy it. It is neceffary, however, to leave the filk a long time in this dye; and the depth of the fhade will be in proportion to the number of times the article is dipped; it muft be well wrung, rinced, and dried, between each immerfion.

The gradation of fhades already noticed are indications of a gradual oxidation.

The gradation of colours produced by mixture of the nitro-muriatic, and muriatic folutions of tin, being much weakened by dropping folution of gold in a great excefs of acid, confiderably diluted with water, into the mixture, feems to me to indicate a gradual oxidation. The acetic folution of iron proves the fame truth; for on being expofed to the atmofpheric air, or to the contact of oxigen gas, it gradually changes from a fea green to a reddifh yellow colour. I have fhewn, in a memoir on the alkaline Tincture of Mars of Stahl, that fulphate of iron may be fuper-oxigenated, and alfo lofe its excefs of oxigen by the action of light. On mixing concentrated fulphuric acid with nitric folution of iron, I obtained, after the nitric acid was evaporated, by leaving the refiduum to imbibe the moifture of the air for feveral months; cryftals of fuper-oxigenated fulphate of iron, which were at firft diftinguifhable by their whitenefs from fulphate of alumine; but the action of the light gradually tinged their furface with a yellow colour; their original whitenefs, however, might, by a gentle wafhing, be reftored at pleafure. Super-oxigenated fulphate of iron, of nearly an equal degree of whitenefs, may in like manner, be obtained by precipitating nitrate of iron, and diffolving the precipitate, edulcorated and freed from water, gradually in fulphuric acid, which, if well concentrated, will produce cryftals of fuper-oxigenated fulphate of iron without evaporation. This falt poffeffes an incomparable degree of aftringency.

Sulphate of iron lofes its excefs of oxigen by expofure to the light.

The fact that linens printed with acetate of iron are liable to become rotten, a proof of the gradual tranfmiffion of oxigen.

The progrefs of the tranfmiffion of oxigen is more manifeft on linen fimply printed with acetate of iron and madder, which muft be a long time expofed in the air to bleach, unlefs the artificial means of bleaching be adopted. The printed part of the linen frequently perifhes, bearing the appearance of having been cut with a fharp inftrument, or burned with concentrated acid; this, it fhould feem, muft proceed from the action of the oxigen contained in the coloured oxide of iron, continually replenifhed from the atmofpheric air.

It

It is not among minerals alone that ſubſtances are found which are gradually oxided, and by intermediate degrees.

Vegetable and animal bodies furniſh ſimilar caſes with minerals, of gradual oxidation.

Indigo affords an inſtance that vegetable and animal bodies offer ſimilar proofs; for any ſolution of indigo (excepting the ſulphate of indigo) will, on diſoxidation, or on having its oxigen reſtored, paſs through all the degrees of ſhade, from blueiſh green to very yellow olive, preſerving in the mean time the ſame quantity of indigo in ſolution. The beauty and ſtability of the colours, either for dying or painting, will chiefly depend on the degree of oxidation. On ſome other occaſion, Sir, I ſhall write to you more amply on this ſubject.

SCIENTIFIC NEWS.

Mémoires de l'Académie impériale des Sciences, &c. Memoirs of the imperial Academy of Sciences, Literature, and fine Arts, of Turin, for the Years 12 *and* 13, 2 *Vols. Quarto.* 1805. *Turin.*

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

WHEN the Royal Academy of Turin aſſumed the name of Imperial, in conſequence of Piedmont being annexed to France, the number of academicians was increaſed, to form a new claſs, that of literature and the fine arts. Of the two volumes publiſhed, one is appropriated to the labours of this claſs, the other to that of the phyſical and mathematical ſciences.

The latter is compiled by the ſecretary, Mr. Vaſſali Eandi, who firſt mentions the changes that have taken place in the liſt of academicians, next the various papers that have been read at their meetings, and then the books and other articles preſented to the ſociety. Theſe liſts are followed by a well written account of the labours of the academy up to the year 1805, which occupies 250 pages. After this follow the different memoirs.

1. Deſcription and uſe of a new portable barometer, for meaſuring heights and depths, with obſervations made with this inſtrument in the circles of Turin and Saluzzo. This inſtrument, of which a figure is given, was invented by the ſecretary; who has ſubjoined to his paper ſome very curious hiſtorical notes on the places where his obſervations were made.

Memoirs of the Imperial Academy of Sciences, &c. of Turin.

2. Account of a waterfpout, that occurred in the territory of Revel, in the circle of Saluzzo, March 27, 1798, with remarks on the caufe of the phenomenon, by the fame.

3. On the different capacities for conducting heat afcertained by experiment in different articles ufed for clothing, by J. Sennebier.

4. Of a new fpecies of hawkweed, *crepis*, to which are added fome cryptogamiæ of Piedmont, by J. Baptift Balbis.

A figure of this plant, which Mr. B. calls *crepis ambigua*, is given. Among the cryptogamiæ are the following new fpecies, *mucor flofculentus*, *peziza amentacea*, *lichen nivalis*. Thefe likewife are figured.

5. Experiments on the effects of the nitric and oxigenated muriatic acid, employed topically in the treatment of various difeafes, by Mr. Roffi. Mr. R. gives an account of the cure of feveral gangrenous ulcers, venereal buboes, and even contagious carbuncles, cured by the application of thefe acids.

6. Meteorological obfervations made during the folar eclipfe on the 30th of Jan. 1805, at the obfervatory of Turin, with reflections on them, by Ant. Mar. Vaffali Eandi.

7. On a fpecies of caffia, that may be fubftituted for the fenna of the fhops, by Mr. Bellardi. This is the *caffia marilandica*, which Mr. B. would call *fuccedanea*, becaufe, according to him, it may fupply the place of the *caffia lanceolata*.

8. Inquiries into the nature of the galvanic fluid, by A. M. Vaffali Eandi.

9. On the mines of plumbago in the departments of the Sture and the Po, by Mr. Bonvoifin.

10. Attempts to improve nut oil,by the fame. Mr. B. points out a method of purifying this oil, and rendering it as fit for lamps as other fine oils.

11. Examination of the action of the galvanic fluid on different gafes, by J. A. Giobert.

12. An anatomical and phyfiological effay on the lymphatic glands, by profeffor Roffi.

13. Solution of a problem depending on the theory of permutations and combinations, by profeffor Balbo.

14. Explanation of the circumftance of a fifh being occafionally found with prickles in the river of the 27th military

 divifion,

division, by Mr Giorna. This fish is the *cyprinus idus*; the male only has prickles, and loses them after spawning time. Memoirs of the Imperial Academy of Sciences, &c. of Turin.

15. A chemico-medical essay on the pulmonary consumption, by Jos. Hyac. Rizzetti. The principal subject of this essay is the nature of the matter expectorated.

The following papers are by foreign members.

1. Memoir on the use of varying the constant quantity in summing up equations with variable coefficients, by Dr. Brunacci.

2. A systematical enumeration of the coleoptera found in the territory of Saluzzo, with observations, by Law. Ponza. To this catalogue are annexed two plates, containing the following new species. *Coccinella numeralis,—c. obsoleta,—curculio spinosus,—c. dubius,—c. rugosus,—cerambyx præustus,—c. melanocephalus,—chrysomela melanocephala,—ch. variegata,—ch. pretiosa,—ch. luctuosa,—scarabæus rufescens,—cantharis impressifrons,—attelabus funereus,—dytiscus silphoides,—tenebrio rufus,—birrhus rossii,—carabus attenuatus,—c. metallicus,—c. rossii,—forficula bipunctata,—silpha sinuata,—s. scabra.*

3. On the motion of the hairs of the *hypnum adiantoides*, by Palamedas de Suffren. Parts endued with irritability had already been observed in the hairs of some mosses. Mr. De S. has found it in those of the *h. a.* and describes all the singularities of the phenomenon. This paper is accompanied with a plate.

4. Of a resin employed by the bee in constructing its combs. By Fr. Mouxy Deloche.

5. Entomological observations; by Mr. Disderi. Mr. D. first sketches the history of the silkworm; and then proceeds to certain hymenoptera, chiefly of the *genera tenthredo, ichneumon, sphex, et vespa.*

6. Specimen of the fungi of the vale of Pisa, by Hugh Camino. The new species are figured on three plates. They are Agaricus elatior: a. miniatus: a. pezizoides: a. astrosanguineus: a. tricolor: Boletus scobinaceus: Helvella grandis: h. reflexa: h. inflata: Peziza achracea: p. pyriformis: Reticularia rosea: Mucor fruticulosus.

7. Observations on the native gold found among sand, by Lew. Bossi, of Milan.

To

Barometer.

MY correfpondent from Edinburgh is reminded, with regard to his project for a barometer, that no enlargement or diminution of the bore will make the leaft difference in the fcale of the common barometer, confifting of a tube or veffel, clofed above, and having its lower end open, and communicating with a bafon of mercury of confiderable diameter.

Subdivifion of an arc by wheel and chain.

The contrivance, received fome time ago from T. I. for making an aftronomical inftrument, in which the angular quantities fhall be meafured by the communication of a chain, ftrap, or ftring, poffeffes fo much ingenuity and promife, that it has exercifed the heads and hands of a number of eminent men. Among thefe are Robert Hooke, for a quadrant; Mufchenbroeck, for a pyrometer, and many operative men, fuch as Siffon and others, for theodolites and quadrants.—Where the intention of the inftrument is fimply to magnify the motion, without any particular attention to precifion, the contrivance has a happy effect; particularly in public lectures, where a number of fpectators may obferve the fame effect at the fame time. It is likewife cheap, and may be carried into effect in fituations where the ufe and application of more accurate apparatus cannot be referred to.

It cannot be made very exact.

A flight attention to the fubject, will fhew that all contrivances of the kind here alluded to muft be confiderably inaccurate. For they demand, 1, that the wheels fhould be very truly circular: 2, and free from all dirt and impurity: 3. that they be well centered: 4. that the chain or ftring fhould be every where of the fame thicknefs: 5, and its tenfion in all pofitions alike, &c. &c. If the quantity of error, taken at a minimum, which muft arife from thefe and other caufes, be attended to, it will be found that a fimple divifion of an arc (fubdivided by a fcrew or a nonius) and examined or read off by a fmall magnifier, will afford greater precifion; even when the work is performed by a careful defigner, who is no mathematical inftrument maker. It is certain that much greater delicacy and precifion may be had in the divifion of mathematical inftruments by the patient diligence of a cultivator of practical mechanics than is generally fuppofed.

A

JOURNAL

OF

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AND

THE ARTS.

SUPPLEMENT TO *VOL. XIII.*

ARTICLE I.

On the Saline Efflorefcences upon Walls; Salivary Concretions; Deflagration of Mercury by Galvanifm; Biliary Calculi; and the freezing Point of Spermaceti. By John Bostock, *M. D.*

To Mr. NICHOLSON.

SIR,

IN the third and fixth volumes of your Journal you have inferted an account of fome experiments that I performed on the faline efflorefcences found upon walls. I have lately had an opportunity of examining two other fpecimens, of which I now fend you the particulars. The firft was obtained in confiderable quantity from the inner walls of a warehoufe that had been erected about twenty years. By a feries of fimple experiments, which it is unneceffary to detail at full length, I found it to be a fulphate of foda, which, as in the former cafes, feemed to exift in a ftate of almoft perfect purity. The circumftances attending the fecond of thefe efflorefcences were more fingular. It was given me by a friend who had fcraped it from off the ftones which are fituated on the infide of the weft aifle of York Minfter. My friend, on whofe ac-

Examination of two fpecimens of efflorefcence found upon walls.

The firft was fulphate of foda;

The fecond was fcraped from the

face of the ftone withinfide York Minfter, and not from the mortar.

curacy I place the fulleft confidence, exprefsly ftated, that was taken from the furface of the ftone itfelf, and not from the joints, or any part that had been covered with mortar. It exifted there in large quantity, and was difpofed in the form of projecting fpiculæ. Upon fubjecting it to the ufual trials, I found it to be a very pure fulphate of magnefia. In order to afcertain with precifion the degree of its purity, I prepared a quantity of the fulphate of magnefia, by uniting together its conftituent parts. This artificial falt, and the falt from York, after being cryftallized, were expofed for fome time to the fame degree of heat, and when all the water of cryftallization appeared to be expelled, equal weights of them were diffolved in equal weights of water: 100 grains of thefe folutions had the muriate of barytes refpectively added, until no farther precipitation was produced, when it appeared that exactly the fame weight of barytes was neceffary to faturate each folution. The portions of precipitated fulphate of barytes were collected and dried, and when examined by a nice balance, exhibited fcarcely any perceptible difference in weight; they each amounted to 7.9 grains. A fimilar procefs being adopted with refpect to the common Epfom falt of the fhops, the precipitate was found to be 7.35 grains only. Before I quit this fubject, I may remark that another friend, in vifiting the cathedral at Tewkefbury, noticed a faline efflorefcence on the infide of fome part of that building; he collected a portion of it, intending to give it me for examination; but it was accidentally loft. Perhaps fome of your readers, who refide in that neighbourhood, may be induced to examine it, and tranfmit the refult to your Journal. I confefs myfelf totally unable to explain the production of the fulphate of magnefia on the furface of a freeftone, fuch as, I believe, forms the body of York Minfter.

It was fulphate of magnefia, very pure.

Qu. Whence came the magnefia?

Account of a concretion in the falivary ducts.

Among the folid concretions which are formed in different parts of the human body, thofe from the falivary ducts are occafionally met with. I lately procured one of thefe fubftances, of which I will give you a brief account. It was a cylinder, pointed at one end, of half an inch in length, and fomewhat more than $\frac{1}{12}$ of an inch in diameter; it weighed $1\frac{1}{2}$ gr. It was white and fmooth on the outfide, and its internal fracture did not exhibit any marks of regular organization. To half a grain of the concretion a few drops of diluted muriatic

muriatic acid were added; no effervefcence was excited. By the application of a gentle heat the whole was diffolved, except a few films that fwam in the fluid. A copious precipitation was produced in this folution by pure ammonia, but none by the carbonate of ammonia. A part of the muriatic folution was evaporated; the refidue was not foluble in water, but was fpeedily re-diffolved by the muriatic acid. The muriatic folution, faturated with the carbonate of ammoniac, had a precipitation produced by the oxalate of ammoniac. It appears therefore that the concretion confifted of the phofphate of lime, mixed with a little animal matter, probably coagulated albumen; it did not contain any carbonate of lime, and its component parts appeared not to poffefs any regularly organized ftructure. M. Fourcroy * and Dr. Thomfon † have examined fimilar bodies, and agree in confidering the earthy matter to be the phofphate of lime; we may therefore reafonably conclude that this fubftance always compofes the earthy part of the falivary concretions. I am difpofed, however, to differ from thefe diftinguifhed chemifts in my idea refpecting the nature of the animal matter which enters into their compofition; M. Fourcroy confiders it as confifting of a fpecies of mucilage, while Dr. Thomfon defcribes it as "a membranous fubftance, which retains the fhape of the concretion after the folution of the phofphate." This was certainly not the cafe with the one which I examined. I am difpofed to confider the animal matter as coagulated albumen, rather than mucus, in confequence of its infoluble nature, and the greater facility with which it would on this account be detained by the phofphate of lime.

It was phofphate of lime chiefly,

—with coagulated albumen.

The power which the electric fluid poffeffes, when generated by the galvanic apparatus, of burning metallic plates, affords one of the moft beautiful experiments of which the fcience of chemiftry can boaft. All the metals have by this means been fubjected to combuftion, except mercury, which, owing to its fluidity, is incapable of being formed into thin laminæ. ‡ I have, however, been fortunate enough to accomplifh this object, and that by the moft fimple method.

Mercury has not heretofore been deflagrated by galvanifm.

* Syftème, IX. 368. † Chemiftry, IV. 658.
‡ Thomfon's Chemiftry, I. 125.

I was performing fome experiments with Mr. Richard Dalton, an ingenious lecturer in natural philofophy of this place, with a pile compofed of 60 pair of fix-inch plates of zinc and copper, when it occurred to me to place a minute globule of mercury in an iron fpoon, refting on the top of the pile, and to approach to it a thick iron wire connected with the other end of the apparatus; the effect was, that a brilliant ftar of light was produced from the mercury, attended with a crackling noife and a copious emiffion of fparks; the mercury was found converted into the black oxide.

Experiment in which this was effected.

The dark coloured particles of gall ftones do not appear to be infpifated refin of the bile.

The moft common fpecies of biliary calculus is that compofed of the peculiar cryftalline matter, which in fome of its properties refembles fpermaceti, through which are interfperfed a number of dark coloured particles, that are fuppofed to confift of hardened bile. This is the idea entertained by M. Fourcroy,* and the one which I adopted, when I made the experiments on this fubject which are related in the fourth volume of your Journal. I have, however, fince that time been difpofed to alter my opinion; in two fpecimens of the biliary calculi, which I examined, after feparating the cryftalline matter by alcohol, I was unable to diffolve the dark coloured particles by any menftruum which I applied to them; they imparted a yellowifh tinge to water and other fluids, but the great bulk of their fubftance remained unchanged. It is, I conceive, not probable that the mere infpiffation of the refin of the bile could fo far alter its properties. I mention this circumftance principally with a view of attracting the attention of any of your readers who may be in poffeffion of a number of gall-ftones, fo as to afcertain whether the untractable nature of thefe particles is a general property of the cyftic-adipobilious concretions, or fomething peculiar to the fpecimens upon which I experimented.

Melting point of fpermaceti. Repetition of experiment confirms that it is a little above 112°.

I fhall conclude this mifcellaneous letter with fome remarks upon the melting point of fpermaceti. In the paper to which I have already referred, I mentioned the diverfity of opinion that had been entertained on this fubject, and afterwards ftated that my own experience induced me to fix it at the 112th degree. Dr. Thomfon, in the firft volume of his Chemiftry, fixes the melting point at 133°,† while in the fourth he ftates

* Syfteme, X. 59.

† Page 358.

it

it to be 112°, upon the authority of my paper.* Yet in his anſwer to the Edinburgh reviewers, he has mentioned this eſtimate of the melting point of ſpermaceti as one of his acknowledged errors, and upon the authority of Dr. Gibbes, fixes it at 115°. This circumſtance determined me to repeat the experiment; I employed a very delicate thermometer, and uſed every requiſite precaution; the reſult was that the inſtrument deſcended to *a little above the 112th degree,* and remained ſtationary until the ſubſtance was become ſolid. I may add that Dr. Irvine, in ſome experiments related in the ninth vol. of your Journal, fixes the point at 113°, which agrees ſo nearly with my obſervations, as to afford me an additional confidence in their accuracy.

I am, Sir,

Your obedient ſervant,

JOHN BOSTOCK.

Liverpool, April 9, 1806.

II.

Inveſtigation of the Temperature at which Water is of greateſt Denſity, from the Experiments of Dr. Hope on the Contraction of Water by Heat at low Temperatures. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

IN your Journal for February 1805 was inſerted a letter of mine containing certain facts relative to the ſubject of my preſent communication, which led me to diſbelieve the common opinion that water is denſeſt at 40°, and inclined me to think it is at 32°. Since that time my attention has again been turned to the ſubject; ſome ſmall but immaterial corrections of the facts have been made and additional ones obtained, by which I have been enabled to demonſtrate, at leaſt to my own ſatisfaction, that the temperature at which water is of greateſt denſity is at or near 36° of Fahrenheit. The reſults have lately been communicated to the Mancheſter Society, and may

Reference to a former communication, in which the maximum denſity of water was taken at 32°.

Preſent inference that it is at 36°.

* Page 512.

perhaps

perhaps appear in a future volume. My prefent object is to fhew that the refults of Dr. Hope's experiments are explicable on the fuppofition of water being denfeft at 36°, but on no other.

Obfervations on the expanfions of water on each fide of its point of greateft denfity.

Dr. Hope and myfelf concur in the opinion that water is denfeft at fome one point of temperature, and that above and below that point it expands alike by heat and cold in a gradually increafing manner. De Luc was the firft to obferve that the expanfion is the fame quantity for the fame number of degrees, whether of increafe or diminution of temperature; the remarkable fact was extended by my former experience from a range of 8° to 25° or more, above and below the ftationary point. I have lately examined this fact with greater attention to precifion than formerly, and find that it is accurate, except that the expanfion for degrees *below* the ftationary point is always fomewhat more than for a corresponding number of degrees *above* the faid point. Thus, water is ftationary in a glafs thermometer at 42°; if heated to 75° by the mercurial fcale, it expands very confiderably; if plunged into a frigorific mixture of 13°, it falls to 42°, and then expands again to the fame point of 75°, at which it remains ftationary as long as continued in the mixture. It may be remarked too, that congelation rarely if ever takes place in the bulb, when the mixture is not below 15°, which may eafily be procured by putting fnow into water faturated with common falt. Hence we fee that 29° below, afford the fame expanfion as 33° above the ftationary point. This, I imagine, is occafioned by the error attached to the equal divifion of the mercurial fcale. For a fmall number of degrees, however, we may admit that the expanfions for correfponding intervals above and below are equal; hence we obtain the following table of correfponding temperatures at which water is of the fame denfity.

	Suppofing greateft denfity at 40°			at 36°
Correfponding denfities will be at	39° and 41°		Correfponding denfities will be at	35° and 37°
	38 —— 42			34 —— 38
	37 —— 43			33 —— 39
	36 —— 44			32 —— 40
	35 —— 45			31 —— 41
	34 —— 46			30 —— 42
	33 —— 47			29 —— 43
	32 —— 48			28 —— 44

Dr.

Dr. Hope alſo admits with me the fact that water ſubjected to be cooled without agitation in a frigorific mixture, uſually deſcends ſeveral degrees below the freezing point, and ſtill retains its liquidity. Though it is eaſy to obtain water in a glaſs bulb 20 or 25° below freezing, I could never cool water in an open jar more than 10 or 11° below freezing, agreeable to the experience of Sir Charles Blagden. But I find water in ſuch circumſtances will admit of being cooled to 25°, and the bulb of a thermometer to be immerſed and withdrawn ſeveral times, without freezing.

The author and Dr. Hope concur that water may continue fluid below its freezing point.

We come now to the experiments of Dr. Hope.

Experiment I.

A jar eight inches deep and 4½ in diameter, filled with water of 32°, and placed on a table, &c. Air 60—62°. Two thermometers inſerted, one at the top, another at the bottom.

Dr. Hope's experiment quoted.

Top Thermometer.		Differences.	*Bottom Thermometer.*	Differences.
	32°	0	32°	
		1+		2+
In 10 min.	33+		34+	
		2.5−		3−
30 ——	35.5		37	
		1.5		1+
50 ——	37		38+	
		1		0
1 hour ——	38		38+	
		4		.25−
1 10 ——	42		38.25	
		2		1.75
1 30 ——	44		40	&c.

In the firſt interval of 10 minutes we obſerve the bottom thermometer to have gained 2°+, and the top only 1°+; the former has the heat which enters directly, together with the heat which deſcends by the ſide of the veſſel; the latter has only the heat which enters directly, and as theſe are nearly as one to two, we may infer that the acquiſition of direct heat, and heat by deſcent, are nearly equal in the bottom thermometer during that interval.

Inference that the water deſcends as it acquires heat,

In the next interval of 20 minutes we obſerve the bottom thermometer gains 3°−, and the top 2°,5−. Here we ſee the

the aſcending current ſtill continues, but has produced little effect, having not added more than half a degree to the temperature.

—until the temperature rather exceeds 36°.

During the next 20 minutes the top gains 1°.5, the bottom only 1° +. In this interval we may obſerve the current has turned, but not yet acquired much force. The point of greateſt denſity muſt therefore have exiſted at the laſt obſervation or near it: the mean of 35°.5 and 37° is $36\frac{1}{4}$° for the required point, as deduced from this experiment.

After which the heated water aſcends.

In 10 minutes more the top gains 1°, and the bottom little or nothing; here we find the aſcending current has become ſuch as to manifeſt its influence very ſenſibly.

In the next 10 minutes the top gains 4°, and the bottom only ,25; here the aſcending current has become quadruple what it was 2° below; becauſe the farther the temperature is raiſed above the ſtationary point, the more powerful is the force of aſcent ariſing from the ſame interval of temperature.

Theſe facts do not agree with the ſuppoſed maximum denſity at 39° or 40°.

It would be in vain to attempt to reconcile the above experiment with the opinion that water is denſeſt at 39° or 40°. At the very moment when the mean temperature of the water is 39°, we obſerve the *aſcending* current the moſt active, when it ought to have been *deſcending* or imperceptible.

The effect is not modified by the table or ſupport.

I once imagined that the experiment might be explained on the ſuppoſition of 32° being the point of greateſt denſity; that the ſudden increaſe of temperature at the bottom aroſe from the heat of the table, and that the coheſion of the particles of water prevented their aſcent under the propulſion of ſo ſmall a force; but having procured a large glaſs jar which could be ſuſpended, I found the ſame order of differences nearly as when placed on a table, and was therefore obliged to abandon that explanation.

Intending to ſend the remainder of this inveſtigation for a future number, I remain

Your friend,

JOHN DALTON.

Mancheſter, April 14, 1806.

III. *Account*

III.

Account of a Series of Experiments, ſhewing the Effects of Compreſſion in modifying the Action of Heat. By Sir James Hall, *Bart. F. R. S. Edinburgh.*

(Concluded from Page 328.)

Section III.

Experiments made in Tubes of Porcelain.—Tubes of Wedgwood's Ware.—Methods uſed to confine the carbonic Acid, and to cloſe the Pores of the Porcelain in a horizontal Apparatus.—Tubes made with a View to theſe Experiments.—The vertical Apparatus adopted.—View of Reſults obtained, both in Iron and Porcelain.—The Formation of Lime-ſtone and Marble.—Inquiry into the Cauſe of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking—Experiments with Porcelain Tubes proved to be limited.

WHILE I was carrying on the above-mentioned experiments, I was occaſionally occupied with another ſet, in tubes of porcelain. So much, indeed, was I prepoſſeſſed in favour of this laſt mode, that I laid gun-barrels aſide, and adhered to it during more than a year. The methods followed with this ſubſtance differ widely from thoſe already deſcribed, though founded on the ſame general principles. Set of experiments in tubes of porcelain.

I procured from Mr. Wedgwood's manufactory at Etruria, in Staffordſhire, a ſet of tubes for this purpoſe, formed of the ſame ſubſtance with the white mortars, in common uſe, made there. Theſe tubes were fourteen inches long, with a bore of half an inch diameter, and thickneſs of 0.2; being cloſed at one end (figs. 9, 10, 11, 12, 13.) *Pl.* XI.

I propoſed to ram the carbonate of lime into the breech (*Fig.* 9. A); then filling the tube to within a ſmall diſtance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the ſhops (borat of ſoda) previouſly reduced to glaſs, and then pounded; to apply heat to the muzzle alone, ſo as to convert that borax into ſolid glaſs; then, reverſing the operation, to keep the muzzle cold, and apply the requiſite heat to the carbonate lodged in the breech. They were cloſed at one end and the other aperture was ſtopped with glaſs of borax.

I thus expected to confine the carbonic acid; but the attempt was attended with conſiderable difficulty, and has led Difficulties of this proceſs.

 to

to the employment of various devices, which I ſhall now ſhortly enumerate, as they occurred in the courſe of practice. The ſimple application of the principle was found inſufficient, from two cauſes: Firſt, The carbonic acid being driven from the breech of the tube, towards the muzzle, among the pores of the pounded ſilex, eſcaped from the compreſſing force, by lodging itſelf in cavities which were comparatively cold: Secondly, The glaſs of borax, on cooling, was always found to crack very much, ſo that its tightneſs could not be depended on.

And the method of partly obviating them.

To obviate both theſe inconveniences at once, it occurred to me, in addition to the firſt arrangement, to place ſome borax (*Fig*. 10. C) ſo near the breech of the tube, as to undergo heat along with the carbonate (A); but interpoſing between this borax and the carbonate, a ſtratum of ſilex (B), in order to prevent contamination. I truſted that the borax in a liquid or viſcid ſtate, being thruſt outwards by the expanſion of the carbonic acid, would preſs againſt the ſilex beyond it (D), and totally prevent the elaſtic ſubſtances from eſcaping out of the tube, or even from wandering into its cold parts.

In ſome reſpects, this plan anſwered to expectation. The glaſs of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued viſcid maſs in the loweſt red-heat; and as the ſtreſs in theſe experiments begins only with redneſs, the borax being heated at the ſame time with the carbonate, becomes united and impervious, as ſoon as its action is neceſſary. Many good reſults were accordingly obtained in this way. But I found, in practice, that as the heat roſe, the borax began to enter into too thin fuſion, and was often loſt among the pores of the ſilex, the ſpace in which it had lain being found empty on breaking the tube. It was therefore found neceſſary to oppoſe ſomething more ſubſtantial and compact, to the thin and penetrating quality of pure borax.

Bottle glaſs was found much preferable to pure borax for the purpoſe of reſtraining the carbonic acid.

In ſearching for ſome ſuch ſubſtance, a curious property of bottle-glaſs occurred accidentally. Some of this glaſs, in powder, having been introduced into a muffle at the temperature of about 20° of Wedgwood; the powder, in the ſpace of about a minute, entered into a ſtate of viſcid agglutination, like that of honey, and in about a minute more, (the heat always continuing unchanged), conſolidated into a firm and compact

pact mass of *Reaumur's porcelain**. It now appeared, that by placing this substance immediately behind the borax, the penetrating quality of this last might be effectually restrained; for, Reaumur's porcelain has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of consolidation, the pounded bottle-glass shrunk, so as to leave an opening between its mass and the tube, through which the borax, and, along with it, the carbonic acid, was found to escape. But the object in view was obtained by means of a mixture of pounded bottle-glass, and pounded flint, in equal parts. This compound still agglutinates, not indeed into a mass so hard as Reaumur's porcelain, but sufficiently so for the purpose; and this being done without any sensible contraction, an effectual barrier was opposed to the borax; (this arrangement is shewn in *Fig.* 11.); and thus the method of closing the tubes was rendered so complete, as seldom to fail in practice †. A still further refinement upon this method was found to be of advantage. A second series of powders, like that already described, was introduced towards the muzzle, (as shewn in *Fig.* 12). During the first period of the experiment, this last-mentioned series was exposed to heat, with all the outward half of the tube *(a b)*; and by this means, a solid mass was produced, which remained cold and firm during the subsequent action of heat upon the carbonate.

Improvements on this method.

I soon found, that notwithstanding all the above-mentioned precautions, the carbonic acid made its escape, and that it pervaded the substance of the Wedgwood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating state of fusion, applied to the inside of the tube; and that the pores of the porcelain might thus be closed, as those of leather are closed by oil, in an air-pump. In this view, I rammed the carbonate into a small tube, and surrounded it with pounded glass of borax, which, as soon as the heat was applied, spread on the in-

Remedy for porosity in the earthen tubes.

* In the same temperature, a mass of the glass of equal bulk would undergo the same change; but it would occupy an hour.

† A substance equally efficacious in restraining the penetrating quality of borax, was discovered by another accident. It consists of a mixture of borax and common sand, by which a substance is formed, which, in heat, assumes the state of a very tough paste, and becomes hard and compact on cooling.

side

ſide of the large tube, and effectually cloſed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffles, (the arrangement juſt deſcribed being repreſented in *Fig.* 13.)

Beſt material for tubes.

I was thus enabled to carry on experiments with this porcelain, to the utmoſt that its ſtrength would bear. But I was not ſatisfied with the force ſo exerted; and hoping to obtain tubes of a ſuperior quality, I ſpent much time in experiments with various porcelain compoſitions. In this, I ſo far ſucceeded, as to produce tubes by which the carbonic acid was in a great meaſure retained without any internal glaze. The beſt material I found for this purpoſe, was the pure porcelain-clay of Cornwall, or a compoſition in the proportion of two of this clay to one of what the potters call *Corniſh-ſtone,* which I believe to be a granite in a ſtate of decompoſition. Theſe tubes were ſeven or eight inches long, with a bore tapering from 1 inch to 0.6. Their thickneſs was about 0.3 at the breech, and tapered towards the muzzle to the thinneſs of a wafer.

Improvement by placing the tube vertically.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally, as before. By obſerving the thin ſtate of borax whilſt in fuſion, I was convinced, that it ought to be treated as a complete liquid, which being ſupported in the courſe of the experiment from below, would ſecure perfect tightneſs, and obviate the failure which often happened in the horizontal poſition, from the falling of the borax to the lower ſide.

Particular deſcription of the proceſs.

In this view, (fig. 16.) I filled the breech in the manner deſcribed above, and introduced into the muzzle ſome borax (C) ſupported at the middle of the tube by a quantity of ſilex mixed with the bottle glaſs (B). I placed the tube, ſo prepared, with its breech plunged into a crucible filled with ſand (E), and its muzzle pointing upwards. It was now my object to apply heat to the muzzle-half, whilſt the other remained cold. In that view, I conſtructed a furnace (figs. 14 and 15.) having a muffle placed vertically (*c d*,) ſurrounded on all ſides with fire (*e e*), and open both above (at *c*), and below (at *d*). The crucible juſt mentioned, with its tube, being then placed on a ſupport directly below the vertical muffle, (as repreſented in fig. 14. at F) it was raiſed, ſo that the half of the tube next the muzzle was introduced into the

the fire. In confequence of this, the borax was feen from above to melt, and run down in the tube, the air contained in the powder efcaping in the form of bubbles, till at laft the borax ftood with a clear and fteady furface like that of water. Some of this falt being thrown in from above, by means of a tube of glafs, the liquid furface was raifed nearly to the muzzle, and, after all had been allowed to become cold, the pofition of the tube was reverfed; the muzzle being now plunged into the fand, (as in fig. 17.) and the breech introduced into the muffle. In feveral experiments, I found it anfwer well, to occupy great part of the fpace next the muzzle, with a rod of fand and clay previoufly baked, (fig. 19. K K), which was either introduced at firft, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid ftate. In many cafes I affifted the compactnefs of the tube by means of an internal glaze of borax; the carbonate being placed in a fmall tube, (as fhewn in fig. 18.)

Thefe devices anfwered the end propofed. Three-fourths of the tube next the muzzle was found completely filled with a mafs, having a concave termination at both ends, (*f* and *g* figs. 17, 18, 19.) fhewing that it had ftood as a liquid in the two oppofite pofitions in which heat had been applied to it. So great a degree of tightnefs indeed was obtained in this way, that I found myfelf fubjected to an unforefeen fource of failure. A number of the tubes failed, not by explofion, but by the formation of a minute longitudinal fiffure at the breech, through which the borax and carbonic acid efcaped. I faw that this arofe from the expanfion of the borax when in a liquid ftate, as happened with the fufible metal in the experiments with iron-barrels; for, the crevice here formed, indicated the exertion of fome force acting very powerfully, and to a very fmall diftance. Accordingly, this fource of failure was remedied by the introduction of a very fmall air-tube. This, however, was ufed only in a few experiments.

Effect of expanfion in the fufed borax upon the tubes.

In the courfe of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above defcribed, amounting, together with thofe made in gun barrels, to one hundred and fifty-fix. In an operation fo new, and in which the apparatus was ftrained to the utmoft of its power, conftant fuccefs could not be expected, and in fact many experiments failed, wholly or partially. The refults, however,

Thefe experiments were upon the whole fuccefsful.

2 upon

upon the whole, were satisfactory, since they seemed to establish some of the essential points of this inquiry.

These experiments prove, that, by mechanical constraint, the carbonate of lime can be made to undergo strong heat, without calcination, and to retain almost the whole of its carbonic acid, which, in an open fire, at the same temperature, would have been entirely driven off: and that, in these circumstances, heat produces some of the identical effects ascribed to it in the Huttonian Theory.

Pounded carbonate of lime in its several varieties became agglutinated into stony masses.

By this joint action of heat and pressure, the carbonate of lime which had been introduced in the state of the finest powder, is agglutinated into a firm mass, possessing a degree of hardness, compactness, and specific gravity * nearly approaching to these qualities in a sound limestone; and some of the results, by their saline fracture, by their semitransparency, and their susceptibility of polish, deserve the name of marble.

The same trials have been made with all calcareous substances; with chalk, common limestone, marble, spar, and the shells of fish. All have shewn the same general property, with some varieties as to temperature. Thus, I found, that, in the same circumstances, chalk was more susceptible of agglutination than spar; the latter requiring a heat two degrees higher than the former, to bring it to the same pitch of agglutination.

The chalk used in my first experiments, always assumed the character of a yellow marble, owing probably to some slight contamination of iron. When a solid piece of chalk, whose bulk had been previously measured in the gauge of Wedgwood's pyrometer was submitted to heat under compression, its contraction was remarkable, proving the approach of the particles during their consolidation; on these occasions, it was found to shrink three times more than the pyrometer-pieces in the same temperature. It lost, too, almost entirely, its power of imbibing water, and acquired a great additional specific gravity. On several occasions, I observed, that masses of chalk, which, before the experiment, had shewn one uniform character of whiteness, assumed a stratified appearance, indicated by a series of parallel layers of a brown colour. This

* See Appendix.

circumſtance may hereafter throw light on the geological hiſtory of this extraordinary ſubſtance.

I have ſaid, that, by mechanical conſtraint, almoſt the whole of the carbonic acid was retained. And, in truth, at this period, ſome loſs of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumſtance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

And moſt of the carbonic acid was retained.

When the loſs exceeded 10 or 15 *per cent.* * of the weight of the carbonate, the reſult was always of a friable texture, and without any ſtony character; when leſs than two or three *per cent.* it was conſidered as good, and poſſeſſed the properties of a natural carbonate. In the intermediate caſes, when the loſs amounted, for inſtance, to ſix or eight *per cent.* the reſult was ſometimes excellent at firſt, the ſubſtance bearing every appearance of ſoundneſs, and often poſſeſſing a high character of cryſtallization; but it was unable to reſiſt the action of the air; and, by attracting carbonic acid or moiſture, or both, crumbled to duſt more or leſs rapidly, according to circumſtances. This ſeems to prove, that the carbonate of lime, though not fully ſaturated with carbonic acid, may poſſeſs the properties of limeſtone; and perhaps a difference of this kind may exiſt among natural carbonates, and give riſe to their different degrees of durability.

Qualities of the product differing according to the loſs of carbonic acid.

I have obſerved, in many caſes, that the calcination has reached only to a certain depth into the maſs; the internal part remaining in a ſtate of complete carbonate, and, in general, of a very fine quality. The partial calcination ſeems thus to take place in two different modes. By one, a ſmall proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the reſt is left entire. Perhaps one reſult is the effect of a feeble calcining cauſe, acting during a long time, and the other of a ſtrong cauſe, acting for a ſhort time.

Some of the reſults which ſeemed the moſt perfect when firſt produced, have been ſubject to decay, owing to partial calcination. It happened, in ſome degree, to the beautiful

Some reſults were ſubject to decay from partial calcination.

* I have found, that, in open fire, the entire loſs ſuſtained by the carbonate varies in different kinds from 42 to 45.5 *per cent.*

ſpecimen

ſpecimen produced on the 3d of March, 1801, though a freſh fracture has reſtored it.

A ſpecimen, too, of marble, formed from pounded ſpar, on the 15th of May, 1801, was ſo complete as to deceive the workman employed to poliſh it, who declared, that, were the ſubſtance a little whiter, the quarry from which it was taken would be of great value, if it lay within reach of a market. Yet, in a few weeks after its formation, it fell to duſt.

Very many were durable marble.

Numberleſs ſpecimens, however, have been obtained, which reſiſt the air, and retain their poliſh as well as any marble. Some of them continue in a perfect ſtate, though they have been kept without any precaution during four or five years. That ſet, in particular, remain perfectly entire, which were ſhewn laſt year in this Society, though ſome of them were made in 1799, ſome in 1801 and 1802, and though the firſt eleven were long ſoaked in water, in the trials made of their ſpecific gravity.

Remarkable fact.

A curious circumſtance occurred in one of theſe experiments, which may hereafter lead to important conſequences. Some ruſt of iron had accidentally found its way into the tube: 10 grains of carbonate were uſed, and a heat of 28° was applied. The tube had no flaw; but there was a certainty that the carbonic acid had eſcaped through its pores. When broken, the place of the carbonate was found occupied, partly by a black ſlaggy matter, and partly by ſphericles of various ſizes, from that of a ſmall pea downwards, of a white ſubſtance, which proved to be quicklime; the ſphericles being interſperſed through the ſlag, as ſpar and agates appear in whinſtone. The ſlag had certainly been produced by a mixture of the iron with the ſubſtance of the tube; and the ſpherical form of the quicklime ſeems to ſhew, that the carbonate had been in fuſion along with the ſlag, and that they had ſeparated on the eſcape of the carbonic acid.

The ſubject was carried thus far in 1803, when I ſhould probably have publiſhed my experiments, had I not been induced to proſecute the inquiry by certain indications, and accidental reſults, of a nature too irregular and uncertain to meet the public eye, but which convinced me, that it was poſſible to eſtabliſh by experiment the truth of all that was hypothetically aſſumed in the Huttonian theory.

Endeavour to improve the experiments by

The principal object was now to accompliſh the entire fuſion of the carbonate, and to obtain ſpar as the reſult of that fuſion

fusion, in imitation of what we conceive to have taken place in nature.

preventing all calcination, and perfectly fusing the carbonate.

It was likewise important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itself, and on account of its consequences; the result being visibly improved by every approach towards complete saturation. I therefore became anxious to investigate the cause of the partial calcinations which had always taken place, to a greater or less degree, in all these experiments. The question naturally suggests itself, What has become of the carbonic acid, separated in these partial calcinations from the earthy basis? Has it penetrated the vessel, and escaped entirely, or has it been retained within it in a gaseous, but highly compressed state? It occurred to me, that this question might be easily resolved, by weighing the vessel before and after the action of heat upon the carbonate.

By experiment it was found that iron barrels vitiated the result.

With iron, a constant and inappreciable source of irregularity existed in the oxidation of the barrel. But with porcelain the thing was easy; and I put it in practice in all my experiments with this material, which were made after the question had occurred to me. The tube was weighed as soon as its muzzle was closed, and again, after the breech had been exposed to the fire; taking care, in both cases, to allow all to cool. In every case, I found some loss of weight, proving, that even in the best experiments, the tubes were penetrated to a certain degree. I next wished to try if any of the carbonic acid separated, remained within the tube in a gaseous form; and in that view, I wrapt the tube, which had just been weighed, in a sheet of paper, and placed it, so surrounded, on the scale of the balance. As soon as its weight was ascertained, I broke the tube by a smart blow, and then replaced upon the scale the paper containing all the fragments. In those experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already escaped during the action of heat. But in the good results, I always found that a loss of weight was the consequence of breaking the tube.

With porcelain tubes this cause of irregularity existed along with the escape of the acid.

These facts prove, that both causes of calcination had operated in the porcelain tubes; that, in the cases of small loss, part of the carbonic acid had escaped through the vessel, and that part had been retained within it. I had in view methods by by which the laſt could be counteracted; bnt I ſaw no remedy for the firſt. I began, therefore, to deſpair of ultimate ſucceſs with tubes of porcelain *.

These laſt could not bear elevated heats.

Another circumſtance confirmed me in this opinion. I found it impracticable to apply a heat above 27° to theſe tubes, when charged as above with carbonate, without deſtroying them, either by exploſion, by the formation of a minute rent, or by the actual ſwelling of the tube. Sometimes this ſwelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate ſuſtaining but a very ſmall loſs. This ductility of the porcelain in a low heat is a curious fact, and ſhews what a range of temperature is embraced by the gradual tranſition of ſome ſubſtances from a ſolid to a liquid ſtate: For the ſame porcelain, which is thus ſuſceptible of being ſtretched out without breaking in a heat of 27°, ſtands the heat of 152°, without injury, when expoſed to no violence, the angles of its fracture remaining ſharp and entire.

IV.

Experiments in Gun-Barrels reſumed.—The Vertical Apparatus applied to them.—Barrels bored in ſolid Bars.—Old Sable Iron.—Fuſion of the Carbonate of Lime.—Its Action on Porcelain.—Additional Apparatus required in Conſequence of that Action.—Good Reſults; in particular, four Experiments, illuſtrating the Theory of Internal Calcination, *and ſhewing the Efficacy of the Carbonic Acid as a Flux.*

Experiments with gun-barrels reſumed,

SINCE I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expoſe the carbonate in them to ſtrong heats; I at laſt determined to lay them aſide, and return to barrels of iron, with which I had formerly obtained ſome good reſults, favoured, perhaps, by ſome accidental circumſtances.

* I am neverthelеſs of opinion, that, in ſome ſituations, experiments with compreſſion may be carried on with great eaſe and advantage in ſuch tubes. I allude to the ſituation of the geologiſts of France and Germany, who may eaſily procure, from their own manufactories, tubes of a quality far ſuperior to any thing made for ſale in this country.

On

On the 12th of February, 1803, I began a feries of experiments with gun-barrels, refuming my former method of working with the fufible metal, and with lead; but altering the pofition of the barrel from horizontal to vertical; the breech being placed upwards during the action of heat on the carbonate. This very fimple improvement has been productive of advantages no lefs remarkable, than in the cafe of the tubes of porcelain. In this new pofition, the included air, quitting the air-tube on the fufion of the metal, and rifing to the breech, is expofed to the greateft heat of the furnace, and muft therefore react with its greateft force; whereas, in the horizontal pofition, that air might go as far back as the fufion of the metal reached, where its elafticity would be much feebler. The fame difpofition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a veffel filled with water; which contributed very much both to the convenience and fafety of thefe experiments.

In a vertical pofition, with the breech upwards, and ftopped with fufible metal.

In this view, making ufe of the brick-furnace with the vertical muffle, already defcribed in page 384, I ordered a pit (*a a a fig.* 20.) to be excavated under it, for the purpofe of receiving a water-veffel. This veffel (reprefented feparately, *fig.* 21.) was made of caft iron; it was three inches in diameter, and three feet deep; and had a pipe (*d e*) ftriking off from it at right angles, four or five inches below its rim, communicating with a cup (*e f*) at the diftance of about two feet. The main veffel being placed in the pit (*a a*) directly below the vertical muffle, and the cup ftanding clear of the furnace, water poured into the cup flowed into the veffel, and could thus conveniently be made to ftand at any level. (The whole arrangement is reprefented in *fig.* 20.) The muzzle of the barrel (*g*) being plunged into the water, and its breech (*b*) reaching up into the muffle, as far as was found convenient, its pofition was fecured by an iron chain (*g f*). The heat communicated downwards generally kept the furface of the water (at *c*) in a ftate of ebullition; the wafte thus occafioned being fupplied by means of the cup, into which, if neceffary, a conftant ftream could be made to flow.

Defcription and drawing of the apparatus.

As formerly, I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel

 would

would admit, in order to exclude as much of the fufible metal as poffible; for the expanfion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the lefs rifk there was of deftroying the barrels.

Simple mode of withdrawing the contents from the tube.

In the courfe of practice, a fimple mode occurred of removing the metal and withdrawing the cradle: it confifted in placing the barrel with its muzzle downwards, fo as to keep the breech above the furnace and cold, while its muzzle was expofed to ftrong heat in the muffle. In this manner, the metal was difcharged from the muzzle, and the pofition of the barrel being lowered by degrees, the whole metal was removed in fucceffion, till at laft the cradle and its contents became entirely loofe. As the metal was delivered, it was received in a crucible, filled with water, ftanding on a plate of iron placed over the pit, which had been ufed, during the firft ftage of the experiment, to contain the water-veffel. It was found to be of fervice, efpecially where lead was ufed, to give much more heat to the muzzle than fimply what was required to liquefy the metal it contained; for when this was not done, the muzzle growing cold as the breech was heating, fome of the metal delivered from the breech was congealed at the muzzle, fo as to ftop the paffage.

According to this method, many experiments were made in gun-barrels, by which fome very material fteps were gained in the inveftigation.

Experiment in the new method.

On the 24th of February, I made an experiment with fpar and chalk; the fpar being placed neareft to the breech of the barrel, and expofed to the greateft heat, fome baked clay intervening between the carbonates. On opening the barrel, a long-continued hiffing noife was heard. The fpar was in a ftate of entire calcination; the chalk, though crumbling at the outfide, was uncommonly hard and firm in the heart. The temperature had rifen to 32°.

Internal calcination, where the carbonic acid did not efcape out of the apparatus.

In this experiment, we have the firft clear example, in iron barrels, of what I call *Internal Calcination*; that is to fay, where the carbonic acid feparated from the earthy bafis, has been accumulated in cavities within the barrel. For, fubfequently to the action of ftrong heat, the barrel had been completely cooled; the air therefore introduced by means of the air-tube, muft have refumed its original bulk, and by itfelf could have

have no tendency to rush out; the heat employed to open the barrel being barely sufficient to soften the metal. Since, then, the opening of the barrel was accompanied by the discharge of elastic matter in great abundance, it is evident, that this must have proceeded from something superadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at least, internal; the separation of the acid from the earthy matter being complete where the heat was strongest, and only partial where the intensity was less.

Part of the included carbonate was calcined, another part retaining its acid. Reasoning on this fact.

The chemical principles stated in a former part of this paper, authorised us to expect a result of this kind. As heat, by increasing the volatility of the acid, tended to separate it from the earth, we had reason to expect, that, under the same compression, but in different temperatures, one portion of the carbonate might be calcined, and another not: And that the least heated of the two, would be the least exposed to a change not only from want of heat, but likewise in consequence of the calcination of the other mass; for the carbonic acid disengaged by the calcination of the hottest of the two, must have added to the elasticity of the confined elastic fluid, so as to produce an increase of compression. By this means, the calcination of the coldest of the two might be altogether prevented, and that of the hottest might be hindered from making any further advancement. This reasoning seemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened some new practical views in these experiments, of which I availed myself without loss of time. If the internal calcination of one part of an inclosed mass, promotes the compression of other masses included along with it, I conceived that we might forward our views very much by placing a small quantity of carbonate, carefully weighed, in the same barrel with a large quantity of that substance; and by arranging matters so that the small fiducial part should undergo a moderate heat, while a stronger heat, capable of producing internal calcination, should be applied to the rest of the carbonate. In this manner, I made many experiments, and obtained results which seemed to confirm this reasoning, and which were often very satisfactory, though the heat did not always exert its greatest force where I intended it to do so.

On

Experiment. Carbonate partly fufed, and in part deprived of carbonic acid.

On the 28th of February, I introduced fome carbonate, accurately weighed, into a fmall porcelain tube, placed within a larger one, the reft of the large tube being filled with pounded chalk; thefe carbonates, together with fome pieces of chalk, placed along with the large tube in the cradle, weighing in all 195.7 grains. On opening the barrel, air ruflhed out with a long-continued hiffing noife. The contents of the little tube were loft by the intrufion of fome borax which had been introduced over the filex, in order to exclude the fufible metal. But the reft of the carbonate, contained in the large tube, came out in a fine ftate, being porous and frothy throughout; fparkling every where with facetts, the angular form of which was diftinguifhable in fome of the cavities by help of a lens: in fome parts the fubftance exhibited the rounding of fufion; in many it was in a high degree tranfparent. It was yellow towards the lower end, and at the other almoft colourlefs. At the upper end, the carbonate feemed to have united with the tube, and at the places of contract to have fpread upon it; the union having the appearance of a mutual action. The general mafs of carbonate effervefced in acid violently, but the thin ftratum immediately contiguous to the tube, feebly, if at all.

Similar experiment, in which the carbonate exhibited more remarkable facettes.

On the 3d of March, I introduced into a very clean tube of porcelain 36.8 of chalk. The tube was placed in the upper part of the cradle, the remaining fpace being filled with two pieces of chalk, cut for the purpofe; the uppermoft of thefe being excavated, fo as to anfwer the purpofe of an air-tube. The pieces thus added, were computed to weigh about 300 grains. There was no pyrometer ufed; but the heat was gueffed to be about 30°. After the barrel had ftood during a few minutes in its delivering pofition, the whole lead with the rod and cradle, were thrown out with a fmart report, and with confiderable force. The lowermoft piece of chalk had fcarcely been acted upon by heat. The upper part of the other piece was in a ftate of marble, with fome remarkable facettes. The carbonate, in the little tube, had fhrunk very much during the firft action of heat, and had begun to fink upon itfelf, by a further advancement towards liquefaction. The mafs was divided into feveral cylinders, lying confufedly upon each other; this divifion arifing from the manner in which the pounded chalk was rammed into the tube in fucceffive portions. In feveral ſeveral places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a ſoft ſubſtance; the globular form and ſhining ſurface of all theſe cavities, clearly indicating fuſion. The ſubſtance was ſemitranſparent; in ſome places yellow, and in ſome colourleſs. When broken, the ſolid parts ſhewed a ſaline fracture, compoſed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, ſo as to render it impoſſible to aſcertain what loſs had been ſuſtained. In general, the line of contact was of a brown colour; yet there was no room for ſuſpecting the preſence of any foreign matter, except, perhaps, from the iron-rod which was uſed in ramming down the chalk. But, in ſubſequent experiments, I have obſerved the ſame brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purpoſely rammed with a piece of wood; ſo that this colour, which has occurred in almoſt every ſimilar caſe, remains to be accounted for. The carbonate efferveſced violently with acid; the ſubſtance in contact with the tube, doing ſo, however, more feebly than in the heart, leaving a copious depoſit of white ſandy matter, which is doubtleſs a part of the tube, taken up by the carbonate in fuſion.

Another experiment with ſlow cooling.

Saline ſtructure and cryſtallization produced in chalk previouſly pounded.

On the 24th of March, I made a ſimilar experiment, in a ſtout gun-barrel, and took ſome care, after the application of heat, to cool the barrel ſlowly, with a view to cryſtallization. The whole maſs was found in a fine ſtate, and untouched by the lead; having a ſemitranſparent and ſaline ſtructure, with various facettes. In one part, I found the moſt decided cryſtallization I had obtained, though of a ſmall ſize: owing to its tranſparency it was not eaſily viſible, till the light was made to reflect from the cryſtalline ſurface, which then produced a dazzle, very obſervable by the naked eye; when examined by means of a lens, it was ſeen to be compoſed of ſeveral plates, broken irregularly in the fracture of the ſpecimen, all of which are parallel to each other, and reflect under the ſame angle, ſo as to unite in producing the dazzle. This ſtructure was obſervable equally well in both parts of the broken ſpecimen. In a former experiment, as large a facette was obtained in a piece of ſolid chalk; but this reſult was of more conſequence, as having been produced from chalk previouſly pounded.

The

The gun-barrels, though fuperior to porcelain were ftill too weak.

The foregoing experiments proved the fuperior efficacy of iron veffels over thofe of porcelain, even where the thicknefs was not great; and I perfevered in making a great many experiments with gun-barrels, by which I occafionally obtained very fine refults: but I was at laft convinced, that their thicknefs was not fufficient to enfure regular and fteady fuccefs. For this purpofe, it appeared proper to employ veffels of fuch ftrength, as to bear a greater expanfive force than was juft neceffary; fince, occafionally, (owing to our ignorance of the relation between the various forces of expanfion, affinity, tenacity, &c.) much more ftrain has been given to the veffels than was requifite. In fuch cafes, barrels have been deftroyed, which, as the refults have proved, had acted with fufficient ftrength during the firft ftages of the experiments, though they had been unable to refift the fubfequent overftrain. Thus, my fuccefs with gun-barrels, depended on the good fortune of having ufed a force no more than fufficient, to conftrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourfe to iron barrels of much greater ftrength, and tried various modes of conftruction.

Barrels formed by boring in folid bars of iron which proved excellent.

I had fome barrels executed by wrapping a thick plate of iron round a mandrel, as is practifed in the formation of gun-barrels; and likewife by bringing the two flat fides together, fo as to unite them by welding. Thefe attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the folid, fo as to form a barrel. In this manner I fucceeded well. The firft barrel I tried in this way was of fmall bore, only half an inch: Its performance was highly fatisfactory, and fuch as to convince me, that the mode now adopted was the beft of any that I had tried. Owing to the fmallnefs of the bore, a pyrometer could not be ufed internally, but was placed upon the breech of the barrel, as it ftood in the vertical muffle. In this pofition, it was evidently expofed to a much lefs heat than the fiducial part of the apparatus, which was always placed, as nearly as could be gueffed, at the point of greateft heat.

Finely levigated fpar became agglutinated by heat; femi-tranfparent, vitreous, with a few facettes.

On the 4th of April, an experiment was made in this way with fome fpar; the pyrometer on the breech giving 33°. The fpar came out clean, and free from any contamination, adhering to the infide of the porcelain tube: it was very much

fhrunk

ſhrunk, ſtill retaining a cylindrical form, though bent by partial adheſions. Its ſurface bore ſcarcely any remains of the impreſſion taken by the powder, on ramming it into the tube: it had, to the naked eye, the roughneſs and ſemitranſparency of the pith of a ruſh ſtripped of its outer ſkin. By the lens, this ſame ſurface was ſeen to be glazed all over, though irregularly, ſhewing here and there ſome air-holes. In fracture, it was ſemitranſparent, more vitreous than cryſtalline, though having a few facettes: the maſs, was ſeemingly formed of a congeries of parts, in themſelves quite tranſparent: and, at the thin edges, ſmall pieces were viſible of perfect tranſparency. Theſe muſt have been produced in the fire; for the ſpar had been ground with water; and paſſed through ſieves, the ſame with the fineſt of thoſe uſed at Etruria, as deſcribed by Mr. Wedgwood, in his paper on the conſtruction of his pyrometer.

With the ſame barrel I obtained many intereſting reſults, giving as ſtrong proofs of fuſion as in any former experiments; with this remarkable difference, that, in theſe laſt, the ſubſtance was compact, with little or no trace of frothing. In the gun-barrels where fuſion had taken place, there had always been a loſs of 4 or 5 *per cent.* connected, probably, with the frothing. In theſe experiments, for a reaſon ſoon to be ſtated, the circumſtance of weight could not be obſerved; but appearances led me to ſuppoſe, that here the loſs had been ſmall, if any.

In theſe experiments the eſcape of carbonic acid appears to have been leſs.

On the 6th of April, I made another experiment with the ſquare barrel, whoſe thickneſs was now much reduced by ſucceſſive ſcales, produced by oxidation, and in which a ſmall rent began to appear externally, which did not, however, penetrate to the bore. The heat roſe high, a pyrometer on the breech of the barrel giving 37°. On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident, that the barrel had not been laid open, for part of the carbonate was in a ſtate of ſaline marble; another was hard and white, without any ſaline grains, and ſcarcely effervefced in acid. It was probably quicklime, formed by internal calcination, but in a ſtate that has not occurred in any other experiment.

Gradual failure of the barrel bored from the ſolid.

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Remarkable fact of cryftals which appear to have been formed by fublimation.

The workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was feen in this piece, I was furprifed to fee the infide of the barrel lined with a fet of tranfparent and well-defined cryftals, of fmall fize, yet vifible by the naked eye. They lay together in fome places, fo as to cover the furface of the iron with a tranfparent coat; in others they were detached, and fcattered over the furface. Unfortunately, the quantity of this fubftance was too fmall to admit of much chemical examination; but I immediately afcertained, that it did not in the leaft effervefce in acid, nor did it feem to diffolve in it. The cryftals were in general tranfparent and colourlefs, though a few of them were tinged feemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a ftrong refemblance to the cryftals of the Lamellated Stylbite of Hauy. Though made above two years ago, they ftill retain their form and tranfparency unchanged. Whatever this fubftance may be, its appearance, in this experiment, is in the higheft degree interefting, as it feems to afford an example of the mode in which Dr. Hutton fuppofes many internal cavities to have been lined, by the fublimation of fubftances in a ftate of vapour; or, held in folution, by matters in a gafeous form. For, as the cryftals adhered to a part of the barrel, which muft have been occupied by air during the action of heat, it feems next to certain that they were produced by fublimation.

The old Sable Siberian iron is very tough at high heats.

The very powerful effects produced by this laft barrel, the fize of which (reduced, indeed, by repeated oxidation) was not above an inch fquare, made me very anxious to obtain barrels of the fame fubftance, which being made of greater fize, ought to afford refults of extreme intereft. I found upon inquiry, that this barrel was not made of Swedifh iron, as I at firft fuppofed, but of what is known by the name of *Old Sable*, from the figure of a Sable ftamped upon the bars; that being the armorial badge of the place in Siberia where this iron is made.*

All iron is crufhed under the hammer at fome definite heat. Caft iron at a low heat;

A workman explained to me fome of the properties of different kinds of irons, moft interefting in my prefent purfuit; and he

* I was favoured with this account by the late Profeffor Robifon.

he illustrated what he said by actual trial. All iron, when exposed to a certain heat, crushes and crumbles under the hammer; but the temperature in which this happens, varies with every different species. Thus, as he shewed me, cast iron crushes in a dull red heat, or perhaps about 15° of Wedgwood; steel, in a heat perhaps of 30°; Swedish iron, in a bright white heat, perhaps of 50° or 60°; old sable itself, likewise yields, but in a much higher heat, perhaps of 100°. I merely guessed at these temperatures; but I am certain of this, that in a heat similar to that in which Swedish iron crumbled under the hammer, the old sable withstood a strong blow, and seemed to possess considerable firmness. It is from a knowledge of this quality, that the blacksmith, when he first takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has sunk to the degree in which the iron can bear the hammer. I observed, as the strong heat of the forge acted on the Swedish iron, that it began to boil at the surface, clearly indicating the discharge of some gaseous matter; whereas, the old sable, in the same circumstances, acquired the shining surface of a liquid, and melted away without any effervescence. I procured, at this time, a considerable number of bars of that iron, which fully answered my expectations.

steel at a higher, Swedish iron at a bright white heat, and old Sable at a still higher temperature.

By the experiments last mentioned, a very important point was gained in this investigation; the complete fusibility of the carbonate under pressure being thereby established. But from this very circumstance, a necessity arose of adding some new devices to those already described: for the carbonate, in fusion, spreading itself on the inside of the tube containing it, and the two uniting firmly together, so as to be quite inseparable, it was impossible, after the experiment, to ascertain the weight of the carbonate by any method previously used. I therefore determined in future to adopt the following arrangement.

The complete fusibility of the carbonate under pressure was ascertained in these strong barrels.

A small tube of porcelain (*ik*, *Fig.* 23.) was weighed by means of a counterpoise of sand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment was ascertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had taken

Arrangement for obviating some difficulties which arose from the fusion. The carbonate was put into a small porcelain tube and this properly secured in a larger. These were placed in a cradle or frame and the whole put into the iron barrel, &c.

taken place between the tube and the carbonate. The balance which I ufed, turned in a conftant and fteady manner, with one hundredth of a grain. When pounded chalk was rammed into this tube, I generally left part of it free, and in that fpace laid a fmall piece of lump-chalk (*i*), dreffed to a cylinder, with the ends cut flat and fmooth, and I ufually cut a letter on each end, the more effectually to obferve the effects produced by heat upon the chalk; the weight of this piece of chalk being always eftimated along with that of the powder contained in the tube. In fome experiments, I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it), in order to provide againft the cafe of ebullition: but as that did not often occur, I feldom took the trouble of this laft precaution.

Continuation of the method of experiment.

It was now of confequence to protect the tube, thus prepared, from being touched during the experiment, by any fubftance, above all, by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The fmall tube (*Fig.* 23, *i k*) with its pounded carbonate (*k*), and its cylinder of lump-chalk (*i*), was dropt into a large tube of porcelain (*p k*, *Fig.* 24). Upon this a fragment of porcelain (*l*), of fuch a fize as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dreffed, fo as nearly to fit and fill up the infide of the large tube, one end of it being rudely cut into the form of a cone. This mafs being then introduced, with its cylindrical end downwards, was made to prefs upon the fragment of porcelain (*l*). I then dropped into the fpace (*n*), between the conical part of this mafs and the tube, a fet of fragments of chalk, of a fize beyond what could poffibly fall between the cylindrical part and the tube, and preffed them down with a blunt tool, by which the chalk being at the fame time crufhed and rammed into the angle, was forced into a mafs of fome folidity, which effectually prevented any thing from paffing between the large mafs of chalk and the tube. In practice, I have found this method always to anfwer, when done with care. I covered the chalk, thus rammed, with a ftratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner, I filled the whole of the large tube with alternate layers of filex and chalk; the muzzle being always occupied with chalk, which was eafily preffed into a mafs of tolerable firmnefs firmneſs, and, ſuffering no change in very low heats, excluded the fuſible metal in the firſt ſtages of the experiment.

Continuation of the method of experiment.

The large tube, thus filled, was placed in the cradle, ſometimes with the muzzle upwards, and ſometimes the reverſe. I have frequently altered my views as to that part of the arrangement, each mode poſſeſſing peculiar advantages and diſadvantages. With the muzzle upwards, (as ſhewn in *Fig.* 24 and 25), the beſt ſecurity is afforded againſt the intruſion of the fuſible metal; becauſe the air, quitting the air-tube in the working poſition, occupies the upper part of the barrel; and the fuſible metal ſtands as a liquid (at *q*, *Fig.* 25.) below the muzzle of the tube, ſo that all communication is cut off, between the liquid metal and the inſide of the tube. On the other hand, by this arrangement, the ſmall tube, which is the fiducial part of the apparatus, is placed at a conſiderable diſtance from the breech of the barrel, ſo as either to undergo leſs heat than the upper part, or to render it neceſſary that the barrel be thruſt high into the muffle.

With the muzzle of the large tube downwards, the inner tube is placed (as ſhewn in *Fig.* 22), ſo as ſtill to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the ſmall tube near to the breech of the barrel: and though there is here leſs ſecurity againſt the intruſion of liquid metal, I have found that a point of little conſequence; ſince, when the experiment is a good one, and that the carbonic acid has been well confined, the intruſion ſeldom takes place in any poſition. In whichever of the two oppoſite poſitions the large tube was placed, a pyrometer was always introduced, ſo as to lie as near as poſſible to the ſmall tube. Thus, in the firſt-mentioned poſition, the pyrometer was placed immediately below the large tube, and, in the other poſition above it; ſo that, in both caſes, it was ſeparated from the carbonate by the thickneſs only of the two tubes.

Much room was unavoidably occupied by this method, which neceſſarily obliged me to uſe ſmall quantities of carbonate, the ſubject of experiment ſeldom weighing more than 10 or 12 grains, and in others far leſs*.

On

* I meaſured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal ſand. By comparing the weight of

Experiment made with the foregoing precautions. The heat amounted to 64° Wedgwood. The carbonates had loſt gas and undergone fuſion.

On the 11th of April, 1803, with a barrel of old ſable iron having a bore of 0.75 of an inch, I made an experiment in which all theſe arrangements were put in practice. The large tube contained two ſmall ones; one filled with ſpar, and the other with chalk. I conceived that the heat had riſen to 33°, or ſomewhat higher. On melting the metals, the cradle was thrown out with conſiderable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my aſtoniſhment, indicated 64°. Yet all was ſound. The two little tubes came out quite clean and uncontaminated. The ſpar had loſt 17.0 *per cent.* the chalk 10.7 *per cent.* The ſpar was half ſunk down, and run againſt the ſide of the little tube: Its ſurface was ſhining, its texture ſpongy, and it was compoſed of a tranſparent and jelly-like ſubſtance. The chalk was entirely in a ſtate of froth. This experiment extends our power of action, by ſhewing, that compreſſion, to a conſiderable degree, can be carried on in ſo great a heat as 64°. It ſeems likewiſe to prove, that, in ſome of the late experiments with the ſquare barrel, the heat had been much higher than was ſuppoſed at the time, from the indication of the pyrometer placed on the breech of the barrel; and that in ſome of them, particularly in the laſt, it muſt have riſen at leaſt as high as in the preſent experiment.

Experiment in which the barrel failed after its contents had undergone fuſion.

On the 21ſt of April, 1805, a ſimilar experiment was made with a new barrel, bored in a ſquare bar of old ſable, of about two and a half inch in diameter, having its angles merely rounded; the inner tube being filled with chalk. The heat was maintained during ſeveral hours, and the furnace allowed to burn out during the night. The barrel had the appearance of ſoundneſs, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63°. On examination, and after beating off the ſmooth and even ſcale of oxide peculiar to the old ſable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithſtanding the failure of the barrel, was one of the moſt intereſting I had

of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330.6 grains, and that each grain of it correſponded to 0.00075 of a cubic inch. From theſe data I was able, with tolerable accuracy, to gage a tube by weighing the tin required to fill it.

made,

made, ſince it afforded proof of complete fuſion. The carbonate had boiled over the lips of the little tube, ſtanding, as juſt deſcribed, with its mouth upwards, and had run down to within half an inch of its lower end: moſt of the ſubſtance was in a frothy ſtate, with large round cavities, and a ſhining ſurface; in other parts, it was interſperſed with angular maſſes, which have evidently been ſurrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no effervefcence, and not turning red like quicklime in nitric acid, which ſeemed to have no effect upon it in the lump. It was probably a compound of quicklime with the ſubſtance of the tube.

With the ſame barrel repaired, and with others like it, many ſimilar experiments were made at this time with great ſucceſs; but to mention them in detail, would amount nearly to a repetition of what has been ſaid. I ſhall take notice of only four of them, which, when compared together, throw much light on the theory of theſe operations, and likewiſe ſeem to eſtabliſh a very important principle in geology. Theſe four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

Account of ſome experiments at very elevated heats. The carbonate moſt heated was calcined: that which had ſuffered leſs heat had the form of lime-ſtone and of marble, which retained their carbonic acid.

The firſt of theſe experiments was made on the 27th of April 1803, in one of the large barrels of old ſable, with all the above-mentioned arrangements. The heat had riſen, contrary to my intention, to 78° and 79°. The tubes came out uncontaminated with fuſible metal, and every thing bore the appearance of ſoundneſs. The contents of the little tube, conſiſting of pounded chalk, and of a ſmall piece of lump-chalk, came out clean, and quite looſe, not having adhered to the inſide of the tube in the ſmalleſt degree. There was a loſs of 41 *per cent.* and the calcination ſeemed to be complete; the ſubſtance, when thrown into nitric acid, turning red, without effervefcence at firſt, though, after lying a few minutes, ſome bubbles appeared. According to the method followed in all theſe experiments, and lately deſcribed at length, (and ſhewn in *Fig.* 24 and 25), the large tube was filled over the ſmall one, with various maſſes of chalk, ſome in lump, and ſome rammed into it in powder; and in the cradle there lay ſome pieces of chalk, filling up the ſpace, ſo that in the cradle there was a continued chain of carbonate of four or five inches in length. The ſubſtance was found to be leſs and leſs calcined, the more it it was removed from the breech of the barrel, where the heat was greateſt. A ſmall piece of chalk, placed at the diſtance of half an inch from the ſmall tube, had ſome ſaline ſubſtance in the heart, ſurrounded and intermixed with quicklime, diſtinguiſhed by its dull white. In nitric acid, this ſubſtance became red, but effervefced pretty briſkly; the effervefcence continuing till the whole was diſſolved. The next portion of chalk was in a firm ſtate of limeſtone; and a lump of chalk in the cradle, was equal in perfection to any marble I have obtained by compreſſion: the two laſt-mentioned pieces of chalk effervefcing with violence in the acid, and ſhewing no redneſs when thrown into it. Theſe facts clearly prove, that the calcination of the contents of the ſmall tube had been internal, owing to the violent heat which had ſeparated its acid from the moſt heated part of the carbonate, according to the theory already ſtated. The ſoundneſs of the barrel was proved by the complete ſtate of thoſe carbonates which lay in leſs heated parts. The air-tube in this experiment had a capacity of 0.29, nearly one-third of a cubic inch.

Another experiment in which the barrel failed.

The ſecond of theſe experiments was made on the 29th of April, in the ſame barrel with the laſt, after it had afforded ſome good reſults. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch.— The heat roſe to 60°. The barrel was covered externally with a black ſpongy ſubſtance, the conſtant indication of failure, and a ſmall drop of white metal made its appearance. The cradle was removed without any exploſion or hiſſing. The carbonates were entirely calcined. The barrel had yielded, but had reſiſted well at firſt; for the contents of the little tube were found in a complete ſtate of froth, and running with the porcelain.

Third experiment, very thin fuſion.

The third experiment was made on the 30th of April, in another ſimilar barrel. Every circumſtance was the ſame as in the two laſt experiments, only that the air-tube was now reduced to half its laſt bulk, that is, to one-twentieth of a cubic inch. A pyrometer was placed at each end of the large tube. The uppermoſt gave 41°, the other only 15°. The contents of the inner tube had loſt 16 *per cent.* and were reduced to a moſt beautiful ſtate of froth, not very much injured by the internal calcination and indicating a thinner ſtate of fuſion than had appeared.

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The fourth experiment was made on the 2d of May, like the rest in all respects, with a still smaller air-tube, of 0.0318, being less than one thirtieth of a cubic inch. The upper pyrometer gave 25°, and the under one 16°: The lowest masses of carbonate were scarcely affected by the heat. The contents of the little tube lost 2.9 *per cent.* both the lump and the pounded chalk were in a fine saline state, and, in several places, had run and spread upon the inside of the tube, which I had not expected to see in such a low heat. On the upper surface of the chalk rammed into the little tube, which, after its introduction had been wiped smooth, were a set of white crystals, with shining facettes, large enough to be distinguished by the naked eye, and seeming to rise out of the mass of carbonate. I likewise observed, that the solid mass on which these crystals stood, was uncommonly transparent.

Fourth experiment under greater pressure, fusion at a moderate heat, slight loss of carbonic acid, &c.

In these four experiments, the bulk of the included air was successively diminished, and by that means its elasticity increased. The consequence was, that in the first experiment, where that elasticity was the least, the carbonic acid was allowed to separate from the lime, in an early stage of the rising heat, lower than the fusing point of the carbonate, and complete internal calcination was effected. In the second experiment, the elastic force being much greater, calcination was prevented, till the heat rose so high as to occasion the entire fusion of the carbonate, and its action on the tube, before the carbonic acid was set at liberty by the failure of the barrel. In the third experiment, with still greater elastic force, the carbonate was partly calcined, and its fusion accomplished, in a heat between 41° and 15°. In the last experiment, where the force was strongest of all, the carbonate was almost completely protected from decomposition by heat, in consequence of which it crystallized and acted on the tube, in a temperature between 25° and 16°. On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced; since the first experiment proved that quicklime by itself could neither be melted, nor act upon porcelain, even in the violent heat of 79°; whereas, in the last experiment where the carbonic acid was retained, both of these effects took place in a very low temperature.

Observations: the fusion takes place at lower heats when the escape of carbonic acid is prevented. The acid acts as a flux.

(To be continued.)

IV.

Obſervations on the Effect of Madder Root on the Bones of Animals. By Mr. B. Gibson.*

Account of the firſt diſcovery of the property of madder to tinge the bones of living animals.

THERE is, perhaps, no phenomenon, which occurs in an animal body more curious, than the tinge communicated to the bones of living animals, whoſe food has been mixed with madder root. This, like many other facts, to which no reaſoning *à priori* could have directed us, was diſcovered by chance. Mr. Belcher, dining with a calico printer on a leg of freſh pork, was ſurprized that the bones, inſtead of poſſeſſing their uſual whiteneſs, were of a deep red colour; and on enquiring the cauſe of it, was informed, that the pig had been fed upon the refuſe of the dyers' vats, and had received ſo much of the colouring matter of madder into the ſyſtem, that its bones were dyed by it. So intereſting a fact has attracted very much the attention of anatomiſts, and has been uſed in many phyſiological and pathological enquiries; it may not therefore be unintereſting to give a ſhort hiſtory of the phenomena connected with it, and the purpoſes to which it has been applied, previous to entering upon the more immediate object of this paper.

Experiments ſhewing that the tinge is more quickly given to the bones of growing animals.

Many experiments have been made to aſcertain how long a time is required to produce the tinge, and whether it be permanent or only temporary. Belcher and Morand, about the ſame time, mixed madder root with the food of chickens and young pigeons. The reſult of their obſervations was, that the tinge was more quickly communicated to the bones of growing animals, than to the bones of animals which had already completed their growth; the bones of young pigeons being tinged of a roſe-colour in twenty-four hours, and of a deep ſcarlet in three days; whilſt the bones of adult animals only exhibited a roſe-colour in fifteen days. They found the tinge moſt intenſe in the ſolid parts of thoſe bones, which were neareſt to the centre of circulation; whilſt in bones of equal ſolidity, at a greater diſtance from the heart, the tint was more faint. The dye was deep in proportion to the length of time the madder had been continued, and when it was diſcontinued, the colour gradually became more and more faint, till

Short time and other facts.

* Mancheſter Memoirs, 1805.

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it entirely disappeared. According to the experiments of these gentlemen, other vegetable dyes, such as logwood, turmeric and alkanet root, did not communicate their respective tints to the bones.*

Du Hamel used this property to shew the growth of bones.

This effect of madder upon the bones, was soon afterwards made use of by Du Hamel, in his attempt to prove the manner in which the bones of animals are encreased in thickness. Observing in the vegetable kingdom, that the bark, by a sort of secretion, formed the ligneous part of a tree, in successive layers; so he conceived that the periosteum, or membrane surrounding bones, being converted into osseous matter, encreased their diameter by adding to them concentric laminæ in succession. In order to prove the justness of his opinion, he mixed the food of a cock with madder root for a month, withheld it for a month, and then gave it again. He afterwards killed the animal, and upon inspection thought he observed the appearance which he expected; viz. two layers of red bone inclosing one of white, corresponding to the periods of the madder's being given or withheld.

It is very doubtful whether that growth could be so indicated.

This experiment, and some others related by Du Hamel, appear to be conclusive in favour of the theory, which he wished to establish; and as they were conducted by a physiologist of high character, the accuracy of the observations could not have been doubted, had these experiments stood alone. But when they are compared with some of his own previous experiments, and those of other authors, it is difficult to reconcile them. In some of Du Hamel's experiments, for instance, the bones of a cock were tinged of a rose-colour through their whole substance in sixteen days, and those of young pigeons of a deep scarlet in three days. In several ex-

* From some experiments I made on young pigeons, I found that a considerable quantity of logwood, in the form of extract, communicated an evidently purple tint to the bones. With regard to turmeric, it appears to be altered in its colour by passing through the digestive organs, for the fœces of the animals, who took it in considerable quantity, were constantly green: whilst either logwood or madder root exhibited their respective hues after passing through the intestines. Saffron exhibits properties different from any of these substances; for though a pigeon took it in considerable quantity, and thereby had its fœces tinged, yet no perceptible alteration of colour was produced in its bones.

 periments

periments I have made on the ſubject, I have found the bones of young pigeons tinged of a uniform roſe-colour, internally as well as externally, in twenty-four hours. This communication of colour to the whole ſubſtance of the oſſeous ſyſtem in ſo ſhort a time, makes it highly improbable that the laminated appearance, remarked by Du Hamel, was produced by the new formation of red and white oſſeous layers, correſponding to the times (months) the madder had been given or withheld. For, as Mr. John Bell very juſtly remarks,* "If a bone ſhould increaſe by layers thick enough to be viſible and of a diſtinct tint, and ſuch layers be continually accumulated upon each other every week, what kind of bone ſhould this grow to?" The only way in which we can reconcile with each other the phenomena obſerved in the different experiments, and account for their apparent contradiction, is, by ſuppoſing that Du Hamel miſtook for an obſcurely laminated appearance, the variety in the tint, which is more deeply communicated to the more ſolid, and more faintly to the leſs compact parts of a bone.

Late experiments of Dr. M'Donald on the bones.

This property of madder of tinging the bones of animals, has lately been employed by Dr. M'Donald,† in his ingenious reſearches into the formation and death of bones. Amongſt other objects, he attempted to aſcertain in what manner and how ſoon a cylindrical bone is regenerated to ſupply the place of one artificially killed. As the proceſs is highly curious, I ſhall briefly relate the principal points.

Very curious proceſs of a bone deſtroyed,

Dr. M'Donald's experiments were made by amputating the proper leg-bone of young pigeons or chickens immediately above the joint. The marrow was then extracted, and the cavity which contained it, filled with lint. This proceſs cauſed the death of the bone, and the formation of a new bone ſurrounding that deſtroyed enſued. Immediately after the experiment, the animal had its food mixed with madder root, and the part was inſpected in different animals, at different periods.

—and the regular proceſs of

On examination three days afterwards, the perioſteum or enveloping membrane, was found much thickened; and underneath it a gelatinous humour was effuſed, ſurrounding the

* Anatomy of the bones, &c. p. 15.

† Diſputatio inauguralis de Necroſi ac Callo. 1799.

dead

dead bone, and ſpotted with red oſſeous nuclei; proving that the regeneration of the bone had commenced at this early period.

—its regeneration.

In ſeven days the new bone was found ſoft and flexible, not to be diſtinguiſhed from cartilage or griſtle, except by the red tint the madder had communicated to it; yet the bone deſtroyed was not at all coloured, although the other bones of the animal had acquired a bright red. From this time the new bone continued to encreaſe in hardneſs, ſurrounding the old one like a ſheath. The latter in about three weeks was ſo looſe as to be drawn out, and in about fifteen days from this time, the cavity of the regenerated bone was filled with marrow, and in every reſpect performed the office of that for which it was a ſubſtitute. This may be conſidered as a general outline of the progreſſive changes which take place during the regeneration of a cylindrical bone, in a young animal, ſuch as a pigeon, or chicken; and the ſame proceſs is frequently performed in the human body, when, from ſome internal cauſe, the life of a bone is deſtroyed. Theſe changes involve many intereſting particulars; but the circumſtance moſt immediately connected with the ſubject of this paper is, that although the ſhaft of the bone required three weeks for its renewal, yet in ſeven days the oſſeous ſyſtem generally had acquired a bright red. Now if we explain this change in colour according to the common opinion of abſorption of the white, and depoſition of the red oſſeous matter,* we muſt neceſſarily draw this concluſion; that the oſſeous ſyſtem of the animal will be renewed three times during the period, which the formation of the ſubſtitute bone requires; a concluſion which we ſhould be inclined to reject merely from its improbability. But beſides this, the appear-

Inference. From the very ſpeedy acquiſition and ſubſequent loſs of the red colour that the oſſeous ſyſtem was naturally abſorbed and renewed in that period.

* The common opinion of phyſiologiſts, with regard to this curious fact, is, that when a bone becomes red, during the exhibition of madder root, the white oſſeous particles which compoſed it, have been entirely removed by abſorption and replaced by new oſſeous matter of a red colour: and when a bone aſſumes its natural colour, theſe red particles have been removed and replaced by white. If this be the fact, it neceſſarily follows, that an animal has at leaſt fifty-two new ſets of bones in a year: for the oſſeous ſyſtem, according to the experiments of the moſt reſpectable phyſiologiſts, acquires a deep red tint from madder in one week, and aſſumes its natural colour in another.

ance

ance of the parts ſtrongly militate againſt it—for, if we may judge at all of the activity of the proceſs in the two parts, by their comparative degrees of vaſcularity, that employed in forming the ſubſtitute bone far exceeds that going on in the oſſeous ſyſtem generally; one ſtriking phenomenon attending the regeneration of a bone being, the very high degree of increaſed vaſcularity which the parts employed in the proceſs rapidly aſſume.

Cauſe of doubt.

The bones are alone reddened by madder, becauſe the phoſphate of lime acts as a mordent on the madder.

After this effect of madder upon the bones was known, it long remained a myſtery, why ſome other white parts of the body, ſuch as nerves, cartilages and perioſteum, were not equally liable to be coloured by it, as the bones. This fact, I believe, did not receive any explanation, until Dr. Rutherford gave a very ingenious and ſatisfactory one. When ſpeaking of this property of madder, he ſays,* "We have, in the fact before us, a beautiful example of a particular caſe of chemical attraction; ſuch as in numberleſs inſtances, is obſerved to take place between the colouring particles of both animal and vegetable ſubſtances and various other bodies, eſpecially earths and earthy ſalts, and oxides of metals. So ſtrong is the affinity of the colouring matter to theſe bodies, that it is frequently obſerved to quit the menſtruum, in which it may chance to be diſſolved, to unite with them: they, in conſequence of its union, acquiring a particular tinge, whilſt the menſtruum is proportionably deprived of colour. From this principle, this mutual attraction, is deduced the various uſe of thoſe bodies as mordents, as they are called, intermedia, or means for fixing the colours in dying or ſtaining thread or cloth, whether it be compoſed of animal or vegetable materials. Upon the ſame principle depends the preparation of thoſe pigments known to painters under the name of lakes; theſe are truly precipitates of the colouring matter, in combination with various mordents, as their baſis.—The colouring of the bones of a living animal by means of madder, is, in every circumſtance, analogous to the formation of theſe lakes. The colouring matter of madder, paſſing unaltered through the digeſtive organs of the animal, enters the general maſs of fluids, and is diſſolved in the ſerum of the blood, to which,

The red matter is a kind of lake.

* See Dr. Blake's inaugural Diſſertation. De dentium formatione, p. 119.—1798.

indeed,

indeed, if it be in large proportion, it communicates a ſenſibly red tinge. *But there is always preſent in the blood, and in a ſtate of ſolution in the ſerum, a quantity of the earthy matter of the bones, phoſphate of lime, ready to be depoſited, as the exigencies of the animal may require. Now the phoſphate of lime is an excellent mordent to madder, and has a ſtrong affinity to it, and is conſequently admirably fitted to afford a baſe for the colouring matter of it: in ſuch experiments, therefore, they concrete in the ſtate of a bright red lake, whence the colour of the bones is derived.* That this is actually the caſe, may be ſhewn by a variety of experiments. Thus, if to an infuſion of madder in diſtilled water, be added a little of the muriate of lime, no change is perceived: but if to this mixture be added a ſolution of the phoſphate of ſoda, immediately a double elective attraction takes place. The muriatic acid combining with the ſoda, remains ſuſpended, or diſſolved in the water; whilſt the phoſphoric acid, thus deprived of its ſoda, combines with the lime which the muriatic acid parted with, and forms phoſphate of lime or earth of bones. This ſubſtance, however, being inſoluble in water, falls to the bottom; but having combined at the inſtant of its formation with the colouring matter of the madder, they fall down united into a crimſon lake; preciſely of the ſame tint with that of the bones of young animals, which have been fed with madder. From this ſimple repreſentation of the matter, we have a ready explication of every circumſtance which has been remarked as extraordinary reſpecting this ſubject."

—formed as it ſeems before the oſſeous depoſition.

Whilſt Dr. Rutherford thus gives a moſt ſatisfactory explanation of the colour of madder being communicated to the bones alone, of all the white parts of an animal; we find that he embraces the ſame opinion as other phyſiologiſts, that the oſſeous materials acquire their colour previous to their depoſition, whilſt in a ſtate of ſolution or mixture in the blood; from whence they are afterwards depoſited, and concrete in the form of a bright lake. In no part of his ingenious remarks does he hint at the probability that the bones already formed in an animal, may, during the uſe of madder, become red, and after its diſuſe gradually reſume their natural colour, by the agency of a power entirely independent of their depoſition and abſorption; that this is probable I ſhall now proceed to prove.

Dr. Rutherford admits the abſorption and depoſition.

Before

More particular explanation of the doctrine of the abforption and regeneration of the parts of animals,

Before it was difcovered that madder poffeffed this property of tinging bones, phyfiologifts had long been of opinion, that the various parts of the body, being worn out by the performance of their actions and functions, were gradually removed, and replaced by new materials. They had feen, as Mr. J. Bell obferves, the whole offeous fyftem by the morbid removal of its folid part, rendered fo foft and flexible as to bend under the common weight of the body and ordinary action of parts; the regeneration of many bones which had been deftroyed by difeafe; the rapid abforption of fat in fome difeafes, and its fpeedy reproduction; and laftly, the gradual change which the fluids of the body undergo, as well as fome of its infenfible parts, the hair and nails; hence they fuppofed that the fame procefs of change and renovation went on in every organ, and that the bodies of animals were not compofed of the fame identical particles of which they would confift at fome future period. This procefs, which was before but conjectural, or fupported by analogy, phyfiologifts confidered as fully proved by the effects of madder upon the bones. They had by this means an opportunity of feeing the bones altered in colour, from the flighteft tint to the deepeft red; they could obferve this gradually removed, until the bones had regained their natural whitenefs; and explaining the whole procefs on the principle of depofition and abforption, they confidered it as ocular demonftration of a moft rapid change in the conftituent elements of a part, of which, from its folidity, they could fcarcely have believed it fufceptible.

—fuppofed to be confirmed in the bones.

Probability that this explanation is erroneous.

I apprehend, however, that it is by giving an erroneous explanation of the phenomena; by fuppofing that a change in the offeous particles is denoted by an alteration in their colour, that phyfiologifts have confidered this fact as conclufive.—— However indubitable and well fupported may be the opinion, which attributes an imperceptible change to the various parts of the body, we fhall, I believe difcover upon a more clofe examination, that it is by no means fupported by the appearances, which the bones difplay on the exhibition of madder root. The rapid change in their particles, which fuch appearances indicate, when explained in the common way, is completely at variance with all the proceffes performed by the bones, both in their healthy and difeafed ftates. Thus we find the formation of the offific matter, called Callus, for the

union

union of fractured bones, or the exfoliation of a part of a bone, are proceſſes requiring a conſiderable length of time for their performance. In Dr. M'Donald's experiments, the formation of a regenerated bone required nearly ſix weeks; but during the ſame ſpace of time, the bones of the ſame animal would be renewed ſeveral times, if the common explanation of the communication and diſappearance of the tinge of madder were well founded. From theſe circumſtances, I am led to believe that the appearances produced by the exhibition of madder, require another mode of explanation. That which I have to offer is not liable to the ſame objections, and is ſtrongly ſupported by comparative experiments.

For the proceſſes by which bones are reſtored are ſlow.

It was obſerved by Du Hamel, in his experiments, that the bones of animals which had been deeply tinged by madder, by long expoſure to air loſt their colour and became white.— It was this fact which ſuggeſted to me a ſimple explanation of the proceſs. It occurred to me, that if any one of the component parts of the blood naturally exerted a ſtronger attraction for the colouring matter of madder, than the phoſphate of lime, it might be deprived of the tint by a chemical power. In order to prove this, as far as I could by experiment, I took one dram of the phoſphate of lime tinged, as in Dr. Rutherford's experiment, and expoſed it for half an hour to the action of two ounces of freſh ſerum, at the temperature of 98 degrees. By this operation, the ſerum gradually acquired a red tinge, whilſt the phoſphate of lime was proportionably deprived of colour. In a comparative experiment, a ſimilar quantity of tinged phoſphate of lime was expoſed to the action of diſtilled water under ſimilar circumſtances; but no change took place. The knowledge of this ſtrong affinity in the ſerum for colouring matter, affords an eaſy and ſimple explanation of the effects of madder on the bones, upon the principle of chemical attraction.

A ſimple explanation grounded on experiment.

The ſerum of blood has a ſtronger attraction for the colouring matter of madder than phoſphate of lime has.

Thus, when an animal has madder mixed with its food, the blood becomes highly charged with it, and imparts the ſuperabundant colouring matter to the phoſphate of lime, contained in the bones already formed; as it circulates through them and moiſtens them throughout. But as ſoon as an animal has ceaſed to receive the madder, and the blood is freed from the colouring matter by the excretions, the ſerum then exerts its ſuperior attraction, and by degrees entirely abſtracts it from the

Hence the bones are dyed when much madder is in the ſyſtem, and bleached by the ſerum when the quantity becomes leſs.

the phosphate of lime, and the bones resume their natural whiteness. In short, the bones are at one time dyed by the colouring matter, at another time bleached by the serum.

Phosphate while suspended does not strongly take the colouring matter.

Whilst I have attempted to explain the probable manner in which the bones, *already formed* in an animal, at one time receive, and at another are deprived of the colouring matter of madder, I by no means intend to assert that the phosphate of lime does not acquire a similar colour during its solution in the serum, or at the time it is precipitated from it to enter into the composition of the bones; the fact is indisputable. I have, however, found from some experiments lately made upon a hen during oviparation, that only a slight tinge can be communicated to the shell, formed whilst a large quantity of colouring matter is circulating with the blood. So slight indeed is the blush, that it would not be seen by a common observer, unless contrasted with a natural egg: which is probably the reason why it has, I believe, been denied by physiologists, that the shell of an egg is altered by the exhibition of madder. If this may be considered as a test of the quantity of colouring matter, which the phosphate attracts at the time it is separated from the blood, it forms another strong argument against the theory, which Dr. Rutherford, and all preceding physiologists have adopted; for, consistent with this fact, the bones should never exhibit more than a slight blush. When explained upon the principle of chemical attraction, we see that the phenomena, exhibited by the bones of an animal, by giving or withholding madder root, give no support to the opinion that the various parts of the body continually undergo an imperceptible change; and I consider it a fortunate circumstance for that doctrine that so simple an explanation of the effect of madder can be given. For whilst so specious a fact has been considered, by the highest authorities, as complete proof of the imperceptible renovation of parts; the rapid change in the constituent elements of the bones, which the communication and disappearance of the colour indicates, must have appeared astonishing to every physiologist. Of this I cannot give you a stronger instance than in the words of Mr. J. Bell.* Nothing," says he, "can be more curious than this continual renovation and change of parts even in the hardest bones. We are accustomed to say of the whole body, that it is daily

Example in eggs.

The doctrine of a rapid and continual change is not supported by the facts of bones tinged by the madder.

* Anatomy of the bones, &c. p. 13.

changed;

changed; that the older particles are removed, and new ones ſupply their place; that the body is not now the ſame individual body, that it was; but it could not be eaſily believed that we ſpeak only by gueſs concerning the ſofter parts, which we know for certain of the bones.—When madder is given to animals, withheld for ſome time and then given again, the colour appears in their bones, is removed, and appears again with ſuch a ſudden change, as proves a rapidity of depoſition and abſorption exceeding all likelihood or belief; all the bones are tinged in twenty-four hours; in two or three days their colour is very deep, and if the madder be left off but for a few days, the red colour is entirely removed."

Although by this chemical explanation of the effect of madder upon the bones, the doctrine of the imperceptible change in the component parts of animal bodies, loſes the ſupport of a fact, which has, ſince its diſcovery, been univerſally conſidered as its ſtrongeſt proof; nevertheleſs, indiſputable arguments, derived from different ſources, ſtill place that doctrine amongſt the beſt ſupported opinions in phyſiology.

V.

On Fairy Rings and the Waſte of Fiſh in Scotland. By A. T.

To Mr. NICHOLSON.

SIR,

HAVING frequently noticed the fairy-rings your correſpondent, M. Florian Jolly mentions in your Journal for February, I ſhould be glad to know from him whether hares or rabbits abounded in Broadlands park, as I have generally obſerved theſe rings moſt prevalent, in light ſandy ſoils, particularly among rabbit burrows. This ſpecies of ſoil from its dryneſs would be very unfavourable to the idea of theſe things being formed from a central heap of horſe dung; beſides, were this the cauſe of them, we ſhould expect them to be always circular, or when not circular, that thoſe parts moſt remote from the centre would appear not to have benefited ſo ſtrongly from the manure as thoſe which were nearer. I have generally obſerved that the rings were compoſed of a double circle, or rather a little circular path, the middle of which appeared to be

Obſervations and inquiry whether fairy rings may not have been made by hares and rabbits.

be trodden, and the edges grown up, and more in vigour than any of the ſurrounding graſs. I had occaofin to remark one of thoſe fairy-rings laſt ſummer: it was perfectly circular, and about ten feet in diameter, it was ſituated at the edge of a copſe wood, and in a vicinity where there are abundance of both hares and rabbits; but what appeared to me moſt ſingular, was its being interſected exactly through the middle, by a well frequented foot path. The hare is rather given to gravity, the rabbit is more playful; but whether it is given to the amuſement of *lounging in the ring*, ſome of your more informed correſpondents may be enabled to inform you.

Fiſh is undoubtedly waſted in Scotland.

I OBSERVE ſome of your correſpondents have got into a controverſy reſpecting the waſte of fiſh in Scotland. No doubt can exiſt upon that head; not however ariſing from the waſteful diſpoſition of the natives, their delicacy in appetite or ſuperabundance of proviſion, but from the want of a market for the conſumption of their overplus. To talk of Aberdeen fiſhermen bringing freſh fiſh to Newcaſtle, Norwich, or Leeds, is as ridiculous as to propoſe taking them to Amſterdam, or London; for beſides the difficulty of again making their own ports, they will conſtantly find an over-ſtocked market, as the ſame weather that permits them to fiſh will permit their neighbours to do the ſame. But the grand cauſe of all the waſte is the horrible monopoly which their country labours under in reſpect to their ſalt laws, where for the ſake of a few paltry pans, Engliſh ſalt is excluded under the ſevereſt penalties, although it can be delivered in any part of Scotland at one half the price that we are forced to pay for Scotch ſalt under the preſent circumſtances. Give them ſalt at a cheap rate, if it does not permit them to export the fiſh, as that requires capital and new eſtabliſhments, it would at leaſt enable them to ſupply the interior; a thing as worthy the attention of the public as the ſupply of any other market I know.

The fiſheries of Scotland are deſtroyed by the operation of the ſalt laws.

Your moſt obedient

A. T.

March 23, 1806.

Letter

VI.

Letter from AMICUS *refpecting the fuppofed Wafte of Crab-Fifh in Scotland.*

To Mr. NICHOLSON.

SIR,

THE very refpectable and diftinguifhed rank which the Philofophical Journal holds among the periodical publications will at all times prevent its becoming the vehicle of unneceffary difpute or contradiction: yet as public information and utility is fometimes promoted by the correction of miftakes, when this is likely to be the cafe, any thing that can elucidate a fact either mifreprefented or partially ftated, is doubtlefs compatible with the fpirit of your publication. In your 48th number it is ftated by "an Enquirer" that the crab fifhery is fo productive about Arbroath that, after boiling them, the bodies of the crabs are thrown away, and the large claws only brought to table, of which the Enquirer fays he has been a witnefs. The fact is literally true, but wants further explanation. It is well known to every perfon refident on the coafts where crab-fifh are commonly to be had, that many of that fpecies are fcarcely eatable, being often found after boiling to contain hardly any thing but water. The writer of this article has repeatedly feen from twelve to twenty crabs boiled at one time, and every one of them, more or lefs, in the above fituation. When this is the cafe, the meat of the great claws (although they ftill may be eaten) is alfo watery and infipid compared to thofe of a good crab, the body of which is filled with a very rich fubftance, which is fo far from being thrown away, that it is in general efteemed a luxury, even where crabs are plenty. Some perfons are, indeed, fond of the claws, who cannot eat the bodies at all; but thefe are only exceptions from general tafte and common practice. The claws of a good crab (as has been already obferved) are much firmer, more rich, and fweet to the tafte than thofe of an inferior kind, which are by far the moft abundant. The claws of the male are larger in proportion than thofe of the female: the male crab is alfo reckoned fuperior in quality, except for a very fhort period (in what time of the year I have not been able

Obfervations concerning the fact that the bodies of crab-fifh are wafted at Arbroath. It is a bad fpecies which is rejected, the good ones are eaten.

to afcertain) when in the the opinion of fome who pretend to be connoiffeurs, the females are equal, or nearly fo in delicacy.

Crabs are in feafon nine months in the year; May, June, and July are the only months in which they are not. Some pifcatory epicures pretend to certain marks for diftinguifhing good crabs, but they are very far from being infallible; perhaps the moft general diftinction is, that a good crab has a fhell of a dufky red colour, with a certain degree of roughnefs, particularly on the claws; while the bad ones have fhells white, clear, fmooth, and watery; but the diftinction is much better underftood from obfervation than any detailed account. Trufting that you, Mr. Editor, will have the goodnefs to infert this communication, and that your correfpondent, "the Enquirer," will do me the juftice to believe that my fole motive for troubling you was to give information, I am with efteem, SIR,

Your moft obedient fervant,

AMICUS.

Arbroath, March 4, 1806.

VII.

Probability that the Hindoos were acquainted with Saturn's Ring.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of requefting the infertion of the following quotation in your Philofophical Journal, from the 7th vol. of Mr. Maurice's Indian Antiquities, page 605. If it does really mean the ring of the planet Saturn, perhaps fome of your readers can explain how it could have been difcovered by the Brahmins in fuch remote ages.

Your's refpectfully,

April 7, 1806. A. B. C.

Extract from Maurice's Indian Antiquities.

"I have already intimated in a former volume, that the *circle* formed around SANI (the Saturn of the Hindoos) by intertwining ferpents, was probably intended to denote his RING. I have fince had the figure engraved for the reader's infpection and decifion. It is impoffible to afcertain the exact age of the pictured image in the Pagoda, from which the portrait was taken;

taken; but probably both are of a very remote age, for the Indian pagodas are not fabrications of yesterday, nor in their conceptions and designs are they given to frequent vicissitude. Now if Sani were thus designated in very antient periods, the fact proves that they must, by what means can scarely be conjectured, have discovered the phenomenon of his *ring*, for what besides could that serpentile oval inclosing the body of Sani be intended to represent? That phenomenon however was not known in Europe till about the year 1628, when Galileo, with the first perfect telescope discovered, what he conceived to be two stars at the extreme parts of the planet, but which in reality proved to be the *ansæ* of that ring, the natural existence of which was afterwards demonstrated by Huygens and succeeding astronomers. The circumstance is not the least wonderful of those that occur in the discussion of Indian antiquities and literature. I have stated the fact, and engraved the image; I leave to abler judges the task of decision."

VIII.

*Explanation of Time keepers constructed by Mr. Thomas Earnshaw: for which a Reward of Three Thousand Pounds was awarded by the Commissioners of Longitude. From the Communications made by him to the Commissioners *.*

THE model, from which the annexed drawings were taken contains, besides the parts necessary to explain the nature of the Escapement, a box inclosing a spring, which when wound up communicates, by means of some more wheels, a force to the balance-wheel, sufficient, when the balance is put in motion, to keep it in action for some time. These wheels are contained between two brass plates, fastened together by four upright

Description of the Escapement of Mr. Earnshaw's time piece.

* The Escapement with a model was communicated in June, 1804, and a subsequent explanation in March, 1805. The former is here given, and so much of the latter as directly relates to the time-keepers. The latter paper is no otherwise abridged than by omitting certain observations upon other artists, and some general remarks which do not form part of the disclosure.

I have been solicitous to give as early an account as might be proper, of the Escapements of Mr. Earnshaw and Mr. Arnold

 which

Defcription of the Efcapement of Mr. Earnfhaw's time piece.

upright pillars; the uppermoft of thefe plates is that which is reprefented by Fig. 1ft. plate XIII, where PQRS are the four fcrews that take into the heads of the four pillars above mentioned, and connect it to the remaining part of the model. The plate PQRS contains, however, the whole of the parts neceffary for the prefent purpofe. The fide of this plate reprefented to view, is the undermoft when fixed in the model; fo that the figure reprefents this plate as taken off, with the fide next to the balance laid upon a table, and the eye is fuppofed to be placed perpendicular over it.

In the plate PQRS is an opening, or a piece taken out, reprefented by TUWXYZ. In this opening, the balance-wheel ABCD, pallet MSK, and part of the balance UV are feen. The balance-wheel is fupported by two pieces of brafs, ONH, OI; the piece ONH is fcrewed to the fide of the plate neareft to view by a ftrong fcrew *t*, and made firm by fmall pins reprefented by $\pi\,\pi\,\pi\,\pi\,\pi\,\pi$; thefe pins are called fteady pins; they are riveted faft into the fupporting piece OH, and take into holes in the plate PQRS, made exactly to fit them. The part ON of this fupporting piece is fuppofed to be raifed above the part *t* H by a joint or bend at N; the other fupporting piece OI is faftened to the oppofite fide of the plate; and between thefe two pieces the balance-wheel turns freely and fteadily in the direction of the letters ABCD. The fmall wheel MSK is called the large pallet; it is a cylindrical piece of fteel, having a notch or piece cut out of it at *l h r*; againft the fide of this notch is a fquare flat piece of ruby, or any hard ftone, *h l*, ground and polifhed very fmooth, and fixed faft into the pallet. The cylinder is fo placed, with refpect to the balance-wheel, that it may not be more than juft clear of two adjoining teeth. EF is a long thin fpring, which

(which laft appears in No. 55 of our Journal) as they have been fo highly diftinguifhed by the national munificence. Some difcuffion of the important fubject of time pieces may be feen in the Philof. Journal, quarto feries, Vol. I. 56, and Vol. II. 106. As I expect fhortly to be favoured with a valuable communication refpecting the original inventors of free 'Scapements and compenfations, and may, according to circumftances, offer a few remarks on the fubject myfelf, I have been careful in the firft place to give the accounts of the above mentioned artifts in their own words.

W. N.

is

which is made faſt at one end, by being pinned into a ſtud, G, and made to bear gently againſt the head of an adjuſting ſcrew *m*; the other end is bent a little into the form of a hook; to this ſpring there is fixed another very ſlender ſpring at γ, which projects to a ſmall diſtance beyond it. This ſmall ſpring lies on the ſide of the thick ſpring neareſt to the balance-wheel. The adjuſting ſcrew, *m*, takes into a ſmall braſs-cock, α *p*, which is ſcrewed faſt to the plate PQRS by a ſtrong ſcrew at *p*. Upon the ſpring EF there is fixed a ſemi-cylindrical pin, which ſtands up perpendicular upon it, and of a ſufficient length to fall between the teeth of the balance-wheel ABCD. This pin is called the locking-pallet, and is placed on the oppoſite ſide of the ſpring repreſented to view. Through the center of the cylindrical pallet MSK, a ſtrong ſteel axis paſſes called the verge; the pallet is made faſt to this axis, which alſo paſſes through the center of the balance, and is made faſt to it; it has two fine pivots at its extremities, upon which it turns very freely, between two firm ſupporting pieces of braſs ſcrewed firmly, and made as permanent as poſſible, by ſteady pins to the principal plate PQRS; one of theſe pieces is repreſented in the figure by *w y* L; the part *w* is raiſed above the part *y* L by a bend or joint at *n*; the part *y* L being repreſented as fixed firm to the plate by the ſtrong ſcrew at *y*. This piece is called the potence, and is exactly ſimilar to the other ſupporting piece, which is called the cock, that is ſimilarly fixed to the oppoſite ſide of the plate and hid from the ſight in the figure. A little above the cylindrical pallet MSK (as it appears in the figure) is fixed a ſmall cylindrical piece of ſteel *in*, having a ſmall part projecting out at *i*, through which the verge alſo paſſes; this is called the lifting pallet; it fixes upon the verge like a collar, and is made faſt by a twiſt, ſo as to be ſet in any poſition with reſpect to the large pallet MSK. The balance lying below the plate PQRS, only the part UV is repreſented to view; the continuation of the poſition of the circumference, however, is repreſented by the dotted lines ULHV. The end EG of the long ſpring EF being made very ſlender, if a ſmall force be applied at the point *o* to preſs that end out from the wheel ABCD, it eaſily yields in that direction, turning as it were upon a center at G; it is alſo made to ſlide in a groove made in this ſtud in ſuch a manner that the end *o* may be placed at any required diſtance

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

Defcription of the Efcapement of Mr. Earnfhaw's time piece.

from the center of the verge. Having defcribed the feveral parts as they appear in the figure, we next come to their connexion or fituation with refpect to each other. Let the long fpring EF be fuppofed to be fo placed that the end of the flender fpring γi may project a little way over the point of the lifting pallet *in*, but not fo clofe but that the point of the pallet may pafs by the hooked end of the fpring EF without touching it; the head of the adjufting fcrew *m* is alfo fuppofed to bear gently on the inner fide of the faid fpring EF, or that neareft to the wheel, and at the fame time the locking pallet is fo placed that one of the teeth D, of the balance-wheel, may juft take hold of it. This pallet is not vifible in its proper place in the figure, being covered from fight by the fcrew *m*, and part of the fpring EF; its pofition is therefore reprefented by the dot *k*, on the oppofite fide of the wheel, having the tooth A juft bearing up againft it. From the above defcription of the feveral parts of the efcapement, and their connexion with each other, it will be eafy to fee the mode of its action, which is as follows.

A force being fuppofed to be applied to the balance-wheel, fo as to caufe it to move round in the direction of the letters ABCD, one of the teeth, as D, will come up againft the locking pallet (as reprefented at A, and the locking pallet by *k*). The wheel is then faid to be locked, being prevented from moving forward by this pin. Let the balance be now fuppofed to reft in its quiefcent pofition, and it will have the fituation reprefented in the figure; the lifting point *i*, of the pallet *in*, will be juft clear of the projecting end of the flender fpring, the face *hl* of the large pallet MSK will fall a little below the point of the tooth B, and the balance having its fpiral or helical fpring applied to it (which is here fuppofed on the other fide of the plate PQRS, and of courfe not vifible in the figure) remains perfectly at reft in this pofition. Now as the balance ULHV, and the two pallets MSK and *in*, are fixed faft to the verge, it is plain they muft all move together; let therefore the balance be carried a little way round in the direction of the letters VULH; by this motion the end *i* of the lifting pallet *in* will be brought to prefs up againft the projecting end of the flender fpring, and as this fpring is fixed on the fide of the fpring EF, neareft to the balance-wheel, the point *i* will prefs the two fprings together out from the balance-wheel; then, as only the point of the tooth D (fee its pofition at *k*) touches the locking

locking pallet, when the ſpring EF was at reſt again the head of the ſcrew *m*, it will, by the ſpring being preſſed out from the tooth, have ſlipped off (for the locking pallet which was before ſuppoſed at *k*, will now be at *a*, clear of the tooth A of the balance-wheel); the wheel being now at liberty will move round by the force ſuppoſed to be applied to it; but as the point *i* of the lifting pallet moves on and preſſes out the ſpring, the point *l* of the large pallet approaches towards the point of the tooth B of the balance-wheel, ſo that when the ſpring EF is ſufficiently puſhed out to unlock the wheel, the point *l* of the large pallet will be got to *d*, and in this poſition the point of the tooth B of the balance-wheel will fall upon it (ſee *Fig.* 2,) where the tooth B is repreſented in contact with the pallet at *l*; at the ſame time the point of the tooth D has juſt dropt off from the locking pallet *m*; the force of the wheel being by this means applied to the top of the pallet *hl*, gives an increaſed momentum to the balance, and aſſiſts it in its motion in the ſame direction, and by the continued motion of the large pallet in the direction MSK the point of the tooth B, which keeps preſſing and urging it forward, moves up towards the bottom of the face of the pallet towards *h*, until the plain flat ſurfaces of the tooth and pallet come into contact (ſee *Fig* 3); by this time the end *o* of the ſlender ſpring has dropt off from the point *i* of the lifting pallet, and the two ſprings have returned again into their quieſcent poſition, the ſpring EF gently bearing againſt the head of the adjuſting ſcrew *m*, and the locking pallet in a poſition to receive the next tooth C of the balance-wheel; (ſee the poſition of the point of the lifting pallet at *i*, *Fig.* 3, alſo the locking pallet at *m*, and the approaching tooth at C.) When the two ſurfaces of the tooth and pallet are thus in contact, the greateſt force of the wheel is exerted upon the pallet, and of courſe upon the balance moving with it. The tooth ſtill preſſing againſt the face of the pallet, and the pallet moving in the direction MSK, it at laſt drops off, (ſee *Fig* 4, where *m* repreſents the poſition of the locking pallet, C the poſition of the tooth of the wheel juſt before it drops upon it, and *l h* the poſition of the face of the large pallet, having the point of the tooth B juſt ready to leave it at *l*,) leaving the balance at perfect liberty to move on in the ſame direction in which it was going. Juſt as the point of the tooth B, which has been preſſing the large pallet round, is ready to leave it, the

Deſcription of the Eſcapement of Mr. Earnſhaw's time piece.

Description of the Escapement of Mr. Earnshaw's time piece.

the next tooth C of the wheel is almost in contact with the locking pallet *m* (see *Fig.* 4) so that the instant the tooth B drops off the wheel is again locked, and the action of that tooth upon the balance is finished. As the balance moves with the greatest freedom upon its pivots, the force of the tooth has given it a considerable velocity, so that the balance still keeps moving on in the same direction, after the pressure of the tooth is removed by slipping off from the pallet, until the force of the pendulum spring (which is not represented in the figure) being continually increased by being wound up, overcomes the momentum of the balance, which, for an instant of time, is then stationary, but immediately returns by the action of the pendulum spring, which exerts a considerable force upon it in unwinding itself. As the balance returns, the point *i* of the lifting pallet *in* passes by the ends of the two springs EF and *yo*, and, in passing by, pushes the projecting end, *o*, of the slender spring in towards the balance-wheel, until it has passed it; which, as soon as it has done, the projecting end *o* again returns and applies itself close to the hooked end of the spring EF, as before. The spring *yo* is made so slender, that it gives but little resistance to the balance, during the time the point *i* of the lifting pallet is passing it, and of course causes but little (if any) decrease in its momentum. During the time the point *i* of the lifting pallet is passing the small spring *yo*, the long spring EF remains steadily bearing against the head of the adjusting screw *m*, as the hooked end at *σ* just lets the end of the lifting pallet pass by without touching of it. As the spring has now been continually acting upon the balance, from the extremity of its vibration in the direction MSK, it has given it the greatest velocity, when the point *i* of the lifting pallet is passing the end *o* of the slender spring; for at this instant the spring which was wound up by the contrary direction of the balance, is now unwound again, or in the same state as it was in its quiescent position at first, and of course has no effect upon the balance at all in either direction; but the balance having now all the velocity it could acquire from the unwinding of the spring, goes on in the direction UVHL, until the force of this spring again stops it and brings it back again, moving in the same direction as at first, with a considerable velocity. By this return of the balance, the point *i* of the lifting pallet comes up again to the projecting end *o* of the slender spring, pushes

pushes back the long spring EF, and unlocks the wheel; and another tooth falling upon the face of the pallet *hl* gives fresh energy to the balance: and thus the action is carried on as before.

Instructions for making the Escapement.

The Escapement should be made in the following manner: The pivots of the balance axis should be the size of the verge-pivots of a good common sized pocket watch, and of the shape of *Fig*. 5. Pl. XIII, which will greatly add to their strength, the extreme end, or acting part only being straight; the jewel hole should be as shallow as possible, so as not to endanger cutting the pivot, and the part of action of the hole made quite back with only a very shallow chamfer behind to retain the oil; deep holes are very bad, for when the oil becomes glutinous, it will make the pivots stick so as to prevent the balance from its usual vibration. The pallet should be half the diameter of the wheel, or a little larger, for if smaller, the wheel will then have too much action on it, which will increase friction most considerably, and likewise cause the balance to swing so much farther to clear the wheel; consequently a check in the motion of the balance may stop the watch. The face of the pallet should run in a line of equal distance between the centre of the pallet and its extremity, and not in a right line to its centre, that is an increase of friction, and a loss of that power which is obtained by the wheel acting on the extremity of the pallet: this is clearly proved by time, by the hole worn by the points of teeth in all pallets that run in a line to the centre. The scape wheel teeth should form the same direction as the face of the pallet, under cut for the said purpose of avoiding friction, and maintaining the power, and for safe locking. The points of the wheel teeth must not be rounded off, but left as sharp as possible. The pivots of the scape wheel are to be a very little larger than the balance pivots.

Detent with a spring joint.

The wheel is locked by a spring instead of a detent with pivots, as the French have made them, for those pivots must have oil, and when the oil thickens then the spring of the pivot detents is so affected by it as to prevent the detent from falling into the wheel quick enough, the consequence of this is irregular time and stoppage of the watch, and if ever such a watch went well for twelve months, chance must have had by far the most hand in it.

How to place the detent, pallets, &c.

When the spring is planted on the side of the wheel, as in my escapements, the part on which the wheel rests should be a little a little ſhort of a right angle, ſo that the wheel may have a tendency to draw the ſpring into it, for if ſtopped the other way, or beyond a right angle it will have a tendency to puſh the ſpring out; in that caſe the wheel will have liberty to run: the wheel ſhould take no more hold on the ſpring than juſt ſufficient to ſtop it, for if more, friction will be increaſed. The ſmall return ſpring ſhould be as thin as poſſible at the end faſtened to the other ſpring, but at the outer end a little thicker; the ſpring ſhould be planted down as cloſe to the wheel as to be juſt free of it. The diſcharging pallet about one-third, or near one-half the ſize of the large or main pallet, the face of it in a right line to the centre, the back of it a little rounding and off from the centre. Great care muſt be uſed in taking off the edges of this diſcharging piece, to make it round to prevent cutting the ſpring, nor can it be made too thin ſo it does not cut; the end of it neareſt the ballance ſhould be a little more out from the centre of the ballance axis than the lower part of it towards the potence, for counteracting the natural tendency of the ſpring downwards from the preſſure of the ſcape wheel; and that part of the ſpring on which the wheel reſts ſhould be ſtopped a little down to give the wheel a tendency to force it up, to counteract the natural inclination the wheel has to draw it down by its preſſure on it.

Conſtruction of the balance with its compenſation weights, &c.

The balance is to be made of the beſt ſteel, and turned from its own centre to its proper ſize, then put it into a crucible with as much of the beſt braſs as when melted will cover it. The braſs melted will adhere to the ſteel (for if any other metal is uſed by way of ſolder, that watch cannot go well), then turn it to its proper thickneſs, and hollow it out ſo as to leave the ſteel rim about the thickneſs of a repeating ſpring to a ſmall ſized repeating watch, turn the braſs to twice or near three times that thickneſs of ſteel, croſs it out with only one arm ſtraight acroſs the centre, and at each end of the arm fix two ſcrews oppoſite to each other through the rim of the balance to regulate the watch to time, the diameter of the heads of theſe ſcrews about equal to the thickneſs of the balance, a little more or leſs is not material. The compenſation weights ſhould be made of the beſt braſs and well hammered, and a groove turned to let the rim of balance into it, and this ſhould be cut into fourteen equal parts by a wheel engine,

gine, then you will have ſeven pair of pieces of equal ſize and weight; two of theſe pieces being ſcrewed on the rim of the balance at equal diſtances will produce an equilibrium, a balance in the full ſenſe of the word, equal in all its parts. In making balances great care ſhould be taken that they get no bruiſes or bendings, for if they get a bruiſe on one ſide ſo as to indent the metal, that part will be leſs affected by heat and cold than the other parts which have not received the ſame violence to cloſe its pores.

Adjuſtment to keep time in all temperatures.

To adjuſt the balance in heat and cold—put the watch into about 85 or 90 degrees of heat, by the common thermometer, mark down exactly how much it gains or loſes in 12 hours, then put it into as ſevere cold as you can get for 12 hours, and if it gains one minute more in 12 hours in cold than in heat, move the compenſation weights farther from the arm of the balance about $\frac{1}{8}$ of inch, and if it gains one minute more in 12 hours in heat than in cold move the weights $\frac{1}{8}$ of inch nearer to the arm of the balance, and ſo on in the like proportion, trying it again and again till you find the watch go the ſame in whatever change of heat or cold you put it.

—and in all poſitions.

Much difficulty has fallen to the lot of watchmakers in the endeavour to make timekeepers go nearly the ſame in the different poſitions. I have had my ſhare of this, but it is now over; by far the greateſt part of this difficulty ariſes from the balance ſpring not being properly made. But if the ſpring is made, as I ſhall deſcribe hereafter, you have only to make the balance of equal weight and it will go within a few ſeconds per day in all poſitions alike, and if it vibrates not more than one circle and a $\frac{1}{4}$, by applying a ſmall matter of weight to that part of the balance which is downward when in the poſition that it loſes moſt, will correct it with great accuracy; but if it vibrates more than one circle and a $\frac{1}{4}$, then it will require the weight to be above inſtead of below; and after the watch has been going a few months and its vibration ſhortens to one $\frac{1}{4}$ circle, then it will go worſe and worſe by reaſon of the weight being in the wrong place; therefore, to avoid this evil, it is abſolutely neceſſary to confine the vibrations to one $\frac{1}{4}$ circle, which will produce the moſt ſteady performance. It is common for watchmakers to adore a timekeeper when they ſee it vibrate a circle and a half, or more, and form an opinion of its excellence from this only; but I know

Rule. If the balance be in poiſe it will go nearly alike in all poſitions. Correct it by adding weight to the loweſt part when in loſing poſition if the ſame vibration be about 200°; if more degrees then add to the upper part. Hence a moderate vibration is beſt.

know from experience what would be the confequence, and have been condemned, becaufe, when I have feen fuch watches I faid I faw enough to declare that it would not give very accurate performance.

Concerning the balance fpring.

Balance fpring. To find out the invifible properties of this apparent fimple part of the machine, has given much more trouble than all the reft. I defpaired of bringing timekeepers to the ftate I have done, and unlefs thofe hidden properties are known to timekeeper makers, however well they may execute all other parts they will find their moft fanguine expectations fruftrated. I have feen watchmakers boaft of their timekeepers going well for a month or two, and from the knowledge I had of the effects produced by the balance fpring, I have told them that a month or two more would deftroy their hopes. The cylindrical fpring being in all its turns of equal diftance from the centre, in courfe every turn will be of equal ftrength, and called ifochronal, and believed that all vibrations whether long or fhort would be performed in the fame time; but this is not true, for if a man is to go four miles in the fame time as he has gone one mile, he cannot do it with the fame power; no, he muft have impelling force to quicken his motion, or he will be four times as long in doing it. Therefore inftead of the fpring being equal in all its parts, it muft be made to increafe in thicknefs to the outer end, in fuch proportion as will caufe the balance when thrown to a greater diftance to return fo much the quicker to make them equal; by long perfeverance I found how to make fuch fprings, and then I thought I had got all I wifhed for. But cruel difappointment nearly broke my heart, for I found I had yet another difficulty to break down, as my watches with fuch perfect fprings were continually lofing on their rates. What farther to do I knew not, and I own I was nearly if not quite mad. But obftinate in the caufe and refolving not to give it up but with life, perfeverance came once more to my aid—and with ftill more unremitting ftudy, which nearly finifhed me, before I applied the following remedy for the before mentioned evil, I found, in the courfe of reafoning on bodies, that watch fprings relax and tire like the human frame, when kept conftantly in motion, and this may be proved by the following experiment: let a watch that has been going a few months go down, let it be down for a week

It is made tapering.

Springs are fubject to a relaxation of force which is regained by reft.

or

or two, or more, then set it going, and if it be a good timekeeper so as not to be affected by the weather it will go some seconds per day faster than it did when it was let down, but it will again lose its quickness in a gradual manner gaining less and less till it comes to its former rate. Therefore finding that isochronal springs would not do—and likewise having made springs of such shape as would render long and short vibrations equal in time—constantly lose the longer the watch went, I then made them of such shape as to gain in the short vibrations about five or six seconds per day more than the long ones, this quantity could only be found by long experience, and the way I proved this was to try the rate of the watch with the balance vibrating about $\frac{1}{3}$ of circle, then tried its rate vibrating one circle and a $\frac{1}{4}$, and if the short vibrations go slower than the long ones that watch will lose on its rate, and if they are equal, it will likewise lose, but that only from relaxation, and if it gains in the short vibrations more than five or six seconds in twenty-four hours it will in the long run gain on its rate, but if not more than that quantity, and the timekeeper is perfect in heat and cold and every other part, the above properties will render it deserving of the name of a perfect timekeeper, and this is a principal cause of my timekeepers excelling all others, and this the principal cause of some of my timekeepers going better than others, though made by me, the springs of them being made to accord more exactly to the above proportions; and this is the cause which has enabled me to foretel what my timekeepers would do, which Dr. Maskelyne, Mr. Crosley, and others can testify. The above effect is produced as follows. I find the common relaxation of balance springs to be about five or six seconds per day on their rates in the course of a year, therefore if the short vibrations are made by the shape of the spring to go about that quantity faster than the long ones, and as the spring relaxes in going by time so the watch accumulates in dirt and thickening of the oil which shortens the vibrations, the short ones then being quicker, compensates for the evil of relaxation of the balance spring. From this it is plain, that the causes of error in timekeepers are not undefined and vague in their nature, which has been supposed; for when it is certain that all causes of error may be over compensated we cannot despair of finding the medium, and which

This gradual effect causes a loss on the rate which may be compensated by giving greater speed to the shorter vibrations in the first construction.

which may be eafily proved by examining the going of my timekeepers. It will there appear that what errors, they are fubject to, arife from caufes certain and natural, and in courfe may be corrected by art*.

IX.

Experimental Enquiry into the Proportion of the feveral Gafes or Elaftic Fluids, conftituting the Atmofphere. By JOHN DALTON.†

On the component parts of the atmofphere.

IN a former paper which I fubmitted to this fociety, "on the conftitution of mixed gafes," I adopted fuch proportions of the fimple elaftic fluids to conftitute the atmofphere as were then current, not intending to warrant the accuracy of them all, as ftated in the faid paper; my principal object in that effay was, to point out the *manner* in which mixed elaftic fluids exift together, and to infift upon what I think a very important and fundamental pofition in the doctrine of fuch fluids:—namely, that the elaftic or repulfive power of each particle is confined to thofe of its own kind; and confequently the force of fuch fluid, retained in a given veffel, or gravitating, is the fame in a feparate as in a mixed ftate, depending upon its proper denfity and temperature. This principle accords with all experience, and I have no doubt will foon be perceived and acknowledged by chemifts and philofophers in general; and its application will elucidate a variety of facts, which are otherwife involved in obfcurity.

Objects of this effay.

The objects of the prefent effay are,

1. To determine the weight of each fep. atmofphere.

1. To determine the weight of each fimple atmofphere, abftractedly; or, in other words, what part of the weight o the whole compound atmofphere is due to azote; what to oxygen, &c. &c.

—and the relative weights of the gafes at the furface of the earth

2. To determine the relative weights of the different gafes in a given volume of atmofpheric air, fuch as it is at the earth's furface.

* To this communication Mr. Earnfhaw has annexed two plates with defcriptions, fhewing the parts of his time-piece; all which, except thofe of the Efcapement (which we have given) are capable of the fame variations as thofe of any other good movements. He afferts that the beft train for time keepers is 18,000; that the fcape wheel for pocket ones fhould have 15 teeth, and for box ones 13 teeth.

† Manchefter Mem. Vol. V.

3. To investigate the proportions of the gases to each other, such as they ought to be found at different elevations above the earth's surface. —as well as at different elevations.

To those who consider the atmosphere as a chemical compound, these *three* objects are but *one*; others, who adopt my hypothesis, will see they are essentially distinct. With respect to the first: It is obvious, that, on my hypothesis, the density and elastic force of each gas at the earth's surface, are the effects of the weight of the atmosphere of that gas solely, the different atmospheres not gravitating one upon another. Whence the first object will be obtained by ascertaining what share of elastic force is due to each gas in a given volume of the compound atmosphere; or, which amounts to the same thing, by finding how much the given volume is diminished under a constant pressure, by the abstraction of each of its ingredients singly. Thus, if it should appear that by extracting the oxygenous gas from any mass of atmospheric air, the whole was diminished $\frac{1}{5}$ in bulk, still being subject to a pressure of 30 inches of mercury; then it ought to be inferred that the oxygenous atmosphere presses the earth with a force of six inches of mercury, &c.

Now each single atmosphere presses by its whole weight; which is measured by its spring and that by its volume.

Take away one of the gases and the loss of volume represents its pressure and the weight of that atmosphere.

In order to ascertain the second point, it will be further necessary to obtain the specific gravity of each gas; that is, the relative weights of a given volume of each in a pure state, subject to the same pressure and temperature. For the weight of each gas in any given portion of atmospheric air, must be in the compound ratio of its force and specific gravity.

The weights of each gas in given volume had from sp. gravity.

With respect to the third object, it may be observed, that those gases which are specifically the heaviest must decrease in density the quickest in ascending. If the earth's atmosphere had been a homogeneous elastic fluid of the same weight it is, but ten times the specific gravity, it might easily be demonstrated that no sensible portion of it could have arisen to the summits of the highest mountains. On the other hand, an atmosphere of hydrogenous gas of the same weight, would support a column of mercury nearly 29 inches on the summit of Mount Blanc.

The proportions at different heights are obtained from the progression with each gas in the same manner as in computation for the whole atmosphere.

The several gases constantly found in every portion of atmospheric air, and in such quantities as are capable of being appreciated, are azotic, oxygenous, aqueous vapour, and carbonic acid. It is probable that hydrogenous gas also is conſtantly preſent; but in ſo ſmall proportion as not to be detected by any teſt we are acquainted with; it muſt therefore be confounded in the large maſs of azotic gas.

1. *On the weight of the Oxygenous and Azotic Atmoſpheres.*

Proceſſes for determining the oxigen in the atmoſphere.

Various proceſſes have been uſed to determine the quantity of oxygenous gas.

1. with nitrous gas.
2. with ſulphuret.
3. Exploſion with hidrogen.
4. Expoſure to green ſulphat of iron.
5. Burning phoſphorus.

1. The mixture of nitrous gas and air over water.

2. Expoſing the air to liquid ſulphuret of potaſh or lime, with or without agitation.

3. Exploding hydrogen gas and air by electricity.

4. Expoſing the air to a ſolution of green ſulphat or muriat of iron in water, ſtrongly impregnated with nitrous gas.

5. Burning phoſphorus in the air.

All produce the ſame reſult.

In all theſe caſes the oxygen enters into combination and loſes its elaſticity; and if the ſeveral proceſſes be conducted ſkilfully, the reſults are preciſely the ſame from all. In all parts of the earth and at every ſeaſon of the year, the bulk of any given quantity of atmoſpheric air appears to be reduced nearly 21 per cent. by abſtracting its oxygen. This fact, indeed, has not been generally admitted till lately; ſome chemiſts having found, as they apprehended, a great difference in the quantity of oxygen in the air at different times and places; on ſome occaſions 20 per cent, and on others 30, and more of oxygen are ſaid to have been found. This I have no doubt was owing to their not underſtanding the nature of the operation and of the circumſtances influencing it. Indeed it is difficult to ſee, on any hypotheſis, how a diſproportion of theſe two elements ſhould ever ſubſiſt in the atmoſphere.

The oxigen and azote are not variable.

The firſt proceſs with nitrous gas tho' diſcredited, is here perfected.

As the firſt of the proceſſes above-mentioned has been much diſcredited by late authors, and as it appears from my experience to be not only the moſt elegant and expeditious of all the methods hitherto uſed, but alſo as correct as any of them, when properly conducted, I ſhall, on this occaſion, animadvert upon it.

Inſtructions for the proceſs.

Pure nitrous gas.

1. Nitrous gas may be obtained pure by nitric acid diluted with an equal bulk of water poured upon copper or mercury; little or no artificial heat ſhould be applied. The laſt product of gas this way obtained, does not contain any ſenſible portion of azotic gas: at leaſt it may eaſily be got with leſs than two or three *per cent.* of that gas: It is probably nearly free from nitrous oxide alſo, when thus obtained.

2.

2. If 100 meafures of common air be put to 36 of pure nitrous gas in a tube 3-10th of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 meafures, and exhibit no figns of either oxygenous or nitrous gas.

Mixture 100 air and 36 n. gas in a narrow tube. Refidue about 80 azote.

3. If 100 meafures of common air be admitted to 72 of nitrous gas in a wide veffel over water, fuch as to form a thin ftratum of air, and an immediate momentary agitation be ufed, there will, as before, be found 79 or 80 meafures of pure azotic gas for a refiduum.

Mixture 100 air and 72 n. gas in a wide veffel with agitation. Refidue as before 80 azote.

4. If, in the laft experiment, *lefs* than 72 meafures of nitrous gas be ufed, there will be a refiduum containing oxygenous gas; if *more*, then fome refiduary nitrous gas will be found.

Intermediate proportion of n. gas leaves either n. gas or oxigen with the azote.

Thefe facts clearly point out the theory of the procefs: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former cafe *nitric* acid is the refult; in the latter *nitrous* acid: but as both thefe may be formed at the fame time, one part of the oxygen going to *one* of nitrous gas, and another to *two*, the quantity of nitrous gas abforbed fhould be variable; from 36 to 72 *per cent*. for common air. This is the principal caufe of that diverfity which has fo much appeared in the refults of chemifts on this fubject. In fact, all the gradation in quantity of nitrous gas from 36 to 72 may actually be obferved with atmofpheric air of the fame purity; the wider the tube or veffel the mixture is made in, the quicker the combination is effected, and the more expofed to water, the greater is the quantity of *nitrous* acid and the lefs of *nitric* that is formed.

Theory of the procefs. In the firft cafe nitric gas was formed; in the latter nitrous.

To ufe nitrous gas for the purpofe of eudiometry therefore, we muft attempt to form *nitric acid* or *nitrous* wholly, and without a mixture of the other. Of thefe the former appears from my experiments to be moft eafily and moft accurately effected. In order to this a narrow tube is neceffary; one that is juft wide enough to let air pafs water without requiring the tube to be agitated, is beft. Let little more nitrous gas than is fufficient to form nitric acid be admitted to the oxygenous gas; let no agitation be ufed; and as foon as the diminution appears to be over for a moment let the refiduary gas be transferred to another tube, and it will remain without any further diminution of confequence. Then $\frac{7}{19}$ of the lofs will be due

Practical refult. Operate fo as to form the nitric gas.

to oxygen. The transferring is neceſſary to prevent the nitric acid formed and combined with the water, from abſorbing the remainder of the nitrous gas to form nitrous acid.

Method with ſulphuret.

Sulphuret of lime is a good teſt of the proportion of oxygen in a given mixture, provided the liquid be not more than 20 or 30 per cent. for the gas (atmoſperic air); if the liquid exceed this, there is a portion of azotic gas imbibed ſomewhat uncertain in quantity.

Volta's method.

Volta's eudiometer is very accurate as well as elegant and expeditious: according to Monge, 100 oxygen require 196 meaſures of hydrogen; according to Davy 192; but from the moſt attentive obſervations of my own, 185 are ſufficient. In atmoſpheric air I always find 60 per cent. diminution when fired with an exceſs of hydrogen; that is, 100 common air with 60 hydrogen, become 100 after the exploſion, and no oxygen is found in the reſiduum; here 21 oxygen take 39 hydrogen.

2. *Of the Weight of the Aqueous Vapour Atmoſphere.*

To find the weight of aqueous vapour in the atmoſphere.

I have, in a former eſſay, (Mancheſter Mem. vol. 5. p. 2, page 559.) given a table of the force of vapour in *vacuo* for every degree of temperature, determined by experiment; and in the ſequel of the eſſay, have ſhewn that the force of vapour in the atmoſphere is the very ſame as in *vacuo*, when they are both at their utmoſt for any given temperature. To find the force of aqueous vapour in the atmoſphere, therefore, we have nothing more to do than to find that degree of cold at which it begins to be condenſed, and oppoſite to it in the table abovementioned, will be found the force of vapour. From the various facts mentioned in the eſſay it is obvious, that vapour contracts no chemical union with any of the gaſes in the atmoſphere; this fact has ſince been enforced in the Annales de Chimie, vol. xlii. by Clement and Deſorme.

M. De Sauſſure found by an excellent experiment, that dry air of 64° will admit ſo much vapour as to increaſe its elaſticity, $\frac{1}{54}$. This I have repeated nearly in his manner, and found a ſimilar reſult. But the table he has given us of aqueous vapour at other temperatures is very far wrong, eſpecially at temperatures diſtant from 64°. The numbers were not the reſult of direct experiment, like the one above. If we could obtain the temperatures of all parts of the earth's ſurface, furface, for any given time, a mean of them would probably be 57° or 58°. Now if we may fuppofe the force of vapour equivalent to that of 55°, at a medium, it will, from the table, be = to .443 of mercury; or, nearly $\frac{1}{70}$ of the whole atmofphere. This it will be perceived is calculated to be the weight of vapour in the whole atmofphere of the earth. If that incumbent over any place at any time be required, it may be found as directed above.

It is on an average about one feventieth.

3. Of the Weight of the Carbonic Acid Atmofphere.

From fome obfervations of Humboldt, I was led to expect about $\frac{1}{100}$ part of the weight of the atmofphere to be carbonic acid gas: but I foon found that the proportion was immenfely over-rated. From repeated experiments, all nearly agreeing in their refults, and made at different feafons of the year, I have found, that if a glafs veffel filled with 102,400 grains of rain water be emptied in the open air, and 125 grains of ftrong lime water be poured in, and the mouth then clofed; by fufficient time and agitation, the whole of the lime water is juft faturated by the acid gas it finds in that volume of air. But 125 grains of the lime water ufed require 70 grain meafures of carbonic acid gas to faturate it: therefore, the 102,400 grain meafures of common air contain 70 of carbonic acid; or $\frac{1}{1460}$ of the whole. The weight of the carbonic acid atmofphere then is to that of the whole compound as 1:1460; but the weight of carbonic acid gas in a given portion of air at the earth's furface, is nearly $\frac{1}{1000}$ of the whole; becaufe the fpecific gravity of the gas is $1\frac{1}{2}$ that of common air. I have fince found that the air in an affembly, in which two hundred people had breathed for two hours, with the windows and doors fhut, contained little more than 1 per cent. of carbonic acid gas.

Deduction of the weight of carbonic acid in the atmofphere about one thoufandth.

Having now determined the force with which each atmofphere preffes on the earth's furface, or in other words, its weight; it remains next to enquire into their fpecific gravities.

These

Specific gravities of gases.

These may be seen in the following Table.

Atmospheric air - - - - -	1.000
Azotic gaz - - - - - -	.966
Oxygenous gaz - - - - -	1.127
Carbonic acid gas - - - -	1.500
Aqueous vapour - - - - -	.700
Hydrogenous gas - - - -	.077*

Kirwan and Lavoisier are my authorities for these numbers; except oxygenous gas and aqueous vapour. For the former I am indebted to Mr. Davy's Chemical Researches; his number is something greater than theirs: I prefer it, because, being determined with at least equal attention to accuracy with the others, it has this further claim for credit, that 21 parts of gas of this specific gravity, mixed with 79 parts of azotic gas, make a compound of exactly the same specific gravity as the atmosphere, as they evidently ought to do, setting aside the unfounded notion of their forming a *chemical* compound. The specific gravity of aqueous vapour I have determined myself both by analytic and synthetic methods, after the manner of De Saussure; that is, by abstracting aqueous vapour of a known force from a given quantity of air, and weighing the water obtained—and admitting a given weight of water to dry air and comparing the loss with the increased elasticity. De Saussure makes the specific gravity to be ,71 or ,75; but he used caustic alkali as the absorbent, which would extract the carbonic acid as well as the aqueous vapour from the air. From the experiments of Pictet and Watt, I deduce the specific gravity of aqueous vapour to be ,61 and ,67 respectively. Upon the whole, therefore, it is probable that ,7 is very nearly accurate.

We have now sufficient data to form tables answering to the two first objects of our enquiry.

* The specific gravity of hydrogen must be rated too low: if 100 oxygen require 185 hydrogen by measure, according to this 89 oxygen would require only 11 hydrogen to form water; whereas 85 require 15. Hydrogen ought to be found about $\frac{1}{10}$ part of the weight of common air.

I. Table

I. *Table of the Weights of the different Gases constituting the Atmosphere.*

	Inch of Mercury.
Azotic gas - - - - -	23.36
Oxygenous gas - - - -	6.18
Aqueous vapour - - - -	.44
Carbonic acid gas - - - -	.02
	30.00

Absoluteweights of the different gases in the whole atmosphere.

II. *Table of the proportional Weights of the different Gases in a given Volume of Atmospheric Air, taken at the Surface of the Earth.*

	per cent.
Azotic gas - - - - -	75.55
Oxygenous gas - - - -	23.32
Aqueous vapour - - - -	1.03*
Carbonic acid gas - - - -	.10
	100.00

Weights of the different gases in equal bulks at the earth's surface.

III. *On the Proportion of Gases at different Elevations.*

Computation of the proportion of gases above the earth's surface.

It is not notably different at any accessible height.

M. Berthollet seems to think that the lower strata of the atmosphere ought to contain more oxygen than the upper, because of the greater specific gravity of oxygenous gas, and the slight affinity of the two gases for each other. (See Annal. de Chimie, Tom. 34. page 85.) As I am unable to conceive even the possibility of two gases being held together by affinity, unless their particles unite so as to form *one* centre of repulsion out of two or more (in which case they become *one* gas) I cannot see why rarefaction should either decrease or increase this supposed affinity. I have little doubt, however, as to the fact of oxygenous gas observing a diminishing ratio in ascending; for, the atmospheres being independent on each other, their densities at different heights must be regulated by their specific gravities. Hence, if we take the azotic atmosphere

* The proportion of aqueous vapour must be understood to be variable for any one place: the others are permanent or nearly so.

ſphere as a ſtandard, the oxygenous and the carbonic acid will obſerve a decreaſing ratio to it in aſcending, and the aqueous vapour an increaſing one. The ſpecific gravity of oxygenous and azotic gaſes being as ſeven to ſix nearly, their diminution in denſity will be the ſame at heights reciprocally as their ſpecific gravities. Hence it would be found, that at the height of Mount Blanc (nearly three Engliſh miles) the ratio of oxygenous gas to azotic in a given volume of air, would be nearly as 20 to 80;—conſequently it follows that at any ordinary heights the difference in the proportions will be ſcarcely if it all perceptible *.

X.

Obſervation which indicates a ſpontaneous Decompoſition of nitrous Acid and Formation of Ammonia. By D. A.

To Mr. NICHOLSON.

SIR,

Decompoſition of nitrous acid.

I SEND you a ſtatement of the following fact, in caſe it may not hitherto have been obſerved; it ſeems to ſhew the mutual decompoſition of nitrous acid and atmoſpheric air; but the explanation of the theory I will leave to you, or ſome of your learned correſpondents. A phial of bright orange coloured nitrous acid, ſo looſely ſtopped that bubbles of gas eſcaped every five or ten minutes, having ſtood within a few inches of a bottle of muriatic acid, cloſely ſtopped for above a twelvemonth, my attention was attracted by obſerving a white incruſtation of ſalts upon the label paper of the laſt mentioned phial. To determine their nature, diſſolved them in diſtilled water; dropped a little nitric acid in, to ſaturate any uncombined alcalies; then with nitrate of ſilver, a copious precipitate was formed, which indicates the muriatic to be the acid; when I ſaturated the acids with pure potaſs, the

* Air brought from the ſummit of Helvelyn, in Cumberland (1100 yards above the ſea—Barometer being 26,60) in July 1804, gave no perceptible difference from the air taken in Mancheſter.—M. Gay-Luſſac determines the conſtitution of air brought from an elevation of four miles to be the ſame as that at the earth's ſurface.

ſmell

ſmell reſembled ammonia; but owing to the ſolution being ſo extremely weak, was ſcarce perceptible; but on a finger being dipped into it, and held near a ſtopper, moiſtened with muriatic acid, evidently produced a white cloud, which diſappeared upon withdrawing the acid, and re-appeared on its approach; which teſt alone I think may be ſufficient to prove ammonia to have been the baſe. I may obſerve, that the ſalts were formed only on that part of the lable on which ſome muriatic acid had been ſpilt; the neck of the nitrous acid phial was covered with a moiſture, which had a conſiderable ammoniacal ſmell, and exhibited the ſame appearances with the moiſtened ſtopper, and was therefore uncombined ammonia, and ſeems to ſhew that the preſence of the muriatic acid was not neceſſary for its formation. I have endeavoured to be as conciſe as poſſible, and remain

Your conſtant reader,

April 17, 1806. D. A.

SCIENTIFIC NEWS.

*Note on the Porcelain of Reaumur Communicated by Veau de Launai**.

M. PECARD of Tours, manufacturer of Rouen ſtone ware, has repeateded in his furnace Reaumur's experiment of transforming glaſs into porcelain; mentioned in the memoir of the Academy of Sciences, for the year 1739, p. 370. M. Pecard obtained a devitrification as complete within as without. His experiment was made upon a common glaſs bottle from the Ancenis Foundry. The bottle was filled with Nevers ſand, and depoſited in a *ſagger,* which was afterwards filled up with the ſame ſort of ſand. The ſagger or caſe was placed with others, containing earthenware in the chimney or upper part of the furnace, and heated as uſual. When the operation was finiſhed, and the furnace was ſufficiently cooled, the bottle was taken from its bed of ſand in the ſagger, and emptied of its contents. The bottle had undergone no alteration of ſhape; but its green colour and tranſparency were exchanged for milky opacity, equally ſpread over all parts of the bottle. In this, his firſt experiment,

Reaumur's porcelain made in an improved ſtate.

* Journal de Phiſique, Vol. LXI. p. 401.

periment, M. Pecard has obtained a much more equal devitrification than that procured by Reaumur; who remarks in his memoir, that he thinks it not impoffible that this point of equality between the internal and external parts may be obtained.

This fubftance is much harder than glafs; it readily gives a fpark with fteel; and from the advantages it feems to hold forth in many refpects deferves to be made an object of inveftigation.

Darcet applies the glafs porcelain to ufeful purpofes.

A diftinguifhed chemift, who purfues the fteps of his father, whofe name will be ever dear to the fciences, and to thofe who cultivate them, M. Dárcet, has already made feveral experiments on this interefting fubject, which form part of a work not yet completed. He has made mullers of this fubftance, exceeding the hardnefs of flint; alfo capfules and other articles which eafily fupport the fire, and are not fubject to the power of re-agents, fuch as fulphuric acid, &c. The little coft of the materials whereof thefe veffels, &c. are fabricated, induce a hope that the labours of Reaumur on this fubject will be refumed, and carried on in a way that will be of utility in different arts.

Anatomical Work.

AN extenfive work on the anatomy of the organs of hearing in animals, generally, together with the phyfiology of their feveral parts, and a feries of accouftic phenomena intended to elucidate the fubject, is in forwardnefs for publication this fpring by Anthony Carlifle, F. R. S. F. L. S. and furgeon to the Weftminfter Hofpital.

Fig. 1. Mr. Earnshaw's Escapement.

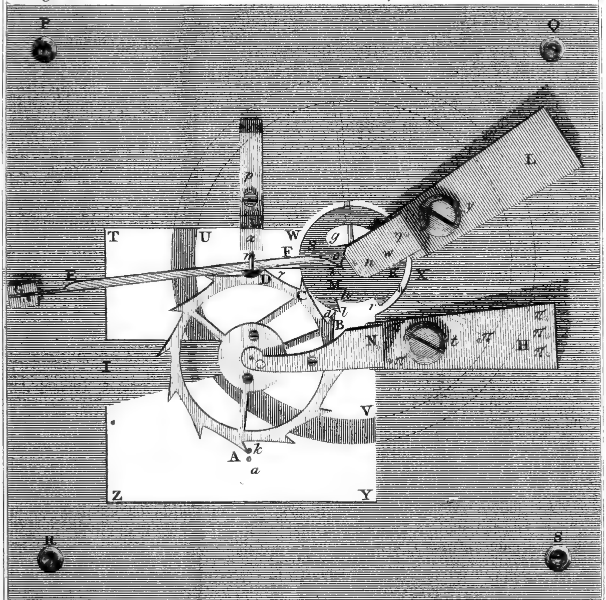

Fig. 2.

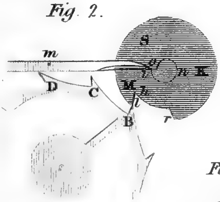

Fig. 3.

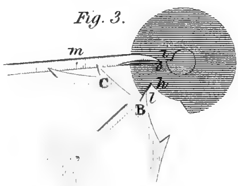

Fig. 4.

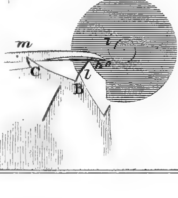

Fig. 5.

Engraved from Trans. R S Edinr Vol 6th P 185.

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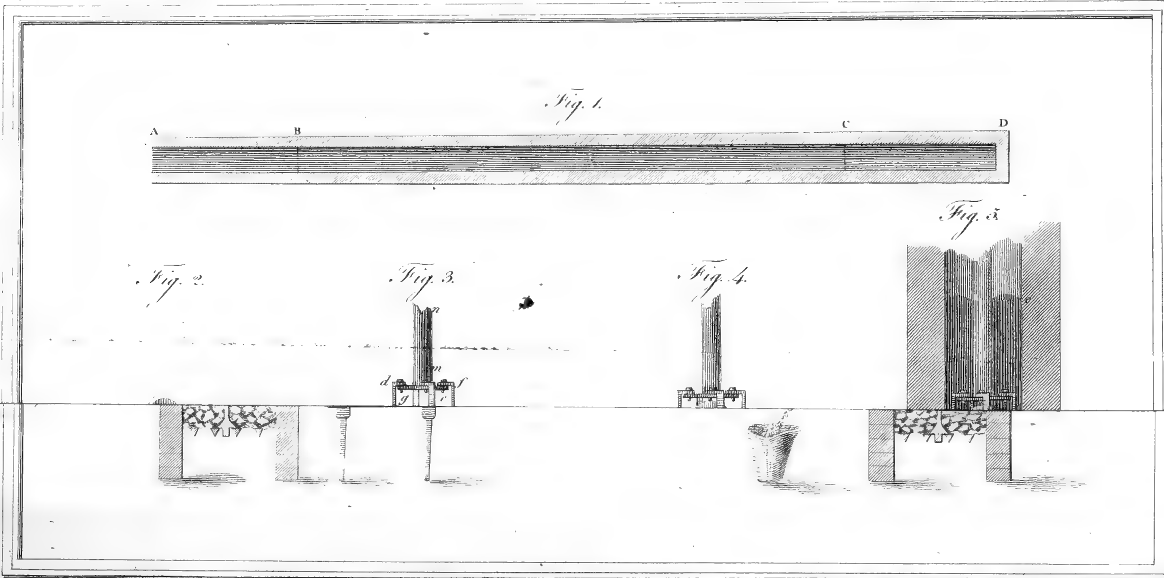

Sir J. H. delt

D. Lizars sculp.

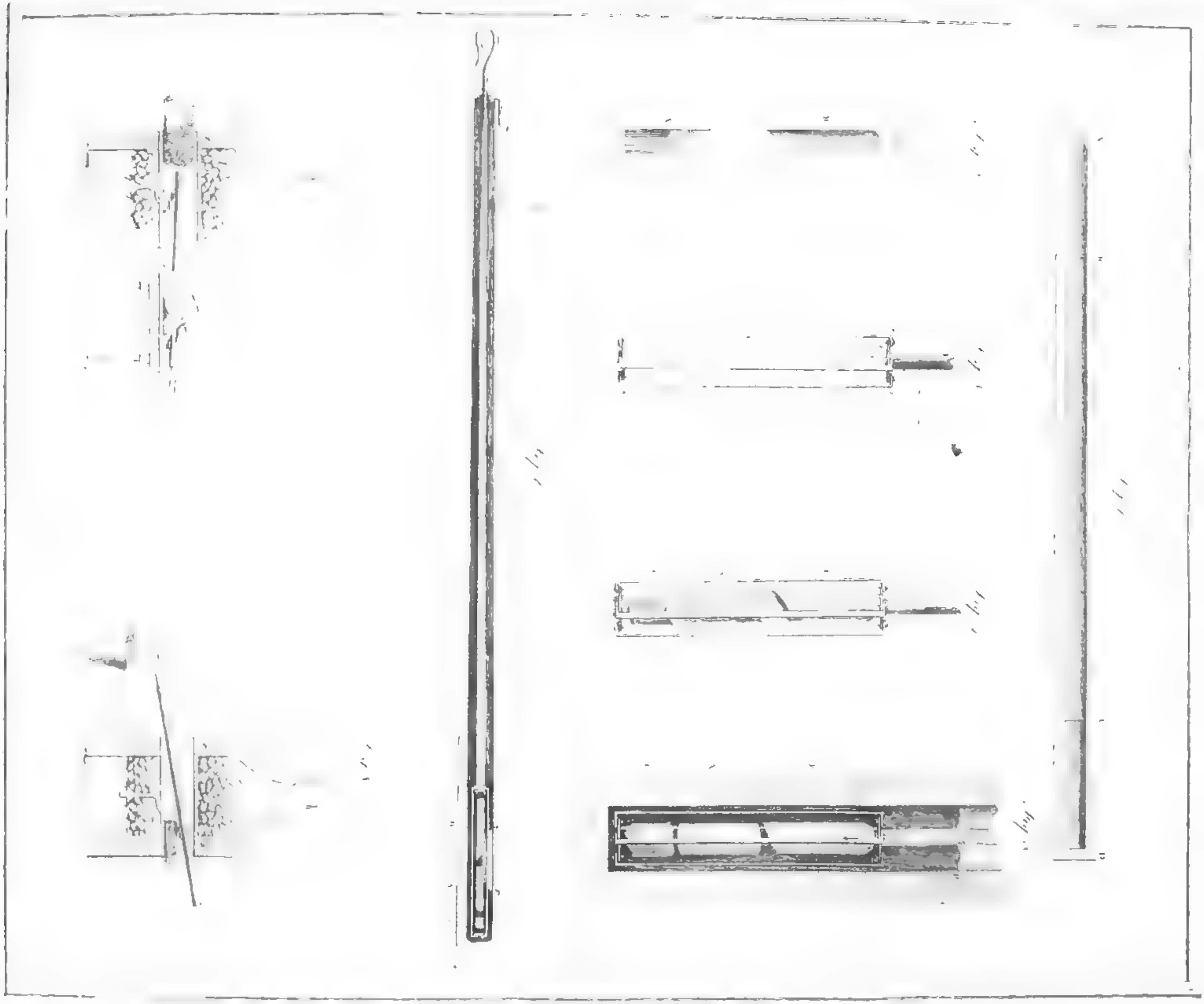

PLATE II.

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Fig. 9.

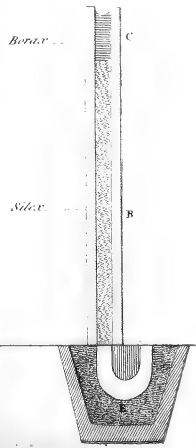


Fig. 10.

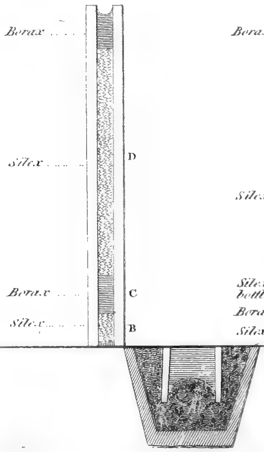


Fig. 11.

Fig. 12.

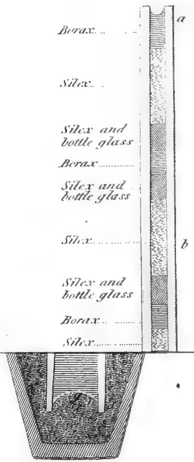
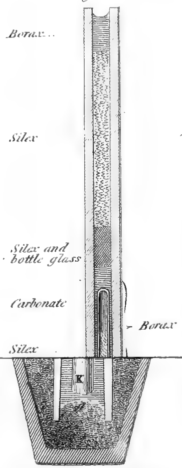


Fig. 13.



Sir J. H. delt. D. Lizars sculpt.

PLATE II

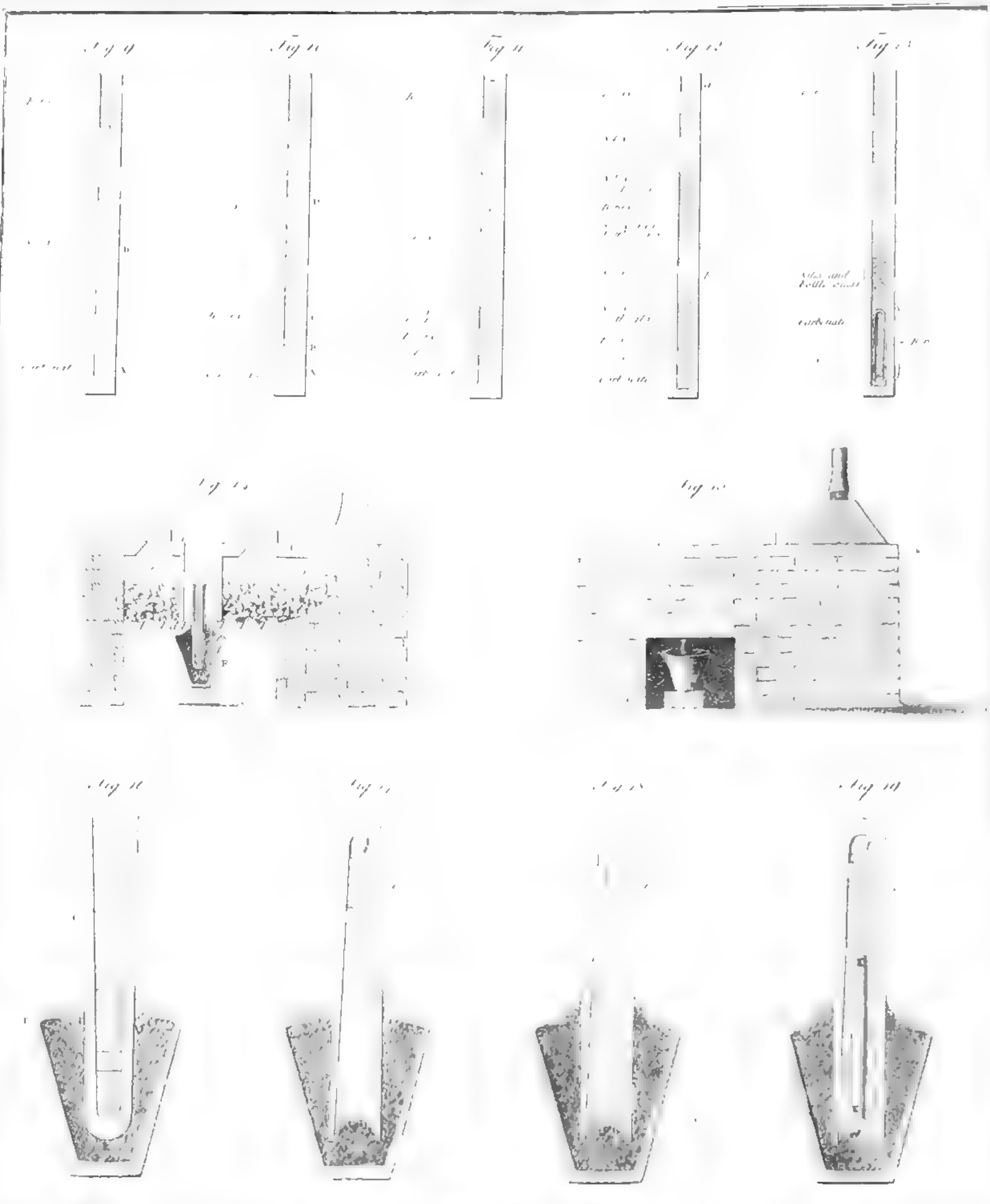


PLATE III.

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Fig. 22.

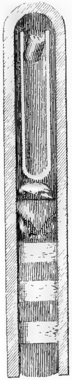


Fig. 23.

Fig. 24.

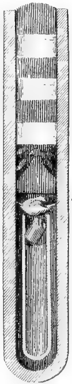


Fig. 25.

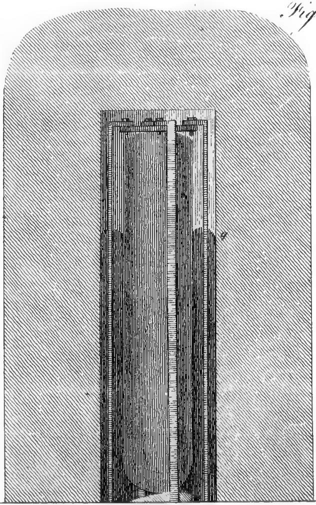


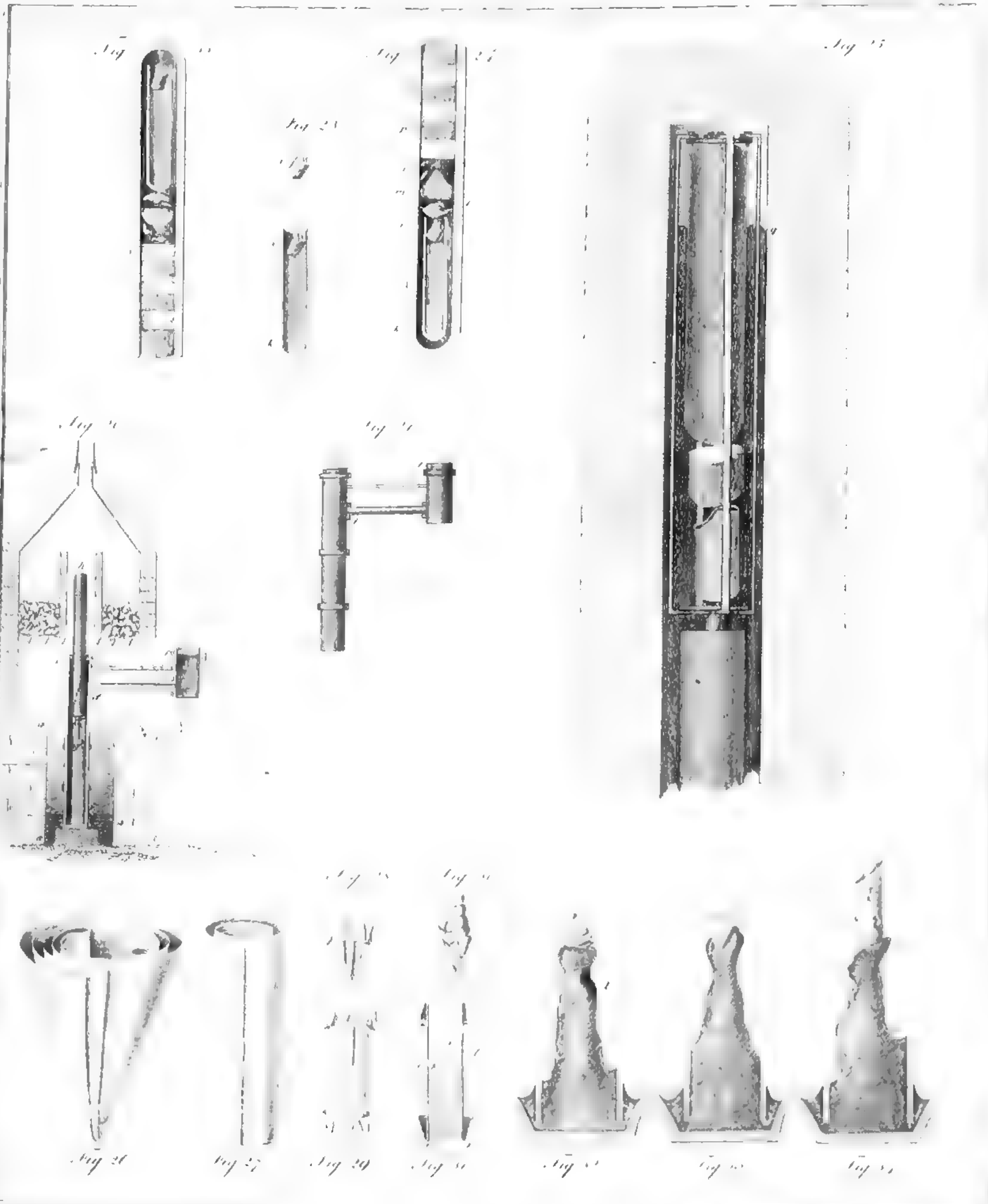
Fig. 20.

Fig. 21.

Sir J. H. delt. *D. Lizars sculp.*

Engraved from Trans. R. S. Vol. 6th P. 183

PLATE III



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THE END OF THE THIRTEENTH VOLUME.

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