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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS:

ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

VOL. I.

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PREFACE.

THERE is ſcarcely a more difficult taſk than to convey an adequate notion of a plan of ſome extent, within the limits of a ſhort diſcourſe. Whatever may be the promiſes, the hopes, or the intentions of an Author or Editor, the world, as in juſtice it ought, will ſuſpend its judgment till the actual performance ſhall afford the knowledge which is indiſpenſable for that purpoſe. Correſpondents will arrange for themſelves ſuch materials as they think fit to publiſh, without committing to memory any of the outlines which the authors of Journals may have drawn up. For theſe reaſons, little more need be ſaid concerning the plan, than is preſented in the title-page. Whatever the activity of men of ſcience or of art may bring forward, of invention or improvement, in any country or nation, within the poſſibility of being procured, by means as reſpectable as the motives that call for them, ſhall appear in this Journal; either in the form of ſhort notices, or the full deſcriptions of their reſpective authors, or the more ample report deduced from actual viſitation and enquiry. The relative magnitude of each object will eſtabliſh the rule from which either of theſe modes will be adopted. Arrangements have already been made, channels of communication opened, and other correſpondences are in proſpect, which muſt increaſe, in value and extent, proportionate to the importance and curioſity of the ſubjects to be diſplayed in this work, and the impartiality and care with which they ſhall be treated.

In a former Addreſs to the Public the Author has mentioned the advantages in regard to accuracy and fidelity which he apprehends muſt reſult from the conductor of a work of this nature becoming, in a certain degree, reſponſible by name for its contents: he has alluded to the general tenor of his purſuits, as known to the Public, and in ſome reſpect qualifying him to engage in ſuch a taſk; and he has deprecated the ſuppoſition of any vain pretence to ſuperiority, by remarking, that no one could ſerve the Public in this way if he were to wait with the abſurd hope of firſt bringing his own knowledge to a ſtate of perfection. This is all that need be ſaid relating to himſelf.

The leading character on which the ſelection of objects will be grounded is utility; and next to this, novelty and originality. The Author's reſearches and collections, and thoſe of his friends, will afford a conſiderable portion of new and curious matter, ſufficient to render the work intereſting, even to that extreme few who are ſo fortunate as to have acceſs to all the expanded ſources of philoſophical intelligence. But in the department of perfectly original matter, much of prudence is required to be exerciſed, in order that the claim of novelty may not operate to the excluſion of much more valuable and important ſubjects. It is certain that, if every article in a journal of ſcience were to be profeſſedly original *, it would be a work of comparatively much leſs value to Philoſophers and the Public. Such a plan would in a great meaſure defeat the attempt to convey the beſt diſcoveries of our cotemporaries in the moſt authentic manner, namely, in their own words. And when we reflect

* Lewis's Philoſophical Commerce of Arts, and various other publications of inferior note, have failed of public ſupport, chiefly from this circumſtance in their plan.

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on the very limited circulation of academical Tranfactions, from their price, their number, their extent, diftance of publication, difference of language, labour of perufal, and the efforts of mental abridgment, it is alfo certain that, from one or other of thefe caufes, even the beft memoirs they contain muft continue unknown to a very large clafs of men of fcience. Under the impreffion of thefe truths, while no exertions will be fpared to obtain immediate original information, concerning any object prefented to the world in this collection, the aim at originality muft neverthelefs be fubordinate to the lefs eafy but more effential requifites of public utility and interefting refearch. Whenever, in the progrefs of inveftigation, difcoveries thus buried from the knowledge of the world, fhall prefent themfelves, the rational plan of a public journal will require them to be brought forward, though years may have elapfed fince their firft publication. It would be eafy to exhibit a numerous catalogue of errors retained in the works of authors of the firft eminence, from the want of fuch general communication.

After this fhort account of the materials, it might be expected that fomething fhould be faid of the manner in which this Journal is to be conducted. On this occafion it would not be difficult to point out imperfections in the works of others, and promife to avoid them; or to enumerate the various requifites which ought to characterife the journalift of ability and integrity, and promife to exert them. Such fpecific engagements may have their value, and are probably entered into with great fincerity. But it appears more natural and eafy to leave every individual of principle and underftanding to imagine what ought to be done. As the events prefent themfelves, the proper mode of conduct will itfelf ftand forward and leave no caufe for hefitation.

Yet, while the Author himfelf avows the decided purpofe of exhibiting his fources of communication in the moft unreferved manner, it is no lefs proper that he fhould pay every attention to the rights of others. He will never take upon him to decide for another, how far he fhall or fhall not come forward as the author of any communications he may receive, and ftill lefs will he dare to infringe that firft and moft facred property which men hold in the products of their own underftanding. It is not, in his opinion, for any man or fet of men to decide what are the cafes of moral obligation in an inventor to communicate his difcoveries, or his private arrangements in bufinefs. Philofophers, manufacturers, and others, may therefore reft affured, that he will publifh no communications he may acquire either in converfation or otherwife, nor mention the name of any individual in his writings; unlefs the object be already before the Public, or unlefs he fhall receive permiffion directly, or moft clearly implied, to that effect. But in every cafe of anonymous communication he will be careful, according to the nature of the object, to mention the degree of credit he himfelf is difpofed to attach to the feveral facts.

Such papers as have no name or fignature are written by himfelf. This will in general appear alfo from the manner, chiefly with refpect to the ufe of the firft perfon fingular. The firft perfon plural will probably be ufed in fuch occafional fentences as refer to the Author and the Reader in the joint confideration of any fubject; but never in the manner adopted by anonymous writers, to denote the concealed individual.

A JOUR-

ADVERTISEMENT.

London, March 1, 1798.

A YEAR has nearly elapfed fince this Journal firft appeared. A complete Volume is now before the Public; on which occafion it feems neceffary to mention the nature of its contents. Enough has already been faid of the utility of fuch a Work and the duties of the Editor; among which fidelity and accuracy are undoubtedly the chief. Whatever defects of ability may appear in the fhare I have had in this collection, I can with confidence affert my claim to thofe moral requifites. I have defcended to none of the arts of Book-makers. No commendatory letters have flowed from my pen: no imaginary congratulations are echoed: no pretended fuccefs forms the fubject of my acknowledgements. I have confided in the fincere performance of my engagement with the Public; and have folicited the approbation of good men by fuch means only as my heart could thoroughly approve. I truft it will give pleafure to many of my Readers to hear that I have not been difappointed. The friendfhip and correfpondence of men whofe talents and virtues I revere, men whofe approval conftitutes the only eftimable part of fame, have amply overpaid my exertions: and in a commercial view, though I have found the fale of my book unequal to what might have been expected in times of lefs general diftrefs, yet it has been progreffively increafing, and fufficient to encourage my perfeverance.

The copious Table of Contents will render it unneceffary to recapitulate any of the excellent works I have received or collected. I have adhered to the principles of felection which are expreffed in the Preface publifhed laft year, and have been more folicitous to offer productions of real merit and utility than fuch as were chiefly remarkable for their novelty. It will be feen, however, that nearly half the papers in this volume are original and interefting; that above a third confifts of new and important works which have never yet appeared in our language; and that the remaining part confifts either of digefted reports and abridgments of excellent, but voluminous papers difperfed in academical collections, or fuch as from other circumftances deferved to be copied intire.

Men of information will hence perceive that this work is not an indifcriminate compilation of things neareft at hand, nor a loofe temporary record of tranfactions which a few years muft render worthy of the oblivion they will experience. It is reafonable to hope that it will every year become of more value, as the Repofitory of Difcoveries in Science and the Arts; and that it will tend to accelerate the progrefs of both.

ERRATA.

Page 13. l. 4. r. *other* engagements.
15. l. 27. r. as in the figure; these lines were parallel to the threads of the cloth.
17. l. 26. r. Instrument. And in the plate certain letters are omitted, which however may be easily supplied from the text.
64. l. 15. r. 0.0166.
66. l. 39. r. one-third *more*.
68. l. 10. r. Figure 11.
73. l. 15. r. Market.
87. l. 18. r. $\overline{31}|^{2}.7854 - \overline{31-18}|^{2}.7854 = 522$.
107. l. 8. of the note, r. which *are* of the same nature.
124. l. 2. r. *be* divided the whole.
139. —— throughout the mathematical part, for ÷ r. *
144. l. 24. r. *Analyses*.
174. bottom line, r. *eighteenth* Volume.

Page 186. l. 5. of the Mathematical question, r. Then *D*'s chance.
207. l. 31. r. *meteorological* apparatus.
266. l. 41. dele *and repelled*.
304. l. 10. r. *on* the water.
313. l. 24. for *communication* r. *paper*.
381. to the note add 1, page 252.
382. l. 37. r. *Surfaces*.
399. l. 5. r. *as* it would have otherwise.
418. l. 22. r. *place* of percussion.
422. l. 36. read five *feet* wide.
432. l. 24. for sine H × sine *b*; or putting radius = unity, Cos. *a* = Cos. *a*, &c. read sine H × sine *b* × radius; or putting radius = unity, Cos. A = cos. *a*, &c.
—— l. 26. read Mr. William Castieau.

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A JOUR-

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THE ARTS.

APRIL 1797.

ARTICLE I.

The Principles and Application of a New Method of conſtructing Achromatic Teleſcopes. By ROBERT BLAIR *, *M. D.*

AFTER a ſhort hiſtorical retroſpect to the diſcoveries of the unequal refrangibility of light by Newton, and the ſubſequent correction of its effects in optical inſtruments by Dollond, together with the remedy which is afforded in compound lenſes for the aberration ariſing from ſphericity, our author remarks, that it was expected by men of ſcience that an increaſe of the aperture and power of the refracting teleſcope would be the neceſſary conſequence of ſuch important ſteps towards the perfection of its theory. Theſe expectations have not hitherto been fully anſwered: for it is certain, that no Achromatic Teleſcopes have yet been made of equal aperture with the ſingle object-glaſſes of Huyghens and others, nor in this reſpect comparable to reflectors, though the errors of workmanſhip are much more noxious in theſe laſt than in lenſes.

The general anſwer made by artiſts to enquiries of this nature is, that the fault lies in the imperfection of glaſs, and particularly in the denſe glaſs known by the name of flint-glaſs.

The imperfection of glaſs, for optical purpoſes, ariſes partly from its opacity and colour, but chiefly from irregularities in the refractive denſities of its parts. The reſearches of chemiſts and manufacturers to remove this defect have been conſiderable, but hitherto without much ſucceſs.

From the conſideration that it is not impoſſible to introduce a fluid medium to ſupply the place of one of the lenſes in the compound object-glaſs, Dr. Blair was led to the experimental enquiry, whether nature afforded fluids poſſeſſed of the requiſite qualities.

* The original paper, from which I have made the above abridgement or ſelection of parts, is inſerted in the Tranſactions of the Royal Society of Edinburgh, vol. ii. It occupies 76 pages. N.

To afcertain the mean refractive and difperfive qualities of fluids, the Doctor made ufe of two kinds of apparatus. The firft, intended to afford a grofs knowledge of their properties, confifted of an apparatus of prifms. In the fecond method, where the fluids promifed to be of practical ufe in Optics, they were more critically examined by means of lenfes, in which the effect, from being magnified, is rendered more confpicuous.

The prifmatic apparatus confifts of a fmall, equi-angular, three-fided prifm, of brafs. Through this prifm, and parallel to one of its fides, are bored two holes, at a fmall diftance from each other, equal in fize to the pupil of the eye. The fides of the prifm are ground flat, and there are two pieces of glafs with parallel fides, of the fame dimenfions as the fides of the prifm. There are alfo prifms of the fame fize, and with the fame angles, of different kinds of glafs; and fome crown-glafs prifms with fmaller angles, which, by being applied to the large prifm, or to each other, vary the refracting angle at pleafure.

When it is propofed to try the properties of any fluid, one of the fmall plates of glafs is applied over the holes on the fide of the brafs prifm. A few drops of the fluid are then dropped into the hole, and, when it is full, the other plate is laid over the holes upon the oppofite fide, and the whole is fecured by tying a piece of packthread round the ends. One of the glafs prifms is now to be applied to the brafs prifm contiguous with one of the parallel plates, the refracting angles of the two prifms being placed in oppofite directions fo as to form a fmall parallelopiped.

Nothing farther is neceffary than to apply the eye to the hole which contains the fluid, in fuch a way as to obferve through it any bright well-defined object. The bars of the window anfwer the purpofe very well in the day-time, and the moon or a candle in the night. The intention of the two holes is for the fake of greater expedition. The properties of two fluids may thus be examined and compared at the fame time. As the prifmatic portion of fluid and the glafs prifm have equal refracting angles, and refract in oppofition to each other, it will eafily be underftood, that, if the object feen through the two prifms coincides with the fame object feen directly, the mean refractive denfity of both mediums will be the fame. When this is the cafe, if the object feen through thefe prifms appears free from prifmatic colour, the difperfive power of the fluid medium is alfo the fame with the difperfive power of the glafs prifm. But otherwife they will be different.

Thofe mediums, it is to be obferved, are faid to have the fame mean refractive denfity, which, under equal obliquities of incidence, equally refract the mean refrangible rays; and two mediums are faid to have the fame difperfive power, which produce an equal inclination of rays of the fame colour to the mean refrangible ray, when the whole refraction of the mean refrangible ray is equal in both.

When an object feen through the equal wedges of glafs and fluid appears coloured, one of the fmaller glafs wedges is to be applied, and fhifted, till the object appears colourlefs. It is eafy to diftinguifh, by the order in which the prifmatic colour lies, whether the fmall prifm is to be applied in fuch a way as to increafe the difperfion of the rays occafioned by the fluid fo as to enable it to counterbalance that of the glafs, or whether the refracting angle of the glafs prifm requires to be enlarged, to enable it to counteract the difperfion occafioned by the fluid.

By proceeding in this way to fhift the angles of the prifms till, firft, the direct and refracted images of an object coincide, without regarding the colour, and, next, till the refracted

fracted image appears colourlefs, without regarding the coincidence, the ratio of the mean refractive and difperfive powers of that kind of fluid, and that kind of glafs, with which the experiments are made, will be obtained from the angles of the prifms being given in both cafes.

In order to afcertain the abfolute refractive denfity of glafs, or any other medium, that is to fay, the general ratio of the fines of the angles of incidence to the fines of the angles of refraction of the mean refrangible ray which obtains in that medium, the Doctor took a direct method, fimilar in principle to that employed by Sir Ifaac Newton, and defcribed by him in the feventh propofition of the firft book of his Optics, and likewife in his Optical Lectures, p. 54; but which, as he juftly remarks, will be found much eafier, and perfectly accurate.

Inftead of caufing the rays to pafs through the fights of a large and accurate quadrant at the diftance of ten or twelve feet, as directed by Sir Ifaac Newton, the Doctor employed a Hadley's quadrant, in the following manner:

Plate I. fig. 1—I reprefents the index-glafs, and H the horizon-glafs of a Hadley's quadrant. SI reprefents a folar ray incident on the index-glafs, thence reflected to the horizon-glafs H, and from it to the eye at E. The line *fg* reprefents another folar ray, incident on the prifm P, and through it refracted to the eye at E. When the prifm is turned flowly round its axis till the fpectrum G appears at its greateft height, this is its proper pofition. The angle formed by the direct and refracted ray is then the leaft poffible, and the angles of incidence and emergence are equal. Let the prifm be fecured in this pofition. A flight infpection of the figure will fhew, that when the reflected and refracted images of the fun are made to coincide, the angle marked by the index of the quadrant is the fame which the incident ray *fg* forms with the refracted ray PE produced. For SZH is the angular diftance of the fun, and his doubly reflected image marked by the index; and the angle *fg* G, which the ray incident on the prifm forms with the refracted ray produced, is equal to it; *fg* and SI being parallel, and PZ and HZ being coincident.

The manner in which the ratio of the fines of the angles of incidence and refraction may be computed, from the above angle and the refracting angle of the prifm being given, is fully explained in the celebrated works which have juft been quoted.

The Doctor here remarks, that as it is the ratio of refraction of the mean refrangible ray which is wanted, the centre of the reflected image of the fun ought to be made to coincide with the centre of the coloured fpectrum, as reprefented in the figure; and if, inftead of this, the coincidence be formed with the moft or leaft refrangible ray, or any of the intermediate rays, it will be the ratio of refraction of thefe rays, and not of the mean refrangible ray, which will be found from the obfervation. Hence this method might be practifed for determining the difperfive power, as well as the mean refractive denfity of any tranfparent fubftance, whether folid or fluid; but the Doctor has preferred a combination of prifms or lenfes, becaufe it is the relative ratios, more than the abfolute ratios, which are moft immediately wanted.

By this prifmatic apparatus, the optical properties of a great variety of fluids were examined. The folutions of metals and femi-metals proved in all cafes more difperfive than crown-glafs. Some of the falts, as, for example, fal-ammoniac, greatly increafed the difperfive power of water. The marine acid difperfes very confiderably, and this quality increafes with its ftrength. The moft difperfive fluids were accordingly found to be thofe in which this acid and the metals were combined. The chemical preparation called *cauflicum* *antimonial*, or *butyrum antimonii*, in its moſt concentrated ſtate, when it has juſt attracted ſufficient humidity to render it fluid, poſſeſſes the quality of diſperſing the rays in ſuch an aſtoniſhing degree, that three wedges of crown-glaſs are neceſſary to remove the colour produced by one wedge of this ſubſtance, of an equal refracting angle oppoſed to them. The great quantity of the ſemi-metal retained in ſolution, and the highly concentrated ſtate of the marine acid, are conſidered by the author as the cauſe of this ſcarcely credible effect.

Corroſive ſublimate mercury, added to a ſolution of *ſal ammoniacum* in water, poſſeſſes the next place to the butter of antimony among the diſperſive fluids which he examined. It may be made of ſuch a degree of ſtrength as to require a wedge of crown-glaſs of double the refracting angle, to remove the colour which a priſm of it produces. The mercury and marine acid contained in this ſolution are manifeſtly the cauſe of its diſperſive power: for neither the water, nor the volatile alkali, which are its other component parts, will be found capable, if tried ſeparately, of contributing towards this effect.

The eſſential oils were found to hold the next rank to metallic ſolutions, among fluids which poſſeſs the diſperſive quality. The moſt diſperſive were found to be thoſe obtained from bituminous minerals, ſuch as the native petrolea, pitcoal, and amber. When the refraction is without colour, the proportion of the refracting angle of a priſm of theſe, to the refracting angle of a priſm of crown-glaſs acting in oppoſition, is about two to three. The diſperſive power of the eſſential oil of ſaſſafras, is not much inferior to theſe. The eſſential oil of lemons, when genuine, requires the refracting angles of the priſms neceſſary to produce a colourleſs refraction, to be as three to four. In oil of turpentine, this proportion is as ſeven to ſix; and the eſſential oil of roſemary is ſtill leſs diſperſive.

Some expreſſed oils, which were examined, were found not to differ ſenſibly in diſperſive power from crown-glaſs; which was alſo the caſe with rectified ſpirits, and with nitrous and vitriolic ether.

Having been thus ſucceſsful beyond his hopes in diſcovering fluids capable of removing the great imperfection of teleſcopes ariſing from the different refrangibility of lights, the next object of the author was to ſelect from this variety thoſe that ſeemed beſt adapted to optical purpoſes.

There was no doubt that thoſe mediums which moſt diſperſe the rays were *cæteris paribus* to be preferred. It will alſo be found, when the method of correcting thoſe errors which ariſe from the ſpherical figures of lenſes comes to be conſidered, that there is apparently an advantage in uſing a diſperſive medium, whoſe mean refractive denſity exceeds the mean refractive denſity of crown-glaſs.

As the antimonial cauſtic poſſeſſes both theſe advantages in a degree far beyond what was to be expected in any fluid, ſome of it was included between two double convex lenſes of crown-glaſs, whoſe radii of convexity were as two to one. The leaſt convex ſides of theſe were turned toward each other, and they were kept at a proper diſtance by means of a glaſs ring. The cavity was then filled with the ſtrongeſt butter of antimony. Here it is evident, that there is a concave lens of the diſperſive fluid acting in oppoſition to the two convex lenſes of crown-glaſs, and that the proportion of the radii of theſe is the ſame which was found by the priſms to correct the colour, namely, three wedges of crown-glaſs to one of the butter of antimony.

This compound object-glaſs being put into a tube, an eye-glaſs was applied, and, accord-
ing

ing to expectation, the colour was found to be removed. But the Doctor was furprifed to find, on directing the inftrument to a planet, and ufing a deep eye-glafs, that this fluid, in its highly concentrated ftate, was fubject, like flint-glafs, to great irregularities in its denfity, difcoverable by ftreams of light, like comets' tails, iffuing in different directions from the difk of Venus, which was the planet obferved. By fhaking the object-glafs, thefe might be in a great meafure removed, but foon returned; and, after ftanding all night, broad veins, in different parts of the included fluid, were perceptible to the naked eye.

It was neceffary, on this account, to reject very denfe fluids. The antimonial preparation was found to be reducible to a fufficient degree of fluidity, by mixing it with fpirit of wine or vitriolic ether, into which a fmall quantity of the marine acid had been previoufly dropped. This prevents any precipitation of the femi-metal in the form of a calx. In this diluted form, either this preparation, or the folution of corrofive fublimate mercury alone in fpirit of wine or in water, with the addition of crude *fal ammoniacum*, may be employed for producing refraction without colour, and without being fubject to that irregularity of denfity to which flint-glafs and very denfe difperfive fluids are fubject.

But as folutions of faline fubftances in this diluted ftate do not differ materially in difperfive power from the effential oils, thefe two kinds of fluids may be ufed indifferently.

In one cafe, however, our author remarks, that water, or vitriolic ether, impregnated with antimony or mercury, will have the advantage from being lefs denfe than effential oils, and that is, where it is required to produce a fingle refraction, in which there fhall be no difference of refrangibility of heterogeneal light. As this expreffion might found ftrange in the ears of Opticians, the Doctor has employed a confiderable part of his paper to explain what is meant by it. He fhews, that there are cafes of fingle refraction in which *the violet rays are the leaft refrangible*; or in which *all the rays are equally refrangible*; or in which the *red rays are refracted from the perpendicular, and the violet rays towards the perpendicular, while the mean refrangible rays fuffer no refraction.*

Thefe pofitions muft, no doubt, at firft confideration, appear paradoxical; but their fingularity will vanifh on attending to two circumftances: 1ft, That thefe refractions, though fingle, are not effected by the fimple agency of one medium on the confine of a vacuum, but by the difference of the contrary actions of two mediums at their common furface; and, 2dly, That Opticians, from the habit of contemplating the crown and flint glaffes, have affociated the notion of a greater difperfive power with that of a greater power of mean refraction.—But our author, having afcertained various facts in which the greater power of difperfion accompanies the lefs mean refractive power, has in confequence fhewn that an achromatic lens may be conftructed, in which all the refractions are made in the fame direction *. The cafes of fingle refraction here mentioned are very perfpicuoufly and at fome length fhewn, with fuitable diagrams, as well from the general facts before enumerated, as from affumed powers of attraction of the mediums on the rays of light, after the fuppofition of Sir Ifaac Newton. For the fake of brevity, however, I fhall give the fubftance of his explanation, without particularly attending to the order of arrangement.

* In an object-glafs formed of oil of turpentine included between two double convex lenfes, the radii of whofe convexities are as fix to one, and the deep fides inwards, there are four refractions, all towards the axis; and the aberration, from difference of refrangibility, is removed. This compound lens has twenty inches focal length, and one inch and half aperture. Its performance as a telefcope is not contemptible. B.

If a pencil of compounded light be imagined to pafs obliquely out of the plane furface of any denfe medium into a vacuum, it is well known that the refraction will be made from the perpendicular, the violet rays being moft refracted and the red leaft, fo that thefe rays will be inclined to each other. But fuppofe, again, that inftead of the rays being fuffered to pafs into the vacuum, another medium were applied, having a plane furface in contact with that of the former, and that its power of difperfion were fuch as precifely to counteract the difperfive power exerted on the rays by the former medium: in this cafe it is evident that the pencil of light would pafs forward colourlefs. Neverthelefs, as the difperfive power may be equal in two mediums which differ in mean refractive power, it is no lefs clear, that refraction will take place wherever thefe two powers are not precifely equal; and that either towards the perpendicular, if the power, or, as it is called, refractive denfity of the fecond medium, be greater than that of the firft, or from it if lefs.

In the next place, fuppofe the two mediums not to agree in difperfive power, but that the fecond medium difperfes more than the other. The violet ray will not only be prevented from feparating on the one fide of the red, but, by the excefs of difperfive power, it will be drawn fo far as to make an angle with that ray on the other fide, and the difperfion will be made the contrary way. If the mean refraction of the fecond medium exceed that of the firft, the whole pencil will be bended towards the perpendicular, and towards the fame region as the difperfion carried the violet ray; fo that the effect will be fimilar (though lefs in power) to that of a common refraction and difperfion by a fingle denfe medium in vacuo. This happens in the tranfition from crown-glafs into flint-glafs. But if the mean refractive power of the fecond medium be leaft, the pencil will be bended from the perpendicular, though the violet ray will notwithftanding continue neareft the fame perpendicular. The red ray will therefore be the moft deflected, and the violet leaft. This effect takes place when light is refracted in the confine of crown-glafs and oil of turpentine, and alfo of many other fluids. If the refractive powers of the two mediums upon the mean ray be equal, the direction of this ray will continue unaltered at its tranfition from the one to the other. But if the difperfive power differs, the extreme rays will form an angle, of which the red rays will be either at the one or the other limit, according to circumftances. For example, if the difperfive power of the firft medium be greateft, the angle will merely be diminifhed by the action of the fecond medium; fo that the violet ray will be the moft, and the red the leaft, refracted from the perpendicular; as would have happened (though more ftrongly) in the tranfition into a vacuum. But if, on the contrary, the difperfive power of the fecond medium were greateft, the violet ray will be carried more towards the perpendicular than the red by the difference of the two powers. This cafe of refraction is found to take place in the confine of crown-glafs and butter of antimony, when the latter is fo far diluted as that both mediums equally refract the mean ray under equal angles of incidence.*

In the cafe of refraction without colour, as before explained, the power of difperfion in the fecond medium was affumed to be fuch as precifely to counteract the effect of the firft; that is to fay, not only with regard to the extreme red and violet rays, but alfo with regard to the mean and all the intermediate rays. But Doctor Blair's Experiments fhew, that, in mediums which difperfe the light but little, the green is the mean refrangible ray; that in by far the greateft number of more difperfive mediums, including flint-glafs, metallic folutions, and effential oils, the green light is not the mean refrangible order, but forms one of the lefs refrangible orders of light, being found in the prifmatic fpectrum nearer to the deep red than to the violet; and that in another clafs of difperfive mediums, which includes the muriatic and nitrous acids, this fame green light becomes one of the more refrangible orders, being now found nearer to the extreme violet than to the deep red. Whenever, therefore, the light paffes out of one of thefe three claffes of mediums into another of a different clafs, the difperfive powers will not accurately counteract each other, even though they may be adopted to caufe the extreme red and violet rays to become parallel; but the rays which would occupy the interior parts of the fpectrum will be difperfed, and that in a greater degree the more remote they are from the extremes.

Thefe feveral cafes of refraction were likewife tried with compound object-glaffes, which fhew the effect better than prifms. Thus, if a plano-convex lens have its plane fide turned toward a diftant object, the rays will enter it, as to fenfe, perpendicularly, and will therefore fuffer no refraction. If the convex furface of this lens be brought in contact with a fluid of lefs mean refractive denfity than the glafs, but exceeding it in difperfive power in that degree which occafions an equal refraction of all the rays, all thefe rays will then be converged to the fame point, which are incident at the fame diftance from the axis of the lens. The focal diftance of this compound lens will be greater or lefs, in proportion to its radius of convexity, and to the difference of refraction between it and the fluid made ufe of. While the fluid is confined on one fide by the plano-convex lens, let the lens which is brought in contact with it on the oppofite fide have one of its fides ground convex, and the other concave; the radii of their fphericities being equal to the focal diftance at which the rays are made to converge by the refraction which takes place when light paffes from the plano-convex lens into the fluid. It is manifeft that the light will now both enter into this compound lens, and emerge from it, perpendicularly, and will therefore fuffer no refraction, except in the confine of the convex fide of the plano-convex, and the difperfive fluid where all the rays are equally refrangible. A compound lens of this kind is reprefented in the fecond figure, Pl. I. which requires no farther explanation; excepting only, that, inftead of being fpherical, it is reprefented with that curvature which converges homogeneal rays incident at all diftances from the axis to the fame point. If the required curvature could be given to lenfes with fufficient accuracy, this figure feems to reprefent as perfect a conftruction of the object-glafs of a telefcope as can be defired. But there is reafon to think that a fpherical figure may be communicated, not only much eafier, but with greater accuracy than a fpheroidal or hyperboloidal, which would then be required; and even if this difficulty could be got over, there would ftill remain a fundamental fault in the theory. Before relating the obfervations by which this was detected, Dr. Blair explains the method of removing the fpherical aberration by a combination of convex and concave lenfes. For, next to the indiftinctnefs arifing from the unequal refrangibility of light, this aberration occafioned by the fpherical figures of lenfes is the great obftacle to the advancement of the powers of vifion. The aberration from the fpherical figure has been treated of, in all the variety of cafes which can occur in fingle glafs lenfes by the great Hugenius in his Dioptrics, a pofthumous work. He there demonftrates, that the quantity of this aberration is very different in different lenfes of the fame focal diftance, according to the convexities or concavities of their two fides, and the manner in which they are expofed to parallel rays.

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In convex lenfes, thofe rays which pafs at a diftance from the axis are converged to a point nearer to the lens than its geometrical focus. The diftance between the point at which the external ray of a pencil incident on a lens interfects its axis, and the geometrical focus, is called the linear aberration of that lens.

Hugenius demonftrates, that when a plano-convex lens is expofed to parallel rays, with its plane fide towards them, this aberration will amount to four times and a half the thicknefs of the glafs. By the thicknefs of a convex lens, is meant its greateft thicknefs in the middle, after fubtracting its thicknefs, if it has any, at the outer edge; and by the thicknefs of a concave lens, is meant its thicknefs at the external edge, after deducting its thicknefs in the middle.

On turning the convex fide of the lens towards the light, the linear aberration will only exceed the thicknefs of the lens by one fixth part.

When both fides of a lens are convex, and the proportion of their convexities is as one to fix; if the moft convex fide be expofed to parallel rays, the aberration will exceed the thicknefs of the lens one fourteenth, which is the fmalleft poffible aberration of any convex lens.

If it is required to increafe the aberration, this may be done by grinding one fide of the lens convex, and the other fide concave, to a longer radius. Such a lens, with its concave fide turned towards parallel rays, will have more aberration than any plano-convex or double convex lens of the fame focal diftance.

Hugenius proceeds to fhew, that the fame aberration is produced by concave lenfes as by fimilar convex ones. When a plano-convex lens is expofed to parallel rays with its plane face outward, the external ray of the pencil being produced backward, after refraction, will interfect the axis of the lens nearer to it than its focus by four times and a half the thicknefs of the lens. But if its concave fide be expofed to the parallel rays, the aberration will only exceed the thicknefs of the lens one fourteenth part. A double concave, whofe radii are as one to fix, with the moft concave fide turned outward, difperfes the rays with the leaft aberration; and a concave menifcus, with its convex fide outward, produces more aberration than any plano-concave or double concave lens of an equal focal diftance.

Thefe are fufficient data for correcting the aberration from the fpherical figure, in cafes where both a convex and concave lens are required in the conftruction of the compound object-glafs.

Fig. 3. Pl. I. Let AB reprefent a convex lens receiving a pencil of rays from the object S, and converging rays incident near the axis at ST to the point F, and external rays as SB to the point D, fo that DF reprefents the greateft linear aberration in this cafe.

Again, let GH (Fig. 4.) reprefent a concave lens receiving the parallel rays SH, RK, which it refracts in the lines HX and KV. This ray KV being produced backward, will interfect the axis of the lens nearly at the point N, which is called the virtual focus of the concave; and the external ray HX produced backward, will interfect the axis in fome point P nearer to the lens than its focus PN, being the linear aberration.

It may here be obferved, that the convex is in that pofition which produces the leaft aberration, and the concave in the pofition which produces moft aberration. Hence, to render the aberrations DF (Fig. 3.) and PN (Fig. 4.) equal, the focal diftance of the convex muft be much fhorter than that of the concave; and if the diftances of the points F

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and N, from the convex and concave lenfes, be required to be the fame as reprefented in the figures, then muft the object be placed much nearer to the convex. Hence the image of the near object S is reprefented at the fame diftance from the convex lens in Fig. 3, as the virtual focus of the concave in Fig. 4, where it is reprefented as receiving parallel rays which are fuppofed to come from an infinitely diftant object.

Now when the diftance between K and N, which is the point from which parallel rays are made to diverge by the concave lens, is equal to the diftance between T and F, which is the point to which rays iffuing from S are made to converge by the convex, and when the aberrations DF and PN are alfo equal; it will follow in this cafe, that if the two lenfes be placed contiguous, in the manner reprefented in the twelfth figure, parallel rays incident on thefe lenfes will be converged to the point S, without any aberration of the external ray.

For it is an axiom in optics, that if a ray of light after refraction be returned directly back to the point of incidence, it will be refracted in the line which was before defcribed by the incident ray.

If, therefore, we conceive the whole of the light emitted from the point S (Fig. 3.) and converged by the convex lens towards the points D and F, to be returned directly back from thefe points, it will be accurately converged to the point S, whence it iffued. Now the parallel rays SH, RK (Fig. 4.) after their emergence from the concave lens in the lines HX, KV, are precifely in the fame relative fituation as the rays fuppofed to be returned directly back from F and D are in at their incidence on the convex; and therefore, when thefe lenfes are placed contiguous in the manner reprefented in the twelfth figure, parallel rays incident on the concave lens, and immediately after their emergence from it, entering the convex lens, will be accurately converged to the point S without any aberration.

This, which is the moft fimple cafe, will fuffice to explain the nature of that aberration which arifes from the fpherical figures of lenfes, and a method of obviating it, by combining a convex and concave.

The demonftration is perfect as far as regards the external ray, which is here reprefented paffing from the external part of the concave into the external part of the convex, in immediate contact with it; and if the furfaces of the two lenfes, which refpect each other, were either in contact or parallel, it would be true with regard to all the rays. But as this is not the cafe, there arifes a fmall fecondary aberration, the effect of which only becomes fenfible in large apertures.

Hence may be underftood the reafon why the indiftinctnefs arifing from the fpherical figures of lenfes, may, in the common Achromatic Telefcope, be more nearly removed in thofe conftructions of object-glaffes in which three lenfes are employed, than in thofe compofed only of two; and alfo the advantages in this refpect which may be derived from introducing fluid mediums which differ from glafs in their mean refractive denfity, and in the quantity of aberration produced by their refractions. For it will be found upon computation, that when the fluid medium is rarer than glafs, the aberration from the fpherical figure is increafed, and becomes greater in proportion as its denfity diminifhes. Now, by making the denfity of the fluid medium approach nearer and nearer to the denfity of the glafs with which it is in contact, we may increafe the rarity of our refracting medium, or,

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which amounts precifely to the fame thing, diminifh the difference of denfity of the two mediums at pleafure.

It will appear from what has been explained, that the aberration from the figure cannot be corrected by interpofing a difperfive fluid between two convex lenfes of a greater refractive denfity than the interpofed fluid. For all the refractions, being made the fame way, tend to converge the external rays to points nearer the lens than its geometrical focus. Hence, when rare fluids are made ufe of to remove the aberration from the difference of refrangibility, fome farther contrivance becomes neceffary to correct the fpherical aberration.

The moft obvious way, and which on trial was found fuccefsful, is to include the rare difperfive fluid between two glaffes, ground concave on one fide and convex on the other, and thus form fuch a concave as fhall be required. An Achromatic Object-glafs may be formed by combining this with a convex. The objection to this conftruction is, that one of the advantages arifing from the ufe of fluids is given up, namely, the prevention of that lofs of light, by reflection, which is a confequence of the fluid being in immediate contact with the glafs, whereas in the prefent cafe the fpace between the convex and concave is occupied by air.

On this account, Dr. Blair attempted to introduce a third medium, by filling this vacancy with a fluid of the leaft difperfive kind, and of lefs mean refractive denfity than the difperfive fluid. For this purpofe he employed fometimes rectified fpirit of wine, and fometimes vitriolic ether; and by giving to the lenfes the proper degree of curvature, in which great variety may be introduced, he fucceeded in forming object-glaffes in which both aberrations are removed, and hardly any more light loft than in a fimple object-glafs.

Having gained this point, he now determined to try how far the aperture of the object-glafs might be increafed, without increafing its focal length, expecting, at leaft, to equal reflectors in this refpect. But the firft trials to execute object-glaffes on this principle, though they left no reafon to complain of want of fuccefs when compared with fuch inftruments as are now in ufe, exhibited new phenomena and new obftacles to the perfection of the theory of Telefcopes more unaccountable and perplexing than any he had before encountered. The hiftory of thefe interefting facts, and the regular progrefs of difcovery by which they were remedied, conftitute a large part of the paper, which well deferves to be confulted by thofe who wifh effectually to profecute this fubject. Brevity, however, demands a lefs hiftorical narration in this place.

Thefe new difficulties arofe from the effects of contrary powers of difperfion, which though equal upon the extreme rays, were not found to be the fame upon the rays that occupy intermediate fpaces in the coloured fpectrum. From what has been already ftated on this fubject, the attentive reader may underftand, that fuch intermediate rays from a compound lens of the kind here defcribed, would not be affembled at the common focus. The particular nature of this difperfion is much more accurately feen by applying a deep eye-glafs to the focal image, than by experiments with prifms. When the image of a lucid point is formed in the focus of a fimple lens, the violet or moft refrangible rays are converged to a focus neareft to the lens, and the deep red rays are converged to a focus at the greateft diftance from it. The confequence of this is, that if the image be examined by an eye glafs nearer to the lens than is required for diftinct vifion, it will be furrounded with a

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red fringe, which is the prevailing colour of the leaft refrangible rays; and if the eye-glafs be placed at a diftance beyond that which is required for diftinct vifion, it will be furrounded with a blue fringe, which is the prevailing colour of the moft refrangible rays.

The reafon of this will appear more clearly from infpecting the fixth figure, where the red rays appear outermoft within the focus at A, and the violet rays appear outermoft beyond the focus at B. Thefe colours are alfo vifible when an image of any luminous object, as the fun, is formed by a lens on a white ground; and they will be fo much the more confpicuous, the greater the diameter of the lens in proportion to its focal diftance. The reverfe of this happens in compound lenfes, when the medium employed to correct the colour difperfes more than it ought to do.

In this way the correction of the colour may be examined, and the qualities of refracting mediums inveftigated, to an extreme degree of accuracy. Yet the effect will be rendered ftill more fenfible by covering half the object-glafs. For, when this is done, the colour produced by the uncovered half of the object-glafs appears, without being mixed with that of the oppofite fide, even when the eye-glafs is adjufted to diftinct vifion. Thus, in Fig. 6, the colours produced by both fides of the lens are mixed at the general focus F. But if the rays coming from one fide be intercepted, thofe which are refracted by the other fide will appear in their proper colours. By thefe means, and by employing a very luminous object furrounded by a dark ground, and a high magnifying power, the leaft uncorrected colour may be rendered fenfible.

The firft conjecture which prefented itfelf on the obfervation of irregularities of this kind, in compound lenfes adjufted fo as to correct the aberration of the extreme rays, was, that the colour might fomehow proceed from the compenfation at different diftances from the axis of lenfes not correfponding as the plane furfaces of prifms everywhere do. Trials on the images formed by the central parts, and afterwards by the parts near the circumference, proved, however, that this conjecture did not reach the caufe. Subfequent experiments and examinations of the coloured fringes led the author to the true caufe, that is to fay, the want of proportionality in the difperfive powers of the different mediums, as has been already ftated. It remained, therefore, to difcover, by a new procefs of experimental enquiry, the adequate remedy for fuch an extreme fource of imperfection. This led to the valuable doctrine, that one clafs of difperfive mediums throw the green nearer to the violet, while another clafs throw the fame colour nearer to the red, than is feen in the fpectrum formed by crown-glafs and other mediums, which difperfe but in a fmall degree. Thefe mediums were therefore applied in oppofition to each other, to correct this fecondary aberration in lenfes more compounded than might have been required if fuch a difficulty had not prefented itfelf. And laftly, the greater degree of compofition in thefe perfect lenfes was got rid of by the happy expedient of mixing the two different kinds of difperfive mediums, which, as it fortunately turned out, did produce a compofition of an intermediate nature, with regard to this proportionality in the arrangement of the prifmatic colours.

It has already been remarked, in the order of this abridgement, that, in metallic folutions and effential oils, the green light is among the lefs refrangible rays; but that, in the marine and nitrous acids, the fame green light becomes one of the more refrangible. It was not probable that the effential oils could be united with marine acid fo as to form a colourlefs

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fluid fit for optical uses; but nothing could be better adapted for this purpose than metallic solutions.

The first trial was made with butter of antimony. On increasing the proportion of marine acid, the fringes of green and purple, which, being the intermediate rays, were irregularly refracted by the metallic solution, grew narrower and narrower, till they entirely disappeared; and if more was then added, they re-appeared in an inverted order. The same thing was tried with a solution of crude sal ammoniac and corrosive sublimate. With a certain proportion of these two substances, the rays of all colours emerge from the compound object-glass equally refracted. If the proportion of the ammoniacal salt, and consequently of the marine acid it contains, be increased, the green rays which were the mean refrangible, in the dispersive fluid as well as in crown-glass, draw nearer to the violet, making a part of the more refrangible half of the spectrum, and consequently emerge less refracted than the united red and violet rays, and are converged to a focus at a greater distance from the object-glass; so that the green fringe now appears within the focus, and the purple fringe beyond it. But, on increasing the proportion of mercurial particles, these same green rays shift their situation to the less refrangible half of the spectrum, which appears from their now emerging most refracted, and being converged to a point nearer to the object-glass than the united red and violet, whose refrangibility does not appear to be affected by these admixtures which occasion such remarkable fluctuations in the refrangibility of the green rays and other intermediate orders. It may possibly seem strange at first view, that the green rays should emerge most refracted from the compound object-glass, when their refrangibility in the dispersive medium is diminished, and least refracted under the contrary circumstances. The cause of this is, that the principal refraction of the compound object-glass is performed by the indispersive convex lens, which is opposite to the refraction produced by the dispersive concave.

Fig. 7. represents an object-glass of this kind in Dr. Blair's possession, in which the metallic particles are so far diminished, and the particles of marine acid so far increased, as to render the refraction of the several orders of rays proportional in both mediums. There are two refractions in the confine of glass and the fluid, but not the least colour whatever. Hence it is inferred that, notwithstanding the considerable difference of density, and the refraction which, on account of the anterior surface being plain, and the posterior surface spherical, having the focus of parallel rays for its centre, is performed only at the two surfaces of the fluid, there is no unequal refrangibility of light. The rays of different colours are, as the Doctor observes, bent from their rectilineal course with the same equality and regularity as in refraction.

As custom has already appropriated the word Achromatic to that kind of refraction in which there is only a partial correction of colour, the author proposes to distinguish this entire removal of aberration by the term Aplanatic.

Thus far I have endeavoured, partly by abridgement, partly by extracts, and partly by taking greater liberties of narration and arrangement, to convey the substance of Dr. Blair's important discoveries to the public. It is now some years since the attention of the world was first directed to his improvements. I have therefore been induced to make enquiry, among the artists of this metropolis, into the state of the undertaking

undertaking confidered as a manufactory. Thefe enquiries led me to the Doctor himfelf; from whom I underftand, that all the practical difficulties are removed, and that the delay which has prevented the philofophical world from being yet fupplied with thefe inftruments has arifen merely from the interruption of engagements. And as I have reafon to expect fome communications from him on this fubject in future, it becomes unneceffary to enter into any of thofe remarks, in this place, which the immediate confideration of the fubject might fuggeft.

II.

A Remarkable Effect of the Inflection of Light paffing through Wire Cloth, not yet clearly explained.

IN the fecond volume of the Tranfactions of the American Philofophical Society, p. 201, an optical problem is propofed in a letter from Mr. Hopkinfon to Mr. David Rittenhoufe, who has given a folution in his anfwer. Mr. Hopkinfon, upon looking through the threads of a filk handkerchief, held clofe before his face, at a diftant lamp, was furprifed to obferve what he thought to be the threads magnified; and ftill more, that thefe fuppofed threads remained ftationary, notwithftanding any motion he gave the handkerchief to the right or left.

Mr. Rittenhoufe, in his remarks on this phenomenon, obferves very juftly, that the appearance, which is that of certain luminous points regularly arranged in right lines croffing each other, cannot confift of any image of the threads of the handkerchief, but muft be certain images of the lamp formed by the inflection of light paffing near the threads. He appears to have confidered the handkerchief as a very imperfect inftrument, and therefore too haftily threw it afide for an inftrument confifting of parallel hairs not croffed by others. With this he obferved that the line of light or portion of the fky feen through a window-fhutter very nearly clofed, appeared multiplied, fo as to confift of three parallel lines almoft equal in brightnefs, and on each fide four or five others, much fainter, and growing more faint, coloured and indiftinct the farther they were from the middle line. When the hairs were thicker, but at the fame diftance apart, or number in the inch, fo as to diminifh the opening between them, the middle lines were lefs bright, but the others ftronger and more diftinct; and he could count fix on each fide of the middle line feeming to be equally diftant from each other, eftimating the diftance from the centre of the one to the centre of the next. In this experiment, the hairs were the 190th of an inch in diameter, and were regulated by paffing them over the threads of a fcrew of 106 turns in the inch: fo that the opening or interval between hair and hair was 1/2[illegible]th of an inch. The middle line was well defined and colourlefs; the next two were likewife pretty well defined, but fomething broader, having their inner edges tinged with blue and their outer with red. The others were more indiftinct, and confifted each of the prifmatic colours in the fame order; which, by fpreading more and more, feemed to touch each other at the fifth or fixth line; but thofe neareft the middle were feparated from each other by very dark lines much broader than the bright lines.

From experiments made by applying this frame of parallel hairs before the object-glafs of a fmall telefcope furnifhed with a micrometer, he found that the pofition of the two neareft

neareft fide-lines indicated an angle of deflection equal to 7′ 45″; and, from eftimate founded on the appearance of the lines, he judged that the other ftreaks were deflected in angles of double and triple the fame original angle.

After this ingenious philofopher had proceeded fo far, and found a ready folution for the facts he had obferved, by referring them to the Newtonian doctrine of inflection, he went on, with more hafte than precifion, to account for the appearance exhibited by the handkerchief. He remarks, that, if the handkerchief be held before the eye, fo that its threads may be difpofed horizontally and perpendicularly, and a luminous fpot be viewed, the perpendicular threads will, by inflection, on the principle explained in his frame of hairs, caufe it to appear as a row or horizontal line of fpots, for example, five; and that the horizontal threads will, on the fame principle, convert every one of thefe into five perpendicular rows; by which means, a fquare figure, compofed of twenty-five luminous points, will be feen, of which he gives the figure.

On thefe laft inductions it may be fufficient to remark, that though his frame of parallel hairs might have converted a real row of luminous points into fuch a fquare, yet it is inconceivable how the fame effect could have followed, with regard to rays of light which muft have already paffed through the handkerchief in order to receive the firft modification. But as Mr. Rittenhoufe, from his figure of 25 fpots, which are in no cafe produced by the handkerchief, fhews that he did not ftrictly attend to the phenomena, but wrote in this inftance from recollection and reafoning, I fhall difmifs thefe particulars of the original obfervers, and mention the facts I have myfelf remarked.

When this problem was firft pointed out to me a few months ago by a very active promoter of the fciences, I took the earlieft opportunity of looking at a ftreet lamp through a piece of muflin containing one hundred threads in the inch. Inftead of one fpot of light, there appeared nine; four of which occupied the corners of a fquare, one the centre, and four others the middle points, in a right line between the corners. Upon examining them with an achromatic perfpective magnifying fourteen times, I faw that the fpots were true images of the flame of the lamp, the central one being perfect, all the others coloured, with the red outermoft, and the corner lights leaft luminous and diftinct. It made no difference in the appearance, whether the cloth was applied clofe to the object-glafs, or at any diftance to which the arm could reach before it; or moved fideways in its own plane; but the apparent dimenfions of the fquare were lefs when the cloth was applied between the object-lens and its focal image, in proportion (by eftimate) as the diftance of the cloth from the focus was lefs. When coarfer muflins were ufed, the fquare was fmaller according to the coarfenefs, but in what proportion was not examined, becaufe it was fuppofed to follow no very fimple ratio. No effect of this kind took place with the object-glafs of a microfcope.

From thefe facts I inferred as follows:—The middle flame is formed of all the pencils of light which pafs through the central parts of the holes in the cloth without any deflection. The four flames in the middle of the fides of the fquare, are formed by the pencils of light inflected towards the fhort threads bounding each fquare hole in the cloth, affifted by the deflective power of the oppofite threads refpectively. The corner flames are formed by the combined action of the two threads contiguous to the angles of every hole; the fum of which force, acting in the diagonal with a power varying from the diftance of the refpective

 parts

parts of the lines about the angle, cannot produce an image fo clear and definite as the fides. The images are farther afunder the finer the cloth, becaufe the nearnefs of the threads increafes the inflection, infomuch that when the interval is about $\frac{1}{405}$th of an inch there will be no central image*. As the images are produced by an equal number of claffes of pencils parallel to each other, any motion of the cloth either parallel or perpendicular to its own plane before the object glafs will not affect them, becaufe they come to the object-lens under the fame circumftances of parallelifm. But when the deflections were made by the cloth between the object-lens and its focus, the pofitions or diftance of the images were affected more or lefs upon the principle of the prifmatic micrometer. And, laftly, the microfcope does not fhew this effect, becaufe the rays from the object, not being parallel, are not uniformly deflected, and have no virtual foci.

I was well fatisfied at firft with this theory, and ftill think the greateft part will, upon further examination, prove to be well founded. But upon repeating the experiment with a piece of new cloth made of brafs wire about fifty-five threads in the inch, the wire itfelf being by eftimate about the $\frac{1}{162}$th of an inch thick, other lights became vifible, the caufe of which does not appear fo obvious. Befides the fquare, confifting of nine lights, there were others much fainter, more coloured, and elongated, as in Fig. 11. In order to difcern thefe to the greateft advantage, the fmall focal image of the fun was viewed in a concave metallic reflector of two inches focus, by means of the achromatic perfpective before mentioned, at the diftance of nine feet, with a power of only eight †. All the images, except that in the centre, were coloured fpectra, extremely brilliant and beautiful; the original nine, however, ftill retaining their pre-eminence. They were fainter and more elongated, the more remote from the centre to which they all pointed; that is to fay, an imaginary line drawn through the middle of all the colours of any one of the fpectra, would have interfected the central colourlefs fun. Neverthelefs their arrangement with regard to each other was not radial, but in lines forming prolongations of the fides of the middle fquare, as in the figure of the cloth: thefe lines were parallel to the threads. The interval between each range of images, meafured from the centre, was greater the farther off, but not regularly increafing; thofe near the middle fquare being almoft equal. Seven ranges were counted beyond the fquare on each fide, and a very long triple line of whitifh faint light extended ftill farther. There were alfo a great number of much fainter images irregularly fcattered in the angular fpaces between the crofs, all pointing to the centre, and produced, as I conjectured, by irregularities in the cloth.

As the diftance of the cloth before the object-glafs did not affect the pencils of light, it readily occurred, that by inclining its plane to the axis of vifion in fuch a manner as that one fet of the wires might ftill remain perpendicular to that axis, the other fet would apparently crowd together, and diminifh the openings through which the rays were to pafs. In this manner the cloth might be made, in effect, of any required finenefs. When the crofs wire or weft of the cloth was inclined to the axis, that part of the crofs in Fig. 11 which correfponded with the direction of the inclined wires, had the intervals of its fpectra enlarged, at the fame time that they became broader and more coloured, but fainter, and in particular the central colourlefs image nearly vanifhed. I have no doubt

* Newton's Optics, III. obf. 6.

† This was for the fake of light, as the chamber was not darkened. The object-glafs of 9 inches focus and 1 inch aperture bears a magnifying power of fifty, without producing any colour.

but

but it would have disappeared totally, if the want of perfect flatness in the cloth had not prevented its being inclined beyond a certain angle with regularity The branches of the cross at right angles to the inclined wires also nearly vanished.

But when the warp or longitudinal wires were inclined to the axis of vision, the images were altered in their relative brightness and position, in a manner which it would be too prolix to relate. These changes terminated in an hexagon with six radii, as in Fig. 12. when the inclination was about 45 degrees. In this, as in all the changes, the spectra were coloured with the red farthest from the centre, and they all pointed to the centre. This unexpected appearance led to an examination of the cloth by a simple magnifier in both positions of inclination, when it was found, that in the first described position the interstices of the wires appeared to be nearly long squares, or parallelograms; but in this last described position the threads of the warp being in reality bended by the operation of weaving over the weft, in angles of about 35° out of the plane, did apparently meet, so as to leave apertures of the form of equiangular triangles, and no doubt caused the hexagonal combination in Fig. 12.

After this imperfect description of a phenomenon of some curiosity, I must decline entering into any discussion of the consequences to which it seems to lead. Sir Isaac Newton's Experiments on Inflection, with the edges of two knives, are simple, and at first consideration do not appear difficult to be understood. Yet, simple as they are, there are certainly many variations yet to be made, and admeasurements to be taken, before the popular explanations of the mutual agency of bodies and light upon each other can be admitted without hesitation. This process of examination affords some peculiar facilities, on account of the great quantity of light which is similarly affected, and the possibility of applying a magnifying power and micrometer. Subsequent meditation may perhaps suggest the cause why the radii in these figures are triple, and parallel to each other, while all the spectra individually diverge from a centre: but I have rather chosen to refer them as they are to the examination of philosophers. And I am the more induced to hope that this examination will be made, when it is considered, that the appearance presents itself through any fine muslin or cambric to the naked eye, though still better with the opera-glass or perspectives, which are in the hands of every one, and that the wire-cloth of fine sieves or wine-strainers is a material easy to be procured.

III.

Description of an Instrument which renders the Electricity of the Atmosphere and other weak Charges very perceptible, without the Possibility of an equivocal Result.

IN the year 1787, when the electrical doubler engaged the attention of philosophers, for its astonishing power of magnifying the minutest quantity of simple electricity, and the subsequent discovery of its spontaneous electricity had greatly reduced its apparent utility, I had the pleasure of a conversation with the Reverend Abraham Bennet, of Wirksworth, the inventor, who shewed me his method of depriving this instrument of much of its adherent electricity, by working it for a time with all its parts in communication with the earth. But at the same time he remarked, that if he were to make an instrument in which this electricity should be totally removed, he would have recourse to a simple multiplier, and not a doubler. I did not apprehend the contrivance from this slight description, which at my request, however, he readily extended, and convinced me of the full value of his invention. I then mentioned my intention to construct an instrument on that principle, which soon afterwards I did. It was shewn to Sir Joseph Banks and his friends, at his house, nearly at the same time, and in the same year transferred to the celebrated Mr. van Marum of Harlem, who now possesses it. From various other avocations, I was prevented from causing any others to be made. It is not therefore wonderful that the same thought should since have occurred to so great a master of the subject as M. Cavallo, who in the third volume of his Electricity, published in 1795, gives a description and engravings of an instrument very different in form, but the same in principle. The form I have given it, which is all the share I had in the contrivance, is more calculated for a speedy repetition of the process than M. Cavallo's: but his instrument is much easier to be made by one who is not professionally a workman. Mr. Bennet's notion with regard to form was very different from either. Both instruments have the property, that a simple or small aggregate of electricity will not be multiplied unless its intensity exceed that of the adherent electricity of the plates, supposed to be contrary; and both are capable of destroying their own electricity, and exhibiting unequivocally what they receive by communication, however weak it may naturally be, provided the supply be kept up. They are both also limited as to the extent of their multiplication; that is to say, the effect will be denoted by a fixed number or multiplier, so long as the nearest distance of the plates continues unaltered. For example, if this number were 100, the instrument would shew no electricity which was less intense than the $\frac{1}{100}$th part of what the electrometer demanded to act upon it. This coefficient is nevertheless capable of being enlarged at pleasure, by adjusting the plates to a less distance asunder.

Fig. 8. represents a vertical section of the instruments. A is a metallic vase, having a long steel axis which passes through an hole in the stand H at K, and rests on its pointed end in an adjustable socket at C. The use of the vase is, by its weight, to preserve, for a considerable time, the motion of spinning which is given by the finger and thumb applied to the nob at the top of the instrument. The shaded parts D and E represent two circular plates of glass nearly $1\frac{1}{2}$ inch in diameter. The upper plate is fixed to the vase, and revolves with it; the lower is fixed to the stand. In the lower plate are inserted two metallic hooks, diametrically opposite each other, at F and G. They are cemented into holes drilled in the edge of the glass, which is near two-tenths of an inch thick. In the upper plate are inserted in the same manner two small tails of the fine flatted wire used in making silver lace. These tails are bended down so as to strike the hooks in the revolution, but in all other positions they remain freely in the air without touching any part of the apparatus. At C is a screw, which by raising or lowering the vase keeps the faces of the glass plates from each other at whatever distance may be required. The faces of the glass plates which are opposed to each other are coated with segments of tin foil, as represented Fig. 9 and 10, the latter of which represents the upper plate. Each of the tails communicates with the tin foil coating to which it is contiguous, as does also the hook F with that coating of the lower plate nearest to it. But the hook G is entirely insulated from the whole apparatus, and is intended to communicate only with the electrified body or atmospherical conductor L.

 The

The lower coating nearest to G is made to communicate permanently with the ſtand H, and conſequently with the earth.

In this ſituation, ſuppoſe the motion of ſpinning to be given to the apparatus, and the effects will be theſe :—One of the tails will ſtrike the hook G, by which means the upper coating annexed to that tail will aſſume the electric ſtate of L by communication. But this ſtate, on account of the proximity of the lower uninſulated plate to which it is, at that inſtant, directly oppoſed, will be as much ſtronger than that of L, as a charge exceeds ſimple electrization. The tail G with its plate or coating proceeds onward, and after half a revolution arrives at the ſituation to touch the hook F. The upper coating, the lower on the ſide of F, the hook F itſelf, and the tail V, muſt then conſtitute one jointly inſulated metallic maſs, in which no charge ſubſiſts, but which is ſimply electrified by the whole charge received at G. And of this maſs the ſurfaces of the plates themſelves, conſtituting the electric well of Franklin, will throw out all their electricity to the hook and tail. But the coating and its tail inſtantly paſs round, leaving F electrified, and proceed to bring another charge from G and depoſit it as before. The balls at F are therefore very ſpeedily made to diverge. It is ſcarcely neceſſary to remark, that the two upper coatings do nothing more than double the ſpeed of the operation ; one of the tails being employed in collecting, while the other is depoſiting : and that the gold leaf electrometer may be advantageouſly ſubſtituted for the cork-balls.

The inſtrument I cauſed to be made was five inches high. The receiving ſide G was connected with a coated jar of four ſquare feet coating, and the giving ſide F was connected with Bennet's gold leaf electrometer. The electrometer was rendered as ſtrongly poſitive as it was capable of being, and the jar was rendered negative, by giving it as much of that power as was produced by drawing a common ſtick of ſealing-wax once through the hand. In this ſtate the jar was incapable of attracting the fineſt thread. The vaſe was then made to ſpin ; and the effect was, that the leaves of the electrometer firſt gradually collapſed, and then in the ſame manner gradually opened, and ſtruck the ſides of the glaſs of the electrometer with negative electricity. The experiment was renewed and repeated with every requiſite variation.

IV.

Obſervations on the Art of Printing Books and Piece Goods by the Action of Cylinders.

—— Experto credite.

WE may conceive three ways of delineating figures, or writing. The firſt and moſt ancient conſiſts in making the traces ſucceſſively by a bruſh, a pen, or other inſtrument. This is deſign, painting, or writing. In the latter methods, either the whole or the greater part of the figures are made by the action or preſſure of an original pattern againſt the material intended to be written or painted upon. It is the art of printing. The colouring matter is either depoſited from the face of prominent parts of the original form, which is uſually called a block or type ; or elſe it is preſſed from cavities cut in the face of the original, which in this caſe is called an engraved plate. Moſt books are printed

printed from original patterns, in relief; and moft of the imitations of paintings are performed by means of engravings. Thefe arts are moft frequently diftinguifhed by the names of letter-prefs and copper-plate printing.

It can fcarcely be matter of new information to thofe who are but moderately acquainted with the ftate of the Arts, to be told that letter-prefs or book-printing is performed by an affemblage of fingle metallic letters, called types, made of lead hardened by an addition of antimony in the metallic ftate; that thefe letters are compofed in the form of book pages, and wedged together in iron frames called chafes; that the ink is a compofition of linfeed oil and lamp black, of fo fingular a nature, that it will adhere to a ball covered with a pelt or fheep's fkin foaked in water, and kneaded to extreme foftnefs under the feet, but quits this fkin with great readinefs to apply to the face of the letter when dabbed with the ball; and ftill more, that it almoft totally quits the letter to adhere to paper rendered femi-tranfparent by foaking in water; or laftly, that the paper is applied and preffed againft the form of compofed letter by means of a flat piece of wood urged downwards by a fcrew. Thefe and numerous early difcovered principles of this moft ufeful art are generally known, and require no more than mere recapitulation in this place.

The genius of the Chinefe language not permitting that people to analyfe its founds into an alphabet, as has been done by moft other nations, has induced them to retain thofe figns of things, and of their correfpondent words, which probably conftituted the firft picture or hieroglyphic writings of every rude fociety. Changed and complicated as thefe may have become by the rapidity of tranfcription, the corruption of ignorance, or whatever other caufes may have operated through a long fucceffion of ages, they ftill for the moft part ufe words that properly denote things, and not founds. Such words cannot, therefore, be fub-divided; and it has accordingly been found moft convenient, by thefe firft poffeffors of the art, to print from entire blocks, as was alfo done by the firft printers in Europe. But our artifts foon difcovered that a few of the fimpleft characters, namely, the letters of the alphabet, would be in many refpects more ufeful, as the elements for compofing blocks for printers, than a number of blocks originally cut for every page of every individual book.

Book-printing, therefore, though in fact of the fame nature as block-printing, has been carried into effect by very different machinery from that made ufe of in the arts which ftill retain the latter method. In book-printing, the heavy metallic form lies on a kind of table, and the colour and the paper are fucceffively applied to its face: but in block-printing, the block is carried and applied to the colour, and afterwards to the work intended to be printed. Thus, for example, in the printing of paper-hangings, the colour is fpread with a brufh upon a woollen cloth ftretched over a furface of parchment or fkin evenly fupported by a half fluid mafs of water and mafhed paper. To this the block is carefully applied by a flight perpendicular ftroke or two; after which it is applied to the dry paper on a table, and preffed againft it either by one or more blows with a mallet, or by the regular action of a lever. The mechanical part of callico-printing is effected nearly in the fame manner; but with fmaller blocks, becaufe of the greater difficulty of making the fucceffive fittings on fo flexible a material. And in both thefe arts, as well as in book-printing, in red and black, the variety of colours are produced by repeated applications of forms or blocks, of which the prominent parts are made to fit each other according to the nature of the defign.

In the art of printing from copper-plates, a colour somewhat more fluid than for book-printing is made use of. It is pressed into the cavities of the plate by smearing it over the surface; and by subsequent careful wiping the redundant colour is cleared away. In this state, if soaked paper, for which purpose the most spongy texture is the best, be strongly pressed against the plate, by passing both together between two cylinders of metal or hard wood, properly defended by woollen cloth, the greatest part of the colour adheres to the paper, and forms what is called a print.

In all these processes, it is easily seen, that in the successive applications of colour, the accurate filling of the form or original with the material intended to receive the impression, and in various other parts of the manipulation, there is much room for the display of skill, or for injury from the want of it. It may moreover be collected, that the motions attendant on the various steps of manufacture are in many instances difficult to be performed with rapidity and ease, until by long continued habit the workman himself is converted as it were into a machine. A very slight degree of attention to this subject must also shew that, if the originals were of a cylindric form, with a contrivance for regularly applying the colour and performing the subsequent operations, it would be easy to print books and piece-goods with a degree of rapidity and uniformity, of which the usual method of successive applications seems scarcely capable without uncommon care and skill. This obvious conclusion has no doubt led to numerous experiments; none of which, as far as I can gather, whatever may have been their particular utility, have given much promise to supersede the ordinary methods. But as the increased demand for the manufacture of printed goods has rendered such an improvement an interesting object to manufacturers, as well as to those indefatigable artists who have directed their efforts towards improvements; and as the latter generally take up a new object under a strong persuasion that it has not before been pursued by others, it will certainly be of advantage to these deserving classes of men, to relate a few of the difficulties of this new art.

The difficulties attendant on any improvement in the arts may be considered either as moral or physical. Under the moral, I would class every thing that relates to the prejudices of men in favour of the old methods, and their fears of risk, together with the œconomical and commercial inconveniencies attending the new processes. The physical difficulties are such as attend the actual performance of any project after the same has been carefully arranged in the mind of the inventor. It happens unfortunately here also that the inventor is seldom aware of the moral impediments, but almost always concludes, that if he can succeed in accomplishing the purpose he has in view, his cares and labour will then be at an end; and that the manufacturer, in particular, instead of pointing out new impediments discernible only from long continued experience, will more readily embrace and approve of the new processes, in consequence of his superior knowledge of their intrinsic value.

Every good invention appears simple in the prospect, but it scarcely ever happens in the execution that the most direct road is taken; and in every case there will infallibly be many things unknown or unforeseen, which practice only can point out as necessary to be done for the complete accomplishment of the object in view. Hence, and likewise because few men possessed of independent fortune are likely to engage or persevere in a labour of this kind, it almost invariably happens that the expences exceed the ability of the inventor himself. For these and other reasons, new undertakings are generally brought forward by the

inventor,

inventor, a man ftrongly prejudiced in favour of his leading purfuit, together with a moneyed friend, who hopes fpeedily to increafe his capital from the abilities of the other. It is not neceffary in this place to defcribe the ufual confequences of a partnerfhip, where the minds, the views, and the circumftances of both individuals are fo very different, and which may be modified ftill more effentially if either of the parties be deficient in the common principles required to bind men to each other. It is certainly of the higheft importance to both, that the circumftances of fuch connections fhould be very maturely weighed before they are entered into.

The commercial difficulties or facilities attending any invention are alfo of great confequence. Every inventor ought to enquire, not only what has been done before, but likewife into the prefent ftate of the manufacture he means to improve. In this way it is afcertained how fmall a part the mere prefs-work conftitutes in the price of a book. He will find that twelve yards of paper-hangings are printed for one penny, in a fingle colour, by hand, which afterwards by the accumulation of price, in paper, colour, duty, and ordinary profit, are fold for three fhillings; none of which the inventor can pretend to diminifh: and if he could annihilate the whole labour, his advantage would therefore be lefs than three per cent. without reckoning the coft and operation of his machinery. In the callico-printing, with a more expenfive material, dyeing and field proceffes, duty and profits of manufacturer and vender, the price of laying the block will turn out to be an object ftill lefs confiderable. Again: it will be feen that fmall flat blocks coft but little money, in comparifon with cylinders of fufficient diameter to retain their figure, and long enough to apply to the whole breadth of the cloth.

Under thefe and other fimilar points of view, the inventor, who may confider the fubject in a fuperficial manner, would be ready to abandon his undertaking. But this again ought not to be rafhly done. It is true, that where the great force of capital is employed on objects not comprehended within his project, the faving, however large in its abfolute amount, or defirable to a manufacturer, will fcarcely come within the reach of the inventor by any bargain he can make fhort of an actual partnerfhip. But it may be poffible to feparate the refpective departments of a manufactory. A fpinner is not neceffarily a weaver, nor a printer a linen-draper or a dealer in paper-hangings. The feveral departments of manufacture and commerce are, generally fpeaking, in the hands of acute men, who feldom reafon ill with regard to the advancement of their peculiar interefts; and thefe departments are continually fluctuating in their arrangement, as convenience, profit, or the accumulation of capital may lead. Experiments are for ever on foot, from day-work to piece-work, and from piece-work to the employ of mafter-workmen with others under them, all fupported by the capital of the large manufacturer, who himfelf in many inftances is the mere inftrument maintained by the advances or acceptances of the warehoufeman, the factor, or the merchant. An inventor who has not capital may feek for employ on the goods or the capital of others; and if he has fkill to maintain his ground againft the numerous enterprifes which the activity of oppofite interefts will raife againft him, he will find that the old order of things will readily alter, as foon as an evident intereft in favour of the new is fhewn by actual and continued proofs in the market.

Moft of the phyfical difficulties attendant on any new procefs are fuch as experience only can fhew. Thus, in the forging of iron by the preffure of rollers inftead of hammers,

mers, a fcheme upon which many thoufands of pounds have been expended in this country, it was apprehended that the more impure parts, which are alfo the moft fluid, might be preffed out by the action of cylinders, with equal or perhaps more advantage than by that of hammers; at the fame time that the determinate figure of bars of any required fize might be given without fkill in the operator. Experience neverthelefs has fhewn, that the more fluid part is driven out much more effectually by the fudden action of a blow, than by the flower compreffion of a cylinder, which allows time for much of the fluid matter to extend itfelf within the mafs. Various fimilar effects prefent themfelves when cylinders for printing are fubftituted inftead of planes. Inftead of the action of dabbing, the colour is ufually applied by fimple and gradual contact, to much lefs effect; and the impreffion, though not effentially different from that of the block, is performed by a gradual action, which affords time for the cloth or paper to fold itfelf in a minute degree into the cavities of the fculpture. Hence it is found that the length of paper or cloth printed from a cylinder by a definite number of revolutions, will be greater or lefs than another piece manufactured precifely in the fame way, but with a lefs or greater degree of preffure. In a block this defect is much lefs, not only from the confiderable hold it takes upon the furface of the material, but alfo becaufe the error is rectified at every fucceffive application. One of the chief difficulties of cylinder printing confifts, therefore, in the difficulty of laying one colour after another; and this would continue to be fo even if the materials were not fufceptible of change, the contrary to which is the fact. There are two projects for obviating this. The one confifts in confining the whole piece to a long table, or to the circumference of a large cylinder, and caufing the printing cylinder to move, not by the fucceffive appofition of its carved furface, but of a bearing face regulated by a toothed wheel. The other method confifts in the ufe of a frame to confine two or more cylinders, each provided with its own toothed wheel, and revolving againft a large clothed cylinder provided with a fuitable wheel to drive the others. The piece is caufed to pafs between the large cylinder and the others, in order to receive the impreffion With regard to the firft of thefe methods, it does not appear eafy to confine paper, and ftill lefs cloth, in fuch a manner that its parts may continue without fhift or wrinkle during the action of a cylinder, which not being allowed to roll without the check of a wheel, muft draw the furface either the one way or the other. The difficulty of confinement will be very much increafed, by the indifpenfable requifite that the paper fhould be afterwards hung up to dry, and the callico be carried to the dye-houfe and the bleach-field, between the fucceffive impreffions, by which means the dimenfions of both will be greatly altered. In the fecond method, it is obfervable that no colours can be printed but fuch as fall clear of each other. In this way, moreover, the gathering action of the cylinders may prove very mifchievous. For, if we fuppofe the paper or cloth to pafs between the great cylinder and the firft printing roller by an action of the latter which tends to make it flip forward on the face of the great cylinder, and that when it arrives at the fecond printing roller it there experiences an action of a contrary nature, the confequence will be, that the material will become flack between the two rollers, and the fittings will be falfe. Not to dwell on that experience which brings forward this obftacle among others, its great probability may be deduced from the allowable fuppofition that the circumference of the firft printing cylinder fhould be one thoufandth part of an inch too large, and that of the fecond the fame quantity too fmall. For in this cafe the material will be fhifted one-twentieth of an inch in fifty turns by the firft cylinder, and the fame quantity in the contrary direction by the fecond; a quantity upon the whole quite fufficient to deftroy the effect of the colours in the progrefs of one fingle piece. Such minute differences can hardly be avoided in the firft inftance; in addition to which we may place the varying dimenfions of the printing cylinder, if not made of metal; and of the great clothed cylinder, which in effect has a larger or fmaller diameter in proportion to the preffure which operates to render its elaftic covering either thicker or thinner. The only method of diminifhing thefe evils feems to be, that all the printing cylinders fhould by dimenfion or preffure, or both, be made to draw the fame way, the outer cylinder moft, and the others gradually lefs and lefs, fo that the material fhould have a tendency to apply itfelf more tightly during its paffage through the apparatus.

The application of the colour to the furface of a cylinder block is attended with fome difficulty. An ingenious mechanic may contrive various means to produce the action of dabbing, if required. When a ftuffed cylinder covered with cloth is made to revolve in the colour, and thence after paffing a fcraper to apply itfelf to the block cylinder, it is found to be no inconfiderable difficulty that its dimenfions change, and its covering becomes wrinkled by the action of the fcraper as well as that of the block. A better method, therefore, confifts in a revolving web of woollen cloth, like a jack-towel, ftretched over three horizontal cylinders parallel to each other, two of which fupport the elaftic furface of the web, which in its revolution accompanies the block cylinder, and the other ferves to guide the fame web to the colour, or a cylinder revolving in it. This method would be very eafy and pleafant in its operation, if it were not for a property common to all ftraps which revolve on the furface of two or more wheels. Thefe are obferved always to feek the higheft place; fo that if a cutler's wheel were made with a groove to carry a ftrap, inftead of a round edge, the ftrap would infallibly mount the ledge, inftead of remaining in the groove*. On this principle, the web would very fpeedily fhift itfelf to one end of the cylinders, if it were not confined fideways, or the lower roller were not made confiderably thickeft in the middle, and gradually tapering towards its extremities. This laft fimple expedient is not without its difficulties; but as I have not actually tried it, I fhall defer entering into any difcuffion on that head.

The running of the paper or piece-goods towards one end of the leading cylinder is alfo one of the greateft difficulties attending this method of printing. It is not perfectly removed by tapering the leading cylinders.

The nature of the trade of paper-ftaining in this country, which requires a large fum to be immediately vefted in the payment of the excife duty, and confequently prevents any confiderable ftock from being manufactured until orders are actually received, and the varying fafhions in printed callicoes, which render the expence of cutting the block by far the heavieft part of the difburfement for printing, are probably the chief reafons why manufacturers in this country have been lefs folicitous for the conftruction of machines calculated to afford profit only in the cafe of very numerous impreffions. The phyfical difficulties of this art have likewife confpired, in no fmall degree, to prevent its having been applied in the large way to any but a few fimple defigns of the fort called running patterns in one colour.

* This curious effect arifes from a flexure produced in the edge of the ftrap, by the elevated part of the wheel, which throws the advancing part of the ftrap more and more towards that elevation. It cannot be explained in a few words, but may eafily be feen by wrapping a ftraight flip of paper round an extinguifher or any other cone.

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V.

An Account of the Diamonds of Brazil. By M. D'ANDRADA.

AS the Society * is defirous of an account of the Diamonds of Brazil, I fhall endeavour to fatisfy them to the utmoft of my power; but previous to a defcription of their form, the place where they are found, and the manner of fearching for them, I think it will be ufeful to convey an idea of the country in which thefe diamonds are found.

The province of Brazil which produces diamonds is fituated inland between 22½ and 16 degrees of fouth latitude. Its circumference is near 670 leagues. On the eaft it is limited by the captainry or province of Rio Janeiro; on the fouth, by that of St. Paul; on the north, by the *Serteens*, or interior part of the maritime province of the bay of All Saints, and part of that of the mines of Goyarel; on the weft, laftly, by another part of the laft-mentioned province, and by thofe deferts and forefts which are inhabited by the favages, and extend to the frontiers of Paraguay. On the fide neareft St. Paul there are vaft uncultivated plains; the interior is divided by chains of mountains and hills, with fuperb valleys and luxuriant fertile plains. It abounds with wood, and is watered by a great number of rivers and brooks, that facilitate the working of the mines of gold, which is obtained by wafhing in fpangles from the river fands, or in veins open to the day. This province is divided into four *comarcas* or diftricts, which, reckoning from north to fouth, are *Santo Joao del Rei*, *Villa Rica*, *Sabara* and *Sero Dofrio*, or cold mountain, called in the language of the favages *Yritauray*. The diamonds are found in this laft diftrict. The whole province is very rich in the ores of iron, antimony, zinc, tin, filver and gold.

The Paulifts and inhabitants of the ancient captainry of St. Vincent were the firft who difcovered thefe mines, and peopled in great part the whole of this rich province, as well as thofe of *Mato Groffo*, *Cuiaba*, *Goyares*, and *Rio grande de San Pedro*. In a word, almoft the whole of the interior of Brazil, with its immenfe riches, would have been ftill unknown but for them. The metropolis at prefent enjoys the fruit of their eccentric activity and hazardous difcoveries. Conftantly with their arms in their hands to defend themfelves againft the favages, in the midft of impenetrable forefts and folitary waftes, expofed for twelve years to famine and the inclemency of the feafons, they overcame every obftacle: nothing could check their unconquerable fpirit. There is not a fingle mountain, brook, or mine, which has not been traverfed, difcovered, and vifited by them. Antonio Soary, a Paulift, who gave his name to one of thefe mountains, was the firft who difcovered and vifited the *Sero Dofrio*. Gold only was fought for, but at laft diamonds were difcovered in the *Rincho Fundo*, whence they were firft obtained, and afterwards in the *Rio de Peire*; a great number were likewife obtained from the *Giguitignogna*, a very rich ftream; and laftly, at the end of 1780 and beginning of 1781, a gang of near three thoufand interlopers, called *Grimpeires*, difcovered diamonds, and obtained an immenfe quantity from the *Terra de Santo Antonio*: but they were forced to abandon this fpot to the Royal Farm, who took poffeffion of it. Then it was that the fufpicion was confirmed, that the mountains are the true matrix of diamonds; but as the work in the beds of rivers and on their banks is lefs tedious, can be conducted on a larger fcale, and affords larger diamonds, the

* The Society of Natural Hiftory of Paris. This account is inferted in the Annales de Chimie, XV. 82, from which this tranflation is made.

Farm abandoned the mountains, and formed great eftablifhments in the river of *Toucanbirnen*, which flows through the valleys of this chain, and is near ninety leagues in length. It was found by examination and digging, that the whole furface of the ground, immediately beneath the vegetable ftratum, contained more or lefs of diamonds, diffeminated and attached to a matrix ferruginous and compact in various degrees, but never in veins, or in the divifions of geodes.

Attempts were made at firft to prohibit the working; but the activity of individuals who infringed the order of Government, and fent home diamonds by the fhipping from Brazil, under the denomination of oriental diamonds, induced Government to eftablifh a farm. The firft farmer was Rifberto Caldera, a Paulift, with the condition that no more than 600 negroes fhould be employed in this work. This condition has always been evaded; for the number of flaves employed are from fix to eight thoufand: and this number was fcarcely diminifhed when the Portuguefe Government, to put an end to this fraud and the depreciation in price of diamonds proportional to the quantity brought to market, caufed the undertaking to be carried on for its own account. But at prefent, from other confiderations, it is farmed again to individuals. Notwithftanding the great profits which enter the royal treafury, it is certain that the inhabitants of the province are greatly injured by it, becaufe the Diftrict of Diamonds being continually enlarged, has condemned to deftructive repofe immenfe tracts very rich in gold.

Let us now proceed to the diamonds.

The figure of the diamonds of Brazil varies. Some are octahedral, formed by the union of two tetrahedral pyramids. This is the *adamas octaëdrus turbinatus* of Wallerius, or the octahedral diamond of Romé de l'Ifle. Thefe are almoft always found in the cruft of the mountains; others are nearly round, whether by a peculiar cryftallization or by rolling. They refemble thofe oriental ftones which the Portuguefe and the natives of India call *reboludos*, which fignifies rolled. And laftly, others are oblong, and appear to me to be the *adamas hexaëdrus tabellatus* of Wallerius. The two laft are ufually found in the beds of rivers and broken places of their banks.

Diamonds are alfo found, as I have remarked, in the cruft or external covering of mountains. Thefe maffes are formed of a bed of ferruginous fand, with rolled flints, forming an ochreous pudding-ftone from the decompofition of emery and muddy iron-ore; it is called *cafcalho*, and the beds or ftrata *taboleiros*. Thefe *taboleiros* have different names, according to their fituation or their nature. When the ftratum is horizontal, and in the plane of the bed of the river, it is properly a *taboleiro;* but if it rifes in banks, it is called *gopiara;* laftly, if the pudding-ftone contains much emery, it is then denominated *tabanhua cauga* in Brazilian, that is to fay, black-ftone, or iron-ftone.

In fome places the *cafcalho* is uncovered; in others, it lies beneath a kind of vegetable muddy earth, *humus damafcena* Linn. or beneath a reddifh fat fand, which fometimes contains rounded flints. This happens in the returns of the mountains, or upon the banks of great torrents. This fand is called *pifarra*. The bank or ftratum beneath the *cafcalho* is either fhiftus rather fandy, or the folid bog-ore of iron. It is likewife in the *cafcalho* that gold in fpangles and in pyrites is found; the former of which is in my opinion afforded by the decompofition of the auriferous pyrites. For the gold in veins has another form,

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and its matrix is either fat quartz or fine-grained tender *cos*, micaceous gneis, or the quartzose ore of iron, *tophus ferreus* Linn.

The exploiting of diamonds is performed by changing the beds of streams, in order that the sand or gravel may be washed, and the diamonds selected; or by breaking the cascalho with large hammers, and afterwards washing it in troughs. This washing differs from that of gold, because it requires a small quantity of very clear water, and very little of the cascalho at a time; proportions which are precisely contrary to those required in washing gold. Black slaves are employed in this business, entirely naked excepting a cloth round their middle, in order that they may not embezzle any of the diamonds; but in spite of every precaution, and the vigilance of numerous inspectors, they nevertheless find means of concealing them, which they sell at a very low price, to the interlopers, for tobacco and rum.

This is all the information I can with certainty state respecting diamonds. I have only to remark, that other provinces likewise afford them; as Cuiaba, and the districts of Guarapuara, in the province of St. Paul; but these parts are not explored.

VI.

Abstract of the Specification of Mr. William Desmond's *new Method of Tanning; with Observations relative to that Subject.*

EARLY last year * a patent was taken out by Mr. William Desmond for the method or process of tanning practised, as it is said, with great success in France by Seguin. The substance of Mr. Desmond's specification is as follows:

The art of tanning consists in impregnating skins with a principle obtained from tan, with which they form a compound, insoluble in water, and possessing other qualities well known in the substance called leather. He obtains the tanning principle by digesting oak-bark, or other proper material, in cold water, in an apparatus nearly similar to that used in the saltpetre works. That is to say, the water which has remained upon the powdered bark for a certain time, in one vessel, is drawn off by a cock, and poured upon fresh tan. This is again to be drawn off, and poured upon other fresh tan; and in this way the process is to be continued to the fifth vessel. The liquor is then highly coloured, and marks, as Mr. Desmond says, from six to eight degrees on the hydrometer for salts †. He calls this the tanning lixivium. The criterion to distinguish its presence is, that it precipitates glue from its aqueous solution, and is also useful to examine how far other vegetable substances, as well as oak-bark, may be suitable to the purpose of tanning. The strong tanning liquor is to be kept by itself. It is found by trials with the glue, that the tanning principle of the first digester which receives the clear water, is, of course, first exhausted. But the same tan will still give a certain portion of the astringent principle, or gallic lixivium, to

* Jan. 15, 1795.

† Probably that of Baumé, described in his Elemens de Pharmacie. For the corresponding specific gravity in the usual form, see p. 39 of this Journal.

water.

water. The prefence of this principle is afcertained by its ftriking a black colour when added to a fmall quantity of the folution of vitriol of iron or green copperas. As foon as the water from the digefter ceafes to exhibit this fign, the tan is exhaufted, and muft be replaced with new. The gallic lixivium is referved for the purpofe of taking the hair off from hides.

Strong hides, after wafhing, cleaning, and flefhing, in the ufual way, are to be immerfed for two or three days in a mixture of gallic lixivium and one thoufandth part by meafure of denfe vitriolic acid *. By this means the hair is detached from the hides, fo that it may be fcraped off with a round knife. When fwelling or raifing is required, the hides are to be immerfed for ten or twelve hours in another vat filled with water and one five-hundredth part of the fame vitriolic acid. The hides being then repeatedly wafhed and dreffed, are ready for tanning; for which purpofe they are to be immerfed for fome hours in a weak tanning lixivium of only one or two degrees; to obtain which, the latter portions of the infufions are fet apart; or elfe fome of that which has been partly exhaufted by ufe in tanning. The hides are then to be put into a ftronger lixivium, where in a few days they will be brought to the fame degree of faturation with the liquor in which they are immerfed. The ftrength of the liquor will by this means be confiderably diminifhed, and muft therefore be renewed. When the hides are by this means completely faturated, that is to fay, perfectly tanned, they are to be removed, and flowly dried in the fhade.

Calf-fkins, goat-fkins, and the like, are to be fteeped in lime-water after the ufual flefhing and wafhing. Thefe are to remain in the lime-water, which contains more lime than it can diffolve, and requires to be ftirred feveral times a-day. After two or three days, the fkins are to be removed, and perfectly cleared of their lime by wafhing and preffing in water. The tanning procefs is then to be accomplifhed in the fame manner as for the ftrong hides, but the lixivium muft be confiderably weaker. Mr. Defmond remarks, that lime is ufed inftead of the gallic lixivium for fuch hides as are required to have a clofe grain; becaufe the acid mixed with that lixivium always fwells the fkins more or lefs; but that it cannot with the fame convenience be ufed with thick fkins, on account of the confiderable labour required to clear them of the lime, any part of which, if left, would render them harfh and liable to crack. He recommends, likewife, as the beft method to bring the whole furface of the hides in contact with the lixivium, that they fhould be fufpended vertically in the fluid by means of tranfverfe rods or bars, at fuch a diftance as not to touch each other. By this practice much of the labour of turning and handling may be faved.

Mr. Defmond concludes his fpecification by obferving that in fome cafes it will be expedient to mix frefh tan with the lixivium; and that various modifications of ftrength and other circumftances will prefent themfelves to the operator. He affirms that, in addition to the great faving of time and labour in this method, the leather, being more completely tanned, will weigh heavier, wear better, and be lefs fufceptible of moifture than leather tanned in the ufual way; that cords, ropes, and cables, made of hemp or fpeartery, impregnated with the tanning principle will fupport much greater weights without breaking, be lefs liable to be worn out by friction, and will run more fmoothly on pulleys; infomuch that, in his opinion, it will render the ufe of tar in many cafes, particularly in the rigging of fhips, unneceffary; and, laftly, that it may be fubftituted for the prefervation of animal food inftead of falt.

* Marking 66 degrees on the hydrometer for acids. Sp. gr. 1.848 of Baumé.

The intelligent manufacturer will readily perceive that this new method is grounded on two particular circumftances, befides a more fcientific management of the general procefs than has been ufual. The firft confifts in the method of determining the prefence and quantity of the tanning principle, by the hydrometer and the precipitation of glue: the fecond, in applying this principle in a concentrated ftate, more early in point of time than has, perhaps, been hitherto done. Our tanners, after the common previous proceffes and unhairing by acids, by lime, or by piling the hides that they may heat and begin to putrefy, apply the folution of tan, which they call ouze, in a great number of pits in the tan-yard. They begin with the weakeft folution, which has been ufed, and is of a lighter colour than the other. And they pafs the hides, according to their judgment and experience, into ouzes which are ftronger and ftronger, until at laft, in certain cafes, the hides come to be buried for a certain time in a folid mafs of tan or oak-bark. The oak-bark itfelf, in the pits, is not only the fource from which the water extracts the tanning principle; but feems, likewife, in fome meafure, during the laft ftages of the procefs, to operate mechanically by keeping the furfaces of the hides from touching each other.

On the occafion of this apparently important difcovery, I applied to one of the firft manufacturing houfes in the Borough of Southwark, to make enquiries concerning its value. One of the partners, who appeared to have paid confiderable attention to the proceffes of his manufactory, informed me, that the ftrong folution obtained from tan had been well known to them for fome years under the name of effence of tan; and that it had been propofed to ufe it as the means of bringing out the complete leather in a fhort time. I afked whether the objection to its ufe might confift in the outer part of the fkin becoming more perfectly tanned in a fhort time than the inner part; fo as to defend this laft from the fubfequent change that might have taken place by a flower operation. He anfwered, that this was partly the reafon why it was neceffary that the ouzes fhould be gradually applied from the loweft to the higheft ftrength. But the chief reafon he urged in favour of the flow procefs was, that the hides were found to feed and improve in their quality by remaining in the pit. I could not gain fatisfactory information what the nature of this feedingand improvement might be. The fact, as loofely ftated, appeared to be, that the fkin became much thicker, denfer, and heavier in the firft ouzes during a certain time; and that thefe advantages of thicknefs, denfity, and weight continued to exift in a certain degree to the very end of the entire procefs. For, as he remarked, they know very well that, by bringing the fkins more haftily into the ftronger ouzes, they will be fooner converted into leather; and that a fkin which ordinarily requires fifteen months, might be tanned in nine. But then, added he, the fkin which comes forward in nine months, from its thinnefs, and other inferior qualities, will afford ninepenny foles for fhoes; whereas, if it had undergone the longer procefs, the foles would have been worth a fhilling:—a difference which largely repays the common intereft of capital for the excefs of time.

On the other hand it may be remarked, that refults of this nature require to be grounded on experiments carefully undertaken, under circumftances differing only with regard to the object of inveftigation, and often repeated. It does not appear from my enquiries that this has actually been the cafe; and I have been informed that excellent leather has been produced in the new method. But I have heard nothing of its appearance in the market, though upwards of a year has elapfed fince this patent was fealed.

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A very full and circumſtantial account of this proceſs, which Mr. Deſmond affirms he received from a certain learned foreigner, was inſerted in the *Moniteur*, and thence tranſlated and publiſhed in the Engliſh *Courier* ſome time in the month of Auguſt 1795.

VII.

Deſcription and Account of a New Preſs operating by the Action of Water, on the Principle of the Hydroſtatic Paradox. Invented by JOSEPH BRAMAH, *Engineer.*

FIG. 1, Plate II, is a front ſection of a common preſs for books and papers, and the like. Fig. 2 exhibits a perſpective view of the ſame inſtrument on that ſide to which the pump is fixed. The letters diſtinguiſh the ſame parts in both. ABCD is the frame. I is a ſtrong metallic cylinder, in which the rammer or piſton EF moves. To the upper part of this piſton is applied an iron table, by the motion of which upwards the preſſure is communicated to the articles H. QR repreſents a ſmall ciſtern, containing water. Within this ciſtern is fixed a ſmall forcing pump, of which K is the barrel, L the piſton, M a ſide valve formed of metal, and opening inwards beneath the piſton. The nature and effect of this valve may be eaſily underſtood from the figure. It conſiſts of a metallic rod, at one end of which there is a nob turned conical next the ſtem, ſo as accurately to fit the conical face of the hole into which it is put. The tail is filed on one ſide, ſo that it does not entirely fill the cylindrical hole which it occupies; by which means a paſſage is afforded for water when the head of the valve is raiſed. In the ſtate of inaction, the valve is kept ſhut by the operation of a ſpiral ſpring at the other end of the tail. N is another valve of the ſame kind opening downwards. O repreſents the rod of the piſton, with a contrivance for keeping a vertical poſition during the working. The effect of this contrivance will be underſtood without difficulty by comparing the two drawings. S is the lever or handle for working the engine.

Its action is as follows: When the handle S is raiſed, it brings up the piſton L, which would leave a vacuum beneath if the preſſure of the atmoſphere did not force the water in through the ſide valve M. The lever is then to be preſſed down, which cauſes the ſide valve to ſhut, and forces the water through the bottom valve N, whence it paſſes through the pipe P into the cavity F of the great cylinder I, and raiſes the piſton or preſſing rammer. A repetition of the ſame proceſs forces more water in, and the preſſure may in this manner be carried to a great extent. When it is propoſed to relieve the action, the lever S muſt be preſſed down, which, by the mechanical contact of the lower extremity of the piſton L againſt the tail of the lower valve N, keeps that valve open. In this ſituation, the lever TU is to be preſſed towards R, and will open the valve M. Both valves being in this manner opened at once, the paſſage between the internal part of the great cylinder and the ciſtern QR becomes free, and conſequently the table G and the rammer EF deſcend by their own weight, and reſtore the engine to its original ſituation.

There is no difficulty in computing the force of this inſtrument. If the diameter of the barrel K be one quarter of an inch, and that of I one inch, that is to ſay, four quarters of an inch; one pound lodged upon the piſton-rod W will be in equilibrio with ſixteen pounds lodged upon the table G; the weights of the parts of the engine attached to, and moving with, each piſton, being reſpectively included. And if the length of the lever SY be fifteen inches, and the diſtance XY between the centres of motion and of action be two inches, one pound at the end of S will gain an advantage of $7\frac{1}{2}$ times when compared with that at W. Inſtead, therefore, of ſixteen pounds upon the table G being equal in effect to counterpoiſe this laſt action, there will be required upwards of 120 pounds. But a man, in this action of pumping by a downward preſſure, can without difficulty apply his whole weight, and with great eaſe one third or one fourth of his weight, ſuppoſe 50 pounds. In this caſe the preſſure will be equivalent to fifty times 120 pounds, or 6000 pounds, that is to ſay, nearly three tons.

To compare this engine with a ſcrew, in theory, we muſt enquire what fineneſs of thread and length of lever would afford a purchaſe of 120 to one. Let us ſuppoſe the thread of a ſcrew, ſubſtituted in the place of the barrel I, to be one tenth of an inch thick; the diſtance from the top of one thread to the top of the next will in this caſe be one fifth of an inch. This is the ſpace through which the weight muſt riſe in one revolution. The power muſt therefore move through 120 times that ſpace, namely twenty-five inches; but a lever or radius four inches long will deſcribe a circle ſomewhat larger than this, and conſequently ſuch an engine would in theory be equal in power to the hydraulic engine we have been contemplating.

But when the ſubject is viewed practically, the difference between the two engines appears to be very remarkable. All practical men know how very large a part of the force operating by means of engines is employed in overcoming frictions. Every one is aware of the extreme friction between ſolids, and the very ſlight friction which takes place between the parts of fluids. This is ſeen in the common expedient of oiling the pivots of wheels, and in the very gradual decay of motion in fluid bodies; while ſolids moving on each other ſtop at once, as ſoon as the force is diminiſhed to a certain degree. The ſcrew is an organ peculiarly liable to friction, and this friction is always much greater than the whole of the reacting force; for there are few inſtances where a ſcrew will return from extreme preſſure, when the agency upon the lever is withdrawn. It is alſo to be conſidered, that the whole force of the weight or reſiſtance acts directly upon the face of the ſcrew, at which the motion is required to take place. It has not been appretiated in what degree this reſiſtance or friction increaſes with the weight. In lighter actions the ſimple ratio has been inferred; but under more ſevere preſſures the two metallic faces extrude the greater part of the half-fluid matter between them, and appear by the magnitude of their reſiſtance to be attached to each other by a proceſs of the nature of coheſive attraction. For theſe and other reaſons, it appears nearly impracticable to form any compariſon between two engines ſo different in principle, but ſuch as ſhall be deduced from immediate experiment of their effects. I am not in poſſeſſion of numerical data to indicate the actual power of ſcrew-engines or preſſes; which are perhaps the leſs neceſſary, becauſe thoſe who are the moſt intereſted in the ſucceſs of an improvement like the preſent, are for the moſt part able to come at theſe without difficulty. I intend, beſides, to make theſe the ſubject of a future communication. The effects which I obſerved were theſe.

A machine of the new conſtruction here deſcribed, was employed to preſs ſome papers. The force applied to the lever was ſo ſlight, that the inſtrument required no faſtening to the table on which it ſtood: but the effect on the upper bar, A B, which was $3\frac{1}{2}$ inches thick, was ſuch as bended it out of a ſtraight line upwards of a quarter of an inch, and I apprehend that it might have eaſily been broken by continuing the preſſure. With a ſcrew-

 preſs,

preſs, the ſcrew of which was iron, and nearly of the dimenſions before mentioned, excepting that the lever was twelve inches long inſtead of four inches, and the action on the lever upwards of two hundred weight, applied with a jerk, the effect was nearly the ſame. Here I ſhould eſtimate the advantage to be very much in favour of the hydraulic engine.

In another engine of this kind, the diameter of the great piſton was four inches, and of the ſmaller three-eighths of an inch; and the advantage given by the lever or handle was twelve to one. Above the piſton of the great cylinder was applied a long lever, at one end of which was an axis, and at the other end a large ſcale to hold weights: it contained twenty hundred weight. The diſtance between the axis of motion of this lever and the part where it acted on the piſton was ſix inches; and the diſtance from the ſame axis to the extremity where the ſcale was hung, was 126 inches. Every hundred weight in the ſcale conſequently preſſed upon the piſton with a force equal to twenty-one hundred weight; whence the whole preſſure was twenty-one tons. It was eaſy to work the lever briſkly with one hand, and each ſtroke raiſed the ſcale near one-third of an inch. Forty-ſeven pounds hung at the end of the lever, carried it down with a moderate ſwiftneſs of working; but a weight of only forty-three pounds remained in equilibrio, and did not deſcend. Now, as the true weight in theory was thirty-two pounds, as deduced from the action of the parts in the manner already done with regard to the ſmall machine, it follows that leſs than one-third of the actual power was employed to give velocity and overcome all friction.

It may be remarked, that the principal frictions in theſe machines muſt be at the circumferences of the piſtons, and that theſe do not increaſe in the ſimple, but in leſs than the ſub-duplicate ratio of the power. For if the diameter of the great cylinder were double, every thing elſe remaining unchanged, the ſurface of its piſton, and conſequently the power, would be quadrupled. But the friction would be only doubled, and that merely at the leathering of the greater piſton.

As the preſſure in the experiment laſt mentioned amounted to 47040 pounds upon the great piſton of four inches diameter, or ſixteen circular inches ſurface, it amounted to 2940 pounds upon each round inch. But the medium preſſure of the atmoſphere on a round inch is near twelve pounds, conſequently the action was equal to 245 atmoſpheres: and as each of theſe correſponds with a column of 34 feet of freſh water at a medium, the water in the cylinder was preſſed in the ſame manner as if the whole column had been 8330 feet, or $1\frac{2}{7}$ mile, long.

Large preſſes of this conſtruction are made with two pumps of $1\frac{1}{4}$ inch bore, and a cylinder of ſeven inches. Theſe have been uſed in preſſing hay and cotton for package; and, as I am informed, are effective in producing a greater condenſation on the material with a much leſs application of moving power and conſumption of time. But of this and other particulars, as I have not yet had an opportunity of examining the facts myſelf, I ſhall forbear to ſpeak at preſent *.

* Mr. Bramah, who conſtructs theſe engines, has obtained a patent for the invention.

VIII. *The*

VIII.

The Procefs for giving a beautiful White Colour to Raw Silk, without Scouring. By M. BAUME.*

BERTHOLLET, in his Elemens de l'Art de la Teinture, publifhed in the year 1791, after defcribing the ufual methods of depriving filk of its refinous or gummy matter †, proceeds to remark, that, in the manufacture of blonds and gauzes, the natural elafticity and ftiffnefs of this article are required to be preferved; whence it has become a defideratum to render the yellow filk of Europe white like that of China, without depriving it of its gum. He adds, that Mr. Baumé has folved this interefting problem, but had kept his procefs a fecret; but from the facts he had poffeffed the means of obtaining, it appeared liable to accidents, and that the chief difficulty confifted in giving an uniform white colour when large quantities were operated upon. He alfo mentions a difficulty in dreffing the whitened filk fo as to prevent its curling, and obferves that it ought certainly to be kept conftantly ftretched during the drying. It is befides requifite that the fpirit of wine fhould be recovered after the procefs, which would elfe be rendered too expenfive. This author does not fay whether the white Chinefe filk is fubject to the fame inconvenience of curling when dyed, which, it may be remarked, is a property of no confequence where the material is to be applied in the manufacture of white goods. The motives which led Mr. Baumé to communicate his procefs to the world, originally retained by him as a lucrative fecret, do not appear. Whether the miftakes of thofe who carried it into effect in the large way might have led him to vindicate the reality of his difcovery by publication; or whether the commercial advantages derived from fuperiority of quality and cheapnefs in his article over the Chinefe filk in the market of France, might in the end have proved of lefs value than the fcientific reputation to be derived from its difclofure; are circumftances which will, no doubt, have their proper weight with fuch manufacturers as may be induced gradually to adopt this procefs.

The filk of Nankin is perfectly white, filvery, brilliant, and poffeffes all the elafticity of raw filk. Our author affirms, that the value of this article imported into Europe amounts to upwards of twenty millions of livres ‡, of which France confumes about four or five millions in gauzes, blonds, ribbons, &c. This was formerly fuppofed to be produced of a white colour from the worm. The late Mr. Trudaine, intendant of commerce, procured the eggs of the filk-worm from China, and cultivated them. The produce confifted of yellow cocons, and others of the moft perfect whitenefs. The latter afforded filk equal in this refpect to that of Nankin. But Mr. Baumé affirms, that moft of the Nankin filk is bleached by art, and, as he thinks, by a procefs fimilar to his own.

As it is impoffible to wind off a large quantity of filk in the fhort time previous to that of the infects eating their way through the mafs, it is ufual, in the firft place, to deprive them of life. This is commonly done by expofing the cocons, properly wrapped up, for two hours to the heat of about 158 degrees of Fahrenheit in an oven; after which they are

* The original Memoire from which the facts here related are taken, is inferted in the Journal de Phyfique, xlii. 375—399.

† Tom. i. p. 146; or p. 141 of Dr. Hamilton's tranflation.

‡ About eight hundred and thirty thoufand pounds fterling.

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kept for a certain time in a mafs to preferve their heat, and effectually deftroy fuch of the infects as might have efcaped the power of the oven. The effect of this procefs is, that the filk is hardened, and is more difficult to wind off than before. Hence the product of filk is lefs by one ninth part in quantity, and inferior in quality to what might have been obtained by winding off without this previous baking. Mr. Baumé, not only from thefe views, but likewife becaufe the filk which has not been baked proves fufceptible of a greater luftre, was induced to deftroy the chryfalis by fpirit of wine. For this purpofe he difpofes them in a wooden box in a ftratum fix inches deep: upon each fquare foot half a chopin, or fomewhat more, of fpirit of wine is to be fprinkled with a fmall watering-pot made for that purpofe. This quantity anfwers fufficiently near to our half-pint. The liquid is to be equally diftributed, but it is not neceffary that all the cocons fhould be wetted. They are then to be mixed by hand. In the next place another ftratum is to be formed over the firft, nearly of the fame depth, which is to be fprinkled and treated as before. By this method of proceeding, the box becomes filled, and muft then be covered, and left for twenty-four hours, during which time they become fpontaneoufly heated to about 100 degrees, and the vapour of the fpirit of wine exerts itfelf with wonderful activity. Five hundred French pounds * of the cocons require ten French pints, which is nearly the fame number of Englifh quarts. After this treatment they muft be fpread out to dry, which happens in a fhort time, and is abfolutely neceffary previous to winding off.

When the operator propofes in this manner to extinguifh various parcels of cocons belonging to different individuals, each parcel may be tied up loofely in a canvas bag, and wetted on the outfide previous to clofing the box.

The fpirit of wine to be ufed in this operation, ought to be of the ftrength of 34 degrees of Baumé's hydrometer at the temperature of 55 degrees. It is of the greateft importance to ufe that fpirit only which has been kept in veffels of glafs, of tinned copper, or of pure tin. Leaden veffels are abfolutely to be rejected; wooden veffels tinge the fpirit, which gives the filk a degree of colour of confiderable folidity, and very inimical to the bleaching procefs.

With regard to the advantages of this method of extinction, in preference to that of the oven, the author remarks, that the coft of labour and fuel added to the lofs of filk, and the probability of injury from too much or too little heat, conftitute a fum of difadvantage much greater than the coft of the fpirit of wine. It is befides a confiderable advantage, that the fpirit of wine renders more diftinguifhable fuch cocons as have perifhed previous to the application of the fpirit. Thefe afford a much worfe filk, and muft be picked out.

The filk is wound off upon a reel, while the cocons are kept immerfed in water almoft boiling. Upon this part of the procefs Mr. Baumé remarks, 1ft, That the dead cocons muft be feparated. Thefe are known by the brown or black fpots on their furface. 2. That well-water, which on account of its clearnefs is almoft univerfally ufed in the filk manufactories, moftly contains nitre, and is extremely prejudicial to the bleaching procefs. The prefence of nitrous acid gives a yellow colour, which refifts bleaching and even fcouring; he therefore recommends river-water. 3. In fome countries a fmall quantity of

* The Paris pound is to the Englifh avoirdupois pound as 756 to 700. I have not reduced thefe quantities, becaufe the operation requires no great precifion. N.

alum is ufed. Neither this nor any other faline fubftance is of the leaft advantage to the colour, beauty, or quality of the filk.

At the four places of contact of the filk upon the reel, all the threads ftick together. It is abfolutely neceffary that this fhould be remedied. The method confifts in foaking the filk in a fufficient quantity of warm water, at about 90 degrees, for about two hours; after which the threads are to be feparated by opening the hanks upon a pin, and lightly rubbing the parts which cohere. When the filk is dry, it is to be loofely folded in its original form, and is ready for bleaching.

The filk while wet is foft, and part of its gummy matter is in fuch a ftate, that its threads would readily adhere, if wrung while warm for the purpofe of clearing it of the water. After fuch improper treatment there would be no other remedy than to foak it again in warm water.

The apparatus for bleaching the filk confifts of a ftone-ware veffel, nearly of a conical form, capable of holding about twelve gallons, having a large opening at the one end, and a fmaller of about an inch diameter at the other end. Common pottery cannot be ufed in this operation, becaufe it is foon rendered unferviceable by the action of the marine acid, and the ftone-ware itfelf is not very durable. This veffel muft be carefully examined, to afcertain that it does not leak in the flighteft degree; after which the infide is to be rubbed with a pumice-ftone, to clear it of afperities which might break the threads. A cover of the fame material is to be fitted on by grinding; and the fmaller aperture, which in the ufe is the loweft, is to be clofed with a good cork, in the middle of which is thruft a fmall glafs tube about a quarter of an inch in diameter; this is likewife ftopped with a cork, excepting at the time when it is required to draw off the liquid contents of the jar. A fmall perforated falfe bottom is placed within the veffel, to prevent this tube from being obftructed.

This jar, or as many of them as the purpofes of the manufactory may require, is fupported by a wooden frame or table, at fuch a height that a cafk may be conveniently placed beneath to receive what may flow from the glafs tube in the feveral periods of the operation.

Six pounds of yellow raw filk are to be difpofed in the earthen pot; upon this is to be poured a mixture, previoufly made, of forty-eight pounds * of fpirit of wine at 30 degrees, with twelve ounces of very pure marine acid, abfolutely exempt from all prefence of nitrous acid, and of the ftrength of 14 or 15 degrees of Baumé's hydrometer. The pot is then to be covered, and the whole left in digeftion till the following day, or until the liquor, which at firft affumes a fine green colour, fhall begin to affume that of a dufky brown (feuille morte).

The acidulated fpirit is then to be drawn off. To prevent evaporation, M. Baumé thrufts a cork in the bung-hole of the receiving cafk, in which is a fliding glafs tube. The ufe of this tube is completely to furround the fmall tube proceeding from the earthen veffel. When the whole of the fluid is thus almoft entirely drawn off, clean fpirit of wine is poured upon the filk, and drawn off repeatedly until it paffes colourlefs. The filk is then fuffered to drain without ftirring it. In this ftate it is ready for a fecond infufion.

Forty-eight pounds of fpirit of wine acidulated with twelve ounces of marine acid is now to be poured on the filk, and the whole fuffered to remain for twenty-four hours or longer, until the filk becomes perfectly white. The time required for this fecond infufion

* The pound is nearly a pint, and is divided into fixteen ounces. With regard to the ftrengths, fee Article IX. p. 37.

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is commonly longer than for the firft: it fometimes amounts to two, three, or even fix days, according to circumftances, particularly the temperature and the nature of the filk. Silk which has been in the oven is in general more difficult to bleach.

When the filk has thus obtained its utmoft degree of whitenefs, the acidulated fpirit is to be drawn off into a feparate veffel. This fluid is but flightly coloured, and may be ufed again in the firft infufion of other yellow filk, with the addition of fix ounces more of marine acid. The receiving veffel is to be removed, and another clean veffel fubftituted in its place. The filk is then fprinkled with clean fpirit, and occafionally preffed down with the hand. As foon as the fpirit of wine comes off abfolutely colourlefs, a third infufion is to be made by pouring upon the filk forty-eight pounds of the pure fpirit without acid, which is to remain till the following day: it is then to be drawn off, and referved for wafhing other filk after the firft infufion.

After the filk has been left to drain, and affords no more fpirit, it ftill retains its own weight of that fluid. This is recovered by the very fimple procefs of fprinkling the filk with a fmall quantity of very clear river-water at a time. While the water applies itfelf and fubfides along the filk, it drives the fpirit of wine before it, fo that the firft portions which flow from the tube are fcarcely diminifhed in ftrength. The addition of water is to be continued until nothing but mere water comes off below.

In this fituation the filk is found to be well bleached, but ftill retains a portion of marine acid fufficient to render it harfh to the touch, and after a time brittle. It muft be wafhed off with water. The beft method is to put the filk loofely into a coarfe woollen bag, which is to be fecured loofely in another cloth like a fmall bed or pillow, then placed in a bafket and left in a running ftream for five or fix hours; but where the convenience of a ftream is wanting, the earthen pot containing the filk is to be covered with a cloth, and water pumped through it for five or fix hours, or until that which iffues from the lower aperture gives no red colour to the tincture of tournfol. At this period the lower opening is to be clofed and the veffel filled with water, which muft be changed once or twice in twenty-four hours.

The time required for wafhing was occafionally abridged by paffing fpirit of wine, or river-water impregnated with a fmall portion of alkali, through the filk. The neutral falt thus produced is in fact lefs adherent to the filk than the acid itfelf, but neverthelefs requires to be wafhed off with a very large quantity of water.

In thefe as in every other procefs relating to the filk, great care muft be taken to afcertain that the water made ufe of contains no nitrous acid, which would infallibly occafion imperfection of colour, or fpots in the article. After this treatment the filk is ready for drying and luftering; previous to the defcription of which, the author makes feveral remarks to the following purport:

Though the mineral acids are the moft powerful and deftructive of all faline fubftances, yet they may be applied to filk when diluted with fpirit of wine in very confiderable dofes. In trials made to afcertain the maximum, two ounces of marine acid were added to one pound of fpirit of wine, without altering the filk. Two drams of marine acid caufe a very perceptible alteration in one pound of filk. I fuppofe he means pure acid, or perhaps diluted with water; for the paffage as it ftands is obfcure. Numerous experiments have fhewn that the marine acid is preferable to any other. The proportions admit of much latitude, though he prefers the dofe hereinbefore defcribed.

Spirit of wine which has been mixed with nitrous acid, cannot be ufed in bleaching, even though afterwards rectified upon an alkali, becaufe it ftill retains a portion of nitrous gas.

Pure fpirit of wine without acid extracts a fine yellow colour from filk, which does not feparate for years, even though expofed to the fun's light. Yellow filk expofed to the fun lofes its colour in a fhort time. The acidulated fpirit which has been ufed in the infufion of filk, is changed by expofure to the fun, but not in fuch a manner as to be rendered fit for ufe a fecond time.

In order to obtain a beautiful white colour, it is effential that the filk fhould be immerfed in a large quantity of the fluid, efpecially at the firft infufion. Without this management it would become neceffary to make three infufions in the acidulated fpirit. When the firft infufion is well managed, the filk will have loft all its yellow colour, and become confiderably white, at the fame time that the liquor will have begun to change colour a little. As long as it continues of a fine green, it is certain that it has not exhaufted its whole action upon the filk.

The duration of this firft infufion may be longer or fhorter, without inconvenience, according to the temperature. When the temperature is at 20 degrees of Reaumur, which anfwers to 77 of Fahrenheit, the firft infufion is often made in ten or twelve hours. In fmall experiments the heat of the atmofphere may be fupplied by the water-bath; in which cafe, all the infufions are eafily made in the courfe of a day.

When the firft infufion is finifhed and the liquor drawn off, the filk appears greenifh: the fubfequent wafhings in fpirit of wine clear it of the liquor it retained. This fprinkling fhould be made with the watering-pot, otherwife the quantity poured will be greater, and the management more wafteful.

The cocons may be bleached in this way, but the inconveniences are too great to render this procefs defirable.

Pieces of gauze and entire garments of filk have been fuccefsfully bleached in this way.

The fineft natural white filks are rendered infinitely whiter by this procefs. Spirit of wine alone has the property of depriving yellow filk of its colour, which it brings to the ftate of the naturally white filk. In this ftate the filk is difpofed to acquire a greater degree of brightnefs by a fingle infufion in the acidulated fpirit. This procefs has its advantages over the other, to which it is alfo inferior in certain refpects; concerning neither of which the author has entered into any detail.

The colouring matter was found to be a refin perfectly animalized, affording by diftillation the fame products as other animal matters, and the concrete volatile alkali.

Silk whitened by fcouring may be dried freely in the air without affecting its luftre. This is not the cafe with the filk bleached in the gum: if it be left at liberty to dry in the air, it refembles white flax without any luftre. The beauty of this filk confifts in its fhining brilliancy; to fecure which, it muft be dried in a ftate of tenfion. Mr. Baumé has contrived a fimple machine for this purpofe. It confifts of a ftrong fquare frame of wood ftanding upright upon feet: the upper horizontal bar is fix feet long, and has fix iron pins driven

through

through it at equal diftances, fo as to project on each fide for the purpofe of receiving twelve bobbins. The lower horizontal bar is moveable up and down in a mortice by means of a fcrew at each end: it is furnifhed with fix holes, adapted to receive as many pins to correfpond with thofe above. The fkains of filk are to be dreffed and arranged upon wooden pins, as they are taken out of the fack from wafhing. As foon as there are twelve together, they are to be wrung with a ftaff; after which the fkains are to be hung one by one upon as many bobbins put upon the upper pins of the fquare frame. Another bobbin with tails is to be inferted in the lower loop of the fkain, and faftened to the correfponding pin of the lower bar, by means of a ftrap and hook, which need not be defcribed to fuch as are flightly acquainted with mechanical objects. When the machine is thus fupplied with fkains on both fides, the lower bar of the frame is to be preffed down by the fcrews until the filk is moderately ftretched. When it is dry, the fcrews are to be equally flackened, the fkains taken off, and folded with a flight twift, that they may not become entangled.

After this defcription of the whole of his procefs, the author proceeds to make certain general remarks on the white China filk. He obferves, that in his procefs the filks acquire the perfect whitenefs without much handling, and confequently that there is little caufe for them to become entangled. Accordingly the lofs in unwinding is found to be no greater than when they are unwound in the yellow ftate: that is to fay, from a dram to a dram and a half in the pound. This faving is of the greateft importance in the price of the filk.

The filk of Nankin, which he fuppofes to be bleached by fome procefs of the fame nature, is probably handled much more. The lofs is nearly twelve per cent. when it comes to be opened, and not unfrequently even twenty-five per cent.; a lofs which cannot in any refpect arife from the package. The quality of the Nankin filk differs much in the package; the external part being always of the beft quality, and that which is packed within is of fuch an inferior quality as fometimes not to exceed half the value. On examining this filk, it not only exhibited unequivocal marks of alkali, but its imperfections were alfo of the fame kind as thofe which had occurred to Mr. Baumé during the progreffive improvement of his own manipulations. The beft China filk was neither improved nor injured by the procefs of Baumé; whence he concludes that they are not naturally white, but have undergone a procefs fimilar to his.

The refult of the whole is, that the yellow filks of Europe may be bleached to equal or greater perfection than thofe of Nankin; and that thefe may be even greatly exceeded by winding the naturally white filk apart from the other, and bleaching it by itfelf.

The methods of recovering the fpirit fo as to be ufed again, and of obtaining the marine acid in the requifite ftate of purity, will be defcribed in our next.

IX.

On the Hydrometer of BAUME.

AS many French chemifts refer to the pefe-liqueur of Baumé, which has never been ufed in this country, it will be of advantage to defcribe the method by which it is conftructed, and fhew the fpecific gravities indicated by the graduations upon the ftem. Inftead

ftead of adopting the fimpler method of immediate numerical reference to the denfity of water expreffed by unity, as is done in all modern tables of fpecific gravity, he had recourfe to a procefs fimilar to that of graduating the ftems of thermometers from two fixed points. The firft of thefe points was obtained by immerfing his inftrument, which is the common areometer, confifting of a ball, ftem, and counterpoife, in pure water. At that point of the ftem which was interfected by the furface of the fluid, he marked *zero*, or the commencement of his graduations. In the next place he provided a number of folutions of pure dry common falt in water: thefe folutions contained refpectively one, two, three, four, &c. pounds of the falt; and in each folution the quantity of water was fuch, as to make up the weight equal to one hundred pounds in the whole; fo that in the folution containing one pound of falt, there were ninety-nine pounds of water; in the folution containing two pounds of falt, there were ninety-eight pounds of water, and fo of the reft. The inftrument was then plunged in the firft folution, in which of courfe it floated with a larger portion of the ftem above the fluid, than when pure water was ufed. The fluid, by the interfection of its furface upon the ftem, indicated the place for marking his firft degree; the fame operation repeated, with the fluid containing two pounds of falt, indicated the mark for the fecond degree; the folution of three pounds afforded the third degree; and in this manner his enumeration was carried as far as fifteen degrees. The firft fifteen degrees afterwards, applied with the compaffes repeatedly along the ftem, ferved to extend the graduation as far as eighty degrees, if required.

This inftrument, which is applicable to the admeafurement of denfities exceeding that of pure water, is commonly diftinguifhed by the name of the Hydrometer for falts.

The Hydrometer for fpirits is conftructed upon the fame principle; but in this the counterpoife is fo adjufted, that moft part of the ftem rifes above the fluid when immerfed in pure water, and the graduations to exprefs inferior denfities are continued upwards. A folution of ten parts by weight of falt in ninety parts of pure water, affords the firft point, or zero, upon the ftem; and the mark indicated by pure water is called the tenth degree; whence, by equal divifions, the remaining degrees are continued upwards upon the ftem as far as the fiftieth degree.

Thefe experiments, in both cafes, are made at the tenth degree of Reaumur, which anfwers very nearly to fifty-five of Fahrenheit.

M. Baumé affirms, that all his inftruments, conftructed in thefe methods, agreed together with the utmoft precifion. From a few experiments, which however require to be carefully repeated, I am difpofed to apprehend that the folutions of common falt do not give a fufficiently accurate original point, and that they may differ not only from the comparative drynefs of the falt in different experiments, but likewife the ftate of its cryftals, whether haftily or flowly feparated in their original fabrication, the purity being fuppofed the fame. Such differences muft confiderably affect the remote terms formed by repetition of the experimental interval in either inftrument. I fuppofe all M. Baumé's inftruments were conftructed from folutions made once for all, and referved for this purpofe; and that the French chemifts who ufed them were fupplied under his direction. For thefe reafons I am more inclined to deduce the fpecific gravities from the experiments of himfelf and another accurate poffeffor of thefe inftruments, than to recur to the original method of conftruction.

 M. Baumé,

M. Baumé, in his Elemens de Pharmacie *, from which the whole of this account is deduced, has given a table (p. 410.) of the degrees of his Hydrometer, indicated by different mixtures of ardent ſpirit and pure water; where, he ſays, the ſpirit made uſe of gave thirty-ſeven degrees at the freezing point of water; and in a column of the table he ſtates the bulk of this ſpirit, compared with that of an equal weight of water, as $35\frac{5}{8}$ to 30. The laſt proportion anſwers to a ſpecific gravity of 0.842, very nearly. A mixture of two parts, by weight, of this ſpirit, with thirty of pure water gave twelve degrees of the Hydrometer at the freezing point. This mixture, therefore, contained $6\frac{1}{2}$ parts of Blagden's ſtandard to 100 water, and, by Gilpin's moſt excellent tables †, its ſpecific gravity muſt have been 0.9915. By the ſame tables, theſe ſpecific gravities of 0.842 and 0.9915 would, at 10° Reaumur, or 55° Fahrenheit, have fallen to 0.832 and 0.9905. Here then are two ſpecific gravities of ſpirit correſponding with the degrees 12 and 37, whence the following table is conſtructed.

BAUME's Hydrometer for Spirits.

Temperature 55° Fahrenheit, or 10° Reaumur.

Degrees		Sp. Gravity	Degrees		Sp. Gravity.
10	=	1.000	26	=	.892
11	=	.990	27	=	.886
12	=	.985	28	=	.880
13	=	.977	29	=	.874
14	=	.970	30	=	.867
15	=	.963	31	=	.871
16	=	.955	32	=	.856
17	=	.949	33	=	.852
18	=	.942	34	=	.847
19	=	.935	35	=	.842
20	=	.928	36	=	.837
21	=	.922	37	=	.832
22	=	.915	38	=	.827
23	=	.909	39	=	.822
24	=	.903	40	=	.817
25	=	.897			

With regard to the Hydrometer for ſalts, the learned author of the firſt part of the Encyclopédie ‡, M. de Morveau, who by no means conſiders this an accurate inſtrument §, affirms, that the ſixty-ſixth degree correſponds nearly with a ſpecific gravity of 1.848; and as this number lies near the extreme of the ſcale, I ſhall uſe it to deduce the reſt.

BAUME's Hydrometer for Salts.

Temperature 55° Fahrenheit, or 10° Reaumur.

Deg.		Sp. Gr.	Deg.		Sp. Gr.	Deg.		Sp. Gr.	Deg.		Sp. Gr.	Deg.		Sp. Gr.
0	-	1.000	15	-	1.114	30	-	1.261	45	-	1.455	60	-	1.717
3	-	1.020	18	-	1.140	33	-	1.295	48	-	1.500	63	-	1.779
6	-	1.040	21	-	1.170	36	-	1.333	51	-	1.547	66	-	1.848
9	-	1.064	24	-	1.200	39	-	1.373	54	-	1.594	69	-	1.920
12	-	1.089	27	-	1.230	42	-	1.414	57	-	1.659	72	-	2.000

* Fifth edition. Paris 1784. † In Philoſoph. Tranſ. 1794. ‡ Tome I. p. 360. à Paris 1796. § Ibid. p. 361.

X. *Obſervations*

X.

*Observations on the Soap of Wool, and its Uses in the Arts**. By J. A. CHAPTAL, Instituter of the Polytechnic School.*

I HAVE shewn the method of forming, at any time or place, and at a small charge, a saponaceous liquid adapted to supply the place of soap for domestic purposes †. On the present occasion I shall offer a supplement to my former work, by exhibiting, as a substitute for the soft soap used in fulling almost every kind of woollen stuff, a soap of little expence, which may be easily made in every manufactory.

In every manufactory of broad-cloth, and other fabrics of wool, it is usual to full the stuff immediately after it has passed the loom. This operation is performed not only for the purpose of clearing them of the oil, but to give them the requisite density. For this purpose about thirty pounds of soap are used for every eighty pounds of the stuff. This soap in the south (of France) cost twenty livres the hundred weight before the Revolution. It consumed a large part of our oils, as well as those of Italy, and all the wood-ashes of the domestic fires in the respective countries in which it was made.

Hence it is obvious how greatly beneficial it must prove to the manufacturer, as well as to commerce in general, to be able to substitute without difficulty, instead of the soft soap, another compound of materials, easy to be procured, and of moderate cost. In addition to the saving in the fabrication of the stuff, very great advantage would be derived from the wood-ashes of our fires being left for domestic use, or for the salt-works, or manufactories of green glass; at the same time that the oil formerly consumed would remain to be totally applied to such works as cannot be conducted without it.

In all ages this problem has offered itself for solution to the Manufacturer and the Government. Fullers'-earth, pure alkalis, and other agents, have been successively employed. The first of these is of inferior quality, either for bleaching or fulling; the second dissolves the stuff. The manufacturers of Lodève still recollect with terror a charlatan sent to them by Government a few years ago, who pretended to substitute the mineral alkali in the place of soap.

To these inconveniencies we must add that of not rendering the cloth supple, but leaving it in possession of that harshness which soap alone removes. It is requisite, therefore, that whatever substance may be offered as a substitute for this article should possess the qualities of cleaning, fulling, and softening, the stuff. The composition I am about to describe possesses all these advantages. Experiments have been made at my request, at Lodève, by Citizen Michel Fabriguette, who is intimately acquainted with natural philosophy, and a skilful manufacturer of drapery.

The whole operation consists in making an alkaline lixivium of wood-ashes or pot-ash, and dissolving therein, at the boiling heat, old rags ‡, or clippings of wool, to the point of saturation.

* Nearly a literal translation from the Annales de Chimie, XXI. 27.

† See the Report of Citizens Pelletier, D'Arcet, and Le Lievre, on the fabrication of Soap.

‡ Old woollen rags are a very cheap article in this country. But, as every other kind of hair must certainly answer, and horns and hoofs probably will, there must be an immense and probably cheaper source in the refuse of the tanners, hog-butchers, horners, and comb-cutters. All these, at present, are used only as manure. N.

faturation. The product is a foft foap, very foluble in water, of a green greyifh colour, well blended (bien lié), and poffeffing an animal fmell, which the cloths lofe by wafhing and expofure to the air.

The various experiments I have made on this fubject have prefented the following refults:

1. As foon as the wool is plunged in the boiling liquid, the filaments adhere together, and a flight agitation is fufficient to effect the complete folution.

2. The lye becomes coloured, and gradually thickens, in proportion as more wool is added.

3. The foap is more or lefs coloured accordingly as the wool is lefs or more clean and white.

4. The pile or hairs which are mixed with the wool are more difficult of folution.

5. The quantity of wool the alkali is capable of diffolving, depends upon the ftrength of the lixivium, its caufticity, and the degree of heat. Two pounds three ounces and fix drams of cauftic alkali, at twelve degrees * of concentration, and at the boiling heat, diffolved ten ounces four drams of wool. The foap, when cooled, weighed one pound four ounces.

An equal quantity of alkali, at the fame degree of caufticity, heat and concentration, in which I diffolved four ounces of wool, did not acquire confiftence fufficient to anfwer feveral of the purpofes required.

An equal quantity of alkali, marking four degrees, diffolved only two ounces feven drams of wool. The foap, when cooled, weighed fourteen ounces. It was of a good confiftence.

6. In proportion as the wool is diffolved in the lixivium, the folvent power of the alkali decreafes, and at laft it takes up no more. It is at this period, namely, when the wool being agitated in the fluid is no longer diffolved, that the operation muft be terminated.

I. *The Choice and Preparation of Materials.*

The materials required to form this foap are two, alkaline matters and wool.

The alkaline fubftances may be obtained from the afhes of common culinary fires †, and the lye made by the well-known proceffes. Lime is to be flaked with a fmall quantity of water; the pafte is to be mixed with fifted wood-afhes, in the proportion of one-tenth part of quicklime compared with the weight of the afhes. The mixture is to be put into a fmall ftone trough (for wooden veffels colour the lye, and become fpeedily ufelefs); water is to be poured on to the depth of feveral inches. After a certain time, the folution may be drawn off at an aperture formed in the bottom of the veffel for that purpofe. It muft not be drawn off but at the moment previous to its ufe, and may have the ftrength from four to fifteen degrees. But, indeed, it is of little confequence what the ftrength may be, becaufe the only difference refulting from the ufe of a weak or a ftrong lye is, that the quantities of wool which are diffolved, will differ accordingly.

The potafh of commerce may be employed in the fame manner, by mixing one-third of its weight of quicklime.

As to the choice of the wool, every one knows, that in the manufactories of woollen cloths of

* By what meafure?—It is greatly to be wifhed that all meafures derived from the denfity of fluids were reduced to the common expreffion of the tables wherein water is taken as unity or 1.000. N.

† Wood being much more ufually burned in France than in England, their common afhes are what in London are obtained only from the bakers. The uninftructed workman fhould be aware that coal-afhes are unfit for this purpofe.

every kind, there are a number of operations performed, from the firſt waſhing of the material to the laſt package of the finiſhed article, which occaſion more or leſs of loſs. The water in which the wool is agitated to cleanſe it, the floor on which it is ſpread out, the warehouſe where it is depoſited, all afford waſte wool; as do the operations of beating, carding, ſpinning, weaving, fulling, napping and folding. In all theſe ſeveral manipulations we every where ſee a reſidue of wool, which, it is true, is collected with ſome care; but many of theſe operations are of ſuch a nature, that the remains of wool they afford are ſoiled and mixed with foreign matters, or elſe cut and rendered too ſhort to enter into ſome fabrics; ſo that they are moſtly thrown on the dunghill. This manufacture of ſoap affords the means of converting them all to uſe. Nothing more is required, but to collect them all in thoſe baſkets in which the wool is waſhed, and to waſh them with care, for the purpoſe of ſeparating impurities and foreign ſubſtances; after which, they are to be reſerved for this uſe.

The cuttings of all the woollen ſtuffs, afforded by the ſhops of manufacturers, dealers, taylors, and the like, may be advantageouſly collected for this purpoſe; and the ſame advantage may be derived from the remains of garments after they are worn out.

II. *Method of Making the Soap.*

When the lye and the wool are both ready, it remains only to cauſe the lye to boil in a veſſel of the common form. When it has arrived at this point, the wool is to be added by ſmall quantities at a time, and agitated to cauſe a more ſpeedy ſolution. Care muſt be taken not to add more wool, until the firſt portions are diſſolved. The operation muſt be ſtopped the moment the liquor refuſes to diſſolve more.

From the operations in the large way, made by Michel Fabriguette, with ſoaps of his own fabrication, after the method I communicated to him, it is certain that this ſoap cleans, felts, and ſupples the cloths perfectly well. But its uſe requires a few important obſervations to be made.

1. When the ſoap is not made with the requiſite care, or when dirty or coloured wool has been employed, the fabric receives from the ſoap a grey tinge, which it is very difficult to eradicate. This tinge is of no conſequence when the ſtuff is intended to be dyed; but it would injure the beauty of that white colour which in certain goods is intended to be preſerved. The remedy conſiſts in employing the moſt ſelect materials to form the ſoap intended for ſuch delicate applications.

2. Stuffs fulled with this ſoap contract an animal odour, which, though not very ſtrong, is nevertheleſs diſagreeable; but water and the air completely remove it.

After having ſucceeded in the employ of this ſoap in fulling cloths made of wool, I attempted to ſubſtitute ſoda for potaſh, and to form, according to the proceſs here deſcribed, a ſolid ſoap, proper for the operations of dyeing cottons. My experiments have ſucceeded beyond my hopes.

Forty-ſix pounds of ſoda at eight degrees diſſolved at the temperature of ebullition five pounds of wool *, and afforded, by cooling, ſixteen pounds fourteen ounces of ſoap ſufficiently ſolid not to be ſpread (couler).

The

* It is affirmed, that when common ſea ſalt is thrown into the combination of oil and vegetable alkali in the proceſs of ſoap-making, the effect conſiſts not merely in the ſeparation of the ſoap from the water, now rendered ſalt, but that the alkalis change place; ſo that the ſoap obtains the mineral alkali, and the fluid, inſtead of containing

The firft wool which is thrown into the foda diffolves readily; but it is afterwards feen that the fluid gradually becomes thicker, and that the diffolution becomes more difficult and flow.

The firft folutions render the liquor green; after which it becomes black, and the foap, when cooled, preferves a blackifh green colour.

This foap has been employed in every manner, and under every form, in my manufactory for dyeing cottons; and I am at prefent convinced that it may be fubftituted, inftead of the faponaceous liquid we make from the lixivium of foda and oil, to prepare (apprêter) the cottons. I have conftantly obferved, that by diffolving a fufficient quantity of this foap in cold water to render the fluid milky, and by working (foulant) the cotton with the apparatus which is well known, it is fufficient to pafs the cotton three times through, drying it each time, in order that it may be as well difpofed to receive the dye as that which has been paffed feven times through the ordinary folution of foap. This will not appear furprifing when it is confidered that animal matters are very proper to difpofe thread and cotton to receive the dye, and that fome of the operations of our dye-works confift fimply in impregnating them with thefe fubftances.

It is to be obferved, that cotton which has paffed through a folution of this foap acquires a grey tinge nearly fimilar to what it gains by aluming, while the common foap liquors give it the moft beautiful white colour. But this grey colour is not at all prejudicial to the dyeing proceffes, as we have remarked in fpeaking of woollens.

I muft remark, in confirmation of this laft ufe, which I attribute to the foap of wool, that after having impregnated cotton with it by the ordinary procefs, I caufed it to pafs through all the operations to which wool is fubjected to produce the fcarlet dye. The cotton acquired a deep and very agreeable flefh colour; whereas the cotton which had not received this preparation came out of the bath with its natural colour. This firft effay promifes advantageous refults, which I mean to purfue.

It may be of fome utility to obferve, that the foap of wool may be beneficially fubftituted inftead of common foap. In domeftic operations I have profitably applied it to wafh linen, and particularly woollen garments and other articles. I have no doubt but the facility and economy which it prefents in its fabrication * will ferve to extend its ufe ftill further; but in the mean time I have thought it proper to fhew the various objects to which I have applied it.

Obfervation.

As the foap of wool gives a grey tinge to piece-goods, which it is difficult to eradicate, it follows, that it cannot be ufed for bleaching linen, unlefs it be made of white wool felected and carefully wafhed.

ing common falt, will be found to contain the combination of marine acid and vegetable alkali. I do not know if this has been fhown to be really the cafe, nor whether this indirect procefs be of much value. If it be fully as here ftated (which I doubt), our foaps muft owe their inferiority to thofe of Spain to the animal oil they contain, and not to their alkali. N.

* The effect of the Excife Laws in Britain confines the manufacture of foap to premifes regiftered in form, and regularly vifited. What the general effects of this arrangement may prove on our great national manufacture of woollens, more efpecially if the prefent invention fhould amply come up to the expectations here excited, is a queftion that well deferves to be inveftigated. N.

XI.

Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas, obtained from Ether and Alcohol by different Processes, forwarded to the National Institute of France by the Society of Dutch Chemists; being Part of a Report read to the First Class of the Institute, by Citizen FOURCROY, at the Sitting of the 26th Frimaire, 16th December 1796.*

CITIZENS Bondt, Deiman, Van Trooftwyk and Lauwerenberg, chemifts, of Amfterdam, who for feveral years paft have made experiments together, and have already rendered great fervices to the fciences, fent on the firft Fructidor of the fourth year to the Inftitute, a Memoir addreffed to that body by C. Van Mons of Bruffels, its affociate. As the fubject it treats of relates intimately to the progrefs and ftate of modern chemiftry, it becomes neceffary to give an account as ample as the novelty and importance of the fubject demand.

Several months before the arrival of this Memoir, the Inftitute had received an account of the difcovery it announces. The contents of the letters of C. Van Mons, on this difcovery made in Holland, forwarded to the Inftitute in Ventofe the fourth year (March 1796), were as follows:

The olefiant gas, which is fo called from its characteriftic property of forming oil in a circumftance which fhall be defcribed, is formed of a mixture of feventy-five parts of concentrated fulphuric acid with twenty-five parts of alcohol, even without the affiftance of foreign heat. It is likewife formed by paffing alcohol or ether in vapour over filex or alumine in a tube of glafs, or fimply in a tube of pipe earth ignited without addition. It is not formed by the paffage of the alcoholic or ethereal vapour in a tube of glafs ignited without filex or alumine, nor in the fame tube containing lime or magnefia. The inflammable gas, which is obtained in this laft cafe, is no longer fufceptible of becoming olefiant, by a fecond tranfition through filex or alumine. The olefiant gas is not abforbed nor altered by remaining over water; with a fmall quantity of oxigenated muriatic acid gas it forms an ethereal oil. Mixed with this gas in equal proportions, and fet on fire, it lets fall a great quantity of carbone; when 0.25, or 0.20, or 0.15 of oxigenated muriatic acid gas is added to 0.75, or 0.80, or 0.85, of olefiant gas, and the mixture fet on fire, the carbone appears immediately in the form of very fine lamp-black. The greater the proportion of the olefiant gas, the more perceptible is the appearance of carbone during the inflammation. Too large a portion of oxigenated muriatic acid converts it into carbonic acid. This experiment proves that the hydrogene attracts oxigene more ftrongly than the carbone does. C. Van Mons thinks, in his letters on this fubject, that the olefiant gas is a true carbonated hydrogenous gas.

Five months after this firft account, on the firft of Fructidor, in the fourth year of the Republic of France (18th Auguft 1796), C. Van Mons forwarded to the Inftitute the Memoir of Citizens Bondt, Deiman, Van Trooftwyk and Lauwerenberg, in which thefe chemifts have with great care defcribed the properties of this gas, which they had difcovered, and which was already known by the name of the olefiant gas. This Memoir, which is very well drawn up, confifts of twenty-four paragraphs; the fubftance of which we fhall here relate.

In the firft paragraph the authors obferve, that the gas which is difengaged during the mutual action of concentrated fulphuric acid and alcohol, which was known to occafion frequent ruptures of the veffels, to burn with an oily flame, which had caufed it to be preferred in

* Annales de Chimie, XXI. 48.

lamps

lamps ſupported by inflammable air, appeared to them deſerving of a particular examination in conſequence of the curious properties it preſented, particularly when compared with the gaſſes afforded by alcohol and ether treated by other methods.

Secondly, From having remarked that the gas is diſengaged towards the end of the proceſs for making ether, they took the proportions of the mixture which exiſts at this period of the etherification; that is to ſay, four parts of concentrated ſulphuric acid and one of alcohol, and treated this mixture in a common glaſs body appropriated to the production of aeriform fluids.

The third paragraph deſcribes the ſeries of phenomena which take place during the production of this gas. The mixture heats and becomes brown; gas is extricated without the application of external heat; but when ſuch heat is applied, the efferveſcence greatly increaſes, the colour of the mixture becomes black, the gas paſſes abundantly; it is even neceſſary, to prevent the whole of the liquor from quitting the veſſel, that the heat ſhould be withdrawn. The reſidue, after the extraction of the gas, conſiſts of ſulphureous acid mixed with coal, which renders it black. Citizen Fourcroy remarks, that the deſcription makes no mention of ether being produced.

[*To be concluded in the next Number.*]

MATHEMATICAL CORRESPONDENCE.

AS the preſent Journal is deſigned to embrace every branch of uſeful knowledge, both in ſcience and the arts, a certain portion of it will be devoted to the inſertion of ſuch mathematical diſſertations and queſtions, as by their novelty or importance appear to deſerve the attention of the public. Every communication, therefore, of this kind, directed to the Editor, poſt paid, will be thankfully received, and publiſhed as early, in ſome of the ſucceeding numbers, as the plan of the work will admit; but it is particularly requeſted that the contributors of queſtions will direct them, as much as poſſible, to practical or theoretical improvement; as none that are forced, or framed upon obſcure enigmatical principles, which when diſcovered are of no value, will be inſerted.

The following QUESTIONS are propoſed for Solution.

QUESTION I. *By J. B.*

IT is required to divide the half of a given right line into a given number of parts, ſo that each part, and the ſum of that part and the remainder of the whole line may be in geometrical progreſſion; this being a queſtion of practical utility in the diviſion of the monochord or muſical ſtring.

QUESTION II. *By Capt. W. MUDGE.*

IT is required to determine the centrifugal force of a body moving in the circumference of a circle, by the pure principles of fluxions, inſtead of deriving it from the doctrine of indiviſibles, as is done by Newton in the Principia.

SCIEN-

SCIENTIFIC NEWS.

THE Public will hear with much pleasure, that the Journal des Sçavans and the Annales de Chimie are both revived. The former work, as usual, is anonymous, though the papers are marked by initials or characters. The latter presents the respectable names of its authors, Guyton, Monge, Berthollet, Fourcroy, Adet, Seguin, Vauquelin, Pelletier, C. A. Prieur, Chaptal, and Van Mons.

Of the Journal des Sçavans, four numbers have reached England, commencing 16 Nivose, an. 5 (Jan. 5, 1797), continued on the 30th of the same trimestre, and afterwards published on the same days of each trimestre respectively. The last, 30 Pluviose, therefore answers to Feb. 18. Regular extracts will in future be given in our Journal.

The first number contains an introduction and a general sketch of the state of letters, sciences, and arts in Europe, at the commencement of the fifth year of the French Republic (23 Sept. 1796). The latter is drawn up with the hand of a master.

Scientific matters contained in the second number are: 1. The public sitting of the National Institute of the 15th Nivose. Citizen Prony announced three astronomical memoirs of Citizen Flaugergue, associate; and the continuation of the great work of Citizens Delambre and Mechain on the Meridian from Bayonne to Dunkirk, which will be finished in the year 6. Citizen Lacepede announced the following memoirs:—On the sulphureous acid, by C. Fourcroy and Vauquelin—Various works of C. Lamarque on the general principles of Chemistry, from which a chromometric scale is obtained—Defence of the new chemical theory against a German, by C. Van Mons, associate—On the substance of gold, by C. Chaptal, associate—On vegetable juices, by the same—On the gluten of wheat, by C. Texier—On the teeth of animals, particularly the horse, by C. Tenon—On the nature and causes of vertigo in horses, by C. Huzard—Researches on the epidemical disorders of cattle, by the same—An elementary table of the history of animals, by C. Cuvier—On the use of mercury in the small-pox, by C. Desessarts.

The travels continued with success by various members of the Institute, afforded Citizen Lacepede an opportunity for a well-timed digression in form of an invocation to Peace, which was greatly applauded.

The Citizen Talleyrand, one of the secretaries of the second class, gave an account of its labours. They consist of two memoirs on Ideology, by C. Tracy, associate—Two memoirs of C. Laromiguiere, associate; one on the operations of the human understanding; the other on the signification of the word Idea—On the conversion of the territorial impost into a duty on successions, by C. Duvillard, associate—On public credit, by C. Dyanniere, associate—Inquiries respecting the Arabian Gulf, by C. Gosselin—Concerning the manners of the Greeks in the time of Homer, by C. Levesque—Three historical memoirs; one respecting the Egyptians, another on the Swiss, and the third concerning Peru, by C. Anquetil—Notice respecting Sylvain Bailly, by C. Delisle de Sales.

Citizen Seguin, associate, read a memoir on the tanning of skins. His process, of which some account has already been given*, is said to produce the same effect in one month, as by the ancient process required fifteen or eighteen.

* Page 26 of this Journal.

Citizen

Citizen Desfontaines read a memoir on the cultivation of ſpices in Guiana. Three hundred exotic trees were carried thither by C. Martin; and it appeared that the clove, among others, afforded a very conſiderable product.

Citizen Dupont de Namours read a memoir on the ſociability and morality of the dog, the fox, and the wolf. The title excited the public attention; and the effects of the diſcourſe, which was original, philoſophical, and lively, was ſuch that it gained much applauſe.

ACCOUNTS OF FOREIGN PUBLICATIONS.

Beſchreibung einer Reiſe, &c. or Travels in Germany and Switzerland in the Year 1781, with Obſervations on the Sciences, Induſtry, Religion and Manners. By Frederick Nicolai. Vol. XI. and XII. Berlin and Stettin, 1796, in octavo.

Nicolai publiſhed the firſt volumes of his Travels in 1783, at that time offered for ſubſcription, and he has ſince brought them out two volumes at a time. The 9th and 10th appeared in 1795. This work is greatly eſteemed in Germany. The title of Travels, though the author really paſſed through the countries he deſcribes, is, properly ſpeaking, no more than a vehicle for numerous obſervations on manners, population, induſtry, and ſcience. Particular diſſertations on various objects are inſerted. Thus the xith volume contains diſſertations on the Celts, the Suabians, the Celtic language, &c.; and the xiith, obſervations of much importance on certain geographical denominations, which Pliny, Ptolomy, and others, give to places ſituated at the Black Foreſt and in Helvetia. It muſt not be concluded that this account contains little novelty becauſe the Travels publiſhed by the author were made in 1781. To the recital of his actual travels Nicolai has added a work of the cloſet, perhaps much too extended, which conſiſts in a numerous collection of notes and tables relative to ſtatiſtics. Some are dated in the year 1796.

The works of Nicolai are more particularly eſteemed in Germany, in reſpect to the objects of political œconomy they contain.

Ueber die Kultur-verhältniſſe, &c. or An Eſſay on the comparative Culture of Land in the various States of Europe; in which an Attempt is made to determine, by the Magnitude and Population of the ſeveral States, the Degree of Cultivation of the European Dominions. With ſixteen large Tables, exhibiting the Surface and Population of the European States. By Aug. Fred. Guil. Crome. Leipſick, 1792; 1 vol. octavo; 398 pages of text, and 112 of additions.

Statiſtiche Ueberſicht, &c. A Statiſtical View of the Provinces of the Ruſſian Empire, with regard to the comparative Cultivation of the Land. By H. Storch. Riga, 1795; Hartknock; 1 vol. ſmall folio, 131 pages.

Geographiſch-Statiſtiche Tabellen; Tables of the Statiſtical Geography of Switzerland. By H. K. Zurich, 1795; Ziegler and Sons; 1 vol. ſmall folio; ſeven tables.

Annalen der Staatskräfte von Europa. Annals of the Powers of the ſeveral States of Europe conſidered, with regard to the preſent State of Natural Philoſophy, Commerce, Science, and Political Relation. The whole drawn up in the Form of Tables. By Ad. Frid. Randel. Berlin, 1792; Frid. Vieweg; 1 vol. ſmall folio.

Der Polynomiſche Lehrſatz, &c. Principles of the Doctrine of Polynomials, the moſt intereſting Problem of Analytical Science, with ſome other Theorems relating thereto, explained and developed, by Tetens, Kluegel, Kramp, Pfaff, and Hindenburg. The latter has added an abridged Summary of the Method of Combinations, and their Use in analytical Proceſſes. Leipſick, Fleiſcher the younger, 1796, 8vo.

Ueber die Methode des Hrn. La Grange, &c. A Diſſertation on the Method of Mr. La Grange, for reſolving all Equations by means of Series. By F. W. A. Murhard.—Gottingen, Roſenburg, 1796, 4to.—16 pages.

Die neueſten Entdeckungen, &c. The new Diſcoveries in Electricity conſidered with regard to Natural Philoſophy and Medicine, collected by Charles Gottlieb Kuenh. Part I. Leipſick, 1796, 8vo.

Dr. Lorenz Deerell has tranſlated into German the firſt volume which has appeared of Kirwan's improved edition of Mineralogy, with Obſervations. It is printed for Nicolai at Berlin.

Handbuch der neueſten Erdbeſchreibung: or, A Manual of the moſt modern Geography. By Gaſparis, Profeſſor at Jena. Vol. I. 8vo. Weimar, December 1796.

Acta Elect. Mogunt. Scientiarum utilium, quæ Erfurti eſt, ad Annum 1794 & 1795, cum Figuris in 4to maj. Erfort, G. A. Reyſer.

Traité d'Harmonie & de Modulation. Par H. F. M. Langlé. A Treatiſe of Harmony and Modulation. By H. F. M. Langlé, ſome time Firſt Maſter of the Conſervatory of Piety at Naples, and Profeſſor at the Conſervatory at Paris.—This treatiſe is divided into two parts. The firſt exhibits all the practicable concords in harmony, and the ſecond all the poſſible modulations. "The authors have treated this ſecond part," ſay the Journaliſts, "in ſuch a manner as to facilitate to beginners on the piano-forte and the harp the difficult accompliſhment of preludes." Boyer, Paris.

Two Tranſlations of Spalanzani's Travels in the Two Sicilies have appeared; the one at Berne, by Senebier; and the other at Paris by Toſcan and Duval, with Notes by Faujas. Neither of theſe can yet be complete, as Spalanzani's Travels are to form ſix volumes in 8vo.

The Encyclopédie Methodique par ordre de Matiéres, which was interrupted in its progreſs by the French Revolution, is again continued. The ſixtieth and ſixty-firſt livraiſons are publiſhed for Agaſſe, Rue des Poitevins, No. 18. The ſixtieth livraiſon contains three half-volumes:

1. The ſeventeenth Part of the Plates of Natural Hiſtory, forming the ſeventh Century of Botany; by Citizen Lamarck, of the National Inſtitute, Profeſſor and Adminiſtrator of the Muſeum of Natural Hiſtory.

2. The ſecond Part of the third and laſt Volume of Ancient Geography; by Citizen Mentelle, of the National Inſtitute.

3. The Dictionary of Fiſhery; by Citizen Lacombe, Author of ſeveral Dictionaries of the Encyclopédie, particularly that of Arts and Trades. The price of this is thirty-one livres in ſheets, or thirty-three ſtitched. A table is given of the parts already publiſhed, by which it is ſeen that the work is already far advanced, and that there is no probability of its not being completed.

The price of the ſixty-firſt livraiſon is the ſame as the foregoing. It contains the eighteenth part of the plates of Natural Hiſtory, conſiſting of one hundred plates of Inſects.

Vol. IV. Part 1, Dictionary of Botany, by Lamarck.

Vol. VII. Part 2, of the Hiſtory of Inſects.

[*Accounts of Engliſh Publications in the next Number.*]

4

Dr. Blair's Improvement of compound object Lenses.

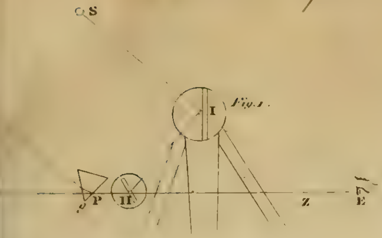


Fig. 1.

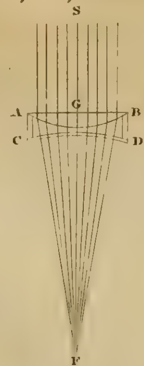


Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.


Fig. 7.

Fig. 8.

Electrical Instrument. p. 16.

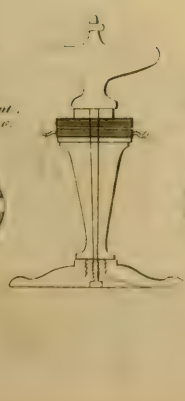


Fig. 9.


Fig. 10.

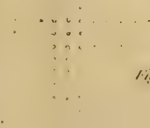


Fig. 11.

Uncommon Solar Spectrum. p. 13.


Fig. 12.

Mr. BRAMAH'S PRESS, OPERATING BY THE ACTION OF WATER.

Fig. 1

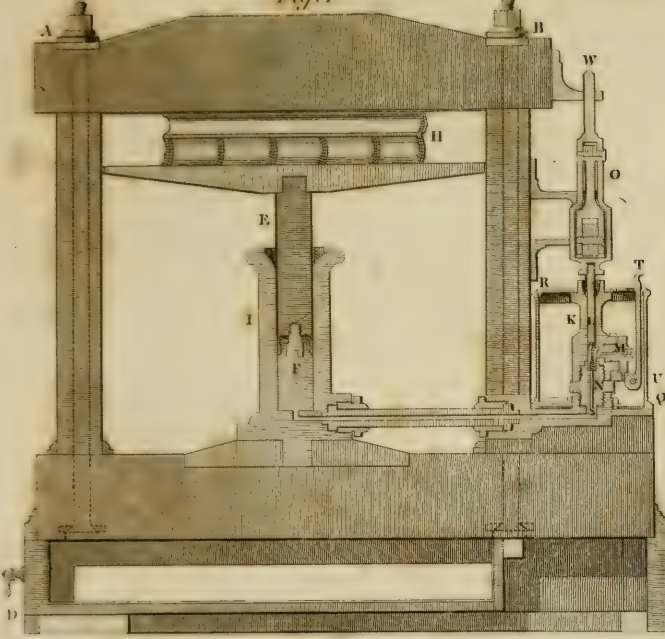
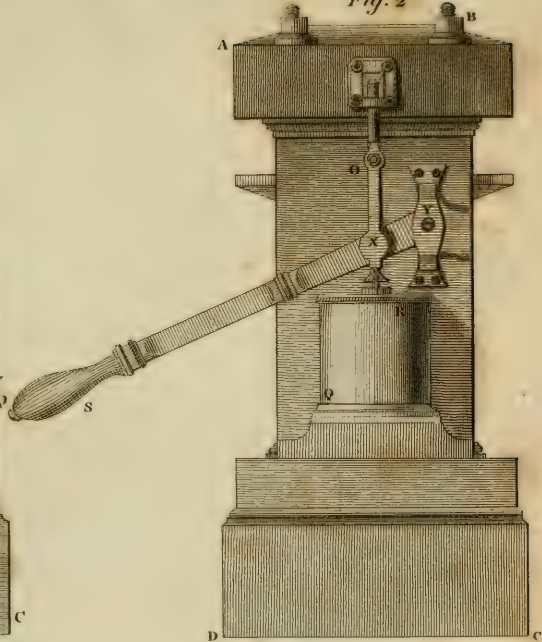


Fig. 2



Scale of Inches.

0 1 2 3 4 5 6 7 8 9 10 11 12

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY 1797.

ARTICLE I.

Extract of a Memoir concerning three different Species of Carbonated Hydrogenous Gas, obtained from Ether and Alcohol by different Processes, forwarded to the National Institute of France by the Society of Dutch Chemists; being Part of a Report read to the First Class of the Institute, by Citizen FOURCROY, *at the Sitting of the 26th Frimaire, 16th December* 1796.

[Concluded from the last Number.]

THE fourth paragraph treats of the choice and purification of the gas. At the commencement and towards the end of the operation, it is mixed with fulphureous acid gas; towards the middle it is purer, and contains only one fixth of fulphureous acid; when washed with water and ammoniac, it is rendered very pure. It does not contain carbonic acid gas.

The fifth paragraph exhibits the phyfical properties of this gas. Its weight compared with that of common air, is as 909 to 1000; its odour is fetid, when well purged from ether and fulphureous acid. It burns with a strong compact flame similar to that of a refinous oil.

The fixth paragraph enumerates feveral properties in fome meafure negative and characteriftic of this gas. When left for feveral months over water, it fuffers no alteration. The fulphuric, fulphureous, nitric and muriatic acids do not act upon it; neither does the nitrous gas, nor alkalis. Ammoniac in the elaftic ftate adds merely to its volume, without occafioning any change. Phofphorus heated even to fufion did not affect it.

In the feventh paragraph the authors defcribe the action of oxigenated muriatic acid gas upon their newly difcovered gas. As this is the only fubftance which acts in any remarkable manner upon it, their defcription is very accurate and full. They announce with reafon this effect as no lefs curious than new and hitherto unknown. They at firft applied the oxigenated muriatic acid, with the intention of proving the prefence of carbone in this gas; becaufe

 the

the proceſs had before ſucceeded in ſimilar caſes. Having mixed in a tube over water equal parts of their inflammable gas and oxigenated muriatic gas, an abſorption took place which was more rapid than what obtains between the latter gas and water; a thick oil of a pearl-grey colour more ponderous than water was depoſited. The tube was filled with a white vapour; much caloric was diſengaged, and there remained one eighth part of the gas made uſe of, which was ſtill inflammable. A ſecond mixture of four parts of oxigenated muriatic acid gas, and one part of the inflammable gas produced by alcohol and the ſulphuric acid, and kept over water for eight days, preſented the ſame phænomena as the foregoing. After the ſeparation of the oxigenated muriatic acid gas, there remained only $\frac{1}{20}$th part, which was azotic gas afforded by the oxide of mangaueſe.

The eighth paragraph is employed in deſcribing the properties of the oil obtained in the foregoing experiment. When collected in an apparatus which the authors do not deſcribe, it preſented the following characters: Its ſemi-tranſparence reſembled the colour of pearls; it ſunk in water, and became yellow by expoſure to air; its ſmell was agreeable and penetrating, its taſte rather ſweet; both theſe properties were very different from that of ether. It was diſſoluble in water, which acquired its odour; liquid vegetable alkali rendered it ſweeter, by depriving it of the ſmell of oxigenated muriatic acid.

In their enquiries, in the ninth paragraph, concerning the compoſition of this gas from the known properties of ſulphuric acid and alcohol, of which it is formed, the Dutch chemiſts aſſume in the firſt place as a principle that it can contain no other ingredient than hydrogene, carbone, and oxigene. This laſt ſubſtance does not appear to them to be preſent, becauſe it ought either to form water or carbonic acid, neither of which compounds appears*. The gas is not ether diſſolved in ſulphureous acid gas; becauſe after the combuſtion with vital air by means of electricity, as well as after its converſion into oil by the oxigenated muriatic acid, the muriate of barytes does not indicate ſulphuric acid, which ought in that caſe to be formed. Neither does ſulphur enter into its compoſition; for it does not exhibit the ſmell of ſulphurated hydrogenous gas, nor does it throw down ſulphur in proportion as the gas is burned. By this method of excluſion, the learned chemiſts of Amſterdam conclude that their inflammable gas can be compounded of nothing but hydrogene and carbone.

The tenth paragraph is employed in proving the exiſtence of hydrogene in this gas. Though the formation of water and of oil by the oxigenated muriatic acid ſufficiently declares this; yet, as the preſence of the water over which the experiment was made might leave ſome doubt, the Dutch chemiſts had recourſe to other proofs. By paſſing their gas through a tube filled with ſulphur in fuſion, they obtained ſulphurated hydrogenous gas, and the ſulphur was blackened.

In the eleventh paragraph they prove the preſence of carbone in their gas, not only by the black colour of the ſulphur indicated in the foregoing experiments, but by the formation of carbonic acid, which takes place whether the gas be burned with vital air, by means of the electric ſpark, or by paſſing it through an ignited tube filled with the oxide of manganeſe.

* To theſe two conditions may be added another, which the learned authors have not directly conſidered, namely, that it may form a triple compound. This, in fact, may happen in the oil; for which vide infra. N.

A later

A later experiment, which likewise proves it, consists in mixing the gas with oxigenated muriatic acid gas, and burning it before their reaction has presented the oil before mentioned. In this experiment the glass vessel is covered with coal resembling lamp-black.

The twelfth paragraph announces the name the authors have chosen for their gas, in consequence of its nature and properties: it is *carbonated oily hydrogenous gas*.

In the thirteenth, they speak of the elastic fluid disengaged from ether treated with the sulphuric acid. Only three-fourths of this gas is converted into oil by the oxigenated muriatic acid; the residue burns blue, and is no longer reducible into oil by this acid. Alcohol and ether passing through a tube of pipe-clay, when ignited, afford a gas of this nature.

The fourteenth paragraph announces the formation of an inflammable gas different from the last mentioned, or from the olefiant gas, by passing ether and alcohol through an ignited glass tube. This gas does not afford oil by the oxigenated muriatic acid gas. In order to determine whether the porosity of the tube of clay was the cause of the formation of the oily gas, by suffering some principles to pass through, or by admitting others, these philosophers included a tube of clay in another of glass, and then passed alcohol and ether, after having previously ignited the tubes. There was a production of olefiant gas in this case, as well as when the glass tube contained fragments of pipe-clay. Hence they conclude that the clay contributes to the formation of this gas.

Similar experiments form the object of the three following paragraphs, which Mr. Fourcroy abridges, by merely prefixing the number without any prefatory connection.

15. A tube of glass charged with alumine and ignited, through which they passed alcohol, afforded an oily gas, the residue of which, after the formation of the oil, burned with a blue flame: the same thing happened when silex was used instead of alumine. When the tube of glass was filled with lime, or magnesia, or vegetable alkali, or charcoal, or sulphate of pot-ash, and the alcohol was respectively passed through these in the ignited state, the product was a gas not capable of forming oil; the earths were blackened in these experiments. Sulphur, in a trial of the same nature, afforded sulphurated hydrogenous gas not oily.

16. Alcohol and ether must necessarily pass, according to these authors, over alumine or silex, ignited in a tube of glass, to afford the olefiant gas. The gas obtained from these two fluids by passing them through a tube of glass does not become olefiant when passed a second time over alumine or silex, or through the tube of clay. This property, therefore, when once lost, cannot be restored.

17. Carbonated oily hydrogenous gas passed through an ignited glass tube does not diminish its volume, and loses the property of forming oil with the oxigenated muriatic acid. The tube and the glass which have been used in this experiment, are blackened and covered with drops of empyreumatic oil; they have the smell of this last product; a black soot covers the water of the apparatus. Six hundred electric shocks passing through carbonated oily hydrogenous gas increased the bulk by two-fifths, and deprived it of its property of forming oil, without having precipitated carbone.

After having thus examined, in the first seventeen paragraphs, the properties of the carbonated oily hydrogenous gas, the chemists of the society of Amsterdam are employed in the five following upon two other kinds of elastic fluids obtained from ether and alcohol by different processes.

In the eighteenth paragraph they describe the process by which they have produced theſe laſt gaſes. It is performed by diſtilling ether and alcohol through a tube of ignited glaſs, the extremity of which is received under an inverted glaſs filled with water. The gas formed in this manner precipitates neither oil nor charcoal by the contact of the oxigenated muriatic acid. During its formation, part of the ether or alcohol paſſes unchanged.

The characteriſtic properties of the gas obtained from ether by this proceſs are ſhewn in the nineteenth paragraph. Its weight, compared with that of air, is as 0.709 to 1.000. The ſmell of ether, which is perceptible in the firſt portions, is ſoon ſucceeded by a fetid ſmell. It burns with a compact oily flame, like the former gas; water neither diſſolves nor alters it; it does not render lime-water turbid; and it is unalterable by acids or alkalis. The oxigenated muriatic acid reduces its volume $\frac{1}{7}$th. It burns with a blue flame, and affords no trace of oil.

The reſearches which form the object of the twentieth paragraph ſhew the preſence of, 1. Hydrogene, by the formation of ſulphurated hydrogenous gas, which is produced when it paſſes over fuſed ſulphur. 2. Carbone, by the black precipitate it affords by burning, after mixture with the oxigenated muriatic acid gas, as well as by the carbonic acid which reſults from its combination with vital air. The authors of the memoir, to diſtinguiſh this gas from the former, call it *carbonated hydrogenous gas obtained from ether.*

The gas which is afforded by alcohol diſtilled through an ignited glaſs tube, forms the ſubject of the twenty-firſt paragraph, and differs from the foregoing in two properties:—1. By its ſpecific gravity, which is to that of air as 0.436 to 1.000. 2. By its paler and leſs oily flame, which reſembles that of alcohol. This is named by the Dutch chemiſts, *carbonated hydrogenous gas obtained from alcohol.*

As the analyſis of theſe three ſpecies of gas preſented to their notice the ſame principles in their compoſition, and as they did not appear to them to differ but in the mere proportion of theſe principles, they have in their twenty-ſecond paragraph united the reſults of their experiments in this behalf. Though they were not entirely ſatisfactory, they were nevertheleſs ſufficiently clear to enable them to deduce an uſeful concluſion. In order to arrive at a knowledge of this proportion of the principles in the three kinds of gas they had to compare, they mixed each of them in tubes of glaſs cloſed at one end, or long glaſs veſſels, over mercury, with a larger quantity of oxigenous gas than was ſufficient for their complete combuſtion. Theſe mixtures were ſet on fire by the electric ſpark; the diminution was carefully determined. The proportion of carbonic acid formed was eſtimated from the quantity of precipitate thrown down from lime-water introduced over mercury. The reſult of their trials on this point was, that theſe gaſes contained from 80 to 74 parts of carbone, with from 20 to 26 of hydrogene. In general the three gaſes exhibited little difference between each. The carbonated oily hydrogenous gas preſented the largeſt proportion of carbone; the carbonated hydrogenous gas from ether, a medium proportion of this principle; and the carbonated hydrogenous gas from alcohol, the leaſt of the three.

The twenty-third paragraph of the memoir, abridged by Mr. F. contains a retroſpect of the principal facts exhibited in the whole treatiſe. This retroſpect is ſo well drawn up, and preſents a view of the whole with ſo much accuracy and condenſation, that he has preferred the method of quotation without abridgment.

"Here then," ſay the Dutch chemiſts, "are three kinds of inflammable gas obtained from alcohol and ether treated in different manners.

"Theſe

" These gases have this in common, that they are composed of hydrogene and carbone. They are species of carbonated hydrogenous gas.

" It appears probable, moreover, that the proportions of these constituent parts do not differ, or at least that they differ very little if equal weights of each were examined.

" They differ from each other in several other respects; as their specific gravity, their manner of burning, and the several methods by which they are produced.

" The most remarkable difference is certainly the formation of an oil by the mixture of oxigenated muriatic acid gas with the carbonated oily gas.

" This gas is produced in its greatest purity in the distillation of ether, or of a mixture of alcohol and concentrated sulphuric acid.

" Ether mixed with the same sulphuric acid also affords it, but less pure.

" It is also obtained by causing the vapours of alcohol and ether to pass through a tube of clay ignited by fire; but this likewise is not perfectly pure.

" This effect is equally observed by taking the component parts of the tube, namely, alumine and silex; which taken separately communicate to the gas this property of forming oil, if the vapour of the alcohol or the ether pass over these substances.

" These vapours passing through a simple tube of glass ignited, afford the two other species of gas, accordingly as either ether or alcohol is taken: and these two last gases do not present the least appearance of oil, formed by mixing them with the oxigenated muriatic acid gas.

" The gas which has the property of forming oil loses it by being made to pass through an ignited glass tube. It deposits charcoal.

" Electric shocks have the same effect, but without the precipitation of carbone. It is moreover observed, that the volume of the gas is augmented when it passes from the oily state to that which is incapable of forming oil.

" These species of gas, lastly, as well the oily as those obtained from ether or alcohol, are truly permanent gaseous fluids, and must not be regarded either as the vapours of ether preserving the aeriform state for a time, or as hydrogene gas holding particles of ether or alcohol in suspension. We have kept these gases for whole months over water; we have repeatedly passed them through that fluid; and we have exposed them to re-agents. They have always preserved their properties without alteration and without loss."

In the twenty-fourth and last paragraph, the Citizens Bondt, Deiman, Paats van Trooftwyk, and Lauwerenberg, conclude their interesting researches by various questions which their experiments have not resolved. They demand, 1. How the oil is formed by the carbonated oily hydrogenous gas, in its mixture with the oxigenated muriatic acid gas: whether it be by the addition of a portion of oxigene from the latter, or a subtraction of part of the hydrogene from the former. 2. What is the nature of this oil, of which hitherto they have not yet procured a sufficient quantity for a proper examination; but which nevertheless appears to them to be a kind of ether. 3. Why is the oily gas formed when ether and alcohol pass over alumine, silex and clay, but not over glass. 4. Whether the difference do not arise from the circumstance (say they) that the one contains a greater quantity of caloric than the other; and whether it be not thus that the first gas loses its property of affording oil in proportion as its bulk is augmented by electricity, without occasioning a precipitation of carbone.—That they cannot positively reply to these questions is ascribed by them to the state of vegetable chemistry, which is less advanced than that of the air and of minerals.

The experiments made upon the olefiant gas by the citizens Hecht and Vauquelin, in conſequence of the invitation of the Philomathic Society of Paris, throw ſome light on the queſtion of the Dutch chemiſts. The olefiant gas, paſſed through an ignited porcelain tube, afforded hydrogene gas mixed with carbonic acid; a large quantity of carbone was depoſited in the glaſs tube in which the tube of porcelain terminated. The difference remarked in this experiment and that of the Dutch chemiſts is owing probably to the high degree of heat given to the tube of porcelain *. The carbonated hydrogenous gas, deprived of carbonic acid, and afterwards mixed with the oxigenated muriatic acid gas, did not form oil as before. The olefiant gas had depoſited its carbone upon the alumine in paſſing through tubes which contained that earth. The ethereal gas burns with the oxigenated muriatic acid gas, and affords with it the ſame oil as the olefiant gas; which appears to mark a great analogy between them. They may probably differ only in the unequal quantity of combined caloric.

In general it follows, from all theſe comparative experiments, that the moſt denſe olefiant gas, containing leſs of caloric than the carbonated hydrogenous gaſes afforded by alcohol and ether paſſed through tubes of glaſs, poſſeſſes the diſpoſition to form oil only by virtue of the proximity of its conſtituent molecules; and that it loſes this property only when they become ſeparated more apart by the intromiſſion of a greater quantity of caloric between them.

It follows alſo, that the olefiant gas is converted into ſimple carbonated hydrogenous gas, whenever a proceſs is executed which increaſes the diſtance between its component parts. In this way, the attraction between the particles of the hydrogene and carbone is diminiſhed, upon ſeparating them by virtue of a greater proportion of caloric introduced between them. This happens whenever the olefiant gas is made to paſs through ignited tubes, of what nature ſoever; or by ſtrongly electrifying it. The ſame experiments prove alſo, that the difference obſerved, and ſo accurately noted, by the Dutch chemiſts, between the three kinds of carbonated hydrogenous gas, which they diſtinguiſh by the names of *oily*, *obtained from ether*, and *obtained from alcohol*, though each may be obtained indiſcriminately either from alcohol or ether, ariſes only from the methods of treating theſe inflammable fluids, and is conſtantly reducible to this; that in forming the olefiant gas a leſs quantity of caloric enters the maſs, and a compound is determined, in which the hydrogene and carbone are more concentrated, and more diſpoſed to form oil by the addition of oxigene, more condenſed itſelf than the ſtate of vital air as it exiſts in the oxigenated muriatic acid gas; whereas, by more ſtrongly heating the alcohol or the ether, by accumulating more caloric in their vapour, theſe fluids become more completely decompoſed; their elements more widely ſeparated; and their ſimultaneous attraction, which might be adapted to form oil with oxigene, is ſo far diminiſhed, that they are no longer capable of that tranſition. Hence we may conceive, how from the ſpecific gravity of 0.909, which diſtinguiſhes the olefiant gas, it becomes, at the ſame time that it loſes this property, ſo light as 0.436.

We † are not of opinion with the Dutch chemiſts, from the alumen, ſilex, and tube of clay having ſerved to form the olefiant gas, while glaſs, lime, and magneſia afforded only the carbonated hydrogenous gas, not olefiant, that the former of theſe bodies poſſeſs a

* Or perhaps to the difference between the tubes made uſe of. In the porcelain tube (like that of glaſs) the ſemi-vitrification might cover the ſilex and alumine, or change its qualities in other reſpects. N.

† Either M. Fourcroy or MM. Hecht and Vauquelin. But this department of reſearch certainly demands experiment. Is it probable that any conſiderable difference of temperature took place when clay and lime were reſpectively ignited in the glaſs tube? It would be eaſy to prove this by giving the higher temperature deciſively to the clay. N.

peculiar

peculiar tendency to produce the olefiant gas, which property is wanting in the lime and magnefia. Thefe different effects certainly arife from a lefs degree of heat having been applied in all the cafes where the olefiant gas was produced, and a greater in all thofe which prefented the fimple carbonated hydrogenous gas.

But the circumftance which is truly important for the general theory of fcience in thefe experiments, is the new light they throw upon the formation of oil; and the force they give to the notions already received and the confiderations long ago prefented in the pneumatic doctrine, refpecting the nature and compofition of vegetable oily bodies. We here fee that an oil is a compound of hydrogene, carbone, and a fmall portion of oxigene. We find that, in the mixture of the olefiant gas, or denfe carbonated hydrogene, with the oxigenated muriatic acid, an oil is immediately formed in the fame manner as when wood, or any other vegetable matter, not in itfelf oily, is heated in a retort. It appears, that the olefiant gas, or a gas very fimilar to it, is difengaged from wood, or any folid vegetable not of an oily nature, which is flowly burned. This gas, when burned in the open air, depofits its foot or carbone, which blackens the wood, and clogs the channels through which the elaftic fluid muft pafs. If wood, mucilage, &c. be more fuddenly and ftrongly heated in clofe veffels, they afford lefs oil, more coal, and more carbonic acid; and in the open air there is more flame, and lefs coal or foot depofited. It is probable that, in the firft cafe, the olefiant gas is difengaged, and in the fecond, the fimple carbonated hydrogenous gas *. We are therefore in progrefs towards the artificial compofition of oil. Nothing more feems to be required for its artificial production, than to obtain, without vegetable matter, a denfe carbonated hydrogenous gas weighing 0.909, and to mix it with the oxigenated muriatic acid gas: and we may hope to fucceed in the production of this olefiant gas with mineral matters containing much carbone, fuch as certain kinds of fteel, which when diffolved in an acid, by the decompofition of water they promote, afford a difengagement of carbonated hydrogenous gas. This laft object deferves all the attention of chemifts, becaufe it opens a new path to the knowledge of vegetables, and particularly of the formation of oil.

The ingenious experiments of the Dutch chemifts are among the few which afford new profpects to philofophers. Together with the difcoveries for which we are already indebted to them, on the decompofition and recompofition of water by electricity, on the alkaline and metallic fulphures, &c. they will poffefs a diftinguifhed rank in the pneumatic chemiftry, to the progrefs of which they have dedicated the reputation of their labours and their difcoveries.

* This excellent courfe of experiments, and the general inductions founded upon them, bring to my recollection fome obfervations I had occafion to make many years ago, when alcohol was burned in the lamp of Argand. In this experiment, the ftrata of ignited matter forming the interior and exterior flames (which have not yet been accounted for) are ftrikingly diftinct. I have fuppofed them to be referable to a general law of the combuftion of volatile matters at certain limits of temperature; as is moft eminently feen in phofphorus and fulphur, both of which have the two kinds of flame in fucceffion, and both begin their flow combuftion at very moderate temperatures. But the fact immediately referable to the prefent theory is this: When the lower aperture or air paffage of the lamp is left open, the flame of the alcohol is as ufual faint and bluеifh, refembling that of carbonated hydrogene; but if the paffage be gradually obftructed by flowly applying the palm of the hand, or any more fuitable obftacle, the flame becomes more and more luminous, like that of oil, until, in the progrefs of obftruction, the aperture is fo much clofed that the combuftion begins to decay for want of air. By a proper adjuftment of the aperture, the alcohol may be made to burn with a conftantly luminous white internal flame. In this cafe it may be fuppofed that the temperature is precifely fuch as to extricate the olefiant gas. N.

II. *On*

II.

On the Methods of obviating the Effects of Heat and Cold in Time-Pieces.

IN every mechanical inftrument for meafuring the flow of time, fome natural power is applied to produce motion; and the portions of this motion being ufually regiftered by an index, are taken to denote certain correfpondent portions of time. The fimpleft inftruments of this nature are the hour-glafs, which is ftill in ufe; and certain clocks, regulated by the flow of fand or water, upon the fame principle as the hour-glafs; of which a confiderable number are defcribed in Ozanam's Mathematical Recreations. Of thefe it is unneceffary to fay more in this place, than that the paffage of fand through an aperture is far from being uniform, from a variety of obvious caufes; and that the flow of water not only varies from circumftances capable of hydroftatical eftimation, but alfo from its fluidity, which thefe very inftruments have fhewn to be greater or lefs in proportion to its heat. The clock and watch, which confift of an affemblage of wheels acted upon by a weight or fpring, and regulated by a pendulum or balance, are fo much fuperior in their performance to every ancient inftrument applied to this purpofe, that the attention of men of fcience has long been confined exclufively to thefe inftruments.

When a larger wheel drives a fmaller, the number of revolutions performed by the latter will be more numerous in a given time than thofe of the driving wheel in the inverted proportion of their diameters, or the numbers of their teeth. Hence it may eafily be deduced, from the infinity of affumable ratios, that combinations or trains of wheels may be made fo that the flow defcent of a weight, or other equivalent agent, may caufe certain wheels in the fyftem to revolve with any determinate velocity, provided the rate or velocity of the firft mover be determined. It is an object of mechanical fkill to adjuft a train of wheels, applied to the folution of a problem of this nature, in the beft manner poffible. For the frictions will increafe the more complex the train, the lower the numbers of the teeth, and the larger the pivots. Thefe are not, however, the objects of the prefent differtation.

When a train of wheels is acted upon by a weight, this laft, inftead of defcending with the accelerated velocity produced by the force of gravitation, is continually checked by a refiftance from the train, chiefly arifing from the fum of the frictions of the parts againft each other, and in a certain degree from the refiftance of the air, which againft the quicker moving parts is confiderable. Thefe retarding caufes fpeedily increafe, until they counterbalance the whole force of gravitation; when the weight of courfe being deprived of the caufe of acceleration, defcends uniformly. Here then we obferve an uniform motion, produced fimply by the application of mechanifm, to impede the free defcent of a weight: but a mere train of wheels, when acted upon by a large weight, fuffers it very foon to run through any moderate height; and if the weight be diminifhed, the refiftances from friction and imperfection of work being far from uniform, and occurring at certain periods of the rotations, are liable to ftop the machine altogether. For this reafon it has been found expedient to increafe that part of the refiftance which arifes from [illegible] by the addition of a fly at the end of the train. The common jack for roafting [illegible] which is to be feen in every kitchen, is an engine of this kind, in which the weight defcends descends very flowly and uniformly, becaufe refifted by the friction of a fcrew in the laft arbor of the train, and a fly which terminates the whole. The late Mr. John Whitehurft, F. R. S. conftructed a clock on this principle for meafuring fhort portions of time, to the hundredth part of a fecond, in which the regulating agent was a broad-leafed fly, which acted upon the air. It was regulated in the firft place by adjufting the weight, and in the next place by altering the obliquity of the ftriking furface of the fly. It may readily be imagined, as was in fact the cafe, that its rate would vary according to the denfity of the air. Perhaps it might have been of ufe in fome experiments to afcertain that varying refiftance. Huygens, in his treatife De Horologio Ofcillatorio, defcribes a clock regulated by a fly, which derived its refiftance from the gravitating power of a ball at the end of a fufpenfion by ftrings. When the maintaining power was weak, it fwung out to a fmall diftance from a vertical axis, to the upper part of which the ftrings were faftened, and was therefore eafily carried round; but when a greater force was applied, the ball fwung out farther, and confequently afforded a greater refiftance. The ftrings were made to apply againft a curve, by virtue of which the divergencies of the ball produced ifochronal rotations.

It is probable that the firft wheel clocks were regulated by a fly; an organ, of which the inconveniencies are too great to require enumeration to fuch as are moderately fkilled in purfuits of this nature. Striking and repeating trains are ftill regulated in this way. M. Le Roy, in the Encyclopédie *, article *Balancier*, affirms, that before the admirable invention of the pendulum by Huygens, clocks were regulated by an horizontal balance, having a vertical axis, that paffed through two holes, with liberty to play up and down; and that it was fufpended by means of a ftring paffed through a hole in the axis, and faftened at both ends, fo as to form equal angles with the axis itfelf. Confequently, when the balance revolved in one direction, the ftring was wound upon the verge, and, being thus fhortened, raifed it up until the weight of the balance had overcome the force of rotation: after which it revolved the contrary way, and defcended to perform a fimilar afcent by winding the ftring the oppofite way. He does not tell us the nature of the connection between this balance and the wheels which were regulated by it.

The application of the pendulum to clock-work conftituted fo important an improvement in engines for meafuring time, that it will probably be very long before our cotemporaries, or even their defcendants, will be able to bring the balance in competition with it. In this organ, the regulating force is fo fteady, and the lofs from the refiftance of the air fo fmall, that the imperfections of the train through which the force of the firft mover muft be conveyed, are capable of producing no more than a fmall part of that irregularity which neceffarily follows when the actual regulator derives moft of its effect from the train itfelf.

From the contemplation of this moft valuable meafure of time, and the fources of its regularity, the artifts of modern times have endeavoured to place it in a fituation which fhall as nearly as poffible refemble that of a pendulum detached from a train of wheels, and vibrating in vacuo without friction or refiftance. This fimilarity is aimed at by a fkilful difpofition of that part of the engine which connects the pendulum with the train, and is called the efcapement, for which there are many contrivances well entitled to the attention of artifts and men of fcience. Another moft effential particular is, that the organ for meafuring time, whether it be a balance or a pendulum, fhould invariably preferve its

* Geneva edition, 1778, vol. iv

dimensions with regard to the centre of oscillation. But heat and cold alter the dimensions of all the substances we know, though not equally in each. The remedy is derived from this inequality, and constitutes the subject of the present memoir.

Since the dimensions of all bodies increase with their temperature, it naturally follows that the pendulums of clocks are longer in hot weather than in cold; and will therefore make their vibrations in longer times. As far, therefore, as this cause of irregularity extends, every time-piece, regulated by a pendulum or balance, will go slower in summer and faster in winter. In the pendulum for seconds, the quantity required to be added to, or subtracted from, the length, to cause a gain or loss of one minute per day, is 0.0545 inch at a medium, which does not differ from either extreme more than the half of unity in the last figure. Hence the mean variation of length answering to one second is 0.000908 inch. And by General Roy's experiments, in the seventy-fifth volume of the Philosophical Transactions, the expansions of five feet of the undermentioned substances, by a variation of temperature of 180 degrees of Fahrenheit, namely from 32° to 212° are,

	Inch.
Rod of English plate brass — — —	0.113568
Rod of steel — — — —	0.068684
Prism of cast-iron — — — —	0.066563
Glass tube — — — —	0.046569
Solid glass rod made long before — — —	0.048472

Hence it is seen that, in a clock with a simple pendulum, having the rod of steel, every difference of temperature of four degrees will cause a variation of one second per day.

That the variation with a brass rod is nearly twice as much.

And with a glass rod, not much more than half the quantity.

The difference between the mean heights of the thermometer within, at the Royal Society's apartments, in summer and winter, is about 25°. Hence the difference of rate between the going of a clock with a simple pendulum in summer and in winter, will be about six seconds per day, or one minute in ten days.

The difference of length in straight grained deal wood is found to be very small; whence it is a general opinion, that in clocks of the common constructions, in which the pendulum is without intermission connected with the train, and the parts of the escapement are oiled, the errors from expansion in such a pendulum are much less than those which arise from the unequal transmission of the maintaining power.

But the best method, in practice as well as in theory, for correcting the effects of heat and cold in pendulums, consists in opposing the expansions and contractions of different kinds of metal to each other. The celebrated George Graham was, as far as I am informed, the first who applied this expedient to clocks. His account is in some of the earlier volumes of the Philosophical Transactions, which I have not at hand, and therefore make reference to it from memory. Having ascertained that the expansion of mercury greatly exceeds that of iron, he formed a pendulum consisting of an iron tube filled with mercury to a certain height. In this compound pendulum it is clear that, if the whole had been iron, the centre of oscillation would have descended by heat; and it is equally clear that, if the iron tube could be deprived of all expansibility whatever, the mercury supported beneath would be lengthened upwards by increase of temperature; and

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in this aſſumed caſe, the centre of oſcillation would aſcend by heat. But, as the expanſion of mercury is about fifteen times greater than that of iron, if the tube had been nearly filled, the aſcent of the mercurial column would be only one-fifteenth part leſs than if the iron had not expanded at all. When the mercurial column is ſhortened by pouring out ſome of the fluid, the effect of its expanſion will alſo be diminiſhed. There is, conſequently, a ſtate in which theſe two contrary expanſions ſhall counteract each other, and the centre of oſcillation continue invariable. When a clock thus fitted up gains by heat, the quantity of mercury muſt be diminiſhed; and the contrary if it loſes. I have ſeen an improvement of this pendulum by Mr. Troughton of Fleet-ſtreet, in which a bulb and tube of glaſs, reſembling the common thermometer, was ſubſtituted inſtead of the ſimple tube of Graham. The variable ſurface of the mercury was brought into that part of the rod where its changes of elevation were beſt calculated to counteract the oppoſite expanſion of the whole apparatus.

John Harriſon is admitted to be the firſt who applied the oppoſite expanſions of braſs and ſteel to correct each other, in the apparatus called the gridiron pendulum *. Since the increaſe by expanſion in braſs and ſteel is as 113 to 68, and the expanſion in any piece of metal is proportionate to its length, it will follow that theſe increments will be equal in two rods of theſe metals reſpectively, when their lengths are inverſely as thoſe numbers. That is to ſay, if a rod of braſs 68 inches long be laid upon a rod of ſteel 113 inches long, ſo that one end of each may coincide, and theſe ends be faſtened together; then, if the oppoſite end of the ſteel be fixed to ſome invariable ſupport, the unconfined extremity of the braſs rod will alſo keep its poſition invariably in all temperatures. For the expanſion of the ſteel will in every caſe carry the whole bar of braſs as much in one direction as the expanſion of this laſt bar would have carried its inner extremity in the oppoſite direction. We may imagine the ball of a pendulum to be fixed at this invariable point, and the centre of ſuſpenſion to be at the ſingle end of the ſteel bar.

But it is inconvenient to have nearly half the pendulum rod beneath the ball; for which reaſon Mr. Harriſon cut the ſteel bar into three parts, and the braſs into two. The firſt ſteel bar hung downwards from the centre of ſuſpenſion. To its lower extremity was attached the firſt braſs bar leading upwards. To the upper extremity of this was attached the ſecond ſteel bar, which in like manner ſupported by its lower extremity the ſecond braſs bar leading upwards. A double combination, of the nature of the ſimple pair of bars firſt deſcribed, was thus formed, in which the predominant expanſion was upwards, becauſe the braſs and ſteel were nearly equal in length. But the addition of the third ſteel bar leading downwards from the upper end of the ſecond braſs bar, anſwered the purpoſe of making the deſired compenſation. To its lower extremity then the ball of the pendulum was attached.

Pendulums of this conſtruction have been made, though it is certain that ſuch a combination of bars, inſtead of remaining parallel, would tend, by their own weight and that of the ball, to open like the legs of a pair of compaſſes, and that the remedy of bracing to keep them together would certainly impede their regular action. Mr. Harriſon had recourſe to another expedient. Inſtead of a ſingle ſteel bar, in the firſt inſtance, he made uſe of

* Elements of Clock and Watch Work, by Alexander Cumming, London, 1766, page 92.

two, connected at some distance asunder by a cross piece above and below, at their extremities, forming a parallelogram. Within and nearly contiguous to these were placed two brass bars, pinned to the lower cross piece, and proceeding upwards to another cross piece which connected their superior extremities. From this last piece hung a second pair of steel bars, within and nearly contiguous to those of brass. This last pair was also connected below by a separate cross piece, like the first, from which a second pair of brass bars proceeded upwards, as before, and were connected at top, like the former pair of the same metal. And lastly, from the upper connecting piece of these brass bars descended one single bar of steel, which passed clear through the lower framing, and served to suspend the lens or ball. In this ingenious contrivance it is evident that the bars of the same metal, which were used in pairs, performed the office of single rods, excepting that they mutually supported each other, so as to need no bracing. It is likewise clear that, since the pairs of brass and steel bars compared with each other are nearly equal in length, the repetition must be more frequent, in order to counteract the expansion of the single steel bar in the middle, in proportion as the difference of their expansions is less. Thus Harrison and the earlier artists cut the steel bar into three parts, and the brass into two, which, with the doubling of the three outer bars, formed a gridiron of seven bars. But at present, by substituting a mixture of zinc and silver in the place of the brass, the expansion of the compound rod is found to be so great, that two pieces of steel and one of the zinc and silver may be conveniently used, which contrivance affords a compound bar of five pieces only. This is the form generally used.

The gridiron pendulum deservedly possesses a higher degree of reputation with astronomers than any other contrivance upon the same principle. But a considerable number of pendulums has been made upon another construction, known by the name of its inventor, Mr. John Ellicott. This artist formed his pendulum-rod of iron; but instead of attaching the ball to this rod, he caused it to rest upon the outer arms of two short levers, moveable upon centre-pins fixed in the iron bar. The inner ends of these levers were acted upon by the lower extremity of a bar of brass of the same figure as the pendulum-rod against which it was applied, and to which its upper extremity was attached. From this arrangement it followed that, upon an increase of temperature, the brass bar, being lengthened more than the iron, would cause the inner tails of the levers to descend more than the fulcrum-pins, and consequently would raise the outer tails, and along with them the ball. By the ingenious application of screws to adjust the bearing parts of the ball upon the levers, the quantity of ascent in the ball itself was so regulated as to counteract the effect of lengthening in the iron bar, which is the true pendulum-rod.

The objections made by curious reasoners to this contrivance are, that the levers may be prevented by friction from obeying minute alterations of temperature; and that the two bars may be bended during the increase of temperature. Mr. Cumming, in his work before quoted, has considerably improved this invention, though, after all, it is liable to more objections than the gridiron. Its chief merit appears to consist in the facility with which it may be adjusted to temperature, with little or no alteration of the adjustment for time.

Several writers on this subject have strongly insisted, that every compensation for temperature, whether in a balance or pendulum, ought to move through the air along with it, in order that the several parts may be heated or cooled together. To this it has been answered, anfwered, that, from the neceffary circulation of heated air, through its difference of denfity, and the fpeed with which metals conduct heat, it is not probable that any appreciable difference of effect can arife from the pendulum itfelf becoming heated or cooled more fpeedily than any fixed part of the apparatus which may be applied to obviate its change of figure; and ftill more it is remarked, that the fmall variations would correct each other in their return. Thefe arguments appear to poffefs confiderable practical force in favour of fixed compenfations, which in fome inftances are fimpler and cheaper than fuch as move with the pendulum.

The ordinary principle of thefe combinations is neverthelefs fuch as to imply the ufe of a lever. If we fuppofe an inflexible bar of brafs, ftanding upright upon a fteady fupport, to afford the fulcrum to an horizontal lever extending each way; and that a vertical bar of fteel be attached to one extremity of the lever, and faftened beneath to the fame fupport, it is certain that the other extremity of the lever will afcend by heat, and defcend by cold, accordingly as the fulcrum rifes and falls through a greater fpace than the extremity of the fteel bar attached to the oppofite end of the lever. If the pendulum, therefore, be fufpended by a flexible piece of fpring to that end of the lever which is fartheft from the fteel bar, and the fpring be flightly confined by a notch in a metallic cock, as ufual, the afcent and defcent of the whole pendulum, by the operation of this contrivance, will counteract the defcent and afcent of the ball by heat and cold. In order that it may accurately do this, it is requifite that the proportion of the arms of the lever fhould be variable by adjuftment.

The direct oppofition of the expanfion of metals is certainly the moft fimple and effectual; and that by levers is evidently lefs fo, though capable of being magnified at pleafure. This direct action has been applied in watches to carry a curb or piece of metal which ferved to lengthen or fhorten the effective part of the pendulum-fpring, accordingly as the difference of temperature, by altering the diameter of the balance, might render it more or lefs eafy to be moved with a determinate velocity. But I have not heard of any fimple contrivance of this nature having been applied to the balance itfelf. It might not be difficult to conftruct a balance upon the principle of the gridiron pendulum; but the difficulties of adjuftment for temperature, pofition, and rate would either prove exceedingly great, or call for expedients, hitherto unthought of, to fimplify the proceffes. A very curious contrivance is at prefent ufed in our beft portable time-pieces, which differs very much from thofe before defcribed. I do not know that it has ever been fcientifically treated, nor even who was the artift that firft applied it to watch-work. One artift in a loofe uncertain manner informed me, that he underftood the contrivance to belong to M. Berthoud at Paris; another pofitively affured me, that the well-known Pinchbeck in Cockfpur-ftreet exhibited a metallic folid thermometer on this principle many years ago. Cumming appears to have known nothing of it in 1766, when he publifhed his Elements; neither do I find it in the French Encyclopédie publifhed in 1779. I fhould be happy to do juftice to the inventors by the affiftance of communications from correfpondents. The principle of this contrivance is, fimply, that if the faces of two ftrait bars of metal, differing in their expanfibility, be faftened together by riveting or foldering, they will continue ftrait only fo long as the original temperature remains; but if the temperature be augmented, the moft expanfible metal will become the longer, and a flexure will take place, fo as to produce convexity on that fide and concavity

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on the other. But if the temperature be diminished, the most variable of the two metals, by contracting more than the other, will form a concavity on its side.

It might afford data for interesting mathematical speculation to ascertain what would be the nature of the curves formed by such a combination of materials in all the assumable varieties of figure, tenacity, flexibility, and temperature. The considerations which present themselves in a loose prospect of the subject may be exemplified in two uniform bars of brass and steel, differing only in thickness, measured from the common face of contact.

Metals are capable of being lengthened or shortened by mechanical force, as well as by altering their temperature. Hence, if the temperature of a piece of metal be increased at the same time that its expansion is prevented, the consequence must be a contraction or change in the texture, of the nature of what workmen call hammer hardening. And the contrary effect may be supposed to take place if a mechanical obstacle be opposed to prevent the contraction which would take place by cold. It is known from experiment, that the dilatations and contractions from change of temperature are made with an extremely great force; but from other experiments it is also known, that the tenacities of bodies are sufficient to oppose and greatly diminish the effect of those forces.

If a thin bar of steel be soldered to a very thick bar of brass, it may be inferred that the greater expansions and contractions of the brass will alternately rarefy and condense the adherent steel, at the same time that the face of the brass will probably undergo an opposite change of the same nature, in the vicinity of the steel. It may be imagined that the immediate properties of the metals would in the course of time become changed by this continual effort; that is to say, they might become more or less flexible, tenacious, hard, expansible, dense, &c. Thus bell-wires become hard by pulling; gold, in the hands of the gold-beaters, becomes soft by bending backward and forward; brittleness is produced by twisting, and so forth. The power of the brass to alter the texture of the steel must be derived from two of its qualities; namely, its resistance to flexure, and its strength or resistance to change, of the same kind which was inferred to take place chiefly in the smaller bar. Whenever, therefore, it is required that the expansions and contractions from temperature should be exerted chiefly in bending the compound bar, it is necessary that the strengths and flexibilities of the two bars should be so proportioned that the texture of each should suffer alike.

The increase of dimensions in brass and steel in like elevations of temperature is nearly as two to one. The weight required to break a wire of steel is about double that required to produce the same effect in brass. If a bar of brass be soldered to a bar of steel of half the thickness, their strengths will then be nearly equal; and the yielding, except so far as depends on the mechanical difficulty of bending a thicker bar, will be nearly the same in both. Steel, being less flexible than brass, diminishes the effect of the mechanical advantage of the latter from its thickness, and renders it less necessary to take it into consideration.

When the strengths of the two bars are nearly equal, and the flexibilities great, there will most probably be some line within each bar which will possess the same length as that of the unconfined bar at the same temperature.

If a right line be divided into any number of equal parts, and a number of equal perpendiculars be erected at the several points of division, the distances between the upper extremities of the several adjacent perpendiculars will be equal. But if these intervals be

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every one alike increafed while the lines joining the lower extremities continue ftrait and unaltered in length, it may eafily be fhewn that the right line will become regularly polygonal: and if the number of divifions were indefinitely great, this polygonal line would become circular. The fteel and brafs, in our experiments, are referable to this theorem; for, while they continue ftrait, an indefinite number of perpendiculars may be conceived to be drawn from the furface of the fteel to the furface of the brafs, at right angles to the plane of contact. Change of temperature will render one bar longer than the other; the perpendiculars will diverge, and the right line will become a circle.

The fhorter the perpendiculars, that is to fay, the thinner the bars, the greater will be the divergency from a given difference of length, and confequently the fmaller the circle; that is to fay, the diameters will be nearly in proportion to the thickneffes. Accordingly it is found that very thin bars of this conftruction are much more bended by the fame change of temperature than fuch as are thicker. If a narrow piece of the thin tinned iron called fheet-tin, about eight or ten inches long, be rubbed bright, and wetted with a folution of fal ammoniac, and upon this there be applied an equal thin flip of fheet brafs, alfo brightened, and wetted with the fame folution, and the faces be preffed firmly together by confining them between two thick ftrait bars of metal, bound together with wire (while in the vice), the whole apparatus may be then gently heated, till the coating of the tin is fufed, and folders the faces together. This period is known by placing a fmall piece of tin upon the outfide, which will flow when the heat is fufficiently elevated. When the compound bar thus obtained is bended up in the figure of a pair of tea-tongs, the change of pofition of its extremities is very perceptible, and is fuch that (the iron being outfide) it not only opens vifibly when held to the fire or touched by the hand, but even when breathed upon.

The practical advantage of this conftruction is, that the range of motion greatly exceeds that of the mere expanfion or contraction. The difadvantage to be fufpected is, that the properties of the combination may change, fo that the original motion produced from a known alteration of temperature may not take place after fome years heating and cooling.

When a thin bar of brafs is attached to another of fteel of nearly the fame tenacity and flexibility, and both become expanded by heat, the form of a circular arc will be affumed, of which the radius may be difcovered by admeafurement; and the difference in length of the two bars afcertained by an eafy calculation. Or converfely, if their expanfion be known at any given temperature, it will be eafy to determine the curve. In order to afcertain from direct experiment, without reference to pyrometrical tables, the proportion between the lengthening of the brafs beyond the fteel, and the fpace moved through by the extremity of a bar of this kind, I had recourfe to experiment. A bar of plate fteel, 0.033 inch thick, 0.55 inch wide, and 6.4 inches long, was riveted to a perfectly fimilar and equal bar of plate brafs by fmall iron rivets, as in Figure 1, Plate V. It was then pinned at one end againft a ftrait bar of copper, which it touched through its whole length, from the pin to the other end that was left at liberty. The diftance from the pin to the free end was 6.35 inches. Immediately oppofite the laft-mentioned end a pin was fixed upright in the copper, very near the end, but not touching it. On the pin was flided a fpring clip, with a fhort tail as at C in Figure 2, where AB reprefents the compound bar and DE the pin. The brafs fide of the bar was next the copper. When the temperature was 40°

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of Fahrenheit, both bars touched; but upon immersing the whole in boiling water, the compound bar bended, and pushed the clip through the space of 0.16 inch. This space is the versed sine of the circular arc AC, produced by raising the temperature from 40° to 212°, namely 172°. But as the versed sine 0.16 is to the sine 6. 35, so is the sine 6.35 to a fourth number 252; which added to the versed sine gives the diameter 252.23 inches of the circle into which the upper face of the steel bar had bended, and its circumference 792 inches. But the difference between the diameter of the circle, measured on the brass convex outer face, and that bounded by the interior or concave face of the steel, was twice the thickness of the bar, or 0.132 inch. But it seems fair to measure the real lengths of the bars through the middle of their solidities. The difference of diameter of two circles passing through the bars in this manner would consequently be no more than 0.066 inch; and of their circumferences 0.207, which is the expansion of the whole circle of brass beyond the steel. Whence the whole periphery 792 inches is to the length of the bar (neglecting the expansion endways) 6.35 as the whole difference of the circumferences 0.207 inch is to the excess of expansion in the brass beyond the steel 0.00166 inch. Now as the motion of the end of the compound bar by flexure was 0.16 inch, it was nearly one hundred times as great as the linear difference of expansion in the two metals.

Since the versed sines of very small arcs may be taken to be as the squares of the lengths of the arcs themselves, and in similar arcs they are as the diameters, the quantities of deflection measured by the versed sines of equal small arcs in circles of different diameters will be inversely as the squares of the diameters, and directly as the diameters, or more simply in the inverse ratio of the diameters themselves. And as the steel made use of in this experiment was about five times the thickness of ordinary watch-spring, it will follow, that a compound bar made of watch spring and brass of the same thickness would have been deflected five hundred times as much as the mean excess of expansion of the brass beyond the steel.

Fig. 10, Plate V, represents the balance constructed on this principle, and known by the name of the expansion balance. The outer part of the rim is brass, and the inner steel. After this compound rim is brought to its figure by turning, it is cut through in three places A, B, C, which sets one end of each third part of the periphery at liberty to move outwards when the temperature is diminished, or inwards when it is increased. D, E, F are three similar and equal masses of metal fitted upon the circular bars in a proper manner to admit of their being fixed at any required distance from the extremity, where the motion is most considerable. G, H, I are three screws, the heads of which may be set nearer to or farther from the centre, and serve as weights to effect the adjustments for position and rate. The peculiar advantage of this balance may be explained as follows: When an increase of heat diminishes the elastic force of the pendulum-spring K, the outer brass rim being lengthened more than the steel, must throw the weights DEF nearer to the axis, and diminish the effect of the inertia of the balance, which consequently is as speedily carried through its vibration as before. And on the contrary, when cold weather adds to the elastic force of the spring, the same weights are also thrown farther out, and prevent the acceleration which would have followed. The exact adjustment of the weights is found by trial of the going of the machine. If it gain by heat, the weights compensate too much, and must be moved farther from the extreme ends of the circular compound bars; but

but if the gain be produced by cold, the fpring predominates, and the weights will accordingly require to be fet farther out.

I have not met with any application of this principle to pendulums, though it may be applied to them in a variety of methods. Fig. 3 reprefents an apparatus by which I have fufpended the fimple pendulum of a clock in my poffeffion. Its chief advantage confifts in the facility of adjufting for temperature and rate, independently of each other, without ftopping the clock. It is peculiarly applicable to the pendulum for obtaining an univerfal meafure, contrived by Hatton and improved by Whitehurft and by Dr. George Fordyce *.

In the Figures 3, 4, 5, 6, 7, 8, 9, Plate V, the fame letters denote the fame things. Fig. 3 exhibits the entire apparatus together, and the other figures fhew the parts feparately taken. II is a brafs platform, which is to be firmly fcrewed in the horizontal pofition upon the fteady fupport intended to fuftain the pendulum. At OO are two flits through which a pair of fprings pafs, and ferve in the ufual manner to afford the centres of fufpenfion of the pendulum. Between two edge bars KK is included the tapped part of a piece of turned fteel FF, of which nearly one half L is tapped with a left hand fcrew, and the other half R with a right-hand fcrew of the fame thread, a fmall portion near the middle being reduced to afford a complete termination to the fcrews. The outer tails at the extremities FF are fquared to receive a milled head G for the purpofe of moving it round. Two bridge-pieces HH are tapped to ferve as nuts to the fcrews, and are fo carefully filed at bottom, together with the edge bars, that when the bars are fixed to the platform the bridge-pieces move ftiffly along the face when the fcrew is turned. The motion, from the nature of the fcrews, is in each equal, and contrary to the other. At the outer corner above of each bridge-piece an edge is prominent, which is reduced at top, all but the corners, to afford a fteady lodgement for a compound bar EE, without fuffering it to touch the reft of the furface of thofe pieces, or to move at all horizontally. The bar EE is compofed of brafs and fteel, the brafs being laid uppermoft. It is perforated in the middle at Q, Fig. 8, and properly opened, to afford a fteady bearing for the lower extremity of the fcrew AB, Fig. 3, of which the figure may be feen at S, in Figure 7. The fcrew is provided with a double milled head, and graduated circle, each divifion of which anfwers to a difference of one fecond in the daily rate. The branches MM fupport a face, bearing either a nonius, or, as I have it, a fingle ftroke, for the purpofe of fetting the fcrew AB. The nut C receives the fprings of the pendulum in a flit P, terminating above in grooves VV, Fig. 7; and the extremities of the legs TT are fufficiently below S to allow the weight of the pendulum itfelf to preferve the vertical fituation of the fcrew. From the nut C proceeds a flat plate DD, which fills the whole fpace between the edge bars KK, and prevents the nut from moving horizontally, while it is left at liberty to rife or fall in the vertical direction.

The effect of this apparatus will be underftood without difficulty after this defcription.

I do not pretend to infinuate that the practical folution of this problem has yet been cleared of the many difficulties which attend it. The pendulum here mentioned confifts of a flat fteel wire fufpending a weight, which vibrates from a notch in a plate adjuftable at different heights to meafure the linear difference between the feconds and half-feconds pendulum. Mr. Hatton, as I am informed, applied many years ago to the Society for the Encouragement of Arts for a premium in favour of this invention. Mr. Whitehurft purfued the fame object afterwards; and at his death the apparatus was purchafed by Dr. George Fordyce, who has given an account of it, with drawings, in the Philofophical Tranfactions for the year 1794, page 2.

When the pendulum N, Fig. 3, becomes lengthened by heat, it is evident that it would go flower if the greater expanfion of the brafs in the compound bar EE did not at the fame time render the whole bar more convex above, and raife the fcrew and its nut, and with it the pendulum itfelf, which is by that means correfpondently fhortened at O. If the common rate of going from day to day differ from mean time, the adjuftment muft be made by the fcrew A. But if this rate, being fteady while the temperature continues unaltered, fhould be found to change when the temperature is changed, the adjuftment muft be made by the fcrew FF. That is to fay, the bridges HH are to be moved further afunder, if the pendulum lofes by heat; but if, on the contrary, heat caufes it to gain, the effect of the compenfation in the bar EE is too confiderable, and the remedy will confift in moving the bridges nearer to each other.

Thus it is feen that an adjuftable compenfation for temperature may be applied by this method with the fame facility as the adjuftment for rate. It only remains to be obferved, for the inftruction of actual workmen, by what method it is afcertained that the bar EE fhall poffefs fufficient variation in its flexure to produce the defired effect within the limits of the bridge-pieces. For this purpofe it is advifable to make a compound bar of brafs and fteel, faftened together by riveting, foft-foldering, or brazing; the latter method of which is moft advifable. The length of this bar may be fix or eight inches, and the thicknefs of each metal may be between the twentieth and thirtieth part of an inch. After reducing the two bars in their conjoined ftate to an uniform thicknefs by filing and calipering as much as may be requifite, the whole flexure of the bar, by a confiderable number of degrees alteration of temperature, may be afcertained in the method defcribed by reference to Fig. 2. The fpace run through by the clip C will be the verfed fine of the arc of flexure; whence the verfed fines or deviations from the right line may be deduced in fhorter lengths; for thefe are very nearly in the prefent cafe as the fquares of the lengths of the bar. The effective length of the compound bar EE muft be meafured from one of the bridges E to the middle point; that is to fay, it will be equal to half the diftance between the bearing edges when drawn as far as poffible from each other. The proportion will therefore be: as the fquare of the whole bar is to the fpace moved through by the clip, fo is the fquare of the effective bar of the apparatus to its flexure by expanfion. From the length of the intended pendulum, and the nature of the metal of its rod, its direct expanfion for the number of degrees in queftion may be known from various tables, or by experiments. The rates of expanfion in brafs and fteel have already been quoted in this paper. It is taken for granted that the compound bar here fpoken of has been made fufficiently thick, and that the deviation by flexure in the effective bar, deduced from the proportion of the fquares, will prove lefs than the direct expanfion of the pendulum. If it fhould prove greater, the bar is too thin, and there is no remedy but to make a thicker. We may however proceed on the fuppofition that the quantity of deviation in the effective bar is confiderably lefs. It will be moft convenient that this deviation fhould be about one-third lefs. If the difference do not exceed this quantity, a piece of the long bar may be cut, and fitted to the apparatus. If it do exceed, the flexibility of the bar from temperature will be increafed by reducing it on both fides with the file. The quantity of deviation will be nearly in the inverfe proportion of the thicknefs, as has already been ftated. Whence it is obvious, that the requifite thicknefs may be deduced and acquired from the original experiment of flexure by change of temperature. But it is fafer and better to take the fmall additional trouble of

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afcertaining the rate of flexure from time to time during the filing, by means of the fimple apparatus, Fig. 2.

III.

Obfervations and Experiments on the Light, Expence and Conftruction of Lamps and Candles, and the Probability of rendering Tallow a Subftitute for Wax.

IF we diftribute the catalogue of human wants in the order of the neceffity of each, food will occupy the firft place; and, next to this, in moft climates of the earth, the articles of fuel, lodging, and clothing will immediately prefent themfelves. The total want of any of thefe neceffarily implies extreme diftrefs; and if fuch a privation be applied even in fancy to an entire fociety, every notion of comfort and civilization feems at once to difappear. Inferior only to thefe in its urgency is the neceffity of artificial light during the abfence of the fun. We might indeed exift without it; but how large a portion of our lives would in that cafe be condemned to a ftate little fuperior in efficacy to that of the animals around us! It might be a curious fpeculation to enquire how far and in what refpects the morals of man would become degraded by the want of fo important an art; fince every ufeful art muft furely in its confequences affect the moral principles of fociety. But it is fufficient on the prefent occafion that, previous to entering upon a differtation refpecting the art of illumination, a train of ideas has flightly been pointed out, which cannot fail to fhew its magnitude and importance.

We are acquainted with no means, unlefs we may except electricity, of producing light but by combuftion, and this is moft probably of the fame nature. The rude method of illumination confifts in fucceffively burning certain maffes of fuel in the folid ftate. Common fires anfwer this purpofe in the apartments of houfes, and in fome light-houfes: fmall pieces of refinous wood, and the bituminous coal called kannel-coal, are in fome countries applied to the fame ufe; but the moft general and ufeful method is that in which fat oil, of an animal or vegetable kind, is burned by means of a wick. Thefe inftruments of illumination are either lamps or candles. In the lamp, the oil muft be one of thofe which retains its fluidity in the ordinary temperature of the atmofphere. The candle is formed of an oil, or other material, which is not fufible but at a temperature confiderably elevated.

The method of meafuring the comparative intenfities of light is one of the firft requifites in an enquiry concerning the art of illumination. Two methods of confiderable accuracy are defcribed in the Traité d'Optique of Bouguer, of which an abridged account is given by Dr. Prieftley in his Optics *, page 540. The firft of thefe two methods has been ufed by others fince that time, and probably before, from its very obvious nature, but particularly by Count Rumford, who has given a defcription and drawings of an inftrument called the Photometer, in the Philofophical Tranfactions for 1794. The principle it is grounded upon is, that if two lights fhine upon the fame furface at equal obliquities, and an opake body be interpofed, the two fhadows it will produce muft differ in blacknefs or intenfity in the fame degree. For the fhadow formed by intercepting the greater light

* The title of this well-known work is: *The Hiftory and prefent State of Difcoveries relating to Vifion, Light, and Colours.* By Jofeph Prieftley, LL.D. F.R.S. London. Johnfon, 1772.

will be illuminated by the ſmaller light only, and reverſely the other ſhadow will be illuminated by the greater light. That is to ſay, in ſhort, the ſtronger light will be attended with the deeper ſhadow. But it is eaſy, by removing the greater light to a greater diſtance, to render the illumination it produces at the common ſurface equal to that afforded by the leſs. Experiments of this kind may be conveniently made by faſtening a ſheet of white paper againſt the wall of a room. The two lights or candles intended to be compared, muſt then be placed ſo that the ray of light from each ſhall fall with nearly the ſame angle of incidence upon the middle of the paper. In this ſituation, if a book or other object be held to intercept part of the light which would have fallen on the paper, the two ſhadows may be made to appear as in Figure 10, Plate V, where A repreſents the ſurface illuminated by one of the lights only; B, the ſurface illuminated by the other light; C, the perfect ſhadow from which both lights are excluded. It will eaſily be underſtood that the lights about D and E, near the angle F, will fall with equal incidences when the double ſhadow is made to occupy the middle of the paper; and conſequently, if one or both of the lights be removed directly towards or from the paper, as the appearances may require, until the two ſhadows at E and D have the ſame intenſity, the quantities of light emitted by each will be as the ſquares of the diſtances from the paper. By ſome experiments made in this way in the year 1785, I was ſatisfied that the degree of illumination could be thus aſcertained to the eightieth or ninetieth part of the whole.

By experiments of this kind many uſeful particulars may be ſhewn. Thus, for example, the light of a candle, which is ſo exceedingly brilliant when firſt ſnuffed, is very ſpeedily diminiſhed to one-half, and is uſually not more than one-fifth or one-ſixth before the uneaſineſs of the eye induces us to ſnuff it. Whence it follows, that if candles could be made ſo as not to require ſnuffing, the average quantity of light afforded by the ſame quantity of combuſtible matter would be more than doubled. In the ſame way, likewiſe, ſince the coſt and duration of candles, and the conſumption of oil in lamps, are eaſily aſcertainable, it may be ſhewn whether more or leſs of light is obtained at the ſame expence during a given time, by burning a number of ſmall candles inſtead of one of greater thickneſs. From a few experiments already made out of the numerous and uſeful ſeries that preſents itſelf, I have reaſon to think that there is very much waſte in this expenſive article of accommodation.

In the lamp there are three articles which demand our attention, the oil, the wick, and the ſupply of air. It is required that the oil ſhould be readily inflammable, without containing any fetid ſubſtance which may prove offenſive, or mucilage, or other matter, to obſtruct the channels of the wick. I do not know of any proceſs for meliorating oils for this purpoſe, excepting that of waſhing with water containing acid or alkali. Either of theſe is ſaid to render the mucilage of animal oils more ſoluble in the water; but acid is preferred, becauſe it is leſs diſpoſed to combine with the oil itſelf. The office of the wick appears to be chiefly, if not ſolely, to convey the oil by capillary attraction to the place of combuſtion. As the oil is conſumed and flies off, other oil ſucceeds, and in this way a continued current of oil and maintenance of the flame are effected. But as the wicks of lamps are commonly formed of combuſtible matter, it appears to be of ſome conſequence what the nature and ſtructure of this material may be. It is certain that the flame afforded by a wick of ruſh differs very conſiderably from that afforded by cotton; though perhaps this difference may, in a great meaſure, depend on the relative dimenſions of each. And if

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we may judge from the different odour in blowing out a candle of each fort, there is fome reafon to fufpect that the decompofition of the oil is not effected precifely in the fame manner in each. We have alfo fome obfcure accounts of prepared wicks for lamps, which are ftated to poffefs the property of facilitating the combuftion of very impure oils, fo that they fhall burn for many hours without fmoke or fmell *.

The accefs of air is of the laft importance in every procefs of combuftion. When a lamp is fitted up with a very flender wick, the flame is fmall, and of a brilliant white colour: if the wick be larger, the combuftion is lefs perfect, and the flame is brown: a ftill larger wick not only exhibits a brown flame, but the lower internal part appears dark, and is occupied by a portion of volatilifed matter, which does not become ignited until it has afcended towards the point. When the wick is either very large or very long, part of this matter efcapes combuftion, and fhews itfelf in the form of coal or fmoke. The different intenfity of the ignition of flame, according to the greater or lefs fupply of air, is remarkably feen by placing a lamp with a fmall wick beneath a fhade of glafs not perfectly clofed below, and more or lefs covered above. While the current of air through the glafs fhade is perfectly free, the flame is white; but in proportion as the aperture above is diminifhed, the flame becomes brown, long, wavering, and fmoky; it inftantly recovers its original whitenefs when the opening is again enlarged. The inconvenience of a thick wick has been long fince obferved, and attempts made to remove it; in fome inftances by fubftituting a number of fmall wicks inftead of a larger; and in others, by making the wick flat inftead of cylindrical. The moft fcientific improvement of this kind, though perhaps lefs fimple than the ordinary purpofes of life demand, is the well-known lamp of Argand. In this the wick forms a hollow cylinder or tube, which flides over another tube of metal, fo as to afford an adjuftment with regard to its length. When this wick is lighted, the flame itfelf has the figure of a thin tube, to the inner as well as the outer furface of which the air has accefs from below. And a cylindrical fhade of glafs ferves to keep the flame fteady, and in a certain degree to accelerate the current of air. In this very ingenious apparatus many experiments may be made with the greateft facility. The inconvenience of a long wick, which fupplies more oil than the volume of flame is capable of burning, and which confequently emits fmoke, is feen at once by raifing the wick; and on the other hand, the effect of a fhort wick, which affords a diminutive flame merely for want of a fufficient fupply of combuftible matter, is obfervable by the contrary procefs.

The moft obvious inconvenience of lamps in general, arifes from the fluidity of the combuftible material, which requires a veffel adapted to contain it, and even in the beft

* The economical wicks of M. Leger, concerning which a Report was prefented to the Academy at Paris in 1782 by Condorcet, Lavoifier, and De Milly, were compofed of cotton of different fizes and forms, namely, round and flat, according to the ufe they were intended to ferve. They were covered with a fat fubftance, of a fmell not difagreeable, but feebly aromatic. From the trials of thefe commiffaries it was afcertained: 1. That they afforded a clearer flame, with lefs undulation. 2. That they confumed fomewhat lefs oil; and 3. That they poffeffed the remarkable property of affording neither fmell nor fmoke, however common the oil made ufe of. I find a difficulty refpecting thefe experiments, where it is faid that the flame with the prepared wick was only ten lines long, and very fteady; while that of the unprepared wick of comparifon was four inches and a quarter long, of a conical figure, and vibrated much. It feems as if this laft had been difadvantageoufly trimmed, or too much raifed, in order to have produced fuch an extravagant flame. It foon blackened a filver plate at upwards of a foot diftance.

constructed lamps is more or less liable to be spilled. When the wick of a lamp is once adjusted as to its length, the flame continues nearly in the same state for a very considerable time.

It is almost unnecessary to describe a thing so universally known as a candle. This article is formed of a consistent oil, which envelopes a porous wick of fibrous vegetable matter. The cylindrical form and dimensions of the oil are given either by casting it in a mould, or by repeatedly dipping the wick into the fused ingredient. Upon comparing a candle with a lamp, two very remarkable particulars are immediately seen. In the first place, the tallow itself, which remains in the unfused state, affords a cup or cavity to hold that portion of melted tallow which is ready to flow into the lighted part of the wick. In the second place, the combustion, instead of being confined, as in the lamp, to a certain determinate portion of the fibrous matter, is carried, by a slow succession, through the whole length. Hence arises the greater necessity for frequent snuffing the candle; and hence also the station of the freezing point of the fat oil becomes of great consequence. For it has been shewn that the brilliancy of the flame depends very much on the diameter of the wick being as small as possible; and this requisite will be most attainable in candles formed of a material that requires a higher degree of heat to fuse it. The wick of a tallow candle must be made thicker in proportion to the greater fusibility of the material, which would otherwise melt the sides of the cup, and run over in streams. The flame will therefore be yellow, smoky, and obscure, excepting for a short time immediately after snuffing. Tallow melts at the 92d degree of Fahrenheit's thermometer; spermaceti at the 133d degree; the fatty matter formed of flesh after long immersion in water melts at 127°; the *pela* * of the Chinese, at 145°; bees-wax at 142°; and bleached wax at 155°. Two of these materials are well known in the fabrication of candles. Wax in particular does not afford so brilliant a flame as tallow . but, on account of its less fusibility, the wick can be made smaller; which not only affords the advantage of a clear perfect flame, but from its flexibility it is disposed to turn on one side, and come in contact with the external air, which completely burns the extremity of the wick to white ashes, and thus performs the office of snuffing. We see, therefore, that the important object to society of rendering tallow candles equal to those of wax, does not at all depend on the combustibility of the respective materials, but upon a mechanical advantage in the cup, which is afforded by the inferior degree of fusibility in the wax; and that, to obtain this valuable object, one of the following effects must be produced: Either the tallow must be burned in a lamp, to avoid the gradual progression of the flame along the wick; or some means must be devised to enable the candle to snuff itself, as the wax candle does; or, lastly, the tallow itself must be rendered less fusible by some chemical process. I have no great reason to boast of success in the endeavour to effect these; but my hope is, that the facts and observations here presented may considerably abridge the labour of others in the same pursuit.

The makers of thermometers and other small articles with the blow-pipe and lamp, give the preference to tallow instead of oil, because its combustion is more complete, and does not blacken the glass. In this operation, the heat of the lamp melts the tallow which is occasionally brought into its vicinity by the workman. But for the usual purposes of illumination, it cannot be supposed that a person can attend to supply the combustible matter. Considerable difficulties

* Pearson, in the Philosophical Transactions for 1794.

arise

arise in the project for affording this gradual supply as it may be wanted. A cylindrical piece of tallow was inserted into a metallic tube, the upper aperture of which was partly closed by a ring, and the central part occupied by a metallic piece nearly resembling that part of the common lamp which carries the wick. In this apparatus the piece last described was intended to answer the same purpose, and was provided with a short wick. The cylinder of tallow was supported beneath in such a manner that the metallic tube and other part of this lamp were left to rest with their whole weight upon the tallow at the ring or contraction of the upper aperture. In this situation the lamp was lighted. It burned for some time with a very bright clear flame, which, when compared with that of a candle, possessed the advantage of uniform intensity, and was much superior to the ordinary flame of a lamp in its colour, and the perfect absence of smell. After some minutes it began to decay, and very soon afterwards went out. Upon examination it was found, that the metallic piece which carried the wick had fused a sufficient quantity of tallow for the supply during the combustion; that part of this tallow had flowed beneath the ring, and to other remote parts of the apparatus, beyond the influence of the flame; in consequence of which, the tube, and the cylinder of tallow were fastened together, and the expected progression of supply prevented. It seems probable, that in every lamp for burning consistent oils, the material ought to be so disposed that it may descend to the flame upon the principle of the fountain reservoir. I shall not here state the obstacles which present themselves in the prospect of this construction, but shall dismiss the subject by remarking, that a contrivance of this nature would be of the greatest public utility.

The wick of a candle, being surrounded by the flame, is nearly in the situation of a body exposed to destructive distillation in a close vessel. After losing its volatile products, the carbonaceous residue retains its figure, until, by the descent of the flame, the external air can have access to its upper extremity. But, in this case, the requisite combustion, which might snuff it, is not effected. For the portion of oil emitted by the long wick is not only too large to be perfectly burned, but also carries off much of the heat of the flame while it assumes the elastic state. By this diminished combustion and increased efflux of half-decomposed oil, a portion of coal or soot is deposited on the upper part of the wick, which gradually accumulates, and at length assumes the appearance of a fungus. The candle does not then give more than one-tenth of the light emitted in its best state. Hence it is that a candle of tallow cannot spontaneously snuff itself. It was not probable that the addition of a substance containing vital air or oxygene would supply that principle at the precise period of time required; but, as experiment is the test of every probability of this nature, I soaked a wick of cotton in a solution of nitre, then dried it, and made a candle. When this came to be lighted, nothing remarkable happened for a short time; at the expiration of which a decrepitation followed at the lower extremity of the flame, which completely divided the wick where the blackened part commences. The whole of the matter in combustion therefore fell off, and the candle was of course instantly extinguished. Whether this would have happened in all proportions of the salt or constructions of the candle I did not try, because the smell of azote was sufficiently strong and unpleasant to forbid the use of nitre in the pursuit. From various considerations I am disposed to think that the spontaneous snuffing of candles made of tallow, or other fusible materials, will scarcely be effected but by the discovery of some material for the wick which shall be voluminous enough

enough to abſorb the tallow, and at the ſame time ſufficiently flexible to bend on one ſide.

The moſt promiſing ſpeculation reſpecting this moſt uſeful article, ſeems to direct itſelf to the cup which contains the melted tallow. The imperfection of this part has already been noticed, namely, that it breaks down by fuſion, and ſuffers its fluid contents to eſcape. The Chineſe have a kind of candle about half an inch in diameter, which, in the harbour of Canton, is called a *lobchock*; but whether the name be Chineſe, or the corruption of ſome European word, I am ignorant. The wick is of cotton, wrapped round a ſmall ſtick or match of the bamboo cane. The body of the candle is white tallow; but the external part to the thickneſs of perhaps one-thirtieth of an inch, conſiſts of a waxy matter coloured red. This covering gives a conſiderable degree of ſolidity to the candle, and prevents its guttering, becauſe leſs fuſible than the tallow itſelf. I did not obſerve that the ſtick in the middle was either advantageous or the contrary; and, as I now write from the recollection of this object at ſo remote a period as twenty-five years ago, I can only conjecture that it might be of advantage in throwing up a leſs quantity of oil into the flame than would have been conveyed by a wick of cotton ſufficiently ſtout to have occupied its place unſupported in the axis of the candle.

Many years ago I made a candle in imitation of the *lobchock*. The expedient to which I had recourſe conſiſted in adapting the wick in the uſual pewter mould: wax was then poured in, and immediately afterwards poured out: the film of wax which adhered to the inner ſurface of the mould ſoon became cool; and the candle was completed by filling the mould with tallow. When it was drawn out, it was found to be cracked longitudinally on its ſurface, which I attributed to the contraction of the wax, by cooling, being greater than that of the tallow. At preſent I think it equally probable that the cracking might have been occaſioned by too ſudden cooling of the wax before the tallow was poured in; but other avocations prevented the experiments from being varied and repeated. It is probable that the Chineſe external coating may not be formed of pure hard bleached wax.

But the moſt deciſive remedy for the imperfection of this cheapeſt, and in other reſpects beſt material for candles, would undoubtedly be to diminiſh its fuſibility. Various ſubſtances may be combined with tallow, either in the direct or indirect method. In the latter way, by the decompoſition of ſoap, a number of experiments were made by Berthollet, of which an account is inſerted in the Memoirs of the Academy at Paris for the year 1780, and copied into the 26th volume of the Journal de Phyſique. None of theſe point directly to the preſent object; beſides which, it is probable that the ſoap made uſe of by that eminent chemiſt was formed not of tallow, but oil. I am not aware of any regular ſeries of experiments concerning the mutual action of fat oils and other chemical agents, more eſpecially ſuch as may be directed to this important object of diminiſhing its ſolubility; for which reaſon I ſhall mention a few experiments made with this view.

1. Tallow was melted in a ſmall ſilver veſſel. Solid tallow ſinks in the fluid, and diſſolves without any remarkable appearance. 2. Gum ſandarach in tears was not diſſolved, but emitted bubbles, ſwelled up, became brown, emitted fumes, and became criſp or friable. No ſolution nor improvement of the tallow. 3. Shell-lac ſwelled up with bubbles, and was more perfectly fuſed than the gum ſandarach in the former experiment. When the tallow was poured off, it was thought to congeal rather more ſpeedily. The lac did not appear

appear to be altered. 4. Benzoin bubbled without much fwelling, was fufed, and emitted fumes of an agreeable fmell, though not refembling the flowers of benzoin. A flight or partial folution feemed to take place. The benzoin was fofter and of a darker colour than before, and the tallow lefs confiftent. 5. Common refin unites very readily with melted tallow, and forms a more fufible compound than the tallow itfelf. 6. Camphor melts eafily in tallow, without altering its appearance. When the tallow is near boiling, camphoric fumes fly off. The compound appeared more fufible than tallow. 7. The acid or flowers of benzoin diffolves in great quantities without any ebullition or commotion. Much fmoke arifes from the compound, which does not fmell like the acid of benzoin. Tallow alone does not fume at a low heat, though it emits a fmell fomething like that of oil-olive. When the proportion of the acid was confiderable, fmall needled cryftals appeared as the temperature diminifhed. The appearances of feparation are different according to the quantity of acid. The compound has the hardnefs and confiftence of firm foap, and is partially tranfparent. 8. Vitriolated tartar, nitre, white fugar, cream of tartar, cryftallized borax, and the falt fold in the markets under the name of falt of lemons, but which is fuppofed to be the effential falt of forrel, or vegetable alkali fuperfaturated with acid of fugar, were refpectively tried without any obvious mutual action or change of properties in the tallow. 9. Calcined magnefia rendered tallow opake and turbid, but did not feem to diffolve. Its effect refembled that of lime.

It is propofed to try the oxigenated acetous acid, or radical vinegar; the acid of ants, of fugar, of borax, of galls, the tanning principle, the ferous and gelatinous animal matter, the fecula of vegetables, vegetable gluten, bird-lime, and other principles, either by direct or indirect application. The object, in a commercial point of view, is entitled to an extenfive and affiduous inveftigation. Chemifts in general fuppofe the hardnefs or lefs fufibility of wax to arife from oxigen, and to this object it may perhaps be advantageous to direct a certain portion of the enquiry. The metallic falts and calces are the combinations from which this principle is moft commonly obtained; but the combinations of thefe with fat oils have hitherto afforded little promife of the improvement here fought. The fubject is however fo little known, that experiments of the loofeft and moft conjectural kind are by no means to be defpifed.

IV.

A Memoir upon the Difcovery of America. By M. Otto.*

It has always been looked upon as a piece of injuftice, not to have given the name of Columbus to that valuable part of the world which he difcovered; and that Americanus Vefpucius, who did nothing but follow his footfteps, has had the good fortune of having his name handed down to the moft diftant pofterity, to the prejudice of his predeceffor. What then will be faid, if it fhall be proved that neither of thofe celebrated navigators was the firft difcoverer of this immenfe country; and that this honour belongs to a man fcarcely known in the republic of letters? This, however, is what I fhall attempt in the following paper; and if the obfcurity of cotemporary writers, and the diftance of time, do not afford arguments fufficient for an abfolute demonftration, there will, however, be enough to call in queftion the pretenfions of Chriftopher Columbus.

* From the American Tranfactions, Vol. II. in French and Englifh. The former appears to be the original; for which reafon I have compared this with it, and corrected feveral paffages.

I ſhall not here enter into an examination of the reveries of ſome hiſtorians, on the voyages of the Carthaginians, the Atlantis of Plato, the bold expedition of Mador Prince of Wales, and ſon of Owen Guinnedd, of which Hakluyt has preſerved ſome account, nor on the voyages of Bacchus, or the land Ophir of Solomon. Conjectures of this kind, whether true or falſe, cannot leſſen the glory of Columbus, were there not proof that he received, juſt before his expedition, the charts and journal of a learned aſtronomer who had been in America.

Garcilaſſo de la Vega, born at Cuſco in Peru, has given us an hiſtory of his country, in which, to take from Columbus the merit of the diſcovery of America, and to give the honour of it to the Spaniards, he aſſures us, that this navigator had been informed of the exiſtence of another continent by Alonzo Sanchez de Huelva, who, in his voyage to the Canaries, had been driven by a gale of wind to the Antilles; but that his chief information was procured from a celebrated geographer of the name of Martin Behenira. Garcilaſſo ſays nothing more of this Behenira: and ſince we know of no Spaniſh geographer of this name, Garcilaſſo has been ſuſpected of making a ſacrifice of truth to the deſire of wreſting from a Genoeſe the glory of diſcovering the New World.

On looking over with attention a liſt of all the learned men of the fifteenth century, I find the name of Martin Behem, a famous geographer and navigator. The chriſtian name is the ſame with that mentioned by Garcilaſſo, and I find that the ſyllables *ira* added to his name are owing to a particular circumſtance, namely, the honour conferred on him by John II. King of Portugal. It is then poſſible that this Martin Behem is the ſame perſon as Martin Behenira mentioned by Garcilaſſo; but this vague conjecture will receive the ſtamp of truth by the following detail:

The literary hiſtory of Germany gives an account of a Martin Behem, Beheim or Behin, who was born at Nurenberg, an Imperial city of the circle of Franconia, of a noble family, ſome branches of which are yet extant. He was much addicted to the ſtudy of geography, aſtronomy, and navigation from his infancy. At a more mature age, he often thought on the poſſibility of the exiſtence of the Antipodes, and of a weſtern continent. Filled with this great idea, he paid a viſit in 1459 to Iſabella, daughter of John I. King of Portugal, and Regent of the duchy of Burgundy and Flanders. Having informed her of his deſigns, he procured a veſſel, in which he made the diſcovery of the iſland of Fayal in 1460. He there eſtabliſhed a colony of Flemings, whoſe deſcendants yet exiſt in the Azores, which were for ſome time called the Flemiſh Iſlands. This circumſtance is proved, not only by the writings of cotemporary authors, but alſo by the manuſcripts preſerved in the records of Nurenberg, from the Latin of which the following is tranſlated: "Martin Behem tendered his ſervices to the daughter of John King of Luſitania, who reigned after the death of Philip of Burgundy, ſurnamed the Good; and from her procured a ſhip, by means of which, having ſailed beyond all the then known limits of the Weſtern Ocean, he was the firſt who in the memory of man diſcovered the iſland of Fayal, abounding with beech trees, which the people of Luſitania call Faye; whence it derived its name. After this he diſcovered the neighbouring iſlands, called by one general name, the Azores, from the multitude of hawks which build their neſts there (for the Luſitanians uſe this term for hawks, and the French too uſe the word eſſos or eſores in their purſuit of this game); and left colonies of the Flemiſh on them, when they began to be called Flemiſh Iſlands, &c." Although this record is contrary to the generally received opinion that the Azores were diſcovered by Gonſalva Velho, a Portugueſe, yet its authenticity cannot be doubted: it is confirmed by ſeveral cotemporary

temporary writers, and especially by Wagenfeil, one of the moft learned men of the laft century; who, after having travelled into Africa, and throughout all Europe, was made Doctor of Laws at Orleans, and chofen Fellow of the Academies of Turin and Padua, although he was a German by birth. The particulars are to be found in his Univerfal Hiftory and Geography. I have moreover received from the records of Nurenberg a note written in German, on parchment, which contains the following facts: "Martin Beham, Efquire, fon of Mr. Martin Beham, of Scoperin, lived in the reign of John II. King of Portugal, in an ifland which he difcovered, and called the Ifland of Fayal, one of the Azores, lying in the Weftern Ocean."

After having obtained from the Regent Ifabella a grant of Fayal, and refided there about twenty years, during which time he was bufied in making frefh difcoveries in geography, by fmall excurfions which need not be mentioned, Behem applied in 1484 (which was eight years before Columbus's expedition) to John II. King of Portugal, to procure the means of undertaking a great expedition towards the fouth-weft. This prince gave him fome fhips, with which he difcovered that part of America which is now called Brazil; and he even failed to the Straits of Magellan, or to the country of fome favage tribes whom he called Patagonians, from the extremities of their bodies being covered with a fkin more like a bear's paws than human hands and feet. This fact is proved by authentic records, preferved in the archives of Nurenberg, one of which in particular deferves attention: "Martin Behem, traverfing the Atlantic Ocean for feveral years, examined the American iflands, and difcovered the Strait which bears the name of Magellan, before either Chriftopher Columbus or Magellan failed thofe feas; whence he mathematically delineated on a geographical chart, for the King of Lufitania, the fituation of the coaft around every part of that famous and renowned Strait long before Magellan thought of his expedition." This affertion is fupported by Behem's own letters, written in German, and preferved in the archives of Nurenberg, in a book which contains the birth and illuftrious actions of the nobility of that city. Thefe letters are dated in 1486, that is, fix years before the expedition of Columbus. This wonderful difcovery has not efcaped the notice of cotemporary writers. The following paffage is tranflated from the Latin chronicle of Hartman Schedl: "In the year 1485, John II. King of Portugal, a man of a magnanimous fpirit, furnifhed fome galleys with provifions, and fent them to the fouthward, beyond the Straits of Gibraltar. He gave the command of this fquadron to James Canus, a Portuguefe, and Martin Behem, a German of Nurenberg in Upper Germany, defcended of the family of Bonna: a man very well acquainted with the fituation of the globe; bleffed with a conftitution able to bear the fatigues of the fea; and who, by actual experiments and long failing, had made himfelf perfectly mafter with regard to the longitudes and latitudes of Ptolemy in the weft. Thefe two, by the bounty of Heaven, coafting along the Southern Ocean, and having croffed the Equator, got into the other hemifphere, where, facing to the eaftward, their fhadows projected towards the fouth and right hand. Thus, by their induftry, they have opened to us another world, hitherto unknown, and for many years attempted by none but the Genoefe, and by them in vain. Having finifhed this cruife in the fpace of twenty-fix months, they returned to Portugal with the lofs of many of their feamen by the violence of the climate."

This paffage becomes more interefting from being quoted in a book on the ftate of Europe during the reign of the Emperor Frederick III. by the learned hiftorian Æneas Sylvius, afterwards Pope Pius II. This hiftorian died before the difcoveries of Behem were made; but the publifhers of his works thought the paffage in Hartman Schedl fo important, that they inferted it in the hiftory. We alfo find the following particulars in the remarks made by Petrus Mateus on the canon law, two years before the expedition of Columbus: "*Primæ navigationes, &c.*—The firft Chriftian voyages to the newly difcovered iflands became frequent under the reign of Henry, fon of John, King of Lufitania. After his death Alphonfus V. profecuted the defign; and John, who fucceeded him, followed the plan of Alphonfus, by the affiftance of Martin Bohem, a very fkilful navigator; fo that in a fhort time the name of Lufitania became famous over the whole world." Cellarius, one of the moft learned men of his age, fays exprefsly, "*Behaimeus non modo, &c.*—Bœhm did not think it enough to furvey the ifland of Fayal, which he firft difcovered, or the other adjacent iflands which the Lufitanians call Azores, and we, after the example of Bœhm's companions, call Flemifh iflands, but advanced ftill farther and farther fouth, until he arrived at the remoteft ftrait, through which, Ferdinand Magellan, following his track, afterwards failed, and called it after his own name." All thefe quotations, which cannot be thought tedious, fince they ferve to prove a fact almoft unknown, feem to demonftrate, that the firft difcovery of America is due to the Portuguefe, and not to the Spaniards; and that the chief merit belongs to a German aftronomer. The expedition of Ferdinand Magellan, which did not take place before the year 1519, arofe from the following fortunate circumftance: This perfon, being in the apartment of the King of Portugal, faw there a chart of the coaft of America, drawn by Behem, and at once conceived the bold project of following the fteps of this great navigator. Jerome Benzon, who publifhed a defcription of America in 1550, fpeaks of this chart; a copy of which, fent by Behem himfelf, is preferved in the archives of Nurenberg. The celebrated aftronomer Riccioli, though an Italian, yet does not feem willing to give his countryman the honour of this important difcovery. In his Geographia Reformata, book iii. p. 90, he fays, "Chriftopher Columbus never thought of an expedition to the Weft Indies until his arrival in the ifland of Madeira, where, amufing himfelf in forming and delineating geographical charts, he obtained information from Martin Bœhm, or, as the Spaniards fay, from Alphonfus Sanchez de Huelva, a pilot, who had chanced to fall in with the ifland afterwards called Dominica." And in another place: "Let Bœhm and Columbus have each their praife; they were both excellent navigators; but Columbus would never have thought of his expedition to America, had not Bœhm gone there before him. His name is not fo much celebrated as that of Columbus, Americus, or Magellan, although he is fuperior to them all."

But the moft pofitive proof of the great fervices rendered to the crown of Portugal by Behem, is the recompenfe beftowed on him by King John, who in 1485 knighted him in the moft folemn manner, in the prefence of all his court. I have before me a German paper, extracted from the archives of Nurenberg, to the following purport: "In the year 1485, on the 18th of February, in Portugal, in the city of Allafavas, and in the church of St. Salvador, after the mafs, Martin Behem of Nurenberg was made a knight, by the hands of the moft puiffant Lord John the Second, King of Portugal, Algarve, Africa, and Guinea; and his chief Squire was the King himfelf, who put the fword in his belt; and the Duke of Begia was his fecond Squire, who put on his right fpur; and his third Squire was Count Chriftopher de Mela, the King's coufin, who put on his left fpur; and his fourth Squire was Count Martini Marbarinis, who put on his iron helmet; and the King himfelf gave him the blow on the fhoulder; which was done in the prefence of all the princes,

princes, lords, and knights of the kingdom; and he efpoufed the daughter of a great lord, in confideration of the important fervices he had performed; and he was made governor of the ifland of Fayal." Thefe marks of diftinction, conferred on a ftranger, could not be meant as a recompenfe for the difcovery of the Azores, which was made twenty years before, but as a reward for the difcovery of Congo, from whence the Chevalier Behem had brought gold and different kinds of precious wares. This difcovery made much greater impreffion than that of a weftern world, made at the fame time, but which neither increafed the wealth of the royal treafury, nor fatisfied the avarice of the merchants.

In 1492 the Chevalier Behem, crowned with honours and riches, undertook a journey to Nurenberg, to vifit his native country and his family. He there made a terreftrial globe, which is looked on as a mafter-piece for that time, and which is ftill preferved in the library of that city. The outline of his difcoveries may there be feen, under the name of weftern lands; and from their fituation it cannot be doubted that they are the prefent Coafts of Brazil, and the environs of the Straits of Magellan. This globe was made in the fame year that Columbus fet out on his expedition; therefore it is impoffible that Behem could have profited by the works of this navigator, who befides went a much more northerly courfe.

[*To be concluded in the next Number.*]

V.

*Analysis of the Oriental Lapis Lazuli**. *By M. Klaproth.*

THE analyfis of lapis lazuli made by Margraff has fhewn that the blue colour of this ftone is not owing to copper, as has commonly been thought, but that it arifes from iron. But as we have not hitherto poffeffed any accurate analyfis of this ftone, I have thought it might be ufeful to examine it anew. In fact, Margraff informs us, that lime, gypfum, and filex, together with iron, are its component parts; but he does not determine their proportions; and his analyfis is incomplete, as he does not mention the alumine which this ftone alfo contains.

According to Rinmann, the lapis lazuli contains lime, quartz, iron, and the acid of fluor. I have not found the latter; and it is probable that Rinmann's opinion was grounded upon the phofphorefcence of this ftone when it is heated.

Cronftedt and fome others have affirmed that the lapis contains a quantity of filver, after the rate of two ounces per quintal. But my effays have afforded no certain indication of the prefence of this metal.

I felected for experiment a fpecies of the lapis of a beautiful deep blue colour, and carefully feparated the white and pyritous fpecks.

A. One hundred parts of lapis lazuli in thin flakes were kept in a ftate of ignition for half an hour in a porcelain crucible. They loft two parts of their weight; the colour was not at all changed. This permanence at a great heat induced me to think that the ftone might be of advantage in enamelling; to which opinion I was the more inclined, from

* From the Annales de Chimie, XXI. 150. The French tranflation is made from the German, by Citizen Taffaert, but whence taken is not faid.

Bergmann

Bergmann having prefumed that the Chinefe and Japanefe make ufe of it for the blue colour of their porcelain *. To afcertain the truth, I mixed fome of the very fine powder of lapis lazuli with a proper flux, and difpofed it upon porcelain, which I afterwards placed in the enameller's furnace. My expectations were not realifed, for the colour changed to a blueifh grey.

B. By expofure to a more violent fire the lapis was deprived of twelve centenaries of its weight, and was vitrified. I apprehend that the two parts loft by the firft ignition confifted merely of water, and that the additional ten in the fecond effay confifted for the moft part of carbonic acid. This opinion is fupported by the effervefcence of the ftone, which takes place when an acid is poured upon it, and indicates, though very feebly, that part of the calcareous earth is united to carbonic acid.

C. Upon two hundred grains of the lapis in impalpable powder I poured muriatic acid diluted with an equal meafure of water (étendu de moitié eau), and digefted the whole together by a progreffive heat. The colour became gradually changed to a grey afh-colour; and when the ebullition commenced the powder was more ftrongly attacked, and at length became converted into a thick jelly. This was diluted with water; after which nitric acid was added, and the whole boiled until the refidue had become white. The filtered folution was of a pale yellow colour.

D. The product which remained upon the filter had the appearance of fand, and weighed one hundred and thirty-eight grains. When this was mixed with three parts of cauftic pot-afh, and ignited, it afforded a greenifh mafs. The folution of the mafs in water was perfectly colourlefs. By an excefs of muriatic acid I feparated the filex, which after ignition weighed 57 grains.

E. The folutions C and D, decompofed by the carbonate of pot-afh, afforded a yellow white precipitate, which, when dried, amounted to 221 grains, and was re-diffolved in the muriatic acid.

F. Ammoniac feparated from the folution E a gelatinous precipitate. This was thrown ftill wet into a cauftic lixivium, in which I digefted it. It was not totally diffolved, but left a yellowifh refidue weighing 113 grains.

G. The fluid which remained after the precipitation by ammoniac was treated with carbonate of pot-afh, and afforded 59 grains of the carbonate of lime.

H. Upon the 113 grains which were infoluble in the cauftic pot-afh, I poured fulphuric acid diluted with water. This mixture, after having been heated, affumed the form of a jelly. It was diluted with a large quantity of water, and afforded a precipitate of filex which after ignition weighed 29 grains.

I. After the feparation of the filex I poured ammoniac into the folution. The precipitate, ftill humid, being mixed with a cauftic lixivium, depofited brown flocks in the liquor, which when dry weighed 13 grains. I diffolved thefe in the muriatic acid; and this folution treated with ammoniac let fall the oxide of iron, which after ignition weighed fix grains. The carbonate of ammoniac likewife precipitated five grains of calcareous earth.

K. The folutions F and I by the cauftic alkalis were treated with the muriatic acid. The

* Opufcules Phyfiques et Chymiques de Bergmann, Vol. IV. page 32. From the good quality of the cobalt at prefent ufed for this purpofe in England and elfewhere, there is no reafon to think that any other material is ufed for full deep blues in the Eaft. N.

precipitate

precipitate which was formed and re-diſſolved by exceſs of acid, and afterwards formed again by carbonate of pot-aſh, was diſſolved again by ſulphuric acid. A new precipitate of ſilex was thus obtained, which after ignition weighed ſix grains. The ſilex was ſeparated, and pot-aſh being then added, cryſtals of alum were obtained. Theſe were diſſolved in water, and the alumine, after precipitation, drying, and ignition, weighed 29 grains.

L. I had aſcertained by a previous experiment, that all the calcareous earth contained in the lapis was not ſaturated by the carbonic acid, but that part was combined with the ſulphuric acid. I had boiled in a large quantity of water a portion of the pulveriſed ſtone; the water, after filtration, did not appear very tranſparent. I poured in the muriate of barytes, and a precipitate was immediately formed conſiſting of ſulphate of barytes. In order to aſcertain the proportion of ſulphate of lime, I ſuperſaturated with muriatic acid the liquor which remained from the precipitate C and the water of edulcoration; after which I poured in muriate of barytes, and obtained a precipitate of ſulphate of barytes, which when perfectly dried weighed 19½ grains.

I ſuſpected that the alkalis made uſe of in the experiments D and E might contain a ſmall portion of ſulphate of pot-aſh, and by that means have contributed to the precipitate of the ſulphate of barytes. To aſcertain this fact, I diſſolved an equal quantity of alkali; and having ſuperſaturated it with muriatic acid, and treated it with the muriate of barytes, the ſulphate of barytes which fell down was carefully collected and dried, and weighed 1½ grain, a quantity to be deducted from the foregoing, and leaves 18 grains of ſulphate of barytes to determine the proportion of the ſulphate of each; and by computation I find that 200 grains of the lapis contain 8.18 of ſulphuric acid, of the ſpecific gravity 1.850, or in combination with calcareous earth 13 grains of ſulphate of lime. This calculation is founded upon my experiments, which ſhew that with the difference of a very ſmall fractional quantity, 100 parts of ſulphuric acid of the ſpecific gravity 1.850 form 220 of ſulphate of barytes. The ſame quantity of acid for its ſaturation with calcareous earth demands either 100 parts of carbonate of lime, or 55 of pure lime, and forms 160 of ſulphate of lime.

The 200 grains of lapis lazuli contain therefore as component parts:

				Grains.
Siliceous earth —	$\left\{\begin{matrix} D & 57 \\ H & 29 \\ K & 6 \end{matrix}\right\}$	—	— —	92
Calcareous earth —	$\left\{\begin{matrix} G & 59 \\ I & 5 \end{matrix}\right\}$	64	but ignited —	35
Alumine — —	K	—	—	29
Oxide of iron —	I	— —	—	6
Sulphuric acid —	L	— —	—	8
Carbonic acid —	B	— —	—	20
Water — —	A	—	—	4
				194
			Loſs	6
				200

But

But becauſe the calcareous earth is combined partly with the ſulphuric acid and partly with the carbonic acid, it follows that the combination of the lapis is

				Grains.	
Silex	—	—	—	46	100
Alumine	—	—	—	14.50	
Calcareous carbonate		—	—	28	
Calcareous ſulphate		—	—	6.50	
Oxide of iron	—	—	—	3	
Water	—	—	—	2	

The ſum is here complete, becauſe I have conſidered the calcareous earth as perfectly ſaturated with the carbonic acid; which does not however appear in fact to be the caſe.

VI.

Uſeful Notices reſpecting various Objects.—Roſe-Water—Eau de Luce—Soap of Wool—Sea Sickneſs.

1. *Roſe-Water.*

THE ſimple diſtilled water from roſe-leaves, which is ſold by the name of roſe-water, has the diſadvantage of loſing its fragrance, by a ſpontaneous change which ſeems to be of the nature of the putrid fermentation. This happens in much leſs time than muſt elapſe between the annual ſeaſons when freſh roſe-leaves are to be had. The article is nevertheleſs to be purchaſed at any time of the year; from which circumſtance it has been ſuppoſed that the manufacturing perfumers were in poſſeſſion of ſome method of preventing the proceſs by which it is changed. A philoſophical friend aſſures me that this is not the caſe, but that they diſtil only ſo much roſe-water at a time as they know will keep during the period of the regular demand for that quantity; at the end of which they diſtil the ſame quantity from other roſe-leaves. Their management for inſuring a regular ſupply conſiſts in packing the freſh roſe-leaves with common ſalt in a maſs, to a portion of which, when required, they add water, and diſtil from the mixture.

2. *Eau de Luce.*

THE ſame intelligent friend informs me, that the uſual recipes in the London Pharmacopœia and other books, for making the fragrant alkaline liquor called eau de luce, the leading perfection of which is, that it ſhall poſſeſs and retain a milky opacity, do not ſucceed, but that a ſeparation takes place, and the fluid becomes more or leſs clear by keeping. The uſe of maſtic in this compoſition has hitherto been kept a ſecret. Upon his aſſurance that this is the chief ingredient, I made the following trials. One dram of the rectified oil of amber was diſſolved in four ounces of the ſtrongeſt ardent ſpirit of the ſhops; its ſpecific gravity being .840 at 60 degrees of Fahrenheit. This is the oily ſpirit which is to be added to volatile alkali to form eau de luce, according to Macquer, in his Dictionary, who ſpeaks highly of a recipe to this effect, but with the addition of ten or twelve grains of white ſoap to the ſpirit, previous to the oil. The purpoſes of my experiment did not require the ſoap. A portion of the clear ſpirit was poured upon a larger quantity of fine

 powdered

powdered maftic, than it was judged could be taken up. This was occafionally agitated without heat, by which means the gum refin was for the moft part gradually diffolved. One part of the oily folution was poured into a phial, and to this was added one part of the folution of maftic. No opacity or other change appeared. Four parts of ftrong cauftic volatile alkali were then poured in, and immediately fhaken. The fluid was of a denfe opake white colour, affording a flight ruddy tinge when the light was feen through a thin portion of it.

In a fecond mixture four parts of the alkali were added to one of the folution of maftic; it appeared of a lefs denfe and more yellowifh white than the former mixture. More of the gum refinous folution was then poured in, but it ftill appeared lefs opake than that mixture. It was ruddy by tranfmitted light.

The laft experiment was repeated with the oily folution inftead of that of maftic. The white was much lefs denfe than either of the foregoing compounds, and the requifite opacity was not given by augmenting the dofe of the oily folution. No ruddinefs nor other remarkable appearance was feen by tranfmitted light.

Thefe mixtures were left at repofe for two days; no feparation appeared in either of the compounds containing maftic; the compound confifting of the oily folution and alkali became paler by the feparation of a cream at the top.

It appears, therefore, that the firft of thefe three mixtures, fubject to variation of the quantity of its ingredients, and the odorant additions which may be made, is a good eau de luce.

Chemical writers fpeak of a milky fluid, under the name of *lac virginale*, made by pouring tincture of benzoin into fome perfumed water. As this fragrant balfam promifed to be in fome refpects fuperior to maftic, I was induced to try it. A fpirituous folution was made of the brown benzoin, which happened to be at hand. To one part of the oily folution firft mentioned was added one part of the clear filtered folution of benzoin. Thefe mixed uniformly. Four parts of the cauftic volatile alkali were then added. The mixture became opake, fawn-coloured and curdled, with much lefs pungency of fmell than was afforded by the other alkaline compounds. The next day the opake part had confiderably fubfided, and left a brownifh tranfparent though turbid fluid above. It appears probable, therefore, that the acid of the benzoin had not only united with part of the volatile alkali, but that the oily and refinous parts had likewife been difpofed to coagulate in this arrangement. It is undoubtedly of no value in the prefent point of view.

3. *Soap of Wool.*

I HAVE made a few experiments on the faponaceous combination of wool and alkali *. A cauftic lixivium was made by mixing a folution of the cryftals of foda with a due portion of lime and water. To the clear ley, in a ftate of ebullition, wool was added, a little at a time. It was fpeedily diffolved by ftirring; but on account of the lixivium being too ftrong, the compound appeared thick, and was poured out before, as it afterwards appeared, the faturation of the alkali was effected. The compound, after being left in an earthen veffel for feveral days, was found to be fcarcely more confiftent than treacle. Its fmell was offenfive, though not ftrong; it readily diffolved in water, but fcarcely lathered at all; and

* Philof. Journal, I. 40.

by its action when smeared on the hands it appeared to possess a considerable portion of disengaged alkali. The whole was then dissolved in water, and left for several days on account of other avocations. In the mean time a weaker lixivium was boiled with the addition of wool in small portions, till the last quantity added remained for a considerable time undissolved, and was taken out. The fluid poured into cups had the consistence of treacle. It lathered scarcely at all with water, in which, however, it readily dissolved; and upon being used with a piece of flannel, it seemed admirably adapted for scouring that cloth. Four successive rinsings in different clear waters seemed to have washed off the soap; but the smell still remained very strong in the flannel. It went off in six or seven hours completely.

I was desirous of ascertaining the state of the wool when it should be again separated by means of an acid. For this purpose I added diluted vitriolic acid to the aqueous solution of the first imperfect soap, which contained about half a pound of wool. A separation instantly followed; and, to my surprise, upon stirring the liquor, it lathered very well. The next day the consistent part had settled to the bottom. The clear liquor, which was considerably acid, and had a sulphureous smell, lathered with agitation. The consistent part had the appearance as if mostly composed of broken fibres. The whole was poured into a coarse cloth, and, after straining away the acid, water was suffered to run from a cock into the bag of the cloth, and fell clear into a small bason which it overflowed below. It was remarkable that this clear tasteless water bore a strong bead or head by agitation. The edulcorated matter, when dry, weighed less than half an ounce. Hence it should seem in this loose experiment that the acid had not only saturated the disengaged alkali, but had either combined with the wool of the soap, or formed a triple compound with the soap itself, which was mostly carried off in the edulcoration. The residue was probably wool, which had not been completely deprived of its organization in the first boiling. But I mean to examine the perfect soap with greater attention to quantities.

4. *Sea Sickness.*

IN an account of the islands of Goree and Senegal, by Citizen Prélong, printed in the xviiith volume of the Annales de Chimie, the author mentions that he suffered prodigiously and for a long time by the sea sickness, but was greatly, and afterwards habitually, relieved by taking ten drops of vitriolic ether in a spoonful of water. I have also been assured by the commander of a packet constantly passing between Harwich and Helvoetsluys, that he always found this distressing illness greatly relieved in his passengers by a small quantity of red wine heated with spices. The sea sickness seems to be a spasmodic affection of the stomach, produced by the alternate pressure and recess of the contents of that viscus against its lower internal surface, accordingly as the rise and fall of the ship opposes or recedes from the action of gravity. Hence it is relieved by change from the erect to the prone posture, or by removing from the extremity of the vessel to the vicinity of axis of the pitching motion, near the mainmast; and hence also, when the organs have become habituated to a regular vibration of one kind in a ship for several months, the sickness may nevertheless be again generated by a different vibration in a boat. As it is a habit which requires some time to be generated, and comes on gradually, it is not difficult to oppose it by mental effort or diversion; but, to such as have not acquired this facility, it may be acceptable to know

 that

that the above fpirituous ftimuli have been found of fervice in counteracting it. The ftimulus of food, taken even againft the inclination, has alfo been frequently found to be beneficial.

VII.

A Comparifon between Electrical Machines with a Cylinder, and thofe which produce their Effect by means of a Circular Plate of Glafs. With a Defcription of a Machine of great Simplicity and Power, invented by DR. MARTINUS VAN MARUM.

IT is a remarkable circumftance that the plate-machine for electricity, firft invented and publifhed in this country by Dr. Ingenhoufz, has never been much ufed here, though it has been well received on the Continent, and almoft univerfally preferred to the machine with a cylinder. This may in fome meafure be owing to the improvements in our manufacture of blown glafs, by which we have been fupplied with cylinders of confiderable dimenfions at a moderate price, inftead of the globes which were originally ufed for this purpofe. Some years ago (1787) I improved the cylinder-machine by a contrivance for changing the electricity of the conductor from plus to minus almoft inftantly, which is defcribed in the Philofophical Tranfactions for 1789. From a comparifon of the quantities of electricity accumulated by the friction of a fquare foot of glafs, I was induced to adopt the opinion, that cylinders were preferable to plates in every refpect, excepting the great quantity of furface afforded by the latter in machines conftructed without regard to expence. The labours of the celebrated Dr. Van Marum, together with fome obfervations of my own, have fince that time tended to alter my opinion: for which reafon I fhall, in the firft place, enumerate a few general facts, and then proceed to defcribe his excellent improvement of my contrivance.

I. Electrical machines were formerly made to revolve with confiderable velocity by a multiplying wheel. This has fince been rejected in confequence of the ftrong excitation and increafed friction produced by a more advantageous application of the amalgam of zinc and mercury. The machines with a fingle winch demand the fame labour as before to work them. They exhibit much more fire in the form of flafhes and fparks. But, as far as my experience fhews, the fpark from the old machine was denfer and more pungent, the excitation more fteady, and the time employed in charging fomewhat fhorter.

II. A cylinder with a fingle winch requires larger terminations of its metallic parts to prevent the fire from flafhing out, than are required either in the old machine, or one of thofe conftructed with a flat plate.

III. It frequently happens that the fimple machine will be in a ftate to throw out ramifications to the table, to the face of the operator, and into the air; though the actual fpark is not very denfe, nor the power great, when examined by the time required to charge a battery or jar.

IV. From thefe circumftances it appeared probable that the electric matter in a charged conductor may be thrown into a ftate of undulation by an irregular fupply from the cylinder, and that in this ftate it will fly off more readily than when fupplied by a more uniform ftream. Thus, when the cylinder is of an irregular figure, the action of the cufhion will be ftronger on the one fide than the other, and this irregularity may be increafed alfo from other caufes. The irregular fupply will therefore be made at more diftant intervals by the fimple machine, at more frequent reiterations in the machine with the multiplying wheel, and perhaps uniformly from a plate.

V. The effect of this undulation may be fhewn from various facts. 1. A fmall wire of many yards in length, communicating from a ball to the ground, will be rendered luminous through its whole length, by fparks of pofitive electricity given to the ball; but it will not be at all luminous when the fame quantity of electricity is given in a more continued ftream by placing the ball in contact with the prime conductor. 2. If a metallic rod be infulated with a ball at one end, of a diameter fufficient to prevent the fpontaneous flafhing of electricity when it forms part of the prime conductor, and at the other end of the ftem be fixed another ball of a proper fize to draw the fpark; when this laft ball is placed in contact with the prime conductor, the other ball will not throw out flafhes; but if it be withdrawn fo as to receive fparks, the external ball, though certainly no more electrified than before, will throw out a flafh every time the fpark is emitted. 3. A brafs ball of four inches diameter was connected by a metallic ftem to the end of the prime conductor of an electrical machine in the pofitive ftate, fufficiently vigorous to throw a flafh into the air now and then. The metallic ftem, which was about fix inches long, was then changed for another of the fame dimenfions of deal wood. In this ftate the ball threw out continual flafhes into the air. The experiment was frequently repeated, and the laft refult may naturally be fuppofed to depend on the imperfection or difcontinuity of the conducting matter in the wooden ftem. 4. A pointed wire was inferted in the pofitive conductor of Nairne's electrical machine, with the point upwards. It was then covered with a clean Florence flafk. The point of the wire occupied the centre of the bottle. Whenever the pofitive fpark was drawn from the conductor, the point of the wire exhibited the luminous negative fign. But when the experiment was repeated on the negative conductor and the fpark drawn, the point emitted the pofitive flafh fo as to fill the whole capacity of the bottle with ramified light. In thefe experiments it may be prefumed, that the efcape at the point was occafioned by undulation. 5. The lateral fpark in difcharging a jar may be urged as an inftance of the fame kind.

Thefe obfervations feem to give the advantage in favour of plate-machines as far as relates to the efcape. I fhall therefore proceed to defcribe that of Dr. Van Marum*, and afterwards ftate from his report, compared with my own experiments, what may be the proportions of electricity collected from each fquare foot, which paffes the cufhion in machines of both kinds.

Plate III exhibits a perfpective view of the machine, and Plate IV a fection, exclufive of the cufhions. In the view it may be obferved that the cufhions are each feparately infulated upon pillars of glafs, and are applied nearly in the direction of the horizontal diameter of the plate, inftead of the vertical diameter as heretofore. The ball diametrically oppofite to the handle is the prime conductor, and the femicircular piece with two cylindrical ends ferves, in the pofition of the drawing, to receive the electricity from the plate. By the happy contrivance of altering the pofition of this femicircular branch from vertical to nearly horizontal,

* Abridged from the *Seconde Continuation des Expériences faites par le Moyen de la Machine Electrique Teylerienne.*

zontal, the cylindrical ends may be placed in contact with the cushions, and the prime conductor instantly exhibits negative electricity. But as it is necessary that the cushions should communicate with the ground when the positive power is wanted, and that they should be insulated when the negative power is required, there is another semicircular branch applied to the opposite side of the plate nearly at right angles to the first. That is to say, when positive electricity is wanted, this second branch denoted by I. I in the section Fig. 1, Pl. IV, is placed nearly horizontal, and forms a communication from the cushions to the ground through a metallic rod from K behind the mahogany pillar which supports the axis; but when on the contrary the negative power is wanted, and the branch from the prime conductor is placed in contact with the cushions, this other branch from the axis is put into the vertical situation, and carries off the electricity emitted from the plate of glass.

The axis of the plate Bh, Fig. 1, Plate IV, is supported by a single column A, which for that purpose is provided with a bearing-piece K, on which two brass collar-pieces DD, represented more at large and in face in Fig. 3, are fixed, and carry the axis itself. The whole of Fig. 1, is reduced to one-eighth of its real dimensions, unless contracted by the shrinking of the paper after printing; to obviate which, it may be remarked that the diameter of the plate is 31 English inches. The axis has a counterpoise O, of lead, to prevent too great friction in the collar D nearest the handle. The arc of the conductor EE, which carries the two small receiving conductors FF, is fixed to the axis G, which turns in the ball H. On the other side of the plate is seen the other arc II, of brass wire, half an inch in diameter, fixed to the extremity of the bearing-piece K, so that it may be turned in the same manner as the arc EE. The two receiving conductors FF are six inches long, and two and a half inches in diameter. The double line P represents a copper tube terminating in a ball Q. It moves like a radius upon the stem R of the ball S, which being screwed into the conductor H, serves to confine the arm P in any position which may be required. The diameter of the ball S is only two inches, which, together with certain other less rounded parts of this apparatus, may serve to shew that the considerable electricity from this machine is less disposed to escape than if it had proceeded from a cylinder. The dissipation of electricity along the glass supports is prevented by a kind of cap T, of mahogany, which affords an electrical well or cavity underneath, and likewise effectually covers the metallic caps into which the glass is cemented. The lower extremity of the cap is guarded in the same manner by a hollow piece or ring V, of mahogany, which covers the metallic socket into which the glass is cemented. The three glass pillars are set in sliding-pieces, as marked on the platform of Plate III, which are 9 inches long.

The rubbers of this machine differ in no essential particular from those described by the inventor in the Journal de Physique for February 1791; and the apparatus for applying them is described in the same work for April 1789. Fig. 2. represents a section of this judicious piece of mechanism seen from above, and one-fourth of the real size. A metallic sliding-piece bb, is slided into a correspondent face, on the ball Z, which is one of those fixed on the top of the glass pillars near the circumference of the glass plate in Plate III. To this is affixed the piece dd, which terminates in two hinges gg, that allow the springs ee to move in the plane of the horizon. The pieces gg represent the wood-work of the cushions attached to the extremities of the springs by the hinges hh. The springs are regulated by the bolt and screw ii. The two cushions are thus made to apply to the plate equally through

their

their whole length; the actions on the opposite sides of the plate are accurately the same; and the play of the hinges gg, prevents the plate from being endangered by any strain in the direction of its axis. It is certain that, before this adequate provision was made to secure those essential requisites, it was impracticable to apply the cushions to a plate with the same safety and effect as to cylinders, which possess much strength from their figure. An ingenious workman will probably find little difficulty in constructing these rubbers from this description and drawing; but the most precise information respecting every circumstance and dimensions is to be found in the letters above quoted.

The inner extremities of the cushions are defended by the plates of gum-lac YY, which cover the three sides or edges, and prevent their attracting the electric matter from the ends of the receiving conductor.

That part of the axis which moves between the collars is made of steel. The middle of the non-conducting part of the axis is a cylinder of walnut-tree wood aaaa, baked until its insulating power is equal to that of glass, and then soaked in amber varnish, while the wood still remains hot. The two extremities of this cylinder, which are of a less diameter, are forced, by strong blows, with a mallet, into the stout brass caps b and c, in which they are retained by three iron screws dd. The cylinder aa, and the brass caps are covered with a layer of gum-lac eeee, to preserve the insulating state of the wooden cylinder more perfectly, and to prevent the cap b from throwing flashes to the rubbers. The bottom of the cap b is screwed home on the tapped extremity of the steel axis b. The base of the cap c, which is four inches in diameter, terminates in an axis one inch thick, and two in length; the extremity of which is formed into a screw. The glass plate is put on this projecting part, and secured in its place by a nut of box-wood, forced home by a key, applied in the holes ii. Two rings of felt are applied on each side of the glass, to defend its surface from the contact of the wood and the metal; and the central hole in the glass, which is two inches in diameter, contains a ring of box-wood, which prevents its immediate application to the axis.

As it is necessary that the axis G should be parallel to the axis of the plate, in order that the conductors FF may move parallel to the plate itself, the pillar M is rendered adjustable by three bearing screws RR at the bottom, which re-act against the strong central screw T, and this is drawn downwards by its nut. The conductors FF are also adjustable by the sliding-pieces vv, and the binding-screws ww, which also afford an adjustment to bring the axis of each small conductor parallel to the face of the glass plate. A similar adjustment may be observed at the extremities of the arc II.

Fig. 4. represents a section of the moving part of the branch II, one-half of its real size. A brass plate $\alpha\alpha$ is screwed to the face of the capital K by three iron screws β. To this is screwed another ring $\delta\delta$, which affords a groove for the moveable ring $\gamma\gamma$, into which the arms II are fixed. This is accordingly applied in its place before the ring $\delta\delta$ is fixed.

The wooden part of the rubbers GG, Fig. 2, Plate IV, is covered with thin plates of iron, excepting the surface nearest to the glass. The intention of this is to maintain a more perfect communication between the rubbed part of the cushion and the earth or negative conductor, as the case may be.

The plates of gum-lac YY, are applied to the rubbers, each by means of a thin plate of brass, to which they are affixed by heat. There are two wires riveted in these plates, which are thrust into correspondent holes in the wooden part of the cushion.

The

The mahogany column A ends in a fquare ζζ, upon which the piece K is fitted and firmly applied, by means of the fcrew and nut exhibited in the fection.

To afcertain the power of this machine, Dr. Van Marum relates an experiment made before the Directors of the Teylerian Eftablifhment, and other philofophical gentlemen, in circumftances not very favourable to the apparatus; but to which he gives the preference, on account of the refpectability of the affiftants. A battery of ninety jars, each containing upwards of a fquare foot of coated glafs, was charged to the higheft degree by 150 turns of the plate, fo that it difcharged itfelf. The great Teylerian machine with two plates of fixty-five inches diameter in its original ftate, before Dr. Van Marum's improved rubbers were applied to it, never charged the fame battery, in the moft favourable circumftances, in lefs than 66 turns. It follows, therefore, that this fmall and fimple machine exhibited $\frac{66}{150}$ths, or about $\frac{4}{9}$ths of the power of that great machine in its firft ftate; and probably, if the circumftances had been alike favourable in each, it would have amounted to one half. The Doctor has grounded a calculation upon thefe facts; but as he ftates the rubbed furfaces of thefe two machines, probably by fome miftake in calculation, to be 1243 and 9636 fquare inches refpectively, I fhall repeat the calculation in this place.

The diameter of the plate is 31 inches, and the length of the cufhion 9 inches. Then $\overline{31}|^2 .7854 - \overline{31-18}|^2 .7854 = 522$ fquare inches rubbed by one cufhion on one fide. And $522 \times 4 = 2088$ fquare inches rubbed by the four cufhions. Again in the great machine, the two plates having a diameter of 65 inches, and eight cufhions of $15\frac{1}{2}$ inches long, $\overline{65}|^2 .7854 - \overline{65-31}|^2 .7854 = 2410.4$. And $2410.4 \times 8 = 19283$ fquare inches rubbed. But the intenfity of the electric power of a machine will be in the compound ratio inverfely of the furfaces and number of turns when the charge is the fame; Or $150 \times 2088 : 66 \times 19283 :: 1 =$ the intenfity of the larger machine $: 4 =$ the intenfity of the fmaller.

To have increafed the power of fteady excitation four-fold, is certainly an aftonifhing acquifition. This expreffion, however, of the intenfities appears to be lefs generally ufeful than that of the ratio of the furface rubbed, to that which is charged. This laft expreffion becomes very fimple when the latter quantity is reduced to 1, or unity. Thus, in the two machines here mentioned, the rubbed furfaces in inches for the battery are $\frac{19283 \times 66}{90 \times 144}$ and $\frac{2088 \times 150}{90 \times 144}$, which are equal to the fimple numbers 90.5 and 24.0, which refpectively denote the number of inches rubbed to charge one inch of coated glafs.

The great machine charged a fingle foot of glafs, by rubbing 66.6 feet; and a battery of 224 feet at the rate of 94.8 feet rubbed per foot. If the gradual decay of excitation be fuppofed the fame in the fmall machine here defcribed, it muft have commenced with an intenfity of 17.6. In the Philofophical Tranfactions, already quoted, I have ftated the commencing intenfity of a cylinder, excited by the amalgam of zinc, as tried by a jar of $2\frac{1}{2}$ feet to be 18.03 and 19.34. But, from my notes, I find that this jar was charged with lefs than 15.0 when the hand was conftantly preffed againft the filk-flap; and alfo, that this preffure increafed the intenfity as 49 to 39 in fome few trials, not enough varied and repeated. The labour of turning was very great; much more than, from various circumftances, I am inclined to fuppofe Mr. Van Marum's method requires. From this confideration, as well as from the numbers, and the probability that, on account of the lefs undulation, the charges by a plate may be higher before they explode than thofe by a cylinder, and likewife from

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the large surface exposed to friction, I conclude, that the machine described in this paper is at least equal in steady intensity, and much superior in power of charging, to any cylinder-machine which has ever been made.

VIII.

*The Process for giving a beautiful White Colour to Raw Silk without Scouring. By M. BAUME *.*

[Concluded from page 32.]

TO complete the description of M. Baumé's process for bleaching silk, nothing more remains, than to shew in what manner he recovers the ardent spirit, and ensures the purity of the acids made use of. These circumstances are of essential importance to the art: for the process would be much too expensive if the spirit were lost, and it could not be made to succeed at all if the acid were impure.

The alcohol which has been used in bleaching silk, is acid, and loaded with colouring matter. In this state it cannot be again used. There are two methods of distilling it; which have their respective advantages and inconveniences. By the first, the acid is lost; which is saturated with pot-ash, in order that the distillation may be afterwards performed in a copper alembic. The second is performed by distilling with glass retorts, or an alembic of silver. In either of these vessels, which are not acted upon by the marine acid, the distillation may be performed, and the greater part of the acid recovered. The inventor most generally practised the saturation of the acid from reasons of convenience; but recommends the use of a silver alembic, as being most economical upon the whole, in a manufactory.

A solution of pot-ash is to be poured into the acid spirit and stirred about to promote the saturation. Carbonic acid is disengaged with strong effervescence from the alkali, and the point of saturation is known by the usual test, that the fluid does not redden the tincture of turnsol. The distillation is then to be made in the copper alembic, and the alcohol reserved in proper vessels, as mentioned at the beginning of this Memoir.

If too much alkali should have been added, the liquor remaining in the alembic may be used in another saturation. The alkali in this process being an expensive article, Mr. Baumé endeavoured to supply its place by chalk, quick-lime, and lime which had been slaked by exposure to the air. But he found that the action of the spirit upon the calcareous earth, or perhaps the absence of water, prevented the acid from uniting with that substance. The union does not take place to perfect saturation in less than five or six weeks, even when the alcohol is diluted with upwards of fifty times its bulk of water.

In the second process for distilling without alkali, the acid spirit is distributed into a great number of glass retorts, placed in the sand-bath, on the gallery of a furnace. The first product is scarcely acid; but what follows is more and more so, and must be kept in vessels of glass or stone ware, which become embarrassing on account of their number. The fluid which remains in the retorts has the colour of beer slightly turbid, and contains the greatest part of the marine acid. It must be poured into one or more retorts, and concentrated by heat

* The Editors of the Journal de Physique, to which reference was made at the beginning of this Abstract, omitted to mention how they obtained it. I find in the Annales de Chimie, XVII. 156, that it was read at the Public Meeting of the Academy at Paris, April 10, 1793.

heat gradually applied. The firft liquor which comes over is flightly red, turbid, and fcarcely acid. This is to be thrown away, and the receivers changed. The fucceeding product is the colourlefs marine acid, of an aromatic fmell refembling the buds of poplar. The refin of the filk remains in the retort decompofed by the acid. The marine acid thus obtained is weaker than it originally was; which is in fact of little confequence, as it is pure, and may be fafely ufed, either by increafing the dofe proportional to its diminifhed ftrength, or by concentrating it, if required, in the ufual way.

If this diftillation be made in a filver alembic, inftead of retorts of glafs, and a capital and worm of pure tin be annexed, the alcohol will be obtained fo flightly acid as fcarcely to redden the tincture of turnfol; but it is fufficiently acid to receive injury if preferved in a copper veffel.

If a cucurbit of filver be prepared, of the capacity of three or four quarts, with a glafs head, the refidues of the firft diftillation may be treated in this veffel in the fame manner as has been directed for glafs retorts. M. Baumé affirms that he has practifed all thefe operations with glafs retorts and a fmall filver alembic, with the moft perfect fuccefs; but that he made ufe of pot-afh to faturate the marine acid, becaufe he had not a filver veffel of fufficient capacity. From the danger of diftilling large quantities of ardent fpirit in glafs veffels, he is of opinion that no motives of economy are fufficient to juftify the rifk attending this method. In the ufe of tin, it is neceffary to be careful that it contains no adulteration of lead, becaufe the vapours of marine acid have fufficient power to alter this laft metal very confiderably.

Upon the firft intimation of this new procefs in France, manufactories were immediately eftablifhed, to the number of twenty or more, without the concurrence of Mr. Baumé, by perfons who confequently were not aware of the apparently minute but very important circumftances neceffary to infure its fuccefs. In particular, the inventor ftates that the marine acid of commerce is unfit for this purpofe.

This acid was formerly prepared with the marine falt of the faltpetre manufacturers; and even when it is made with good falt, the decompofition is effected with common vitriolic acid which contains nitrous acid. Marine acid mixed with a fmall quantity of nitrous acid does not prevent the filk from being beautifully whitened: it even accelerates the procefs confiderably, and in the moft fatisfactory manner. But the alcohol, every time it is ufed and rectified, becomes charged with the acid and gas of nitre, which affume the characters of the nitrous anodyne liquor. In this ftate, neither diftillations nor repeated rectifications from alkali are fufficient to feparate the nitrous matter from the alcohol. Then it is that the fuccefs of the operator vanifhes, with a degree of rapidity equal to the advances which encouraged his hopes at the commencement. The fame difappointments befel M. Baumé at the beginning of his labours; to prevent which, he directs the preparation of the vitriolic and marine acids to the following effect.

The vitriolic acid of commerce is obtained by burning fulphur in chambers of lead, with the addition of faltpetre, either crude or of the fecond cryftallization, and a fmall portion of flax. This acid is concentrated and rectified in France, at the place of its fabrication, to 66 degrees of Baumé's hydrometer, or fpecific gravity in the ufual form 1.848. It contains fulphur, lead, vitriolated tartar, Glauber's falt, alum, felenite, and particularly the nitrous and marine acid.

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To purify it, one hundred pounds of this vitriolic acid is to be mixed in a large bafon of copper with the fame quantity of river water, and ftirred with a wooden fpatula. The mixture inftantly becomes heated to the boiling-water point, and a great quantity of red vapour is difengaged, which has the fmell of aqua-regia, and arifes from the nitrous and marine acids. When this mixture is made, it is proper to immerfe the bafon to a fuitable depth in a large veffel of water, to haften the cooling. As foon as it is fufficiently cooled it is to be drawn off into bottles, and left to become clear during feveral days. Great part of the fulphur falls down. The author obtained from four to fix drams.

A gallery muft be provided, on which two rows of iron pots of eleven or twelve inches in diameter are to be properly placed for feparate fand-baths, as M. Baumé always practifed in the fublimation of fal-ammoniac. By this means the retorts are ifolated, and if one breaks, the acid cannot diffufe itfelf and break the others in its vicinity. An empty retort is then to be placed in each pot, and covered with fand. In this way they are much more convenient to arrange, and are attended with no rifk.

The acid is in the next place to be decanted and conveyed into the retorts by a fyphon funnel, and the rectification proceeded upon until it becomes perfectly white. Towards the end of the operation a fmall quantity of fulphur fublimes in the neck of the retort. Inftead of receivers a fmall glafs cup is placed beneath the aperture of each retort, in order to facilitate the diffipation of the nitrous and marine acids.

When the acid in the retorts is fufficiently cooled, it is poured a fecond time into the copper bafon, and mixed with 100 pounds of river water, as at firft, and again concentrated in the retorts till it becomes perfectly clear. Sulphur has been afforded in fome inftances by the fecond rectification. The liquor which diftils is received in the cups as before, and the acid in the retorts is then fufficiently pure: that is to fay, it is purified from all volatile matter. The lead and neutral falts ftill remain combined with the acid, but fortunately they can in no refpect injure the purity of the marine acid.

This concentrated acid exhibits 68 degrees by the hydrometer, or fpecific gravity 1.836. It ftill contains a portion of gas, but fo fmall in quantity as not to injure the purity of the marine acid, to which it only gives the property of cryftallifing when the temperature of the air is near the freezing point.

During the rectification of this acid, what firft comes over is mere water, and muft be thrown away; but that which fucceeds is the aqueous acid. If this be fet apart, and concentrated, a confiderable quantity of vitriolic acid is obtained of the greateft purity. As it has been carried over in diftillation, it contains no foreign matter.

The author attempted, but in vain, to diffipate the nitrous acid from the acid of vitriol by ebullition in an open veffel without concentration. The experiment was made with 50 pounds of common vitriolic acid and 60 of river water. This was kept boiling in the copper bafon for four days, water being added from time to time to fupply the lofs by evaporation. The copper bafon, by weighing before and after the operation, had loft by folution no more than ten drams of copper. The acid was blue, but became white as ufual during the rectification in the retorts. From this experiment, as the author obferves, it is feen not only that the nitrous acid cannot be diffipated by fimple ebullition without concentration, but that the action of the vitriolic acid upon copper is extremely flight.

The marine acid is to be difengaged from common falt by the application of this vitriolic acid

acid in the ufual manner. But as M. Baumé's experience led him to various fimple manipulations and remarks of importance, and more efpecially as he confiders the defcription of this procefs as part of the new art of bleaching filk, he has annexed it to his memoir.

The vitriolic acid obtained by the foregoing procefs being too concentrated, muft be diluted in the copper bafon as before with river water. It is convenient to add 18 ounces of water to each pound of the acid, becaufe the marine acid is not wanted in a ftate of high concentration. This mixture ought to give 35 or 36 degrees by Baumé's hydrometer; which laft anfwers to a fpecific gravity of 1.333. When it is cold it may be preferved in bottles for ufe.

In the next place, four pounds of marine falt dried, becaufe in that ftate it pours beft, is to be put into a retort of the capacity of five or fix French pints, or Englifh quarts. This may be done by means of a paper funnel, or a long-necked funnel of glafs, which muft enter the body of the retort in order that the neck may remain clean. A number of thefe muft be difpofed on a gallery in two oppofite rows, with the necks properly enclofed and enveloped in fand as ufual.

A bottle or gauge being provided of fuch a fize as by previous experiment is known to hold four pounds of the vitriolic acid before mentioned; this quantity of the acid muft be meafured into each of the retorts by means of a curved funnel, the tube of which may pafs into the body, to prevent the acid being fpilled in the neck. If neverthelefs a few drops fhould fall, no inconvenience will follow, as this pure acid is not detrimental to the bleaching procefs.

The fupports for the receivers are then to be placed, and the receivers applied, each being pierced with a fmall hole. The junctures are to be made good with pafted paper, and the diftillation begun. A gradual heat is to be applied until the fluid boils gently. The marine acid which firft rifes is volatile and expanfible *, and requires the fmall holes of the receiver to be occafionally opened; but after one fourth part of the time of diftillation the acid comes over freely, and the vapours ceafe to be elaftic.

This diftillation lafts two days; but it is practicable to avoid fitting up the intermediate night. The fire muft be fo managed that the contents of the retort may be very liquid in the evening: if it begins to thicken, there is reafon to apprehend that it may be too hard the next day; in which cafe the heat will dilate the concrete matter before it liquefies, and break the containing veffel.

Towards the clofe of the diftillation the matter fwells up confiderably. When this happens, it is proper to empty the receivers, and raife the retorts, that more fand may flow in beneath them. When the matter is dry, and nothing more comes over, the operation is finifhed.

Each retort affords five pounds of marine acid, of the ftrength of 14 or 15 degrees; fpecific gravity 1.114. When the retorts are half cooled, one pound of hot river water is to be poured into each, and the diftillation being refumed affords 24 ounces of the fame marine acid from each retort.

It is remarkable, that in this procefs fome of the retorts afford the colourlefs and fome the yellow acid; which is an object of no confequence with regard to the bleaching. The author thinks the yellow colour is owing to a portion of fulphur ftill remaining in the

* It might be of advantage, even in the large way, to adapt a fimple pneumatic apparatus to condenfe the marine acid air in water, as is ufual in philofophical proceffes. N.

vitriolic acid. And if from curiofity the products of feveral of the retorts be received in eight different parts, it will be feen that in fome the acid which paffes firft is the moft concentrated, and gives 20 degrees by the hydrometer; that the products diminifh fucceffively in the progrefs of concentration, till the laft exhibits fometimes no more than eight degrees; but that others afford the moft concentrated acid at the beginning and end, while that in the middle of the diftillation is the weakeft. All thefe products mixed together afford a mean refult of 14, 15, or 16 degrees *.

The hard compact faline matter in the retort confifts of much Glauber's falt, and a fmall quantity of undecompofed common falt. M. Baumé's method of extracting it is fimple and ingenious. He fills the retort with water, corks it, and inverts it in an open veffel alfo containing water, over which is fixed a board with holes for receiving the neck of the retort. It is proper that the faline mafs fhould be detached, which foon happens in the filled retort, and fuffered to flide down towards the neck before it is placed in the hole of the board. The cork is then taken out, and by that means the water in the retort communicates with that of the open veffel. As the falt diffolves, the brine flows down, and is replaced by purer water from below, which from its lefs denfity rifes to the uppermoft place in the retort. In this way the evacuation is made in two days without trouble, which could not fafely be effected in eight or ten days by fucceffive filling and emptying the retorts.

M. Baumé concludes his memoir by defcribing the method of giving a bright yellow to filk, whether raw or bleached. For this purpofe ten gros or drams of nitrous acid are to be mixed with one pound of alcohol, and into this a few ounces of filk are to be immerfed, and kept on the water bath at between 30 and 40 degrees of Reaumur, or 100° and 130° of Fahrenheit. The filk acquires a tarnifhed brown colour, and muft be cleared of its acid by wafhing in feveral waters, and afterwards fcoured with foap in the ufual manner. When thus cleaned and dry, it has the appearance of gold threads when feen in the fun's light. Different fhades may be given by keeping it a fhorter time in the acidulated fpirit; all which are equally permanent, and refift wafhing and every other teft. The author propofes them to be ufed in articles of furniture wrought in defigns which require light and fhade.

MATHEMATICAL CORRESPONDENCE.

IN order to accommodate fuch mathematical correfpondents as may refide at a diftance from the metropolis, and are difpofed to contribute to this part of the work, the folutions to the queftions propofed in any number of the Journal will be uniformly given the fecond month after their publication; but it is requefted that they may be fent as early as poffible, that proper time may be allowed to prepare them for infertion.

QUESTIONS propofed for Solution.

QUESTION III. *By ANALYTICUS.*

IT is required to determine the odds againft the dealer, at the game of whift, having all the thirteen trumps in his own hand.

* For the correfpondent fpecific gravities fee p. 39 of this Journal.

QUESTION IV. *By J. B.*

GIVEN the time in which mercury is raifed to the boiling point by the heat of a furnace, and the rate of cooling per minute, after it has been removed, to determine the heat of the furnace, or that to which the mercury has been expofed.

SCIENTIFIC NEWS.

THE particular memoirs contained in the four firft cahiers of the Journal of the Polytechnic School at Paris are thirteen in number *: two on ftereotomy; one on fortification; fix on chemiftry; three on general phyfics; and one on the application to the arts.

The memoirs of ftereotomy treat: 1. On the determination of tints in defigns, by feveral pupils of the fchool; and 2. On the curve lines of the furface of the ellipfoid, by Monge.

The folution of the interefting problem of the determination of tints is deduced from the leffons which Monge has given upon perfpective. This folution depends on the fundamental principle, that all bodies reflect the white rays; and that the quantity of rays of this colour, reflected from each point of a body, depends on its polifh, and the angles formed by the incident and reflected rays which come to the eye; that the more white rays are reflected, the more luminous the body will appear; and on the contrary, the lefs of thefe rays are reflected, the more obfcure it will be.

All polifhed bodies prefent a white fpot or line; the line is vifible upon cylinders, cones, &c.; the point, upon furfaces of double curvature.

When bodies are perfectly polifhed, a white point or determinate furface is perceived; but when they are obfcure, a fucceffive degradation of tints is feen, which depends on the form of the body.

From the point, the line, or the moft enlightened furface, it is poffible to trace a fucceffion of curves of equal tint: thefe are the curves of which the authors of this memoir have in the firft place determined the equation, and afterwards fought to afcertain the law of the degradation of the tints.

If it be affumed that any tint laid on white paper will poffefs an intenfity dependent on the quantity of colour fpread, and the proportion of white points left uncovered, the authors of the memoir have fought to determine whether the law of the application of tints upon each other ought to depend on their degradation: by this means they have fucceeded in giving the theory of wafhed tints, properly fo called.

The folution of this problem, fo interefting to thofe who cultivate the art of defign, and are capable of purfuing the calculation it demands, proves the extended knowledge to which the pupils have arrived, and the progrefs they daily make, under their Inftitutor.

The memoir on the curve lines of the furface of the ellipfoid, which Monge has given in the fecond cahier of the Journal Polytechnique, contains an application of the property of curve furfaces with regard to their curvature, to the configuration of ftones for arches.

The joints of arch-work ought to be made agreeable to a number of conditions; the chief of which are: "1. They muft be everywhere perpendicular to the arch,

* This Analyfis is tranflated from the Journal des Sçavans, p. 117.

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in order that the angles of two contiguous key-ftones being refpectively equal, they may alike refift difunion by the action they exercife upon each other. 2. They muft be perpendicular between the ftones, for the fame reafon. 3. They muft be generated by the motion of a right line; for the furfaces generated in this manner are alone fufceptible of being accurately wrought; and it is neceffary that the joints of the contiguous ftones fhould be perfectly well executed, becaufe very flight irregularities would produce a rupture of the arch."

All thefe conditions may be obtained by dividing the curved furfaces of a vault by lines drawn from the one to the other of the two curves; and formed by the movement of a point of the furface which meets a feries of normals placed in the fame direction. The diftance between thefe lines muft be a finite quantity, dependent on the nature of the materials. . . . . . .

"The operations of artifts having been conftantly directed to this general folution, they have obtained it only for the moft eafy cafes of cylindric and conical furfaces, and fuch as are generated by the revolution of a plane. But with regard to curved furfaces of which they know not the lines of curvature, they almoft generally excluded them from the conftruction of arches, even when circumftances urgently demanded them; and it is principally to this that we are to attribute the bad effect generally produced in architecture by leaning arches, or portions of arch-work (les morceaux de trait de coupe des pierres), becaufe, in order to render this practicable, a furface or curvature is chofen for the arch which is not always fuch as the nature of things demands."

The example chofen by Monge for the application of his principles, is apparently the moft happy he could have felected in the prefent fituation of the French government, in which architects are bufied in the conftruction of halls deftined for the fittings of the Legiflative Council. Now the form moft favourable to the diftribution of the members of a deliberative affembly, and the fituation of the orator, is the elliptic curve commonly called an oval. From the projection of thefe curves, traced on the defigns at the end of this memoir, it may be feen what agreeable and elegant forms are thus produced; and how eminently the architect may avail himfelf of the principles here eftablifhed, even for the objects of decoration. Students in architecture will find this memoir highly deferving of their perufal. It will more and more convince them of the immenfe refources afforded by the ftudy of ftereotomy for the folidity of conftruction, and even for beauty of new forms, afforded by this theory to the fyftem of ornament. . .

The art of fecuring thofe who defend a rampart from the ftroke of balls and fhot is known in French by the term *defilement*. Say, affiftant profeffor in the Polytechnic School, has given a memoir on this fubject in the fourth cahier of the Journal of that inftitution.

He fhews in the firft place, that the outline of the defilement is the fame as that of a fhadow afforded by a row of luminous points fituated in the fpace from which the defenders of a fortification may be fired on.

He divides his ground into two parts: the exterior fpace in which the attacking party may be placed, and the interior fpace which is to be defended.

He examines the art of fecuring a work (de filer) in two different cafes: 1. That in which the parapet is determined. 2. That in which the ground plan is given, but not abfolutely

folutely the relief or profile. To thefe two cafes he adds certain rules for tracing fortifications, deduced fingly from confiderations relative to their defilement.

Thefe three queftions are preceded by fome preliminary notions; and by the method in which Say has treated his fubject, we have acquired in this memoir a more complete treatife than any which has yet appeared on defilements. He has the merit of having fixed with clearnefs and precifion, a feries of principles which hitherto have been only tranfmitted in a fugitive and as it were traditional manner, in the School de Mezières, appropriated to the inftruction of the *élèves du génie*, and deftined again to receive them, according to the meffage long ago fent from the Government to the Legiflative Body.

The fix memoirs of chemiftry are: 1. Defcription and ufe of an eudiometer of fulphate of pot-afh (liver of fulphur), by Guyton. 2 Obfervations on the eudiometric properties of phofphorus, by Berthollet. 3. Analyfis of the calcedony of Creufot, by Guyton. 4. Experiments on the fufibility of earths, and their habitudes with faline fluxes, together with the folvent action they exert on each other, by Guyton. 5. Experiments on the formation of the colouring pruffic principle, by Bonjour. 6. The properties of the fulphureous acid, and its combination with earths and alkaline bafes, by Fourcroy and Vauquelin.

[*To be continued.*]

NEW PUBLICATIONS.

Travels in Hungary, with a fhort Account of Vienna in the Year 1793. By Robert Townfon, LL. D. F. R. S. Edin. &c. &c. Illuftrated with a map and fixteen other copper-plates. 4to. 506 pages including the Index. Printed for Robinfon. Price 1l. 1s.

To thofe who attend chiefly to diplomatical politics, or the balance of power, the kingdom of Hungary will probably afford an object of fubordinate value; but on the larger, more extended and important confiderations of internal government, political economy, and the ftate of man with regard to fcience and manners in the progrefs of civilization, it will prove highly interefting. Mr. Townfon has publifhed the prefent work from the corrected notes of a five-months' tour; in which thefe and other objects of utility and entertainment have engaged his attention. As I hope fhortly to give a fuller account of this work, the prefent notice is intended only to announce the publication of a valuable and entertaining book.

The Hiftoire Naturelle of Valmont de Bomare, rangée par ordre de matieres par L. Blondelin of the Univerfity of Bâle, ornamented with coloured plates engraved by J. J. de Méchel, was announced for publication in the middle of February laft, in the Decade Philofophique, &c. It will be printed at Bâle; and the Quadrupeds were then ready for the prefs. It will amount to about 150 fheets, or 6 or 7 volumes in octavo, with at leaft the fame number of plates. One number, containing feven fheets and eight plates, will appear every fecond decade, or one volume of three numbers every two months. The price four livres per number, with the figures plain; or fix, if coloured. The fubfcribers to the firft 500 copies will have the advantage of a deduction of one-fourth. The plates may be had at the price of two livres, plain; or fix, coloured.

Subfcribers pay in advance for one number or volume; and the reft, on delivery, to Cit.

 Fuchs,

Fuchs, bookfeller, rue des Mathurins, hotel de Cluny, à Paris; and at Bâle, by Cit. L. Blondèlu (perhaps Blondelin as above), agrégé à l'Univerfité.

Refutation de la Théorie Pneumatique, &c. A Refutation of the Pneumatic Theory, or the New Doctrine of the Modern Chemifts, prefented Article by Article, in a Series of Replies to the Principles collected and publifhed by Cit. Fourcroy in his Philofophie Chimique: to which is prefixed, a Complementary Supplement to the Theory exhibited in a Work entitled *Recherches fur les Caufes des Principaux Faits Phyfiques;* or, Refearches into the Caufes of the Principal Phyfical Facts, to which this forms a neceffary Continuation. By J. B. Lamarck, of the National Inftitute of France. Octavo, 481 pages. Publifhed at Paris, by the Author, at the Mufeum of Natural Hiftory; and by Agaffe, rue des Poitevins. L'an 4.

I have not yet procured the *Recherches* here mentioned; but, when in poffeffion of both, it will become an object of careful enquiry, whether an abftract of the leading points of difference in this author's theory and that which he attempts to refute may be of value to the fcientific world.

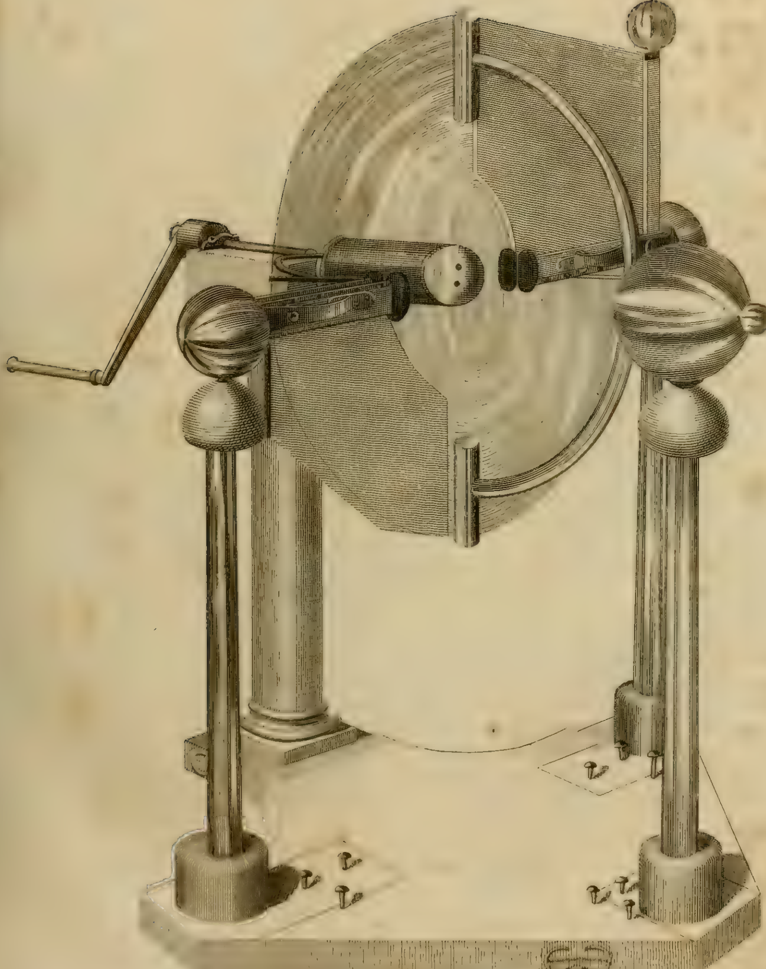
Specimens of Britifh Minerals felected from the Cabinet of Philip Rafhleigh, of Menabilly, in the county of Cornwall, Efq. M. P. F. R. S. and F. A. S. with general defcriptions of each article: 4to—56 pages—33 plates coloured. Nicol and White. Price 2l. 12s. 6d.

This magnificent work difplays with great effect the fpecimens, chiefly confifting of tin, copper and lead ores, with calcareous fpars and quartz. The defcriptions are intended to complete the knowledge, in part and very ftrikingly, conveyed by the plates. Little is faid in general concerning analyfis, or the component parts.

Cit. Prony, of the French National Inftitute, has publifhed the fecond part of the *Nouvelle Architecture Hydraulique.* This part contains the defcription of fteam engines. It is fold by Firmin Didot, No. 116, rue de Thionville. Quarto, 240 pages, with 40 plates. Price 40 liv.

M. Lalande, in the Journal des Sçavans page 125, gives the following account of this work: "Since the appearance of the firft part of the Architecture Hydraulique in 1790, the public has waited with impatience for the continuation. This part is a complete Treatife on Steam Engines. It contains new experiments on the expanfive power of heat; an account of pumps which have the pifton acted on by the fteam alternately on both fides (pompes à double effet), and thofe which are fimple; inftructions for difpofing the latter to act in the manner of the former; the theory of the right-lined motion of the pifton by a combination of circular movements; and a method of interpolation applicable to phenomena which depend on elaftic fluids. The analytic part of this work is no lefs curious than the mechanical and experimental parts. No one but a geometer could have treated thefe fubjects in a manner fo perfpicuous for the learned; nor could a lefs able engineer have rendered them fo fatisfactory to practical men."

Citizen Garnier, Profeffor of Mathematics, has added feveral ufeful explanations of parts of the firft volume of Citizen Prony, which render this work ftill more complete.



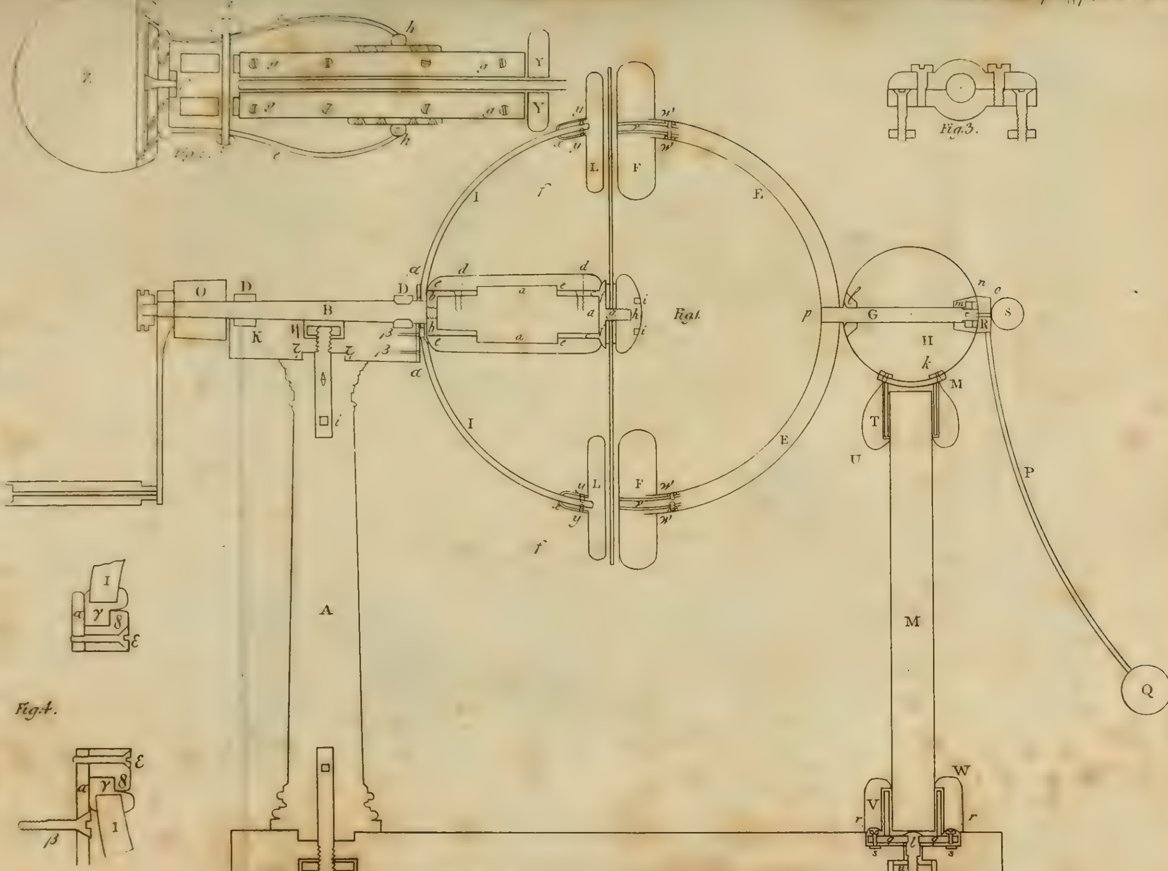


Fig. 1.
Fig. 3.
Fig. 4.
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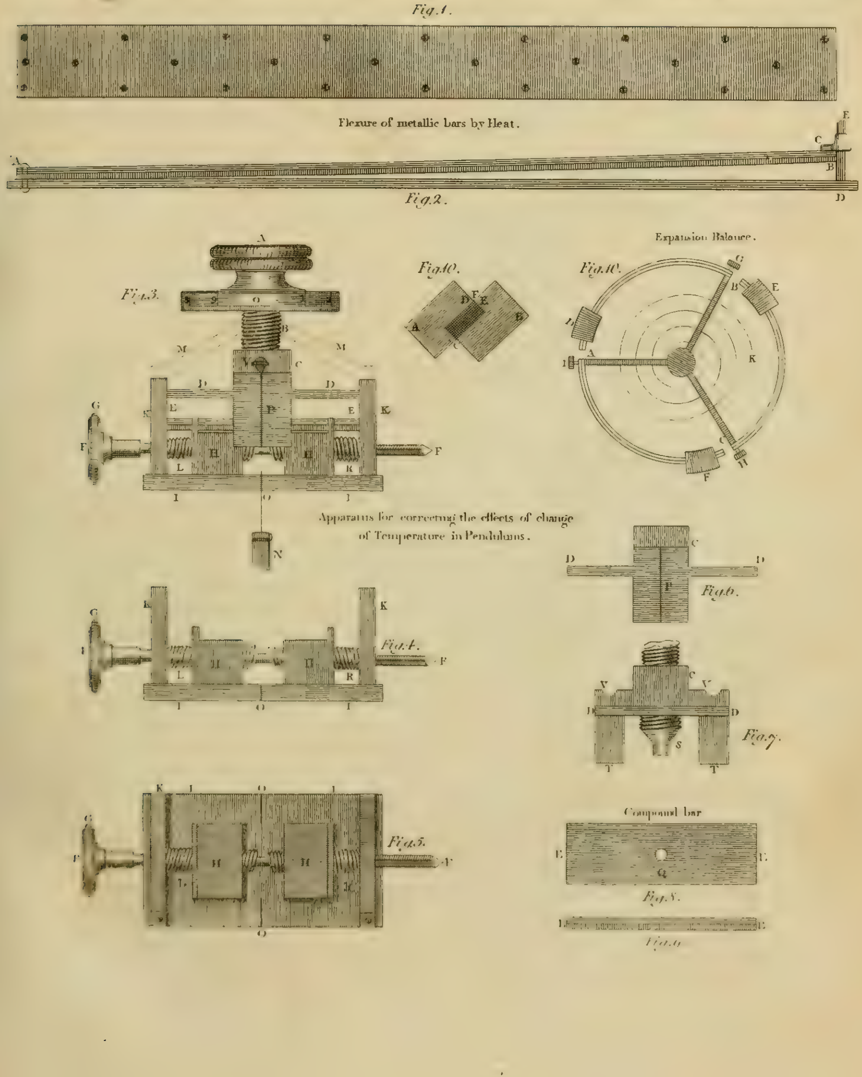
Fig. 1.
Flexure of metallic bars by Heat.
Fig. 2.
Fig. 3.
Fig. 10.
Expansion Balance.
Fig. 10.
Apparatus for correcting the effects of change of Temperature in Pendulums.
Fig. 6.
Fig. 4.
Fig. 7.
Fig. 5.
Compound bar
Fig. 8.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE 1797.

ARTICLE I.

A Letter from M. de HUMBOLDT *to M.* PICTET, *on the Magnetic Polarity of a Mountain of Serpentine* *.

AT the beginning of the eighteenth century the attention of natural philofophers was entirely fixed on the phenomena of magnetifm. The progrefs which has fince been made in the theory of electricity, and the preponderance which chemiftry has acquired over all the other branches of natural hiftory, have diminifhed the intereft with which enquiries into the nature of the magnetic fluid ought to have been purfued. It is true that your celebrated countrymen Meffrs. de Sauffure and Prevôt have given vigour to this purfuit by difcoveries worthy of their fagacity: the firft, by inventing an inftrument capable of meafuring the comparative intenfity of the magnetic forces in different regions of the globe; and the other, by reducing the laws of polarity to the fimple laws of attraction. But thefe difcoveries have not afforded inducement fufficient to lead philofophers into a path fo honourably explored. The moft valuable work on the origin of magnetic forces has been neglected, together with the calculations of the ingenious Coulomb, and his experiments with the balance of Torfion.

Having traverfed with the compafs in my hand great part of the mountains of Europe, I became convinced that declinations caufed by maffes of iron in beds or in veins are infinitely lefs frequent than naturalifts affirm. The obfervations which Meffrs. de Sauffure and Trembley have made on the fummit of Cramont †, appear to me the more curious, as

* I received this communication in manufcript from the Right Hon. Sir Jofeph Banks, Bart. P. R. S. &c. It is written in French, under the title of "Lettre quatrième de M. de Humboldt à M. Pictet, fur la Polarité Magnetique d'une Montagne de Serpentine." The fame liberal promoter of fcience has favoured me with a fpecimen of this rock, with permiffion to make experiments upon it. A few obfervations on this fpecimen are added at the end of this memoir. N.

† Voyage dans les Alpes, T. I. p. 375.—T. II. p. 343.

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it ftands alone, and prefents to our knowledge a very extended image of the dimenfions of magnetic fpheres. It is among the Alps of Sweden and Norway, thofe northern regions which nature has enriched with an enormous depofit of iron lefs oxided than in our country, that we were entitled to expect fimilar phenomena.

I haften to communicate to you a difcovery I made in the month of November, and which appears to me of confiderable importance in the progrefs of geology. You are acquainted, Sir, with the laws and the harmony which I have obferved in the direction and inclination of the primitive ftrata, from the banks of the Mediterranean to thofe of the Baltic Sea. You have even condefcended, jointly with our friend Dolomieu, to exprefs an intereft with regard to this laborious undertaking; which, in more fkilful hands than mine, would, I am well affured, throw great light on the conftruction of the globe. I traverfed the chain of mountains of the High Palatinate and the margraviate of Bayreuth; and I found, in the bottom of the Fichtelgebirge, between Munichberg and Goldcronach, an ifolated hill, which rifes to the elevation of fifty toifes above the furrounding plain. Its height above the level of the fea may be eftimated at two hundred and eighty, or three hundred, toifes. This hill extends in length from weft to eaft, and forms a pyramid extremely obtufe. The rocks which crown the fummit or ridge are compofed of ferpentine of confiderable purity, which, by its colour and foliated fracture, approaches in various parts to the chlorithfchiefer of Werner *(fchiftous chlorite)*. This ferpentine is divided into ftrata rather diftinct, of which the inclination to the north-weft prefents an angle between 60 and 65 degrees. It repofes on a foliated granite, mixed with hornblende; a mixture which we diftinguifh by the name of fyenite. I approached this ferpentine with the compafs, in order to determine more accurately the angle it formed with the meridian. The magnetic needle was in a ftate of continual agitation. I advanced two fteps farther, and beheld that the north pole was entirely turned to the fouth. I called two friends, Meffrs. Godeking and Killinger, who affifted me in my geological purfuits; and we were alike penetrated with that joy which the contemplation of interefting phenomena produces in the minds of thinking men. I fhall not detain your attention by a full recital of our obfervations; but fhall merely prefent the refults, to which I may hereafter make additions, if my occupations fhould not lead me from this part of Germany.

The action of this mountain of ferpentine upon the magnet fhews itfelf in a very curious manner. The uncovered rocks which are feen on the northern flope, and thofe on the declivity towards the fouth, have poles directly oppofite. The former exhibit only fouth poles, and the latter north poles. The whole mafs of foliated ferpentine does not therefore poffefs a fingle magnetical axis, but prefents an infinity of different axes perfectly parallel to each other. This parallelifm alfo agrees with the magnetic axis of the globe, though the poles of the ferpentine are inverted; fo that the northern pole of the hill is oppofed to the fouth pole of the earth. The eaft and weftern flopes prefent what in the theory of magnetifm would be called points of indifference. The magnet does not at this part appear to be in any refpect affected, though the fubftance of the rock differs in no external character from the other parts. It is the fame on the fouth fide of the fummit *. I have obferved not only that the magnetic axes are not difpofed in the fame horizontal plane; but I

* I fufpect an error of the copyift, as the words "Il en eft de même du côté meridional de la fommité" contradict the general defcription immediately preceding. N.

have

have likewife remarked, that two points, of which the action is very ftrong, are joined by rocks which do not exert the leaft attraction. The chemical analyfis of thefe compounds affords the fame refults; and it would be no lefs difficult to difcover any difference of aggregation between them, than between iron which has received the touch, and other iron which had never acquired the magnetic power.

On this occafion a queftion prefents itfelf which cannot be refolved in lefs than half a century. The tables founded on the obfervations of Picard, La Hire, Maraldi, Caffini, and Le Monnier, fhew that the needle has declined fince 1660 towards the weft; and that this declination continues to increafe, though the ofcillations caufed by the heats of the fouth, and the temperature of the feafons, often produce a retrograde courfe. If the magnetic axis of our mountain were aftronomically determined by the culmination of the ftars, whether its direction would remain the fame until the year 1850, or whether its fouth-pole would turn towards the weft, in connection with the variation of the magnetic needle? From our profound ignorance of the caufes of the declination, as well as of moft geological phenomena, it is not in our power to refolve fo complicated a problem.

Other obfervations equally interefting may be made on the identity of magnetic forces. I have difcovered a mafs of rocks which affect the needle at the diftance of twenty-two feet. With an apparatus fimilar to the magnetometer of M. de Sauffure, we might obferve whether the intenfity of the forces of magnetic action remains the fame in winter and in fummer; whether it be ftronger in the morning, at noon, at the folftices, during the aurora borealis, or in an atmofphere loaded with electric fluid? I fuppofe that thefe fame rocks might act on the needle fometimes at 16, and fometimes at 28 feet diftance.

It has been obferved, that metals expofed to the air gradually imbibe the magnetic fluid. A flight oxidation of the iron feems to favour this effect. I have myfelf obferved, that in a magnetic bed of iron thofe parts only which were in contact with the air affected the needle. This phenomenon is confidered as the effect of atmofpheric electricity. I am aware that lightning converts a bar of iron into a magnet; that the difcharge of the Leyden vial fometimes increafes the intenfity of magnetic forces; but I do not fee why the atmofpheric electricity fhould act fimply on the external furface of a bed of magnetic iron, which is a good conductor of the electric fluid. Does not the oxygene of the atmofphere rather act a part in this operation? Without wandering in the fphere of probabilities, I have chofen to adhere to enquiries refpecting facts. I have obferved the rocks which were covered with turf, from which I detached pieces that had not been in contact with the air. I found that the magnetic force was conftantly the fame.

The mountains of the Harz prefent a granite rock called the fchnarcher, which is elevated in the form of a tower, or broken pyramid. This granite likewife affects the needle; but it acts only in the mafs, and in a fingle band or perpendicular vein. Detached pieces fhew no action upon the needle. It is to Mr. de Trebra, celebrated for his refearches concerning the internal parts of mountains, that we are indebted for this important difcovery.

Some philofophers pretend that the fchnarcher contain in their bowels a mafs of magnetic iron; others prefume that a ftroke of lightning has caufed the magnetic vein in thefe mountains.

The nature of the rocks which I have the honour to prefent to your notice in this paper does not admit of fimilar explanations. The ferpentine not only acts in a mafs, in its natural ſituation, but all the pieces, when broken, to infinity, ſtill exhibit two very diſtinct poles. Pieces of five inches diameter act on the needle at the diſtance of half a foot. The examination of the magnetic axes affords an object of curious enquiry. They are moſtly found in a direction parallel to that of the foliated grain; nevertheleſs, I have found ſome which croſs it perpendicularly. Fragments extremely ſmall, of the magnitude of 0.01 of a cubic line, ſhew a very ſtrong polarity in proportion to their maſſes. You ſee them turn very ſuddenly when the poles of the weakeſt magnet are ſucceſſively preſented to them. It is a very ſtriking phenomenon, that a ſtone poſſeſſed of ſo high a degree of polarity ſhould exhibit no attraction for iron which is not magnetiſed. I have never obſerved the ſmalleſt particle of filings of iron adhere to the ſerpentine; but the ſerpentine, reduced to powder, attaches itſelf very readily to the magnet.

You will enquire with impatience, if it be well proved that my ſerpentine is not mixed with magnetic iron; whether this mixture may not be ſufficiently intimate to enter into the compoſition of each particle of the rock? I can aſſure you, that I have made the moſt aſſiduous enquiries in this reſpect. All my experiments were made in conjunction with Mr. Godeking, whoſe knowledge and abilities are a ſufficient aſſurance againſt error; but we were decidedly convinced, that if the magnetic force cannot adhere to the earthy ſubſtances which form the baſe of the ſerpentine, it can be attributed only to the oxide of iron with which it is coloured. Theſe are our reaſons: The rock has no mixture of metallic ſubſtances. It preſents only here and there a few fragments of talc or amianthus; but neither pyrites, nor ſchoerl, nor octahedrons of magnetic iron. When reduced to very fine powder, it reſembles pounded chalk. The microſcope diſcovers only earthy parts, of a clear whitiſh green. The ſpecific gravity of this ſerpentine is very ſmall. I find it only from 1.901 to 2.04 aſſuming water to be 1.0. There are not conſequently any minerals but pumice-ſtone, mountain-leather, and ſome varieties of the opal, which do not equal our ſerpentine in denſity. The chemical experiments we have hitherto made, prove that it contains, like the jade or lapis ollaris, oxided iron; but not iron capable of attraction by the magnet. The ſolutions in muriatic acid, mixed with the nitric acid, are yellow, and not green like thoſe which the micaceous iron, and all the ores which contain pure or metallic iron, afford.

Here then is a very ſtriking phenomenon, namely, the polarity of ſuper-oxigenated iron. We learn by the valuable experiments of my celebrated countrymen Klaproth and Wenzel, that pure nickel and cobalt are attracted by the magnet; we know that iron ſlightly oxided (the black oxide) is alſo affected; but how great the difference between this ſtate of oxidation, and that of the iron which colours the ſerpentine, various calcareous ſtones, and perhaps even certain vegetable matters! What difference between a ſubſtance which acts alike on the two extremities of the needle, and a ſtone of which the ſmalleſt portions exerciſe a ſpontaneous polarity! Let us purſue the path of obſervation; let us collect indubitable facts. By this method the theories of natural philoſophy will be eſtabliſhed on ſolid and durable foundations.

Obſervations on the Stone which was forwarded to Sir JOSEPH BANKS *with the preceding Memoir.*

DESCRIPTION.—IT is of a blueiſh opake black colour, every where interſperſed with minute particles of a yellowiſh rather ſilky white, and of no regular figure. No appearance of ſymmetry or cryſtallization preſents itſelf, except on one ſide, where a rough indication of of laminæ is ſeen. This is in the direction of the magnetic poles. The tenacity is very conſiderable, as it was not broken but by a violent blow on a round flint. Hardneſs between 6 and 7 of Kirwan; that is to ſay, it yields to the knife and file, but greatly injures thoſe inſtruments. Powder white, or greeniſh-white. Fracture coarſe, earthy. Fragments rather angular. No ſmell, nor adheſion to the tongue; but when two pieces are ſtruck together its ſmell is the ſame as is given in like circumſtances by rock cryſtal. Under the hammer it gives fire, but rarely with the ſteel. Specific gravity 2.91. Does not imbibe water. When touched with nitrous acid, a very ſlight effervescence took place, which was general and diſtinct, but ſcarcely viſible without the magnifier. When ſtrongly urged with the blow-pipe, it acquired an irregular light-brown colour, and was much leſs tenacious, but ſuffered no other change. Acid of borax and microcoſmic ſalt diſſolve it with little effervescence, and very ſlowly. The glaſs of the former is clear green, the latter clear white.

Its whole weight was 8238 grains; and that of the piece broken off for experiment, without the contact or uſe of iron, was 614 grains. It weighed 403 grains in water, and conſequently loſt 211 grains. Hence $\frac{614}{211} = 2.91$, or the ſpecific gravity to water aſſumed as 1.00. This differs from the reſults found by Mr. Humboldt.

The fracture was made acroſs the line of magnetical direction. The ſurfaces which had been thus ſeparated, exhibited oppoſite poles, in the ſame manner as when any other natural magnet is broken. The ſmaller piece was then broken into many fragments, and in part pulveriſed. All the pieces were poſſeſſed of polarity. A ſmall five-bar horſe-ſhoe magnet took up the ſmaller fragments, and the powder, though weakly, as they were eaſily ſhaken off. I could not ſatisfactorily aſcertain that any part of the ſtone did either attract the fineſt iron filings, or influence their arrangements when laid upon paper.

A very delicate magnetic needle, 1¾ inches long, moving on an agate ſocket, was ſuffered to diſpoſe itſelf in the magnetic meridian. The larger piece was preſented due eaſt from the centre, and then moved upon its own centre, to find the poſitions of the greateſt attraction and repulſion. At the diſtance of 18 inches from the middle of the ſtone, the needle was perceptibly affected; but its greateſt deviation did not exceed ten minutes of a degree. At 12 inches the deviation was one degree, and at ſix inches it was about 14 degrees. By application of the ſtone cloſe to the compaſs-box, the needle was led into any poſition at pleaſure.

It was found on compariſon with various pieces of natural magnet, that its directive force or effect on the needle is much weaker than that of any of the pieces. But whether a natural magnet as weak in directive power as this ſtone would be equally inactive with regard to iron filings, does not appear from the facts I have hitherto obſerved.

II.

An Account of ſome Experiments upon Coloured Shadows. By Lieutenant General Sir BENJAMIN THOMPSON, *Count of Rumford, F. R. S. In a Letter to Sir* JOSEPH BANKS, *Bart. P. R. S.**

DEAR SIR,

SINCE my laſt letter, being employed in the proſecution of my experiments upon light, I was ſtruck with a very beautiful, and what to me appeared to be a new appearance. De-

* From the Philoſophical Tranſactions, 1794, p. 107.

ſirous

ſirous of comparing the intenſity of the light of a clear ſky by day with that of a common wax-candle, I darkened my room, and letting the day-light from the north, coming through a hole near the top of the window-ſhutter, fall at an angle of about 70° upon a ſheet of very fine white paper, I placed a burning wax-candle in ſuch a poſition that its rays fell upon the ſame paper, and, as near as I could gueſs, in the line of reflection of the rays of day-light from without; when, interpoſing a cylinder of wood, about half an inch in diameter, before the centre of the paper, and at the diſtance of about two inches from its ſurface, I was much ſurpriſed to find that the two ſhadows projected by the cylinder upon the paper, inſtead of being merely ſhades without colour, as I expected, the one of them, that which, correſponding with the beam of day-light, was illuminated by the candle, was yellow; while the other, correſponding to the light of the candle, and conſequently illuminated by the light of the heavens, was of the moſt beautiful blue that it is poſſible to imagine. This appearance, which was not only unexpected, but was really in itſelf in the higheſt degree ſtriking and beautiful, I found upon repeated trials, and after varying the experiment in every way I could think of, to be ſo perfectly permanent, that it is abſolutely impoſſible to produce two ſhadows at the ſame time, from the ſame body, the one anſwering to a beam of day-light and the other to the light of a candle or lamp, without theſe ſhadows being coloured, the one yellow, and the other blue.

The experiment may very eaſily be made at any time by day, and almoſt in any place, and even by a perſon not in the leaſt degree verſed in experimental reſearches. Nothing more is neceſſary for that purpoſe than to take a burning candle into a darkened room, in the day-time, and open one of the window-ſhutters a little, about half or three-quarters of an inch for inſtance; when, the candle being placed upon a table or ſtand, or given to an aſſiſtant to hold, in ſuch a ſituation that the rays from the candle may meet thoſe of day-light from without at an angle of about 40° at the ſurface of a ſheet of white paper, held in a proper poſition to receive them, any ſolid opake body, a cylinder, or even a finger, held before the paper, at the diſtance of two or three inches, will project two ſhadows upon the paper, the one blue and the other yellow.

If the candle be brought nearer to the paper, the blue ſhadow will become of a deeper hue, and the yellow ſhadow will gradually grow fainter; but if it be removed farther off, the yellow ſhadow will become of a deeper colour, and the blue ſhadow will become fainter; and the candle remaining ſtationary in the ſame place, the ſame varieties in the ſtrength of the tints of the coloured ſhadows may be produced merely by opening the window-ſhutter a little more or leſs, and rendering the illumination of the paper, by the light from without, ſtronger or weaker. By either of theſe means, the coloured ſhadows may be made to paſs through all the gradations of ſhade, from the deepeſt to the lighteſt, and *vice verſa*; and it is not a little amuſing to ſee ſhadows thus glowing with all the brilliancy of the pureſt and moſt intenſe priſmatic colours, then paſſing ſuddenly through all the varieties of ſhade, preſerving in all the moſt perfect purity of tint, growing ſtronger and fainter, and vaniſhing and returning, at command.

With reſpect to the cauſes of the colours of theſe ſhadows, there is no doubt but they ariſe from the different qualities of the light by which they are illuminated; but how they are produced does not appear to me ſo evident. That the ſhadow correſponding to the beam of day-light which is illuminated by the yellow light of a candle, ſhould be of a yellowiſh hue, is not ſurpriſing; but why is the ſhadow correſponding to the light of the

2 candle,

candle, and which is illuminated by no other light than the apparently white light of the heavens, blue? I at firſt thought that it might ariſe from the blueneſs of the ſky; but finding that the broad day-light reflected from the roof of a neighbouring houſe covered with the whiteſt new-fallen ſnow produced the ſame blue colour, and, if poſſible, of a ſtill more beautiful tint, I was obliged to abandon that opinion.

To aſcertain with ſome degree of preciſion the real colour of the light emitted by a candle, I placed a lighted wax-candle, well trimmed, in the open air, at mid-day, at a time when the ground was deeply covered with new-fallen ſnow, and the heavens were overſpread with white clouds; when the flame of the candle, far from being white, as it appears to be when viewed by night, was evidently of a very decided yellow colour, not even approaching to whiteneſs. The flame of an Argand's lamp, expoſed at the ſame time in the open air, appeared to be of the ſame yellow hue. But the moſt ſtriking manner of ſhewing the yellow hue of the light emitted by lamps and candles, is by expoſing them in the direct rays of a bright meridian ſun. In that ſituation the flame of an Argand's lamp, burning with its greateſt brilliancy, appears in the form of a dead yellow ſemi-tranſparent ſmoke. How tranſcendently pure and inconceivably bright the rays of the ſun are when compared to the light of any of our artificial illuminators, may be gathered from the reſult of this experiment.

It appearing to me very probable that the difference in the whiteneſs of the two kinds of light which were the ſubjects of the foregoing experiments, might ſomehow or other be the occaſion of the different colours of the ſhadows, I attempted to produce the ſame effects by employing two artificial lights of different colours; and in this I ſucceeded completely.

In a room previouſly darkened, the light from two burning wax-candles being made to fall upon the white paper, at a proper angle, in order to form two diſtinct ſhadows of the cylinder, theſe ſhadows were found not to be in the leaſt coloured; but upon interpoſing a pane of yellow glaſs, approaching to a faint orange colour, before one of the candles, one of the ſhadows immediately became yellow, and the other blue. When two Argand's lamps were made uſe of, inſtead of the candles, the reſult was the ſame: the ſhadows were conſtantly and very deeply coloured, the one yellow, approaching to orange, and the other blue, approaching to green. I imagined that the greeniſh caſt of this blue colour was owing either to the want of whiteneſs of the one light, or to the orange hue of the other, which it acquired from the glaſs.

When equal panes of the ſame yellow glaſs were interpoſed before both the lights, the white paper took an orange hue; but the ſhadows were to all appearance without the leaſt tinge of colour; but two panes of the yellow glaſs being afterwards interpoſed before one of the lights, while only one pane remained before the other, the colours of the ſhadows immediately returned.

The reſult of theſe experiments having confirmed my ſuſpicions that the colours of the ſhadows aroſe from the different degrees of whiteneſs of the two lights, I now endeavoured, by bringing day-light to be of the ſame yellow tinge with candle-light by the interpoſition of ſheets of coloured glaſs, to prevent the ſhadows being coloured when day-light and candle-light were together the ſubjects of the experiment; and in this I ſucceeded. I was even able to reverſe the colours of the ſhadows, by cauſing the day-light to be of a deeper

yellow

yellow than the candle-light. In the courfe of thefe experiments I obferved that different fhades of yellow given to the day-light produced very different and often quite unexpected effects: thus one fheet of the yellow glafs interpofed before the beam of day-light changed the yellow fhadow to a lively violet colour, and the blue fhadow to a light green; two fheets of the fame glafs nearly deftroyed the colours of both the fhadows; and three fheets changed the fhadow which was originally yellow to blue, and that which was blue to a purplifh yellow colour.

When the beam of day-light was made to pafs through a fheet of blue glafs, the colours of the fhadows, the yellow as well as the blue, were improved and rendered in the higheft degree clear and brilliant; but when the blue glafs was placed before the candle, the colours of the fhadows were very much impaired.

In order to fee what would be the confequence of rendering the candle-light of a ftill deeper yellow, I interpofed before it a fheet of yellow or rather orange-coloured glafs; when a very unexpected and moft beautiful appearance took place. The colour of the yellow fhadow was changed to orange, the blue fhadow remained unchanged, and the whole furface of the paper appeared to be tinged of a moft beautiful violet colour, approaching to a light crimfon or pink; almoft exactly the fame hue as I have often obferved the diftant fnowy mountains and valleys of the Alps to take about fun-fet. Is it not more than probable that this hue is in both cafes produced by nearly the fame combinations of coloured light? In the one cafe it is the white fnow illuminated at the fame time by the pureft light of the heavens, and by the deep yellow rays from the weft; and in the other it is the white paper illuminated by broad day-light, and by the rays from a burning candle, rendered ftill more yellow by being tranfmitted through the yellow glafs. The beautiful violet colour which fpreads itfelf over the furface of the paper will appear to the greateft advantage if the pane of orange-coloured glafs be held in fuch a manner before the candle, that only a part of the paper, half of it, for inftance, be affected by it, the other half remaining white.

To make thefe experiments with more convenience, the paper, which may be about eight or ten inches fquare, fhould be pafted or glued down upon a flat piece of board, furnifhed with a ball and focket upon the hinder fide of it, and mounted upon a ftand, and the cylinder fhould be faftened to a fmall arm of wood or of metal, projecting forward from the bottom of the board for that purpofe. A fmall ftand, capable of being made higher or lower as the occafion requires, fhould likewife be provided for fupporting the candle; and if the board with the paper faftened upon it be furrounded with a broad black frame, the experiments will be fo much the more ftriking and beautiful. For ftill greater convenience, I have added two other ftands for holding the coloured glafs through which the light is occafionally made to pafs in its way to the white furface upon which the fhadows are projected. It will be hardly neceffary to add, that, in order to the experiments appearing to the greateft advantage, all light which is not abfolutely neceffary to the experiment muft be carefully fhut out.

Having fitted up a little apparatus, according to the above directions, merely for the purpofe of profecuting thefe enquiries refpecting the coloured fhadows, I proceeded to make a great variety of experiments, fome with pointed views, and others quite at random, and merely in hopes of making fome accidental difcovery that might lead to a knowledge of the caufes of appearances which ftill feemed to me to be enveloped in much obfcurity and uncertainty.

Having

Having found that the ſhadows correſponding to two like wax candles were coloured, the one blue and the other yellow, by interpoſing a ſheet of yellow glaſs before one of them; I now tried what the effect would be when blue glaſs was made uſe of inſtead of yellow, and I found it to be the ſame; the ſhadows were ſtill coloured, the one blue and the other yellow, with the difference, however, that the colours of the ſhadows were reverſed, that which with the yellow glaſs was before yellow being now blue, and that which was blue being yellow.

I afterwards tried a glaſs of a bright amethyſt colour, and was ſurpriſed to find that the ſhadows ſtill continued to be coloured blue and yellow. The yellow, it is true, had a dirty purple caſt; but the blue, though a little inclining to green, was nevertheleſs a clean, bright, decided colour.

Having no other coloured glaſs at hand to puſh theſe particular enquiries farther, I now removed the candles, and, opening two holes in the upper parts of the ſhutters of two neighbouring windows, I let into the room, from above, two beams of light from different parts of the heavens; and placing the inſtrument in ſuch a manner that two diſtinct ſhadows were projected by the cylinder upon the paper, I was entertained by a ſucceſſion of very amuſing appearances. The ſhadows were tinged with an infinite variety of the moſt unexpected, and often moſt beautiful colours, which, continually varying, ſometimes ſlowly and ſometimes with inconceivable rapidity, abſolutely faſcinated the eyes, and, commanding the moſt eager attention, afforded an enjoyment as new as it was bewitching. It was a windy day, with flying clouds; and it ſeemed as if every cloud that paſſed brought with it another complete ſucceſſion of varying hues and moſt harmonious tints. If any colour could be ſaid to predominate, it was purple; but all the varieties of browns, and almoſt all the other colours I ever remembered to have ſeen, appeared in their turns; and there were even colours which ſeemed to me to be perfectly new.

Reflecting upon the great variety of colours obſerved in theſe laſt experiments, many of which did not appear to have the leaſt relation to the apparent colours of the light by which they were produced, I began to ſuſpect that the colours of the ſhadows might in many caſes, notwithſtanding their apparent brilliancy, be merely an optical deception, owing to contraſt, or to ſome effect of the other neighbouring colours upon the eye. To determine this fact by a direct experiment, I proceeded in the following manner: Having, by making uſe of a flat ruler, inſtead of the cylinder, contrived to render the ſhadows much broader, I ſhut out of the room every ray of day-light, and prepared to make the experiment with two Argand's lamps, well trimmed, and which were both made to burn with the greateſt poſſible brilliancy; and having aſſured myſelf that the light they emitted was preciſely of the ſame colour, by the ſhadows being perfectly colourleſs which were projected upon the white paper, I directed a tube about 12 inches long, and near an inch in diameter, lined with black paper, againſt the centre of one of the broad ſhadows; and looking through this tube with one eye, while the other was cloſed, I kept my attention fixed upon the ſhadow, while an aſſiſtant repeatedly interpoſed a ſheet of yellow glaſs before the lamp, whoſe light correſponded to the ſhadow I obſerved, and as often removed it. The reſult of the experiment was very ſtriking, and fully confirmed my ſuſpicions with reſpect to the fallacy of many of the appearances in the foregoing experiments. So far from being able to obſerve any change in the ſhadow upon which my eye was fixed, I was not able even

even to tell when the yellow glass was before the lamp, and when it was not; and though the assistant often exclaimed at the striking brilliancy and beauty of the blue colour of the very shadow I was observing, I could not discover in it the least appearance of any colour at all. But as soon as I removed my eye from the tube, and contemplated the shadow with all its neighbouring accompaniments, the other shadows rendered really yellow by the effect of the yellow glass, and the white paper, which had likewise from the same cause acquired a yellowish hue, the shadow in question appeared to me, as it did to my assistant, of a beautiful blue colour. I afterwards repeated the same experiment with the apparently blue shadow produced in the experiment with day-light and candle-light, and with exactly the same result.

How far these experiments may enable us to account for the apparent blue colour of the sky, and the great variety of colours which frequently adorn the clouds, as also what other useful observations may be drawn from them, I leave to philosophers, opticians, and painters to determine. In the mean time, I believe it is a new discovery; at least it is undoubtedly a very extraordinary fact, that the eyes are not always to be believed, even with respect to the presence or absence of colours.

I cannot finish this letter without mentioning one circumstance which struck me very forcibly in all these experiments upon coloured shadows, and that is, the most perfect harmony which always appeared to subsist between the colours, whatever they were, of the two shadows; and this harmony seemed to me to be full as perfect and pleasing when the shadows were of different tints of brown, as when one of them was blue and the other yellow. In short, the harmony of these colours was in all cases not only very striking, but the appearances were altogether quite enchanting; and I never found any body to whom I shewed these experiments, whose eyes were not fascinated with their bewitching beauties. It is however more than probable, that a great part of the pleasure which these experiments afforded to the spectators, arose from the continual changes of colour, tint, and shade with which the eye was amused, and the attention kept awake. We are used to seeing colours fixed and unalterable, hard as the solid bodies from which they come, and just as motionless; consequently dead, uninteresting, and tiresome to the eye; but, in these experiments, all is motion, life and beauty.

It appears to me very probable, that a further prosecution of these experiments upon coloured shadows may not only lead to a knowledge of the real nature of the harmony of colours, or the peculiar circumstances upon which that harmony depends; but that it may also enable us to construct instruments for producing that harmony for the entertainment of the eyes, in a manner similar to that in which the ears are entertained by musical sounds. I know that attempts have already been made for that purpose; but when I consider the means employed, I am not surprised that they did not succeed. Where the flowing tide, the varying swell, the crescendo is wanting, colours must ever remain hard, cold, and inanimate masses.

I am very sorry that my more serious occupations do not at present permit me to pursue these most entertaining enquiries. Perhaps at some future period I may find leisure to resume them *.

I am, &c.

Munich, March 1, 1793.

* Otto Guericke, Buffon, Mazeas, Beguelin, and many other philosophers, have paid attention to the coloured shadows of bodies which form the subject of this interesting letter. An abridgement of their observations

and

III.

A Memoir upon the Diſcovery of America. By M. OTTO.

[Concluded from page 77.]

AFTER having performed ſeveral other intereſting voyages, the Chevalier Behem died at Liſbon in July 1506, regretted by every one, but leaving behind him no other work than the globe which we have juſt been ſpeaking of. It is made from the writings of Ptolemy, Pliny, Strabo, and eſpecially from the account of Mark Paul, the Venetian, a celebrated traveller of the thirteenth century; and of John Mandeville, an Engliſhman, who, about the middle of the fourteenth century, publiſhed an account of a journey of thirty-three years in Africa and Aſia. He has alſo added the important diſcoveries made by himſelf on the coaſts of Africa and America.

From theſe circumſtantial accounts, little known to modern writers, we muſt conclude that Martin Behenira, of whom Garcilaſſo makes mention, is the ſame Chevalier Behem, upon being the place of whoſe birth Nurenberg prides itſelf ſo much. It is probable that, as ſoon as he was knighted in Portugal, he thought it neceſſary to give a Portugueſe termination to his name, to make it more ſonorous and more conformable to the idiom of the country. Garcilaſſo, deceived by this reſemblance of ſound, has made him a Spaniard, in order to deprive Chriſtopher Columbus of the honour of having procured to his country ſo great an advantage. And what ought to confirm us in this opinion is, that we neither find in Mariana, nor any other Spaniſh hiſtorian, the name of this Martin Behenira, who was certainly a man of too much importance not to have had a diſtinguiſhed place in hiſtory. Beſides, the Spaniſh pride would have been flattered in giving to a native thoſe laurels with which it crowned Chriſtopher Columbus.

It is then very unlikely that this navigator was treated as an enthuſiaſt, when he offered to the Court of Portugal to make diſcoveries in the weſt. The ſearch after unknown countries was at that time the reigning paſſion of this Court; and even if the Chevalier Behem had not offered the intereſting ideas which he had procured, the novelty of the project would undoubtedly have engaged King John to ſupport the views of Columbus: but it appears that this prince declined it, becauſe all his thoughts were turned at that time to the coaſt of Africa, and the new paſſage to the Indies, from whence he procured great riches; whilſt the ſouthern coaſt of Brazil, and the territories of the Patagonians, ſeen by Behem, offered to him only barren lands inhabited by unconquerable ſavages. The refuſal of John II., very far from weakening the teſtimony of Behem's diſcoveries, is then

and deductions may be ſeen in Prieſtley's Optics, p. 436. Count Rumford is the firſt, as far as I know, who has ſhewn that the effect depends not immediately on the nature of the light, but principally upon the manner in which the organs of ſight, or perhaps the organs of thought (if in truth there be any difference here between them), are affected by the ſucceſſive actions they undergo. The colours called accidental, which are rendered permanent for a time after the impreſſion of bright objects upon the eye, and the effects of leſs forcible impreſſions, for which the laſt quoted work, p. 631, and the authors there referred to, may be conſulted;—the harmony and diſcord of colours, probably ariſing from the pleaſure or diſguſt afforded by the admixture of an accidental colour with a new ſenſation, or real colour;—the phenomena of dazzling, which is of the ſame nature as this laſt combination;—and the general arrangement and inferences to be found in the Zoonomia of Darwin, all bear evident relation to the facts exhibited by Count Rumford, and open a wide field for curious reſearch. N.

rather a proof of the knowledge which this politic prince had already procured of the existence of a new continent: and it was only in 1501, that is to say, three years after the voyage of Vasco de Gama to the Indies, that Emanuel thought proper to take advantage of the discoveries of Behem, by sending Albarez Cabral to Brazil; a measure which was perhaps rather owing to the jealousy which has always existed between Portugal and Spain, than to a desire of making advantageous establishments, for which the Indies were much more proper than this part of America.

If any doubts yet remain respecting the important discovery made by the Chevalier Behem, it is particularly the authority of Dr. Robertson which attacks the testimony of the different authors we have transcribed. This learned writer treats the history of Behem as a fiction of some German authors, who had an inclination to attribute to one of their countrymen a discovery which has produced so great a revolution in the commerce of Europe. But he acknowledges, nevertheless, after Herrera, that Behem had settled at the island of Fayal; that he was the intimate friend of Christopher Columbus; and that Magellan had a globe made by Behem, by the help of which he undertook his voyage to the South Sea; a circumstance which proves much in favour of our hypothesis. He relates also, that in 1492 this astronomer paid a visit to his family at Nurenberg, and left there a map drawn by himself, of which Dr. Forster procured him a copy, and which, in his opinion, partakes of the imperfection of the cosmographical knowledge of the 15th century: that he found in it, indeed, under the name of the island of St. Brandon, land which appears to be the present coast of Guiana, and lies in the latitude of Cape Verd; but that there is reason to believe that this fabulous island, which is found in many ancient maps, merits no more attention than the childish legend of St. Brandon himself. Although Dr. Robertson does not appear disposed to grant to Behem the honour of having discovered the new continent, we find the means of refuting him in his own History. He allows that Behem was very intimate with Christopher Columbus; that he was the greatest geographer of his time, and scholar of the celebrated John Müller, or Regiomontanus; that he had discovered, in 1483, the kingdom of Congo upon the coast of Africa; that he made a globe which Magellan made use of; that he drew a map at Nurenberg, containing the particulars of his discoveries; and that he placed in this chart land which is found to be in the latitude of Guiana. Dr. Robertson asserts, without any proof, that this land was but a fabulous island. We may suppose, upon the same foundation, that the Chevalier Behem, engaged in an expedition to the kingdom of Congo, was driven by the winds to Fernambeuc, and from thence by the currents very common in those latitudes towards the coast of Guiana; and that he took for an island the first land which he discovered. The course which Christopher Columbus afterwards steered makes this supposition still more probable; for if he knew only of the coast of Brazil, which they believe to have been discovered by Behem, he would have laid his course rather to the south-west. The expedition to Congo took place in 1483: it is then possible that at his return Behem proposed a voyage to the coasts of Brazil and Patagonia; and that he requested the assistance of his sovereign, which we have mentioned above. It is certain that we cannot have too much deference for the opinion of so eminent a writer as Robertson; but this learned man not having it in his power to consult the original German documents which we have quoted, we may be allowed to form a different opinion without being too presumptuous.

4 But

But ſhould it be aſked, Why we take from Chriſtopher Columbus the reputation which all Europe has to this day allowed him?—why ſearch into the archives of an imperial city for the cauſes of an event which took place in the moſt weſtern extremity of Europe?—why the enemies of Chriſtopher Columbus, who were numerous, did not take advantage of the pretended Chevalier Behem to leſſen his conſequence at the Spaniſh Court?—why Portugal, jealous of the diſcovery of the New World, has not proteſted againſt the aſſertions of the Spaniards?—why Behem, who died only in 1506, had not left to poſterity any writing to confirm to himſelf ſo important a diſcovery?

To anſwer all theſe queſtions, I ſhall ſubmit to the impartial reader the following remarks:

1. Before Columbus, the great merit of a navigator conſiſted rather in conceiving the poſſibility of the exiſtence of a new continent, than in ſearching for lands in a region where he was ſure to find them. If it is then certain that Behem had conceived this bold idea before Columbus, the fame of the latter muſt be conſiderably diminiſhed.

2. The hiſtorical proofs which we have given above leaving us no doubt of the fact, we have only to explain the moral cauſes of the ſilence of the Spaniſh and Portugueſe authors, of the enemies of Columbus, and of Behem himſelf.

3. It is well known that, previous to the reign of Charles V. there was little communication between the learned men of different nations. Writers were ſcarce, excepting ſome monks, who have related, well or ill, the events which came to their knowledge, in chronicles which are no longer read; or they had but little notion of what paſſed in foreign countries. Gazettes and Journals were unknown, and the Learned were obliged to travel, to inform themſelves of the progreſs of their neighbours. Italy was the centre of the arts, and what are called ſciences, at that time. The frequent journeys of the German emperors to Rome gave them an opportunity of knowing perſons of merit, and of placing them in the different univerſities of the empire. It is to this circumſtance that we ought to attribute the great progreſs which the Germans made, particularly in mathematics, from the fourteenth to the ſixteenth century; during which time they had the beſt geographers, the beſt hiſtorians, and the moſt enlightened politicians. They were particularly attentive to what paſſed in Europe; and the multiplied connections of different princes with foreign powers aſſiſted them greatly in collecting, in their archives, the original pieces of the moſt important events of Europe. It is to this ſpirit of criticiſm and enquiry that we are indebted for the reformation of Luther; and we cannot deny that, particularly in the fifteenth century, there was more hiſtorical and political knowledge in Germany than in all the reſt of Europe, Italy excepted. It is not then aſtoniſhing that we ſhould find, in the archives of one of the moſt ancient imperial cities, the particulars of an expedition planned upon the banks of the Tagus, by a German, a man of great repute in his own country, and whoſe every action became very intereſting.

4. It was different in Portugal, where the whole nation, except the king, was plunged in the moſt profound ignorance. Every one was either merchant, ſailor or ſoldier; and if this nation has made the moſt important diſcoveries, we muſt aſcribe them rather to avarice than to a deſire of knowledge. They were ſatisfied with ſcraping together gold in every quarter of the known world, whilſt the German and the Italian took up the pen to tranſmit to poſterity the remembrance of their riches and cruelties. The Spaniards were not

not much more informed before Charles V. introduced at Madrid the learned men of Flanders and Germany. It is then very poſſible that the Chevalier Behem made very intereſting diſcoveries in geography in 1485, without the public being acquainted with them. If he had brought back from his expedition gold or diamonds, the news would have been ſpread in a few weeks; but ſimple geographical knowledge was not of a nature to intereſt men of this turn of mind.

5. The long ſtay which Chriſtopher Columbus made at Madeira makes his interview with Behem more than probable. It is impoſſible that he ſhould have neglected ſeeing a man ſo intereſting, and who could give him every kind of information for the execution of the plan he had formed. The mariners who accompanied the Chevalier Behem might alſo have ſpread reports at Madeira and the Azores concerning the diſcovery which they had been witneſſes of. What ought to confirm us in this is, that Mariana ſays himſelf (book xxvi. chap. iii.) that a certain veſſel going to Africa was thrown by a gale of wind upon certain unknown lands; and that the ſailors, at their return to Madeira, had communicated to Chriſtopher Columbus the circumſtances of their voyage. All authors agree that this learned man had ſome information reſpecting the weſtern ſhores; but they ſpeak in a very vague manner. The expedition of the Chevalier Behem explains this myſtery.

6. This aſtronomer could not be jealous of the diſcoveries of Columbus, becauſe the laſt had been farther north, and that in a time when they did not know the whole extent of the New World; and when geographical knowledge was extremely bounded, it might be believed that the country diſcovered by Columbus had no connection with that diſcovered by Behem.

It appears however certain, that Behem diſcovered this continent before Columbus; and that this queſtion, which is only curious in Europe, becomes intereſting to the American patriot. The Grecians have carefully preſerved the fabulous hiſtory of their firſt founders, and have raiſed altars to them: Why are not Behem, Chriſtopher Columbus, and Veſpucius deſerving of ſtatues in the public ſquares of American cities? Theſe precious monuments would tranſmit to poſterity the gratitude which theſe benefactors of mankind ſhould inſpire. Without knowing it, they have laid the foundation of the happineſs of many millions of inhabitants; and Seſoſtris, Phul, Cyrus, Theſeus, and Romulus, the founders of the greateſt empires, will be forgotten before the ſervices rendered by theſe illuſtrious navigators can be effaced from the memory of man.

IV.

Deſcription of a Gravimeter, or Inſtrument for meaſuring the Specific Gravity of Solids and Fluids. By Citizen GUYTON *.

EVER ſince the art of chemiſtry, by an approach to the accurate ſciences, has ſhewn that the phenomena of combinations produced or deſtroyed are not the reſult of occult qualities, but of a rupture of equilibrium determined by forces which afford the hope of admeaſurement by computation, philoſophers have been aware of the neceſſity of conducting their experiments with preciſion, that they might take account of all the circumſtances

* Read to the National Inſtitute on the 11th Germinal, in the 4th year of the Republic. It is inſerted in the twenty-firſt volume of the Annales de Chimie. This tranſlation is nearly verbal.

which

which might impede or favour this motion. The ſpecific gravity of bodies muſt neceſſarily conſtitute part of thoſe obſervations: for it ſerves not only to indicate the nature of bodies under examination, but likewiſe affords information reſpecting their purity, and the ſtate of their aggregation, condenſation, or rarefaction, which are alſo the immediate cauſes of divulſion or repoſe. It is therefore of importance that the inſtruments for meaſuring this property of bodies ſhould be brought to the utmoſt perfection they are capable of receiving, and that they ſhould be rendered as convenient as poſſible for habitual uſe.

Of all the hydrometers hitherto known, that of Fahrenheit is acknowledged to be the moſt accurate. The well known principle of this inſtrument is, that it aſcertains the weight of a conſtant bulk of the ſeveral fluids under examination. Such hydrometers as are conſtructed to meaſure the denſity by the degree of immerſion, may ſerve, in ſome manufactories, to give an approximation ſufficiently near for the purpoſe required. But without conſidering the inequality of the ſtem, the tedious work of graduating by obſervation, and the uncertainty of eſtimate from one diviſion to another, it may be remarked, that they are not capable of correction for the different temperatures. In a word, they are not fit for the hand of the philoſopher.

The form which Nicholſon gave ſome years ago to the hydrometer of Fahrenheit *, rendered it proper to meaſure the denſity of ſolids. At preſent it is very much uſed. It gives, with conſiderable accuracy, the ratio of the ſpecific gravity to the fifth decimal, water being taken as unity. It is ſuſceptible of correction for the variations of temperature, and the impurity of the water which it is ſometimes more convenient to uſe, as may be ſeen in the article *Areometer*, in the chemical part of the Encyclopedie Methodique. It does not appear that any better inſtrument need be wiſhed for in this reſpect.

But this hydrometer has hitherto been conſtructed in metal only; ſo that it could not be applied either to ſalts or acids. It is known likewiſe that areometers conſtructed on the principles of Fahrenheit, for ſpirituous, ſaline, and acid liquors, require to be varied in magnitude, form, and quantity of ballaſt. In the one kind, the lower weight muſt be at a great diſtance from the buoyant part, to maintain the vertical ſituation; in the other, it is brought nearer, to operate on ſmaller maſſes of the fluid. Thoſe inſtruments which are intended for alcohol muſt be light, and thoſe applied to determine the denſity of concentrated acids muſt be heavy. The maſs and dimenſions of every inſtrument of this nature muſt be adjuſted in ſuch a manner that the additional weight may operate not only as a load, but as ballaſt. It cannot be applied in the upper baſon without deranging the vertical ſituation. From all which circumſtances it happens, that a collection of theſe inſtruments would be required, to anſwer every purpoſe of experiment. As a remedy for part of theſe inconveniences, it was ingeniouſly propoſed that the areometer ſhould terminate beneath in a hook, to which were ſuſpended at pleaſure certain glaſs balls filled with mercury,

* From Lowthorpe's Abridgement of the Philoſophical Tranſactions, I. 604, or Boyle's Works, in quarto, London, 1772, IV. 204, it appears that the hydrometer was firſt invented by Boyle, and deſcribed under the name of a new Eſſay Inſtrument. It had a graduated ſtem, and, by means of a ſtirrup or clip underneath, it was applied, as perfectly as a graduated inſtrument could be, to aſcertain the ſpecific gravities of ſolids as well as fluids. Fahrenheit firſt applied a diſh for weights at the top, to aſcertain the ſpecific gravities of fluids only; as may be ſeen in Read and Gray's Abridgement of the Philoſophical Tranſactions, Vol. VI. Part I. p. 294. My inſtrument, referred to by Citizen Guyton, is a combination of both. It is deſcribed in the 2d volume of the Mancheſter Memoirs, and is repreſented in Plate VI. Fig. 1. N.

and

and conftituting different ballafts; but this did not anfwer in every refpect. In all the inftruments of this kind which I have had occafion to examine, no attempt had been made, or at leaft the object had not been accomplifhed, to render the point of interfection of the ftem with the fluid, common to all the ballafts. Whence it followed, that, upon changing the ballaft, it was likewife neceffary to change the flip of glafs which carries the mark within the upper ftem.

I have thought it poffible, by following the principles of Fahrenheit and executing the inftrument of Nicholfon in glafs with a flight addition, to render it more generally ufeful and commodious, without diminifhing its accuracy in any refpect. I was well aware of the prejudice which is naturally entertained againft polychreft, or univerfal inftruments, moft of which are rendered of no ufe from the attempt to extend their application too far; but at the fame time I was convinced that it would be a real advantage to every philofophical obferver, to require only one fingle meafure for determining the denfity of all bodies, whether folid or liquid. This is the object I propofe to accomplifh. It will be feen how far I have fucceeded. I muft obferve, on this occafion, that the name hydrometer *(pefe-liqueur)* as well as that of areometer, is fcarcely applicable to an inftrument poffeffing thefe qualities: for thefe terms fuppofe that the liquid is always the thing weighed; whereas, with regard to folids, the liquid is the term of comparifon which is known, and to which the unknown weight is referred. I propofe therefore to make ufe of the name gravimeter, which will eafily be underftood, and applied with propriety in every cafe.

This inftrument, being executed, as already remarked, in glafs, is of a cylindric form, being that which requires the fmalleft quantity of the fluid, and is on that account preferable, except fo far as it is neceffary to deviate for the fecurity of a perpendicular pofition.

Like the inftrument of Nicholfon, it carries two bafons; the one fuperior, at the extremity of a thin ftem; towards the middle of which the fixed point of immerfion is marked. The other lower bafon terminates in a point; it contains the ballaft, and is attached to the cylinder by two branches. The moveable fufpenfion by means of a hook has the inconvenience of fhortening the lever which is to fecure the vertical pofition.

The cylinder is 22 millimeters (0.71 inches) in diameter; and 21 centimeters (6.85 inches) in length. It carries in the upper bafon an additional conftant weight of five grammes. Thefe dimenfions might be increafed, fo as to render it capable of receiving a much more confiderable weight; but it will hereafter be fhewn, that this is unneceffary.

I have added a piece which I call the diver *(plongeur)*, becaufe in fact it is placed in the lower bafon when ufed, and confequently is entirely immerfed in the fluid *. It is a bulb of glafs, loaded with a fufficient quantity of mercury, in order that its total weight may be equal to the conftant additional weight, added to the weight of the volume of water difplaced by this piece.

It will readily be underftood, that the weight being determined at the fame temperature at which the inftrument was originally adjufted, it will fink to the fame mark on the ftem, whether it be loaded with a conftant additional weight in the upper bafon, or whether the effect of this weight be produced by the additional piece in the lower difh.

From this explanation there will be no difficulty in deducing how this inftrument may be adapted to every cafe of practice.

* As we have no word in the Englifh language which can with propriety be ufed as the tranflation of the word *plongeur*, I fhall take the liberty to call this weight the additional piece. N.

It

It may be uſed, 1. for ſolids. It is the hydrometer of Nicholſon, from which it differs in no reſpect. The only condition will be, as in his inſtrument, that the abſolute weight of the body to be examined ſhall be rather leſs than the conſtant additional weight, which in this inſtrument is five grammes (115 grains).

2. For liquids of leſs ſpecific gravity than water, the inſtrument, without the additional weight above, weighs about two decagrammes (459 grains) in the dimenſions before laid down. It would be eaſy to limit its weight to the utmoſt accuracy. We have therefore the range of one-fifth of buoyancy, and conſequently the means of aſcertaining all the intermediate denſities from water to the moſt highly rectified ſpirit of wine, which is known to bear in this reſpect the ratio of eight to ten with regard to water.

3. When liquids of greater ſpecific gravity than water are to be tried, the conſtant weight being applied below, by means of the additional piece, which weighs about ſix grammes (138 grains), the inſtruments can receive in the upper baſon more than four times the uſual additional weight, without loſing the equilibrium of its vertical poſition. In this ſtate it is capable of ſhewing the ſpecific gravity of the moſt concentrated acids.

4. It poſſeſſes another property common to the inſtrument of Nicholſon, namely, that it may be uſed as a balance to determine the abſolute weight of ſuch bodies as do not exceed its additional load.

5. Laſtly, the purity of the water being known, it will indicate the degrees of rarefaction and condenſation in proportion to its own bulk.

I have little to ſay reſpecting the conſtruction of this inſtrument. Every workman in glaſs who ſhall once ſee it, will be able to make it without difficulty. The additional piece for the lower baſon will require ſome attention to make it perfectly agree with the conſtant upper weight, as to the immerſion of the inſtrument. But this object may, by careful adjuſtment, be aſcertained with the utmoſt certainty and accuracy.

The bulb of glaſs is for this purpoſe drawn out to a fine point; a ſufficient quantity of mercury is then introduced to ſink it, and the aperture cloſed with a morſel of wax. The bulb being then placed in the lower baſon of the inſtrument, the upper baſon is to be loaded until the mark on the ſtem becomes accurately coincident with the ſurface of the water. The ſum of the weights added above is preciſely equal to that of the quantity of mercury neceſſary to be added to that in the glaſs bulb; which done, nothing more is neceſſary than to ſeal the point by fuſion, taking care not to change its bulk.

Though this inſtrument is rather delicate in form, it has no other imperfection than the natural brittleneſs of the material, which muſt neceſſarily be uſed for experiments with ſaline and acid liquors. For ſix months paſt I have made very frequent uſe of one of theſe inſtruments in the Polytechnic School, without any other inconvenience than that one of the branches of the lower baſon was accidentally broken.

Nothing more remains but to render it portable. I apprehend that this object is ſufficiently ſecured by means of a caſe in which all the delicate parts are ſecured from preſſure, and the heavier parts ſupported in ſuch a manner as to reſiſt the exceſs of motion they are capable of acquiring by virtue of their maſs. This laſt circumſtance is frequently overlooked by ſuch workmen as are employed in the package of inſtruments; whence it neceſſarily follows, that ſome ſtrain or fracture muſt be produced when matters of very unequal denſity are expoſed to receive a common impulſe.

The method of securing this instrument in its case will be better understood from the figure, than from the most extended verbal description.

ADDITION to the foregoing MEMOIR.

THE constant use I have made of the gravimeter since I presented the description to the National Institute, has led me to make some slight changes, by which its construction is rendered more easy, and the effects of brittleness in one of its principal parts are removed.

It has also appeared to be of some utility to annex to this memoir the formula, by means of which, the gravimeter being once well adjusted, we are enabled, by a very simple calculation, *to find the specific gravity of any substance whatever in proportion to that of distilled water at the temperature of* 12.5 *degrees of the decimal thermometer, and* 757.7 *millimeters of pressure, without the use either of distilled water, or the thermometer or barometer.* The astonishment which I have sometimes remarked when the problem has been thus announced, induces me to think that the solution here added will be acceptable, instead of the reference to the article *Areometer* of the Dictionary of Chemistry, in the Encyclopédie Methodique.

I shall likewise add an explanation of the figures which represent the instrument and its case, together with examples of the application of the gravimeter to tables of specific gravity.

Description of a Solid Additional Piece for the Lower Bason.

THE condition of hermetically sealing the additional piece without changing its volume necessarily requires that the part near the aperture should be very thin. Hence it has sometimes happened that the point was broken without any external blow, but merely in consequence of the motion of the mercury contained within. A similar glass bulb might indeed be added; but this constitutes but a small part of the remedy. For it is necessary to adjust the instrument again to preserve the property of measuring the specific gravity of the denser fluids, and it has been found that this operation was not exempt from difficulties when the desired degree of precision was to be sought.

I have remedied this inconvenience by substituting in the place of the glass bulb loaded with mercury a small mass of solid glass, as the stopper of a bottle, which is first brought to the proper form by grinding, and afterwards carefully diminished, until, when placed in the lower bason of the instrument, its immersion in distilled water at the required degrees of temperature and pressure shall be exactly the same as when the instrument is floated in the same liquid with its constant additional weight in the upper bason only.

By this means there is a certainty of acquiring the utmost degree of precision at first trial; because the whole process is reduced to the mere adjustment of a weight.

Concerning the Application of the Gravimeter, to find the Specific Gravity of any Substance whatever, without requiring Distilled Water, or the Thermometer or Barometer, or any subsequent Correction.*

THE gravimeter being supposed to be well regulated, let x represent the specific gravity sought; b the additional weight necessary to sink the instrument to the mark in the unknown fluid;

* There is a correction overlooked by the celebrated author of this paper, and by almost every writer on this subject. It arises from the expansion or contraction of the instrument itself, and of the solid under examination.

fluid; c the weight which placed with the folid in the upper bafon immerfes the inftrument to the mark; d the additional weight to produce the fame effect when the body is in the lower bafon; Π the fpecific gravity of diftilled water, at the temperature of 12.5 degrees of the decimal thermometer, and the preffure of 757.7 millimeters $= 1$; Π' the fpecific gravity of the water made ufe of.

The following formula gives the folution*: $x = \frac{(b-c)\,\Pi'}{d-c}$

The value of Π' is firft to be found, which is greater than unity when the water made ufe of is heavier than diftilled water, and in the contrary cafe is a fraction.

Let P reprefent the weight of the gravimeter without any additional weight †; V the conftant volume of the immerfed part; a the additional conftant weight in the upper bafon, or that which immerfes it to the mark in diftilled water Π; and we fhall have $P + a = V\Pi$; whence $V = \frac{P+a}{\Pi}$

Again b reprefents the weight more or lefs than a, which muft be fubftituted to produce the fame immerfion in another liquor different from diftilled water.

‡ We fhall therefore have $\Pi' = \frac{P+b}{V} = \frac{P+b}{P+a}$.

The value of Π' being found, every thing elfe is known; nothing more being neceffary than to fubftitute this value in the formula.

I am perfuaded that philofophers will immediately perceive the advantages of this method. Diftilled water was wanting; but this praxis renders it unneceffary. Even if diftilled water were at hand, it would feldom happen that the times of the ftandard temperature and preffure would agree with thofe of the experiment; and when an artificial temperature is produced, it is fubject to vary during the courfe of the experiment. All thefe difficulties are removed; and even when diftilled water is at hand, I prefer, more efpecially in fummer, water containing a fmall portion of neutral falt. Two motives juftify this preference:

mination. I am not prepared to ftate the method of applying this correction; and fhall only remark, in this place, that the mere change of dimenfions from temperature in a piece of fteel (independent of the greater change in the whole inftrument) will alter the fourth figure at every third degree of Fahrenheit. For pyrometrical data fee Philof. Journal, I. 58. N.

* For the fake of thofe who are unacquainted with fymbols, I fhall here give the rule in words at length: From the weight in the upper difh, when the inftrument is properly immerfed in the unknown fluid, take the weight which is placed with the body in the fame fcale at the like adjuftment. The remainder is the abfolute weight of the folid. Multiply this by the fpecific gravity of the fluid, and referve the product. From the additional weight when the body is placed in the lower bafon, take the additional weight when it was placed in the upper. The remainder will be the lofs of weight by immerfion. Divide the referved product by the lofs by immerfion, and the quotient will be the fpecific gravity of the folid with regard to diftilled water at the ftandard temperature and preffure. N.

† In the upper difh. For as the lower additional piece acts only by its refidual weight, and this muft vary with the fluid, it is clear that it muft be confidered as part of the inftrument, and enter into the value of P whenever it is ufed. In fact, the gravimeter with this piece is an inftrument perfectly diftinct from that in which it is not ufed, and the adjuftment may then be made as well (and perhaps more eafily) by a conftant fmall weight in the upper difh, as by the very delicate procefs of the author. N.

‡ Or in words: To the weight of the gravimeter add the weight required in the upper bafon to fink it in the unknown fluid. Again, to the weight of the gravimeter add the weight required in the fame manner to fink it in diftilled water. Divide the firft fum by the latter, and the quotient will be the fpecific gravity of the fluid in queftion. N.

 1. It

1. It is more convenient to add a few milligrams to the constant additional weight, than to compose a less by a series of sub-multiples; 2. By making use of a liquid, at the temperature of the surrounding air, it is evidently less exposed to sudden variations. These circumstances are more favourable to accuracy in the result.

Explanation of the Figures.

Fig. 2, Plate VI.—The gravimeter.

a. The lower bason.

b. The upper bason.

c. The point of immersion marked on a thin piece of glass in the inside of the stem.

Fig. 3. The piece called *plongeur*, which is placed in the lower bason a when experiments are made on fluids of greater density than water.

Fig. 4. The gravimeter in the cylindric vessel filled with water, in which it floats, immersed to the mark c, by means of the additional constant weight d.

It is convenient to choose a vessel of such a depth that the instrument may be at liberty to float at the level of the mark, or even beneath it, without its being possible that the bottom of the upper bason should ever descend to the surface of the water.

Fig. 5. The gravimeter in its case.

A. The cylindrical part of the instrument lodged in a groove in the case, and secured above by the two projections e e which leave the stem at liberty. It is secured at the middle by the brass button f, and is pressed below by a piece of cork g, which rests on the fixed block h.

i. Sliding piece and screw to support the ballast-piece, and prevent the branches from being endangered by any internal movement of the mercury.

k. The additional ballast-piece, or *plongeur* in its separate cell.

l. The constant additional weight placed in a cell cut in the solid wood, and cleared out at the sides, so that it may be conveniently taken up when wanted.

m. The inner surface of the cover hollowed out at n to receive without friction the projecting part of the upper bason. A paper is pasted on the inner surface of the cover, to shew the weight of the gravimeter with or without the additional ballast-piece, and the volume of water it displaces in either case, which are often required to be accurately known.

On the useful Application of the Gravimeter to the Results of Tables of Specific Gravity.

IT is frequently necessary, in philosophical as well as commercial transactions, to determine the proportions of a mixture of two liquids, or an alloy of two metals; and it has been for some time known, that in order to render this operation easy and certain, recourse must be had to tables drawn up from observation with the assistance of an instrument capable of giving the specific gravity to at least three decimals. The gravimeter will perfectly answer this purpose. To shew this more effectually, I shall apply it to the mixture of alcohol and water, and the alloy of tin and lead. These two compositions are precisely what in a commercial point of view most frequently require to be examined in this respect. And I am convinced that an exhibition of the most accurate results of experiments of such extent and minuteness, cannot fail to prove acceptable in this place.

A TABLE

A TABLE of the Specific Gravities of the Mixtures of Alcohol and Water, with the Proportion of theſe Fluids at the Temperature of 60 Degrees of the Scale of Fahrenheit, 12,44 of Reaumur, 15,55 of the Decimal Thermometer.

Note. I have placed in this table the ratios publiſhed by Citizen Chauſſier, from the Memoirs of Citizen Gouvenain, at the article ALCOHOL of the Encyclopédie Methodique, and thoſe given in the tables of Mr. Gilpin, which are recommended by the philoſophers of Germany. Journ. Phyſ. de M. Gren, 1796, Tom. III. p. 128.

Centeſimal parts of the mixture *.	Specific Gravities. According to Chauſſier.	Specific Gravities. According to Gilpin (laſt table. N.)
Alcohol - 100	0.7980	0.825
95	0.8165	0.83887
90	0.8340	0.85244
85	0.8485	0.86414
80	0.8620	0.87606
75	0.87525	0.88762
70	0.8880	0.89883
65	0.9005	0.90941
60	0.9120	0.91981
55	0.9230	0.92961
50	0.9334	0.93882
45	0.94265	0.94726
40	0.9514	0.95493
35	0.95865	0.96158
30	0.96535	0.96736
25	0.97035	0.97239
20	0.97605	0.97723
15	0.9815	0.98213
10	0.9866	0.98737
5	0.99335	0.99327
0	0.99835	1.00000

We ſhall not be ſurpriſed at the difference between theſe two tables, if we attend to the circumſtance that the alcohol uſed by Chauſſier was more highly rectified, its ſpecific gravity having been only 0.798 at the ſame temperature at which the ſpirit uſed by Gilpin gave .0825. But it is not eaſy to conceive that this difference could produce ſo conſiderable a change in the penetration or diminution of volume of the mixtures; that indicated by Gilpin at equal parts of ſpirit and water being 0.025, whereas, according to Chauſſier, it is 0.0454 †.

TABLE

* By meaſure, the water being added as the ſpirit is diminiſhed, ſo as to keep the volume conſtantly = 100.

† For [illegible] of that part of the New Encyclopedie which contains the tables of Chauſſier, I am not qualified to give an account of the degree of accuracy they may poſſeſs. The tables of Gilpin are to be found in the ſecond part of the Philoſophical Tranſactions for 1794. They are comprehended in one hundred quarto pages, and expreſs—for every ſingle degree of Fahrenheit, from 30° to 80° inclusive;—and for all mixtures of ardent ſpirit and water, formed by the addition of 1, 2, 3, &c. parts by weight of water, as far as 100, to the conſtant quantity 100 of ſpirit, at 0.825 ſpecific gravity when at 60;—and for all mixtures formed in like manner by

 adding

TABLE of the Correfpondencies of the Specific Gravities of Alloys of Tin and Lead, with the refpective Proportions of thefe Metals.

In conftructing this table, I have made ufe of the obfervations publifhed by M. Bergenftierna in the Memoirs of the Academy of Stockholm for 1780, and reprinted in the Manuel Syftematique of M. Gren (§ 3195). But as he grounded all his ratios on the variations of abfolute weight under equal volumes, and made ufe of foreign weights, I have been obliged to change the expreffion for the more fimple and ufual proportion to diftilled water, taken as unity.

At every fifth line I have added the mathematical fpecific gravity, or that which is determined by calculation, in order to fhew the changes of bulk which refult from the combination, and in this particular cafe diminifh the denfity inftead of increafing it.

The fpecific gravity of pure lead to water is as 11.1603 to 1.

The fpecific gravity of pure tin is 7.2914 to 1.

Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.	Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.
Tin.	Lead.			Tin.	Lead.		
99	1	7.3300	7.3252	89	11		7.6468
98	2		7.3552	88	12		7.6787
97	3		7.3871	87	13		7.7106
96	4		7.4189	86	14		7.7425
95	5	7.4848	7.4508	85	15	7.8717	7.7744
94	6		7.4828	84	16		7.8063
93	7		7.5146	83	17		7.8382
92	8		7.5511	82	18		7.8701
91	9		7.5835	81	19		7.9020
90	10	7.6782	7.6149	80	20	8.0652	7.9339

adding 1, 2, 3, &c. parts by weight of the fpirit, to the conftant quantity 100 of water. (1) The fpecific gravity to five places of figures; (2) the proportion of water by meafure to the fpirit taken as 100; (3) the actual bulk of this laft mixture; (4) the diminution of bulk by mixture; (5) the quantity of fpirit per cent.; and (6) a decimal multiplier, which being applied to the bulk of the mixture gives the meafure of the pure fpirit in a mixture of that denfity. Hence it may be inferred how extenfively ufeful this table muft prove to all practical philofophers and manufacturers. With regard to its accuracy, I muft refer to the ample reports of Dr. (now Sir Charles) Blagden, in the Tranfactions for 1790, and fhall only obferve that the experiments were made with the balance of the Prefident of the Royal Society; of the admirable mechanifm and fenfibility of which, fome account, though imperfect, is given in the Journal de Rozier, Vol. XXXIII. and that the term or ftandard fpirit .825 was arbitrarily chofen from motives of convenience and accuracy.

The fpirit was obtained from malt. The greateft ftrength obtained by adding hot cauftic alkali to fpirit and then carefully diftilling, gave a fpecific gravity of 0.813 at 60 degrees. Perhaps the circumftances of M. Gouvenain's experiments may fhew by what means he fucceeded in arriving at 0.7980 at the fame temperature. No ufeful purpofe would be anfwered by conjecture in this place. From the laft line in the table here given by Citizen Guyton, the fpecific gravity of mere water is fuch as to indicate a temperature of 75 degrees inftead of 60 in Chaulier's column. In which cafe Gilpin's ftandard fpirit would have a fpecific gravity of 0.8178. With regard to the confiderable difference in the penetration or diminution of bulk, it remains to be fhewn whether it depends upon any part of the proceffes by which the alcohol was obtained and concentrated, or, as is more probable, upon fome miftake in the calculations or methods of expreffing the quantities. N.

Compound

Compound Metal.		Mathematical Specific Gravity.	Real Specific Gravity.	Compound Metal.		Mathematical Specific Gravity	Real Specific Gravity.
Tin.	Lead.			Tin.	Lead.		
79	21		7.9658	39	61		9.4727
78	22		7.9977	38	62		9.4355
77	23		8.0296	37	63		9.4788
76	24		8.0615	36	64		9.5221
75	25	8.2586	8.0934	35	65	9.8074	9.5676
74	26		8.1253	34	66		9.6132
73	27		8.1572	33	67		9.6565
72	28		8.1891	32	68		9.7021
71	29		8.2210	31	69		9.7454
70	30	8.4520	8.2529	30	70	10.0010	9.7887
69	31		8.2848	29	71		9.8297
68	32		8.3167	28	72		9.8730
67	33		8.3486	27	73		9.9163
66	34		8.3828	26	74		9.9573
65	35	8.6455	8.4170	25	75	10.1945	9.9983
64	36		8.4511	24	76		10.0416
63	37		8.4853	23	77		10.0871
62	38		8.5195	22	78		10.1350
61	39		8.5537	21	79		10.1806
60	40	8.8397	8.5879	20	80	10.3881	10.2261
59	41		8.6228	19	81		10.2717
58	42		8.6562	18	82		10.3173
57	43		8.6904	17	83		10.3629
56	44		8.7246	16	84		10.4084
55	45	9.0333	8.7588	15	85	10.5799	10.4586
54	46		8.7929	14	86		10.5062
53	47		8.8271	13	87		10.5543
52	48		8.8613	12	88		10.6021
51	49		8.8955	11	89		10.6500
50	50	9.2258	8.9319	10	90	10.7734	10.7001
49	51		8.9729	9	91		10.7479
48	52		9.0139	8	92		10.7958
47	53		9.0550	7	93		10.8414
46	54		9.0960	6	94		10.8869
45	55	9.4203	9.1373	5	95	10.9668	10.9354
44	56		9.1552	4	96		10.9781
43	57		9.2190	3	97		11.0236
42	58		9.2600	2	98		11.0692
41	59		9.3033	1	99		11.1148
40	60	9.6139	9.3466	0	100	11.1216	11.1603

V.

Descriptions of the Improved Air Pumps of PRINCE *and* CUTHBERTSON; *with Observations.*

THE important uses of the Air Pump are too many and too well known to require enumeration. It is an engine, not only calculated to demonstrate many of the leading principles in natural philosophy, but indispensable for the performance of some of the most accurate experiments in the Pneumatic Chemiftry. The air is exhaufted out of a receiver by this inftrument, by an action nearly fimilar to that of the common fucking pump, as it is called. In the common pump there is a bucket, which by leathering is made to flide, water-tight, up and down the pipe; but inftead of a bottom, it is provided with a valve or flap opening upwards. By this contrivance the bucket may be moved downwards through a column of water without producing any progreffive motion in the fluid, becaufe the valve then opens and affords a free paffage; and, on the contrary, when it is moved upwards, it muft alfo move the whole of the column above the flap or valve, which is then fhut. Another valve, alfo opening upwards, is fixed in the pipe below the ordinary range of the bucket, and ferves to prevent the return of any fluid which may lie between the two valves during the defcent of the bucket itfelf.

In working this common and very ufeful engine, it is firft neceffary to pour a fmall quantity of water into the pipe above the bucket; after which, the handle being moved produces the following effect: As the bucket defcends, the fpace between the two valves becomes diminifhed, and the air contained in that fpace lifts the upper valve, and in part rifes with noife through the water; after which, as the bucket rifes, and again enlarges the fpace, the remaining air expands by its fpring, which is thus weakened. The air in the pipe beneath the lower valve will therefore act more ftrongly upwards by its entire fpring than the air above does downwards. It will open the valve, and rife up into the upper fpace; at the fame time that the preffure of the external air, upon the water in the well, will drive part of the water after it into the lower part of the pipe. By continuing the work for a few ftrokes the included air is thus entirely drawn out and fucceeded by water, provided the height from the furface of the fluid in the well to the upper valve be not fo great as to include a column of upwards of thirty-three feet. For fuch a column would be fufficiently heavy to counterpoife the whole preffure of the atmofphere, and would accordingly prevent any farther afcent.

If this laft-mentioned cafe were to exift, or if the lower aperture of the pipe were clofed, it may without difficulty be underftood that in theory, that is to fay, with a perfect inftrument, the air beneath the lower valve would become more and more rarefied to a certain point as the procefs was continued. And the progrefs and extent of this rarefaction would be computable from the known ratios of the greateft and leaft fpaces between the valves when in their extreme pofitions, together with the affumed conditions that the air, however greatly rarefied, would diffufe itfelf equally through the containing fpace, and that the lower valve fhall afford no refiftance to the power applied to open it when the bucket is elevated. It will alfo follow, that the fame rarefaction might be produced in any veffel containing air, and communicating only with the pipe of the pump beneath the lower valve.

Thus it is feen that the common fucking pump is, in fact, an air pump before it operates as a pump for water; and that nothing more is required to conftitute the philofophical air pump than convenience of fize, accuracy of workmanfhip, and, that the refiftance at the valves be either diminifhed or removed.

It would be to little purpofe to enter into any difcuffion concerning the original conftruction of the air pump, as invented by Otto Guericke, and afterwards improved and moft ufefully employed by our eminent countryman Robert Boyle. It is generally allowed at prefent, that fuch air pumps as operated by means of ftop-cocks inftead of the lower valve, are not

not durable, but soon become leaky. If ever these pumps, of which engravings are to be seen in the works of Muschenbroek and S'Gravesande, were used in England, which I think was not the case, they must have been long superseded by the cheaper and more simple contrivance of valves formed by tying a strip of bladder over a small hole through which the air is allowed to pass in one direction only. Mr. Smeaton was of opinion, that the operation of the air-pump is rendered defective from the small surface of pressure at which the air acts against the bladder of the lower valve, as well as from the pressure of the external air, which is supposed to prevent the upper valve from opening in the last stages of the process. To remedy these imperfections, he made a number of hexagonal holes instead of the one small hole in the valve; and by passing the rod of the piston through a collar of leathers at the top of the barrel, which was closed, with the exception of a valve opening outwards, he prevented the external air from acting upon the valve of the piston in its descent *. Mr. Abraham Brooke of Norwich, by experiments with the common air-pump, has rendered it at least doubtful, whether, in pumps equally well made, these be improvements at all †. But, previous to the time this last author wrote, there existed two other air-pumps, containing improvements both in principle and workmanship sufficient to annihilate any controversy respecting the merits of the common pump, and that of Smeaton. These were made by the Rev. Mr. Prince, of Salem in North America, and Mr. John Cuthbertson of Amsterdam. The first is described in the only volume of Memoirs published by the American Academy, and is scarcely known in the philosophical world. The other is described in a publication ‡ by the inventor; of which the circulation must, in the ordinary course of things, be considerably limited. I have thought it my duty to present both to the world in this place.

Mr. Prince's paper contains a considerable portion of matter relative to the air-pump of Smeaton, partly descriptive of the improvements made by that ingenious mechanic, and partly by way of comparison with his own. For the sake of brevity, I have omitted these, and other matters of less consequence; but in the essential parts of the account I have kept as nearly as possible to the words of the author.

From the account of Mr. Smeaton's success in facilitating the opening of the valves at the bottom of the barrel and in the piston, Mr. Prince supposed, if those valves were entirely removed, and the remaining air in the barrel could be more perfectly expelled, the rarefaction might be carried still further. Upon this plan he constructed his pump. He removed the lower valve, and opened the bottom of the barrel into a cistern on which it was placed, and which had a free communication with the receiver. For the valve on the plate at the top of the barrel (which is constructed like Smeaton's) makes it unnecessary, as he remarks, that there should be any at the bottom in order to rarefy the air in the receiver.

The cistern is deep enough to allow the piston to descend into it below the bottom of the barrel. Suppose then the piston to be solid, that is, without a valve in it; when it enters the barrel and rises to the top plate, which is made air-tight with a collar of leathers, &c.

* Philosophical Transactions, XLVII.

† Miscellaneous Experiments and Remarks on Electricity, &c. by A. Brooke. Norwich, 1789. P. 123, and elsewhere.

‡ Description of an improved Air Pump. By John Cuthbertson. Octavo, 41 pages. London; no date. This artist is at present settled in London.

like Smeaton's, it forces out all the air above it; and, as the air cannot return into the barrel on account of the valve on the top plate, when the piston descends, there will be a vacuum formed between that and the plate, every thing being supposed perfect. But in working the pump, the piston is not allowed to descend entirely into the cistern so far as to leave the bottom of the barrel open; because, as the cistern, for another purpose, is made larger than the bore of the barrel, this might make the piston-rod work unsteadily in the collar of leathers, and cause it to leak; but it descends below a hole in the side of the barrel near the bottom, which opens a free communication between the barrel, cistern, and receiver. Through this hole the air rushes from the cistern into the exhausted barrel, when the piston has dropped below it; and by its next ascent this air is forced out as the other was before. If now, the capacity of the receiver, cistern, pipes, &c. below the bottom of the barrel, taken together, be equal to the capacity of the barrel, half the remaining air will be expelled by every stroke.

But as the working a pump of this kind with a solid piston would be laborious, on account of the resistance it would meet with in its descent from the air beneath (though this would be lessened by every stroke as the air became more rarefied), the inventor, to remedy this inconvenience, pierced three holes in the piston at equal distances from each other; and a circular piece of bladder, which is tied over the top of the piston, to make the joint more perfect with the top plate, and to defend them from injury when the piston is brought up against it, forms a kind of valves over the holes, which open easily enough to prevent any labour in working the pump, as it allows the air to pass through the piston when it descends. But the air does not necessarily depend upon a passage through the piston in order to get into the barrel: for when the air becomes so weak from its rarefaction that it cannot open this valve, it will still get into the barrel when the communication is opened by the hole at the bottom. This piston, therefore, will descend as easily as any other; and these valves do not impede the rarefaction, since it is of no consequence, as to this, whether they open or not. By this construction the valves, which Mr. Smeaton only made to open with more ease, are rendered unnecessary in rarefying the air, and that at the bottom of the barrel, which is the most difficult to be made and kept in order, is entirely removed; the valve on the top plate being the only one necessary in rarefying the air.

But as in a single-barrelled pump of this construction, where there is no valve at the bottom to prevent the air, which follows the piston in its ascent, from returning into the receiver in its descent, a fluctuation would be produced which might prove detrimental in some experiments, this pump is made with two barrels, which rarefy the air at every stroke of the winch. In this construction, the capacity of the two barrels taken together below the pistons is always the same; for while one is descending the other ascends, and what is taken from the one is added to the other.

Having thus set aside the valves which in some measure prevented the air from entering the barrel above the piston, he next attempted to expel the air more perfectly out of the barrel than Mr. Smeaton had done, by making a better vacuum between the piston and the top plate, which would allow more of the air to expand itself into the barrel from the receiver.

Upon Mr. Smeaton's plan, our author also contrived to connect the valves on the top plates with the receiver occasionally, by means of a pipe and cock, by the turning of which the machine

machine may be made to exhauſt or condenſe at pleaſure. This is done in the following manner: There is a croſs piece laid over the valves extending from one barrel to the other, which has a duct through it connected with a ſmall pipe ſtanding between the barrels; through this pipe the air paſſes into a duct at the bottom piece leading to the cock. In this piece is likewiſe the duct leading from the ciſtern to the cock; and with this cock alſo is connected the pipe leading to the receiver. The key is pierced with two holes, in ſuch a manner, that one of them will connect the pipe coming from the receiver with the duct in the bottom piece leading to the ciſtern, or with the other leading to the valves, as may be required for exhauſting or condenſing. The other hole through the key will open, occaſionally, to the atmoſphere either of theſe ducts round the cock: ſo that, having the direction of the air which paſſes through the valves under the command of this cock, the pump may exhauſt or condenſe at pleaſure; for when the key connects the pipe from the receiver and the duct leading to the ciſterns together, the pump will exhauſt; and when it connects the pipe with the duct leading to the valves, it will condenſe, as the other hole in the key at the ſame time opens to the atmoſphere the duct leading to the ciſterns, by which paſſage the air enters the barrel from the atmoſphere, is forced out at the valves, and through the pipe and cock into the receiver. In this part of the machine which is contrived for condenſation, the inventor has, by an additional part, endeavoured to get the air more perfectly out of the barrel.

We have ſeen, ſays he, that Mr. Smeaton, by making the piſton of his pump fit more exactly to the bottom of the barrel, and by ſhutting up the top to prevent the preſſure of the atmoſphere on the piſton valve, was able to get more of the air above it than could be effected in the common pump: but ſtill the difficulty, though ſo far removed, remains in the top of the barrel; for, as the piſton cannot be made to fit ſo exactly to the top plate, but that there will be ſome lodgement for air, it is impoſſible to expel it entirely. More perhaps might be expelled if the valve on the top could be made to open more eaſily, by removing the weight of air from it; for the atmoſphere preſſing on this valve will prevent its opening freely, in the ſame manner as, when preſſing on the piſton valve, it obſtructs the opening of that in the common pump.

The difficulty which Mr. Smeaton removed from the piſton valves, Mr. Prince endeavoured to remove from the valve on the top plate; that this valve, having the preſſure of the atmoſphere taken off, might open with the ſame eaſe as the piſton valve does in his pump. To effect this, there is connected with the duct on the bottom piece which conveys the air from the valves to the cock, a ſmall pump of the ſame conſtruction as the large one; having the barrel opening into a ciſtern, the piſton rod moving through a collar of leathers, and a valve near the top, through which the air is forced into the atmoſphere. This piſton is ſolid: becauſe the diameter being only half an inch, does not make it work hard. This pump, which is of one barrel only, he calls the valve pump; its chief uſe being to rarefy the air above the valves, or remove the weight of the atmoſphere from off them. To uſe this pump, it is neceſſary the key of the cock ſhould be pierced differently from that of Mr. Smeaton's; for, as the pipes round his are placed at equal diſtances, when the one from the bottom of the barrel is connected with that from the receiver to exhauſt it, the other from the valve on the top plate is opened to the atmoſphere by the other paſſage through the cock. But in order to rarefy the air above the valve in my pump, it is neceſſary this laſt paſſage ſhould be ſhut

 up

up when the valve pump is ufed. Inftead, therefore, of placing the three ducts at equal diftances round the cock, I have divided the whole into five equal parts; leaving the diftance of one fifth part between the ducts leading from the ciftern and the valves to the cock, and two fifths between each of thefe and the one leading from the cock to the receiver. By this adjuftment, when the communication is open between the receiver and the valves for condenfation, the other hole through the cock opens the cifterns to the atmofphere; but when the communication is made between the cifterns and the receiver for exhauftion, a folid part of the key comes againft the duct leading to the valves, and fhuts it up, and the air which is forced out of the barrel paffes through the atmofphere into the valve pump; for the valve of the fmall pump may be kept open while the great one is worked.

Upon this conftruction alfo we are able to make the pump with two barrels like the common pump, which cannot be done conveniently where the lower valve is retained; becaufe it would be difficult to make the pifton in one barrel come exactly to the bottom, at the fame time that the pifton in the other touched as exactly at the top; it would at leaft require a nicety in the workmanfhip which would be troublefome to execute.

In this pump the piftons do not move the whole length of the barrels: there is a horizontal fection made in them a little more than half way from the bottom where the top plates are inferted. See Fig. 6. Pl.VI. By this means the pump is made more convenient and fimple, as the head of it is brought down upon the top of the barrels in the fame manner as in the common air pump. The barrels alfo ftand upon the fame plane with the receiver plate, and this plane is raifed high enough to admit the common gauge of thirty-two or three inches, to ftand under it without any inconvenience in working the pump, as the winch moves through a lefs portion of an arch at each ftroke, than it would if the piftons moved the whole length of the barrels.

There is alfo placed, between the barrels in this pump, on the crofs piece over the valves, a gauge to meafure the degree of condenfation, having a free communication with the valves, cock, &c. This gauge is fo conftructed that it will alfo ferve to meafure the rarefaction above the valves when the air is worked off by the valve pump. It confifts of a pedeftal, which forms a ciftern for the mercury, a hollow brafs pillar, and glafs tube hermetically fealed at one end, which moves up and down in the pillar through a collar of leathers. The die of the pedeftal is made of glafs, as well to hold the quickfilver as to expofe its furface to view, that it may be feen when the open end of the tube is put down into it or raifed out of it. The body of the pillar is partly cut away to expofe the tube to view in the fame manner.

If the pump be ufed as a condenfer, the degree of condenfation is fhewn by a fcale marked on one edge of the pillar: if it be ufed as an exhaufter, the degree of the rarefaction of the air above the valves is fhewn by a fcale marked on the other edge of the pillar.

This gauge will alfo ferve to fhew when the valves have done playing, either with the weight of the atmofphere on them or taken off. If we want to know when they ceafe opening with the weight of the atmofphere on them, draw the pifton of the valve pump into its barrel, to prevent any air efcaping through that valve: in this fituation work the great pump again; and if any air paffes through the valves into the pipe, the gauge will rife by condenfation. This condenfed air muft then be let out by opening the communication at the cock with the outward air. By repeating this till the gauge riſes no longer, we may know the valves will open no more while the weight of the atmoſphere lies on them; and the rarefaction in the receiver can be carried no further. When the weight of the atmoſphere is to be removed, after conducting as in the former experiment, raiſe the open end of the tube above the ſurface of the mercury, and then work the valve pump, and the air will be rarefied over the valves and in the tube to the ſame degree (we may ſee when the valve of this pump has done playing, by unſcrewing the cap that covers it). The open end of the tube is then to be immerſed into the mercury, and the great pump worked. The air which paſſes through the valves will then raiſe the gauge by condenſation; and thus by alternately raiſing and depreſſing the tube, and working the two pumps in their turns, we may carry the rarefaction of the air in the receiver as far as the power of the pump will go. If one of Mr. Smeaton's pear gauges be uſed in the receiver, as he directs, the difference of the rarefaction in the two experiments may be known. And as the air above the valves may be rarefied to different degrees, it may be known, by the two gauges, what proportion the rarefaction above the valves bears to the degree of exceſs in the receiver. This condenſing gauge can be taken off, and a button ſcrewed into the hole in its ſtead, in any caſe wherein a greater degree of condenſation is required than the glaſs will bear. When a glaſs receiver is uſed, this gauge may be placed within it, where it will meaſure any degree of condenſation the receiver will bear without danger to the gauge; or the capacity of any receiver may be meaſured by this gauge before it is removed from its place, by ſhewing how many ſtrokes of the winch will throw one atmoſphere into the receiver; then turning the cock to prevent any air eſcaping, change the gauge for the button. When this is done, the degree of condenſation may be further meaſured by the number of ſtrokes.

As in caſes where great condenſation is required, there muſt be a great deal of labour, and a great ſtrain on the teeth of the wheel and piſton rods, on account of the great diameter of the piſtons *; to remedy this, Mr. Prince has fitted a condenſer of a ſmaller bore than the barrel of the great pump to the ciſtern of the valve pump, to be ſcrewed on occaſionally, by which the condenſation may be finiſhed, inſtead of the great pump. Or, to ſave the work and expence of this condenſer, the valve pump, if made a little larger, may be eaſily fitted for the ſame purpoſe, by having a plate made to ſcrew into the bottom of the cylinder occaſionally, with a valve on it opening into the ciſtern: a hole muſt alſo be made to be opened on the ſame occaſion near the top of the cylinder, to let air in below the piſton when this is drawn up above it.

The common gauge, which is generally placed under the receiver plate in this pump, is placed in the front, that it may be ſeen by the perſon who is working the pump, and that the place may be left free for other uſes.

The plate is ſo fixed to the pipe leading to the cock, that it may be taken off at pleaſure, and uſed as a transferer; or any tube or apparatus may be fixed to it to perform ſome experiments without removing it, which will ſave trouble and make leſs apparatus neceſſary.

* In this pump the piſtons are two inches diameter; ſo that there will be about forty-eight pounds added to the reſiſtance in opening the valves for every atmoſphere thrown into the receiver. P.

It is certainly an injury to an exhauſting pump to uſe it for condenſing. This operation ought always to be performed with a long ſyringe of ſmall bore. Such an inſtrument would coſt much leſs than the condenſing part of an air pump, and will at the ſame time have much more power. N.

The

The head of this pump is not divided as the common one is, to diſlodge the teeth of the wheel from the piſton rods when the pump is to be taken apart, but is made whole, except a ſmall piece in the back, where the wheel is let in; which makes it much more convenient to remove the head, or place it on the barrels. The wheel is freed from the piſton rods when required, by puſhing it into the back part of the head; and when it is drawn into its place and connected with them again, a button is ſcrewed into the ſocket of the axis behind, to keep it in its place. This makes the head leſs troubleſome to remove; but its chief uſe is to diſlodge the piſton rods from the wheel, that they may be put down into the ciſterns when the pump is not in uſe, where they will ſtand uncompreſſed, and retain their elaſticity better than if kept in the barrels. In theſe ciſterns they may alſo ſtand covered with oil, if neceſſary, as they are large enough to admit of it.

The principal joints of the pump are ſunk in ſockets, that the leathers which cloſe them may be covered with oil to prevent leaking*.

For convenience, the lower part of the pump is fitted with drawers to contain the apparatus. A door opens behind one range to a place reſerved the whole height, to get at the under part of the receiver plate, and fix apparatus to it for ſome experiments. In this place ſtand the long tubes, and ſuch tall glaſſes belonging to the apparatus as will not go into the drawers. The barrels, &c. of the pump are covered with a caſe or head, which keeps them from duſt and accidents when the pump is not in uſe. The apparatus is ſecured between ſliders, &c in the drawers, ſo that the whole machine may be eaſily removed in one body, without danger.

As there was no glaſs manufactory in Mr. Prince's vicinity, he ſent to Europe for his apparatus; but unluckily the gauges, with ſome other parts, had not been forwarded to him when he wrote his account. He had only a ſmall tube of two-tenths inch bore, which he uſed as a common gauge; but this was not ſufficient to determine the power of the pump.

All that he could ſay of the inſtrument was, that he found it much more convenient to uſe than one of the common ſort; that it would exhauſt a receiver much ſooner, and keep in order much longer, for being made without valves, which muſt depend on the ſpring of the air to open them. When a common pump in his poſſeſſion had been fitted up with valves, leathers, &c. at the ſame time with this, the valves of the common pump have become too dry and ſtiff to uſe, while this pump has continued in good order. He attributes this, in part, to the moiſture which the valves on the top plates receive from the piſtons every time the pump is uſed; the piſtons being always kept moiſtened with oil in the ciſterns, where they ſtand when the pump is not in uſe; and in part to the power which the piſtons have over theſe valves, by condenſing the air againſt them.

Fig. 6, Plate VI, repreſents a perpendicular ſection of one of the barrels, the two ciſterns, condenſing gauge, &c. where AB repreſents the barrel; CD is the ciſtern on which it ſtands; a a a a the leathered joint, ſunk into a ſocket and buried in oil: EF is the piſton; the cylindrical rod paſſing through a collar of leathers, GG, in the box HI. K ſhews the place of the valve on the top plate K, covered by the croſs piece MM, into which the pipe OO

† This, I find, is very effectual; having never known one of the joints ſecured in this way to leak, though the pump has ſtood for a long time; whereas a portable pump which I have, made by Mr. Nairne, London, has leaked, and repeatedly been refitted with new oiled leathers in the ſame time. P.

is

is foldered, that conveys the air from the valves to the duct going under the valve pump, as may be feen in Figure 8; o is part of the faid duct; p is the joint funk into a focket in the crofs piece PP, which connects the cifterns, and has a duct through it leading to them. Into this duct open the ducts q and r, the firft leading to the gauge in front of the pump, the other to the cock and receiver.

The other barrel is left out of the figure to fhew fome of the parts more diftinctly, except Q, which is the top of the barrel retained and brought down out of its place to fhew the top plate that fhuts up the barrel, feparated from the box which contains the collar of leathers. S fhews one of the holes in the plate over which the valve lies, and which is covered by R in the crofs piece. VV is the pifton fhewing the valve open on the top, which is to prevent labour when the pump condenfes. WX is the ciftern, in which is more diftinctly feen the fhoulder for the leather which clofes the point between this and the barrel, and alfo the focket in which the oil lies over the leather. YZ is the condenfing gauge, with the orifice of the tube raifed above the furface of the quickfilver. e e is the collar of leathers, through which the glafs tube moves. i is a fmall pipe coming up through the quickfilver to make a communication between the valves and the gauge.

Fig. 7. is a view of the upper furface of the top plate which clofes the barrel, being foldered into it, fhewing the place of the valve over the three fmall holes, one of which only can be feen at S in Fig. 3.

Fig. 8. is a perpendicular fection of the bottom piece, pipes, valve pump, cock, &c. at right angles with the other fection, Fig 6. AB is the pipe between the barrels, as reprefented in Fig 6. The button o is here fcrewed into the top inftead of the gauge. CD is the valve pump and its ciftern, e the place of the valve under the cap. EF the cock, fhewing the duct through it leading to the atmofphere. GH the pipe leading from it to the ftem of the receiver plate, in which is the cock I, to fhut up the duct when the plate is ufed as a transferer. KK is the plate. L, a piece to fhut up the hole, into which tubes &c. are occafionally fcrewed to perform experiments without removing the plate. The dotted line at O fhews the place of the fcrew which preffes the plate againft the pipe: PQ the pipe and common gauge ftanding in front of the pump.

Fig. 9. is a horizontal fection of the cock, and pieces containing the ducts leading from it to the receiver, the cifterns, and the valves on the top of the barrels. AB the duct connecting the cifterns together. CD the duct leading from the cifterns to the cock. GH the duct leading from the cock through the pipe AB (Fig. 8.) to the valves. DE the duct through the cock, which occafionally connects the two laft mentioned ducts with the duct EF, leading from the cock to the receiver. I, the duct in the cock leading to the atmofphere, which, when connected with the duct at D, lets the air into the cifterns and barrels for condenfation; the other duct through the cock at the fame time connecting H and E. This duct alfo, when connected with E, reftores the equilibrium in the receiver. KL is part of the duct leading from the cifterns to the gauge. The dotted circles fhew the places of the pipe and valve pump on the piece, and r the place where the air enters the valve pump from the duct GH, and is thrown into the atmofphere when the pump exhaufts.

Fig. 10. fhews the under furface of the boxes which contain the collars of leathers with the crofs piece which connects them together, having a duct through it, as reprefented by the

dotted line, through which the air paffes from the valves to the pipe. This Fig. is defigned chiefly to fhew the places in which the valves play, as at I.

Fig. 11. is a fide view of the pump, fhewing the fituation of the valve pump and handle of the cock; where A is the pump, and B the handle.

Fig. 12, Plate VII. is a perfpective view of Cuthbertfon's pump, with its two principal gauges fcrewed into their places: thefe need not be ufed together, except in cafes where the utmoft exactnefs is required; for in common experiments either of the two may be taken away, and a ftop fcrew put into its place. When the pear gauge is ufed, a fmall round plate, large enough for the receiver to ftand upon, muft firft be fcrewed into a hole at A; but when this gauge is not ufed, this hole muft be clofed with a ftop fcrew. When all three gauges are made ufe of, and the receiver is exhaufted, the ftop fcrew B, at the bottom of the pump, muft be unfcrewed, to admit the air into the receiver; but when the gauges are not all ufed, the ftop fcrew at A, or either of the other two which are in the place of the gauges, may be unfcrewed for this purpofe.

In Figure 1, CD reprefents one of the barrels of the pump, F the collar of leathers, G a hollow cylindrical veffel to contain oil; R is alfo an oil veffel, which receives the oil that is driven with the air through the hole a a, when the pifton is drawn upwards; and when this is full, the oil is carried over with the air along the tube T into the oil veffel G. c c is a wire which is driven upwards, from the hole a a, by the paffage of the air; and as foon as this has efcaped, falls down again by its own weight, fhuts up the hole, and prevents any air from returning by that way into the barrel: at d d are fixed two pieces of brafs, to keep the wire c c in fuch a direction as may preferve the hole air-tight. H is a cylindrical wire which carries the pifton I, and is made hollow to receive a long wire q q that opens and clofes the hole L, which forms the communication with the receiver ftanding on the plate. m is part of a pipe, one end of which is fcrewed into L, and the other into the centre of the receiver plate. M is a ftop fcrew, ferving only to clofe that hole. OP is a fmall fteel fcrew, one end of which is fcrewed into the wire q q, that opens and fhuts the hole L; and upon the other end O is fcrewed a nut, which, ftopping in the fmalleft part of the hole, prevents the wire from being lifted or carried too high. This wire and fcrew are more clearly fhewn in Figure 2 and in Figure 6; they flide through a collar of leathers r r, Figure 2 and Figure 5, in the middle piece of the pifton. Figures 4 and 5 are the two main parts which compofe the pifton; and when the pieces in Figures 3 and 6 are added to it, the whole is reprefented by Figure 2. Figure 5 is a piece of brafs, turned in a conical form, with a fhoulder or ledge at the bottom; a long female fcrew is cut in it, about two thirds of its length; and the remaining part of the hole, in which there is no fcrew, is about the fame diameter as the fcrew part, except a thin plate at the end, which is of a width exactly equal to the thicknefs of q q. That part of the infide of the conical piece of brafs, in which no thread is cut, is filled with oiled leathers with holes in them, through which q q can flide air-tight; there is alfo a male fcrew with a hole in it, which is fitted to q q, and ferves to prefs down the leathers r r. In Figure 4, a a a a is the outfide of the pifton, the infide of which is turned exactly to fit the outfide of Figure 5. b b are round leathers, about 60 in number; c c is a circular plate of brafs, of the fize of the leathers; and d d is a fcrew, which ferves to prefs them down as tight as is neceffary. The male fcrew at the end of Figure 3, is made to fit the female fcrew in Figure 5: now if Figure 6 be pufhed into Figure 5, this into Figure 4, and Figure 3 fcrewed ſcrewed into the end of Figure 5, theſe will compoſe the whole piſton, as repreſented by Figure 2. H in Figure 1 repreſents the ſame part as H in Figure 2, and is that to which the rack is fixed. If this therefore be drawn upwards, it will make Figure 5 ſhut cloſe into Figure 4, and drive out the air above it; and when it is puſhed downwards, it will open as far as the ſhoulders a a Figure 4 will permit, and ſuffer the air to paſs through. A A Figure 7, is the receiver plate; B B is a long ſquare piece of glaſs ſcrewed to the undermoſt ſide of the plate, through which a hole is drilled, correſponding with that in the centre of the receiver plate, and with the three female ſcrews b b c.

Suppoſe the piſton to be at the bottom of the barrel, and a receiver to ſtand upon the plate, the inſide of the barrel, from the top of the piſton to a, is full of air, and the piſton ſhut; when drawn upward, by the hollow cylindrical wire H, it will in its courſe drive the air before it, through the hole a a, into the oil-veſſel R, and out into the atmoſphere by the tube T. The piſton will then be at the top of the barrel at *a*, and the wire *q q* will ſtand nearly as it is repreſented in the figure, juſt raiſed from the hole L, and prevented riſing higher by means of the nut *o*. While the piſton is moved upwards, the air will expand in the receiver, and be driven along the bent tube *m* into the inſide of the barrel. Thus the barrel will be filled with air, which, as the piſton riſes, will be rarefied, in proportion as the capacity of the receiver pipes and barrel is to the capacity of the barrel alone. When the piſton is moved downwards again by H, it will force the conical part, Figure 5, out of the hollow part, Figure 4, as far as the ſhoulders *a a:* Figure 2 will reſt upon *a a* Figure 4, which will then be ſo far open as to permit the air to paſs freely through it, while at the ſame time the end of *q q* is forced againſt the top of the hole, and cloſes it in order to prevent any air from returning into the receiver. Thus the piſton, while moved downwards, ſuffers the air to paſs out between the ſides of Figure 4 and 5, and when it is at the bottom of the barrel will have the column of air above it; conſequently, when drawn upward, it will ſhut and drive out this air, and by opening the hole L give a free paſſage to more air from the receiver. This proceſs being continued, the air will be exhauſted out of the receiver as far as its expanſive power will permit; for in this machine there are no valves, as in the common air pumps, to be forced open by the air in the receiver, which, when its elaſticity is diminiſhed, it becomes unable to effect; but every thing is contrived to open by the motion of the piſton, and there is nothing to prevent the air from expanding to its utmoſt degree.

In exhauſting with this machine, no other directions need be obſerved, but ſuch as are common to all air pumps; nor is any peculiar care required to preſerve it in order, except that the oil veſſel G be always kept about half full of oil. When it has ſtood a conſiderable time without being uſed, it will be proper to draw a table-ſpoonful or two through it, by pouring it into the hole in the middle of the receiver plate when the piſton is at the bottom of the barrel; then, by moving the winch backward and forward, to raiſe and depreſs the piſton, the oil will be drawn through all the parts of the machine; and what is more than is neceſſary in the inſide will be forced out through the tube T into the oil veſſel G. Near the top of the cylindrical wire H is a ſquare hole, which is intended to let in ſome of the oil from the veſſel G, that the oiled leathers through which the wire *q q* ſlides, may always be ſufficiently ſupplied with it.

When the pump is required to condenſe, either at the ſame time that it exhauſts, or ſeparately, the piece which contains the bent tube T muſt be taken away, and Figure 8 put into

its

its place, and faſtened by the ſcrews which had before confined that piece. Figure 8 is drawn in the plate as it is made for a double-barrelled pump; but for a ſingle barrel only one piece is uſed, repreſented by b a a, the double piece being cut off at the dotted line a a. In this piece is a female ſcrew, to receive the end of a long braſs tube; to which a bladder, if ſufficient for the experiment, muſt be tied; or elſe a glaſs, properly confined for this purpoſe, muſt be ſcrewed to it. Then the air, which is exhauſted out of a receiver ſtanding on the plate, will be forced into the bladder, or glaſs connected with the braſs tube. But if the pump be double-barrelled, the apparatus, as repreſented by Figure 8, muſt be uſed, and the long braſs tube ſcrewed into the female ſcrew at C.

Figures 9 and 10 repreſent the two gauges; namely, the ſyphon gauge, and the barometer or long gauge. When theſe are uſed, Figure 9 muſt be ſcrewed into the female ſcrew c b, or into that at the other end c Figure 7, and Figure 10 into the female ſcrew a b Figure 7.

If it be uſed as a ſingle air pump, either to exhauſt or condenſe, the ſcrew K, which faſtens the rack to the cylindrical wire H, muſt be taken out; then turning the winch till this wire is depreſſed as low as poſſible, the machine will be rendered fit to exhauſt as a ſingle air pump: and if it be required to condenſe, the directions given in the laſt paragraph but one, with regard to the bent tube T, and Fig. 8, muſt be obſerved.

Mr. Cuthbertſon's treatiſe contains a relation of various experiments made with this air pump, which ſhew its great power of exhauſting, and are in other reſpects entitled to the conſideration of philoſophers. With the double ſyphon gauge, and alſo with the long gauge, compared with an attached barometer, in which the mercury had been repeatedly boiled, the difference between the heights of the mercurial columns proved to be no more than one fortieth of an inch, the barometer then ſtanding at 30 inches. This gives an exhauſtion of 1200 times. On ſome occaſions, when the air was in a very dry ſtate, he obſerved the difference to be as low as one hundredth of an inch, which indicates more than double the rarefaction.

On a review of theſe improvements of the air pump, I perceive in each inſtrument ſo happy a combination of philoſophical acuteneſs and mechanical ſkill, that it is with a degree of diffidence that I venture to ſpeculate on their reſpective merits and blemiſhes. There is no proviſion to open the upper fixed valve of Prince's greater barrel, except the difference between the preſſures of the elaſtic fluid on each ſide of the ſtrip of bladder; and this may reaſonably be inferred to limit the power of his ſmall pump. In Cuthbertſon's pump, the ſame valve is expoſed to the action of the atmoſphere, together with that of a column of oil in the oil veſſel. The miſchief in either inſtrument is probably trifling; but in both the valve might have been opened mechanically. If this were done, the ſmall pump of Prince might perhaps be unneceſſary in moſt ſtates of the atmoſphere. With regard to the lower valves, Cuthbertſon, by an admirable diſplay of talents as a workman, has inſured their action. Prince, on the other hand, has, by the proceſs of reaſoning, ſo far improved the inſtrument that no valves are wanted. In this reſpect he has the advantage of ſimplicity and cheapneſs, with equal effect. The mechanical combination of Cuthbertſon's pump reduces the operation to one ſimple act of the handle: but Prince's engine requires ſome manipulation with regard to the play of the ſmall pump; though this might have been remedied by a more ſkilful diſpoſition of the firſt mover.

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The moſt perfect ſcheme for an air pump, taking advantage of the labours of theſe judicious operators, ſeems to be that in which two piſtons of the conſtruction of Prince ſhould work in one barrel; one piſton being fixed at the lower end of the rod, and the other at the middle. The lower piſton muſt come clear out of the barrel when down, and work air-tight through a diaphragm at an equal diſtance from the effective ends of the barrel. In the diaphragm muſt be a metallic valve of the form of Cuthbertſon's lower valve, but with a ſhort tail beneath, that it may be mechanically opened when the piſton comes up. Above the diaphragm, muſt work the other piſton ſimilar to the firſt; but as it cannot quit the barrel when down, a ſmall portion of the barrel muſt be enlarged juſt above the diaphragm, ſo that the leathers may be clear in that poſition. Laſtly, the top of the barrel muſt be cloſed and fitted with a valve and oil veſſel, according to the excellent contrivance of Cuthbertſon.

If we ſuppoſe the workmanſhip of ſuch a pump to leave the ſpace between the diaphragm and lower piſton, when up, equal to one-thouſandth part of the ſpace paſſed through by the ſtroke of that piſton, the rarefaction produced by this part of the engine will in theory bear the ſame proportion to that of the external air. And the ſame ſuppoſition applied to the upper piſton would increaſe the effect one thouſand times more. Whence the rarefaction would be one million times. How far the practical effect might fall ſhort of this from the imperfections of workmanſhip, or the nature of the air, which in high rarefactions, may not diffuſe itſelf equally through the containing ſpaces, or from other yet unobſerved circumſtances, cannot be deduced from mere reaſoning without experiment.

VI.

Uſeful Notices reſpecting various Objects.—A Method of preventing Heat in Grinding—Concerning Gold, Silver, and other Metals reduced into very thin Leaves by the Hammer—Globules for Microſcopes—On the Plumb-Line, and Spirit-Level.

1. *On the Art of Grinding.*

A CURIOUS fact was mentioned to me, ſome years ago, reſpecting grinding, which promiſes to be of ſome uſe in the arts. Daily experience, as well as philoſophical experiment, ſhews us that heat is produced or developed by friction. The fact of ſparks flying from a dry grindſtone when a piece of iron or ſteel is applied to its ſurface during the rotation, has been ſeen by every one. The heat produced during this proceſs is ſuch that the ſteel very ſoon becomes ignited, and hard tools are very frequently ſoftened and ſpoiled, for want of care during the grinding. When a cylindrical ſtone is partly immerſed in a trough of water, the rotation muſt be moderate and the work ſlow, otherwiſe the water would ſoon be thrown off by the centrifugal force; and when this fluid is applied by a cock from above, the quantity is too ſmall to preſerve the requiſite low temperature. It is even found, that the point of a hard tool, ground under a conſiderable maſs of water, will be ſoftened if it be not held ſo as to meet the ſtream; ſparks being frequently afforded even under the water. My informant aſſured me, that fine cutlery is ground in Germany on a cylinder of a peculiar kind of pottery inſtead of ſtone, upon the face of which pulverifed hone is occafionally applied by means of tallow. The peculiar advantage of this kind of pottery was ftated to be, that it never heats, however rapid the motion.

This object feemed to deferve an experimental inveftigation. The three bodies fubjected to experiment were pottery, pulverifed filex, and tallow. The effect, namely that it could fupport violent and rapid friction without increafe of temperature, appeared at firft fcarcely credible. The mind was rather difpofed to reject the evidence, than inveftigate the caufe. No indication was given refpecting the nature of the pottery. It did not feem probable that any peculiarity in the filiceous fand fhould produce this defirable effect; but an eafy procefs of reafoning might have pointed out the effect of the tallow, which is indeed curious. I cannot however affume the merit of inveftigating the fubject *à priori*; for my apparatus was ready for experiment, and the fact fpoke for itfelf before I had fyftematifed the notions which occurred to me.

The pottery grindftone was not eafily attainable. I therefore procured a Newcaftle grindftone of a fine grit and ten inches in diameter; and alfo a block of mahogany to be ufed with emery on its face. Both the ftone and the wooden block were mounted on an axis to be occafionally applied between the centres of a ftrong lathe. In this fituation both were turned truly cylindrical, and of the fame diameter. The face of the wood was grooved obliquely in oppofite directions, to afford a lodgement for the emery. The face of the ftone was left fmooth, and there was a trough of a proper fize applied beneath the ftone to hold water. The grindftone was then ufed with water, and the wooden cylinder was faced with emery and oil. The inftrument ground was a file, out of which it was propofed to grind all the teeth. The rotation was produced by the mechanifm of the lathe; the velocity being fuch as to turn the grinding apparatus about five revolutions in a fecond. The ftone operated but flowly, and the water from the trough was foon exhaufted, with inconvenience to the workman, who could fcarcely be defended from it but by flackening the velocity. The emery cylinder cut rather fafter. But notwithftanding the friction was made to operate fucceffively and by quick changes on the whole furface of the file, it foon became too much heated to be held with any convenience; and when a cloth was ufed to defend the hand, the work not only became awkward, but the heat increafed to fuch a degree that the oil began to be decompofed, and emitted an empyreumatic fmell. The ftone was then fuffered to dry, and the file tried upon its face. It almoft immediately became blue, and foon afterwards red-hot. Both the cylinders were then covered with tallow, by applying the end of a candle to each while revolving, and emery was fprinkled upon the cylinder of wood. The fame tool was then applied to the grindftone in rapid motion. At the firft inftant the friction was fcarcely perceptible; but very fpeedily afterwards the zone of tallow preffed by the tool became fufed, and the ftone cut very faft. The tool was fcarcely at all heated for a long time; and when it began to feel warm, its temperature was immediatly lowered by removing it to a new zone of the cylinder. The fame effect took place when the experiment was repeated with the wooden cylinder.

It is not difficult to explain this by the modern doctrine of heat. When oil was ufed upon the wooden cylinder, the heat developed by the friction was employed in raifing the temperature of the tool and of the fluid oil. But when tallow was fubftituted inftead of the oil, the greateft part of the heat was employed in fufing this confiftent body. From the increafed capacity of the tallow, when melted, this heat was abforbed, and became latent, inftead instead of being employed to raise the temperature: and whenever, by continuing the process, the tallow already melted began to grow hot, together with the tool, it was easy to reduce the temperature again by employing the heat on another zone of consistent tallow. I used these two cylinders with much satisfaction, in a considerable quantity of work.

In this stage of experiment, I concluded that the cylinder of pottery mentioned to be used in Germany was either of no particular utility beyond that of a common grindstone, or that the report might be inaccurate in this respect. But it happened that the small stone here mentioned was laid aside for about three years. At the end of this term it was again brought into use, and it was found that the tallow had undergone some change, either from the stone itself, or the action of the external air, which enabled it to defend the grit much more effectually than at first. It seemed to have become less fusible. I think it probable that this might not have happened with a pottery cylinder, or at least that it would have been more easy to clean and restore the surface to its first state.

2. *Concerning Gold, Silver, and other Metals reduced into very thin Leaves by the Hammer.*

IT is generally thought by chemists, and others, that leaf gold consists of the metal in a high state of purity. It is never pure, because pure gold is too ductile to be worked between the gold-beaters' skin. The newest skins will work the finest gold, and make the thinnest leaf, because they are the smoothest. Old skins, being rough or foul, require coarser gold. The finer the gold, the more ductile; insomuch that pure gold, when driven out by the hammer, is too soft to force itself over the irregularities, but would pass round them, and by that means become divided into narrow slips. The finest gold for this purpose has three grains of alloy in the ounce, and the coarsest twelve grains. In general the alloy is six grains, or one-eightieth part. That which is called pale gold contains three pennyweights of silver in the ounce. The alloy of leaf gold is silver, or copper, or both, and the colour is produced of various tints accordingly. Two ounces and two pennyweights of gold is delivered by the master to the workman, who, if extraordinarily skilful, returns two thousand leaves, or eighty books, of gold, together with one ounce and six pennyweights of waste cuttings. Hence one book weighs 4.8 grains; and as the leaves measure 3.3 inches in the side, the thickness of the leaf is one two hundred and eighty-two thousandth part of an inch.

Silver leaf is said to be pure silver. It is extensible in this way when compared with gold, rather more than in the proportion of the specific gravities. Some leaf silver which I tried was thicker than the gold in the proportion of seven to four. So that the weight of metal covering equal surfaces approached to equality.

The yellow metal called Dutch gold is fine brass. It is said to be made from copper-plates, by cementation with calamine, without subsequent fusion. Its thickness, compared with that of leaf gold, proved as 19 to 4, and under equal surfaces it is considerably more than twice as heavy as the gold.

The Dutch silver appears to be tin, not only from its habitudes with re-agents, but from the consideration that there is no other cheap white metal of sufficient ductility. It is somewhat more than ten times as thick as gold leaf, and about two and a half times as heavy under equal surfaces.

The thinneft tin-foil in fmall fheets for filvering looking-glaffes, is about one-thoufandth of an inch thick, or near three hundred times the thicknefs of gold leaf. It ufually contains lead.

3. *Globules for Microfcopes.*

AT the beginning of the prefent century, the fimple microfcope was very much ufed. Among other advantages, it poffeffes the very defirable requifites of fimplicity and cheapnefs. In particular it is an inftrument not difficult to be conftructed by fuch ingenious philofophers as by narrow circumftances and remote fituations are obliged to have recourfe to their own fkill and ingenuity for experimental implements. The hiftory of natural philofophy abounds with inftances of eminent men who come under this defcription. To thefe at leaft it will be of importance to know a ready method of forming very bright fpherules of glafs for microfcopic ufes. The ufual method has been to draw out a fine thread of the foft white glafs called cryftal, and to convert the extremity of this into a fpherule by melting it at the flame of a candle. But this glafs contains lead, which is difpofed to become opake by partial reduction, unlefs the management be very carefully attended to. I find that the hard glafs ufed for windows feldom fails to afford excellent fpherules. This glafs is of a clear bright green colour when feen edgeways. A thin piece was cut from the edge of a pane of glafs lefs than one-tenth of an inch broad. This was held perpendicularly by the upper end, and the flame of a candle was directed upon it by the blow-pipe at the diftance of about an inch from the lower end. The glafs became foft, and the lower piece defcended by its own weight to the diftance of about two feet, where it remained fufpended by a thin thread of glafs about one five-hundredth of an inch in diameter. A part of this thread was applied endways to the lower blue part of the flame of the candle without the ufe of the blow-pipe. The extremity immediately became white-hot, and formed a globule. The glafs was then gradually and regularly thruft towards the flame, but never into it, until the globule was fufficiently large. A number of thefe were made, and, being afterwards examined by viewing their focal images with a deep magnifier, proved very bright, perfect, and round.

4. *On the Plumb-Line and Spirit-Level.*

THE fpirit-level is an inftrument fubftituted inftead of the plumb-line for determining the level. It feldom happens that wire can be procured fo fine as the one-thoufandth of an inch in diameter. Upon examining this with the microfcope, it fcarcely ever proves truly cylindrical. It may with reafon be fuppofed that wire, from its curl upon the bobbin, and its elafticity, may not, even when loaded with a confiderable weight, form an accurate right line. As the radius of a circle is equal to 206265 feconds of meafure nearly, the diameter of fuch a wire would cover a fpace on the line equal to 206 feconds on an inftrument of one inch radius, or 17 feconds if the radius were 12 inches. How far this quantity, in the bifection of two dots, of near half a minute broad, may be affected by the caufes of error here mentioned, muft be afcertained, in each particular inftance, by careful experiments. But thefe confiderations, joined to the circumftance that the fpirit-level is fomewhat more portable and eafy of application, have tended to bring this laft very delicate inftrument into eftimation with aftronomers.

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The ſpirit-level conſiſts of a tube of glaſs nearly filled with the pureſt ardent ſpirit, or with highly rectified ether. In order that this inſtrument may anſwer the intended purpoſe, it is neceſſary that the inner ſurface ſhould not be cylindrical or ſtraight on that ſide which is placed uppermoſt when in uſe. Suppoſe this tube to be laid nearly in the horizontal poſition; the ſpirit will occupy the lower ſpace, and the empty part above will exhibit the appearance of a bubble, which will run to the higher end. It follows therefore, ſuppoſing the tube to be ſlightly curved, and the convexity uppermoſt, that the bubble will be near the middle when the tube is level. In the portable ſpirit-level, the tube is properly ſet in braſs, and fixed by means of ſcrews in a ſmall braſs trough, the bottom of which is ground very ſtraight. The ſcrews are uſeful to place the bubble in ſuch a poſition that the lower ſurface of the trough may be parallel to a tangent ſuppoſed to be applied to the middle point of the curve of the level. The adjuſtment is effected without much difficulty, by placing the level on an adjuſtable plane, and then reverſing it. If the bubble ſtand accurately in the ſame poſition between two marks made on the tube in both ſituations of the level, it follows that neither end of the plane nor of the lower ſurface of the frame of the level is elevated; or in other words, that every ſurface to which the level may be applied, and on which the bubble ſtands in the poſition here mentioned, is horizontal.

This eaſy praxis may be effected in various ways, according to the nature and figure of the inſtrument of which the poſition is to be determined: but the accuracy of the reſult will depend upon the ſenſibility of the level; that is to ſay, the ſpace paſſed over by the bubble for every minute or ſecond of the quadrant, and the certainty with which, under circumſtances preciſely ſimilar, it ſhall arrive at the ſame poſition. In the beſt levels the curve muſt be circular; for in ſuch the bubble will move with more activity, ſettle itſelf with more certainty, and deſcribe equal ſpaces by equal changes of inclination. An ordinary good ſpirit level will exhibit a movement of upwards of half an inch for each minute of inclination, and alter the poſition of its bubble by a change of five ſeconds, or leſs. In ſuch a tube the radius of the curve will be about 150 feet. But extraordinary levels are much more delicate. De la Lande * ſpeaks of a level filled with ether, the bubble of which paſſed over fourteen inches by equal ſpaces of one-tenth of an inch for every ſecond. The radius of this curve was conſequently 1719 feet, or near one-third of a mile.

The tubes of ſpirit-levels are ſelected by trial. If a long piece of tube be nearly filled with ardent ſpirit, and corked at the ends, the run of the bubble may be tried with a ſuitable inſtrument called the level-trier, throughout the whole length on all ſides. By this means it may be known whether, and in what parts, it may be deſirable to divide the tube for the purpoſes of filling and cloſing. It is remarkable that theſe tubes in general prove either good throughout, or good for nothing; for it ſeldom happens, where one good level can be taken, that the remainder is unſerviceable. A reſpectable mathematical inſtrument-maker aſſures me, that he finds it a good practice to go to the glaſs-houſe and cauſe the tubes to be drawn without ſuffering them to be turned round. The uſual practice in drawing tubes is, that two workmen extend a piece of blown glaſs, while hot, by drawing the ends aſunder; while a third cools the thinner parts, by blowing the air againſt it with a hat. To prevent the glaſs from bending downwards by its own weight, the workmen are in

* In his Aſtronomie; not now at hand. I ſpeak from memory; but have no doubt of the numbers, nor of the fact.

the habit of turning the iron pipes they hold in their hands, both the fame way, as they walk backwards. This is the practice which, by producing undulations and irregularities in the tube, is fuppofed to render them unfit for fpirit-levels. I have remarked, that fuch tubes as have veins, or extended bubbles, running lengthways very ftraight through the fubftance, have afforded good levels.

But the moft regular and accurate levels are obtained by grinding the infide of the tube. I do not know that this operation requires any particular management capable of being defcribed in words; though there is no doubt but in this, as well as in every other mechanical procefs, facility and fkill will be acquired by practice. A cylindrical piece of wood is turned fo as to go eafily through the portion of tube intended to be ground. It is then worked in the tube with water and fine emery in the ufual way. As foon as the polifh has by this means difappeared on one fide, the tube is cleaned, filled, and tried; and accordingly as its figure proves to be more or lefs ftraight or curved, the grinding is either repeated or difcontinued. Some operators polifh the infide again after grinding; but this has not been found to increafe their fenfibility.

From the great delicacy of the fpirit-level, compared with the few obfervations here prefented on the plumb-line, the former inftrument may appear greatly to deferve the preference. Aftronomers are not however agreed on this point. When a fpirit-level is adjufted by reverfing, at a certain temperature, and both ends of the bubble marked, it may be allowed that the inftrument may be fuccefsfully applied to ufe. But if the temperature be raifed, the fpirit will expand, and of courfe the bubble will become fhorter. Whence it appears neceffary that a divifion and adjufting-piece fhould be applied, from experiment, to afcertain the true ftation of the bubble at different temperatures; and even this application feems fcarcely adequate to fupply the place of repeated adjuftments. The variation of the bubble will differ according to the quantity of fpirit contained in the tube. In two good levels, of nearly the fame magnitude and figure, I found it amount to one-fifth of an inch for every ten degrees of Fahrenheit. The bubble therefore may be one inch longer in winter than in fummer, which in thefe individual levels amounts to near one-third of the fummer length. The curvature of a fpirit-level will alfo vary from unequal temperature; fuch, for example, as may arife from one end of the tube being touched or breathed upon, while the other end is left at the original temperature. The error from each of thefe caufes may amount to feveral minutes, as is eafily fhewn by trial; but I do not find that the prefence or abfence of funfhine caufes any perceptible difference. It is probable that the rays may not fpeedily alter the temperature, on account of the tranfparency. And with regard to thefe three laft fources of error, it muft be allowed that they are eafy to be avoided, and indeed not likely to be prefent in the operations of accurate obfervers.

MATHE-

MATHEMATICAL CORRESPONDENCE.

Question I. *Anſwered by the Propoſer.*

LET a be the length of the whole line, and $x, y, z, v, w,$ &c. the parts into which the half of it is to be divided. Then $\frac{a}{x} = \frac{a-x}{y} = \frac{a-x-y}{z} = \frac{a-x-y-z}{v}$ &c. by the queſtion: Or $y = \frac{x}{a}(a-x)$, $z = \frac{x}{a}(a-x-y) = \frac{x}{a}(\overline{a-x} - \frac{x}{a} \times \overline{a-x}) = \frac{x}{a}(\overline{a-x} \times \overline{1-\frac{x}{a}}) = \frac{x}{a}(\overline{a-x} \times \frac{a-x}{a}) = \frac{x}{a^2}(a-x)^2$, $v = \frac{x}{a}(a-x-y-z) = \frac{x}{a}(\overline{a-x} - \frac{x}{a} \times \overline{a-x} - \frac{x}{a^2} \times \overline{a-x}^2) = \frac{x}{a} \times \overline{a-x}\,(1 - \frac{x}{a} - \frac{x}{a^2} \times \overline{a-x}) = \frac{x}{a} \times \overline{a-x}\,(\frac{a-x}{a} - \frac{x}{a^2} \times \overline{a-x}) = \frac{x}{a} \times \overline{a-x}\,(\frac{a \times \overline{a-x} - x \times \overline{a-x}}{a^2}) = \frac{x}{a^3} \times \overline{a-x}^2 \times (a-x) = \frac{x}{a^3} \times \overline{a-x}^3$, $w = \frac{x}{a}(a-x-y-z-v) = \frac{x}{a}(a - x - \frac{x}{a} \times \overline{a-x} - \frac{x}{a^2} \times \overline{a-x}^2 - \frac{x}{a^3} \times \overline{a-x}^3) = \frac{x}{a^4} \times \overline{a-x}^4$, &c. But $x+y+z+v+w$, &c. $= \frac{a}{2}$, by the queſtion. Whence, alſo, $x + \frac{x}{a}(a-x) + \frac{x}{a^2}(a-x)^2 + \frac{x}{a^3}(a-x)^3 + \frac{x}{a^4}(a-x)^4$ &c. to n terms $= \frac{a}{2}$; which laſt being a geometrical ſeries, whoſe firſt term is x, and the ratio $\frac{a-x}{a}$, its ſum, by the common rule, is

$$\frac{x\left(\overline{\frac{a-x}{a}}\Big|^{n} - 1\right)}{\frac{a-x}{a} - 1} = \frac{a}{2},$$

from whence x is found $= a \times (1 - \frac{1}{2^{\frac{1}{n}}})$; and conſequently the ſeveral diviſions of the half line are $a \times (1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{1}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{2}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, $\frac{a}{2^{\frac{3}{n}}}(1 - \frac{1}{2^{\frac{1}{n}}})$, &c. where n is any number whatever.

Question II. *Anſwered by the Propoſer.* See Fig. 12. Plate VI.

LET AF $= x$, AC $= r$, $c =$ celerity, or velocity in the circumference, and F $=$ variable force in the ſine EF.

Then CE : FC : : EG : EK by ſim. Δ^s. or $r : r-x :: c : \frac{c}{r}(r-x) = v =$ velocity in FE. But, by the laws of variable forces, $F\dot{t} = \dot{v} = -\frac{c}{r}\dot{x}$, and $\dot{t} = \frac{\text{flux. EF}}{v} = \frac{\text{flux. } \sqrt{2rx - x^2}}{\frac{c}{r}(r-x)} = \frac{r\dot{x} - x\dot{x}}{\sqrt{2rx - x^2}} \times \frac{r}{c\,(r-x)}$; whence $\frac{F\,(r-x)\,\dot{x}}{\sqrt{2rx - x^2}} \times \frac{r}{c\,(r-x)} = -\frac{c}{r}\dot{x}$, or $\frac{Fr}{c\sqrt{2rx - x^2}} = -\frac{r}{r}$, and confequently $F = -\frac{c^2}{r^2}\sqrt{2rx - x^2} = -\frac{c^2}{r}\sqrt{\frac{2rx - x^2}{r^2}}$. But when the body, in revolving, comes to D, x then becomes $= r$; in which cafe, $F = -\frac{c^2}{r}$, or F is as $\frac{c^2}{r}$, or as the fquare of the velocity divided by the radius.

NEW MATHEMATICAL QUESTIONS.

QUESTION V. *By W. SIMPSON.*

A BOTTLE which held exactly 5 oz. 106 gr. of diftilled water, was capable of containing $5\frac{1}{2}$ oz. and 170 gr. of a folution of common falt in diftilled water: Required the weight of the falt held in folution, its fpecific gravity being 2.8?

QUESTION VI. *By J. B.*

THE mean annual temperature of any two latitudes, in the fame hemifphere, being given, to determine the mean annual temperature of any other latitude in that hemifphere?

To Mr. NICHOLSON, Editor of "The Journal of Natural Philofophy, Chemiftry, and the Arts."

SIR,

I DO not know whether you will confider the following as of fufficient importance to merit a place in your valuable publication;—if fo, I fhall perhaps occafionally, in future, trouble you with other attempts of a fimilar nature.

Being about four years ago engaged in fome mathematical fpeculations, in the courfe of which the refolution of pretty high equations became frequently neceffary, I was induced to fet about the inveftigation of an eafy arithmetical rule for the folution of an adfected quadratic, in order to fhorten the bufinefs of approximation.—It is, indeed, you know, on account of the prolixity of the ordinary methods by completing the fquare or exterminating the fecond term, that Dr. Halley's method of obtaining the approximate value of the root, by taking fuch terms of the affumed equation as involve the fquare of the converging quantity, is now feldom ufed; fuch only as contain its firft power being ufually admitted into the calculation. The mode which I adopted for this purpofe will be beft underftood by a ftatement of the principles from which it is deduced.

Let $x^2 + ax$ be $= b$. Then will the affirmative value of x be $= \frac{1}{2}(\sqrt{a^2 + 4b} - a.) = \frac{1}{2} \times : \frac{4.b}{2.a} - \frac{4^2.b^2.}{2.4.a^3.} + \frac{1.3.4^3.b^3.}{2.4.6.a^5.} - \frac{1.3.5.4^4.b^4.}{2.4.6.8.a^7.} + \frac{1.3.5.7.4^5.b^5.}{2.4.6.8.10.a^9.}$ &c. . . . . $= \frac{b}{a} - \frac{b^2.}{a^3.} + \frac{2.b^3.}{a^5.} - \frac{5.b^4.}{a^7.} + \frac{14.b^5.}{a^9.}$ &c. But in order to obtain this latter feries, we have only to divide b by a, adding to the original divifor a, every time of repeating the divifion, double the quotient already found, together with the term which fuch new divifion fhall produce, as in the common operation for extracting the fquare root, as follows:

$a +$

$$a + \frac{b}{a} \;\Big)\; b \;\Big(\; \frac{b}{a} - \frac{b^2}{a^3} + \frac{2.b^3}{a^5} - \frac{5.b^4}{a^7} + \frac{14.b^5}{a^9} - \frac{42.b^6}{a^{11}} \text{ \&c.}$$

$$b + \frac{b^2}{a^2}$$

$$a + \frac{2b}{a} - \frac{b^2}{a^3} \;\Big)\; \div - \frac{b^2}{a^2}$$

$$- \frac{b^2}{a^2} - \frac{2.b^3}{a^4} + \frac{b^4}{a^6}$$

$$a + \frac{2b}{a} - \frac{2b^2}{a^3} + \frac{2b^3}{a^5} \;\Big)\; \div + \frac{2b^3}{a^4} - \frac{b^4}{a^6}$$

$$+ \frac{2.b^3}{a^4} + \frac{4.b^4}{a^6} - \frac{4.b^5}{a^8} \;\ldots \text{ \&c.}$$

$$a + \frac{2b}{a} - \frac{2b^2}{a^3} + \frac{4.b^3}{a^5} - \frac{5b^4}{a^7} \;\Big)\; \div - \frac{5.b^4}{a^6} + \frac{4b^5}{a^8} \;\ldots \text{ \&c.}$$

$$- \frac{5b^4}{a^6} - \frac{10.b^5}{a^8} \;\ldots \text{ \&c.}$$

$$a + \frac{2.b}{a} - \frac{2.b^2}{a^3} + \frac{4.b^3}{a^5} - \frac{10.b^4}{a^7} + \frac{14.b^5}{a^9} \;\Big)\; \div + \frac{14.b^5}{a^8} \;\ldots \text{ \&c.}$$

$$+ \frac{14.b^5}{a^8} \;\ldots \text{ \&c.}$$

$$\div$$

The variations which a change of ſigns in the original equation will produce are too obvious to need deſcription.

Hence we get the following rules for the arithmetical ſolution of the three forms of quadratics, which may be reduced to two caſes.

CASE I. $x^2 \pm ax = b$.

RULE.—*Place the numerical values of a and b as diviſor and dividend, as in common diviſion.—Having found the firſt figure of the quotient, add it in its proper place (which is the ſame as that of ſuch figure of the dividend b as ſtands over the units place of the firſt product of the increaſed diviſor into ſuch quotient figure, when placed for ſubtraction) to the original diviſor a; multiply the ſum by ſuch added figure, and ſubtract the product from the dividend, as in common diviſion. Then, if the next figure of the quotient to be found will fall under any of the figures of the firſt increaſed diviſor, ſo as not to increaſe the number of its digits when added to it, bring down the next figure of the dividend, as in common diviſion; but, if ſuch next figure of the quotient will, when added in its proper place, increaſe the number of figures in the diviſor, bring down the two next figures, as in the extraction of the ſquare root.—Find ſuch next figure of the quotient, and add both the figures of the quotient to the firſt increaſed diviſor for a new diviſor (as in extracting the ſquare root); multiply ſuch laſt increaſed diviſor by the laſt found figure; ſubtract the product; bring down another figure or figures of the dividend, and proceed as before: always adding to the laſt diviſor the two laſt figures of the quotient, including the quotient-figure obtained by each diviſion, for a new diviſor, as in extracting the ſquare root; and ſo on till the operation be finiſhed.*

The

The quotient so obtained is a value of x, whose sign is the same as that of the second term of the original equation :—for it's other value add to such quotient the original divisor a, and prefix to the sum the contrary sign.

EXAMPLE I.

Given . . . $x^2 + 5613x = 2579080$. Quære x.

a = ...	5613		
	+ 4..		
1st Divisor	= 6013 . . . .)	2579080 (	+ 427. = x
	+ 42	24052''	5613.
2d D. . .	= 6433 . . . .)	17388.	— 6040. = x.
	+ 27	12866.	
3d D. . .	= 6460 . . . .)	45220	
		45220	
		.0.	

EXAMPLE 2.

Given $x^2 + 13.24687x = 123.45678932$. Quære x.

a	= 13.24687.		
	+ 6		
1st Divisor	= 19.24687.)	123.45678932 (	+ 6.3120464569 . . &c. = x
	+ 63	115.48122'''	13.24687
2d D. . .	= 2554687.	7975569	— 19.5589164569 . . &c. = x.
	+ 31	3115083	
		5293962	
3d D. . .	= 2585687	1201880.	
	+ 12	167045.	
		11820.	
4th D. . .	= 2586887	1472.	
	20	179.	
		24.	
5th D. . .	= 2587087	2.	

EXAMPLE 3.

Given $x^2 - 45x = 522.47695432$. Quære x.

a	= 45		
	+ 9		
1st Divisor . .	= 54)	522.47695432 (	— 9.573773622 . . &c. = x
	+ 95	486	45
2d D.	= 635	3647	+ 54.573773622 . . &c. = x
	+ 57	47269	
		242054	
3d D.	= 6407.	4962532.	
	+ 73	472263.	
		23236.	
4th D.	= 64143	3992.	
	+ 37.	144.	
		16.	
5th D.	= 641467.	3.	

CASE II. $x^2 - ax = -b$.

RULE.—*Proceed as before, except that inſtead of adding the two laſt figures of the quotient to each diviſor, as in the former Caſe, they muſt in this be ſubtracted from it, and the remainder will be the new diviſor for each repetition of the diviſion.*

The quotient is the leſſer value of the root, which, ſubtracted from the original diviſor a, will give the other value, both of them being affirmative. If b be greater than the ſquare of $\frac{1}{2}a$, the ſolution is however impoſſible.

EXAMPLE.

Given $x^2 - 19.23456x. = - 83.57214567.$ Quære x.

a	$= 19.23456.$		
	$-\ 6$		
1ſt Diviſor . .	$= 13.23456.$)	83.57214567. (	$+\ 6.630654987.$ &c. $= x$.
	$-\ 66$	79.40736.	19.23456.
2d D.	$= 663456.$	4164785	$+12.603905012.$ &c. $= x$.
	$-\ 63$	1840496	
		3912870.	
3d D.	$= 600456.$	328494.	
	$-\ 30$	29796.	
		5900.	
4th D.	$= 597456.$	524.	
	$-\ 06$	46.	
		5.	
5th D.	$= 597396$		

It is unneceſſary to remark, that the contractions ordinarily made uſe of in the extraction of the ſquare root are equally applicable here; and they are accordingly applied in all the examples except the firſt.

I am inclined to believe that ſuch as uſe this proceſs will find it attended with ſome advantages in point of expedition, eſpecially where a conſiderable number of places is required in the root—and being mathematically correct, it may of courſe be extended to any length.

I am, Sir, your moſt obedient Servant,

9th *May* 1797. J. F—:—:—R.

SCIENTIFIC NEWS.

Account of Memoirs publiſhed by the Polytechnic School at Paris.

[Continued from page 95.]

It has long been known, that the ſulphure of potaſh attracts the oxygene in the atmoſphere, and ſeparates it from the azote with which it is combined. Attempts were conſequently made to conſtruct eudiometers with this ſulphure in imitation of Scheele; but none of theſe gave the refult with the requifite fpeed and accuracy. Guyton difcovered, that by heating the fulphure the combination is made inftantly. He therefore advifes to take a fmall retort filled with water, to place therein the fmall piece of fulphure, then to introduce the air and heat with a taper to that place on which the fulphure refts. The abforption of the oxygene is immediately made, and the defired refult afcertained.

By this invention, which contributes to the accuracy and value of an inftrument greatly ufeful in meteorological obfervations, Guyton has rendered a real fervice to philofophers.

Gottling had maintained, in a memoir publifhed in Gren's Journal, that phofphorus burns in azotic gas, forms an acid by this combuftion, and does not burn in oxygenous gas. Berthollet made feveral experiments on the combination of phofphorus with different gafes. He found, 1. That the phofphorus does not combine with oxygene at the ordinary temperature, but requires a more elevated temperature to produce combuftion. 2. That phofphorus is foluble in the azotic gas; and that, by means of this folution, it burns with oxygenous gas at the ordinary temperature, and appears luminous in the dark.

By means of thefe experiments, Berthollet explained the extraordinary phenomena which Gottling, Lemps, and Lampadius, had obferved; and he imagined that phofphorus might be ufed to meafure the quantity of oxygene gas contained in the air of the atmofphere, or any other gas containing azote.

This new eudiometer has an advantage over other inftruments of the fame nature, by fhewing the precife inftant of the total abforption of oxygene, which happens when no more white vapour is feen in the day light, nor light in an obfcure place; but it has likewife the inconvenience of requiring the prefence of a quantity of azote fufficient to diffolve the phofphorus readily before its combination with oxygene.

In the analyfis of the calcedony of Creufot, of which the conftituent parts proved to be, filex 86.08, iron 7.68, alumine 4.11, lime 1.16, lofs 1.0, Guyton aimed lefs to afcertain the proportions of ingredients in a ftone fo variable, than to prefent to the pupils of the fchool an example of the analyfis of ftones. In this refpect his memoir is one of the moft inftructive in the collection.

A great number of chemifts, with Pott at their head, have laboured at the folution of the interefting problem of the fufibility of earths, upon which they have made many fcientific experiments. To thefe Guyton has added a great number of his own. He firft tried the fufibilities of the earths with oxygene gas, and afterwards the effect of carbonate of foda, calcined borax, pulverized ammoniacal phofphate of foda, and the red oxide of lead. He afterwards mixed the earths two and two with the preceding fluxes, and at laft terminated by the action of the earths upon each other, and that of calcareous phofphate on each of them. The feries of facts muft be confulted in the memoir itfelf, as their connection does not allow of detached extracts.

Clouet had publifhed in a memoir, that the pruffic colouring principle might be obtained by paffing ammoniacal gas through ignited charcoal. The experiments related by Bonjour, in the third Cahier of the Journal of the Polytechnic School, confift merely of the proceffes he made ufe of, and the trials he made in the prefence of Clouet to verify the phenomenon he had announced, and which, after a moft careful examination, proved to be in every refpect agreeable to the account which was publifhed.

2 The

The memoir of Fourcroy and Vauquelin on the ſulphureous acid, conſiſts in an examination or hiſtory of its action on every known ſubſtance. They, in the firſt place, examined this action

upon	caloric oxygene gas,		
next on the acids	ſulphuric nitric muriatic	with the alkalis	potaſh ſoda ammoniac,
afterwards on the combuſtibles	hydrogen phoſphorus phoſphorated hydrogen gas ſulphurated hydrogen gas carbon	with the earths	alumine lime barytes magneſia.

In every caſe wherein the ſulphureous acid formed a ſalt by combination, theſe chemiſts have deſcribed the form of its cryſtals, its fuſibility, the affinities and proportion of conſtituent parts.

This memoir preſents a complete table of the binary combinations of the ſulphureous acid, and renders it very deſirable that the authors ſhould continue, for the advancement of ſcience, to publiſh ſimilar accounts of a great number of ſubſtances, on which our notions are yet very incomplete.

The three memoirs of general phyſics: 1. Of the congelation of mercury, by Haſſenfratz, Bonjour, and Welte. 2. The laws of dilatation in elaſtic fluids, by Prony. 3. The influence of ſnow and rain on vegetation, by Haſſenfratz.

It has long been known, that mercury was rendered ſolid at Peterſburg by cold 24 degrees of Reaumur below zero, and that in this ſtate it becomes malleable. The experiment was repeated at London by Cavendiſh, by producing the ſame degree of artificial cold; but this reſult had never been obtained in France.

The memoir of Haſſenfratz on the congelation of mercury contains only a repetition of this experiment, in which the proportion of caloric, abſorbed during the congelation of the metal, is determined, which had not before been done by any philoſopher. This experiment has afforded a very curious reſult. In water the caloric, abſorbed during the tranſition from the ſolid to the fluid ſtate, is ſufficient to have raiſed its temperature 60 degrees Reaumur; and in mercury the quantity to produce the ſame effect would have been ſufficient to raiſe the temperature 64 degrees.

In order to aſcertain the principle and the laws to which a great number of phenomena are referable, it is neceſſary to make a ſeries of reiterated experiments on the ſame facts. This is attended with two kinds of difficulties, namely, the time they require, and the imperfection of the inſtrument.

Nevertheleſs, when the exiſtence of a law in the progreſs of any phenomenon is aſcertained, it is always poſſible, from a ſmall number of experiments, to diſcover by analyſis an equation which may expreſs the law. This is the object of the memoir which Prony has printed in the ſecond Cahier of the Journal of the Polytechnic School. It is divided into two parts. In the firſt he inveſtigates a method of interpolation applicable to the phenomena which depend on elaſtic fluids; in the ſecond he applies this method to aſcertain the law of of dilatability in these fluids. For this application he avails himself of the experiments made by Prieur, and published by Guyton, on the dilatability of oxygene gas, azote, hydrogenous, nitrous, carbonic acid, and ammoniacal gases; and lastly, on the expansive force of the vapour of water and that of alcohol.

This memoir contains tables deduced from the equations afforded by the methods made use of by the author, and the results obtained by Prieur. They are likewise expressed by engraved curves.

It was formerly observed, that snow preserves the vegetative power, and that rain accelerates the growth of plants more than artificial waterings. Hassenfratz has enquired, in his memoir, into the cause of these comparative effects. He shews, from several experiments, that the preserving power of snow arises from two circumstances—its imperfect conducting power, and its oxygenation. This philosopher has proved, that snow is water oxygenated and converted into the solid form. He has also shewn, that rain is more oxygenated than any other water, and that a large portion of its good effects is to be attributed to this circumstance.

The memoir on the arts is written by Chaussier. Its object is to describe the composition of a liquor proper to be substituted instead of wine lees in fulling, to obtain the greatest effect with the most facility and economy.

The liquor indicated by Chaussier is a small quantity of sulphuric acid. This acid bath, which experience has assured him may be used with invariable success, likewise affords the advantage of obtaining a very white colour in hats, because the felt does not become coloured in the working.

Extract of a Letter from Professor LORENZ DE CRELL.

MR. KLAPROTH* is continuing his chemical analysis, and will shortly publish a second volume of them. He has found that the newly discovered *Titanium* is by no means so rare as might be supposed from its not having been known till lately. He has detected it in a singular kind of mineral found at Aschaffenberg by Prince Gallitzin, and in some other minerals, as well as in menakanite, in which it is mixed with iron.—Mr. Westrumb's method of obtaining ardent spirits from different sorts of grain, proves to be excellent, and succeeds well on a large scale as a branch of trade.—Sulphate of barytes does not require more than an equal quantity of well aërated alkali for its decomposition both in the humid and dry way. Muriate of barytes is obtained perfectly pure by boiling it in ten times its weight of highly rectified alkohol vini, which dissolves nothing but this muriate.—To separate hepatic gas and carbonic acid gas when united, employ acidulated acetite of silver or mercury. Either of these metallic salts will absorb the hepatic gas, and leave the carbonic acid gas untouched.

* This article was forwarded to the learned editor of the English translation of Crell's Journal; from whom I received it in a letter, containing very encouraging expressions of approval and good wishes for the success of the present undertaking.

AIR PUMP by the REVD. JOHN PRINCE

Fig 11.

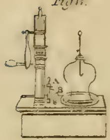


Fig 10.

Fig 8

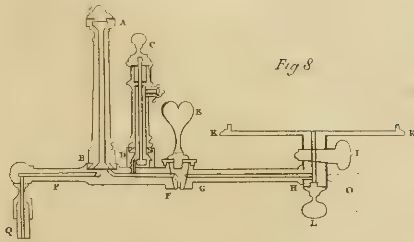


Fig 6.

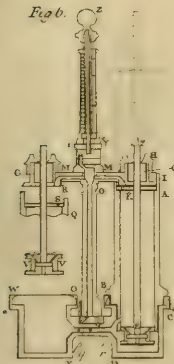


Fig 7.

Fig 9.

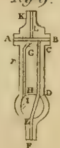
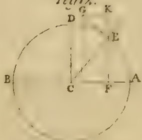


Fig 12.



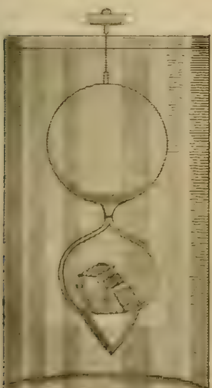


Fig 1

Gravimeter

Fig. 4.

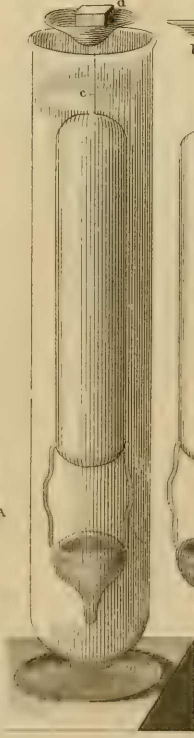


Fig. 2.

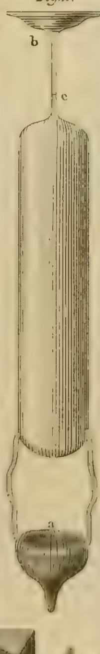
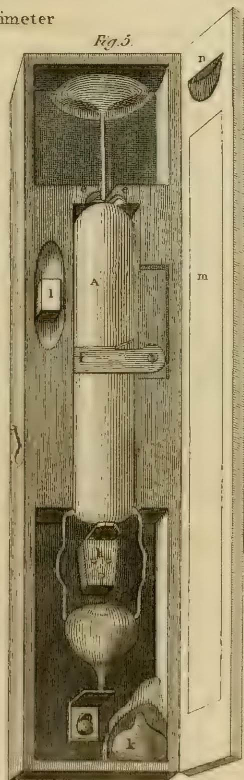
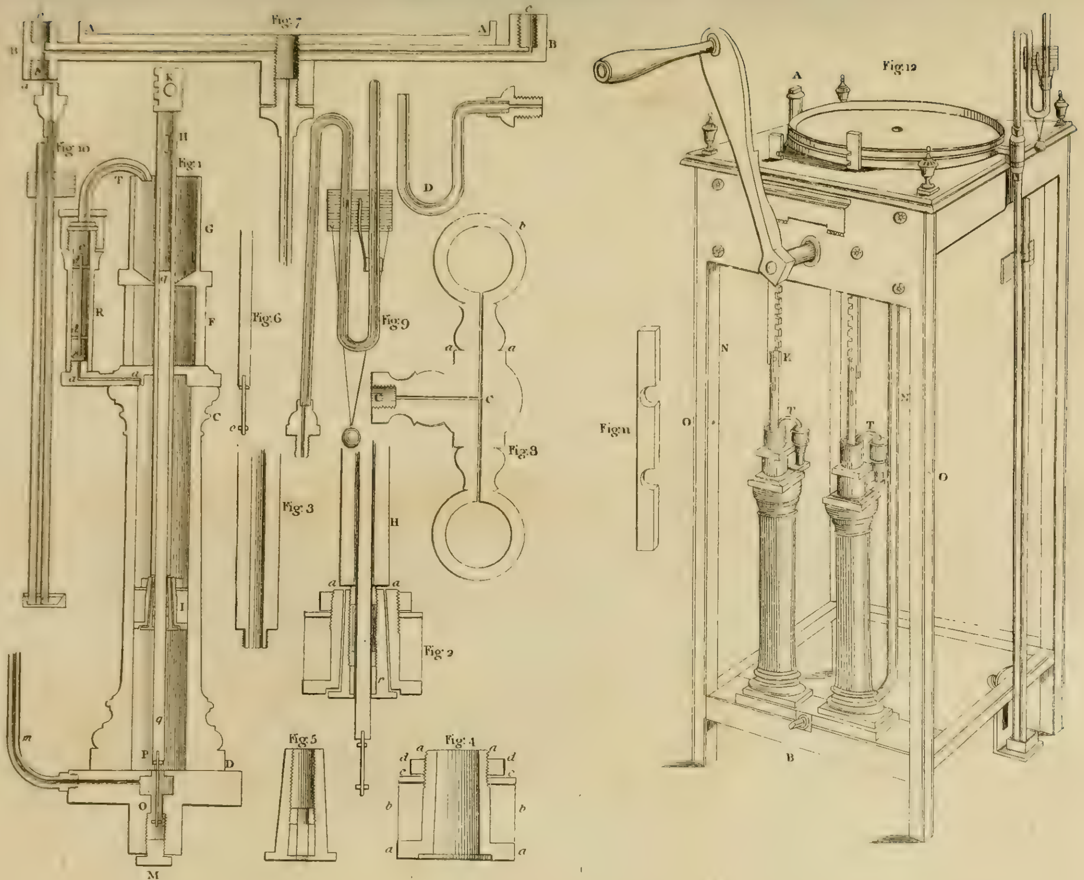


Fig 3.

Fig. 5.



The AIR PUMP of CUTHBERTSON



Philos Journal Vol.2.Pl. 7 facing p.241

A

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AND

THE ARTS.

JULY 1797.

ARTICLE I.

Obſervations on Horizontal Refractions which affect the Appearance of Terreſtrial Objects, and the Dip or Depreſſion of the Horizon of the Sea. By JOSEPH HUDDART, *Eſq. F. R. S.**

THE variation and uncertainty of the dip in different ſtates of the air, taken at the ſame altitude above the level of the ſea, was the reaſon of my turning my thoughts to this ſubject, as it renders the latitude obſerved incorrect, by giving an erroneous zenith diſtance of a celeſtial object.

I have often obſerved, that lowlands, and the extremity of headlands, or points forming an acute angle with the horizon of the ſea, and viewed from a diſtance beyond it, appear elevated above it, with an open ſpace between the land and the ſea. The moſt remarkable inſtance of this appearance of the land, I obſerved at Macao, for ſeveral days previous to a typhon, in which the Locko loſt her topmaſts in Macao roads; the points of the iſlands and lowlands appearing the higheſt, and the ſpaces between them and the ſea the largeſt, I ever ſaw. I believe it ariſes and is proportional to the evaporation going on from the ſea. And in reflecting upon this phenomenon, I am convinced that thoſe appearances muſt ariſe from refraction, and that, inſtead of the denſity of the atmoſphere increaſing to the ſurface of the ſea, it muſt decreaſe from ſome ſpace above it; and that evaporation is the principal cauſe which prevents the uniformity of denſity and refraction being continued by the general law down to the ſurface of the earth. And I am inclined to believe, though I mention it here as a conjecture, that the difference of ſpecific gravity in the particles of the atmoſphere may be a principal agent in evaporation; for the corpuſcles of air, from their affinity with water, being combined at the ſurface of the fluid from expanſion, form air

* Philoſophical Tranſactions for 1797.

ſpecifically lighter than the drier atmoſphere; and therefore float or riſe, from that principle, as ſteam from water; and in their riſing (the ſurrounding corpuſcles from the ſame cauſe imbibing a part of the moiſture) become continually drier as they aſcend, yet continue aſcending until they become equally denſe with the air *. However, theſe conjectures I ſhall leave, and proceed to the following obſervations upon refractions:

In the year 1793, when at Allonby in Cumberland, I made ſome remarks on the appearance of the Abbey-head in Galloway, which in diſtance from Allonby is about ſeven leagues; and from my window, at fifty feet above the level of the ſea, at that time of tide, I obſerved the appearance of the land about the Head as repreſented in Pl. VIII. Fig. 1. There was a dry ſand *x y*, called Robin Rigg, between me and the Head, at the diſtance from my houſe of between three and four miles; over which I ſaw the horizon of the ſea H O; the ſand at this time was about three or four feet above the level of the ſea. The hummock *d* is a part of the headland; but appeared inſulated or detached from the reſt, and conſiderably elevated above the ſea, with an open ſpace between. I then came down about twenty-five feet, when I had the dry ſand of Robin Rigg *x y* in the apparent horizon, and loſt all that floating appearance ſeen from above; and the Abbey-head appeared everywhere diſtinct from the ſurface of the ſand. This being in the afternoon, the wet or moiſture on the ſand would in a great meaſure be dried up. I have reaſon therefore to conclude, that evaporation is the cauſe of a leſs refraction near the ſurface of the ſea; and when ſo much ſo as to make an object appear elevated wholly above the horizon (as at *d* in Fig. 1.) there will from every point of this object iſſue two pencils of rays of light which enter the eye of the obſerver, and that below the dotted line AB (parallel to the horizon of the ſea HO) the objects on the land will appear inverted.

To explain this phenomenon, I ſhall propoſe the following theory, and compare it with the obſervations which I have made. Suppoſe HO, Fig. 2. to repreſent the horizontal ſurface of the ſea, and the parallel lines above it the lamina or ſtrata of corpuſcles, which, next the fluid, are moſt expanded, or the rareſt; and every lamina upwards increaſing in denſity, till it arrive at a maximum (and which I ſhall in future call the maximum of denſity) at the line D C, above which it again decreaſes in denſity *ad infinitum*.

Though this in reality may be the caſe, I do not wiſh to extend the meaning of the word denſity farther than to be taken for the refractive power of the atmoſphere; that is, a ray of light entering obliquely a denſer lamina, to be refracted towards a perpendicular to its ſurface; and in entering a rarer lamina the contrary; which laminæ being taken at infinitely ſmall diſtances, the ray of light will form a curve agreeable to the laws of dioptrics.

In order to eſtabliſh this principle in horizontal refractions, I traced over various parts of this ſhore, at different times, when thoſe appearances ſeemed favourable, with a good teleſcope, and found objects ſufficient to confirm it; though it be difficult at that diſtance of the land to get terreſtrial objects well defined ſo near the horizon, as will afterwards appear.

* Mr. Hamilton, in his very curious Eſſay on the Aſcent of Vapours, does not allow of this principle even as an aſſiſtant; though by a remark (page 15) he takes notice of thoſe appearances in the horizon of the ſea, and ſays they ariſe from a ſtrong or unuſual degree of refraction; the contrary of which I hope to illuſtrate in the courſe of this paper.

One

One day, obferving the land elevated, and feeing a fmall veffel at about eight miles diftance, I from my window directed my telefcope to her, and thought her a fitter object than any other I had feen for the purpofe of explaining the phenomena of thefe refractions. The telefcope was forty feet above the level of the fea: the boat's maft about thirty-five feet; fhe being about twenty to thirty tons burthen. The barometer at 29.7 inches, and Fahrenheit's thermometer at 54°.

The appearance of the veffel, as magnified in the telefcope, was as reprefented in Fig. 3, and from the maft-head to the boom was well defined. I pretty diftinctly faw the head and fhoulders of the man at the helm; but the hull of the veffel was contracted, confufed, and ill defined. The inverted image began to be well defined at the boom (for I could not clearly perceive the man at the helm inverted), and from the boom to the horizon of the fea the fails were well defined; and I could fee a fmall opening above the horizon of the fea in the angle made by the gaff and maft; and had the maft been fhorter by ten feet (to the height of *y*), the whole would have been elevated above the horizon of the fea, and from *y* to *d* an open fpace. This drawing was taken from a fketch I took at the time, and reprefents the proportion of the inverted to the erect object, as near as I could take it by the eye; the former being about two thirds of the latter in height, and the fame breadth refpectively; though at one time, during my obfervation, which I continued for about an hour, I thought the inverted nearly as tall as the erect object. The day was fine and clear, with a very light air of wind, and I found very little tremor or ofcillation in viewing her through the telefcope.

I have laid down Fig. 4. for the explanation of the above phenomena, in which A reprefents the window I viewed B the veffel from; HO the curved furface of the fea; CD parallel to HO the height of the maximum of denfity of the atmofphere; the lines marked with the fmall letters *a a*, *b b*, *c c*, *d d*, the pencils of rays under their various refractions from the veffel to the eye or object-glafs of the telefcope.

The pencil of rays *a a*, from a point near the head of the mainfail, is wholly refracted in a curve convex upwards, being everywhere above the maximum of denfity; and the pencil of rays *d d*, which iffues from the fame point in the fail, and paffes near the horizon of the fea at *x*, is convex upwards from the fail to W, where it paffes the line of maximum of denfity, which is the point of inflection; there it becomes convex downwards, paffing near the horizon at *x* to *y*, where it is again inflected, and becomes convex upwards from thence to the eye. The pencil of rays *b b*, from the end of the boom, paffing nearly parallel to the horizon, and near the maximum of denfity, fuffers very little deviation from a right line in the firft part; but in afcending (from the curvature of the fea) will be convex upwards to the eye. The pencil of rays *c c*, from the fame point in the boom, may have the fmall part to *c* convex upwards: from *c* to *z* it will be convex downwards, and from *z* to the eye convex upwards.

From this inveftigation it appears that two pencils of rays cannot pafs from the fame point and enter the eye from the law of refraction, except one pencil pafs through a medium which the other has not entered; and therefore the maximum of denfity was below the boom, and could not exceed ten feet of height above the furface of the fea at the time thefe obfervations were made.'

Refpecting the hull of the veffel being confufed and ill defined in the telefcope, as by Fig. 3, it arifes from the blending of the rays from the different parts of the object

 refracted

refracted through the two mediums; some parts of the hull appearing erect, and some inverted. Suppose the dotted line *i i*, Fig. 4. an indefinite pencil of rays passing from between the inverted and erect parts of the object, or the upper part of the hull of the vessel to the eye (for the lower part of the hull could not be observed), the objects cannot appear inverted, except the angles at the eye *a* A *c* and *a* A *d* exceed the angle *a* A *i*; for the intermediate space could only be contracted by the secondary pencil of rays. The lengths of the inverted, compared with the erect image of the sail, is as the sines of the angles at the eye *a* A *i* to *i* A *d*; and the angle at the eye *a* A *d*, made by the two pencils of rays from the same point near the head of the sail, must be double the angle *a* A *i*, when the inverted image is as tall as the erect. In this case the sines of the angles *a* A *b*, *a* A *c*, *a* A *d*, Fig. 4, are proportional to the altitudes *a b*, *a c*, *a d*, in the magnified view of the vessel Fig. 3.

Under this consideration, no inverted image of the sail will be formed, until the angle at the eye, made by the two refracted pencils of rays *a a* and *d d*, exceed the angle made by *a a* and *b b*, the apparent height of the sail of the vessel; for, were those angles equal, the inverted sail would only be contracted into the parallel of altitude of the boom *b*, and render the appearance confused, as in the hull of the vessel.

Respecting the existence of two pencils of rays entering the eye from every point of an object not more elevated than *a*, or less than *i*, Fig. 3, in this state of the atmosphere, I cannot bring a stronger proof than that of the strength of a light when the rays pass near the horizon of the sea, proved by the following observations:

Going down Channel about five years ago in the Trinity yacht, with several of the elder brethren, to inspect the light-houses, &c. I was told by some of the gentlemen who had been on a former survey, that the lower light of Portland was not so strong as the upper light at near distances; but that at greater distances it was much stronger. I suspected that this difference arose from the lower light being at or near the horizon of the sea, and mentioned it at the time; but afterwards had a good opportunity of making the observation. We passed the Bill of Portland in the evening, steering towards the Start; a fresh breeze from the northward, and clear night. When we had run about five leagues from the lights, during which time the upper light was universally allowed to be the stronger, (several gentlemen keeping watch to make observations thereon), the lower light drawing near the horizon suddenly shone with double lustre. Mr. Strachan, whose sight is weak, had for some time before lost sight of both lights; but could then clearly perceive the lower light. I then went aloft (as well as others), but before I got half mast up, the lower light was weaker than the upper one. On coming down upon deck, I found it again as strong as before. We proceeded on, and soon lost the lower light from the deck; and upon drawing the upper light near the horizon, it, like the former, shone exceeding bright. I again went aloft, when it diminished in brightness; but from the mast-head I could then see the lower light near the horizon as strong as before. This is in consequence of the double quantity of light entering the eye by the two pencils of rays from every point. To illustrate which, we compare the vessel Fig. 4. to a light-house built upon the shore, and A the place of the observer; and having brought down the light so low as to view it in the direction *a a*, another light would appear in the horizon at *x*, from the pencil *d d*; and had the vessel been still enough to have observed it at this time with a good glass, I doubt not but the two images might have been distinctly seen. As the light dropped (by

increasing

increasing the distance) the two images would appear continually to approach each other, till blended with double light in one, and disappear at the altitude *i*, above the apparent horizon of the sea. But, as explained before, if the strength of evaporation did not separate by refraction the pencils *a a* and *d d* to a greater angle than double the angle that the lamps and reflectors appear under, the two images would be blended, and the strong appearance of light would be of shorter duration. The distance run from the lights during the time each of the lights shone bright, would have been useful; but this did not occur at the time, nor have I had the like opportunity since. However, I recommend to the mariner to station people at different heights in looking out for a light, in order to get sight of it near the horizon, when it is always strongest.

Respecting the appearance of the Abbey-head, before mentioned, Fig. 1, the dotted line AB represents the limit, or the lowest points of the land that can be seen over the sea; for, as above stated, all the objects appearing below this line are the land above it inverted; and where the land is low, as at *d* and *m*, it must appear elevated above the horizon of the sea.

In Figure 5, let HO represent the curve of the ocean, and *d* the extreme top of the mount visible at A by the help of refraction; the dotted pencil of rays *c c* passing from *d* to the eye, in some part a little below the maximum of density, where inversion begins; therefore no land lower than this can be seen; for any pencil from a point in the land lower than this must, in the refraction, have a contrary flexure in the curve, and therefore pass above the observer. Let AD be a tangent to the curve at A, then the object *d* will appear to be elevated by refraction to D; also let A *v* be a tangent to the pencil A *x* at A, then the angle DA *x* will appear to be an open space, or between D and the horizon of the sea. Suppose a star should appear very near and over the mount *d*, as at *, two pencils would issue from every point of it, and form a star below as well as above the hummock *d*. There are always confused or ill defined images of the objects at the height of the dotted line, Fig. 1, above the level of the sea, as before mentioned; and instead of the points of *d* ending sharp in that line, they appear blunted, and the Abbey-head is frequently insulated at the neck *m*.

I have viewed from an elevated situation a point or headland at a distance beyond the horizon of the sea, forming, as in Fig. 6, a straight line AB, making an acute angle BAO with the horizon of the sea. Seeing the extreme point blunted and elevated, I descended; and though in descending the horizon cut the land higher, as at HO, HO, yet the point had always the same appearance as *a a a*, Fig. 6, though the land is known to continue in the direction of the straight line AB to beneath the horizon, or nearly so, as viewed from the height above.

If then, from a low situation, we view this headland through a telescope, the inclination of the surface A B to the horizon being known to be a straight line, it will appear as in Fig. 7; the dotted line (at the height of the point where a perpendicular *x y* would touch the extreme of the land) being at the limit or lowest point of erect vision. And if a tangent to the curved appearance of the land *a b* is drawn parallel to the inclined surface of the land A B, Fig. 6, touching it at C, the point C will shew the height of the maximum of density, where the pencil of the rays of light from thence to the eye approaches nearest the sea; for pencils of rays from this land, taken at small distances from C, will form parallel curves nearly through the refracting mediums, and C will be the point of greatest refraction;

tion; for above C, as to B, the refraction somewhat decreasing, will appear below the line $a b$, or the parallel to the surface of the land, and the refractions decrease below the point C; for, had they increased uniformly down to the surface of the sea, it would render the apparent angle of the point of land z more acute than the angle C a O, contrary to all observations.

Thus I have endeavoured to explain the phenomena of the distorted appearance of the land near the horizon of the sea when the evaporation is great, and when at the least I never found the land quite free from it when I used a telescope, and from thence infer that we cannot have any expectation to find a true correction for the effect of terrestrial refraction by taking any certain part of the contained arc; for the points z CB, Fig. 7, will have various refractions, though they are at nearly the same distance from the observer. And if the observations are made wholly over land, if the ground rises to within a small distance of the rays of light, in their passage from the object to the eye, as well as at the situation of the object and observer, the refractions will be subject to be influenced by the evaporation of rains, dews, &c.; which is sufficiently proved by the observations of Colonel Williams, Captain Mudge, and Mr. Dalby, Phil. Trans. 1795, p. 583.

The appearances mentioned by Colonel Williams, Captain Mudge, and Mr. Dalby (Phil. Trans. 1795, p. 586, 587) cannot be demonstrated upon general principles, as they arise from evaporation producing partial refractions. In those general principles it is supposed that the same lamina of density is everywhere at an equal distance from the surface of the sea, at least as far as the eye can reach a terrestrial object; but in the partial refractions, the lamina of the expanded or rarefied medium may be of various figures, according to circumstances, which will refract according to the incidence of the rays, and affect the appearance of the land accordingly, which I have often seen to a surprising degree. But my principal view is to shew the uncertainty of the dip of the sea, and that the effect of evaporation tends to depress the apparent horizon at x when the eye is not above the maximum of density; and from hence the difficulty of laying down any correct formula for these refractions whilst the law of evaporation is so little understood; which indeed seems a task not easy to surmount. The effect indicated by the barometer and thermometer is insufficient; and should the hydrometer be improved to fix a standard for moisture in the atmosphere, and shew the variations near the surface of the ocean, which certainly must be taken into the account (evaporation going on quicker in a dry than a moist atmosphere), the theory might still be incomplete for correcting the tables of the dip. I shall therefore conclude this paper, by shewing a method I used in practice, in order to obviate this error in low latitudes.

When I was desirous to attain more accurately the latitude of any headland, &c. in sight, I frequently observed the angular distances of the sun's nearest limb from the horizons upon the meridian both north and south, beginning a few minutes before noon, and taking alternately the observations each way, from the poop, or some convenient part of the ship, where the sun and the horizon both north and south were not intercepted; and having found the greatest and least distances from the respective horizons, which was at the sun's passing the meridian, and corrected both for refraction, by subtracting from the least, and adding to the greatest altitude, the quantity given by the table; and also having corrected for the error of the instrument, and the sun's semi-diameter, the sum of these two angular distances reduced as above—180° is equal to double the dip, as by the following

Example.

Example.

The ſun's declination 4° 32′ 30″ and its ſemi-diameter 15′ 58″ took the following obſervation:

	South.	North.
The meridian diſtance of the ſun's neareſt limb from the horizon of the ſea —— ——	78° 36′ 30″	= 101° 1′ 20″
Refraction per table —— ——	— 0 11	= + 0 11
Diſtances corr. for refraction —— ——	= 78 36 19	= 101 1 31
Error of the ſextant —— —— ——	+ 1 32	+ 1 32
Sun's ſemi-diameter —— ——	+ 15 58	+ 15 58
	78 53 49	101 19 1
Half difference or the dip found —— ——	— 6 25	78 53 49
Altitude reduced —— —— ——	= 78 47 24	180 12 50
Zenith diſtance —— ——	= 11 12 36	180
		Diff. 12 50
The ſun's declination N. —— ——	= 4 32 30	Half = 6 25
		Dip.
Latitude of the ſhip N. —— ——	= 15 45 06	

I regret that I cannot in this paper inſert the dip which I have found in my obſervations; for I only retained the latitude of the ſhip determined thereby as is uſual at ſea. I generally rejected the error of the inſtrument, the dip, and ſemi-diameter, as they affect both obſervations with the ſame ſigns; and reduced the obſervation by the following method:

	South.	North.	
Sun's diſtance as before ——	78° 36′ 30″	101° 1′ 20″	
Refraction —— ——	— 0 11	+ 0 11	
Dif. corr. for refraction ——	78 36 19	101 1 31	101° 1′ 31″
		+ 78 36 19	
		Sum 179 37 50	
Sum of ſemi-diameter, dip, and refraction = half difference ——	+ 11 5	180	+ 11 5
	78 47 24	Diff. 22 10	
		Half 11 5	101 12 36
	— 90		90
The zenith diſtance as before ——	= 11 12 36	Zenith diſt.	= 11 12 36

It may be obſerved, that neither the dip, ſemi-diameter, or index error can affect the zenith diſtance of the ſun's centre; and the refraction being ſmall near the zenith, the reſult muſt be true, if the angles are accurately taken; and it is only neceſſary to obſerve, that when the ſum of the diſtances is leſs than 180, the half difference muſt be added to the diſtances, as by the laſt reduction. There is a difficulty in making this obſervation when the ſun paſſes the meridian very near the zenith, as the change in the azimuth from eaſt to weſt is too quick to allow ſufficient time: nor can it be obtained by the ſextant when the ſun paſſes the meridian more than 30 degrees from the zenith; for I never could adjuſt the back obſervation of the Hadley's quadrant with ſufficient accuracy to be depended upon *.

II.

Remarkable Effect of Terreſtrial Refraction on a Diſtant Headland.——Extract of a Letter from ANDREW ELLICOT *to* DAVID RITTENHOUSE, *Eſq. dated at Pittſburg, Nov.* 5, 1787, *concerning Obſervations made at Lake Erie* †.

ON the 13th of laſt month, while we lay on the banks of Lake Erie, we had an opportunity of viewing that ſingular phenomenon by ſeamen termed looming. It was preceded by a fine aurora borealis on the evening of the 12th. The 13th was cloudy, but without rain. About 10 o'clock in the morning, as I was walking on the beach, I diſcovered ſomething that had the appearance of land in the direction of Preſqu'iſle; about noon it became more conſpicuous, and when viewed by a good achromatic teleſcope, the branches of the trees could be plainly diſcovered. From three o'clock in the afternoon till dark, the whole peninſula was conſiderably elevated above the horizon, and viewed by all our company with admiration. There was a ſingular appearance attending this phenomenon, which I do not remember to have ſeen taken notice of by any writer. The peninſula was frequently ſeen double, or rather two ſimilar peninſulas, one above the other, with an appearance of water between. The ſeparation and coincidence was very frequent, and not unlike that obſerved in ſhifting the index of an adjuſted Godfrey's quadrant. As ſingular as this may appear, it is not more ſo than the double refraction produced by the Iceland cryſtal. The next morning Preſqu'iſle was again inviſible, and remained ſo during our ſtay at that poſition. Preſqu'iſle was about 25 miles diſtant; its ſituation very low.

The ſame evening the wind began to blow briſkly from about two points weſt of north,

* The optical phenomena which relate to the conſtitution of the atmoſphere have not yet been much inveſtigated. From the barometrical admeaſurement of heights, as well as from experiments with the macrometer, or marine barometer of Halley (Roy, in Phil. Tranſ. Vol. LXVII.), it is aſcertained that air abounding with humidity is more expanſible by heat, and leſs denſe at like temperatures than air which is more dry. And from Sir Iſaac Newton's table, the refractive power of air, in reſpect to its denſity, compared with that of water in the ſame reſpect, is as 4160 to 7845 (Optics, Part III. Prop. 10.). Theſe and other facts ſhew that the rays of light muſt be variouſly affected, according to the nature and circumſtances of evaporation, condenſation, and other proceſſes carried on in the air, and particularly near the ſurface of water. That a lighter and leſs refractive ſtratum of air may thus be generated near the ſurface of the ſea to a conſiderable elevation, and remain for a time in equilibrio without aſcending, is highly probable; and in this caſe, whenever the quieſcent ſtate is from local or temporary circumſtances deſtroyed, it ſeems a natural conſequence, that ſudden and irregular currents of the lower air, and deſcending blaſts of the upper, may occaſion all the ſtrange effects of the typhon and other phenomena, not well deſcribed in that intereſting tropical combination of ſea and land to the eaſtward of Sumatra and Malacca. The Mediterranean Sea affords ſimilar appearances, particularly the aſtoniſhing ſpectra in the Strait of Reggio, deſcribed by Kircher, Minazi, and others, called Fata Morgana, of which I purpoſe ſhortly to give an account, with an engraving. N.

† American Philoſ. Soc. III: 62.

and

and continued to increafe till the evening of the 14th, when it was more violent than any thing of the kind I had ever been witnefs to before, and continued till the evening of the 16th, without the leaft intermiffion. Our tents were all blown down, and we were under the neceffity of fortifying our camp by driving pofts near to each other firmly into the ground on the windward fide, and filling up the vacuities with bufhes in the form of an hedge. During the continuance of this wind we frequently obferved fmall black clouds hanging over the lake: they had but little velocity, and were fometimes exhaufted, and difappeared without reaching the fhore.

From the large bodies of timber blown down about the lakes, it appears that hurricanes are not uncommon. Coxe obferves in his Travels through Ruffia, that the lakes in that country are fubject to terrible ftorms.

III.

Extract of a Memoir of M. BENEDICT PREVOST, of Geneva, on the Emanations of Odorant Bodies. By Citizen FOURCROY.*

CITIZEN Duc la Chapelle, Director of the Society of the Sciences and Arts at Montauban, in the department of Lot, has forwarded to the Firft Clafs of the Inftitute, in the name of the Society, a Memoir of M. Benedict Prevoft, of Geneva, *concerning various means of rendering the emanations of odorant bodies perceptible to the fight.* The Firft Clafs of the Inftitute heard the reading with great intereft at its fitting of the 16th Pluviofe, in the year 5.

The following are the facts configned in this Memoir:

1. A concrete odorant fubftance, laid upon a wet glafs, or broad faucer, covered with a thin ftratum of water, immediately caufes the water to recede, fo as to form a fpace of feveral inches around it.

2. Fragments of concrete odorant matter, or fmall morfels of paper or cork, impregnated with an odorant liquor, and wiped, being placed on the furface of water, are immediately moved by a very fwift rotation. Romieu had made this obfervation on camphor, and erroneoufly attributed the effect to electricity. The motion was perceptible even in pieces of camphor of feven or eight gros.

3. An odorant liquor being poured on the water ftops the motion till it is diffipated by evaporation. Fixed oil arrefts the motion for a much longer time, and until the pellicle it forms on the water is taken off.

4. When the furface of the water is cleaned by a leaf of metal, of paper, or of glafs, plunged in and withdrawn fucceffively until the pellicle is removed, the gyratory motion is renewed. If a piece of red wax or of taper be dipped in water, and the drops fhaken off into a glafs of water containing odorant bodies in motion, the movement will be ftopped. The fame effect is not produced by metal.

5. An atmofphere of elaftic fluid is formed round odorant fubftances, and is the caufe of the effects here defcribed.

6. A morfel of camphor plunged to the depth of three or four lines in water, without floating, excites a movement of trepidation in the furrounding water, which repels fmall

* Annales de Chimie, XXI. 254—Tranflation. N.

bodies in its vicinity, and carries them again to the camphor by ſtarts The author concludes that an elaſtic fluid eſcapes from the odorant body in the manner of the fire of a fuſee, or the diſcharge of fire-arms.

7. When there is a certain proportion between the height of the water and that of the ſmall fragment of camphor, the water is briſkly driven off, returns again to the camphor, and again retires, as if by an exploſion, the recoil of which often cauſes the camphor to make part of a revolution on its axis.

8. Fragments of camphor of the ſize of a pea, floated upon water in a diſh of metallic leaf four or five lines in diameter, communicated motion to theſe diſhes, though leſs ſwift than was exhibited by the camphor alone. If the glaſs in which this experiment is made be nearly filled with water, and covered by a (plain) glaſs which intercepts the contact of the air, the motion decays and ceaſes.

9. Camphor alone moves more rapidly than when it is placed upon the metallic plate. The author infers that the immediate contact of the water favours the diſengagement of the fluid which produces the motion.

10. Camphor evaporates thirty or forty times more ſpeedily when placed upon water, than when entirely ſurrounded with air.

11. Camphor, during the act of diſſipation in the air, preſerves its form and its opake whiteneſs; upon water it is rounded, and becomes tranſparent, as if it had undergone a kind of fuſion. It may be inferred that it ariſes from the acquired motion, which cauſes it to preſent a greater ſurface to the air.

12. In fact, out of 12 equal ſmall pieces of camphor, ſix being ſuſpended under a glaſs with quick-lime, and very dry, and ſix others being ſuſpended in a moiſtened glaſs, together with a wet ſponge, the volatilization was alike in each; and the water in this caſe not touching the camphor did not appear to contribute to the effect.

13. It is neceſſary for this purpoſe that the water ſhould directly touch the camphor. Accordingly, fragments of camphor placed on blotting-paper continually wetted, are diſſipated with the ſame ſpeed, and become equally tranſparent, as when actually placed on water, though they exhibit no motion.

14. When ſmall pieces of camphor are plunged in water, the camphor becomes rounded and tranſparent, does not acquire any motion, and its diſſipation is leſs perceptible than in the air. The concurrence of air and water is therefore neceſſary to diſengage the fluid which is the cauſe of the motion and total diſſipation of odorant bodies.

15. The motion of odorant bodies upon water decays and ceaſes ſpontaneouſly at the end of a certain time; becauſe, the water having then contracted a ſtrong ſmell, the volatilization takes place in all the points of its ſurface; and the ſmall maſs being thus ſurrounded by the odorant fluid, which is no longer air, diſſolves, as in the ordinary odorant fluids, without forming the gaſeous jet which is the cauſe of the motion. The author compares the volatilization of the aromatic ſubſtance to a combuſtion excited by water.

In this place the author dwells for a ſhort time on the phenomena he had deſcribed. He obſerves that theſe effects may be rendered palpable by touching the ſurface of the water on which the odorant ſubſtances move, with a pin dipped in oil. At the very inſtant, quick as lightning, theſe particles, as if ſtruck, ceaſe to move. A coloured pellicle, formed by the oil, is ſeen on the ſurface of the water. The water penetrates the pores of the oil, like

like thofe of the camphor, and difengages a fluid which prevents the water from penetrating the odorant fubftance. The motion ceafes, becaufe this fubftance is then plunged in a fluid formed by the oil, which fluid is not air.

16. All bodies which are not odorant prefent, when hot, the fame phenomena as odorant bodies. In fact, the heat gives them a degree of fmell. To this phenomenon our author refers the bubbles and the motion, which are feen when an ignited piece of money is thrown into water. The elaftic fluid is according to him the caloric, which is difengaged in proportion as the water penetrates the pores of the metal. It is neverthelefs much more fimple to attribute this phenomenon to the water converted into vapour round the ignited metal, and fometimes to hydrogene gas, when the metal is capable of decompofing water, efpecially at this temperature.

After the explanation of thefe experiments, Mr. Prevoft hopes with reafon that they will contribute to the theory of odours, which fo nearly refembles that of the gafes. He does not flatter himfelf with having exhaufted this fubject, but confiders his difcoveries as the means of rendering odour perceptible by water not only to the fight but even to the touch, as are likewife the vibrations of fonorous bodies. Men deprived of the fenfe of fmell, and even the blind, according to him, may in this manner diftinguifh odorant bodies from thofe which have no fmell. "Perhaps," fays he, "this kind of odorofcope may, by improvement, become an odorimeter. The exceptions, fuch for example as that of the cerumen of the ears, which produces much effect on water without being perceptibly odorant, and that of the fingers when hot or moift, are merely apparent; for, if our fenfes do not in thofe cafes difcover odour, thofe of animals more powerfully energetic, fuch as the dog, perceive and diftinguifh individuals by its peculiar character. The odorofcope may afford the information which is wanting refpecting thefe effluvia. Thus it is that the fat of game, the fmell of which is nearly to us imperceptible, is very much fo to dogs, and exhibits fenfible marks by the odorofcope."

Mr. Prevoft, who profeffes much brevity in this memoir, affirms, that water placed on the odorant liquor, inftead of this laft being put upon the water, affords a phenomenon confiderably interefting, but which he has not defcribed. A fpeedy communication of the reft of this interefting work is much to be defired.

We fhall not in this place explain the manner in which it appears neceffary to confider the principle of fmell, which no longer appears to us proper to be regarded as a peculiar matter, and one of the immediate materials of vegetables, always identical, and enjoying the fame properties. We fhall have occafion in future to enter more fully into this fubject, and perhaps to rectify the notions which have hitherto been formed concerning the fpiritus rector or aroma of plants. We fhall content ourfelves with obferving, that the phenomena defcribed with clearnefs and precifion by Mr. Prevoft, as well as the theory he has given, are referable to the attraction of the odorant matter in the mafs for air and water, and the folution which takes place in one or the other, or in both at the fame time.

IV.

A Method of meaſuring the Force of an Electrical Battery during the Time of its being charged. By Lieutenant Colonel HALDANE *.

LET the battery be inſulated; and at a ſmall diſtance from it place an uninſulated electrical jar; alſo near to the jar place one of Mr. Cuthbertſon's electrometers.

The electrometer being adjuſted according to the degree of force which is intended to be employed as a meaſure of force to be communicated to the battery, connect the electrometer with the jar; make a metallic communication between the interior ſide of the jar and the exterior ſide of the battery, and connect the interior ſide of the battery with the conductor of an electrical machine.

Then, by the operation of the electrical machine, the battery receives a quantity of the electrical fluid, and becomes charged. The fluid which departs from the exterior ſide of the battery, is received by the electrical jar, which alſo becomes charged; but this jar, being connected with the electrometer, explodes as ſoon as it acquires a force ſufficient to put the electrometer into motion.

Now, the quantity of the electrical fluid which is received by this jar, between each of the exploſions, is a meaſure of the quantity of the fluid in the battery; and the number of exploſions or diſcharges of this jar ſhews the number of meaſures which the battery contains, and conſequently the force which it is capable of exerting when diſcharged.

Demonſtration.

THE electrometer remaining under the ſame adjuſtment will require the ſame force to put it in motion: this force reſults from the quantity of electrical fluid received by the jar; and ſince it is admitted, that, when effects are the ſame, the cauſes of them muſt be equal, it is evident that the quantity of electrical fluid contained in the jar at the time of each exploſion is the ſame.

It is alſo obvious, that the ſum of all theſe equal quantities of the electrical fluid which was contained in the jar at the time of each exploſion, is equal to the whole quantity contained in the battery; for, the battery being inſulated, the jar received all the electrical fluid which departed from the exterior ſide of the battery; and that quantity is ſaid (in the theory of Dr. Franklin) to be equal to the quantity in the interior of the battery.

Therefore it is manifeſt that the number of exploſions or diſcharges of the electrical jar, is the number of equal meaſures of the electrical fluid which the battery contains.

But without putting too much confidence in any philoſophical theories, the effects of this operation may be more ſatisfactorily ſhewn by the following experiments:

Experiments.

A PIECE of iron wire, about 0.045 inches in diameter, and about two inches in length, was placed in the circuit through which the diſcharge of a ſmall electrical battery, which contained about ſix feet ſuperficial of coated glaſs, was to paſs.

The electrical jar employed as the meaſure of the charge of the battery contained about 90 ſquare inches; and the adjuſtment of the electrometer was varied in each ſet of

* Communicated by the Author.

experiments,

experiments, by changing the weight applied to the balance, and alſo the diſtance of the diſcharging balls.

Experiment 1.

THE electrometer being adjuſted with its leaſt weight; the diſcharging balls placed at the diſtance of one inch; and the other parts of the apparatus arranged as before deſcribed; the electrical machine was put in motion, the battery and alſo the jar began to receive a charge, as was ſhewn by the repulſion of a pith-ball on a graduated quadrant placed upon the electrometer:

1. After the firſt exploſion of the electrical jar, that is, after the battery had received one meaſure of the electrical fluid, a diſcharging rod was applied to complete the circuit in which the iron wire was placed; but, upon the diſcharge of the battery, no change of appearance was viſible in the wire.

2. The operation of the electrical machine being continued, the diſcharging rod, after two exploſions of the jar, that is, after the battery had received two meaſures, was applied as before; but, upon the diſcharge of the battery, no change appeared on the wire.

3. The battery was then charged with three meaſures; and upon diſcharging it as before, luminous particles of the wire were thrown off.

4. The battery, having received four meaſures, the wire, upon the diſcharge, exhibited nearly the ſame appearance as before.

5. The battery, having received five meaſures, was diſcharged; the wire was red-hot, and ſeparated.

6. The battery, having received ſix meaſures, was diſcharged; the wire was diſperſed in red-hot globules.

The battery, upon receiving between nine and ten meaſures, made a ſpontaneous diſcharge.

Experiment 2.

IN the ſecond ſet of experiments, the apparatus was arranged as before, and the electrometer adjuſted with the ſame weight, but the diſcharging balls were placed at the diſtance of two inches. The reſults were, upon diſcharging the battery, after having received

1 Meaſure,	-	No alteration in the wire.
2 Meaſures,	-	Luminous particles thrown off.
3 Meaſures,	-	The ſame, with ſmoke.
4 Meaſures,	-	Red-hot, and ſeparated.
5 Meaſures,	-	Diſperſed in red-hot globules.
Between 7 and 8 Meaſures,	-	A ſpontaneous diſcharge of the battery.

Experiment 3.

THE apparatus being arranged as before, the electrometer was adjuſted with its greateſt weight, and the diſcharging-balls placed at the diſtance of one inch: the reſults were, upon diſcharging the battery, after having received

1 Meaſure,	-	No alteration in the wire.
2 Meaſures,	-	Luminous particles thrown off.
3 Meaſures,	-	The ſame.

4 Meaſures,

4 Meafures,	-	Red-hot, and feparated.
5 and 6 Meafures,	-	Red-hot, and difperfed in globules.
Between 8 and 9 Meafures,	-	A fpontaneous difcharge.

Experiment 4.

THE electrometer being adjufted with the greateft weight, the difcharging-balls were placed at the diftance of two inches. The refults were:

1 Meafure,	-	Luminous particles thrown off.
2 Meafures,	-	The fame, with fmoke.
3 Meafures,	-	Red-hot.
4 Meafures,	-	The wire was difperfed in red-hot globules.
Between 6 and 7 Meafures,	-	A fpontaneous difcharge of the battery.

Experiment 5.

THE apparatus remained as in the fourth experiment, with the addition of another battery, containing 12 feet fuperficial of coated glafs; and the iron wire placed in the circuit of the difcharge of this battery was about 0.08 inches in diameter, and two inches in length. The refults were:

1 Meafure,	-	No alteration in the wire.
4 Meafures,	-	Luminous particles thrown off.
6 Meafures,	-	The fame, with fmoke.
8 Meafures,	-	Red-hot, and feparated.
10 Meafures,	-	Difperfed in red-hot globules.
Between 15 and 16 Meafures,	-	A fpontaneous difcharge.

Harley-ftreet; May 27, 1797. HENRY HALDANE.

The electrical machine employed in thefe experiments has a glafs cylinder of nearly 18 inches in diameter. It was conftructed by Mr. Nairne, and is a moft powerful inftrument, particularly in exhibiting all the phenomena in which the negative, or electricity of the rubber is concerned.

V.

On the Mechanical Conftruction and Ufes of the Screw.

ELEMENTARY writers on mechanics, from demonftrations founded on the leading property of the lever, teach us that the action of any power to raife a weight or overcome an obftacle is facilitated by the inclined plane in the proportion of its length to its perpendicular height, when the power operates in the direction of the plane itfelf, or as the horizontal bafe to the height when the power acts in an horizontal direction. They alfo fhew, by the eafy procefs of applying a right-angled triangle to the furface of a cylinder, that the thread of a fcrew, when its axis is placed in the perpendicular, is in effect an inclined plane, whofe length in one turn is meafured by the portion of the thread itfelf, and height is the diftance between two contiguous threads: that is to fay, it would be juft as eafy to raife a weight by ſliding it along this thread, as along an inclined plane of the ſame length and height. The female ſcrew, or nut containing an hollow ſcrew, is in fact a weight adapted to this ſliding proceſs, and does accordingly correſpond with the weight on the ſimilar inclined plane, provided the power be applied at the very thread of the ſcrew; but if the advantage of a lever be taken to move the nut, the reſiſtance will be leſs in proportion as the radius of the ſcrew is leſs than the length of the lever.

Hence it is ſeen that the ſcrew affords a convenient and powerful application of the inclined plane to mechanical purpoſes. To compute its effect, whether a lever be uſed or not, is equally ſimple; for the power, wherever it be applied, is directed in a plane at right angles to the axis of motion; and the agent itſelf deſcribes the thread of a ſcrew conſtantly riſing through the ſame quantity in each turn. The proportion of the power to the weight, will therefore be as the diſtance between two contiguous threads to the circumference of the cylinder in which the power moves. Or in the form of a rule. Multiply the length of the lever by 44, and divide the product by 7; the quotient will bear the ſame proportion to the diſtance between thread and thread as the weight does to the power when in equilibrio.

In mixed hydraulic engines, ſuch as water-mills, pumps, and the like, the uſual addition allowed to overcome frictions, and give the velocity of working, is three-eighths of the power in equilibrio. The ſcrew requires more than this, but the quantity muſt vary according to the obliquity of the thread.

The obliquity is greater the coarſer the thread, and the ſmaller the diameter of the generating cylinder. For ſlow and ſtrong preſſures a fine thread is to be preferred; but for a quick ſtroke and ſpeedy return, as in the printing-preſs and coining-engine, the thread muſt riſe with conſiderable ſpeed.

The man of ſcience is moſt particularly intereſted in the conſideration of this mechanical organ, from the extreme accuracy with which it may be applied in the meaſurement of lines or ſpaces. Thus, for example, if the threads of a ſcrew be one-fiftieth of an inch apart, every turn in a fixed nut will cauſe its extremity to advance or recede that quantity: and if a graduated circular plate of one foot in diameter be fixed to the other extremity, the parts of each turn may be aſcertained. One quarter of a degree, or the 1440th part, on ſuch a circle, will be a very conveniently viſible diviſion, and will correſpond with the 1440th part of one-fiftieth of an inch, which is leſs than the ſeventy-thouſandth part of an inch.

A queſtion of no ſmall importance immediately preſents itſelf on this occaſion. Does the practice juſtify this theoretical concluſion? or, in other words, Do not the irregularities of workmanſhip ſhew themſelves to be much greater than the quantities here attempted to be meaſured? The ſolution, which will ſhew the true value of the meaſuring ſcrew, muſt be deduced from experiment.

The moſt obvious method of forming a ſcrew conſiſts in tracing helical lines upon the ſurface of a cylinder, and filing or cutting away the part between the intended threads. In this way ſome large ſcrews are actually made; but it is immediately perceived on reflection, that the original tracing is ſubject to all the inaccuracies of diviſion by hand; to which muſt alſo be added the ſtill greater errors of filing or cutting. By this proceſs alone it would be impracticable to make a ſcrew even of moderate correctneſs.

If

If a number of deep marks, fuch as c d, be cut obliquely acrofs the thread with a three-fquare file, in a fteel fcrew AB Figure 1. Plate IX. of the kind here defcribed, and the fcrew be then hardened, and tempered to the pale ftraw-colour, it may be ufed to form the inftrument Fig. 2. called a fcrew-tool. For the fcrew-tool being rendered quite foft by annealing, and fixed in a proper handle, will be cut into teeth by preffing the end A againft the fcrew AB Fig. 1, while turning in the lathe. It is fcarcely neceffary to fay that the fcrew-tool will be fupported by the Reft, and that oil or tallow muft be ufed to prevent the work from heating.

It is evident that the fcrew-tool will be much more regular than the fcrew it was made from; becaufe all the irregularities of the fcrew will pafs in fucceffion through each of its teeth. If the fcrew therefore be now foftened, and the fcrew-tool hardened and tempered to ftraw-colour, and whetted on the upper face, the former operation in the lathe may be repeated; but with this difference, that the fcrew-tool will now cut the fcrew, and render it ftill more regular, for the reafon juft mentioned. After this rectification, it is ufual to deepen the groove between the threads by applying an acute angled file or tool to the fcrew while turning.

The procefs of rectification naturally fuggefts an improvement in the order of working. It is certainly much more eafy to make a regular fcrew-tool by hand than a fcrew; more efpecially when the thread is fine. Suppofe Fig. 2. to be fuch a fcrew-tool, and the fcrew part of AB Fig. 1. to be merely cylindrical. If the hard fcrew-tool be then fteadily applied againft the foft cylinder while turning in the lathe, it will cut a number of equidiftant circular grooves; but if, inftead of being held in one place, the tool be regularly moved along the reft, the grooves will be helical; and the fucceeding teeth, falling into the marks made by thofe which went before, will render the threads equidiftant, and tend to produce more and more of regularity. Such a fcrew may neverthelefs vary in the obliquity of its threads on different fides, and this imperfection will be very little amended by continuing the action of the fcrew-tool. It would require too long a detail to ftate the manipulations by which workmen acquire the facility of making fcrews in this way with confiderable regularity; and it is befides probable that they would occur to any ingenious beginner as he proceeds.

Whether the tap (which is the name given to a fcrew referved for making or tapping female fcrews) be made and rectified in this or the other way, the next ordinary ftep is to tap a pair of dies. Figure 5. Plate IX. reprefents a die for making fcrews. Its thicknefs is fhewn in Fig. 6; and in Fig. 3. a pair of fuch dies are feen properly difpofed in the frame ABCD, called a ftock. The thicknefs of the ftock is precifely equal to that of the dies, which are well fitted to flide in the opening GHIK. On the lower fide of the ftock, as it now lies, is fixed, by rivets or fcrews, a flat plate of the dimenfions of Fig. 4, the width of the opening PPPP being fomewhat lefs than that of GHIK Fig. 3. It therefore affords a fupport to prevent the dies from falling through; and the plate Fig. 4. being applied on the upper fide of the ftock ferves effectually to prevent any motion whatever in the dies, except the direct approach or recefs with regard to each other by means of the fcrews EF. The top plate is fecured by four fcrews LLLL, which pafs through holes of the Figure reprefented in the fketch. The convenience of thefe holes is, that the plate may be taken off to change the dies without actually drawing the four fcrews. A variety of fafhions prevail in the form of ftocks, which are fuppofed to have their refpective advan-

 tages.

tages. Steadinefs, accurate fitting, and a regular fair action of the end preffure, are the effential requifites of a good ftock. The drawing here given is one of the moft fimple. When it is ufed for ftrong work, the ftock is fixed in a piece of wood terminating in a handle on each fide.

Suppofe the dies NN Fig. 3. to be foft; and a notch A Fig. 5, of about fixty degrees of the circumference of the tap E Fig. 1, not reckoning the thicknefs of the thread, filed in each. Let them be placed in the ftock, two pieces of brafs OO being put in to defend them againft the thumb-fcrews, and the whole fecured by the face plate. The dies are then ready for tapping. The hardened tap AB muft be fixed in the vice, with its end B uppermoft, and the dies be then made to bite gently upon its fcrew. When the ftock in this fituation (the cutting parts being previoufly well oiled) is moved backwards and forwards on the axis of the fcrew, the tap cuts an hollow thread in the dies, which becomes deeper and deeper as the fcrews E and F are driven inwards. The dies muft be taken out and examined from time to time during the procefs, which is to be continued until the thread is found to be as deep and perfect as may be expected. A notch C Fig. 6. is then to be filed in the bottom of the thread, the dies hardened and tempered, and the face A Fig. 5. whetted on the Turkey hone. The dies are in this ftate ready for tapping other fcrews.

For the fame reafon why an irregular fcrew will cut a more regular fcrew tool (Fig. 2.), it happens that the female fcrew is more regular than the male. The dies will therefore rectify the tap to a ftill greater degree of accuracy; and this tap, if thought neceffary, may be again ufed to rectify the dies.

Excellent fhort fcrews have been made by very flow and careful tapping with new fharp dies. The principal fources of error in this procefs are the following:

1. The ftop from change of hands produces a wave in the thread. This is frequently perceptible to the eye, and may be partly remedied by the workman walking round his work, or frequently fhifting the fcrew in the vice.

2. Preffure in the direction of the axis, by one hand more than the other, occafions a periodical change in the obliquity of the thread, and the fcrew is what the workmen call *drunk*. It is difficult to avoid this error when the fcrew is very fhort, or the dies thin.

Mechanical means have been ufed to remove thefe two fources of error, by fixing the ftock, and confining the tap to move by a regular rotation in the direction of its own axis. *

3. Since the corners of the dies cut firft, there will be little difpofition in the firft ftrokes to form the required fcrew. If the operation be not favoured by preffure in the direction of the axis, the dies will follow accidental irregularities of the furface or of the material; and as probably cut circular rings, or a left-handed fcrew, as the right-handed fcrew fuppofed to be in the dies. In thefe circumftances, therefore, the thread frequently proves undulated, with very little rife in the run of each corner of the die, until it fuddenly falls into the cut made by the corner it follows. Each turn confifts accordingly of four waves, which are amended as the dies fink deeper, and are led by their own flope. In extreme ftrictnefs it may be queftioned whether a tapped fcrew be ever begun without this irregularity, or perfectly corrected by the fubfequent operation.

4. If the dies be not well fitted in the ftock, or the material to be cut be veiny or unequally hard, they will recede from the hard parts, and produce an irregular thread. Long dies are leaft fubject to this imperfection.

5. The oppofite extremes of the thread in the dies incline towards different regions, and therefore in effect crofs each other. For this reafon, it is impoffible for two dies to be made to approach each other in the direction of the thread. In fact, they approach in a plane at right angles to the axis. This is the circumftance which limits the diameter and the depth and inclination of the cut, beyond which dies cannot operate. Hence alfo it is, that a true flat-thread fcrew cannot be cut in dies, and a many-threaded fcrew, or fcrew of great obliquity, can only be cut in the common ftock by two or three pair of dies in fucceffion.

6. As it is certain that a pair of well fitted dies can never run both along the fame ftroke till perfectly home to their natural place of coincidence, the cut made by one die will tend to draw the other along the cylinder. So that while one die cuts the upper fide of the thread, the other die will cut the oppofite or under fide. In this crofs action, the frame and the dies themfelves will yield from elafticity, and that the more where the material is the hardeft, or the work forced. Hence with a like preffure the foft fide will have the wideft cut, and be fooneft cut down, and the fides of the thread will be waving. This feems to be the chief reafon why tapping a fcrew renders it lefs ftraight and round than before.

7. The dies operate, rather in confequence of the force of preffure by means of the fcrews of the ftock than by the effect of their edge. This force muft not only be productive of an oval figure and other irregularities, from the uncertain fpring and yielding of the inftrument and material, but muft probably occafion a change, of the nature of lamination or hammering. Whenever this laft event happens the thread will be thinner, the cut wider, and the fcrew itfelf coarfer than it would otherwife have been. I have three flat-thread fteel fcrews, each 11 inches long, 0.6 in. outfide diameter, and depth of thread near 0.1 inch. They were tapped in dies 0.9 inch deep produced by an original tap containing 13 threads in 1.4 inch. The motion was flow and uniform between the centres of a kind of lathe conftructed for the purpofe. None of the fcrews are of the fame uniform finenefs from end to end. One fcrew is fcarcely coarfer than the original tap; another is half a thread coarfer in 102 threads, or 11 inches; and the third is a whole thread coarfer in the fame quantity. Thefe differences feemed to arife from the different degrees of foftnefs in the fteel; the laft having been more annealed than the fecond, and the firft not at all.

Hence is deduced the important conclufion, that however we may by mechanical expedients diminifh the irregularities, of obliquity and diftance of contiguous threads, in a tapped fcrew, yet in fcrews of feveral inches long there is an irregularity growing out of the nature of the materials, which does not appear to admit of any other remedy in this method than extreme care and flownefs of working.

The regular inclination of a fhort tapped fcrew of about forty threads in the inch, was found, by experiments at the end of a very fhort lever or radius fet by a fpirit-level, to be difcernible in much lefs quantities than the hundred thoufandth part of an inch.

Since all thefe fources of irregularity in tapping would be much diminifhed by increafing the number of dies, I have noted among my Agenda a ftock with four pair of dies acting at once with eight edges. In thefe the error of obliquity in coming up, as remarked in the numbered paragraph 5, and of the firft run (paragraph 3.) are incomparably lefs than in a fingle pair of dies. As I have not yet made it up, and every new inftrument is likely to be altered and fimplified during the actual conftruction, I fhall not particularly ftate ſtate either the dimenſions, or ſuch circumſtances as are neceſſary to inſure the truth of workmanſhip.

Fig. 7. Plate IX. repreſents a ſtock containing eight dies. HHIHI repreſents a braſs ring with parallel faces. It is perforated ſtraight through at the circle DDD. Upon a plate of braſs of the ſame diameter as the ring, are fixed the eight pieces marked with the letter A, which afford grooves for eight dies marked B to ſlide in. When the braſs plate is applied to the ring H, the pieces A with the dies occupy the internal part of the ring except the central hole at C; and in this ſituation the oppoſite or upper face of H forms one plane with the pieces A and the dies B. Every die is thruſt forward when required by the action of a nut F upon a ſcrew EK, which is prevented from turning by the form of the inner end K. The nuts F are properly ſecured in the ring H, ſo as to turn fairly and without ſhake, at the ſame time that they are ſupported againſt the re-action of the dies. A covering plate LLL is ſcrewed on to ſecure the dies and nuts in their places. The nuts F are provided with equal and ſimilar ſmall wheels, which juſt riſe above the plane of the ring, and are driven by a contrate wheel, attached to a ſecond covering plate, rather larger in diameter than the ring H. The edge of this laſt plate is milled, and it turns upon and is ſecured in its place by a central ring GGG fixed on the firſt covering plate by three binding ſcrews.

The uſe of this ſtock and dies needs very little explanation. Suppoſe the eight dies to be regularly numbered and put into their places; the pinions F all accurately ſet with a marked tooth uppermoſt in each; the upper plate ſcrewed on, and the contrate wheel piece applied to the pinions in a ſituation marked by the coincidence of diviſions made for that purpoſe on the central piece G. Imagine the ſtock in this ſituation to be well ſecured againſt the face of an upright poppet head, and another head at a proper diſtance to ſuſtain a metallic hole lying truly in the axis of this apparatus. Let the finiſhed and hardened tap be provided with a cylindrical tail to move in this hole, while the ſcrew part is ſupported by the faces of the eight dies. In this ſituation let the tap be gently turned by a handle of ſuch a conſtruction, that by means of a joint near the centre, and another near the holding part, it ſhall not be poſſible for the hand to exert any bearing in the direction of its axis. By a careful continuation of this proceſs, uſing oil, and ſetting up the ſyſtem of back ſcrews a very little at a time, the dies will at length receive the ſcrew, and may be rendered perfect by the ſeveral proceſſes of rectification before mentioned.

With ſuch an inſtrument, in ſkilful hands, I think the proceſs of tapping might be fairly tried in the conſtruction of fine meaſuring ſcrews, even in the hardened and tempered ſtate. A leading ſcrew might be uſed to drive the cylindrical part, if thought neceſſary, for the firſt taking of the dies. For notwithſtanding all the difficulties of the method by dies, and the improvements of ingenious artiſts in the method of turning ſcrews by a tool mechanically carried along the axis, there are ſufficient reaſons to wiſh that the tapping proceſs, on which they are originally founded, ſhould be brought to perfection. But the preſent communication is already of ſuch a length, as to require that the value and importance of thoſe methods ſhould be diſcuſſed in a future paper.

VI. *The*

VI.

The Method of obtaining the Fixed Alkalis in Crystals of the greatest Purity. By LOWITZ, *Professor, &c. at Petersburg* *.

1. THE caustic alkalis, as obtained by the ordinary processes, are sufficiently pure for the purposes of pharmacy and the arts; but for the experiments of philosophical chemistry the greatest purity is required in every substance made use of.

2. Caustic alkalis have hitherto been very seldom used in the analysis of mineral substances. M. Klaproth † has shewn their utility in analyses, not only in effecting a more easy separation of aluminous earth, but likewise to disunite the principles of the most refractory fossils. This circumstance has induced me to give a more particular description of the discovery I made some years ago of the crystallization of the caustic alkalis, as the principal means of obtaining them in a state of absolute purity. I have likewise embraced this opportunity of rectifying the mistake of various chemists, who have imagined that crystallization cannot take place but during the most extreme cold of winter.

3. My first experiments on vegetable alkali were made in the hottest days of summer, and the most beautiful crystallization was formed even over the fire.

4. The crystallization of soda does not succeed but in winter; but the ordinary cold of five degrees (R.) is sufficient for this effect. The reason why this alkali requires so low a temperature arises from the property its crystals possess of being soluble at the slightest heat in their own water of crystallization. The same thing is observable in the muriate of soda or common salt crystallized by cold ‡.

5. Before the discovery of this method, no one had succeeded by the ordinary processes to obtain a colourless lixivium of caustic alkali. These solutions were always more or less brown. By repeated crystallizations this lixivium may be had in the most concentrated state, as limpid as the purest water; which incontestably proves that the common lixivia are contaminated by heterogeneous matters, which charcoal and the strongest calcination are incapable of destroying.

6. The whole of the operation for obtaining a caustic alkali of the greatest purity and without the least colour, consists in this: A caustic lixivium of pot-ash is evaporated to a thick pellicle. After the cooling, the foreign salt which has crystallized is to be separated, and the evaporation of the lixivium continued in an iron pot, as in the preparation of the lapis causticus. During this second evaporation the pellicle of foreign salts, particularly of carbonate of pot-ash, which continues to be formed, must be carefully taken off with an iron skimmer. When no more pellicle is formed, and the matter ceases to boil up, it is removed from the fire, and suffered to cool, with continual agitation by an iron spatula. It is then to be dissolved in double the quantity of cold water, the solution filtered,

* Von Crell's Chemische Annalen auf 1796, b. i. s. 306.

† Beytrage zur Chemischen Kenntniss der Mineral Koerper, b. i.

‡ Lowitz obtained this singular crystallization of the muriate of soda by exposing a solution of this salt to intense cold. The crystals, which are hexagonal, are two inches in diameter, and one line thick. They liquefy at a few degrees below the freezing point of water, and fall into a very fine white powder at an extremely low temperature. Note of Van Mons in the Annals of Chemistry, XXII. 27.

and evaporated in a glafs retort, till it begins to depofit regular cryftals. If the mafs fhould confolidate ever fo little by cooling, a fmall quantity of water is to be added, and it muft be heated again to render it fluid. After the formation of a fufficient quantity of regular cryftals, the fluid, which is very brown, is to be decanted, and the falt, after being fuffered to drain, muft be re-diffolved in the fame quantity of water. The decanted liquor muft be kept in a well clofed bottle, and fuffered to become clear by fubfidence for feveral days. It muft then be decanted for a fecond evaporation and cryftallization. The procefs muft be repeated as long as the cryftals afford, with the leaft poffible quantity of water, folutions perfectly limpid. Thefe folutions are to be preferved in well clofed bottles, to defend them from the accefs of air.

7. The greateft difficulty of this procefs arifes from the facility with which the lixivium affumes the folid form. To obviate this inconvenience, a fmall portion of the lixivium may be concentrated to the point at which it becomes converted into a folid mafs by cooling. The faturation of a lixivium confiderably evaporated may be afcertained by throwing fmall pieces of this mafs into it during its cooling. When thefe are no longer diffolved, it is a proof that the lixivium is at the required point. I have fhewn, in a particular article on the cryftallization of falts, the principles on which this practice is founded *.

8. This method of proceeding is abfolutely indifpenfable in the cryftallization of foda, which in other refpects is made in the fame manner as that of pot-afh. The alkali would not fail, without this management, to form a folid mafs during its cooling.

9. With regard to the foreign falts which are mixed with the pot-afh, the greateft portion feparates by cryftallization after the firft evaporation of the lixivium. The reft is feparated during the fecond concentration by the continual fkimming of the pellicle. The little which may remain with the pot-afh muft precipitate, for want of water of folution, in a lixivium wherein the alkali itfelf is no longer diffolved but by its own water of cryftallization.

10. The property of cauftic alkalis to diffolve in highly rectified alcohol, with the exclufion of every foreign falt, would afford an excellent means of obtaining this falt very pure, if their mutual action did not afford a new fource of impurity. For when an alkali abfolutely pure and cryftallized is diffolved in fpirit of wine, even without heat, the fluid affumes a very brown colour, which becomes ftill deeper after decantation from the faline mafs.

11. The cryftallization of pot-afh is very different, accordingly as the cryftals are formed with cold or heat. In the firft cafe, the cryftals obtained are octahedrons in groups, which contain 0.43 water of cryftallization, and excite by their folution in water, even in the fummer, a degree of cold very near the point of aqueous congelation. In the fecond cafe, cryftalline tranfparent very thin blades, of extraordinary magnitude, are formed, which, by an affemblage of lines, in directions that crofs each other to infinity, prefent an aggregate of cells or cavities, moft commonly fo perfectly clofed that the veffel may be inverted without the efcape of the fmalleft drop of the lixivium, though fometimes included to the quantity of an ounce or two. For this reafon it is neceffary to break this fine cryftallization, that the fluid may run off. The cryftals prefent in their regular formation rectangular tetragonal blades, which, as they contain little water of cryftallization, produce a confiderable degree of heat when diffolved in water.

* Chem. Annal. auf 1795, b. i. f. 6.

12. By

12. By expofing fuch alkaline cryftals to a red heat in a very clean crucible, they become fufed; and, after cooling in proper moulds, afford a lapis caufticus as white as fnow, and extremely cauftic and deliquefcent.

13. As the cryftals and the lixivium, during the length of time required to drain the falt, may frequently become charged with a portion of carbonic acid, it is advifable, in order to avoid this inconvenience as much as poffible, that the lixivium, as foon as it is brought to the requifite point of faturation, fhould be poured into a narrow-necked bottle, and well clofed therein to cryftallize. After the cryftals are formed, the bottle is to be reverfed, without opening, and kept at a temperature rather warm until the cryftals are well dried. During the winter, the liquor, after the firft cryftallization, continues to cryftallize without being fubmitted to a new evaporation, provided only that it be expofed to a temperature fomewhat colder than that wherein the firft cryftals were formed *.

VII.

Experiments on Eau de Luce. By a Correfpondent.

To Mr. NICHOLSON, Editor of "The Journal of Natural Philofophy, Chemiftry, and the Arts."

SIR,

OBSERVING in your fecond number a paper on the compound called Eau de luce, I here fend you the refult of fome curfory experiments, which I was induced, for the benefit of my female friends, to make fome years fince on this article. You will perhaps yourfelf profecute them farther than I have had an opportunity of doing.

1. Macquer's recipe, and that in the London Pharmacopeia, were both tried; but neither of them fucceeded. I was however informed by a very intelligent apothecary, that the beft eau de luce might be, and always was, made without any other ingredient than oil of amber, foap, alcohol and ammoniac: he added, that thofe who are judges of this article detect in a moment the prefence of maftic, and efteem fuch as contains it of very little value. The proportions were accordingly varied. Different fpecimens of the oil were ufed—fome of the firft diftillation, others twice rectified—but with no better fuccefs.—The beft product was too highly coloured. When dafhed againft the fides of the phial, it did not flow back uniformly, but prefented a fomewhat greafy appearance, not much unlike that of a common emulfion with oil and an alkali; and after fome days the oil feparated like a cream on the top, and the liquor became femi-tranfparent.

2. Being affured from a number of enquiries, that maftic was the moft ordinary bafis of the emulfion, this was alfo tried, and fucceeded tolerably at firft. The milky appearance is

* Cryftals of cauftic pot-afh may alfo be obtained by the fucceffive evaporation of a folution of the pot-afh of commerce until the appearance of a light pellicle. The firft cryftals which appear are the alkaline carbonate, or mild alkali, and the latter the pure pot-afh. In this way the lixivium is reduced into very regular cryftals, to the very laft portions of falt which it contains. Half an ounce, the refidue of a folution of feveral pounds, ftill affords very regular cryftals. It is even remarkable, that the latter cryftallizations are formed with infinitely more facility than the firft. I inferted this obfervation in a Memoir on the preparation of the carbonate of pot-afh for the alkaline mephitic water of Colburn, which I addreffed at the beginning of 1793 to the Societe Philomatique de Paris, and has in part been printed in L'Efprit des Journaux for the fame year. Note of Van Mons.

however

however not fufficiently permanent. After a certain time—which is longer in proportion to the weaknefs of the refinous folution, and the ftrength and caufticity of the ammoniac—this alfo feparates, and the maftic forms a hard adhefive coagulum which floats on the furface of the liquor. This effect is much accelerated by frequently opening the phial, but retarded by the addition of alcohol.

3. Elemi fucceeds as well, or better. The fame obfervations are applicable when this is ufed, as in the preceding cafe. The precipitate, however, when formed, fubfides to the bottom of the liquor. It is more bulky, and not fo hard as that of the maftic. It is not unlike that which is formed in fuch ink as contains much gum and alum.

4. A mixture of elemi and maftic in folution feems much fuperior to either; and would perhaps remain fufpended any length of time, if the ingredients were fo apportioned as to make the compound precipitate as nearly as poffible of the fame fpecific gravity as the liquor. The beft product was obtained nearly as follows; but the ingredients not having been accurately weighed, the quantities are only given by eftimation.—Digeft ten or twelve grains of the whiteft pieces of maftic, felected for this purpofe and powdered, in two ounces of alcohol; and, when nearly diffolved, add twenty grains of elemi. When both the refins are diffolved, add ten or fifteen drops of rectified oil of amber, and fifteen or twenty of effence of bergamot: fhake the whole well together, and let the fæces fubfide. The folution will be of a pale amber colour. It is to be added in very fmall portions to the beft aqua ammoniæ puræ, until it affumes a milky whitenefs—fhaking the phial well after each addition, as directed by Macquer. The ftrength and caufticity of the ammoniac are of moft effential confequence. If, upon the addition of the firft drop or two of the tincture, a denfe opake coagulated precipitate is formed—not much unlike that which appears on dropping a folution of filver into water flightly impregnated with common falt—it is too ftrong, and muft be diluted with alcohol. A confiderable proportion of the tincture, perhaps one to four, ought to be requifite to give the liquor the proper degree of opacity. I have frequently fince made an eau de luce in this way, in which no depofition has afterwards appeared.

5. The refin formed from oil of amber by the nitric acid was, after boiling it in water, tried for this purpofe. It was itfelf of a light chocolate colour, and produced an opake fawn-coloured liquor. Common yellow refin formed a coagulum like that of maftic, but almoft inftantly. Its colour was bad, and its fœtor too ftrong to be difguifed.

6. Balfam of copaiba was tried, and appeared to be a very good fubftance for this purpofe; but fome other objects of enquiry intervening, the experiments were difcontinued. Copal is, it is faid, equal, if not fuperior, to any of the refins; particularly on account of its having a lefs unpleafant odour than moft of them. I have not tried it myfelf; but, from the deferved reputation of fome trading chemifts who I know make their eau de luce of it, I have no doubt of its anfwering extremely well *.

I am, Sir, your moft obedient Servant,

12th *June* 1797. J. F—:—:—:—:—R.

* The theory of opake fluids depends on confiderations of an optical, mechanical, and chemical nature. For, 1. Such a fluid muft confift of parts that differ in their refractive power, and are not fmaller than a determinate magnitude. Newton's Optics, III. Prop. 4. 2. The denfer particles will defcend with velocities nearly uniform,

VIII.

An Account of Experiments described, and in part repeated, at the Sitting of the National Institute of France, on the 15th Germinal, in the Year 4. By Citizens FOURCROY *and* VAUQUELIN: *On Detonations produced by Concussion* *.

THE energy and rapidity with which the fuper-oxigenated muriate of pot-afh inflames and burns moft combuftible fubftances, is the fource from which the Citizens Fourcroy and Vauquelin have derived the new facts which they have communicated to the National Inftitute of France, and exhibited at the public fitting of the 15th Germinal, in the 4th year of the Republic. Several chemifts had obferved, that this falt, difcovered by Berthollet about nine years ago, and which feems to contain the elements and phenomena of thunder, decrepitated and emitted fmall electrical fparks by friction, and fpontaneoufly took fire with fulphur. And an unfortunate experiment made in October 1788, at Effone, had proved, that when ground with fulphur and coal, to convert it into gunpowder, it takes fire fpontaneoufly. Such were the firft obfervations made by Lavoifier, Pelletier and Van Mons, when Citizens Fourcroy and Vauquelin undertook to examine the feries of effects of the fuper-oxigenated muriate of pot-afh on the feveral known combuftible bodies. The following are the principal facts which they have difcovered:

1. Three parts of fuper-oxigenated muriate of pot-afh, and one part of fulphur in powder, triturated in a metallic mortar, caufed numerous and fucceffive detonations to be heard, refembling the cracks of a whip, the report of a piftol, or even that of mufquetry, according to the rapidity of the motion, or force of preffure applied. A few centigrams (or fifth parts of a grain) of the fame mixture, ftruck brifkly with a hammer on an anvil, exploded with a noife fimilar to that of a mufket, and torrents of purple light were feen round the anvil. The fame mixture thrown into concentrated fulphuric acid immediately takes fire, and burns without noife with a white dazzling flame.

2. Three parts of the falt, half a part of fulphur, and half a part of charcoal, afford ftronger detonations than the foregoing, when triturated in the mortar, and a more confiderable noife when ftruck on the anvil. The flame of this mixture, when caufed to detonate, or when thrown into the fulphuric acid, is more rapid, ftrong and red than that of the preceding mixture.

3. Equal parts of fuper-oxigenated muriate of pot-afh, and of antimony in powder, fulminate by the ftroke, and produce only reddifh fparks with the fulphuric acid. Zinc at a light dofe alfo fulminates with a white flame, but fuffers no change by the fulphuric acid.

4. Metallic arfenic detonates very ftrongly by the ftroke of the hammer, and takes fire with rapidity and extraordinary brilliancy by the contact of fulphuric acid. In this laft

form, and cæteris paribus in the direct fubduplicate ratio of their diameters. Atwood on Motion, Sect. V. Prop. 10. And 3. The magnitude, and perhaps denfity, of all chemical depofitions are affected not only by the nature of the ingredients and the quantity of folvent, but very much by the order of mixture and other circumftances of manipulation. This laft obfervation ferves to account for the various refults of thefe and many other experiments in which tranfparency, opacity, or colour, are required to be produced. N.

* Annales de Chimie, XXI. 235.

experiment

experiment a ſmoke ariſes which aſſumes the form of a crown, in the ſame manner as phoſphorated hydrogenous gas when ſpontaneouſly inflamed in a calm atmoſphere.

5. The ſulphure of iron, or martial pyrites, inflames rapidly, but without noiſe, when triturated in a metallic mortar with the ſuper-oxigenated muriate of pot-aſh. This mixture, when ſtruck on the ſteel anvil, detonates ſtrongly with a red flame.

6. The red ſulphure of mercury, or cinnabar, and the ſulphurated oxides of mercury, detonate by the ſtroke with the ſuper-oxigenated muriate of mercury (q. pot-aſh); but they do not take fire with the ſulphuric acid. Charcoal alone mixed with this ſalt has the ſame effects.

7. Sugar, the gums, fixed and volatile oils, alcohol, or ether mixed with the ſuper-oxigenated muriate of pot-aſh, ſo that theſe laſt liquid combuſtible ſubſtances formed a paſte with the ſalt, have the property of fulminating very ſtrongly by the ſtroke of the hammer. They all emit a very ſtrong flame on detonation. Theſe mixtures do not detonate nor take fire by ſimple trituration. Some of them take fire in the concentrated ſulphuric acid. Their combuſtion in this caſe is ſlow and ſucceſſive.

8. All the ſubſtances before mentioned, which, when mixed with the ſuper-oxigenated muriate of pot-aſh, take fire and burn in an inſtant, and with conſiderable noiſe, by the rapid preſſure of the ſtroke of the hammer, produce a much ſtronger detonation when wrapped up in double paper, which compreſſes them together before the ſtroke.

9. The electric ſhock from a ſtrong machine, by the charge of a battery of large ſurface, cauſes all the foregoing mixtures to detonate in the ſame manner as concuſſion; and in this caſe alſo they emit a ſtrong light.

To all theſe facts the authors add, that it was already known that gunpowder detonates by a violent blow or ſudden preſſure; but they obſerve, that the ſtroke muſt be much ſtronger than is neceſſary to produce fulmination in all the mixtures of combuſtible ſubſtances with the ſuper-oxigenated muriate of pot-aſh, and that its detonation is far from being equally remarkable with thoſe produced by the new ſalt.

With regard to the theory of this ſingular phenomenon, it appears to them to be the ſame as that publiſhed by Berthollet relative to the detonation of fulminating ſilver by the ſlighteſt contact. Preſſure, and more particularly that which is effected in a very ſhort time, as by a blow, favours the union of oxigene with the combuſtible body. This combination effected by the oxigene, ſeparated all at once from the ſuper-oxigenated muriate of pot-aſh, is accompanied by a ſudden expanſion, and inſtantaneous formation of gaſeous matters, which ſtrike and compreſs the ſurrounding air with ſuch velocity as to produce a conſiderable noiſe. The light, the vapour, and the odour peculiar to each combuſtible body made uſe of, prove that a true inflammation takes place, and that the ſtrong detonation is owing to its violence and rapidity.

The inflammation produced by the concentrated ſulphuric acid ariſes from the diſengagement of the ſuper-oxigenated muriatic acid in gas, by means of which the combuſtible matters mixed with the ſalt take fire ſtill more rapidly than in the ordinary oxigenated muriatic acid gas.

IX.

A Memoir on the Combination of Oils with Earths, Volatile Alkali, and Metallic Subſtances. *By M.* BERTHOLLET *.

SOAP agrees ſo far in its properties with neutral ſalts, that oil muſt form in the manner of acids a great number of combinations hitherto neglected. This notion led me to make the experiments I am about to relate.

The action of oil upon calcareous earth has long been known; but Mr. Coſtel was the firſt who deſcribed the method of accurately combining theſe two ſubſtances, by pouring a ſolution of ſoap into lime water †.

The lime unites to the oil of the ſoap, and forms a combination which is inſoluble, and may be retained on the filter while the cauſtic alkali is ſet at liberty, and may be ſeparated by evaporation. It retains a ſmall quantity of oil, which, according to Mr. Thouvenel, may be waſhed off with ſpirit of wine ‡. This chemiſt remarks, in his analyſis of the waters of Contrexeville, that the cauſtic alkali cannot decompoſe calcareous ſoap; ſo that we may rigorouſly affirm, that oil has a ſtronger affinity to calcareous earth than to fixed alkali; but according to the ſame chemiſts, if the effervefcent fixed alkali be poured on calcareous ſoap, this laſt is decompoſed, the alkali unites with the oil, and the calcareous earth becomes diſengaged at the ſame time that it acquires the property of effervefcence. The diſcoveries made ſince this diſſertation was written, explain what happens upon this occaſion. A double decompoſition and recompoſition take place. The cretaceous acid unites with the calcareous earth of the oleo-calcareous combination, and the oil of the ſame combination unites with the alkali thus deprived of its cretaceous acid.

M. Thouvenel on this occaſion makes a reflection of too great importance to be omitted. Phyſicians often preſcribe the uſe of ſoap and lime together, or lime water, without attending to the change and decompoſition which reſult from the mixture of theſe two ſubſtances. For, in this caſe, it is the cauſtic alkali diſengaged from the ſoap which becomes the active medicament; and conſequently that the effects of ſuch mixtures muſt vary according to the proportions and other concomitant circumſtances.

I have likewiſe examined the effects of volatile alkali upon calcareous ſoap. The cauſtic volatile alkali had no ſtronger action upon this combination than the cauſtic fixed alkali; but the volatile effervefcent alkali decompoſed it in the ſame manner as the effervefcent fixed alkali. The volatile alkali aſſumed the appearance of an oil, and the earth remained at the bottom in the effervefcent ſtate.

After having decanted this ſaponaceous ſubſtance, I evaporated the ſuperfluous volatile alkali at a gentle heat, and there remained a ſoap of a more pungent taſte than common ſoap, and ſomewhat leſs conſiſtent. It became decompoſed by long expoſure to the air. Spirit of wine diſſolves it well, but the quantity taken up by water is extremely ſmall. This laſt property convinced me that there was no need of ſo complicated and ſo long a proceſs to

* Acad. Par. 1780.

† Analyſe des Eaux de Pongues.

‡ Qu. M. Berthollet has ſince ſhewn the cauſtic alkali is alſo ſoluble in that fluid. See our Philoſ. Journal, I. 165. N.

make

make this foap; and that, for this purpofe, I might make ufe of the action of double affinities in a different manner.

I therefore mixed a folution of common foap with a folution of fal ammoniac, and I faw clots formed at the fame inftant, which confifted of the ammoniacal foap, and were retained on the filter; fo that the fixed alkali of the foap united with the marine acid, while the volatile alkali combined with the oil. Strongly perfuaded as I am, that new medicines ought not to be propofed but with extreme circumfpection; and that we fhould rather endeavour to diminifh than increafe the lift of materia medica, I am neverthelefs tempted to propofe the medical ufe of this foap, which muft poffefs more active virtues than the common foap, and which has the advantage over that of Starkey, of a very eafy and fpeedy compofition, uniformity of properties, and is capable of being preferved in clofe veffels. I am aware that ufe has been made in medicine of a mixture of volatile alkali and oil, of which a combination is attempted to be made by agitation or triturating; and that a mixture of this kind is to be found in the London Pharmacopeia, under the name of volatile liniment. But the combination obtained in this way is very imperfect, and totally different from the foap here defcribed, as may be eafily feen by infpection.

When common foap is mixed with felenitic water, two decompofitions and two new combinations take place, as Mr. Coftel has proved. The alkali of the foap unites with the acid of the felenite, and the earth of the felenite combines with the oil of the foap, forming an infoluble oleo-calcareous matter in flocks, which cannot anfwer the purpofes of ordinary foap. Hence the felenitic waters have been diftinguifhed by the name of hard water. Selenite, however, is not the only fubftance proper to form the oleo-calcareous combination; for every folution of this earth is equally proper. The folution in the marine or nitrous acid may be ufed for this purpofe. When foap is therefore decompofed in hard waters, the effect depends not only on the felenite and the calcareous earth held in folution by carbonic acid, but likewife upon all the falts with bafis of lime, or even magnefia, which may exift in the water, as will hereafter be fhewn.

The mixture of a folution of foap and a folution of Epfom falts afforded a combination of the utmoft whitenefs. It is unctuous, dries with difficulty, and preferves its white colour after deficcation. It is infoluble in boiling water, but has neverthelefs a decided tafte of foap. Expreffed oil, as well as fpirit of wine, diffolves it in confiderable quantity; when water is added to the folution in the latter fluid, it becomes milky. This combination melts with a moderate heat, and forms a tranfparent mafs flightly yellow and very brittle. The oleo-calcareous combination cannot be fufed but very imperfectly, and at a much ftronger heat.

I have combined oil with clay by mixing a folution of alum with a folution of foap. The refult of this mixture was a flexible combination, foft to the touch, which preferves its fupplenefs and tenacity in drying. It appeared to me to be infoluble in water, fpirit of wine, and oil. It very readily enters into fufion, after which it exhibits a mafs of a beautiful tranfparence rather yellow.

The folution of ponderous earth in marine acid, afforded with foap a combination nearly the fame in appearance and properties as the calcareous compound.

The very fimple method ufed in thefe experiments to form combinations of oils and earths, was attended with equal fuccefs when applied to metallic fubftances.

 When

When a mixture of the folutions of foap and corrofive fublimate was made, the fluid affumed the appearance of milk, but foon after exhibited fmall coagula. It is almoft impoffible to filter this liquor, but the greateft part of the mercurial combination is flowly depofited. This depofition may be accelerated by means of fpirit of wine. The fame combination may be much more readily effected with the nitrous folution of mercury *.

The oleo-mercurial combination is vifcid, dries difficultly, diffolves very well in oil, and very fparingly in fpirit of wine. It lofes its white colour by expofure to the air, and acquires a flate colour, which gradually becomes deeper, more efpecially if it be expofed to the fun or any other heat. It readily becomes foft and fluid. It muft be diftinguifhed from the mercurial ointment ufed in medicine, for in this the mercury poffeffes the metallic ftate; whereas, in our combination, the mercury is in the ftate of calx, and forms with the oil a true combination, which perhaps might prove ufeful in medicine.

The combination of oil and zinc, which I made by means of white copperas, is of a white colour inclining to yellow. It dries fpeedily, and becomes friable. The combination of cobalt and oil, made by means of the folution of regulus of cobalt in aqua fortis, is of a dull leaden colour, and dries with difficulty, though its parts are not connected together. Towards the end of the precipitation fome coagula of a green colour, and much more confiftent, fell down. I apprehend that this was a combination of oil and nickel; for it is known that this femi-metal is almoft always contained in the regulus of cobalt, and that it forms green folutions with acids, while thofe of cobalt are red. I could not eftablifh my conjecture, becaufe I was unable to procure nickel. But if it fhould prove well founded, this procefs will afford an eafy method of feparating the two metallic fubftances.

I made the combination of oil and tin by means of the folution of this metal in aqua-regia. It is white, not fufible when expofed to heat, like all the other oleo-metallic combinations; but it is decompofed without any change in the form of its parts. I attribute this circumftance to the great quantity of metal contained in this combination, as we fhall fee.

The oleo-martial combination is of a reddifh brown colour, tenacious, and eafily fufible. When fpread upon wood it foaks in and dries. It is eafily foluble in oil, more particularly oil of turpentine, to which it gives a good colour, which may prove ufeful as a varnifh.

The oleo-cupreous combination which I have made by means of blue vitriol, is refinous to the touch while moift, is of a green colour, and becomes dry and brittle. When digefted in fpirit of wine its colour becomes deeper, and it liquefies, but does not diffolve in the cold. Ether renders its colour deeper and more beautiful, inftantly liquefies it, and diffolves a confiderable quantity. This combination is abundantly foluble in oils, to which it communicates a pleafant green colour.

The combination of oil and lead, made by means of the folution of fugar of lead, is white, tenacious, and very adhefive when heated. The union of oil and the lead is not fo intimate in the diachylon (or direct folution of litharge in oil) as in this combination; for this laft, when fufed, is tranfparent, and becomes rather yellowifh if the heat be fomewhat increafed; but the diachylon is opake, and the oil which enters into its compofition

* Probably with different effects, accordingly as the folution, fuppofed to be faturated, is made with or without heat. N.

has

has acquired an acrid property from the heat it has been fubjected to. It is probable, therefore, that the combination I have formed might in fome cafes be advantageoufly fubftituted for that compound. Geoffroy formerly remarked, in the Memoirs of the Academy for 1741, that the combination of oil which he made in the manner of plafters, formed a kind of foap.

The combination of oil and filver is white when firft made; but after a few inftants' expofure to the air, it affumes a red tinge, which no doubt depends on the facility with which this metal yields its oxigene to all combuftible matters *. The change of colour in the oleo-mercurial combination appears to me to depend on the fame principle. When the combination of filver is fufed, its furface becomes covered with a very brilliant iris; and beneath the fuperficies it is black.

The combination of gold and oil partly floats on the mixture in the form of a cream, which is at firft white, foon after which it affumes a dirty purple colour. It dries with difficulty, and adheres to the fkin, fo that it is difficult to efface the impreffion.

I have combined the metallic principle of manganefe by mixing a folution of foap with a folution of manganefe in the marine acid. This combination is at firft white. It affumes in the air a reddifh colour of peach bloffoms, which becomes more and more deep. It fpeedily dries to a hard brittle fubftance, and by liquefaction it affumes a brown blackifh colour.

In order to afcertain whether effential oil had likewife the property of combining with metallic fubftances, I mixed a folution of Starkey's foap newly made with a folution of vitriol of copper. The fame thing happened as with the common foap, excepting that the combination was rather a lighter green, and more friable. Black foap, which is faid to be made with whale-oil, afforded me, with the folution of vitriol of copper, a combination which, compared with that obtained by means of common foap, is rather of a deeper green, remains fomewhat fofter, and poffeffes a very difagreeable fmell.

I was defirous of forming a foap with cauftic alkali and rectified animal oil, with the intention of forming other combinations afterwards; but this oil formed no union with alkali.

We have feen that the calcareous earth (and it is the fame with ponderous earth) has more affinity with oil than fixed alkali, and this has more than magnefia; but the combination of magnefia is not decompofed by the cauftic volatile alkali, fo that magnefia follows the fixed alkali in the order of affinity. Afterwards comes the volatile alkali, which decompofes all the metallic combinations with more or lefs facility. It totally diffolves the combination of filver; but the mercurial combination is that which appeared to me moft ftrongly to refift decompofition. With regard to clay, its combination is decompofed by the cauftic volatile alkali, even more readily than the metallic combinations. Whence I infer that it may be placed after the metallic fubftances.

Oils by expreffion did not appear to diffolve the calcareous and argillaceous combinations; oil of turpentine diffolved only a fmall portion of the calcareous combination, but rather more of that of clay, with which it formed a jelly. Ardent fpirit diffolves fome of the oleo-metallic combinations without heat. It requires heat to diffolve fome others;

* The learned author will certainly approve my tranflating the only theoretical paffage in his memoir according to the fimple doctrine he now maintains. In the original the effect is afcribed to the attraction of filver to phlogifton. N.

but

but though by this means it attacks them all, it neverthelefs diffolves much lefs than the oils, particularly the oil of turpentine.

I calcined part of the combinations here defcribed to determine the quantity of earth or metallic calx they contained. I employed half an ounce of each. The refidue of magnefia amounted to 32 grains, which did not effervefce with an acid; that of calcareous earth was 36 grains, which effervefced; that of clay 28 grains; of iron 48 grains; of copper 33; of zinc 42; of manganefe 40; the refidue of filver amounted to 30 grains in the metallic ftate; that of tin 1 gros 7 grains of the reduced metal; that of lead formed by calcination a pyrophorus. When thefe combinations are to be made, it is proper to ufe the folutions in a faturated ftate; for, if there be an excefs of acid, part of the foap is decompofed by this excefs; a portion of the oil floats above; but part of the oil enters the combination which is formed, and alters its properties. In whatever acid an earth or a metal may be diffolved, the fame combination is always formed by means of foap; neverthelefs this combination fometimes exhibits different appearances. Thus the oily compound of mercury is much more tenacious and adhefive when corrofive fublimate is ufed, than when the nitrous folution of the metal has been employed *.

When the filtered liquor, after forming the oily combination, is evaporated, a falt is obtained of the particular kind which refults from the alkali of the foap, and the acid of the folution made ufe of. M. Coftel made the experiment with felenite, and I repeated it with Epfom falt and vitriol. I firft evaporated to perfect drynefs, after which I diffolved the refidue in filtered water, and then evaporated and cryftallized †.

X.

Analyfis of a Memoir of Citizen BONHOMME *on the Nature and Treatment of Rachitis, or the Rickets* ‡.

SINCE the object of the Society of Medicine, in the offer of its prizes, is to bring our knowledge of the healing art to the greateft degree of precifion and accuracy of which it is fufceptible, this company muft neceffarily direct its attention to the progrefs of the chemical analyfis of animal matters, and the information which fooner or later it muft afford refpecting the nature and treatment of many diforders.

Thofe diforders which alter the compofition of the fluids and the confiftence of the folids, are affuredly the firft which ought to be determined by chemical refearch. The rachitis is of this number. The change which the bones undergo in this diforder, has long been attributed to the action of an acid on their fubftance; but this opinion was grounded on mere fuppofition and remote analogy. The fubject is treated in a new manner in the memoir, of which the Society has directed us to prefent the refults; and the experiments as

* Subfequent experiments have fhewn that a much greater quantity of oxigene is prefent in the former than in the latter cafe. N.

† I fuppofe M. Berthollet's experiments were made with foap of vegetable expreffed oil. The common foaps of this country contain animal fat. N.

‡ This Memoir was read to the National Society of Medicine at Paris. The analyfis was made by Hallé, and is inferted in the feventeenth volume of the Annales de Chimie, whence this tranflation is made.

well

well as the obfervations upon which the author grounds his inferences, prefent to view matters of fact fufficiently remarkable to afford a prefumption that new experiments will confirm their truth. We announce them on the prefent occafion with expreffions of doubt, becaufe thefe are the expreffions which the wifdom and modefty of the author have fuggefted, and becaufe we think with him, that facts of this nature cannot be ftated as if fully proved, but in confequence of multiplied experiments.

The principal notions which conftitute the bafis of this memoir are the following:

1. According to the author, the nature of the rachitic diforder arifes on the one hand from the development of an acid approaching in its properties to the vegetable acids, particularly the oxalic, and on the other from the defect of phofphoric acid, of which the combination with the animal calcareous earth forms the natural bafis of the bones, and gives them their folidity.

Whence it follows, that the indication refulting from this propofition, if once adopted, would be, that the treatment of rachitis muft depend on two principal points, namely, to prevent the development of the oxalic acid, and to re-eftablifh the combination of the phofphoric acid with the bafis of the bones to which they owe their folidity.

2. The author proves by experiments and obfervations, in the firft place, that alkaline lotions of the parts affected with rachitis contribute to their cure; next, that the calcareous phofphate taken internally is really tranfmitted by the lymphatic paffages, and contributes to offification; and laftly, that the internal ufe of calcareous phofphate, whether alone or combined with the phofphate of foda, powerfully contributes to reftore the natural proportions in the fubftance of the bones, and accelerate the cure of rachitis.

On the prefent occafion we fhall only collect the proof of thefe fundamental facts, which form the abfolutely new part of this memoir, in which the author has befides inferted an excellent abridgement of all that had been afcertained before him on the nature of the bones, the rachitis, and the treatment of this diforder.

With regard to the firft parts, the author endeavours to eftablifh thefe two propofitions: 1. That the calcareous phofphate is wanting in the bones of thofe who are difordered with rachitis. 2. That the development of oxalic acid is the caufe of this alteration.

We muft not conceal, that this ingenious part of his memoir contains rather views than abfolute proofs of the nature of the rachitic acid. The author himfelf declares, that he was not provided with the neceffary means to eftablifh an exact and complete analyfis. He therefore prefents his ideas in this refpect, merely as conjectures approaching to the truth.

The effect of the action of acids upon bones, was before known; that is to fay, that when deprived of calcareous phofphate, and reduced to the gelatinous parenchyma which forms one of their elements, they lofe their confiftence and become flexible. Hence it was already conjectured by various phyficians, that the rachitis was the effect of a peculiar acid.

A difpofition to acefcence in the firft paffages is obfervable in all infants. The odour which characterizes this acefcence is often manifeft in their breath, and even their perfpiration. The bile corrects this difpofition; but in general the bile is wanting in rachitic infants. It does not colour their excrements, and the acids accordingly are developed in a very decided manner. They difturb the circulation, and attack and foften the bones. As it is by defect of animalization that thefe acids develop themfelves, it follows that their

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character is analogous to the fermentefcible vegetable acids, and more or lefs to the oxalic acid; and that, on the contrary, the animal acid or phofphoric acid ceafes to be formed, and to unite with the animal calcareous earth; whence they are deprived of the principle of their folidity. This is the theory of Citizen Bonhomme.

In order to eftablifh this doctrine upon precife experiments, it was requifite to analyfe rachitic bones comparatively with thofe of healthy individuals of the fame age; and as it is known that the urine of rachitic fubjects depofits a great quantity of a fubftance of fparing folubility and earthy appearance, it would have been advantageous to have joined a complete analyfis of this urine and its fediment.

Citizen Bonhomme not being provided with the means fufficient to make thefe analyfes, and being befides of opinion that fuch rachitic bones as are deftroyed by this malady exift in a progreffive ftate of change, which might render their analyfis fcarcely fufceptible of comparifon, limited himfelf to a collection of fome of the moft remarkable phenomena of the urine of the aged, the adult, and infants in the healthy ftate, of infants in the rachitic ftate, and of patients after the perfect cure of this diforder. From thefe obfervations he has deduced feveral important refults.

It is known that when the urine contains difengaged phofphoric acid, as happens to aged individuals, and in fome peculiar circumftances of the fyftem, if lime water be poured in there is a fpeedy depofition of calcareous phofphate. It is alfo known, that when a folution of the nitrate of mercury is poured to the frefh urine of adults, a rofe-coloured precipitate is formed, which is a phofphate of mercury produced by the decompofition of the phofphates contained in the urine. Thefe two proofs are therefore extremely proper to afcertain the prefence of phofphoric acid, whether free or combined, in a fluid which in its natural ftate contains a remarkable proportion. Befides this principle, the urine depofits more or lefs of fediment, either gelatinous or of an earthy appearance; and laftly, by evaporation, a faponaceous and faline extract, in greater or lefs abundance, is obtained by evaporation. By means of thefe four methods of examination, the author has afcertained the following facts:

1. In the healthy ftate, the fediment naturally depofited by urine is almoft totally gelatinous in the infant and the adult, and in the aged individual it is furcharged with an abundant fediment of an earthy appearance fimilar to the earth of bones, which confequently is calcareous phofphate.

2. The quantity of brown faponaceous faline extract afforded by evaporation is greater in proportion to the age.

3. The prefence of difengaged phofphoric acid, as fhewn by lime water, is none in the urine of infants, fcarcely perceptible in that of adults, but very remarkable in that of old men. For two ounces of this laft urine afforded by this means ten grains of phofphate of lime.

4. The decompofition of the phofphates by nitrate of mercury is not feen in the urine of infants; an abundant precipitate of a light rofe-colour is produced in this way from the urine of adults; and in that of old men this precipitate is always of a grey colour, and very abundant.

Hence Citizen Bonhomme concludes, that the phofphoric acid, whether at liberty or combined, does exift in the urine of healthy individuals in proportion to the deftruction of the folids by age, and that it increafes with the age.

With

With regard to the urine of rachitic fubjects, the moft remarkable facts are, 1. The abundant and apparently earthy fediment it depofits (fpontaneoufly) is different from that of old men, by its colour, which is grey and does not refemble phofphate of lime, and alfo by its much greater quantity. For a pound of this urine let fall two gros, whereas the fame quantity of the urine of old men depofited only 45 grains.

2. The extract left by evaporation is likewife much more confiderable than in other urine. It is one third more in quantity than the extract afforded even by the urine of aged perfons.

From thefe two firft obfervations it follows, that the folids in rachitic fubjects are deftroyed with much more rapidity than even in old men; and that they afford a much more abundant portion of wafte to the urine.

3. The light depofition occafioned by lime water in the urine of rachitic fubjects is very fmall in quantity, brown, gelatinous when frefh, and pulverulent when dry. It does not at all refemble calcareous phofphate.

4. The depofition formed by the folution of mercurial nitrate is not abundant, neither of a rofe colour as in the urine of adults, nor grey like that of old men. It is always white, and confequently has no external refemblance to the phofphate of mercury. The author affirms that it refembles a mercurial oxalate.

Laftly, the urine of the fame rachitic fubjects when cured, exhibits again all the characters obferved in the urine of healthy children.

We fhall not add to the reflections of the author. In effect, though thefe firft obfervations are curious, they are incomplete. We offer them to phyficians fimply as the elements of an inveftigation which it is of importance to continue and bring to perfection. We fhall therefore proceed to the curative and experimental parts of the memoir.

We muft remark, however, in this place, that the author prefents in his work a judicious and methodical expofition of the whole treatment of the rachitis by other phyficians; that he eftimates the value of each remedy, and every part of the treatment, from the circumftances to which they are applicable, the degrees of the malady in which they are to be admitted, and the indications they anfwer. Setting afide, therefore, every thing which is not peculiarly his own, we fhall in this place attend only to that which relates to the ufe and effects of calcareous phofphate and alkaline lotions.

One of the facts which it was of the utmoft importance to eftablifh, was the tranfition of the calcareous phofphate from the inteftinal paffages, into thofe of circulation and fecretion. Fourcroy had already well afcertained that the ferum of milk contains this falt naturally. Vauquelin had proved its exiftence, as well as that of pure foda, in the feminal fluid; but was it poffible that it could pafs unaltered from the ftomach and inteftines into the veffels which contain the blood and lymph? Could it by this means apply itfelf to the bones? This was to be afcertained by experiments.

[*To be continued.*]

XI.

On the Nature of the Diamond. By SMITHSON TENNANT, *Efq. F. R. S.* *

SIR Ifaac Newton having obferved that inflammable bodies had a greater refraction in proportion to their denfity than other bodies, and that the diamond refembled them in this

* Philofophical Tranfactions, 1797, p. 123.

property, was induced to conjecture that the diamond itſelf was of an inflammable nature. The inflammable ſubſtances which he employed were camphor, oil of turpentine, oil of olives, and amber; theſe he called "fat, ſulphureous, unctuous bodies;" and uſing the ſame expreſſion reſpecting the diamond, he ſays, "it is probably an unctuous body coagulated." This remarkable conjecture of Sir Iſaac Newton has been ſince confirmed by repeated experiments. It was found, that though the diamond was capable of reſiſting the effects of a violent heat when the air was carefully excluded, yet, that on being expoſed to the action of heat and air it might be entirely conſumed. But as the ſole object of theſe experiments was to aſcertain the inflammable nature of the diamond, no attention was paid to the products afforded by its combuſtion; and it ſtill therefore remained to be determined, whether the diamond was a diſtinct ſubſtance, or one of the known inflammable bodies. Nor was any attempt made to decide this queſtion till M. Lavoiſier, in 1772, undertook a ſeries of experiments for this purpoſe. He expoſed the diamond to the heat produced by a large lens, and was thus enabled to burn it in cloſe glaſs veſſels. He obſerved that the air in which the inflammation had taken place had become partly ſoluble in water, and precipitated from lime-water a white powder, which appeared to be chalk, being ſoluble in acids with effervеſcence. As M. Lavoiſier ſeems to have had little doubt that this precipitation was occaſioned by the production of fixed air, ſimilar to that which is afforded by calcareous ſubſtances, he might, as we know at preſent, have inferred that the diamond contained charcoal; but the relation between that ſubſtance and fixed air was then too imperfectly underſtood to juſtify this concluſion. Though he obſerved the reſemblance of charcoal to the diamond, yet he thought that nothing more could be reaſonably deduced from their analogy, than that each of thoſe ſubſtances belonged to the claſs of inflammable bodies.

As the nature of the diamond is ſo extremely ſingular, it ſeemed deſerving of further examination; and it will appear from the following experiments that it conſiſts entirely of charcoal, differing from the uſual ſtate of that ſubſtance only by its cryſtallized form. From the extreme hardneſs of the diamond, a ſtronger degree of heat is required to inflame it, when expoſed merely to air, than can eaſily be applied in cloſe veſſels, except by means of a ſtrong burning lens; but with nitre its combination may be effected in a moderate heat. To expoſe it to the action of heated nitre free from extraneous matters, I procured a tube of gold, which, by having one end cloſed, might ſerve the purpoſe of a retort; a glaſs tube being adapted to the open end for collecting the air produced. To be certain that the gold veſſel was perfectly cloſed, and that it did not contain any unperceived impurities which could occaſion the production of fixed air, ſome nitre was heated in it till it had become alkaline, and afterwards diſſolved out by water; but the ſolution was perfectly free from fixed air, as it did not affect the tranſparency of lime-water. When the diamond was deſtroyed in the gold veſſel by nitre, the ſubſtance which remained precipitated lime from lime-water, and with acids afforded nitrous and fixed air; and it appeared ſolely to conſiſt of nitre partly decompoſed, and of aërated alkali.

In order to eſtimate the quantity of fixed air which might be obtained from a given weight of diamonds, two grains and a half of ſmall diamonds were weighed with great accuracy, and, being put into the tube with a quarter of an ounce of nitre, were kept in a ſtrong red heat for about an hour and a half. The heat being gradually increaſed, the nitre was in ſome degree rendered alkaline before the diamond began to be inflamed; by which means almoſt all the fixed air was retained by the alkali of the nitre. The air which

came

came over was produced by the decompoſition of the nitre, and contained ſo little fixed air as to occaſion only a very ſlight precipitation from lime-water. After the tube had grown cold, the alkaline matter contained in it was diſſolved in water, and the whole of the diamonds were found to have been deſtroyed. As an acid would diſengage nitrous air from this ſolution as well as the fixed air, the quantity of the latter could not in that manner be accurately determined. To obviate this inconvenience, the fixed air was made to unite with calcareous earth by pouring into the alkaline ſolution a ſufficient quantity of a ſaturated ſolution of marble in marine acid. The veſſel which contained them, being cloſed, was left undiſturbed till the precipitate had fallen to the bottom; the ſolution having been previouſly heated, that it might ſubſide more perfectly. The clear liquor, being found by means of lime-water to be quite free from fixed air, was carefully poured off from the calcareous precipitate *. The veſſel which was uſed on this occaſion was a glaſs globe, having a tube annexed to it, that the quantity of the fixed air might be more accurately meaſured. After as much quickſilver had been poured into the glaſs globe containing the calcareous precipitate as was neceſſary to fill it, it was inverted in a veſſel of the ſame fluid. Some marine acid being then made to paſs up into it, the fixed air was expelled from the calcareous earth, and in this experiment, in which two grains and a half of diamonds had been employed, occupied the ſpace of a little more than 10.1 ounces of water.

The temperature of the room when the air was meaſured was at 55°, and the barometer ſtood at about 29.8 inches.

From another experiment made in a ſimilar manner with one grain and a half of diamonds, the air which was obtained occupied the ſpace of 6.18 ounces of water; according to which proportion, the bulk of the fixed air from two grains and a half would have been equal to 10.3 ounces.

The quantity of fixed air which was thus produced by the diamond does not differ much from that which, according to M. Lavoiſier, might be obtained from an equal weight of charcoal. In the Memoirs of the French Academy of Sciences for the year 1781, he has related the various experiments which he made to aſcertain the proportion of charcoal and oxigene in fixed air. From thoſe which he conſidered as moſt accurate, he concluded that 100 parts of fixed air contain nearly 28 parts of charcoal and 72 of oxigene. He eſtimates the weight of a cubic inch of fixed air, under the preſſure and in the temperature abovementioned, to be .695 parts of a grain. If we reduce the French weights and meaſures to Engliſh, and then compute how much fixed air, according to this proportion, two grains and a half of charcoal would produce, we ſhall find that it ought to occupy very nearly the bulk of 10 ounces of water.

M. Lavoiſier ſeems to have thought that the aërial fluid produced by the combuſtion of the diamond was not ſo ſoluble in water as that produced from calcareous ſubſtances. From its reſemblance, however, in various properties, hardly any doubt could remain that it conſiſted of the ſame ingredients; and I found, upon combining it with lime, and expoſing it to heat with phoſphorus, that it afforded charcoal in the ſame manner as any other calcareous ſubſtance.

* If much water had remained, a conſiderable portion of the fixed air would have been abſorbed by it. But by the ſame method as that deſcribed above, I obſerved that as much fixed air might be obtained from a ſolution of mineral alkali, as by adding an acid to an equal quantity of the ſame kind of alkali. T.

XII.

Ufeful Notices refpecting various Objects.—Improvement of Telefcopes.—Imperfections of Optical Glafs.—Purification of Mercury.

THE eye has often been defcribed with admiration by opticians as a perfect inftrument, as well as by others in a more loofe way, in illuftration of the argument from final caufes. When the correction of the aberrations from fphericity and colour began to be invefti-gated, it was prefumed that the arrangement and figure of its parts were directed to re-medy thefe defects; a notion which has fince been fufficiently refuted by Drs. Mafkelyne * and Blair †. If the general remarks had ever been extended to the improvement of opti-cal inftruments, inftead of having been applied to them fubfequent to the periods of actual difcovery; it might afford matter for furprife, that the moft variable of all its adjuftments, namely, that of aperture, has never yet been introduced into our artificial combinations. When we look towards a window or any luminous object, the iris immediately contracts; and on the contrary, when we direct our fight to the inner part of a room, or to any obfcure place, the aperture for the reception of light is as fpeedily enlarged. To what extent this enlargement may be carried in fituations of extreme darknefs, cannot be obferved; but it is certain, that in the obfervable variations of the human eye, the aperture is thirty times as large at one time as at another; and that in the cat the proportion is much greater than an hundred to one. Hence we might reafonably infer, on general principles, that the dif-tinctnefs of vifion through telefcopes would be improved, if this adjuftment alfo were added to that of mere focal diftance.

Every attentive obferver muft have taken notice, that light is of as much confequence to artificial vifion as magnifying power. Diftant woods and other land objects are invifible to an high magnifying power for want of light, when the fame objects may be diftinctly feen with a lower. Luminous objects are feen very perfectly by a fmall aperture, though coloured by a greater: but objects lefs enlightened will receive all the advantage of fuperior diftinctnefs from greater light, when the fringes of colour may be too faint to be of any confequence. From thefe circumftances it frequently happens, that an achromatic perfpec-tive, admitting plenty of light, may exhibit land objects, efpecially in the evening, to much greater advantage than a reflector, admitting lefs light but performing its office with more accuracy; whereas, on the other hand, the latter inftrument fhall have greatly the advan-tage when tried upon a planet.

An ingenious mechanic will find little difficulty in contriving an artificial iris. Suppofe a brafs ring to furround the object end of the telefcope, and upon this let eight or more trian-gular flips of brafs be fixed, fo as to revolve on equi-diftant pins paffing through each triangle near one of its corners. If the triangles be flided inwards upon each other, it may readily be apprehended that they will clofe the aperture; and if they be all made to revolve or flide backwards alike, it is clear that their edges will leave an octagonal aperture, greater or lefs according to circumftances. The equable motion of all the triangles may be produced either by pinions and one concave toothed wheel, or by what is called fnail-work. Another kind of iris more compact may be made by caufing thin elaftic flips of brafs to flide along

* Philof. Tranf. LXXIX. Art. 21. † Edinburgh Tranf. II.

parallel

parallel to the tube, and be conducted each through a flit in a brafs cap which fhall lead them acrofs the aperture in a radial direction. It is probable alfo that the artift who fhall carry thefe hints into effect, may alfo think of feveral other methods.

2. *Imperfections of Optical Glafs.*

THE only fure method of afcertaining whether glafs be fit for optical purpofes, is to give it the true figure of a lens, and examine this by the teft of a high magnifying power. It is defirable however to make fome felection by trial previous to this labour. A lens may be fuppofed to confift for the moft part of a fubftance of uniform refractive denfity, but more or lefs mixed with glafs of another kind, which exhibits the appearance of fpots or veins which interrupt the regular courfe of the light. Where this interruption is produced by confiderable maffes, the imperfections may be feen by the naked eye; but where the maffes are fmaller, the glafs may appear very clear, and yet be lefs fit for optical purpofes than fuch as may contain feveral vifible imperfections. From thefe confiderations it appeared probable to me, that glafs might be advantageoufly examined by the magnifier applied near its furface.

An exceedingly clear, brilliant, yellow-tinged, plano-convex lens, of about four inches focus, was examined by looking at a candle through it, and a lens of half an inch focus next the eye. When the candle was about two feet diftant, I obferved a prodigious number of fmall erect images of candles of various fizes, which became fpots at a greater diftance of the large lens from the eye. Removing to the diftance of feven or eight feet from the candle, the fmall images became luminous fpots, fome of them furrounded by circles of alternate light and darknefs. At this diftance, the fpots, by removing the large lens further from the eye, were rendered much more perceptible.

Several other lenfes were viewed, which exhibited a few images; but in a white common fpectacle glafs of twelve inches focus, the images appeared fo numerous, that the lens refembled the waved window-glafs, and, at a diftance, appeared prodigioufly fpotted. This lens had nothing of an opake appearance, but the light paffed through it too irregularly to give a diftinct picture in the camera obfcura.

A good greenifh object-glafs of fix feet focus, which has a large vein or two acrofs it, produced no more effect in thefe circumftances than if it had not been interpofed between the eye and the candle.

From the circumftances of thefe experiments, we may infer that the images are produced by fpherical particles of lefs denfity than the reft of the glafs. We may fuppofe them to have been filex, the fpecific gravity of which is about 2.7, which is nearly the fame as that of green or crown glafs, but is one-fifth lefs than that of flint-glafs taken at 3.2.

When the full moon was looked at in this manner, through the firft-mentioned lens, the images were confiderably diftinct, and all nearly of the fame fize. Their magnitude was about one two-hundredth part of an inch, or one-fifth part of the fize of a bubble in the glafs, which, by eftimate, might have a diameter of one-fortieth of an inch.

3. *Purification of Mercury.*

MERCURY which is confiderably adulterated may be moft conveniently and effectually purified by diftillation. But many perfons, fuch as mathematical inftrument-makers, water-

water-gilders, and others, who have occaſion to uſe pure mercury, may reject ſuch as contains very little adulteration for want of the chemical apparatus to diſtill it. The proceſs by agitation, firſt diſcovered by Dr. Prieſtley, is far from being generally known or uſed; but will, no doubt, prove an uſeful acquiſition to many operators.

This proceſs is grounded on the fact, that the metals with which mercury is uſually contaminated, become converted into a black powder by partial calcination when agitated with reſpirable air. Take a glaſs vial with a ground ſtopper * (ſuch being generally pretty ſtrong), capable of containing 10 or 12 ounces of water, and fill about one-fourth of it with the foul quickſilver; then putting on the ſtopper, let the bottle be held inverted with both hands, ſhaking it violently by ſtriking the hand that ſupports it againſt the knee. After twenty or thirty ſtrokes, take out the ſtopper, and blow into the vial with a pair of bellows to change the air. For the purer the air the faſter the proceſs advances. After a ſhort time, if the mercury be very foul, the ſurface will not only become black, but a great quantity of the upper part will appear as if coagulated. In this ſituation the vial is to be inverted; and covering the mouth of it with the finger, all the mercury that will flow eaſily is to be let out, and the black coagulated part put into a cup by itſelf. The running mercury may be ſeparated from the black powder, by preſſing this maſs repeatedly with the finger. This mercury is to be added to the reſt, in order to be agitated again.

The proceſs muſt be repeated till no more black matter can be ſeparated; and it is not a little remarkable, that the operator will be at no loſs to know when the operation is completed. For the ſame quantity of lead ſeems to come out of it in equal times of agitation, and conſequently the whole becomes pure at once. The ſound of the pure mercury is louder and more harſh than while it contained the adulteration; ſo that by this criterion the end of the proceſs is eaſily diſtinguiſhed.

Dr. Prieſtley purpoſely diſſolved both lead and tin in a large quantity of mercury, which he afterwards purified in this way, and found by ſubſequent diſtillation, that it had been rendered quite pure. I have myſelf uſed this proceſs, which I find eaſy and convenient. The laſt portions of black matter, which give the mercury a duſty look, are readily ſeparated by the paper funnel as uſual.

XIII.

A Memoir containing ſome Reſults ariſing from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in ſeveral of thoſe Oils †. *By Cit.* MARGUERON, *Member of the Societé des Pharmaciens at Paris.*

THE volatile or eſſential oils obtained from vegetables owe their fluidity, in the ſame manner as water, to the portion of caloric they contain. Several of them loſe this ſtate at 8° above ‡ the term of aqueous congelation (50° Fahrenheit), and aſſume the concrete form; others,

* Experiments and Obſervations on Air, abridged and methodized, by Joſeph Prieſtley, LL.D. F.R.S. III. 439.

† Annales de Chimie, XXI. 174.

‡ The temperatures in this paper are according to the ſcale called Reaumur's, in which the freezing point is marked 0°, and the boiling water point 80°. N.

others, on the contrary, preferve their fluidity at a much lower term. It is upon thefe laft chiefly that I have directed the operation of cold, with two objects of refearch: the firft, to obferve the cryftallization which thefe oils might exhibit by the lofs of their caloric; the fecond, to afcertain the other phenomena which might arife during the experiments.

Thefe experiments, though apparently fimple, are not however without their difficulties.

When we reflect on the different characters of volatile oils, we find in general, that the oils obtained from the plants of hot climates are heavier and lefs limpid than thofe of colder regions, and that they contain a falt capable of cryftallization; that the oils extracted from the fame plants of our climates vary in colour and fluidity, according to the nature of the foil in which the plants have vegetated, the care in cultivation, the influence of light, the different ftates of growth, and whether they be recent or dry when fubjected to diftillation: that when extracted from the vegetable, they are expofed by the nature of their principles to different alterations, according to the more or lefs effectual action of the air, the contact of light, whether kept in bottles entirely or only in part filled, or clofed with a ground ftopper or with a cork. Thefe differences will eafily fhew that the refults muft be fubject to variation: and I have accordingly been very careful to ufe only fuch volatile oils as were newly extracted from frefh vegetables; with the exception of thofe from exotic plants.

Concerning the Action of Cold on feveral Volatile Oils.

I HALF filled feveral very thin bottles with the volatile oils of peppermint, orange flowers, lemon-peel, bergamot, buds of lavender and of thyme, being plants of our climates; oil of turpentine, a refinous fubftance, and oil of cinnamon, a foreign bark.

Thefe bottles, clofed with ground ftoppers, were expofed for feveral days on a terrace where a mercurial thermometer marked 11° beneath the freezing point (7° Fahrenheit).

At this degree of cold I obferved that there was formed at the upper internal part of the bottles a cryftallization in different ramifications, fimilar to thofe feen on the windows of apartments in very cold weather.

Oil of bergamot exhibited a number of fmall elliptical laminæ in its fubftance; that of lemons had depofited fmall cryftals; oil of orange-flowers exhibited a diminifhed fluidity; that of cinnamon was partly congealed on the augmentation of the cold, when the thermometer in my laboratory ftood at 15° below freezing (— 1° Fahrenheit). I took advantage of this time to expofe the fame oils to the action of an artificial cold, which I procured by a mixture of ammoniacal muriate and ice, which caufed the thermometer to fink to 22 degrees (— 17° Fahrenheit). I kept up the fame degree of cold for two hours, by alternately adding the ammoniacal muriate and ice. During the action of this cold upon the oils, the ftoppers were raifed by the expanfion and difengagement of a gafeous fubftance, which perfumed the laboratory and its environs more ftrongly than when thefe oils were diftilled in fummer. The upper part of the bottles was lined with needles, of which the ramifications formed dendrites. When the furrounding ice began to change its temperature, I proceeded to examine the oils as fpeedily as poffible, and remarked:

1. The interior furface of the bottle which contained the oil of peppermint was covered with fmall needles which formed a capillary vegetation. They were white, and liquefied on the fingers. When applied to the tongue, they afforded the frefh and pungent fmell of the oil.

oil. Their folution in alcohol became white by the addition of water. The oil poffeffed an imperfect fluidity; its fmell was lefs intenfe, and its colour deeper. It was foluble in alcohol, and had loft by congelation one * part of its weight †.

2. The bottle containing volatile oil of orange flowers exhibited different ramifications in its fuperior internal cavity. Upon drawing the ftopper a difengagement of air took place, together with the other phenomena obferved in the oil of peppermint, with this difference, that the mafs of the oil was more coloured; that it had loft its fluidity, fo as to adhere to the bottle like turpentine; and when poured into water fome particles feparated, which remained conftantly at the bottom of the fluid.

3. The artificial cold produced no other effect on the oil of bergamot than the appearance of fome cryftalline laminæ of an elliptical form. This oil refumed its fluidity at four degrees below 0 (or 23 Fahrenheit). Its properties were not perceptibly changed.

4. The volatile oil of lemon-peel, when taken out of the bath of ice and falt, appeared to have loft its fluidity. At the expiration of fome days, I perceived that an amber-coloured liquor had feparated from this oil, with feveral fmall cryftals. I decanted thefe feveral products from each other, and obtained the following refults:

A. The oil from which the amber-coloured liquor had been feparated poffeffed the colour and tranfparency of the common oil, but its fmell and tafte were lefs pungent. Its folution in alcohol was fimilar to that which had not been fubjected to cold.

B. The amber-coloured liquor afforded by the volatile oil of lemon-peel by the action of cold, poffeffed an empyreumatic fmell. Its tafte was bitter and flightly acid. It mixed with water, reddened the tincture of tournfole, did not precipitate lime-water, and caufed an effervefcence with the carbonate of pot-afh. This liquor was afforded from the oil of lemons in the proportion of one-tenth part of the whole.

C. The concretions or fmall cryftals obtained from the volatile oil of lemons did not poffefs a well determined form. They were white, fcarcely confiftent, and became opake and friable by the contact of the atmofpheric air. They emitted an odour of oil of lemons, were infoluble in cold water, liquefied in boiling water, and formed a pellicle at its furface while cooling. When heated in a capfule, they melted, and cryftallized in needles by cooling. They did not take fire by the flame of a candle. Their folution in alcohol reddened the tincture of tournfole.

5. The bottle containing the volatile oil of turpentine had its fuperior internal furface covered with a flight efflorefcence, produced by a portion of the oil which had rifen by evaporation. Concretions were formed in the mafs of the oil adherent to the fides of the bottle in the form of flattened tears. Thefe were white, opake, and more firm than turpentine.

* This fraction in the original is rendered imperfect by the type for denominator having fallen out. N.

† Citizen Pelletier has fome oil of peppermint which cryftallizes in long needles at the temperature of fix degrees below congelation (or 18 Fahrenheit), and almoft totally at a few degrees lower.

In a package of the volatile oil of peppermint, which he received from London, he found a pint bottle broken, and the oil fpilled among the hay made ufe of for the package. He referved this hay for diftillation, in order to recover the oil. At the end of eight days, perceiving that the hay began to ferment, he immediately fubjected it to diftillation, and obtained a very fine oil poffeffing the property of cryftallization. Citizen Pelletier is of opinion that it acquired this property by combining with the elaftic fluid produced in the incipient fermentation of the hay. M.

tine. They refumed the fluid ftate at feven degrees below congelation (or 16° Fahrenheit). This fluid, being poured into water, funk to the bottom.

The oil from which the concretions were obtained did not appear to be changed.

6. The volatile oil of lavender, newly diftilled, rofe flightly in vapour, and formed a congelation in the upper part of the bottle. The portion of oil that remained had loft its fluidity, and acquired a deeper colour.

7. The oil of thyme, recently obtained, afforded the fame refults.

8. Volatile oil of cinnamon, fubjected to the action of artificial cold, became very thick, and exhibited an irregular cryftallization; and, as Citizen Baumé has obferved, this oil refumed its fluid ftate at four degrees below 0 (or 23° Fahrenheit) without alteration.

Concerning the Action of Cold and of Water, during its Tranfition to the Glacial State, upon the Volatile Oils.

WATER, in its paffage to the ftate of ice, lofes part of its caloric, increafes its volume, and exerts a preffure on furrounding bodies.

Being defirous of knowing what would be the refult of thefe phenomena on the volatile oils, I made the following experiment:

I poured diftilled water into feveral bottles, and added volatile oils which had not been fubjected to any experiment. I placed thefe bottles in capfules of glafs, and expofed them at a window where the thermometer marked 11 degrees below freezing (or 7 of Fahrenheit). At the moment when the water affumed the ftate of ice, feveral of the ftoppers were forced up by the difengagement of an aromatic principle. The frozen water fpeedily afterwards increafing its volume, caufed feveral of the oils to flow over the external furface of the bottles.

The bottle containing volatile oil of peppermint was covered with a capillary vegetation, as well as the capfule. The other oils exhibited nothing particular.

Prouft had occafion to obferve a fimilar phenomenon on pouring out the oil of lavender which had been evaporated by cold: part of the volatile oil which had been thus expofed to cold having efcaped out of the bottle, he faw a few inftants afterwards a kind of fnow on the whole furface of the bottle which had been covered with the oil.

The action of cold and of water on the oil of peppermint had augmented the intenfity of its colour, and weakened its fmell. This oil had preferved its folubility in alcohol. The needles which had feparated from the volatile oil of peppermint during its efcape, were white, filky, and brittle; they had the fmell of mint. When applied to the tongue, they left the cool and pungent tafte of the peppermint drops or paftils. They fuffered no alteration by the contact of the air; were fufed without inflammation by the flame of a candle, and became confiftent and tranfparent by cooling. When thefe needles were triturated in diftilled water, they communicated to the fluid the property of reddening the tincture of tournfole; their folution in alcohol was attended with a flight noife and agitation (fremiffement), and did not become white by the addition of water, as ufually happens with fuch folutions. From thefe various experiments it follows, that a temperature of 11 degrees below 0 of the thermometer of Reaumur (or 7° of Fahrenheit) produces no change in feveral of the volatile oils, but thickens thofe of cinnamon and bergamot, and determines a cryftallization more or lefs regular; that the intenfe cold obtained by a mixture of muriate

of

of ammoniac and ice diſengages from the volatile oils part of their aroma; renders their colour more intenſe, thickens them, produces concretions in the oils of turpentine and lemon, and ſeparates from the latter an amber-coloured fluid, which appears to poſſeſs ſaline properties; and laſtly, that the action of the cold from water, with the concurrence of the air on the volatile oil of peppermint, afforded a cryſtalline matter diſpoſed in filky needles, which various experiments prove to be a peculiar ſalt; whence it may be concluded, that the needles of camphor, which are ſaid to have been found in the diſtilled water of peppermint, are of the nature of this ſalt.

[*To be concluded in the next Number.*]

MATHEMATICAL CORRESPONDENCE.

QUESTION III. *Anſwered by J. F . . . . . . R.*

LET A, B, C and D, be the players in the order of ſeniority. Then, the laſt card being a trump, there will, when D begins to deal, be 12 trumps out of 51 cards, to be diſtributed among them. The chance that A's firſt card is not a trump, will, therefore, be $\frac{39}{51}$. This having happened, the chance that the next card will not be a trump, will become $\frac{38}{50}$, and the next $\frac{37}{49}$. Then A's chance that his firſt card will be one, will, of courſe, be $\frac{12}{48}$ or $\frac{1}{4}$; which having happened, A's chance of failure becomes $\frac{36}{47}$, and ſo on. By proceeding in this way, we find the whole value of D's chance for all the trumps $=$ $\frac{39.\ 38.\ 37.\ 36.\ 35.\ 34.\ 33\ \&c. \ldots\ldots 4}{51.\ 50.\ 49.\ 47.\ 46.\ 45.\ 43\ \&c. \ldots\ldots 5} \times \left.\frac{1}{4}\right]^{12}$, which, by ſtriking out common multipliers, becomes $\frac{1}{17.\ 10.\ 7.\ 47.\ 46.\ 5.\ 43.\ 7.\ 41} = \frac{1}{158753389900}$: ſo that the odds againſt it are 158753389899 to 1.

The ſame. Anſwered by W. T. of Bath.

SUPPOSING a pack of cards to be properly ſhuffled, and the bottom card to be afterwards turned up, and conſequently known; it is required, in the ſtrict ſenſe of the queſtion, to find the probability that every fourth card, dealt off from the top, ſhall be of the ſame ſuit with the trump card which is expoſed. But as the chance for ſome one of the trumps lying in the 4th, 8th, 12th, &c. place, is evidently the ſame as for its lying in any other place; it amounts to the ſame thing as to determine the probability of a perſon's drawing the 12 remaining trumps, one by one, at random, in 12 trials. To effect this, it is neceſſary to obſerve, that as there are now 51 cards in all, and only 12 in his favour, the probability of his drawing one of the trumps, the firſt trial, is $\frac{12}{51}$; and if this be ſuppoſed to be done, and the card taken away, there will now remain 50 cards in the pack, and only 11 in his favour; whence the probability of his ſucceeding the next trial is $\frac{11}{50}$. In like

manner,

manner, the probability of his succeeding the third time is $\frac{10}{49}$, the fourth time $\frac{9}{48}$, the fifth $\frac{8}{47}$, the sixth $\frac{7}{46}$, the seventh $\frac{6}{45}$, the eighth $\frac{5}{44}$, the ninth $\frac{4}{43}$, the tenth $\frac{3}{42}$, the eleventh $\frac{2}{41}$, and the twelfth $\frac{1}{40}$. And since the probability of the happening of any number of events, is equal to the product of the several probabilities of each of them happening, when considered separately, we shall have $\frac{12}{51} \times \frac{11}{50} \times \frac{10}{49} \times \frac{9}{48} \times \frac{8}{47} \times \frac{7}{46} \times \frac{6}{45} \times \frac{5}{44} \times \frac{4}{43} \times \frac{3}{42} \times \frac{2}{41} \times \frac{1}{40} = \frac{1}{158753389900}$ for the probability required. So that the chances against the dealer, at the game of whist, having all the 13 trumps in his own hand, are above a hundred thousand million to one. And if the chances thus determined be substituted in the analytical formula, given by Demoivre, in his Doctrine of Chances, 3d edit. Prob. III, it will be easy to shew that the number of trials in which this event may probably take place is 111127372229; whence, reckoning every game, one with another, to take 10 minutes, it would be necessary, in order to make it an equal chance for the event happening or failing, that the gamesters should play on, uninterruptedly, for above two hundred thousand years.

QUESTION IV. *Answered by ANALYTICUS.*

IT is laid down, by Newton, as a law of refrigeration, that the quantities of heat lost, in given small spaces of time, are always proportional to the heat remaining in the body; or that, if the times be taken in arithmetical progression, the remaining heats will be in geometrical progression; reckoning these heats to be the excess by which the body is warmer than the surrounding atmosphere.

Suppose, therefore, that h is the temperature of any body above that of the atmosphere, and that it cools d degrees in one minute. Then, by the above law, its temperature at the end of one, two, three, &c. minutes, will be $h - d$, $\overline{h-d}\,(\frac{h-d}{h})$, $\overline{h-d}\,(\frac{h-d}{h})^2$ &c. or $h-d$, $h\,(\frac{h-d}{h})^2$, $h\,(\frac{h-d}{h})^3$ &c. Whence, universally, its temperature at the end of m minutes will be $h\,(\frac{h-d}{h})^m$, and at the end of n minutes $h\,(\frac{h-d}{h})^n$.

And if these temperatures be determined by experiment, and denoted by a and b respectively, we shall have $h\,(\frac{h-d}{h})^m = a$, and $h\,(\frac{h-d}{h})^n = b$; from which equations h is found $= \dfrac{a^{\frac{n}{n-m}}}{b^{\frac{m}{n-m}}}$, or log. $h = \frac{1}{n-m}$ (n log. $a - m$ log. b), and $d = h \times \left(1 - \frac{b}{a}\Big|^{\frac{1}{n-m}}\right)$

$$= \frac{a^{\frac{n}{n-m}}}{b^{\frac{m}{n-m}}} \times \frac{a^{\frac{1}{n-m}} - b^{\frac{1}{n-m}}}{a^{\frac{1}{n-m}}}.\quad \text{Also } \frac{h-d}{h} = \frac{b}{a}\Big|^{\frac{1}{n-m}} = r = \text{rate of cooling.}$$

Again,

Again, becaufe $h\left(\frac{h-d}{h}\right)^m$ is the temperature of the body at the end of m minutes, and $h\left(\frac{h-d}{h}\right)^n$ its temperature at the end of n minutes, either of thefe forms will exprefs the temperature of any body at the end of any given time, from its firft beginning to cool; and their difference $h\left(\frac{h-d}{h}\right)^m - h\left(\frac{h-d}{h}\right)^n$ or $h \times \left(\frac{h-d}{h}\right)^m - \left(\frac{h-d}{h}\right)^n$ is the heat which the body would lofe between the times m and n; h and d being found as above.

In like manner, fince $h\left(\frac{h-d}{h}\right)^m = a$, or $\left(\frac{h-d}{h}\right)^m = \frac{a}{h}$, we fhall have m log. $\frac{h-d}{h} =$ log. $\frac{a}{h}$, or $m\,(\log.\overline{h-d} - \log.h) = \log.a - \log.h$; and confequently $m = \frac{\log.a - \log.h}{\log.\overline{h-d} - \log.h}$ $=$ time which a body takes in cooling, till it arrives at the obferved temperature a.

And if it be required, as in the queftion, to find the degree of heat to which any body has been expofed, we fhall have, in this cafe, $hr^m =$ number of degrees between that temperature and the heat to which the body has been raifed in the time m. Hence $h - hr^m =$ number of degrees the body has been heated, which call t; then $h - hr^m = t$; from which equation h is found $= \frac{t}{1-r^m}$, where r, m and t are known terms.

Thefe formulæ will be found of confiderable ufe in feveral branches of chemiftry, and are capable of a number of practical applications, which are left, for the prefent, to the confideration of the reader.

NEW MATHEMATICAL QUESTIONS.

Question VII. *By W. T. of Bath.*

AS a proper fequel to Queftion III, it is propofed to determine the odds againft the dealer and his partner, at the game of whift, having between them all the thirteen trumps.

Question VIII. *By S. S. of Reading.*

Given $a^x + b^x = c$, to determine the value of x, either by logarithms or a converging feries.

SCIENTIFIC NEWS.

Letter from Sir Benjamin Thompson, Kt. Count of Rumford, F. R. S. to the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. announcing a Donation to the Royal Society for the Purpofe of inftituting a Prize Medal *.

AT the anniverfary of the Royal Society, held the 30th of November 1796, the Prefident acquainted the Society that Count Rumford had transferred one thoufand pounds

* From the Tranfactions for 1797.

three

three per cent. consolidated Bank Annuities to the use of the Society, on certain conditions stated in a letter to the President; which was read as follows:

"SIR,

"DESIROUS of contributing efficaciously to the advancement of a branch of science which has long employed my attention, and which appears to me to be of the highest importance to mankind; and wishing at the same time to leave a lasting testimony of my respect for the Royal Society of London, I take the liberty to request that the Royal Society would do me the honour to accept of one thousand pounds stock in the three per cent. consolidated public funds of this country; which stock I have actually purchased, and which I beg leave to transfer to the President, Council, and Fellows of the Royal Society; to the end that the interest of the same may be by them and their successors received from time to time for ever, and the amount of the same applied and given, once every second year, as a premium to the author of the most important discovery or useful improvement which shall be made and published by printing, or in any way made known to the public, in any part of Europe during the preceding two years, on Heat or on Light; the preference always being given to such discoveries as shall, in the opinion of the President and Council of the Royal Society, tend most to promote the good of mankind.

"With regard to the formalities to be observed by the President and Council of the Royal Society, in their decisions upon the comparative merits of those discoveries, which, in the opinion of the President and Council, may entitle their authors to be considered as competitors for this biennial premium, the President and Council of the Royal Society will be pleased to adopt such regulations as they, in their wisdom, may judge to be proper and necessary. But in regard to the form in which this premium is conferred, I take the liberty to request, that it may always be given in two medals, struck in the same die, the one of gold and the other of silver, and of such dimensions that both of them together may be just equal in intrinsic value to the amount of the interest of the aforesaid one thousand pounds stock, during two years; that is to say, that they may together be of the value of sixty pounds sterling.

"The President and Council of the Royal Society will be pleased to order such device or inscription to be engraved on the die they shall cause to be prepared for striking these medals, as they may judge proper.

"If, during any term of years, reckoning from the last adjudication, or from the last period for the adjudication of this premium, by the President and Council of the Royal Society, no new discovery or improvement should be made in any part of Europe, relative to either of the subjects in question (Heat or Light), which in the opinion of the President and Council of the Royal Society shall be of sufficient importance to deserve this premium, in that case it is my desire that the premium may not be given; but that the value of it may be reserved, and, being laid out in the purchase of additional stock in the English funds, may be employed to augment the capital of this premium; and that the interest of the same, by which the capital may from time to time be so augmented, may regularly be given in money with the two medals, and as an addition to the original premium, at each succeeding adjudication of it. And it is further my particular request, that those additions to the value of the premium, arising from its occasional non-adjudications, may be suffered to increase without limitation.

4 "With

"With the higheft refpect for the Royal Society of London, and the moft earneft wifhes for their fuccefs in their labours for the good of mankind, I have the honour to be, &c.

"*London, July* 12, 1796. (Signed) "RUMFORD.

"To Sir JOSEPH BANKS, Bart. K. B. Prefident
of the Royal Society of London."

The Society hereupon refolved, that they accept of the donation, and accede to the conditions annexed to it by the Count; and alfo directed, that a letter be written to the Count, acquainting him of this acceptance; returning him thanks for the liberal donation, and affuring him that the conditions annexed to it will be ftrictly adhered to.

NEW PUBLICATIONS.

VOYAGE dans les Alpes, &c. Tom. V. VI. VII. & VIII.; or Travels in the Alps, with a Preliminary Effay on the Natural Hiftory of the Environs of Geneva. By Horace Benedict de Sauffure, Emeritus Profeffor of Philofophy in the Academy of Geneva, and Member of various other Academies; two volumes in quarto, or four in octavo. Printed at Neuchatel, and imported by De Boffe, Gerrard-ftreet. Price of the former , and of the latter 1l. 1s.

The former volumes of the Travels of this celebrated author, fo rich in geological and philofophical facts and obfervations, are too well known and efteemed to require any general character in this place. Thefe volumes are equally interefting, and contain a treafure of information, which, neverthelefs, from its peculiar nature and locality in many refpects can admit of no abridgement.

The Firft Report of the Society for bettering the Condition and increafing the Comforts of the Poor. Octavo, 68 pages. Becket, London, 1797. Price one fhilling.

The contents of this valuable pamphlet are enumerated with fo much perfpicuity and effect by Thomas Barnard, Efq. in the Preliminary Addrefs, that I fhall have the pleafure of giving them in his own words:

"Friendly focieties are the objects of the firft paper, which prefents an interefting detail refpecting one at Caftle Eden, upon a fcale capable of general adoption: it contains an important illuftration of the true principle of action with regard to the poor, and proves how much they may in a fhort time learn to do for themfelves, and to what a degree of kindnefs and affection they may be habituated to extend their intereft in the welfare of each other. The manner in which the poor and induftrious member of that fociety has been affifted in the purchafe of his cow, and its beneficial confequences both to the individual and to the property with which he is connected, by increafing and improving the ftock upon it, is deferving of attention and imitation.

"The fecond is an account of a village fhop:—a fubject, the importance of which will be felt by all who intereft themfelves in the domeftic concerns of the poor, when it is known that a faving of 20 per cent. may be thereby made to the labourer in the purchafe of the neceffaries of life; that it is the moft effectual means to prevent his running in debt; that the expence and trouble to the charitable founder of the fhop are inconfiderable; and that it is liable to no objection but what may be eafily obviated.

"The next communication is upon workhoufes of united hundreds, an enquiry of no fmall importance at the prefent moment. The mode of their management *, and the ob-

* See Sir William Young's Obfervations, publifhed in 1788, and his Confiderations on the Subject of Poor-houfes and Work-houfes 1796.

jections

jections and inconveniences that attend them under the beſt regulations and management, are ſtated with clearneſs and perſpicuity. The rules of a ſpinning-ſchool, eſtabliſhed with ſucceſs at Oakham, upon the principles of Count Rumford, is the next in order;—a ſchool where the poor attend with pleaſure and regularity, and thankfully receive the benefit of a cheaper and more nouriſhing diet ſupplied to them at a very ſmall price; and for theſe reaſons ſimply:—becauſe they are allowed to continue free agents, and to retain an option on the ſubject; and becauſe they have the whole of their earnings inviolably at their own diſpoſal. May the example be ſpeedily followed in other parts of England!

"The fifth is an account of the jail and houſe of correction at Dorcheſter. When we conſider the important conſequence of what has been effected there, in annually ſaving, to the public and to themſelves, many perſons otherwiſe abandoned to deſtruction, we cannot help lamenting that ſo very few ſimilar inſtances are to be found in the whole kingdom. The principle of this reform will apply, with ſtill greater force, to every meaſure that regards the local and domeſtic concerns of mankind; in all of which it will invariably be found, that in proportion as coercion is given up, and the intereſt of the party is made the ſpring of action, temptations to vice will be excluded, and habits of labour and honeſty will be gradually acquired.

"In the next paper, upon fuel, the reader will find a very gratifying proof that the poor may be eaſily reconciled to incloſure, or to any other meaſure of public benefit, where their own feelings and intereſts are only properly conſulted.

"The laſt communication is on parochial relief, and the mode and principle on which it has been adminiſtered by the magiſtrates of the hundred of Stoke."

Philoſophical Tranſactions of the Royal Society of London for the Year 1797, Part I. quarto, 218 pages, with 26 pages of Mineralogical Journal, and four plates. Sold by Elmſly, London, price 8s. 6d.

This Part contains: 1. The Croonian Lecture, in which ſome Morbid Affections of the ſtrait Muſcles and Cornea of the Eye are explained, and their Treatment conſidered. By Everard Home, Eſq. F. R. S. 2. Obſervations on Horizontal Refractions, &c. By Joſeph Huddart, Eſq. F. R. S. (See p. 145 of this work). 3. Recherches ſur les principaux Problemes de l'Aſtronomie. Par Don Joſef de Mendoza y Rios, F. R. S. 4. On the Nature of the Diamond. By Smithſon Tennant, Eſq. (ſee p. 177). 5. A Supplement to the Meaſures of Trees printed in the Philoſophical Tranſactions for 1759. By Robert Marſham, Eſq. F. R. S. 6. On the Periodical Changes of Brightneſs of two Fixed Stars. By Edward Pigott, Eſq 7. Experiments and Obſervations made with the View of aſcertaining the Nature of the Gas produced by paſſing Electric Diſcharges through Water. By George Pearſon, M. D. F. R. S. 8. An Experimental Enquiry concerning Animal Impregnation. By John Haighton, M. D. 9. Experiments in which, on the third Day after Impregnation, the Ova of Rabbits were found in the Fallopian Tubes; and on the fourth Day after Impregnation in the Uterus itſelf; with the firſt Appearance of the Fœtus. By William Cruikſhank, Eſq. 10. Letter from Sir Benjamin Thompſon, Knt. Count of Rumford, F. R. S. &c. (for which ſee p. 189.). Appendix, containing the Meteorological Journal kept at the Apartments of the Royal Society.

Geometrical and Graphical Eſſays; containing a general Deſcription of the Mathematical Inſtruments uſed in Geometry, Civil and Military Surveying, Levelling, and Perſpective, with many new practical Problems; illustrated by thirty-four Copper-Plates. By the late George Adams, Mathematical Instrument Maker to the King, &c. Second edition, corrected and enlarged by William Jones, Mathematical Instrument Maker. Octavo, 515 pages. Sold by Jones in Holborn, price 14s. boards.

The character of this work is already well known to the public. It contains a large quantity of useful matter, treated with much perspicuity, and illustrated by a number of accurate and beautiful engravings. Mr. Jones has considerably augmented and improved the present edition.

Count Rumford's Experimental Essays, Political, Economical, and Philosophical. Essay VI. of the Management of Fire, and the Economy of Fuel. Octavo, 196 pages, with six plates. Price 3s. 6d. in boards. Cadell and Davies, 1797.

The reputation of this author, as a philosopher and earnest promoter of the immediate interests of humanity, is fully established. The present essay is distinguished by the same clear and perspicuous detail of interesting facts and inferences as the former Essays which appeared before the commencement of this Journal. It is divided into six chapters: 1. General View and Importance of the Subject. 2. Of the Generation of Heat in the Combustion of Fuel. 3. Of the Means of confining Heat and directing its Operations. 4. Of the Manner in which Heat is communicated by Flame to other Bodies. 5. Experiments with Boilers and Fire-Places of different Constructions. Relative Quantities of Heat from various Fuels. Estimates of the Total Quantities and Loss under various Circumstances. 6. An Account of a Number of Kitchens, Public and Private, and Fire-Places for various Uses, which have been constructed under the Direction of the Author, in different Places.

*** J. F . . . . . . R is respectfully desired to reconsider his solution of the fifth question (p. 138)—more especially because he has inadvertently supposed the second table on p. 39 to be capable of affording the specific gravities of mixtures of common salt and water. For the reasons there mentioned, instead of attempting to give the specific gravities answering to degrees from a repetition of Baumé's experiment, I took the extreme number from the instrument, as found by De Morveau, and calculated the intermediate numbers, as they correspond with equal differences of immersion, of a cylindrical stem; which was the case above the first fifteen degrees, and probably also in those very nearly. Various authors have given the specific gravities of the solutions of common salt. The following table was formed by experiment with very fine dry crystals of marine salt, by Dr. Watson, now Bishop of Landaff. I have extracted it from a paper inserted in the supplement to the thirteenth volume of the Journal de Physique, page 76, for the year 1778. I suppose it to be taken from the Philosophical Transactions, but have not at this time the opportunity of consulting the whole set.

A TABLE of the Specific Gravity of various Solutions of common Salt, at the Temperature of between 46° and 55° of Fahrenheit. Quantity of Salt by Weight = 1.

Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.	Water.	Sp. Gr.
2	1.206	11	1.059	26	1.027	47	1.014	143	1.004	847	1.0008
3	1.160	13	1.050	27	1.025	53	1.013	161	1.003	1023	1.0006
4	1.121	14	1.048	29	1.024	55	1.012	191	1.0029		
5	1.107	15	1.045	31	1.023	71	1.009	255	1.0023		
6	1.096	17	1.040	35	1.020	83	1.007	319	1.0018		
7	1.087	20	1.032	38	1.019	107	1.006	447	1.0017		
8	1.074	23	1.029	41	1.015	125	1.005	511	1.0014		

ing p.192.

Fig. 3.

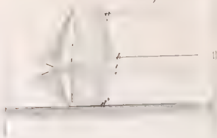


Fig. 2.

Fig. 1.

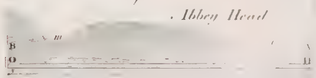


Fig. 4.

Fig. 5.

Fig. 7.

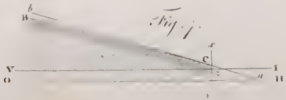
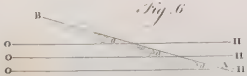


Fig. 6.



Stock for tapping Screws. Fig. 3.

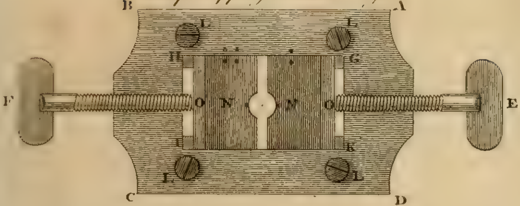
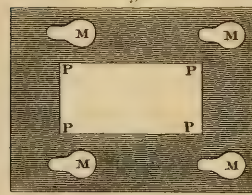


Fig. 4.



Tap. Fig. 1.

Screw Tool. Fig. 2.

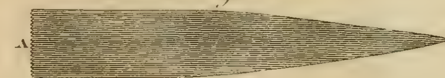


Fig. 5.

Compound Stock for four pair of Dies. Fig. 7.

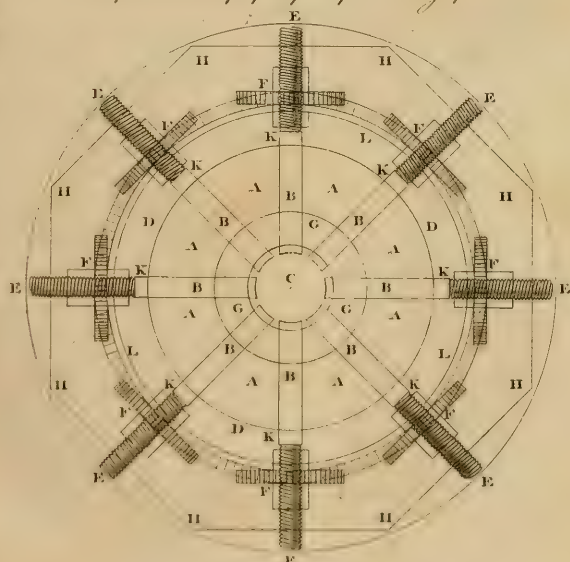


Fig. 6.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST 1797.

ARTICLE I.

An Account of the New Syſtem of Meaſures eſtabliſhed in France. By CH. COQUEBERT*.

FOR ſeveral centuries, reaſon, good faith, and all the principles of ſocial order have exclaimed againſt the diverſity of meaſures in France. Nothing was wanting for the deſtruction of this abuſe, already condemned by the public opinion, but a favourable concurrence of circumſtances. The members of the Conſtituent Aſſembly, charged by their electors to eſtabliſh at laſt that uniformity which had conſtantly been demanded by the States General, and no leſs frequently promiſed by Miniſters, gave charge to the Academy of Sciences to direct their reſearches to this great change. There was no doubt of finding in the learned a ſufficient degree of zeal for a work that muſt facilitate the communication of intelligence, extend the dominion of reaſon, and economiſe that time of which men the moſt inſtructed are ever the moſt ſparing. From this rare and valuable concurrence between the ſciences and the legiſlation has reſulted the ſyſtem we are about to explain to our readers. In this ſyſtem they will diſcern the character of a work independent of peculiar times or individual locality. Its authors have been aware that they could not worthily ſerve the French nation in this reſpect, without embracing in their plan the intereſts of the whole human race. They were deſirous that their labours ſhould be uſeful to all nations; and that the commerce which ſerves to unite them might everywhere ſpeak the ſame language. It is more particularly on this account, that inſtead of confining themſelves to render the meaſures of Paris of general uſe throughout France, they have ſought in the nature of things for the invariable baſis of a complete ſyſtem connected in all its parts.

The pendulum was propoſed in the laſt century to ſerve as the original of all meaſures;

* Journal des Mines, publié par le Conſeil des Mines de la Republique, No. XIV. 73.

but

but it preſented two inconveniences: the firſt, that the diviſion of time is in itſelf arbitrary *; the ſecond, that the meaſures thence reſulting, and geographical meaſures depending on the magnitude of the earth, would have no mutual agreement †.

The preference was given to refer all meaſures large and ſmall to the terreſtrial globe. Among the great circles which meaſure its circumference, the preference was not given to the equator. The nature of the countries over which it paſſes, renders the obſervations too difficult. The determination was made in favour of meridians ‡.

The whole meridian was not taken as the unity for ſubdiviſion, for two reaſons: the firſt, becauſe the quadrant is a true unity for mathematicians and aſtronomers, as is well known to all that cultivate the ſciences; ſecond, becauſe the magnitude of the circumference of the earth is known to us only by conjecture. All the great aſtronomical and geodeſial undertakings have been performed in the northern hemiſphere. We have yet no correct notion of the figure of the ſouthern hemiſphere. It would even reſult from the obſervations of Lacaille, at the Cape of Good Hope, that the terreſtrial ſpheroid is more flattened on this ſide. No hypotheſis can be admitted in a ſubject of this nature. The learned could therefore propoſe as a fundamental unity that only which was well known to them, namely, the diſtance from the north pole to the equator.

This diſtance is obtained with certainty from the meaſure of an arc of the meridian interſected by the forty-fifth parallel. France alone offers in a civiliſed country an arc of the requiſite magnitude, which, if we comprehend a ſmall part of the Spaniſh territory, terminates in the ſea at both extremities. This arc has beſides the advantage to have been repeatedly meaſured with the utmoſt care. Two celebrated aſtronomers are at this moment employed on this operation; not that it is preſumed that the reſult of their labour can have

* We reckon 86400 ſeconds in a day; becauſe the day is divided into 24 hours, and the hour into 60 minutes. Every other diviſion would render the element of time longer or ſhorter, and accordingly influence the length of the pendulum. The pendulum which ſhould make one hundred thouſand oſcillations in a day, would be only 2.283 feet in length, whereas the pendulum for ſeconds is three feet 7.21 lines under the equator. C.

All the meaſures in this paper muſt be underſtood to be French, as I have thought it unneceſſary to make any other reductions but thoſe in the table p. 199. N.

† Under the name of geographical meaſures we denote in this place the degrees of latitude and longitude. It is commonly ſuppoſed that a degree of the meridian is twenty-five common leagues; but in order to afford this accurate quotient in round numbers, it is neceſſary to reckon the league at from 2281 to 2283 toiſes. The league cannot be at the ſame time a multiple of the pendulum in round numbers, and an aliquot part of a degree. C.

‡ This notion ſeems to have preſented itſelf to the ancients. There are ſatisfactory proofs that the cubit of the Nilometer in Egypt, the length of which is 1.712 foot, was contained four hundred times in a ſtadium, two hundred thouſand times in a degree, and conſequently ſeventy-two millions of times in the circumference of the earth. It is thought that the great Pyramid is a monument of this ſyſtem, and that its baſe is exactly the length of the ancient Egyptian ſtadium of five hundred to the degree. The ancients had nevertheleſs neglected to adopt an uniform method of diviſion, and in this their notion required improvement. The ſame obſervation may be made on the ſyſtem of meaſures likewiſe propoſed to be deduced from the magnitude of the earth, in the year 1670, by Mouton, an aſtronomer of Lyons. He thought it neceſſary to preſerve the diviſion of a circle into three hundred and ſixty degrees, and the degree into fixty minutes; ſo that he does not propoſe the decimal diviſion but from the minute of the meridian. The thouſandth part of the minute of the meridian affords a meaſure of five feet eight inches and a half, which he propoſes for the uſual unity. C.

any fenfible influence on the ufual inftruments of meafure, but to prefent to Europe, in all the parts of this great operation, a degree of precifion honourable to the Sciences and to the Republic.

It was not thought neceffary to wait the termination of their work previous to afcertaining the diftance from the pole to the equator in the ancient meafures. It is fufficiently known, by the obfervations of learned men, that the length of a degree of the meridian, taken at an equal diftance from the pole and the equator, is 57027 toifes.

It is likewife known, that from this mean degree the degrees become longer each by about twelve toifes in approaching the pole, and fhorter in the fame proportion by advancing towards the equator, on account of the oblate figure of the terreftrial globe towards the poles. This degree, interfected by the forty-fifth parallel, is therefore the mean term of an arithmetical progreffion, which multiplied by 90 will give the diftance from the pole to the equator.

57027 multiplied by 90 produces 5132430 toifes, or 30794580 feet.

This is the fundamental unity expreffed in ancient meafures from the iron toife of the Academy, fuppofed to be at the temperature of 13 degrees of Reaumur's thermometer.

If the new obfervation fhould give this diftance a minute quantity larger or fmaller, it will not be neceffary on that account to change the number of toifes which reprefent it. It will be fufficient to vary in the proper direction the affumed temperature of the iron toife *.

The unity being thus determined, the fecond ftep was to divide and fubdivide it in fuch a manner as to obtain linear meafures appropriated to the different ufes. An effential condition was to choofe a proper number to be employed, and to employ it conftantly and exclufively, in order to reduce the arbitrary elements of the fyftem as much as poffible. Some learned men were defirous that the number 12 might be adopted, becaufe divifible immediately into the half, the quarter, and the fixth; others, on the contrary, being impreffed with the inconvenience of thefe fractions of variable denominators, would have preferred fome prime number, fuch as 7 or 11, which would have rendered the ufe of all fuch fractions impracticable. But very effectual confiderations proved decifive in favour of the number 10. For, 1. This number is in fome refpect pointed out by nature, fince we find the decimal numeration among nations which do not appear to have had any communication.

2 Nothing can be better adapted to facilitate calculations, than to fubject the divifion of meafures to the ordinary laws of arithmetic; whence the converfion of one meafure into another of a fuperior or inferior order is effected by a fingle ftroke of the pen, and fractions may be worked with the fame eafe as whole numbers. This laft advantage could not be united to thofe which were fuppofed to exift in the numbers 7, 11 and 12, without entirely changing the fyftem of oral and written numeration; an enterprife which perhaps is out of the power of any government to accomplifh.

The diftance from the pole to the equator being taken as the fundamental unity, and the number 10 for the only divifor, conftitute the fimple and productive principles on

* Men of fcience agree that the meafure of the arc of the meridian may be depended on to the five thoufandth part (or barely to four places of figures. N.). If therefore it fhould happen that the error amounts to the utmoft poffible quantity, it would be entirely remedied by fuppofing the toife to be lengthened or fhortened one five-thoufandth part by the effect of temperature. Every degree of variation, according to the fcale of Reaumur, produces on the length of an iron bar a variation of one-75000th part. C.

which this ſyſtem is eſtabliſhed. They are ſo little arbitrary, that poſterity, even if our books and monuments were loſt, might eaſily recover them; whereas, for want of a clue of this nature, the learned are not yet agreed concerning the true dimenſions of moſt of the Greek and Roman meaſures. Magnitudes arbitrarily taken cannot be preſerved but by means of metallic ſtandards, which are altered by time or negligence, and in the revolution of empires are totally loſt. The French nation will have no variation to fear in its meaſures, becauſe it has referred them not to the periſhable works of man, but to every thing which is moſt durable in nature.

Names were to be affixed to each of the meaſures reſulting from the ſyſtem we have expoſed. It was not enough to denote them by their relation to the fundamental unity, and to ſay, for example, the hundred thouſandth, the millionth, or the ten millionth part of the quarter of the meridian. Theſe are definitions; but the mind has need of particular ſigns, to give, as it were, a body to each idea.

The words primes, ſeconds, thirds, and the like, might have been uſed to denote each degree of the decimal diviſion. And theſe words would have poſſeſſed the advantage of always denoting the proportion of each of theſe inferior unities to the prototype which is the baſis of the ſyſtem.

It was thought proper to follow another method, perhaps leſs exact, but more conformable to the ordinary practice. Having arrived, after ſeven ſucceſſive diviſions, to a meaſure of three feet eleven lines and forty-four hundredth parts, which is the ten millionth part of the fundamental unity, a name was given to this uſual and portative meaſure, adapted to take place of the foot, the toiſe, and the ell. It was called metre. Proceeding afterwards from this meaſure as the common unity, its multiples have been named by prefixing to the word metre, one of the Greek words deca, hecaton or hecto, chilio or kilo, and myria, which ſignify ten, a hundred, a thouſand, and ten thouſand; and its ſubmultiples by means of the Latin prefixes deci, centi, milli.

Conſequently a decametre is a meaſure conſiſting of ten metres (30 feet nine inches), proper to be ſubſtituted in place of the meaſuring chain for land.

The kilometre and the myriametre are itinerary meaſures; the one of 1000, and the other of 10,000 metres (513 and 5132 toiſes), the firſt being equal to a ſmall quarter of a league, and the ſecond two medium leagues, or one poſt.

Ten myriametres make the 100th part of the quarter of the meridian, which may, if thought fit, be called the decimal degree.

The decimetre, or 10th part of a metre, anſwers to three inches eight lines and four points.

The centimetre, or 100th part of a metre, is four lines and five points.

And laſtly, the millimetre is five points and a third.

Every other kind of meaſure is derived from the metre and its parts; ſuch as the meaſures of land, of ſolids, of liquids, of grain, and even of weights and coin. The whole is therefore derived from the ſame origin, and the ſyſtem exhibits a complete whole.

A portion of land of one ſquare decametre, or one hundred metres ſquare, has received the name of are.

A ſquare hectometre, or ten thouſand ſquare metres, conſtitutes one hundred ares, or an hectare. Theſe are the agrarian meaſures, ſubſtituted in the place of the perch and the acre (arpent).

The

The cubic metre receives the name of ſtere when employed to meaſure wood for fuel, in which it is ſubſtituted for the cord and other ancient meaſures. The tenth part of the ſtere, or deciſtere, correſponds very well with the meaſure commonly called ſolive in the commerce of wood for carpentry.

A veſſel of a cubical form, having for its ſide one decimetre, or a cylindrical veſſel of the ſame ſolid contents, has received the name of litre. It contains about two pounds of water, or twenty-five ounces of wheat. It is aſſumed as the element for meaſures of capacity, and every other meaſure of this kind is conſidered as the decimal multiple or ſubmultiple of this. So that we have the decalitre of ten litres, containing ſixteen pounds of wheat, or four-fifths of the buſhel of Paris; the hectolitre, which contains near 160, or two-thirds of the ſetier. One thouſand litres are equal in capacity to the cubical metre.

With regard to weights, the original weight is taken to be equal to the quantity of diſtilled water contained in a cubical veſſel, the ſide of which is one-hundredth part of the metre. This water weighed in vacuo, and at the temperature of melting ice, is equal to eighteen grains and eight hundred and forty-one thouſandth parts. The denomination gramme is given to this weight, and from the gramme are deduced by multiplication or diviſion all the weights ſuperior and inferior.

So that in aſcending we have the decagramme equal to $2\frac{2}{3}$ gros.

The hectogramme equal to $3\frac{1}{4}$ ounces.

The kilogramme equal to 2 pounds 5 gros 49 grains.

The myriagramme equal to about $20\frac{1}{2}$ pounds.

And by deſcending we have the decigramme, which is nearly 2 grains.

The centigramme, $\frac{1}{5}$th of a grain nearly.

And the milligramme, which is nearly the 1-50th of a grain.

Laſtly, the diviſion may be carried to the ten-thouſandth part of a gramme, which may be ſubſtituted for the eſſayer's weight of the 512th part of a grain.

We ſee therefore that the gramme is placed between two ſeries of like number of terms; the one, which increaſes to the myriagramme, with which all heavy weights will be aſcertained; and the other, which deſcends to the ten-thouſandth part of the gramme.

It is this conſideration which has cauſed the radical name to be given to a weight ſo ſmall and ſo little adapted to the ordinary uſes of life. It is not pretended, that on this account it ſhould be conſidered as the ſole or even the principal unity. Like the others, it is merely a term in the ſeries of weights; any one of the unities of which may be taken according to convenience. The word radical could not be applied to a unity of a ſuperior order, without ſacrificing much of the regularity and method of this nomenclature.

The pieces of money being adjuſted to a certain number of grammes, are connected with the general ſyſtem of meaſures.

Pieces of one centime will be formed of copper, weighing one gramme; of five centimes, or a ſou, weighing five grammes; of one decime, weighing ten grammes; and of two decimes, weighing twenty grammes.

In ſilver there will be pieces of one franc weighing five grammes, and of five francs weighing twenty-five grammes.

Laſtly, in gold, there will be pieces of ten grammes.

This explanation is ſufficient to exhibit the ſyſtem in its true light, and to ſhew its beauty, ſimplicity, and numerous advantages. Thoſe who are deſirous of more extenſive developments,

developments, will find them in the works published by the temporary Commission of Weights and Measures, and by the Agency instituted to conduct this great and useful operation to its end. We have thought it necessary to annex to this memoir, 1. A table of the new system. 2. Tables of the relation between the old (French) measures and the new; by which it is easy to reduce the one to the other. The object of these tables is to reduce all the calculations relative to this transformation to simple addition. They will likewise serve to determine the price of the new measures from that of the old. In order to render them less voluminous, the simple unities are given from one to nine only. From the value of the unities it is easy to infer the value of decimal multiples or fractions, by shifting the decimal point.

To these tables we have added the logarithms of the ratios between the old and the new measures.

By this means every one who is engaged either in the art of mining, or the sciences relating to the same, will find collected in one view all that is wanting to facilitate the study and the practice of the new system.

*** The table numbered 2 in the original, occupies considerable space, and, though of great utility to the inhabitant of France, is comparatively of small importance to any other nation. It is merely a table for facilitating reductions of the old and new measures of France into each other, and does not convey any additional information to that in the memoir. For these reasons I have not copied it.

In the way of cursory observation on the objects of this honourable and useful undertaking, it may be remarked, that arguments deduced from considerations of the immediate numerical application of an original measure, are of little consequence as to the choice of the means of obtaining it. The chief, and indeed the sole, motive of preference must consist in the greater degree of accuracy, or more perfect agreement of the results wrought out by different observers. In experiments to determine the length of a pendulum vibrating through any known portion of the time employed by the earth in its rotation, we have to enquire, 1. Whether that rotation be theoretically uniform? and if not, what are the quantities and periods of irregularity, perceptible in such an experiment? 2. What are the best methods and limits of error in measuring the length of a pendulum between its centres of oscillation and suspension? 3. Whether the errors from temperature can be rendered insensible in this measure of time and length; and if not, what are their limits? And above all, 4. What are the effects of the escapement part, or simplest maintaining power in a clock; and what are the best means of diminishing or removing them altogether.

In the method of deriving an original standard from the measurement of lines and angles upon the earth, the objects of enquiry are certainly not less numerous than in the pendulum; but the superiority of one method beyond the other would require a complete treatise for its discussion. Among the latest modern surveys, those of General Roy, in the Philosophical Transactions, made with instruments of wonderful accuracy, will afford the greatest instruction to such as may wish to enter this path of investigation. In the last paper of the continuation of this survey, by Lieutenant Colonel Williams, Captain Mudge, and Mr. Dalby, inserted in the Transactions for 1795, a fundamental base was measured on Hounslow Heath, equal to 27404.2 English feet, and connected, by observation of the angles of seventeen triangles, with a base of verification measured on Salisbury Plain. As the triangles presented several ranges of connection, these were computed from the fundamental base, and gave the numbers 36574.8 and 36573.8 for the extreme results, expressing the base of verification in feet. The mean of these, namely, 36574.3, proved to be about one inch short of the actual admeasurement of the base of verification. It seems probable that there may have been some fortunate compensation of errors in this wonderful co-incidence; but the result appears to prove, that the method of terrestrial admeasurement will give an original measure to be depended on for more than four places of figures, and less than five.

When surfaces and solids are to be derived from linear measures, if there be any error in the original line, the error in the surface or area will be twice as great, and that in the solid three times as great, as in the line very nearly, as may be easily deduced from the common process of involution. That is to say, in most cases it will affect the same figure of the result. N.

TABLE

TABLE OF THE NEW MEASURES OF THE (FRENCH) REPUBLIC,

Containing the Methodical System of Nomenclature, and their Proportion to the Ancient Measures.

Proportions of the Measures of each Species to its principal Measure, or Unity. — Letters.	Figures.	First Part of the Name which indicates the Proportion to the principal Measure, or Unity.	PRINCIPAL MEASURES OR UNITIES. — Length.	Capacity.	Weight.	Agrarian.	For Fire-wood.
Ten thousand	10000	Myria					
Thousand	1000	Kilo					
Hundred	100	Hecto					
Ten	10	Deca					
One	1		Metre	Litre.	Gramme	Are	Stere
Tenth	0,1	Deci					
Hundredth	0,01	Centi					
Thousandth	0,001	Milli					
Proportion of the principal Measures between themselves and the Length of the Meridian			Ten millionth part of the distance from the Pole to the Equator.	A decimetre cube.	Weight of a centimetre cube of distilled water.	100 square metres.	One cubic metre.
Value of the principal Measures, and the ancient French Measures - - - -			3 feet 11 lines and $\frac{1}{2}$ nearly.	A pint and $\frac{1}{20}$, or one litron and a quarter nearly.	18 grains & 841 thousandth parts.	Measure of two square perches des eaux et forêts.	One demi-voie nearly, or one quarter of a cord mesure des eaux et forêts.
Value in English Measures - - -			39.383 inches.	61.083 cubic inches, which is more than the wine quart, and less than the ale quart.	22.966 grs.	119.68 squ. yards.	

EXAMPLES

Of the Names, composed, in order to express different Unities of Measures.

Myriametre, length of ten millimetres, equal to about two leagues (or six English statute miles one furlong and $150\frac{3}{4}$ yards)

Kilogramme, weight of 1000 grammes, equivalent to a little more than two pounds, *poids de marc* (or 22966 grains English, which is 3lb. $4\frac{1}{2}$oz. very nearly).

Hectare, Agrarian measure, equal to about two arpents des eaux et forêts, or three, mesure of Paris (or 11968 square yards English, which is rather less than $2\frac{1}{2}$ acres).

Decalitre, Measure of capacity for corn, equal to about 4-fifths of the Paris bushel (or 610.83 cubic inches English, which is to the English peck of 537.6 inches, nearly as 8 to 7).

Decimetre, tenth part of a metre, equal to about three (French) inches and a half (or 3.9383 inches English).

Centigramme, hundredth part of a gramme, equal to about 3-16ths of a French grain (or 0.22966 grains English).

N. B. Many compounds, such as *Decaare*, *Kiloare*, and all those which are formed with the stere, will not be used.

COIN.

The monetary unity is called *Franc*.
The Franc is divided into ten *Decimes*.
And the *Decime* is divided into ten *Centimes*.

The *Franc* does not differ from the ancient *livre tournois*; its value is that of a piece of silver of nine-tenths fine, weighing 5 grammes.

PROPORTION between the ANCIENT FRENCH MEASURES and the NEW.

The distance from the Pole to the Equator being { 5132430 toises. or 30794580 feet.

		Logarithms of the Proportions.			Logarithms of the Proportions.
Value of the metre in Paris ells - -	0,841712	9,925164	Value of the Paris ell in metres - - -	1,188,05	0,074836
Value of the metre in feet - - -	3,07946	0,488474	Value of the foot in metres - - -	0,324732	9,511526
Value of the square metre in square feet	9,48306	0,976948	Value of the square foot in square metres	0,105451	9,023052
Value of the are in poles or perches of 18 feet square - - -	2,92687	0,466403	Value of the pole or perch of eighteen feet square in ares - -	0,341,662	9,533597
Value of the cubic metre in cubic feet - -	29,2027	1,465423	Value of the cube foot in cube metres -	0,0342434	8,534577
Value of the litre in Paris pints - - -	1,05130	0,021725	Value of the Paris pint in litres - -	0,951206	9,978275
Value of the decalitre in parish bushels	0,788473	9,896787	Value of the Paris bushel in decalitres - -	1,26827	0,103213
Value of the gramme in grains, *poids de marc*	18,841	1,275104	Value of the grain, *poids de marc*, in grammes	0,053076	8,724896
Value of the decagramme in ounces - -	0,327101	9,514681	Value of the ounce in decagrammes - -	3,05716	0,485319
Value of the kilogramme in pounds, *poids de marc* - - -	2,04438	0,31056.	Value of the pound, *poids de marc*, in kilogrammes - - -	0,489146	9,689439

II.

Analysis of a Memoir of Citizen BONHOMME *on the Nature and Treatment of Rachitis, or the Rickets.*

[Concluded from page 177.]

THE following are the experiments by which Citizen Bonhomme appears to have succeeded in proving this truth. I shall transcribe his own words.

" I caused several young fowls of the same incubation to be fed in different manners. Some received the usual food without any mixture; others received daily a certain quantity of calcareous phosphate mixed in the same paste as formed the support of the others; and lastly, one of them was fed with variations in the use of the mixture: the calcareous phosphate was sometimes given and sometimes suspended. When these fowls, after two months, had acquired their ordinary growth, I examined and carefully compared the state of their bones. The progress of the ossification in the epiphyses was various according to the nature of the food the animal had received. The bones of the last fowl, which had received the phosphate only from time to time, were rather more advanced than the bones of those which had

6 been

been fed without mixture. The bones of thofe fowls which had been habitually fed with the mixture were evidently more folid, and their epiphyfes were much lefs perceptible. Simple infpection was fufficient to fhew thefe differences when the bones were mixed together.

"I had fed feveral young fowls of the fame incubation according to another plan. Some were fed on a fimple pafte, without mixture; for others it was mixed with pulverifed madder-root; and a third compofition was made of this laft pafte and calcareous phofphate. This was alfo given habitually to other fowls. When after two months I examined the progrefs of offification in the bones of thefe different animals, I eafily perceived the red traces of the madder in the offified parts of all thofe which had ufed it; but I obferved that the offification was not more advanced by the fimple mixture of this root than by the ordinary food: on the contrary, the bones of thofe fowls which had fwallowed the phofphate mixed with madder were much more folid than the others. The red colour ferved admirably to diftinguifh the extremities of the long bones from their epiphyfes. After an exact comparifon, there could be no doubt of the efficacy of calcareous phofphate in favour of the progrefs of offification. The virtue of the madder feemed confined to that of giving colour to the offified parts."

From thefe experiments, it was natural to make the trial of calcareous phofphate in addition to the remedies made ufe of in the treatment of rachitic fubjects. Here follows what the Author himfelf fays, after having fpoken of the exaggerated praifes given by Haen to the ufe of oyfter-fhells (oftracodermata) in the treatment of rachitis.

"Without pretending," fays he, "to a refult fo brilliant as that announced by Haen, I can affirm that the calcareous phofphate has fucceeded very well in the greateft number of rachitic fubjects to whom I have given it. I fhall not multiply my accounts of fuccefsful cafes in this place, but fhall relate only two remarkable inftances.

"The daughter of Mr. Ranchon, watch-maker, aged two years and a half, walked with a feeble and tottering pace, and the extremities of all her bones prefented epiphyfes very prominent. In this fituation fhe exhibited the appearance of imperfect rachitis, or the firft period of this diforder. Alkaline lotions, which I immediately advifed, were attended with a good effect. Her fleep became more firm; and as the firft paffages were in a good ftate, I gave, without internal preparation, one fcruple of a mixture of equal parts of phofphate of lime and phofphate of foda twice a day. In the courfe of three weeks her legs were perfectly reftored; and this amiable infant has ever fince had the fatisfaction to run with fpirit and agility.

"A female infant, of the name of Boiard, aged four years, had experienced from her birth the moft decided fymptoms of rachitis. The protuberance of the epiphyfes and tumefaction of the abdomen firft indicated the difeafe. The impoffibility of fupporting herfelf and walking at the ufual age confirmed thefe unfortunate fymptoms. By degrees the glands of the neck and of the mefentery became fwelled; the teeth were blackened, became carious, and were not replaced. This fituation became ftill more afflicting by crifes almoft periodical at an interval of three or four weeks. At thefe afflicting periods, a fever of confiderable ftrength, cardialgia, and even convulfions, particularly in the night, were obferved. The termination of each paroxyfm was announced or afcertained by abundant ftools, and the evacuation of urine ftrongly charged with an earthy fediment. The imprudent exhibition of a purge at the beginning of one of thefe crifes had nearly deprived the patient of her life. In this ftate it was that I beheld her for the firft time in the month of January 1791. The alkaline lotion was the only remedy the mother adopted in the firft inftance, and it produced a remarkable effect. After eight days the infant was fo much better as to be able to fupport herfelf. The remedy was then laid afide, and eight days afterwards the child was incapable of ftanding without fupport. The ufe of the alkaline folution being renewed, was attended with the fame fuccefs, and its difcontinuance was again followed by the complete return of all the fymptoms. In the firft days of March, the other remedies I had advifed were exhibited. The conftipation which had always exifted became lefs, and the following crifis was effected without pain. And at length the convulfions, the pains, and the crifes difappeared; but the impoffibility of walking ftill remained. At this time, namely on the fecond of May, I gave the child the phofphate of foda and calcareous phofphate mixed together, in the dofe of half a dram twice a-day. At the end of the month fhe was able to ftand upright, leaning againft a chair, and the fwellings began to diminifh. She continued for a long time afterwards to take the mixture of the phofphates. I likewife gave her occafionally one grain of the extract of bile, prepared with fpirit of wine; and at length in the month of July I had the pleafure to fee the patient run and play in the middle of the ftreet with the other children of her own age, &c.

" J. B. Magné, aged two years and a half, appeared ftrong and well formed. A general rachitis manifefted itfelf rapidly in his conftitution, without apparent caufe. He foon became incapable of walking, and his relations learned from his playfellows that he had frequently evacuated white and thick urine. Moft of the remedies and methods propofed by different authors were made ufe of. At length difcouraged at fo many ufelefs trials, the parents gave up every remedy, and the child remained incapable of walking. The bones of the legs were foftened and bent. At the age of four years and a half young Magné had the fmall-pox; the diforder was acute, but terminated happily without any particular attention. When the defquamation was complete, the child was no longer capable of fupporting himfelf. At this period he was brought hither, and I was confulted. I obferved that part of the bones were fo much foftened as to occafion variations and even violent pain in his walk. I advifed a purgative, with alkaline lotions and the ufe of calcareous phofphate, and a proper regimen. Eight days afterwards, the whole furface of the fkin was covered with an infinity of fmall blifters, refembling the itch. My prefcriptions were regularly continued, and in one month the folidity of the bones was entirely fecured. For more than a year he has experienced no relapfe. In this cafe we may remark the inutility of the ufual remedies, the advantage of cutaneous eruptions, and the efficacy of the treatment I propofe."

The incurvated fpine, (though apparently confined to the vertebræ, which bones are not only affected with preternatural enlargement, but frequently alfo with caries) is this diforder, I fay, to be regarded as analogous to rachitis, or at leaft to accidental rachitis? Both have been produced by the repercuffion of cutaneous eruptions. Citizen Bonhomme relates obfervations in which the two affections are fo combined that they appear to form one fingle diforder, varied only according to the parts affected. Is the ufe of calcareous phofphate applicable to the treatment of the vertebral difeafe, to complete and accelerate

accelerate the frequently partial process of other remedies? The Author thinks that the following observation is conclusive in this respect:

"At the end of the month of February 1790," says this physician, "a rachitic child, aged eight years (J. Esprit Guinde), was presented to me in the General Hospital of this town. He had been for several weeks among the patients under the care of the surgeon. At the first moment a prominence of the first dorsal vertebræ was observed, with a considerable weakness of the legs. A large blistering plaister had been applied over the commencing gibbosity, and the blister was kept open by powder of cantharides for more than a month. The situation of the infant was not amended. I remarked in this patient all the characters of the disease described by Pott. I concluded that the blister had produced as great a discharge as was necessary, and that it was more essential to oppose the progress of the rachitic habit. I ordered the whole of the spine to be carefully washed three times a-day with an alkaline and aromatic lixivium. The phosphate of soda was mixed with twice as much powder of burnt hartshorn. Of this I gave to the child one scruple twice a-day in a spoonful of soup. After the first week he was able to walk with assistance; by degrees the solidity of his legs was re-established, and the spine appeared to recover itself. And at length, on the 31st of December, when I quitted the service of that hospital, the boy thought himself sufficiently recovered to return to his friends.

"Eight or nine months afterwards, I found this unfortunate child in another hospital appropriated to the reception of scrophulous patients. From his answers to the questions I proposed, I found that the treatment of the month of December had not been continued long enough to destroy the principle of the rachitis; that the disorder had been as it were subdued during six months, but that coarse food had developed it again. And lastly, that the swelling of certain glands, and a diminution of the solidity of several ribs and of the humerus of the right side, were the effects of its renewed activity. I ordered purgatives to be repeated every eight days, the alkaline lotion, and the mixture of the phosphate of lime and soda; in addition to which I prescribed the use of the saponaceous and mercurial pills. The efficacy of these remedies was very sensible. The advances of the rachitic disorder were stopped; the evacuations were kept up; *the distortion of the ribs and of the humerus disappeared;* but the original gibbosity remained. This deformity, which could not be effaced, did not diminish the pleasure I had to see this child perfectly cured after three months treatment."

In most of these observations mention is made of alkaline lotions and their beneficial effects. The following is the Author's own account:

"In ordinary cases of rachitis, particularly at the commencement of the disorder, it is of advantage to use a simple solution of pot-ash to wash the parts affected. This solution is made by dissolving from half an ounce to an ounce of purified pot-ash * in a pound of dis-

* M. Hallé, in a note on this passage, observes, that pure pot-ash, or the vegetable alkali, is a most powerful caustic, and cannot be used in these proportions. He found a solution of only one-eighth part of the salt here indicated was too strong a lotion for the skin of an infant which was very irritable. Citizen Bonhomme, upon enquiry made, acquainted him that the pot-ash he used was that of the shops, which is very far from being in a caustic state. Probably it may be the same as the common salt of tartar of most of our shops in London, which is nothing but pot-ash or pearl ash dissolved in water, then filtered, and the water driven off by evaporation. It accordingly retains all its saline impurities. N.

tilled or very pure ſpring water. When it is to be uſed, the ſkin muſt firſt be rubbed with a dry cloth, or a piece of fine flannel. After this precaution, the diſeaſed extremities are to be waſhed carefully with the warm ſolution, and at length wiped, ſo as to leave no trace of moiſture. This practice and waſhing muſt be repeated at leaſt twice a-day. I can affirm, from repeated trials, that it will ſoon be attended with ſucceſs.

"The ſolution of pot-aſh is not very coſtly; nevertheleſs, the habitual and long continued uſe of this remedy for a rachitic patient in the ſecond period, becomes expenſive to poor parents. The lixivium of wood-aſhes, which has been uſed for waſhing fine linen, in which aromatic plants may be infuſed, becomes of remarkable utility for the rachitic children of the poor. I have ſeen the moſt decided ſucceſs reſult from its uſe. Several children, who, after having walked alone, found a difficulty in ſupporting themſelves, were waſhed two or three times a-day with this lixivium, and in the ſpace of one or two weeks they walked with eaſe and agility, &c.

"I have ſeen various inſtances of children cured of their diſpoſition to rachitis merely by waſhing with alkaline liquids; but in moſt caſes I have thought it neceſſary to ſecure the firſt ſucceſs by other remedies. When the rachitis has already made ſome progreſs, it is evident that all the remedies muſt be united, and ſteadily continued. The alkaline lotion is a remedy the more preferable, as it is not at all diſguſting, and ſcarcely in any reſpect troubleſome to children; but the internal remedies poſſeſs ſuperior efficacy."

If the facts here announced ſhould be confirmed by further experience, might we not hope to obtain ſimilar advantage in ſuch other diſeaſes as attack the ſubſtance of the bones, and probably have more analogy to the rachitis than has been imagined? Such are the *ſpina ventoſa*, ſcrophulous tumours and caries, the difficulties in the formation of the bony proceſs, after fractures, ſlowneſs and irregularities of dentition, &c. Theſe queſtions, highly deſerving of inveſtigation, are produced by the Author of this memoir as conſequences of what he has related. He likewiſe communicates ſome uſeful notions reſpecting the caſes in which the other combinations of phoſphoric acid may be employed, ſuch as the phoſphates of iron and of mercury, concerning which experience has yet afforded him no information.

With the ſame candour and diffidence as is diſplayed by the Author of this memoir, we preſent his experiments as eſſays worthy of the attention of enlightened phyſicians. We have no doubt but the Author will direct his attention in future to extend his proofs and increaſe the degree of preciſion and evidence. And even if others ſhould go beyond him in this important enquiry, he will nevertheleſs enjoy the honour of having begun it.

Let us conclude this abſtract by a few reflections. It ſeems to us, that a multitude of ideas muſt preſent themſelves to him who ſhall meditate on the facts here diſplayed. Without dwelling on thoſe which are ſtill too hypothetical, the ſingle fact of the tranſition of calcareous phoſphate through the paſſages of circulation, and conſequently its ſolution in the fluids, and its application to the ſubſtance of the bones, deſerves the higheſt attention. The means which nature employs to render the calcareous phoſphate ſoluble are well entitled to the reſearches of chemiſts: without doubt its combination with ſoda may have ſome part in this effect; but it is not leſs important, in this place, as a remarkable phenomenon in the animal economy, which is eſſentially connected with nutrition, and the development of our organs. If we compare the experiments of Vauquelin on the ſeminal

5 fluid,

fluid, with those of Fourcroy on the serosity of milk, and more especially a remarkable fact communicated to us by this last chemist, namely, that the nearer the milk of the human female approaches to the period of parturition, the more the serosity is charged with calcareous phosphate; and the more remote, on the contrary, the time is, from that moment, the less is the proportion of that substance, while the other nutritive parts of which it is composed are augmented in an inverse ratio;—if we consider that at the epocha of gestation and parturition a diminution of solidity takes place in all the articulations of the mother, and a relaxation of the cartilages; that fractures of the bones at this time are most particularly slow in uniting by the formation of the callosity; that it is at this very time of change in the osseous system that the milk becomes charged with calcareous phosphate, which it loses in proportion as the infant and the mother become more removed from the time of the birth; so that the fluids which contribute most to the formation, the growth, and the nutrition of the fœtus, contain in themselves the essential principle of solidity, the elements of ossification; and lastly, that it is not till this ossification is very distinct, and the digestive organs of the infant are sufficiently strengthened to answer the purposes and the work of animalization, that the basis disappears from the milk of the mother;—we shall be compelled to acknowledge a particular direction of nature, by which the calcareous phosphate becomes the matter of a peculiar secretion, essentially ordained to give firmness to our organs, and to consolidate the first elements of man.

May these reflections impart to medical men a conviction how truly important the physical sciences may become to their researches, though too often regarded as merely accessary to medical science! May they see the advantage and necessity of studying the progress of nature in these respects! For it is by these sciences, and more especially by the improvements of animal analysis, which are rapidly advancing, that we may probably expect in a short time to diminish the uncertainty of a large number of our methods of practice, and, to speak freely, the too frequent ambiguity of our conjectures.

III.

*Abstract of a Memoir read at the Sitting of the French National Institute on the 26th Pluviose, containing an Account of some Experiments concerning the Section which Cylinders of Camphor undergo at the Surface of Water; with Reflections on the Motions which accompany this Section. By J. B. VENTURI, Professor of Natural Philosophy at Modena, Member of the Institute of Bologna, &c.**

ROMIEU formerly observed, that small pieces of camphor have a progressive and rotatory motion upon water, and attributed this effect to electricity. Lichtenberg attributed it to the emanation of an ethereal spirit from the camphor itself. Volta produced the same motion of turning, by throwing upon water small bodies soaked in ether, or particles of the acids of benzoin and amber. Brugnatelli found that the bark of aromatic plants moves on the surface of water like camphor. There was always a certain mysterious caprice in these motions, according to which they sometimes could not be produced; and on other

* Annales de Chimie, XXI. 262.

occasions,

occasion; the motions were inftantly ftopped when the water was touched with certain bodies, without its being eafy to guefs the reafon. When particles of camphor were fixed to the extremity of a very delicate electrical fly, they produced no motion. All thefe circumftances tended to envelop the phenomenon with obfcurity. Other philofophers in Italy conftantly fupported the opinion of Romieu in their publications. Citizen Venturi communicated his notions on thefe facts two years ago at one of the public fittings of the Literary Society of Modena, his country. He was prefent at the fitting of the National Inftitute on the 21ft Pluviofe, when a memoir of Citizen Prevoft on the emanations of odorant bodies was read *. He announced the obfervations he had made; and in confequence of an invitation to that purpofe, he communicated a memoir of which the following is an abftract:

Pieces of camphor were cut into the form of fmall columns, one inch in length; a bafe of lead was fixed to each column; they were then placed upright in very clean faucers, and pure water poured in, to half the height of the column. Two or three hours afterwards, an horizontal notch was manifeft in the column of camphor at the furface of the water; in the courfe of twenty-four hours, or thereabouts, by the notch becoming gradually deeper, the column of camphor was cut in two at the middle. The two pieces of the column, neverthelefs, that is to fay, the lower, which was immerfed in the water, and the upper in the air, fuffered fcarcely any perceptible diminution.

From this experiment, and others made with different pieces of camphor, kept feparately in the air, in the water, and at the furface of the water, the Author deduces that the moft active virtue for diffolving camphor refides at that part where both the air and the water touch the camphor at the fame time. Hence he explains, why, in like circumftances, camphor evaporates more quickly in a moift than in a dry air; and why the Hollanders ufe water in their procefs for fubliming this fubftance.

It might be thought that the camphor was decompofed at the furface of the water; that the water might feize the acidifying part, which renders the camphor concrete; and that the volatile part is diffipated in the atmofphere. The Author rejects this notion. He thinks that water with camphor floating on its furface becomes charged with no more than a very fmall portion: 1. Becaufe in thefe circumftances the water acquires the fame tafte and fmell of camphor as it obtains when a fmall quantity of this fubftance is kept plunged in the fame fluid. This water, by expofure to the air, lofes the qualities with which it had been charged, and becomes infipid, and without fmell. 2. Becaufe when the water is faturated with all it can take up, the diffipation of the camphor continues at its furface as before. 3. Becaufe the aërial emanations of camphor made at the furface of water, do themfelves cryftallize into camphor.

Camphor, at the furface of the water, does nothing, therefore, but diffolve; and when diffolved at the ordinary temperature of the atmofphere, it is not at firft in the ftate of vapour, as has been thought. It is fimply a liquid which extends itfelf over the furface of water itfelf; and by this means coming into contact with a great furface of air, it is afterwards abforbed and evaporated. This is proved by the following facts: 1. The folution of camphor at the furface of water is more rapid in proportion to the extent of the furface.

* Philofophical Journal, I. 153.

In narrow veffels, the fection of the column would not be completed in a decade (ten days), even though the water might be extremely pure. 2. When the column of camphor has projecting parts, the liquid may be feen iffuing by preference from certain points of the column, covering the furface of the water, and driving fmall floating bodies before it, in the fame manner as floating bodies go and return in a bafon into which the water of a canal enters with rapidity. 3. If a fmall piece of camphor, already wetted at one end, be brought near the edge of water contained in a broad faucer, and be made to touch the faucer itfelf, it depofits a vifible liquor, which is oily, and by attaching itfelf to the faucer deftroys the adhefion between the veffel and the border of the water; fo that the water retires on account of the affinity of aggregation, which not being oppofed by the attraction of the faucer, caufes the water to terminate in a round edge. If you remove the piece of camphor, the water will not return to its place until the oily fluid is evaporated. 4. In the fame manner, when the column of camphor is half immerfed in the water, the oily liquor which iffues forth deftroys the adhefion of the water to the column, and produces a fmall furrounding cavity. The folution ftops, or is retarded for a moment, until the fluid, extending itfelf over the water, becomes evaporated: the water then returns to its place, and touches the fame part of the camphor; the folution begins again, and in this manner the procefs is effected by alternations of contact and apparent repulfion.

The rotation of fmall pieces of camphor at the furface of the water, is fimply the mechanical effect of the re-action which the oily liquor, extending itfelf upon the water, exercifes againft the camphor itfelf. If the retro-active centre of percuffion of all the jets do not coincide with the centre of gravity, a combined motion of rotation and progreffion muft follow. Since the departure of the oily folution takes place only at the furface of the water, the rotation cannot be effected but round an axis perpendicular to the horizon; and fince in fimilar bodies of different magnitudes the algebraic ratio of the fides to the mafs increafes in the inverfe duplicate ratio of the fides themfelves *, the fmall particles muft have proportionally more jets, and muft revolve more fpeedily than the larger.

The Author reduces to one general rule all the apparent irregularities obfervable in the motions of the camphor. When thefe fmall parts are brifkly moved at the furface of the water, if you touch this fluid with any other body, whether conductor or non-conductor of electricity is of no confequence, provided it be well cleaned from every oily fubftance; in this cafe, the motion of the camphor will not be affected. But if the fame body be afterwards greafed by a fmall drop of fixed oil, or a greater quantity of volatile oil, and then applied to the water at one extremity of the plate, you will difcern a fcarcely perceptible film advancing at that inftant over the whole furface of the water, which will repel the fmall morfels of camphor, and, as if by a ftroke of magic, deprives them of their motion and vivacity. One ounce of oil, poured on one extremity of a bafon of water of twenty feet diameter, very foon ftops the motion of camphor revolving at the other end. This rapid diffufion even of a fixed oil over a great furface of water, prevents the camphor from extending itfelf, and ftops the folution and rotation of the fmall particles. It is for this reafon, likewife, that particles of faw-duft, foaked in fixed oil, begin to turn the moment they touch

* So in the original. In truth, the fides of like folids are in the inverfe triplicate ratio of their folidities. This overfight of the ingenious Author does not impair his deduction with regard to the velocities of rotation. N.

the water, but cannot continue their motion becaufe the film which they form at the furface of the water is not diffipated in the atmofphere. From this laft obfervation the Author deduces this confequence, that volatility and the odorant property are not qualities neceffary to produce thefe rotations. Volatility is merely neceffary for their continuation.

At the end of his memoir, the Author fpeaks of certain other motions obferved in nature, which, by the mechanifm of their caufe, are in fome refpect analogous with the motions of oily bodies at the furface of water. In bodies brought near the fire, the humidity retires always to the extremities moft remote from the fire itfelf; becaufe the vapour which is difengaged from the part moft heated repels the reft in the oppofite direction. So likewife drops of water, thrown on an ignited plate of metal, remain, and are agitated in the form of a fphere; becaufe the vapour which is produced by the contact of the plate agitates them, and does not permit them to touch the metal. By the fame principle, it is very eafy to explain the motions of the tremella defcribed by danfon and Corti, which had inclined fome naturalifts to rank this byffus in an intermediate clafs between plants and vegetables. The tremella is a mafs of very minute fibres, which being fufpended in the water muft be fenfible to the flighteft impreffion. It is at prefent known, that plants, when expofed to light, decompofe the water in their veffels, of which they feize the hydrogene, and extrude the oxigene in the form of gas, by an operation nearly the reverfe of animal refpiration. If from one of the fibres of the tremella the gas iffues on one fide, the fibre itfelf muft bend toward the oppofite part. If, as in large trees, the gas iffues from the tremella in the greateft quantity from the fide of the plant which is oppofite the light, the tremella muft be repelled towards the fource of the light itfelf, to which it always tends when included in an opake veffel, into which the light enters through a hole. It is not therefore neceffary for the explanation of this fact, either that we fhould fuppofe any degree of animality in the plant, or attraction at a diftance between the oxigene and the light. In the fame manner, the gas which is abundantly produced by the light of the day, becomes accumulated between the fibres of the tremella, and floats it to the furface of the water. The Author takes occafion to exprefs his acknowledgment to Citizen Fourcroy, who afforded him the means of repeating the experiments in his laboratory, and who, by his excellent leffons, informed him of the facts newly difcovered fince the communication between his country and France had been unfortunately interrupted.

ADDITIONS to the preceding MEMOIR. In a Letter from the AUTHOR to Citizen FOURCROY.

CITIZEN,

AS I underftand that an extract of my memoir upon the fection of camphor at the furface of water is intended to be printed, I requeft you will add the following facts and reflections:

1. Dry camphor is very perceptibly volatilized under the weight of the atmofphere, at the 50th degree of Reaumur (145 Fahrenheit). It melts at the 120th degree (302 Fahrenheit), and its volatilization is then extremely rapid. In the Torricellian vacuum it rifes even at the ordinary temperature of the atmofphere. This vapour has very little elafticity. It cryftallizes along the fides of the tube.

2. A column of camphor is cut afunder much more fpeedily at the furface of boiling than

than of cold water. Camphor upon boiling water ſublimes in great abundance with the vapour of the water itſelf.

3. Camphor, when floating upon water, turns and is diſſipated by the contact of the oxigene, carbone, hydrogene, and azotic gaſes. Theſe two laſt afforded more rapid movements, and a more ſpeedy diſſipation. They likewiſe more readily diſſolve carbone, phoſphorus, and ſulphur.

4. When camphor is burned or heated on a float at the ſurface of water, if it touch the liquid, it gives a conſiderable motion to its ſupport; but if it do not touch the water, it remains motionleſs. The motion is not therefore produced by the ſimple emiſſion of volatilized particles from the camphor. An action likewiſe takes place on the part of the water.

5. This action appears to me to depend on the principle of the motions of bodies which float at the ſurface of water, and has been explained with remarkable perſpicuity by Monge. Of two ſmall pieces of paper, twiſted up and moiſtened, the one with the pure water, and the other with water well ſaturated with camphor, the firſt attracts and the ſecond repels camphor on the ſurface of water, which does not actually hold that ſubſtance in ſolution. Water conſequently has more attraction for ſolid camphor, than for the little portion which it has already diſſolved to ſaturation. It mounts along the ſolid piece, where it forms a curvilinear inclined ſurface. The ſmall portion which is diſſolved and ſaturated, deſcends along this inclined ſurface, and in its deſcent it repels backwards, by the laws of mechanics, both the ſurface itſelf, and the ſolid particle to which it adheres. This ſeparation of the diſſolved part accelerates the diſſipation of the ſolid piece, by affording a current of water conſtantly renewed. The atmoſphere abſorbs that part of the camphor which is already diſſolved and extended on the ſurface of the water; the evaporation being perhaps aſſiſted by a ſmall portion of the water itſelf.

6. If a drop of oil had no affinity with the ſurface of the water, it would lodge itſelf in a ſmall cavity; and, though more elevated than the ſurface itſelf, it would preſerve the globular form by its affinity of aggregation. But ſince it extends in a film over the water, the drop itſelf, or ſome of its principles, muſt neceſſarily be attracted in the ſame manner as fluids which riſe along the ſides of veſſels.

7. Air, ſtrongly impregnated with ether, or very hot exhalations of camphor, exerciſes on the ſmall floating particles at the ſurface of the water a repulſion ſimilar to that of oil, or of camphor diſſolved without heat in the water itſelf. The former are elaſtic fluids, and the latter ſimple liquids. They muſt not be confounded together.

I have perhaps dwelt too long upon a ſubject which ſeems rather to be matter of curioſity than immediate utility. But it is always a pleaſure to me to embrace an opportunity of expreſſing my eſteem and attachment to you.

J. B. VENTURI.

IV.

Analysis of four Specimens of Steel; with Reflections on the new Methods employed in this Analysis. By Citizen VAUQUELIN *.

Preliminary NOTE of the (French) EDITOR.

THE steels which Citizen Vauquelin has analysed were sent to the Council of Mines numbered 864*a*, 864*b*, 977 and 1024. These steels were produced at the forge of Remmelsdorff, situated in the canton of Grofs-Remmelsdorff, in the department of the Moselle, at the distance of 5000 metres north from Bouzonville, and the left side of the Nied. The crude irons which are there refined come from the furnaces of Dilling and Betting, which, according to the report of Citizen Dietrich, obtain their iron stone from the communes of Gresaubach and Steinbach, as well as from the forest of Hommelswald; and their calcareous flux (castine) from the woods of the commune of Mersching. The establishment consists of two fineries, one large hammer, two smaller, and a heating furnace. Citizen Soller, the proprietor, had established before 1785 a manufactory of steel by cementation, which, after having been abandoned for several years, has again been put into activity. The cementing furnace contains, according to the account furnished by this citizen himself to the Council of Mines, about six thousand weight of iron, and can be charged two or three times a month.

Citizen Dietrich, who visited this establishment in 1785 by order of Government, inspected the process, and the furnace of Citizen Soller, of which he gave the most advantageous account. By this report it is seen that Citizen Soller uses coak for cementation †. We think it may be of advantage to transcribe in this place the report which was made on these same steels in 1786, by the Commissaries of the Academy of Sciences. It is thus by comparing the results of operations performed in manufactories under the inspection of learned men, with those afforded by the chemical analysis, that we may hope to learn to what extent the nature and proportion of the constituent parts may influence the qualities of steel; and whether we may expect from chemical science new and direct methods of ascertaining what kinds are best appropriated to the different uses of this substance in the arts.

Extract from the Registers of the Academy of Sciences, March 15, 1786.

ON the 15th of February last, Mr. Soller presented some steel of cementation from his works at Remmelsdorff, in Lorraine, near Saar-Louis. The Academy has charged Mr. Vander Monde and myself to give an account of the same.

We must distinguish three kinds of steel, each of which possesses properties relative to the different uses they are applied to.

1. Natural steel (acier de fonte) is obtained immediately from the crude iron. This steel is usually unequal in its quality; subject to have cracks and scales; is less hard and brittle than the two other kinds; welds better, and is principally used for instruments of

* Journal des Mines, publié par le Conseil des Mines par la République, N° XXV. 1.

† Houille. Doubtless as the fuel; not as the cementing compound. N.

husbandry,

hufbandry, common cutlery, and fprings. As this kind of fteel demands lefs expence in its fabrication, it is of courfe the cheapeft. The greateft quantity is imported (into France) from Germany.

2. The fecond kind is fteel of cementation. It prefents a more equal grain in its fracture, and takes a better polifh than the former. It is alfo harder, more brittle, and requires more particular attention in the forging. This kind being more perfect, is of great ufe in all the circumftances which require the qualities here mentioned.

3. Laftly, the third kind is caft-fteel, obtained by fufion of one of the other of the foregoing kinds. It is characterifed by its uniformity of texture, and is deprived of thofe fcabrous places which are obfervable even in the fteel of cementation. This third kind of fteel is therefore fufceptible of the moft beautiful polifh, and is proper to form razors, lancets, ornamental work, wire-plates, laminating rollers, &c. The Englifh have hitherto been almoft totally in poffeffion of the manufactory of caft-fteel, which is the moft valuable of any.

Four bars of cemented fteel, manufactured by Mr. Soller, and chofen from among the fteels of different qualities in his warehoufe at Paris, were fubmitted to the following experiments:

With the firft piece fome hook-tools * for turning iron were made. They forged very well, the fteel being very hard in the fire, and confequently eafy to be wrought. The hook-tools, on trial, proved very good. A graver for turning iron in the turn-bench was made out of the fame piece. It was tried upon fteel, and found to be as good as an Englifh graver.

The fecond piece was ufed to make a plane iron for metal. It fucceeded perfectly well, and kept its edge in planing iron as long as thofe which are made of Englifh fteel.

Out of the third piece, which was doubled into three, and welded upon a piece of iron, a carpenter's chifel was made 26 lines in breadth. The fteel welded perfectly, without requiring any particular care. The edge proved very good, and on comparifon with an Englifh chifel of the fame dimenfions it was found to poffefs the fame qualities.

A carpenter's chifel, 14 lines broad, was made out of the fourth piece of fteel. It welded extremely well, and proved to be as good as the foregoing inftrument. The workmen eftimate that a greater number of Englifh chifels would be found at the venders' below than above this quality.

Mr. Soller likewife fent to the commiffioners a fample of iron, which was part of the tie

* The hook-tool is ufed by the turners of ftrong metallic work; but from its fteadinefs, and other good qualities, it well deferves to be applied in many delicate operations with fteel and brafs, by a numerous fet of workmen in this capital who are entirely unacquainted with it. Its ufual form is that of a hook or claw, lefs than half an inch long, proceeding nearly at right angles from the fide of a ftraight ftem, which laft terminates below in a point. The cutting edge is at the extremity of the claw, and is varioufly figured, according to the nature of the work. By bearing the angular point on the Reft of the lathe, the claw may be applied to the work with great power and fteadinefs, while the perpendicular handle bears againft the fhoulder of the workman. The fteel commonly ufed in London for this inftrument is called fheer-fteel, from its mark The edge ftands very well againft foft fteel, when the tool is a little harder than a hard faw, or juft capable of yielding to a good Lancafhire file. If it be harder, there is a greater probability of its breaking. This kind of work is performed by a flow motion, with the conftant application of water to the piece to prevent heating. It is obvious from thefe facts that tenacity is the requifite moft wanted here. N.

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of a faggot of ſteel, and manufactured out of a mixture of half its weight of old iron with the ſame quantity of pig-iron. This iron, from which the ſteel preſented to the Academy appears to have been made, is of the firſt quality. It cannot be broken without tearing. It extends under the hammer with the greateſt facility, whether heated or cold.

The ſamples of ſteel from Mr. Soller exhibited in theſe trials the qualities of the ordinary ſteel of cementation which we receive from England under the name of acier punte *. Like that ſteel, they may be twiſted while hot without cracking, but they cannot be compared with the fine Engliſh ſteels.

Mr. Soller has likewiſe communicated to us the details of his proceſs, and the deſign of his furnaces. We have every reaſon to believe that he may fabricate ſteel of an excellent quality. But to prevent the objections which might be made on the ſuppoſition that theſe pieces had been ſelected, he has drawn out the whole contents of a caſe of ſteel made by a new cementation. This ſteel will be ſent to us, with every atteſtation in form which can be deſired. We ſhall ſubmit it in like manner to different trials, and render an account to the Academy. In the mean time, we are of opinion, that the enquiries and the undertaking of Mr. Soller are among thoſe which the Academy will never fail to applaud.

Done at Paris, at the Academy, March 15, 1786. (Signed) VANDER MONDE AND BERTHOLLET.

I certify that the preſent extract agrees with its original, and the judgment of the Academy.

Paris, March 18, 1786. (Signed) CONDORCET.

SECTION I.

INTRODUCTION †.

THE analyſis of ſteels is one of the leaſt advanced and moſt difficult parts of chemiſtry, more eſpecially when the accurate proportions of conſtituent parts are required to be determined.

The agents commonly made uſe of to diſunite theſe principles do themſelves ſuffer part of their elements to eſcape, which re-act on thoſe of the ſteel, and put them into a ſituation which renders it difficult to form any eſtimate.

Thus, for example, we find, that when ſteel is diſſolved in the ſulphuric acid, diluted with water, the hydrogene gas which is developed, diſſolves, and carries with it a portion of carbone, the quantity of which varies from a great number of circumſtances.

The inſufficiency of the methods hitherto propoſed for acquiring this knowledge is well ſhewn from the reſults they have afforded to thoſe who invented them, and who were no leſs ſkilful in experiments than in the art of reaſoning.

It is well known that we are indebted to Bergman for the firſt analytical proceſſes with iron and ſteel; and that little addition has been made to them ſince. But theſe proceſſes

* Is this ſpur-ſteel? N.

† This word, and ſeveral other ſentences or ſubordinate titles, appear in the margin of the original as ſide-notes. I have inſerted them in the text. N.

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are inaccurate, as we fhall fhew in the courfe of the prefent memoir, and as it alfo appears from the different refults he obtained from the analyfis of various kinds of fteel, the ufual properties of which did not indicate fo great a difference.

It is admitted, fince the valuable experiments of Bergman, and particularly thofe of Berthollet and Monge, that fteel differs from pure iron only from the prefence of a certain quantity of carbone, which is intimately combined; a proportion which perhaps may admit of fome degree of latitude; but which, neverthelefs, if above or below a certain quantity, affords either an over-cemented, brittle, and too fufible kind of fteel, or elfe fteel which too nearly approaches to iron in its qualities.

If fteel were merely and conftantly a combination of carbone and iron, in determined quantities, it would be eafy to afcertain this point once for all; but it almoft always happens, that fteel contains filex, phofphorus, and fometimes manganefe, of which the influence upon the qualities of the fteel is not known, even on the fuppofition that fteel is neceffarily no more than a combination of iron and carbone.

If we admit that thefe different fubftances are not indifpenfable to the conftitution of fteel, it muft neverthelefs be allowed that they will affect its properties by greater or lefs modifications, dependent on their own quantities refpectively.

If, therefore, as it feems out of doubt, the various qualities of iron and fteel arife from the feveral principles of which they are compofed, and their refpective proportions, it is equally interefting to philofophy and the arts to determine, by chemical experiments, what influence each of thefe principles may exert in the combination, and to find by fimple, ready, and cheap experiments, in what ufes thefe metallic fubftances may be moft advantageoufly employed. But in order to arrive at this defirable termination, a more precife knowledge is required of the ufual properties of the known kinds of iron and fteel, compared with the chemical nature of thofe matters. There is not, however, any other method than that of examining the two means at the fame time, by which we may fucceed hereafter in eftablifhing and fixing by chemical effays the qualities of the kinds of iron and fteel already ufed, and even thofe which may hereafter be fabricated.

It may be eafily conceived that this undertaking would demand a great number of experiments. For it not only requires a knowledge of the number and nature of the elements neceffary for the formation of fteel of the beft quality, but likewife the proportions of thefe elements, which may be infinitely varied, and to determine befides the modifications which may be produced by various quantities of fubftances not effential to the conftitution of this metal.

In the mean time, till circumftances fhall permit the execution of this ufeful plan, I proceed to offer the refults of the analyfis of four kinds of fteel, the difficulties I have found in the progrefs of this work, the means I have ufed to overcome them, and the methods I have fubftituted, inftead of ancient proceffes, to afcertain and meafure the effential and accidental principles of the fteel.

SECTION II.

STEEL, NO. 864—SMALL PIECE.

Experiment I. 576 grains, or 30,57 grammes, of this fteel, reduced into filings, and diffolved in fulphuric acid, diluted with five parts of water, afforded 1,98 grains of black refidue.

Experiment

Experiment II. 100 grains, or 5,3 grammes, of the same steel dissolved in the requisite quantity of sulphuric acid, produced 108 inches, or 2169 millimetres cube of hydrogene gas.

New Process to separate the Phosphate of Iron from Steel.

Experiment III. The excess of acid contained in the solution No. 1. having been saturated with carbonate of pot-ash, there were deposited 19 grains, or about one gramme, of a white tasteless powder, completely soluble in muriatic acid.

This matter, boiled in a solution of caustic soda, assumed a deep red colour, and greatly diminished in volume.

The liquor having been filtered, and mixed with concentrated muriatic acid, gave no sign of effervescence, and formed, before and after its mixture with the muriatic acid, a white precipitate, on the addition of lime-water.

This calcareous deposition, being washed and dried, was soluble in acids, and without producing any effervescence; whence we may conclude that it is phosphate of lime, and consequently that the steel No. 864, the small piece, contains phosphorus. Experiments still more positive afford an incontestable proof of the truth of what is here advanced on the nature of this substance.

The Inconvenience of using the Sulphuric Acid to determine the Quantity of Carbone contained in Steel.

CHEMICAL experiments having demonstrated that hydrogene, when it develops itself from the interior part of bodies which contain carbone in a very divided state, dissolves a certain quantity, which depends on the more or less elevated temperature, and the more or less rapid disengagement of the gas, I had reason to presume that the results offered by the experiment numbered 1, could not give an accurate expression of the quantity of carbone contained in the steel. I therefore sought another method, in which the iron dissolved without the disengagement of hydrogene gas should afford the real or absolute quantity of carbone which renders it steel.

New Method of accurately determining the Quantity of Carbone contained in Steel.

AS the sulphureous acid possesses the double advantage of dissolving iron without producing gas, and does not act upon the carbure of iron, it presented this desirable requisite.

Experiment IV. I put into a bottle 288 grains, or 15,28 grammes, of steel, in fine filings, with about two pounds, or about 978,24 grammes, of distilled water, and I passed into this bottle sulphureous acid gas, disengaged from the sulphuric acid by mercury *. When the sulphureous acid had ceased to act on the steel, I carefully decanted the liquor with a siphon. I washed the black precipitate several times with distilled water. After desiccation by the gentle heat of the stove, it weighed seven grains, or about 6,37 grammes.

We see that this quantity of carbure of iron is much more considerable than that of the first experiment. For 576 gave only 1,98, whereas, in the present experiment, I obtained 7 from 288. But on examining this matter, I found that it contained sulphur in a state of mixture. For by exposing it on a heated metallic plate, it took fire, like sulphur alone.

* We shall not in this describe the phenomena which appear during this operation. They have been exposed by Citizen Berthollet, and we have developed and extended them to other metals in a particular memoir on the metallic sulphates. V.

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To extract this combustible substance from the carbure of iron, I heated the mixture slightly with a solution of caustic pot-ash; and having left the matter to settle, I decanted the fluid. The powder, then, after washing and drying, weighed no more than four grains, and gave no further signs of sulphur by combustion *. This experiment evidently proves, that in the experiment No. 1, where sulphuric acid was used, the greatest part of the carbone was dissolved, and carried off by the hydrogene gas, since 576 grains afforded only 1,98 grains of this matter; though the same quantity of steel with the sulphureous acid afforded eight grains. This steel contains therefore 0,014 of its weight of carbure of iron.

Analysis of the Carbure of Iron.

Experiment V. To ascertain the quantity of carbone contained in the four grains of carbure of iron obtained in the experiment No. 4, they were submitted to the action of fire in a small porcelain saucer, under a muffle. As soon as the temperature was sufficiently raised, it took fire, and left 1,9 grains of a grey yellowish mass, which treated with the boiling muriatic acid became white, and was reduced to 0,44 grains.

The muriatic acid acquired in this operation a lemon colour. It afforded with very pure prussiate of pot-ash a blue precipitate, and with ammoniac reddish flocks, which consisted of oxide of iron. The 0,44 grains of white matter insoluble in the muriatic acid, presented on examination all the characters of silex.

From these experiments it follows, that the steel No. 864, small piece, contains phosphorus, carbone, and silex, of which the proportion with the mass of iron is established in thousandth parts in a table at the end of this memoir.

Essay to discover the Presence of Manganese in the Steel No. 864, *Small Piece.*

Experiment VI. Bergman having announced the presence of manganese in all the iron and steel he examined, it became a necessary part of my plan to examine whether this metal existed in the steel which formed its object. Accordingly I took a known quantity of the solution of 576 grains, *(Experiment No.* 1,*)* from which the phosphate of iron had been separated. I precipitated it by well saturated prussiate of pot-ash, to which I had added a small portion of acid, in order that there might be no precipitation of iron in the state of oxide.

The washed precipitate was then boiled with muriatic acid a little diluted. The mass of the matter was perceptibly diminished, and the liquor assumed a purplish red colour; which phenomenon led me to presume that this steel contained manganese. During the solution or rather the ebullition of the liquor, a very perceptible smell of prussic acid was emitted. The muriatic fluid, mixed with a solution of carbonate of potash, afforded a white precipitate, which became yellow by the contact of the air. This precipitate, dried by the stove, and exposed to the action of heat, under a muffle, in the cupelling furnace, acquired a black colour. It was again dissolved in the muriatic acid, and, upon the addition

* It appears that pot-ash acts not only on the sulphur, but that it dissolves also a portion of iron. When an acid is poured into the fluid, there is found at first a black precipitate, which is soon followed by another white deposition. This precipitate being collected and examined afforded sulphur and perceptible traces of iron. The four grains of black precipitate obtained in this experiment are therefore carbone in a pure state, or nearly so. V.

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of pruſſiate of pot-aſh, afforded pruſſiate of iron as beautiful as at firſt. The ſecond precipitate, treated as before, and ſtrongly heated under a muffle, became at firſt black, but afterwards acquired a brown-red colour, which began to deſtroy the opinion I had conceived reſpecting the exiſtence of manganeſe.

Nevertheleſs, to clear up this doubt, I applied the ſulphureous acid to the calcined precipitate, in the hope of diſſolving the manganeſe, if it ſhould exiſt, without touching the iron. But I was convinced that a ſmall portion of iron only had been taken up; for the ſulphureous acid afforded a blue precipitate by the pruſſiate of pot-aſh, and a deep green by cauſtic pot-aſh. This proves at the ſame time that the iron does not contain manganeſe, and that this method is inſufficient to demonſtrate its preſence; more eſpecially to ſeparate it from the iron. For, on the one hand, the muriatic acid decompoſes, or at leaſt diſſolves, the pruſſiate of iron; and, on the other, the ſulphureous acid likewiſe diſſolves the oxide of iron.

This proceſs being founded on the aſſertion of chemiſts, that the pruſſiate of iron is inſoluble and not decompoſed in acids; and that the pruſſiate of manganeſe, on the contrary, is eaſily diſſolved by the ſame agents;—if the laſt aſſertion be true, the preſent experiments prove that the firſt is falſe. It is nevertheleſs probable, that the weaker acids, ſuch as the acetous and ſulphureous, greatly diluted, may not have ſo evident an action on the pruſſiate of iron; and that they may be preferable to ſeparate it from the pruſſiate of manganeſe. But experiment has not yet decided in this reſpect, though it is certain that it is by no means a matter of indifference what acid is employed in this operation.

The Method of Bergman to ſeparate Manganeſe from Iron.

THE bad ſucceſs of this proceſs engaged me to follow another method propoſed by Bergman, in his Diſſertation on the White Ores of Iron, and other parts of his works, to ſeparate iron from manganeſe.

I took one part of the oxide of iron which had been precipitated from the ſolution of the ſame ſteel by the cauſtic alkali, and afterwards waſhed, dried, and calcined in the cupelling furnace. Upon this I repeatedly boiled the nitric acid to dryneſs, and calcining it in each operation. Laſtly, I treated it with the ſame acid in which a ſmall quantity of ſugar had been diſſolved. During this operation, a great quantity of nitrous gas was diſengaged, and the fluid aſſumed a brown colour. After evaporation to dryneſs, and waſhing of the reſidue, part was diſſolved in water. The ſolution afforded a beautiful blue precipitate by the pruſſiate of pot-aſh, green by cauſtic pot-aſh, and white by the carbonate of pot-aſh.

The ſulphuric acid, poured on the ferruginous ſolution, did not diſengage nitrous vapours, but a white fume, the ſmell of which conſiderably reſembled that of the acetous acid.

It is therefore proved, that the method propoſed by Bergman, and followed and repeated by chemiſts in general, is very faulty, becauſe it favours the ſolution of iron, and the acid does not, when applied to a mixture of this metallic gas and the oxide of manganeſe, act excluſively on the latter, unleſs by accident ſuch a quantity of ſugar ſhould be employed as might be capable of forming ſo much acid only as could diſſolve the oxide of manganeſe alone. But how is it poſſible to aſcertain this quantity, when the proportion of the two metals is unknown?

2 A new

A New Method for ſeparating Manganeſe and Iron, and determining their Proportions.

THE uncertainty of the methods propoſed by chemiſts to aſcertain and determine the quantities of manganeſe contained in different kinds of iron and ſteel, induced me to ſeek another which combines ſimplicity and accuracy. I think I have obtained, in this reſpect, all the ſucceſs which can be deſired in a matter of ſuch importance.

For this purpoſe, a known quantity of iron crude or malleable, or of ſteel, ſuſpected to contain manganeſe, is to be diſſolved in ſulphuric acid, diluted with four or five parts of water. The ſolution is to be precipitated with the cauſtic alkali; and the precipitate, after waſhing, and drying in the air, is to be calcined under the muffle of the aſſayer's furnace. When the calcination is as much advanced as it can be by this method, the metallic oxide is to be weighed, and diſſolved in muriatic acid; and after having evaporated the exceſs of acid, a ſolution of carbonate of pot-aſh, well ſaturated with carbonic acid, is to be poured into the fluid diluted with water, until no more precipitate is afforded. The filtered liquor is then to be boiled; and if it contains the oxide of manganeſe, this metal will fall down in the form of a white powder, which is a true carbonate of manganeſe.

This proceſs is founded on the conſideration that the carbonate of pot-aſh, well ſaturated with carbonic acid, does not precipitate the ſalts of manganeſe, becauſe the quantity of carbonic acid contained in the pot-aſh neceſſary to the ſaturation of the acid, united to the oxide of manganeſe, is ſufficiently great to hold the latter ſubſtance in ſolution; whereas the ferruginous ſalts, when their baſis is highly oxided, are entirely precipitated by the ſame re-agents.

On this ſubject I have made many experiments, ſynthetical and analytical, upon very ſmall quantities of iron and of manganeſe, which have left me no doubt with regard to the accuracy of the method. But the manganeſe exiſts in iron in the metallic ſtate; and in the preſent experiment it is obtained in combination with oxigene and carbonic acid. It is neceſſary, therefore, to divide the maſs, after drying it in the air, by 2,5. Thus, if 100 parts of carbonate of manganeſe be obtained, the metallic part will be 100 divided by 2,5, that is to ſay, 40.

I have not yet had occaſion to apply this method to a very great number of ſpecimens of ſteel or iron; thoſe which I have hitherto examined have exhibited no traces of manganeſe. Whence I am ſtrongly inclined to think that Bergman, who found this ſubſtance in every kind of iron and ſteel, muſt frequently have miſtaken iron for manganeſe, particularly in the inſtance where he affirms that he obtained 30 per cent.

[*To be continued.*]

V.

Deſcription of an Artificial Rock Cryſtal produced in the Humid Way. By M. TROMMSDORFF, *Profeſſor of Chemiſtry at the Univerſity of Erfurt* *.

IT is well known that ſilex, either pure, or mixed with heterogeneous ſubſtances, and cryſtallized in beautiful tranſparent columns, conſtitutes rock cryſtal. The curious enquirer,

* Journal der Pharmacie fur Arzte Apotheker und Chemiſten, 2 Band. 1 St. pag. 76.

who for the firft time beholds a fine group of thefe cryftals will naturally be difpofed to afk, What are the means employed by nature to produce this form? Does this power operate by the humid or by the dry way?

The fecrets of nature are concealed behind a thick veil, which is feldom to be penetrated. Too often it happens, that when we think ourfelves in poffeffion of the object of our enquiry, we find ourfelves deceived by the varying powers around us, which exhibit new forms. The prefumptuous fpirit of man becomes impatient; cool experimental inveftigation is laid afide; the powers of imagination are recurred to, and a region of chimerical images prefents itfelf. Vanity and indolence both confpire to render the philofopher a creator of hypothetical worlds. Under thefe circumftances it is not furprifing that a variety of difcordant theories have been invented concerning the production of rock cryftal; that imaginary reafoning has been offered and received, and that no chemift has yet fucceeded in the artificial production of this fubftance.

The extent of our deductions by analogy is, that filex, before its cryftallization, muft have been diffolved in fome liquid; but the queftion is to determine which? In fire this is not credible, for filex is apyrous. In acids?—Except the acid of fluor there is no other which acts on this earth, and this acid is a folvent which would not have again been feparated.

Bergman has indeed affured us, that he found hard cryftals having the form of rock cryftal, in the fluate of filex; but as he did not carefully examine them, we may ftill queftion whether they were true rock cryftal.

Achard likewife attempted to imitate nature, and endeavoured, as is well known, to compofe even the precious ftones, by means of carbonic acid; but it is alfo known that his notions were not confirmed, and that this celebrated man either deceived himfelf, or was deceived by others; for his excellent character abfolutely forbids the fufpicion of voluntary deception.

It is known that the alkalis are capable of diffolving filex; it is alfo known that the acids only are capable of deftroying this combination. The filex in that cafe, when the folution is weak, falls down. The precipitate is very fine and tranfparent. When the folution is more faturated, it affumes the form of a jelly, or appears like gelatinous condenfed flakes, but never in cryftals.

Profeffor Siegling the elder, my intimate friend, had prepared the liquor of flints after the ordinary procefs, which he fet afide in a glafs veffel covered with paper. This fluid remained undifturbed for eight years, when by accident his attention was directed to it, and he remarked a beautiful cryftallization. On this occafion he had the goodnefs to fend me the veffel.

It was a jar of greenifh glafs, capable of holding twelve ounces. The bottom was covered with a number of fmall cryftals, over which repofed about two ounces of a very tranfparent fluid. This liquid was covered with a cryftalline cruft, fo ftrong that the veffel could be inverted without any lofs of the fluid.

The jar was then broken, and the contents more particularly examined.

The cryftals at the bottom were compofed of fulphate of pot-afh, and cauftic pot-afh, in a cryftallized ftate. The fluid exhibited the appearance of a cauftic lixivium; but which had attracted a fmall quantity of carbonic acid, and ftill contained a little filex. Hence it effervefced, and depoſited filex when treated with acids. The cruſt was viſibly compoſed of two kinds of cryſtals. When boiled with water, a great part of theſe were diffolved, and afforded, after the evaporation, carbonate of pot-aſh. The remaining cryſtals were tetrahedral pyramids in groups. Acids had no action upon them. *They were perfectly tranſparent, and ſo hard as to give fire with ſteel.* They diffolved in four parts of deliqueſcent pot-aſh, and afforded a maſs of ſoluble glaſs, from which ſilex could be precipitated. The fluoric acid attacked them, and diffolved a conſiderable part. They were therefore perfect rock cryſtals, produced by art. Part of the cruſt, which was leſs tranſparent, fell down during the boiling. It was ſiliceous earth, ſo hard and firm that it acted as a grinding powder on glaſs.

Though I am convinced that this obſervation affords us no light with regard to the proceſs of nature in the production of rock cryſtal, it has not given me leſs ſatisfaction, from the proof that it is poſſible at leaſt for us to imitate this natural production, though perhaps by different means.

I explain the origin of rock cryſtal in this glaſs nearly in the following manner: The liquor of flints was very weak, and ſuper-ſaturated with alkali. This diſengaged alkali inſenſibly attracted the carbonic acid from the atmoſphere, and formed in this manner a cruſt upon the liquor, which was at reſt, and was not therefore ſo much expoſed to the action of the carbonic acid. The alkali could not attract this laſt principle but with difficulty, and conſequently the ſilex was ſeparated very ſlowly in the form of a fine powder. Now as the moſt perfect repoſe obtained in this operation, the fine particles of the ſilex were not diſturbed in their attraction, which they exerted without impediment, and formed cryſtals. As ſoon as a double cruſt was formed of carbonate of pot-aſh and rock cryſtal, the carbonic acid could no longer penetrate to the liquid; whence followed a cryſtallization of part of the diſengaged alkali.

An admirer of hypotheſes might apply this obſervation to the formation of natural rock cryſtal. It would be ſufficient for this purpoſe to ſuppoſe that a ſoluble maſs of ſilex and ſoda was formed by any ſubterraneous revolution; that, by the preſence of moiſture, this maſs was converted into the liquor of flints, from which afterwards, by a ſlow attraction of carbonic acid, the rock cryſtal was depoſited.

But it will reaſonably be aſked, What has become of the ſoda in this operation? Perhaps it may have been neutraliſed in the courſe of time by other acids, or elſe (for it is eaſy to find ſupport for an hypotheſis) it may be changed into magneſia; and in this caſe the experiments of Oſburg and ſome other authors would preſent themſelves to be quoted. For my part, I prefer an avowal of my ignorance, and am diſpoſed to ſay with a certain poet, that "no mortal can penetrate the depths of nature, and few even the ſurface." I am nevertheleſs deſirous that this obſervation may engage other chemiſts to repeat the experiment, and examine more particularly into the facts. Circumſtances, perhaps, in part to me unknown, may have a marked influence on the ſucceſs of the experiment.

VI. *Geological*

VI.

Geological Obfervations on North Wales. By Mr. ARTHUR AIKIN.*

I. THERE are no proper volcanic productions to be met with in North Wales. By proper volcanic fubftances I mean afhes, pumice, lava, and fcoriæ, or femi-vitrified ftones, fuch as are the peculiar products of Etna and other acknowledged volcanos. A variety of porous ftones may be found on Cader Idris, and Snowdon; and thefe have been miftaken for cellular lava; they confift, however, merely of decompofed granite, porphyry, or toad-ftone. Fragments of this laft, indeed, I have found in the plain of Salop, fo porous, and penetrated with carbonate of iron, as greatly to refemble a flag.

II. The indefatigable Sauffure, whofe accurate refearches into the pofition and nature of the Alps, and the other furrounding mountains, have defervedly ranked him among the moft illuftrious and perfevering mineralogifts, fays, in the firft volume of his Voyages dans les Alpes, "It is a general obfervation, with few exceptions, that, in the greater chains of mountains, the exterior ridges are of lime; the next contain flates; to thefe fucceed the primitive ftratified rocks, and then the granites." The relative pofition of the Welfh mountains tends to confirm a remark made among the Swifs Alps. For if from the central ridge of the Snowdon chain (in which term I comprehend the whole mountainous extent of Caernarvonfhire from north to fouth) we proceed to the Menai, it will be found that the primitive rocks in mafs, fuch as the granites and porphyries, occupy the interior and higher peaks; to the fide of thefe are applied the banks of primitive ftratified rocks; then come the flates, which terminate in the lime-ftone, which forms the bank of the Menai. The fame gradation of ftrata will appear, if, inftead of the weftern, we examine the eaftern fide of Snowdon; the variation is not indeed fo fudden, but perhaps on that very account is more interefting, as the fpecies and varieties of rocks are more numerous, and in larger maffes. From the peak of Snowdon to Llanrwft through Capel Cerig are found granite and porphyry in mafs, micaceous fchiftus, and other primitive ftratified rocks; ferpentine in large blocks and of extraordinary beauty, and hornblen flate mixed with veins and rocks of quartz: from the vale of Llanrwft to Llangollen extend the flates, which are there circumfcribed by the lime-ftone range already mentioned. The general difpofition of the mountains of North Wales may be defcribed in a very few words. There are two ridges of primitive mountains extending nearly due north and fouth, of which one is the Snowdon chain, and the other the Cader Idris chain (comprehending, befides this mountain, the Arrans and other lofty peaks that overlook the fouthern extremity of Bala-Pool). Owing to the near approach of the primitive and fecondary mountains to the coaft of Merioneth, the lime does not commence till near the port of Crickaeth; hence proceeding northwards in an interrupted line

* From "A Journal of a Tour through North Wales and Part of Shropfhire; with Obfervations in Mineralogy and other Parts of Natural Hiftory." By Arthur Aikin. Small octavo, 231 pages. Printed for Johnfon, London, 1797.

This Journal is of much value for the interefting fcientific obfervations it contains, as well as for authentic accounts of the manufactories and mine-works in the diftrict through which the tour was made. Its plan in fome refpect refembles that of the celebrated De Sauffure; like whom our author has given fome animated fketches of the grand romantic fcenery of a mountain country. N.

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along the ſhore, it arrives at Caernarvon. From this place it proceeds along the Menai, forming the eaſtern bank, as far as Bangor Ferry. Hence to Orme's-head it is cut off by the northern extremity of the Snowdon chain, which terminates in the bay of Conway, by the cliffs of Penmaen-mawr and Penmaen-bach. The lime recommencing in the lofty promontory of Orme's-head, continues the boundary of the coaſt as far as the mouth of the Dee; it then takes a weſterly direction, curving to the ſouth, as it paſſes by Holywell and the upper end of the vale of Clwyd to the Eglwyſeg rocks in Llangollen vale; then, paſſing due ſouth, it appears on the oppoſite ſide of the vale; is broken near Oſweſtry by the Ferwyn mountains, appears again at Llanymynech, and is at length ſtopped in its courſe by a line of primitive mountains ſtretching northwards out of Radnorſhire. The ſlates occupy the whole intermediate ſpace between the ridge of lime and the primitive mountains.

The primitive, ſecondary, and derivative mountains may in general be diſtinguiſhed by peculiarities in their form, as well as by their relative poſition; the primitive rocks are craggy, ſteep, and tending more or leſs to a peak, or ſlender-pointed ſummit; the loftieſt mountains are generally about the middle of the chain, which both commences and terminates in abrupt precipices: theſe, together with the inſulated peaks that are continually interrupting the outline of the chain, form a very ſtriking diſtinctive character. The plates to Mr. Pennant's Snowdonia will convey a clearer idea of this than the moſt laboured deſcription. Indeed it is but juſtice to obſerve, with reſpect to theſe engravings, that they are perfectly accurate repreſentations of the ſcenes which they profeſs to deſcribe.

The ſlates are diſtinguiſhable from the primitive mountains by their inferior height, by the evenneſs and almoſt ſquareneſs of the individual hills, and by the eaſy flowing though varied outline of the chains, ſuch as that of the Ferwyn mountains already deſcribed, Chap. III.

The lime and ſand-ſtone hills are conſiderably lower even than the ſlates; riſing in general very gradually at one extremity, and terminating abruptly at the other. The banks of ſand rock are however broader and rounder than the lime: where the lime is the hardeſt, its form is the moſt perfect; but as it becomes ſlaty, ſoft, and mixed with clay, it approaches nearly to the form of the ſlate-hills, as is remarkably the caſe in the ſouthern part of Wenlock-edge. The ſand-ſtone, too, where it contains but little iron and clay, being almoſt wholly compoſed of ſand and lime, reſembles moſt the limeſtone hills. This may be obſerved by comparing the difference of form between the red ſand rocks of Neſcliff, and the white freeſtone of Grinſhill.

III. I have already mentioned the beds of rounded pebbles that are to be found on the higheſt parts of the ſlate mountains. Their preſent ſituation could never have been that in which they were formed, for they conſiſt almoſt univerſally of porphyry, quartz, ſerpentine and other ſtony ſubſtances which lie in large maſſes, compoſing the primitive mountains: their rounded ſhape too, like that of the pebbles on the ſea ſhore, ſeems to intimate that they have been carried by the force of water to the places which they now occupy. Another circumſtance which appears to point out the quarter whence they originally proceeded is, that, in proportion to their vicinity to the primitive mountains, is their ſize: a circumſtance that might naturally be expected, ſince the further they were carried, the more would they be rounded and comminuted. Still, however, there is a difficulty attending this hypotheſis; namely, by what means could theſe rounded pebbles have been

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forced acroſs the many deep valleys that interſect the mountains in all directions, without firſt filling up the valleys themſelves? And, if this was the caſe, by what means were the valleys ſo entirely cleared of them afterwards, as they appear now for the moſt part to be? The difficulty, however, I think, is more apparent than real; for it ſeems highly probable, that at the time when theſe ſlate mountains were formed under the water, there were no valleys, but the whole maſs was one uniform bank, the valleys being afterwards formed by the rivers as the water ſubſided: by this not improbable ſuppoſition, as it appears to me, the objection is wholly removed. On deſcending from the ſlate rocks to the limeſtone and the derivative hills, the marks of ſubmerſion are more numerous and unequivocal; ſand, pebbles, ſhells, and other marine exuviæ, being found in conſiderable abundance. Following the example of moſt mineralogiſts, I might indeed have mentioned the exiſtence of lime as of itſelf a ſufficient proof of ſubmerſion: it appears to me, however, that there is by no means evidence ſufficient to ſupport the large aſſertion, that all the lime which forms ſo conſiderable a part of the ſurface of the earth, has been actually produced by the proceſs of animalization. The only fact that I recollect, which has any reference to this queſtion, would lead one to draw a directly contrary inference; it being well known that the eggs of hens are without ſhells, when care has been taken in the feeding of the bird to hinder its having acceſs to mortar or any other ſubſtance that contains a large quantity of calcareous matter. Theſe tokens of the preſence of water on the tops of mountains that are now 2000 feet above the level of the ſea, naturally lead me to enquire into the cauſe of this phenomenon, in explanation of which two hypotheſes have been ſtarted; one, that the continents were forcibly elevated to their preſent height above the ſea by ſucceſſive exploſions, as ſome of the Lipari iſlands are known to have been formed; the other, that the ſea has gradually, or ſuddenly, ſubſided to its preſent level, but that no alteration has taken place in the poſition of the mountains. The extreme ſcarcity of acknowledged volcanic productions ſeems to render the firſt ſuppoſition highly improbable; and it may alſo be objected to it, that an immenſe power, to which we ſee at preſent ſcarcely any thing analogous, is called into exiſtence to accompliſh that which the operation of common allowed cauſes would effect juſt as well. The grand difficulty is to account for the diſappearing of ſo great a quantity of water, nor indeed have I ever ſeen this explained in a tolerably ſatisfactory manner: it is eaſy to imagine vaſt chaſms in the earth into which the waters have retired; but of this there is no proof whatever: it is alſo contrary to the gradual decreaſe of the ſea, which, from the preſent appearance of the earth, and from hiſtorical records, appears probable.

IV. The Welſh primitive mountains in maſs contain no metals; copper, however, is found in ſeveral of the hornſtone ſtratified mountains, of which the Parys mine, and thoſe at Llanberris and Pont-aberglaſlyn are examples. In theſe mines the ore is for the moſt part yellow ſulphuret of copper: the green and blue malachites, or carbonates of copper, are found in limeſtone, as at Orme's-head and Llanymynech-hill; nor have I heard any inſtance of theſe two laſt mines furniſhing copper in any ſtate but that of carbonate. Carbonated copper is alſo found in the calcareous cement of ſand rocks, as has been already mentioned to be the caſe at Pym-hill and Hawkſtone in the plain of Shrewſbury. Lead and calamine, I believe, are not to be found in North Wales, in any of the primitive ſtratified rocks. Theſe metals are moſt frequently found in ſlate, with a matrix however of calcareous ſpar,

ſpar, as in the vale of Conway, at Llangynnog, and the Snailbeach mines; they are frequently alſo found in limeſtone as at Llanymynech and Holywell. Reſpecting the formation of the above-mentioned metals, it is not eaſy to give a tolerably probable opinion; it appears, however, that carbonate of copper is of conſiderably later formation than the ſulphuret, the former probably originating from the decompoſition of the latter, and deriving its acid from the carbonate of lime, in which it is found. It is not likely that the lead found in limeſtone was originally formed elſewhere, becauſe lead, even in ſlate rocks, lies in a matrix of calcareous ſpar; and eſpecially becauſe it does not form thin ſtrata between the ſtrata of lime, as is the caſe with copper, but it traverſes, in a ſtream, the ſeveral ſtrata without any alteration in the line of its direction; to which may be added, that ſulphuret of lead is the general ſtate in which the metal is found, both in the ſlate rocks and limeſtone, the carbonate being equally rare in both ſituations.

There is no coal found in North Wales between the primitive mountains and the ſlates: a very ſmall quantity is procured between the ſlates and limeſtone; but by far the moſt extenſive beds are between the limeſtone and the ſand rocks, as about Wrexham or Coalbrook Dale, or between theſe laſt and the alluvial hills, as round Wolverhampton.

V. From the above-mentioned circumſtances it would appear, that at ſome former period the ſea covered the whole of North Wales, of the preſent plain of Salop, and of Cheſhire, except a line of iſlands conſiſting of the Snowdon chain, another to the ſouth conſiſting of the preſent Cader Idris chain, and a few detached rocks, ſeveral leagues to the eaſt, which now form the tops of the Wreakin, Caer Caradoc, and Stiperſtones. Under this primitive ſea, and prior to the exiſtence of animals or vegetables, the vaſt banks of ſlate appear to have been formed. My reaſons for thinking that animals and vegetables had as yet no exiſtence are, becauſe there was no ſoil upon theſe hard inſulated rocks for the growth of plants upon which the animals might feed, and becauſe we meet with no impreſſions or remains of organized bodies in the primitive mountains or banks of ſlate. By ſlow degrees the water ſubſided; being poſſibly in part abſorbed by the earth, in part fixed in a ſolid form in the ſlates, and in part decompoſed, forming oxygene and hydrogene: the former of which conſtituted the oxygenous baſe of the lower atmoſphere; the latter, by its ſuperior levity, riſing above the atmoſpherical air, according to the opinion of ſome philoſophers, forms a vaſt ſtratum many miles above the ſurface of the earth; whence originate meteors, the aurora borealis, and other ſimilar appearances.

The water having for the moſt part retired from the beds of ſlate, the greater part of Wales, and the ſecondary hills of the Engliſh counties weſt of the Severn, would form one or more conſiderable iſlands, ſeparated from the ſmall part of England then above water by a wide channel occupying the flat part of Cheſhire and Shropſhire, and the preſent vale of the Severn from Coalbrook Dale to the Briſtol Channel. At this period I imagine the ſecondary limeſtone hills to have been formed. The deſiccation of the water ſtill continuing, the tops of the limeſtone ridges themſelves would begin to appear above the ſurface; and then the plain of Salop, the flat part of Cheſhire, and the ſouthern extremity of Lancaſhire, would form one vaſt bay; into which the Severn, Dee, and Merſey emptied themſelves, flowing into the ſea by an united ſtream, filling the preſent mouths of the two latter rivers, and the intermediate ſpace, the hundred of Wirral. Into this bay or eſtuary a large quantity of ſand would be conſtantly poured in by the violent weſtern winds, and the

the currents of the three rivers not being able entirely to clear it away, banks of ſand would be by degrees formed, conſtituting the preſent ſand and free-ſtone rocks extending from Neſcliff eaſtward by Pym-hill and Grinſhill to the hills of argillaceous ſchiſtus round the Wreakin. This accumulation of ſand would prevent the free egreſs of the waters of the Severn into the main bay, which by degrees, or more probably at once, urged by a ſtrong weſt wind, and ſwelled unuſually by rains or ſnow, broke through the lime-ſtone rock at Coalbrook Dale, and ruſhed into the Channel, which it has ever ſince flowed in. The banks of ſand, that almoſt entirely ſhut out the Severn from the bay of Cheſhire, prevented the Dee from deviating from its original courſe, and the further decreaſe of the water added conſtantly to the difficulty. A number of particulars might be mentioned in confirmation of the foregoing hypotheſis, derived from the appearance of the hills, the ſoil, and other circumſtances, were it my intention to enter minutely into the ſubject; but this could not be done without the aid of charts and engravings, in a manner capable of intereſting the attention of any, except thoſe who have viſited and carefully obſerved the tract in queſtion.

The ſea, however, has not been uniformly receding; for ſome time paſt it appears to have been advancing upon the Welſh coaſt: a brief enumeration of the proofs of this will conclude the ſubject.

The coaſt of Cardigan from Aberyſtwith northward, if it does not furniſh any direct proof of the advance of the ſea, yet ſhews at leaſt that the water is not retreating, from the circumſtance of there being no beach at high tide, and the many caverns and receſſes in the ſlate rocks on the coaſt that are every day filled by the ſea. The ſouthern part of Merionethſhire exhibits certain proofs of the progreſſive ſtate of the ſea, in the vaſt banks of peat already mentioned, which extend along the ſhore to Tovyn, and ſtretch to an unknown diſtance into the water. From near Harlech a long range of ſand and gravel, including Traeth-mawr and Traeth-bychan, runs twenty-two miles into the ſea, being called at preſent Sarn Badrig, or the Ship-breaking Cauſeway; the whole of which tract, formerly called Cantrer Gwaelod, or the Lowland Hundred, was about the year 500 overwhelmed by an inundation, occaſioned by the careleſſneſs of thoſe who kept the flood-gates, as is mentioned in an extant poem of Talieſſin. Northward of the town of Abergeley in Denbighſhire, a vaſt extent of inhabited country is ſaid to have been deſtroyed by the ſea; in proof of which an epitaph without date or name in Abergeley church-yard is cited, ſignifying that the perſon to whoſe memory the monument was erected lived three miles to the north. A more deciſive evidence is furniſhed by Mr. Pennant in his Snowdonia. "I have obſerved," ſays he, "at low water, far from the clayey banks, a long tract of hard loam, filled with the bodies of oak trees, tolerably entire, but ſo ſoft as to be cut with a knife as eaſily as wax." Finally, I have obſerved on the Lancaſhire coaſt, a few miles north of Liverpool, the beach overſpread with trunks and branches of oak-trees; the whole ſhore to a conſiderable diſtance inland, being a peat moſs, now for the moſt part covered with ſand; the extent of the moſs along the ſhore is very evident by the almoſt blood-colour of the beach, occaſioned by the boggy iron ore with which the water that oozes out of the peat is highly impregnated. From theſe facts it may, I think, be fairly inferred, that moſt of the preſent ſands which border the coaſt of North Wales and Lancaſhire, were formerly foreſts or cultivated land; and that the ſea is at preſent, and for theſe twelve or thirteen centuries paſt has been, gaining upon the ſhore.

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VII.

An Account of the Fata Morgana; or the Optical Appearance of Figures, in the Sea and the Air, in the Faro of Meſſina. With an Engraving.

As when a ſhepherd of the Hebrid' Iſles
Placed far amid the melancholy main,
(Whether it be lone fancy him beguiles,
Or that aërial beings ſometimes deign
To ſtand, embodied, to our ſenſes plain)
Sees on the naked hill, or valley low,
The whilſt in ocean Phœbus dips his wain,
A vaſt aſſembly moving to and fro:
Then all at once in air diſſolves the wondrous ſhow.

THOMSON.

VARIOUS philoſophical writers and travellers, and among them our Engliſh travellers Brydone and Swinburne, make mention of a very ſtriking phenomenon which occaſionally appears in the Straits of Meſſina, and is known by the name of Fata Morgana, or, as ſome render it, the Caſtles of the Fairy Morgana. The accounts differ from each other, as well with reſpect to the appearances, as the concomitant circumſtances which are ſuppoſed to be neceſſary for producing them. How far the effects themſelves may be ſubject to variation, or to what extent the imagination of the narrators, who ſpeak of the exhibition as calculated to produce aſtoniſhment, may be ſubject to irregularity, would admit of diſcuſſion; but the general certainty of the events is matter of univerſal notoriety, and admits of no doubt. I have not had the good fortune to meet with any of the authors who treat on this ſubject expreſsly from their own knowledge and obſervation, till lately that the Diſſertation of Minaſi * was lent me by the Right Honourable Sir Joſeph Banks, Bart. &c. In this treatiſe the facts are related with much ſimplicity and preciſion, and the philoſophical reaſoning of the author is kept diſtinct from the narrative. I have therefore choſen to collect the preſent account from this author. The engraving, plate X. is copied from the ſame work.

His firſt chapter contains a deſcription of the phenomenon. "When the riſing ſun ſhines from that point whence its incident ray forms an angle of about forty-five degrees on the ſea of Reggio, and the bright ſurface of the water in the bay is not diſturbed either by the wind or the current, the ſpectator being placed on an eminence of the city, with his back to the ſun and his face to the ſea;—on a ſudden there appear in the water, as in a catoptric theatre, various multiplied objects, that is to ſay, numberleſs ſeries of pilaſters, arches, caſtles well delineated, regular columns, lofty towers, ſuperb palaces, with balconies and windows, extended alleys of trees, delightful plains with herds and flocks, armies of men on foot and horſeback, and many other ſtrange images, in their natural colours and proper actions, paſſing rapidly in ſucceſſion along the ſurface of the ſea during the whole of the ſhort period of time while the above-mentioned cauſes remain.

* Diſſertazione prima ſopra un Fenomeno volgarmente detto Fata Morgana: O ſia apparizione di varie, ſucceſſive, bizzarre immagini che per lungo tempo ha ſedotti i popoli, e dato a penſare ai dotti. A ſua Eminenza il Signor Cardinale de Zelada. Del P. Antonio Minaſi Domenicano. In Roma 1773.

"But if, in addition to the circumftances before defcribed, the atmofphere be highly impregnated with vapour, and denfe exhalations not previoufly difperfed by the action of the wind or waves, or rarefied by the fun, it then happens that in this vapour, as in a curtain extended along the channel to the height of about thirty palms, and nearly down to the fea, the obferver will behold the fcene of the fame objects not only reflected from the furface of the fea, but likewife in the air, though not fo diftinct or well defined as the former objects from the fea.

"Laftly, if the air be flightly hazey and opake, and at the fame time dewy and adapted to form the iris, then the above-mentioned objects will appear only at the furface of the fea, as in the firft cafe, but all vividly coloured or fringed with red, green, blue, and other prifmatic colours."

The author therefore diftinguifhes three forts of Fata Morgana: that is to fay, the firft at the furface of the fea, which he calls the Marine Morgana; the fecond in the air, called the Aërial Morgana; and the third only at the furface of the fea, which he calls the Morgana fringed with prifmatic colours.

In a note in this chapter P. Minafi enquires into the etymology of Morgana. After various remarks, he thinks the opinion of thofe who derive this word, which is fo foreign to the Roman idiom, from μωρὸς triftis and γανόω lætitiâ afficio, is not far from the truth, confidering the great exultation and joy this appearance produces in all ranks of people, who on its firft commencement run haftily to the fea, exclaiming Morgana, Morgana! He remarks that he has himfelf feen this appearance three times, and that he would rather behold it again than the moft fuperb theatrical exhibition in the world.

In the fecond chapter the author defcribes the city of Reggio, and the neighbouring coaft of Calabria; by which he fhews that all the objects which are exhibited in the Fata Morgana are derived from objects on fhore.

In his third chapter, confifting of phyfical and aftronomical obfervations, he affirms that the fea in the Straits of Meffina has the appearance of a large inclined fpeculum; that in the alternate current or tide which flows and returns in the Straits for fix hours each way, and is conftantly attended by an oppofite current along fhore to the medium diftance of about a mile and a half, there are many eddies and irregularities at the time of its change of direction; and that the Morgana ufually appears at this period. Whence he enters into confiderations of the relative fituations of the fun and moon, which are neceffary to afford high water at the proper time after fun-rife, as before defcribed. It is high water, that is to fay, the northern current ceafes, at full and change, at nine o'clock. There is probably a fmall rife and fall, though the annotation to a large chart before me affirms that there is none.

In the fourth chapter and fubfequent part of the work, the author collects the opinion and relations of various writers on this fubject, namely, Angelucci, Kircher, Scotus, and others; and he afterwards proceeds to account for the effects, by the fuppofed inclination of the furface of the fea, and its fubdivifion into different plains by the contrary eddies. The aërial effects are referred to confiderations of faline and other effluvia fufpended in the air, which I forbear to abridge, becaufe it feems difficult to make any clear or productive ftatement either from the narrative or the reafoning.

What I feem to collect upon the whole from the feveral relations, and the curious print which

which I have copied, is: 1. That by the fituation of the Faro of Meffina, the current from the fouth, at the expiration of which this phenomenon is moft likely to appear, is fo far impeded by the figure of the land, that a confiderable portion of the water returns along fhore. 2. That it is probable the fame coafts may have a tendency to modify the lower portion of the air in a fimilar manner, during the fouthern breeze; or, in other words, that a fort of bafon is formed by the land, in which the lower air is more difpofed to become motionlefs and calm than elfewhere. 3. That the Morgana Marina prefents inverted images below the real objects, which are multiplied laterally as well as vertically; and that there are repetitions of the fame multiplied objects at more confiderable vertical intervals. This I gather from the appearance of the dome and other objects in the plate. 4. That the Aërial Morgana is not inverted, but, as I am difpofed to conjecture, is more elevated than the original objects. 5. That the fringes of prifmatic colours are produced in falling vapours, fimilar to many appearances which have been defcribed by authors, but not accurately explained by the general principles of refraction through fpheres of water. The fhip is referred to by the author as an object furrounded by thefe fringes: whence it appears that the colours apply to the direct rays from objects, as well as to thofe of the Marine Morgana. 6. Various other objects in the drawing, as well as in the defcription, afford matter for queftion and conjecture, but none perhaps which it may be proper to enlarge upon, until the theory be better known. 7. It feems at all events more probable that thefe appearances are produced by a calm fea, and one or more ftrata of fuper-incumbent air differing in refractive, and confequently reflective power, than from any confiderable change in the furface of the water, with the laws of which we are much better acquainted than with thofe of the atmofphere. 8. By attentive reflection upon the facts and reafonings in Mr. Huddart's paper *, we may form a theory to account for the erect and inverted images: the polifhed furface of the fea may perhaps account for the vertical repetition; but for the lateral multiplication we muft have recourfe to reflecting or refracting planes in the vapour, which appear nearly as difficult to deduce or eftablifh, as thofe which have been fuppofed on the water.

VIII.

A Memoir containing fome Refults arifing from the Action of Cold on the Volatile Oils, and an Examination of the Concretions found in feveral of thefe Oils. By Cit. MARGUERON, Member of the Societé des Pharmaciens at Paris.

[Concluded from page 186.]

An Examination of the Concretions found in feveral Volatile Oils.

THE volatile oil of fennel-feed, which had been long made, had depofited a concretion of a lamellated form. The oil was of an amber-colour, and fmell fcarcely perceptible. It was fluid, and no longer capable of affuming the concrete ftate at the temperature of four or five degrees below congelation. The concrete matter being feparated was yellowifh, and had the tafte and fmell of fennel. When expofed to the air it became dry and friable

* Philofophical Journal, I. 145.

 between

between the fingers, like a resin. When placed between two capsules of glass, and exposed to the hot wood-ashes, it melted and sublimed in needles. On suspending the operation there remained in the capsule a brown matter, which by cooling crystallized in a striated mass. The needles obtained by sublimation were white and silky, with the taste and smell of fennel. On exposure to the flame of a candle they liquefied without taking fire, and by cooling they assumed a concrete transparent state. They were insoluble in the nitric acid; but their solution in alcohol reddened the tincture of tournsole; and when submitted to evaporation it left a crystallized matter in silky needles.

Volatile oil of wormwood, not rectified, and preserved several years in a bottle with the distilled water of the same plant, had deposited a yellowish matter; the oil had little fluidity; its colour was of a deep green, and its smell considerably pungent; the deposited matter was yellowish, and emitted the smell of wormwood; when applied to the tongue it communicated the peculiar taste of the plant; it adhered to the fingers, like turpentine; exposure for some days to the air did not augment its consistence; when it was afterwards presented to the flame of a candle, it took fire, and was covered with small needles. This matter crystallized by cooling, and became dry and brittle, like resin.

The water on which the oil had been preserved very speedily reddened the tincture of tournsole.

The volatile oil of sage, distilled long before, had deposited a concretion which lined the bottom of the bottle in which it was preserved. This concretion was white, and when examined by the magnifier appeared brilliant, crystallized, and laminated. Before I proceeded to examination I compressed it between several folds of the paper Joseph, to deprive it of the oil which might be interposed between its parts. This concretion, exposed to the air, became dry and friable; its solution in alcohol became white by the addition of water. Part of the solution, subjected to spontaneous evaporation, left in the capsule a whitish covering, and some small needles. Exposed to the flame of a candle it did not take fire, but liquefied, and became firm by cooling, and of a resinous appearance. When placed between two capsules, and subjected to a gentle heat, it melted, but only a few particles sublimed. When it was treated comparatively with the camphor of the shops by the nitric acid, and with the same degree of heat, the concretion was liquefied, assumed the consistence of turpentine, and became much coloured: the camphor, on the contrary, entirely dissolved in the nitric acid, and was recovered again with its properties by the addition of water. The nitric acid which I heated with the concretion, did not afford the same result when treated with a like quantity of water.

From these experiments we may conclude, that the concretions observed in the several volatile oils, approach rather to the nature of resins with a superabundance of acid, which form a kind of salt similar to the flowers of benzoin * rather than camphor, to which they

* In 1792, Citizens Deyeux and Vauquelin informed us, that the concretions deposited by cinnamon-water had the properties of the acid of benzoin. M.

That the concretions were not camphor, is no doubt ascertained by the different action of nitrous acid; but how far they may agree with the acid of benzoin, seems to require further proof. It is generally agreed, from the experiments of Lichtenstein and others, that the acid of benzoin is soluble in the vitriolic and nitrous acid, and separable by the addition of water, without alteration of its properties, and that it is not readily altered by digestion or abstraction of the latter acid. N.

have

have frequently been compared. For, if we compare the effects of the nitric acid on these concretions, the foregoing observations shew that its action upon them is very different from its action upon camphor.

IX.

On the Cold Winds which issue out of the Earth. By Professor DE SAUSSURE *and others; with Observations.*

IN the fifth volume of De Saussure's Voyages dans les Alpes, page 342, lately published, the author, after having ascertained by experiment that the water at the bottom of the Alpine Lakes is much colder than temperate, namely, about 4 degrees of Reaumur, while that of the surface indicated about 15°, whereas the temperature of the water at 886 feet and at 1800 feet depth in the Gulph of Genoa, near the land, proved to be above temperate, namely, 10,6 degrees, while at the surface the heat was $16\frac{1}{2}$ degrees, proceeds to make remarks on a phenomenon probably of the same nature, that there are many subterraneous cavities out of which winds issue, which are colder than the mean temperature of the earth. The former effects are considered by this author as very difficult to be accounted for; and indeed they appear to depend on the principle of the almost perfect non-conducting power of fluids with regard to heat, which has scarcely, if at all, been treated by any author but Count Rumford in his seventh essay, published a few days ago. The latter he thinks may more easily be explained. I shall here relate the facts by a translation nearly close, and afterwards give the substance of his reasonings, with such remarks as may present themselves.

He begins his account with the caves of Mont Testaceo near Rome, which were the first that fixed the attention of an accurate observer. The Abbé Nollet observed them in his travels into Italy *, and found their temperature was $9\frac{1}{2}$ degrees on the 9th of September 1749, in the afternoon, while the thermometer in the open air stood at 18 degrees; and he remarks with reason, that their coolness is so much the more astonishing, as they are not deep; that the entrance has scarcely any descent, and that the sun shines for the greatest part of the day upon the door of the entrance.

M. De Saussure found them cooler than the Abbé Nollet did, though he visited them in hotter weather; the reason of which, as he remarks, may be deduced from the explanation of the phenomenon. On the first of July 1773, the external air in the shade was at $20\frac{1}{2}$ degrees; that of one of the caves was at 8; of another at $5\frac{2}{3}$; and of a third at $5\frac{1}{4}$. These caves are made in the side of the mountain, and occupy nearly its whole circumference. Those he entered were on the west side. The walls at the bottom have perforations through which the cold air enters.

The air itself comes through the interstices left between the pieces of broken urns, amphoræ, and other vessels of pottery of which this small mountain appears to be entirely composed. He went to its summit, which is not above two or three hundred feet in

* Acad. des Sciences, 1749, page 486.

height, and every where observed these fragments, which he does not doubt were formerly collected in this place by some public order. At present the police maintains them; for these caves are so useful and valuable, and there is so much apprehension lest their quality should be altered, that every excavation, and even the cultivation of the ground on this small mountain, is prohibited. And it is truly a very singular phenomenon, that in the midst of this district, of which the air is so hot and stifling in summer, there should be found a small isolated hill, from the base of which currents of air of extraordinary coolness should issue on all sides.

It is not less singular, that under a still more southern climate, and in an island like that of Ischia, which is entirely volcanic, and every where abounding with hot springs, a cold subterraneous wind should be found like that here described. Sir William Hamilton assured our author, that there is a similar grotto at Ottiano, at the foot of Vesuvius. These grottos have even an appropriate name; they are called Ventaroles. That of Ischia is called Ventarola della Funera. It is beneath a small chapel dedicated to St. Anthony, which is itself beneath the Casa Monella. On the 9th of March 1773, the thermometer in the open air in the shade out of the grotto stood at 14 degrees, and that which M. De Saussure placed at the bottom of the grotto stood at 6 degrees; and he was assured that in the summer, during the great heats, he would have found it much lower.

The cold caves of St. Marino are at the foot of a hill of grit-stone, on which the capital of this small republic is built. On the 9th of July 1773, about three in the afternoon, the thermometer in the open air stood at 13 degrees; and in the caves at 6. The floor of these caves lies between 320 and 330 toises above the level of the sea.

The caves of Cesi are situated in the town of the same name, which is six miles north of Terni, in the Ecclesiastical State. That inspected by M. De Saussure was in the house of Don Giuseppe Cesi. The cold of this cave, like the preceding caves, does not proceed from its depth, but from a cold air which issues from the crevices of a rock against which it is built. This air issued out with so much force at that time, that it almost extinguished the flambeaux which enlightened them; and the proprietor assured our author, that the wind would have been much stronger, if the weather had not been cold, as it was for the season. In the winter, on the contrary, the wind rushes violently in, and the more so the colder the weather. This is expressed in the Latin verses which the master of the house shewed him:

> Abditus hic ludit vario discrimine ventus,
> Et faciles miros exhibet aura jocos:
> Nam si bruma riget, quæcumque objeceris haurit;
> Evomit æstivo cum calet igne dies, &c.

The master of this house derives great advantage from the coolness of this cave, not only by preserving wines, fruits, and provisions of every kind, but likewise by conducting this cool air by pipes into the apartments. Cocks, at the extremity of these tubes, emit the cold air in such quantities as may be desired. This refinement is carried so far as to convey the air under certain stands, the foot of which is pierced; so that bottles placed on these stands are continually cooled by the wind which issues forth. On the day when M. De Saussure measured the temperature of this subterraneous wind at the entrance of the

 small

ſmall cavern whence it iſſued, he found it at $5\frac{3}{4}$ degrees, while the external air was at $14\frac{1}{2}$ degrees. This was on the 4th of July 1773, in the afternoon; whence, in fact, it appears that the day was very cold for that climate and ſeaſon.

The Cantines, as they are called in Italian Switzerland, or the cold caves of Chiavenna, are likewiſe ſituated againſt a rock to the ſouth-eaſt of the town. The cold air enters into the caves through the crevices of this rock, of which the compoſition is an indurated ſteatites, intermixed in various places with aſbeſtos and flexible amianthus. On the 5th of Auguſt 1777, at noon, the thermometer ſtood in theſe caves at 6 degrees, while in the open air it was at 17.

On this occaſion our author remarks, that the ſtones which compoſe the mountains whence the cold winds iſſue, are very different in their nature; which, as he obſerves, affords an anſwer to the queſtion of the Abbé Nollet, relative to the caves of Mont Teſtaceo, "Is pottery of a nature to be more difficultly heated than other materials; or do the influences of the atmoſphere cauſe refrigeration, which would not take place elſe-where?" It is certain, adds M. De Sauſſure, that this phenomenon does not depend on the nature of the pottery; for the cold winds of Ceſi iſſue from a calcareous mountain, thoſe of St. Marino from grit-ſtone, thoſe of Chiavenna from a ſteatites, &c.

The caves in which M. De Sauſſure found the air coldeſt, and which he moſt carefully obſerved, were thoſe of Caprino, near the lake of Lugan, and oppoſite that agreeable ſmall town of Italian Switzerland. Theſe caves are ſituated at the foot of a calcareous mountain, whoſe very rapid ſlope terminates near the lake.

Before your entrance, you are deſired to remark the cold wind which iſſues from the key-hole of the door, which is ſenſible even at ſeven or eight inches diſtance. On your entrance, the ſenſation of cold is very ſtriking, ſo as even to produce an apprehenſion of inconvenience; and on coming out, you ſeem to enter an oven. In the firſt viſit to theſe caves (June 29, 1771), the author found the thermometer at the bottom of the cave to be $2\frac{1}{3}$ degrees; while in the ſhade in the open air it was at 21. The ſecond time that he viſited them, namely, on the 1ſt of Auguſt 1777, the thermometer deſcended no lower than $4\frac{1}{2}$ degrees; while in the air it ſtood at 18.

What he thinks remarkable in theſe caves is, that they are not deep, nor hollowed into the earth. Their floor is on a level with the ground; the external wall and the roof are entirely expoſed to the open air. There is only the wall at the bottom, and part of the lateral walls, which are within the foot of the mountain. This foot is every where covered with angular fragments of the ſame mountain, and it is from among theſe fragments that the cool air iſſues. By a fortunate chance, he ſaw one of theſe caves conſtructed. The maſon who overlooked the work, affirmed that he was in poſſeſſion of the art of finding proper ſituations; and that it was neceſſary to ſeek for thoſe places whence the wind iſſued out, and to bore apertures correſponding to thoſe places. It is by theſe ventiges that the caves are cooled, as may be eaſily perceived by applying the hand; and againſt theſe it is that the thermometer muſt be placed to find the loweſt temperature.

It is aſſerted that the country is indebted to the ſheep for this diſcovery. A ſhepherd obſerved, that during the great heats his ſheep all repaired to certain places in preference, and applied their noſes to the earth. He endeavoured to aſcertain the reaſon of this preference,

preference, by applying his hand to the ground. The cool air, which, even without any construction, is perceptible, induced him to build a cave.

In the cave which M. De Saussure saw built, there was only the interior wall raised, so that its external face was, at that time, absolutely in the open air; nevertheless his thermometer, placed at the entrance of these ventiges, stood at 4 degrees. He plunged his thermometer to the depth of 8 inches in the ground of this open cave; it then stood at 7 degrees; and at 8, when merely laid on the ground; but on the floor of a close cave it stood at 5. The thermometer, as already remarked, stood at 18 degrees in the open air.

This cold air has no sensible quality different from pure atmospheric air cooled to the same degree. It has neither smell nor taste; but, as the author observes, it would certainly be curious to analyse it.

The constructor of these caves, who appeared to be an intelligent man, expressed his belief to M. De Saussure, that the cold air comes from the inside of the mountain, and issues from clefts concealed beneath the rubbish: but that nevertheless there was no knowledge existing of any cavern or natural repository of ice in this mountain, in which the snows might accumulate during the winter. The mountain is not high enough to preserve visible snow during the summer. It is necessary however, according to M. De Saussure, that the cause of this phenomenon should be very extended; for he was certainly informed that these caves exist as far as Capo di Lago, at the distance of eight miles from Caprino, and even at Mendrisio, which is a league farther off. There are some also on the opposite shore of the lake. It is affirmed that there are several on the banks of the lake of Come; which M. De Saussure more readily believes, because he found the water of the intermitting spring at the Villa of Pliny, situated on the banks of the lake, to be as cold as $7\frac{1}{2}$ degrees.

The last caves described by De Saussure, from whence cool streams of air are emitted, are those of Hergisweil. These were the only ones he saw on that side of the Alps. At Winckel, a village at the distance of one league from Lucerne, he embarked on the lake; and in less than half an hour he was opposite Hergisweil, a village belonging to the canton of Underwald, situated at the bottom of a small bay, and surrounded with meadows and vineyards, in a position highly rural and romantic. At the distance of ten minutes from the village, at the foot of the mountain, are the cold caves, which are nothing but huts of wood except the bottom wall. This wall, like those at Lugan, is applied to the accumulated fragments at the foot of the mountain. The stones of the wall are not bound with mortar; and it is through the interstices between them that the cold wind enters, which issues from the fragments of the mountain.

On the 31st of July 1783, at noon, the thermometer stood at 18,3° in the shade, and 3,3° at the bottom of the cave. The master of the house assured him that milk could be kept in the cave for three weeks without change, flesh meat a month, and strawberries from one year to the other. Near this hut there was another of the same kind, in which snow is kept for sale at Lucerne in the summer; but there was no snow in the cave where he observed the temperature. Close to the hut, under the same roof, a fire was kept for domestic purposes, without any apprehension of its affecting the temperature of the cave. In winter it freezes in these huts rather later than in the open air, but afterwards, as they affirm, more strongly; no doubt, says M. De Saussure, by reason of the returning current of air into these subterraneous cavities.

The

The mountain which overlooks thefe caves is calcareous, its fteep fide lying to the north, againft which the huts are placed. Its name is Renq, and its foot advances into the lake of Lucerne, where it forms a promontory. It is one of the bafes of Mount Pilate, of which it forms part. M. Peyffer informed M. De Sauffure, that the lake is very deep near the rock.

It appears that the cold wind iffues at many places; for at the foot of the mountain in the neighbourhood, if the earth be any where removed which covers the rubbifh, the cold wind which iffues forth may be felt againft the hand *.

The theory by which M. De Sauffure explains the cold of thefe caves is as follows:

It muft be fuppofed that the air which cools thefe caves was included in fubterraneous cavities, not fufficiently deep to be inacceffible to the heat of fummer, and the cold of winter, but yet enough fo to admit of a variation of a few degrees from thefe changes. It muft alfo be fuppofed, that after this air has been fomewhat condenfed by the winter's cold, and the fummer's heat begins to caufe it to iffue forth by dilatation, it is again cooled by evaporation during its paffage through humid clefts, or the interftices of wet ftones.

He thinks that the exiftence of fuch refervoirs, acceffible to the cold of winter and the heat of fummer, is not hypothetical, but an immediate confequence of the facts univerfally attefted by the poffeffors of thefe caves, namely, that the air iffues out in the fummer with fo much the more force as the weather is hotter, and returns again by the fame apertures in proportion to the intenfity of the cold.

Though the cooling effect of evaporation admits of no doubt, he thought it proper to make an experiment refembling what he conceives to happen in this phenomenon. He took a tube of glafs of one inch in diameter, which he filled with fragments of wetted ftone. Through this tube he forced the blaft of a large pair of bellows: the air iffued out of the bellows at the temperature of 18 degrees, and its paffage through the tube caufed it to defcend to 15. He had the fame refult when he ufed a chemical receiver with two necks, half filled with fmall flints wetted; but when he directed the wind of the fame bellows againft the ball of a thermometer furrounded with a wet cloth, the refrigeration was four degrees. A ftill greater refrigeration, namely, of nine degrees, was produced by inclofing the ball of the thermometer in a wet fponge, and whirling it round in the air. But, as this candid obferver juftly remarks, the air in this laft cafe is continually renewed; fo that the circumftances are not accurately parallel to thofe in which the air is fuppofed to become continually more and more loaded with moifture.

* To thefe inftances may be added the caves of Roquefort, of which a defcription is given by Chaptal in the Annales de Chimie, IV. 31, 45. From the defcription of the caves themfelves, which is lefs precife than thofe of M. De Sauffure, it appears that the air iffues from among the fragments of a calcareous mountain. Thefe caves are ufed in the manufacture of a peculiar and highly efteemed cheefe. M. Marcorelle in the month of October found the thermometer of Reaumur defcend in thefe caves to $5\frac{1}{2}$ degrees; while it ftood at 13 degrees in the open air; and Chaptal on the 21ft Auguft 1787, with a good thermometer, which ftood at 23 in the fhade in the open air, found the temperature of a rapid current in one of the caves to be 4 degrees. He was informed that the thermometer had been feen in that expofition as low as 2 degrees above zero. The hotter the external air, the cooler the caves are found to be, becaufe the current is then ftronger.

Mr. Chaptal fuppofes, in his explanation, which is very concife, that the external air enters the earth, and is cooled by evaporation; but he does not point out any caufe why the current is produced, and varies in its velocity in proportion to the external heat. N.

He thinks, therefore, that evaporation would not suffice to explain the refrigeration of seven or eight degrees below temperate, such as is observed in the caves of Lugan, but that it would suffice to explain a cold of five or six degrees below this term, such as that at Cesi, Ischia, and Mont Testàcco. In fact, he supposes a great subterraneous reservoir of air to exist sufficiently near the surface of the earth, that the cold of the winter may cause it to descend three degrees below temperate, or the tenth degree, and that the heat of summer may cause it to rise as much above that term.

When the cold shall have penetrated this reservoir to its maximum, the temperature will be seven degrees.

Afterwards, when the heat of spring begins to dilate it, its temperature will rise, suppose to eight. It will begin to issue out; and the evaporation, diminishing its heat three degrees, will reduce it to five; and this will be its term of the greatest cold. In proportion as the heats of summer shall penetrate the reservoir, the heat of the air which issues out will increase also: it will not however rise above temperate, because the greatest heat of the reservoir will be 13, and evaporation will reduce it to 10.

The comparison of his experiments with each other, as well as with those of the Abbé Nollet, proves, in fact, that the heat of these cool winds increases as the season advances. For the Abbé Nollet found the caves of Mont Testacco at 9,5 on the 9th of September, whereas M. De Saussure observed them at 5,3 on the 1st of July. In the same manner he found the caves of Lugan at $4\frac{1}{2}$ on the 1st of August, and at $2\frac{1}{7}$ a month earlier, namely, on the 29th of June.

When the air has obtained its highest degree of heat, it must remain for a certain time in a state of stagnation; after which the reservoir begins to cool, and absorb the external air. In this situation the coolness of the autumn, and the frosts of winter, are sufficient to preserve the cold state of the caves.

The author remarks, that from the original supposition of the medium temperature being at 10 degrees, and the cold produced by evaporation 3 degrees, it is impossible to explain a degree of cold which in summer is lower than five degrees. For if the reservoir be supposed nearer the surface, such for example as that the cold of the winter would cause it to descend to 5, it is true that the air with the additional cooling by evaporation would issue out in the spring at the temperature of two degrees. But it will likewise follow, that the reservoir would rise in its temperature proportionably higher by the heat of summer.

If, therefore, it be required to explain a greater degree of coolness than 4 or 5 degrees, such as that of Lugan and Hergisweil, and it be not supposed that evaporation in these circumstances can produce more refrigeration than three degrees, it will be necessary to suppose the mean temperature of the reservoir to be lower than 10 degrees; a supposition, as the author observes, by no means forced, at least for the vicinity of the Alps; where alone observations have been made of caves possessing so low a temperature.

M. De Saussure anticipates the objection against his theory, that if the air in these caverns be already saturated with humidity, it cannot produce evaporation, nor consequently cold. But he remarks, that caverns are not all humid; that the caverns here supposed must be of vast extent, in order that the dilatation caused by a few degrees shall afford considerable currents of air through the whole summer; and consequently, that a great quantity of cold dry air must enter in with the winter, which will be very deficcative when expanded

by

by heat, and will dry the fides of thefe caverns. Whence he thinks that the air may be confidered as fufficiently dry to produce an evaporation which may cool it three degrees.

After having given fo interefting and accurate a detail of facts on this curious fubject, from the work of a philofopher to whom the world is fo greatly indebted, I have thought it a point of juftice to relate his theory in a manner fcarcely lefs copious, and that the more particularly, as the notions which occur to me on the fubject have led me to differ from him in opinion.

In the firft place I muft remark, that we have no actual information of the exiftence of fuch vaft caverns, efpecially fo near the furface of the earth. 2. By the author's own experiments it appears, that at very inconfiderable depths, compared with the thicknefs required for the roofs of fuch caverns, the influence of the feafons is fcarcely perceptible. He found the greateft difference at the depth of $29\frac{1}{2}$ feet, in compact ground, during three years, was only from 8,95 degrees to 7,75 degrees, or 1,2 degrees in the whole. 3. If the roof were ever fo thin, it appears from Count Rumford's experiments *, that heat, which in air, and very probably in all fluids whatfoever, is fcarcely tranfmitted but by the afcent of the heated parts or defcent of the parts cooled, would not pafs downwards, becaufe the rarefied parts could not defcend; confequently the expanfion, even in this moft improbable fituation, could not take place. 4. From the experiments of Duvernois, related in the Encyclopédie Méthodique, article Air, page 686, which are alfo to be found in the firft volume of the Annales de Chimie, it appears that common air dilates rather more than $\frac{1}{12}$th part of its bulk by the firft twenty degrees of Reaumur, and therefore fcarcely more than $\frac{1}{80}$th part by the difference of temperature fuppofed by M. De Sauffure. This cave muft therefore contain near the furface of the earth, to be within the reach of the feafons, eighty times as much air as flows out of all the apertures during the whole fummer. For want of data, that is to fay, the apertures and velocities of emiffion, it is impoffible to inftitute a calculation; but it feems utterly improbable that fuch a volume of air fhould have been referved in an appropriate veffel. At all events, it is not in the fmalleft degree likely, that Mont Teftaceo contains, or communicates with, fuch a refervoir. 5. Laftly, it feems a gratuitous fuppofition, that the refervoir fhould be always dry, and the paffage through which the air iffues always wet.

After this undifguifed examination of the confiderations of M. De Sauffure, I fhall myfelf prefent a few notions refpecting the caufe of this phenomenon; with the perfect wifh to fee them refuted, if exifting facts, future difcoveries, or undetected errors fhould render it neceffary.

The whole, then, appears to me to depend upon the fimple circumftance of the cavity between a confiderable mafs of ftones or other fubftances not being capable of fpeedily changing its temperature, by any other means than the contact of the air, or other fluids, which may pafs through it. Let us fuppofe, for example, the cathedral of St. Paul's in London, the dome of which is near 400 feet in height, to be filled with fragments of broken pottery. This large mafs might be fuppofed at firft to poffefs the temperature of the air at the time when it was accumulated. From the imperfect conducting power of pottery and moft earthy fubftances, the effect of the fun's rays, or of the actual heat of the furrounding air, would pene-

* Experimental Effays, VII.

trate to a very little depth. From feveral known facts, it does not feem probable that the variations from thefe caufes would extend to the depth of three feet. Suppofe now the external air to be cooled ten degrees; the whole body of air contained in the interftices of the pottery, being lighter than that without, would afcend through the upper openings of the mafs, in confequence of the preffure from the denfer air into the lower apertures. This procefs would continue until the contents of the edifice had acquired the common temperature; that is to fay (without attending to the capacities of pottery and of air for heat, and the relative bulks of the interftices to the folid parts, but fuppofing thefe to be equal), it would follow that a quantity of air near one thoufand times the bulk of the edifice muft pafs through the interftices before the common temperature would be reftored. As the winter advanced, the current would continue to flow in at the doors, or near the bafe of the heap, and out from the fuperior parts. This would continue fo long as the temperature of the external air continued to be colder than the internal parts of the heap; and the velocity would be greater the greater the difference between thefe temperatures. But when the winter had reduced the whole mafs nearly to the freezing point, or perhaps below it, the return of fpring, rarefying the outward air, would fuffer the internal cold denfe air to flow out below. The warm air would neceffarily prefs in above, become cooled, and flow out again beneath in that ftate of diminifhed temperature. And in this cafe alfo the velocity of the current would be greater the hotter the external air.

It is eafy to fuppofe a prodigious variety of cafes. If the mafs be very large, the extreme variations will be more confiderable, and the effect more permanent and fteady. If the fragments be large, the current will be fwifter, but the refrigeration lefs. And if among thefe fragments there be a conftant caufe of humidity, it may be inferred that this will render the defcending current colder by evaporation than it would otherwife have been.

I was led to mention the cathedral of St. Paul's, from having very frequently in hot weather met a ftrong cool blaft iffuing out of the doors and vaults of that edifice, produced, as I fuppofe, by the refrigeration of the air againft the ponderous maffes of mafonry within its area. As the probable caufe did not occur to me till the prefent inveftigation offered itfelf, I have not examined all the circumftances of this laft fact. Currents of this nature are however very common. The well-known experiment of holding a candle firft at the top and then at the bottom of a door, is of this kind. If the air within the room, or the walls, be warmer than the external air, the flame will be blown outwards at the top, and inwards at the bottom of the door; but the contrary will happen if the walls of the room or its contents be colder. In this cafe, the lower current refembles the cooling blaft of the caves. There is no doubt, if two holes were bored in the door of a cellar in fummer, the one near the top and the other near the bottom, that the upper hole would draw in the air, and the other emit a cold ftream, until the walls had acquired the heat of the external air.

X.

The Combuftion of Phofphorus in the Vacuum of the Air Pump. By DR. MARTINUS VAN MARUM*.

1. HAVING propofed in December 1794 to exhibit in one of the lectures on the Teylerian foundation the combuftion of phofphorus in oxygene, and its combination with that

* Annales de Chimie, XXI. 158.

fubftance

fubftance in a glafs veffel, after the manner of M. Lavoifier; but not being able to avail myfelf of the burning-glafs to fet fire to the phofphorus, I fuppofed I might fucceed by electric fparks. I attempted therefore to give fire to the phofphorus by electrical fparks or fmall difcharges. Neither of thefe being attended with fuccefs, I attached to a fmall piece of phofphorus a little cotton fcarcely weighing three quarters of a grain, which I fprinkled with a little finely powdered refin. I afterwards placed this cotton, which rofe about one-fourth of an inch above the phofphorus, in fuch a pofition between the extremities of two conducting wires within the glafs receiver, that the electric fpark might pafs through it.

In this manner, on the 4th of December 1794, I attempted for the firft time the inflammation and combuftion of phofphorus; without any other defign, however, in this firft experiment than to afcertain, before the lecture, that there fhould be no fault in the apparatus or method of making the experiment; and the time I could give on that day to this experiment being already expired, I did not rarefy the air in the receiver, more than till the mercury in the barometer gauge was about one inch lower than in the ftandard barometer befide it. I then paffed the oxygene gas from the gafometer into the receiver: I fet fire to the phofphorus after the receiver was thus filled, and the experiment was made without any difficulty.

II. As I thought I could now repeat the experiment in my lecture without any rifk of failure, I made the attempt a few days afterwards. I placed another fmall piece of phofphorus in a fmall crucible of platina, which hung in the centre of a glafs receiver thirteen inches in diameter, having firft furrounded the upper part of the fmall ftick of phofphorus with a little cotton powdered with refin. I then proceeded to rarefy the air as much as poffible; but an unexpected appearance prevented me. The candles had been removed, for the better obfervation of the light at the furface of the phofphorus. We faw the light increafe very perceptibly in magnitude and ftrength, when the height of the mercury in the barometric gauge ftill differed one inch from that in the barometer. This light increafed in proportion as the air was more rarefied. I was far from fuppofing that the phofphorus would take fire, for which reafon I continued to work the pump; but, contrary to all expectation, we faw the phofphorus take fire when the mercury was about half an inch lower than that in the barometer.

[*To be continued.*]

MATHEMATICAL CORRESPONDENCE.

QUESTION V. *Anfwered by J. F . . . . . . R.*

THE bulk or volume of a chemical compound being very feldom, and perhaps never, equal to the fum of thofe of its component parts before their union, it follows that the relative quantities of its ingredients cannot be inferred from its fpecific gravity by the mere rule of alligation; but recourfe muft be had to the refults of actual experiment. Perhaps M. Baumé's experiments on the fpecific gravities of different folutions of common falt in water, preparatory to the conftruction of his pefe-liqueur (an account of which will be found in this work, p. 38.), with the appreciation of the fcale, deduced from the obfervation of M. De Morveau, will afford us the beft data for the folution of the prefent queftion. The fpecific specific gravity of the solution given being 1.1213, we shall find, from the table in p. 39, that it contained about 0.1581 of salt, or 444 grains in the whole; being near 29 grains less than what would result from mere computation.

*** The above was the answer sent previous to the appearance of the Editor's note, p. 192. I. was sufficiently aware that the abovementioned table was not to be relied on to indicate the specific gravities of all possible solutions of common salt in water; which indeed he conceived to be so obvious as to preclude the necessity of more restrictive terms. It is however certainly true, that, if Baumé's construction of the instrument, and De Morveau's observation, were both correct, the tabular specific gravity answering to 15° would be accurately that of the solution of 0.15 of salt used by the former; and that the errors of the table, for all inferior degrees of saturation, would be those only arising from the hypothesis of equal graduation on which it is calculated; which, as the Editor observes, p. 192, are probably not very considerable. According, therefore, to this mode of reasoning, the table ought to be perfectly correct about that degree, and serve well enough for giving nearly the specific gravities of all aqueous solutions of common salt, in which its proportion does not exceed 0.17 or 0.18. The great difference between this table and that of Dr. Watson (p. 192.), which gives upwards of 0.18 of salt, instead of 0.15, for the specific gravity of 1.114, seems only to be attributable to the reasons assigned p. 38. Perhaps M. Baumé had either exposed the salt to a considerable heat before he dissolved it, or suffered the solution to evaporate before he used it for the instrument which fell into the hands of M. De Morveau; or possibly even its scale had been transferred from another whose stem bore a smaller proportion to its bulb. The latter table is probably the most to be depended on in all cases.

NEW MATHEMATICAL QUESTION.

Question IX. *By Trigonometricus.*

THE angles of elevation of a terrestrial object situated above the horizon, taken at three given stations in a horizontal plane, being given; it is required to determine from thence its perpendicular height.

NEW PUBLICATIONS.

Count Rumford's Experimental Essays, Political, Economical, and Philosophical. Essay VII. Of the Manner in which Heat is propagated in Fluids. Of a remarkable Law which has been found to obtain in the Condensation of Water with Cold, when it is near the Temperature at which it freezes; and of the wonderful Effects which are produced by the Operation of that Law in the Economy of Nature. Together with Conjectures respecting the final Cause of the Saltness of the Sea. Octavo, 108 pages, with two plates. Price 2s. stitched. Cadell and Davies, 1797.

THIS essay, which contains important discoveries and applications of the doctrine of heat, will demand particular and more ample notice in the present work than can be here given. The author had before proved that the communication of heat through air is almost totally effected

effected by the circulation of its parts. He has since ascertained, that water, and thence probably all other fluids, conduct that quality or matter by a similar process; and from the general facts observed by him, he concludes that they are non-conductors of heat. The experiments contained in this essay are very striking and ingenious, and the author has availed himself of them in forming a set of deductions of great use in the explanation of the economy of heat on the surface of the planet we inhabit.

Traité de la Fonte des Mines, &c.; or, A Treatise on the Fusion of Ores by Pitcoal, and the Construction and Use of Furnaces proper for the Fusion and Refining of Metals and Minerals by means of Pitcoal. Together with the Method of rendering this Coal fit for the same Uses as Charcoal of Wood. By Mr. De Genssane, of the Royal Academy of Sciences at Montpellier, &c. Sold at Paris at the Veterinary Library of M. R. Huzard, Rue de L'Eperon-Saint-Andre-des-Arcs, No. 11. 2 volumes in quarto; the first containing 400 pages, with 34 plates, and the second 534 pages, with 42 plates. Price 20 francs for the two volumes in boards, and 12 francs for the second separately.

The first volume of this work appeared in 1770, and the second, which was retarded at first to verify certain facts, and afterwards for other reasons, was printed in 1776. Other circumstances, unnecessary to be detailed, have prevented the proprietor from offering the work regularly to sale until lately.——Journal des Mines.

Anfangsgrunde der Chemie zum Grundrisse Academischer Vorlesungen nach dem neuen Systeme, &c.; or, Elements of Chemistry, to serve as the Plan of Academical Lessons according to the new Theory. By M. G. Fr. Hildebrandt, Professor of Medicine and Chemistry at the University of Erlang. Printed at Erlang 1794, 3 volumes, large octavo.

The celebrated L. B. Guyton, in the Annales de Chimie, XXI. 333, speaks very highly of this work, for its order and perspicuity. The author begins by exhibiting the general principles of mixture, solution, affinity, precipitation, &c. together with the operations of chemistry. In the next place he treats in separate sections on caloric, light, oxygene, azote, atmospheric air, hydrogene water, the earths, alkalis, acids, and compound salts. These subjects occupy his first volume.—The second volume treats of metals, their alloys, and combinations.—And in the third the chemistry of organized substances is explained.

Notizie, &c. An Account of the Siliceous Incrustations of the Thermal Baths of Italy; and certain remarkable Products found under the Lava which buried Part of the Town of Torre del Greco in 1794. By M. Tompson. Octavo. Naples, 1795.

The author has observed siliceous stalactites in the mountain of Santa Fiora in Tuscany, in the Euganean mountains, in the isle of Ischia, and in the crater of the Solfatara or Pozzuolo. In most of these places there are likewise soda, humid and hot vapour, and sulphur, either in substance or in the state of sulphuric acid. The solution of the silex may therefore be attributed to the combined action of these three chemical agents, particularly the soda. A kind of liquor silicum is formed. The author has discovered that the arundo donax of the family of the bamboos contains sea-salt, and consequently soda; and it is known that the knots of the bamboo contain siliceous concretions. The celebrated Black ascertained the presence of mineral alkali in the Geyser spring in Iceland; round which siliceous incrustations are formed.—Edinburgh Transactions, Vol. 2 and 3.

By digging into the houses of the town of Torre del Greco, which were buried under the lava in 1794, among other things were found: 1. Glass changed into Reaumur's

 porcelain.

porcelain. 2. Iron mineralized, augmented in volume to thrice its former bulk; cryſtallized within in grains or plates, ſome of them three lines in length, with the argentine brilliancy; other portions in red roſettes of ſpecular iron ore, more or leſs participating of the nature of ſulphate of iron. 3. Silver coin fuſed in the ſame degree of heat which copper money reſiſted. 4. A candleſtick of braſs (chandelier de laiton) entirely metamorphoſed, the outſide cryſtallized into blende, intermixed with octahedrons of copper, more or leſs red: the fracture of the candleſtick towards the centre preſents very fine cubes of red copper. 5. Lead converted in ſome inſtances to litharge, and in others to minium, which is ſolid, compact, and of the moſt beautiful red colour.—Pictet, Bibliothéque Britannique.

An Inaugural Diſſertation on the Chemical and Medical Hiſtory of Septon, or Azote. By Winthorp Saltonſtall. Octavo. New York, 1796.

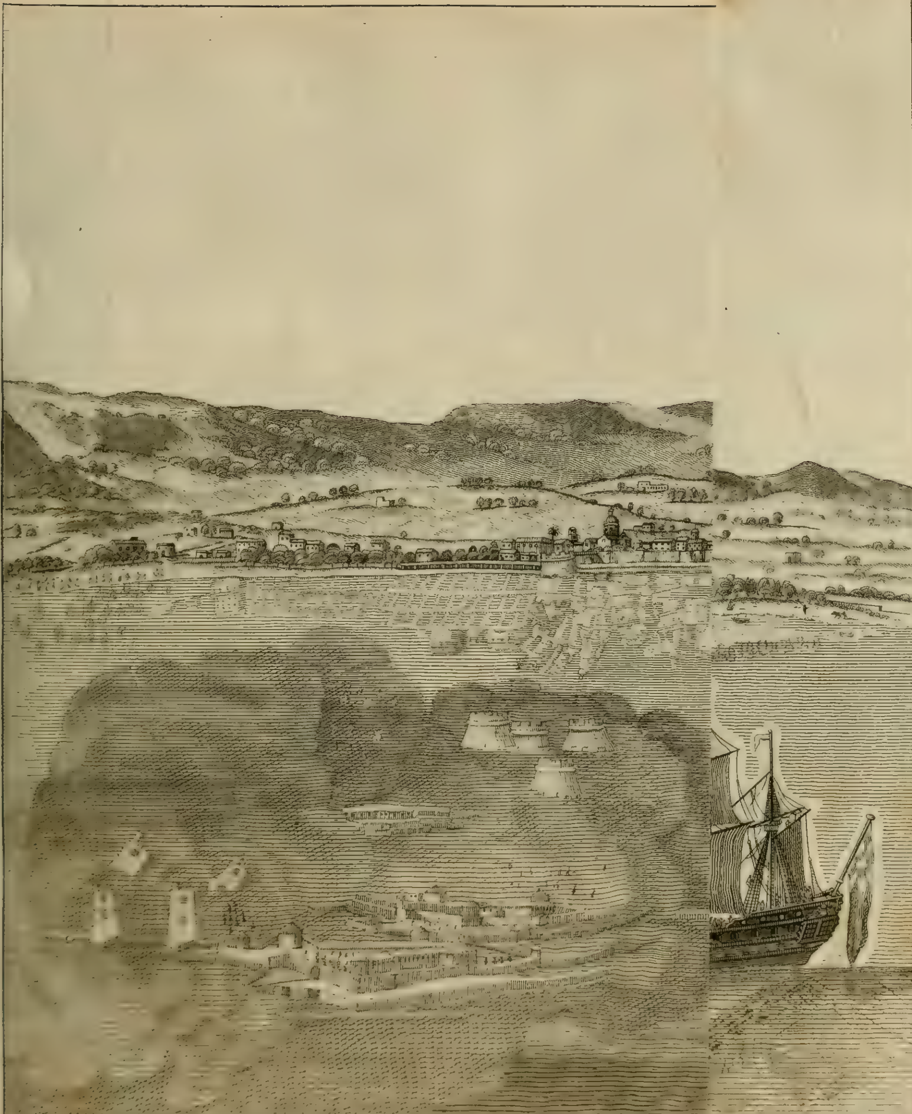
I find this work announced in the 22d volume of the Annals of Chemiſtry, No. 64, for laſt April, with the following obſervations:

It is known that azotic gas forms 73 parts out of 100 in the atmoſphere. Cavendiſh made the important diſcovery, that 68 parts of oxygene, with 32 of azote, produced nitrous gas. Berthollet has proved that 6 parts of azote and 1 of hydrogene form ammoniac. Deimann and Van Trooſtwyk have lately proved that 37 parts of oxygene, chemically united to 63 of azote, form that gaſeous oxide called dephlogiſticated nitrous air by Prieſtley, which has the ſingular property of ſupporting combuſtion, though it deſtroys the life of animals which reſpire it. So that we know of four degrees of oxidation or combuſtion of azote: 1. The dephlogiſticated nitrous air of Prieſtley, called by the modern chemiſts oxide of azote. 2. Nitrous gas. 3. Nitrous acid gas. 4. Nitric acid.

This author thinks that the putrefaction of animal ſubſtances has an epocha in which the azote, at the inſtant of its diſengagement, coming into contact with oxygene, may combine with it without requiring a very elevated temperature. The oxide of azote thus formed may become a very active poiſon; and the cancer, with the whole family of corroding ulcers, has probably no other origin. The exhalations of azotic ſubſtances which putrefy in marſhes, in priſons, and in damp hot countries, form, according to the hypotheſis of the author, a certain chemical combination of azote with oxygene, which becomes the cauſe of contagion, and of various endemic and epidemic diſorders. This deſtructive oxide may introduce itſelf into the animal ſyſtem by the lungs, and produce putrid fevers and aſthmatic ſymptoms; it may penetrate with the aliments into the ſtomach, and become the leaven of epidemic bilious diſorders. Does not the yellow fever of America depend on the ſame morbific gas, ſince we know that it is azote which gives the more or leſs intenſe orange colour to wool, ſilk or ſkin, when it is abundantly contained in thoſe ſubſtances, and combined with a ſmall portion of oxygene? Laſtly, The oxide of azote may act on the abſorbent veſſels of the animal, and produce peſtilential bubos and ſores.

Here, ſay the editors, are conjectures not entirely without ſome appearance of truth; but which require many experiments and obſervations before they can be admitted in the extenſive degree propoſed by this author. For example, it is ſo far from being demonſtrated, as the author thinks, that the orange colour of wool, &c. is owing to azote, that it rather appears to ariſe from an exceſs of carbone. The ſame may be remarked of the oxide of ſepton of this author, or dephlogiſticated nitrous acid. This gas appears to be nitrous vapour mixed with oxygene gas, which laſt does not unite with the ſurplus of nitrous gas in the acid.

X facing page 210.



A View of the

A View of the City of Reggio, with a Sketch of the Phenomenon called Fata Morgana.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER 1797.

ARTICLE I.

Experiments and Observations made with the View of ascertaining the Nature of the Gaz produced by passing Electric Discharges through Water; with a Description of the Apparatus for these Experiments. By GEORGE PEARSON, *M. D. F. R. S.* *

IN the Journal de Physique for the month of November 1789, were published the very curious and interesting experiments of Messrs. Paets Van Trooftwyk and Deiman, with the assistance of Mr. Cuthbertson, on the apparent decomposition of water by electric discharges.

The apparatus employed was a tube 12 inches in length, and its bore was 1-8th of an inch in diameter, English measure; which was hermetically sealed at one end, and, while it was sealing, 1½ inch of gold or platina wire was introduced within the tube, and fixed into the closed end, by melting the glass around the extremity of the wire. Another wire of platina, or of gold, with platina wire at its extremity, immersed in quicksilver, was introduced at the open end of the tube, which extended to within 5-8ths of an inch of the upper wire, which, as was just said, was fixed into the sealed extremity †.

The tube was filled with distilled water, which had been freed from air by means of

* Communicated by the Author. This paper was read before the Royal Society, and an abstract of the same published in the Transactions for 1797. N.

† In another part of Mr. Van Trooftwyk's memoir it is stated that the distance was 1¼ inch from the end of the upper wire to the top of the lower wire; and that the distance between the insulated ball and prime conductor was at first 3-4ths of an inch, but that afterwards it was increased to one inch. Although the wire fastened into the top of the tube was said to be 1½ inch in length, it is observed, that when a column of 3-8ths of an inch of air was collected, it was almost at the extremity of the upper wire. From these and other inaccuracies, it will [illegible] appear, that no one, from the account published, has been able to repeat the experiment.

Cuthbertſon's laſt improved air pump, of the greateſt rarefying power. As the open end of the tube was immerſed in a cup of quickſilver, a little common air was let up into the convex part of the curved end of the tube, with the view of preventing fracture from the electrical diſcharges.

The wire which paſſed through the ſealed extremity was ſet in contact with a braſs inſulated ball; and this inſulated ball was placed at a little diſtance from the prime conductor of the electrical machine. The wire of the lower or open extremity, immerſed in quickſilver, communicated by a wire or chain with the exterior coated ſurface of a Leyden jar, which contained about a ſquare foot of coating; and the ball of the jar was in contact with the prime conductor.

The electrical machine conſiſted of two plates of 31 inches in diameter, and ſimilar to that of Teyler. It poſſeſſed the power of cauſing the jar to diſcharge itſelf 25 times in 15 revolutions. When the braſs ball and that of the prime conductor were in contact, no air or gaz was diſengaged from the water by the electrical diſcharges; but on gradually increaſing their diſtance from one another, the poſition was found in which gaz was diſengaged, and which aſcended immediately to the top of the tube. By continuing the diſcharges, gaz continued to be diſengaged, and aſcend, till it reached nearly to the lower extremity of the upper wire; and then a diſcharge occaſioned the whole of the gaz to diſappear, a ſmall portion excepted, and its place was conſequently ſupplied by water.

The reſiduary portion of gaz being let out after each experiment, and the diſcharges being continued in the ſame water, this reſiduary gaz was left in ſmaller and ſmaller quantity; ſo that after four experiments, probably made on the ſame day, it did not amount to more than 1-80th of the bulk of gaz which had been produced. If it had been poſſible to paſs electric ſparks through this very ſmall quantity of gaz a ſecond time, or oftener, it was ſuppoſed it would have been diminiſhed ſtill more. But when the tube had been left for a night only filled with water, the reſiduary gaz was in greater quantity than after the laſt experiment the preceding day *.

It was concluded that the gaz produced by the electrical diſcharges was oxygen and hydrogen gaz, from decompounded water:

1. Becauſe no other gaz hitherto known inſtantly diſappears on paſſing through it an electric ſpark.

2. The gaz obtained muſt have been the oxygen and hydrogen of decompounded water, becauſe they were in exactly thoſe proportions in which by combination they reproduce water; the trifling reſidue being conſidered to be merely a portion of air which had been diſſolved in the water.

3. Liquids which are not compounded of hydrogen and oxygen, as ſulphuric and nitric acids, afforded gaz by the electric diſcharges, but which did not diſappear on paſſing through it an electric ſpark; but which did diſappear on adding to it nitrous gaz over water. Mr. Schurer alſo aſſerts, on the authority of Mr. Van Trooſtwyk, that even liquid

* In at leaſt fifty experiments I have never ſeen the reſidue of gaz leſs than 1-40th of the gaz produced, although the water had been freed from air by the moſt effectual means. But Mr. Schurer (Annales de Chimie, tom. v. p. 276.) teſtifies that he ſaw Mr. Van Trooſtwyk make the experiment; and that after it was repeated many times, on the ſame parcel of water, *there was no reſidue at all.* I have very good grounds for believing, that this is one of the number of inaccuracies in the account publiſhed of this ſubject.

muriatic

muriatic acid, which contains a very large proportion of water, affords hydrogen gaz only, the oxygen being abſorbed by the muriatic acid, and becoming oxy-muriatic acid.

From much experience I can ſafely affirm, that it is ſcarcely poſſible for the ſtudent, or even the proficient, to inſtitute the above experiment with ſucceſs from the explanation publiſhed. Hence, during the ſix years which have elapſed ſince its publication, no confirmation has been publiſhed except the experiment repeated by Mr. Cuthbertſon, for my ſatisfaction, as related in my work on the chemical nomenclature; but I have heard of many perſons, and ſome of whom were experienced electricians and chemiſts, who made the attempt.

Since Mr. Cuthbertſon came to reſide in London, I have learned from him the circumſtances requiſite to the ſucceſs of the experiment; and I have received from him alſo very great aſſiſtance in continuing a proceſs with the objects I had in view, the tedioufneſs and even difficulties of which can only be conceived by thoſe who have been engaged in the ſame purſuit.

I am very ſenſible that it would be unneceſſary for me to explain the importance of a proceſs which may at laſt afford demonſtration of the compoſition of water, by the fulleſt and unequivocal evidence of its analyſis and ſyntheſis; a demonſtration which no other ſingle proceſs but the preſent promiſes to afford.

I propoſe therefore in this paper:

1. To give ſuch a deſcription of the experiment of rendering water into gaz by electric diſcharges, as ſhall enable any perſon who is verſed in pneumatic chemiſtry, and acquainted with the theory and practice of electricity, to repeat it with ſucceſs. By this deſcription, alſo, I apprehend I ſhall make known more generally the very elegant, and frequently moſt ſatisfactory, mode of decompounding and compounding bodies, by means of the fire of the electric diſcharge.

2. It is propoſed to relate the additional evidence which I have already obtained from this proceſs, concerning the compoſition of water. For although it ſeems moſt probable that water is really decompounded in Mr. Van Trooſtwyk's experiment, it muſt be confeſſed that it does not make appear a ſingle unequivocal and deciſive property of hydrogen and oxygen in the gaz produced. The diſappearance of this gaz by combuſtion, or in ſome other way, inſtantly on paſſing through it an electric ſpark, it is true, is a property known only to belong to the mixture of oxygen and hydrogen gaz; but it is well aſcertained, that things of totally different ſpecies may agree in one or more properties. And there is at leaſt a poſſibility, that electric diſcharges may produce various other kinds of gazes in water, beſide hydrogen and oxygen from decompounded water; and which may have the property of inſtantly diſappearing on paſſing through them an electric ſpark.

3. I ſhall attempt to reſolve the phenomena of the proceſs into a general law of the action of fire, or of the joint action of caloric and light.

SECTION I.

Of the Manner of conducting the Proceſs.

ELECTRIC diſcharges may be employed in two different manners to decompound water. One of theſe is by what has been termed *the interrupted exploſion;* which was the method,

thod, although not fo explained, of Mr. Van Trooftwyk. And the other method is by means of *the uninterrupted or complete explofion*; for which there are two different kinds of apparatus. Thefe were invented by Mr. Cuthbertfon in the courfe of my inveftigation of this fubject.

To fucceed by the method of the *interrupted explofion*, the following are the neceffary parts of the apparatus to be ufed, and the circumftances to be attended to:

1. *The electrical machine muft poffefs fufficient power.* I do not think any cylindrical machine can be made to anfwer in this procefs if a large quantity of gaz be required; becaufe they cannot be made to act with due regularity, conftancy, and force. Inequality of the furface of the cylinder is unavoidable, which caufes undulation. A cylindrical machine never continues in full force above five or fix minutes, without frefh amalgam. Hence, from the repeated amalgamization, the difcharges will be fo variable that the tubes muft be frequently broken.

I ufed plate machines of a peculiar conftruction by Cuthbertfon. Thefe machines do not require frefh amalgamization oftener than once in eight hours, and they poffefs fuperior powers of acting, in point of regularity, force, and duration. A plate of 24 inches in diameter, with a jar containing 150 fquare inches of coating, afforded an adequate difcharge every fecond or third revolution, for feveral hours; and for a ftill longer time every third or fourth revolution, with one application of amalgam. A 31 inch plate machine afforded a due difcharge at firft every revolution, and afterwards every fecond revolution for many hours, with one application of amalgam. The moft ufeful and expeditious machine was that with two plates, each 24 inches in diameter, and fimilar to that of Teyler. It produced 25 difcharges every 15 revolutions for an hour or two; and for four or five hours longer a difcharge was produced by lefs than two revolutions, with one amalgamization.

2. *The Leyden jar muft have a fufficient quantity of coated furface*; without which the difcharge will not be fufficiently powerful to produce the gaz required. The proper quantity, as found by experience, was about 150 or 160 fquare inches, with an ufual proportional prime conductor.

3. *The diftance between the infulated ball, and the prime conductor, muft always be lefs than the diftance between the extremities of the wires.* Not the leaft notice of this circumftance has been taken; yet without attention to it the experiment can never fucceed, or only for a very fhort time. Accordingly, as the diftance between the extremities of the wires within the tube anfwered beft when it was 5-8ths or 7-8ths of an inch, the diftance between the infulated ball and prime conductor was feldom more, but frequently lefs, than 5-8ths or 6-8ths of an inch. The eye muft be kept upon the fparks within the tube, and by practice a perfon may become a judge of their force by their vividnefs; which will direct him to bring the receiving ball nearer to the prime conductor, when there appears danger of the tube being broken; and on the contrary, to remove them to a greater diftance from one another when the fparks do not produce gaz duly from the water. When the difcharge is of the moft productive force, both ends of the wire within the tube will be illuminated by a fpark; but, when it is weaker, one end only of the wire will be illuminated; and when this is the cafe, there is no rifque of the tube being fractured, but gaz will rife from the end of one wire only inftead of two.

4. *The extremities of the upper and under wire within the tube muft be at a certain diftance*

from one another. If they be too near one another, the points of them will not be illuminated; and provided the insulated ball be as near to the prime conductor as the two wires are to one another, the tube will be broken, becaufe there will be a complete explofion. But if the wires be at too great a diftance from one another, the electric fluid of the difcharge will be fo diffufed through the water that no gaz will be produced. If the Leyden jar contain, as above ftated, 150 or 160 fquare inches of coated furface; and the ball of the prime conductor and of the infulated ball be about three inches in diameter, the diftance between the wires which generally anfwered beft, was about 5-8ths or 7-8ths of an inch, as above faid. The narrower the bore of the tube, the greater may be the diftance between the two wires; accordingly, the diftance may be one inch with a tube 1-14th of an inch wide.

5. *The upper wire fixed into the clofed extremity of the tubes muft be of a proper length and thicknefs.* If this wire be too long, either the difcharge will not be carried through to the end of the lower wire in fufficient quantity to produce gaz; or, if it be in fufficient quantity to produce gaz, the tube will be fractured. The fmaller the diameter of the tube, the longer may be the upper wire, for a reafon to be given under the next head. I generally found that the difcharge requifite to produce gaz fractured the tube, if the upper wire was more than 6-8ths or 7-8ths of an inch in length, within a tube of more than 1-8th of an inch in diameter. But with very narrow tubes, fuch as thofe of 1-16th of an inch in diameter, I frequently fucceeded when the upper wire was 1¼th of an inch in length. It is obvious, that the fhorter the upper wire the more readily will gaz be produced: the procefs, however, will be rendered ftill more tedious in thofe cafes in which a quantity of gaz is to be collected in a refervoir for examination; on account of the time confumed in transferring fuch fmall parcels of gaz by each experiment.

The diameter of the upper wire cannot, perhaps, be too fmall; for the greater its fuperficies, the more electric fluid will be parted with to the furrounding water. Hence platina wire of the fineft fort, as that of 1-240th of an inch in diameter, may be ufed with fuperior advantages. This fort of wire alfo cannot be melted while it is foldering to the glafs, which can hardly be prevented with fine wire of other metals. However, I found that copper, brafs, or gilt wire, of about 1-80th or 1-100th of an inch in diameter, could be foldered to the glafs, and anfwered perfectly. I did not find that any of the metal wires were affected by the difcharge; but iron or fteel ones are not proper, on account of their being fo foon oxidified by the water, and confequently extricating from it hydrogen gaz. I did not find any advantage from ufing feveral fmall wires twifted together, but feparated at the end within the tube; for gaz was extricated generally, at the point of one of them only, namely, the undermoft. I think care fhould be taken to fix the upper wire fo that it fhall be in the middle of the tube; as in that cafe the tube will be lefs liable to be broken. As to the under wire, the diameter of it feems to be of little importance; for, if it be a thick one, as much gaz will be extricated as if it were a fmall one; becaufe the electric difcharge will take the firft point of the furface of the wire where the gaz is produced.

6. *The tubes muft be of a proper length and diameter.* If they be fhorter than feven inches, the difcharge will be liable to pafs over the outfide; and if they be longer than twelve inches, they will be of an inconvenient length. I found the moft convenient length to be from nine to ten inches, exclufive of the curved part. The curved part was found very ufeful in preventing air afcending, which was accidentally let into the tube, by which the product

product of gaz from the experiment would have been contaminated. Such curved extremities were however lefs convenient than ftraight ones, on account of the greater difficulty of transferring from the former than from the latter. See Fig. 1 and 2, Plate XI.

The diameter of the tubes fhould not be more than 1-8th or lefs than 1-10th of an inch. At leaft in my experiments thefe tubes anfwered beft. If they were wider, the difcharge requifite to produce gaz broke the tube. If the tubes be narrower than juft mentioned, the experimenter will find it difficult and tedious to transfer the gaz, through the curved part, into a refervoir, in thofe cafes in which a large quantity is wanted for examination. Where however the object is merely to fhew the production of gaz by means of the electrical difcharge paffed through water, between two wires; and the inftant difappearance of gaz fo produced by paffing through it an electric fpark, the narroweft tubes are moft eligible; as with them the experiment can be made in a fhorter time. But there was one inconvenience experienced from very narrow tubes, namely, the bubbles of gaz were very apt to hang near the end of the upper wire inftead of afcending; and they were apt to form large bubbles, with water between each; in which fituation the difcharge frequently fractured the tube, or made gaz to difappear by combuftion in the courfe of the experiment. To poffefs at once the advantages of the narrow and wide tubes; about three to fix inches of narrow tube were joined by fufion to a bottom part of wider tube (fee Fig. 3); into the curve of which tube the gaz produced was let up from time to time; fo that this part frequently contained the products of ten or more experiments before the gaz was transferred into the refervoir. The vaft number of tubes which were broken in this experiment induced me to try various different kinds of them. I experienced no advantages from annealing tubes; but their being feemingly rendered more brittle, and harder, by this treatment, was an effect which I leaft of all expected. Tubes with thin fides anfwered juft as well as with thick ones. Bohemian green glafs tubes were found excellent for this experiment, as they were much lefs apt to be cracked by the difcharges than any kind of Englifh glafs. To fave time and trouble in fo frequently letting out gaz produced in very narrow tubes, a fmall bulb was made at the fealed end (fee Fig. 4). In this cafe, however, as the upper wire muft be fhorter than other narrow tubes, it was more difficult to regulate the explofion.

Although common atmofpherical air is an electric, and water is a conductor of electricity, it appears that the difcharge paffes with more refiftance from wire to wire through water in the above experiment, than through common air under otherwife the fame circumftances. The reafon of which is this: air being an elaftic and very rare fluid, it more readily gives way to the electric difcharge than water; and it can therefore pafs through a longer and thicker column of air between two wires, without breaking the glafs tubes, than it can through water. For although water is a conductor, yet in a very fmall quantity it is a very indifferent one; fo that its denfity and defect of elafticity more than compenfate for its conducting power. Hence alfo, and on account of the conducting power of water, the reafon of the upper wire in this experiment being fhorter in proportion as the tube is wider; and on the fame account will be feen the reafon of the advantages of a fmall upper wire over thicker ones.

It will be neceffary to add, that the tubes with curved extremities can only be filled by fetting them in water under a receiver, and exhaufting the air from the receiver, tubes, and water; then, by letting in the air again, the water will be forced up into the tubes. Sometimes, however, I have filled the tubes by fetting them in Papin's digeftor.

 These

These are the directions for making the experiment; but the rationale of it cannot be understood unless the nature of the *interrupted explosion* be explained; because I believe books on electricity do not contain the necessary information. It must be considered, in the above experiment, that if in place of water the tubes be filled with air, the whole of the charge of the Leyden jar will pass, at each explosion, from the upper to the under wire, and no interruption in the discharge will happen; but if they are filled with water, then an *interrupted discharge* may be caused; by which is meant that a part of the charge only passes at each explosion through the water, from wire to wire, and with much diminished velocity. The residuary electricity in the Leyden jar is nearly one half; as may be accurately demonstrated from the difference in point of density, elasticity, and conducting power of the medium of water and air as already observed. It must be added, that although water in large quantity is a good conductor, and air is not; yet water being here in very small quantity, it proves a bad conductor, as is the case with the very best conductors. A cubic foot of water is only just capable of receiving, or letting pass through it, a full discharge from a jar of one foot of coated surface; and the quantity of water employed in this experiment not being $\frac{1}{100000}$th part of a cubic foot, it is a very imperfect conductor; so that an interrupted discharge only can pass through the tube, without dispersing the whole of the water. But if the discharge be not seemingly as strong as the tube can bear without breaking, the gaz is not produced from it; and on this point hinges this extremely delicate process.

The situation of the different parts of the above described apparatus is shewn by Fig. 5.

To succeed by the method of the *complete* or *uninterrupted explosion*, the following apparatus must be used, and rules observed:

1. A tube (Fig. 6.) is employed, about four or five inches in length, and 1-5th or 1-6th of an inch in diameter. One end is mounted with a brass cap (Fig. 7.), and the other end is sealed at the lamp, with a wire about 1-40th of an inch in thickness fixed into it, as above described; which extends into the brass cap, so as to be almost in contact when the explosion is made. If the wire touches the brass cap there will be no explosion. The tube being filled with, and set in, a cup of water, the discharge may be made into it as in the above described process; but here the insulated ball must be placed at a greater distance from the prime conductor, and a Leyden jar, with only 50 square inches of coating, will answer the purpose. In this way of making the experiment, gaz is produced by each discharge in the brass tube, and in much greater quantity, and with much less frequent accidents, and less trouble, than in the former method with the interrupted discharge. But the gaz obtained with this apparatus always contains a larger proportion of atmospherical air, on account of the quantity of water, and more immediate and extensive communication of it with the atmosphere. By repeated discharges there is an impression made in the brass tube, in the part where the discharge passes through it, and at last a small hole is made in that part. On this account the same mounted tube cannot serve for producing a large quantity of gaz.

The other sort of apparatus invented by Mr. Cuthbertson is represented by Fig. 8. At first it consisted of a glass tube half an inch wide and about five inches in length, mounted at one end with a brass funnel, and inverted in a brass dish; but afterwards the tube was blown funnel-wise at the end, as shewn by Fig. 9. The other end must have a wire about

about 1-40th of an inch thick, ſealed into it at the lamp; which wire extends to nearly the bottom of the braſs diſh in which the tube ſtands.

The exact diſtance between the end of the wire and braſs diſh muſt be found by trials; that which generally anſwered in my experiments was about 1-20th of an inch. If it be properly arranged, gaz will be produced at each diſcharge.

The Leyden jar uſed with this apparatus muſt contain about 150 ſquare inches of coating.

The diſtance between the inſulated ball and the prime conductor, at which the experiment ſucceeded, was commonly about half an inch.

If experiments be propoſed in which electrical diſcharges muſt be paſſed through water or other fluids, for even a much longer time than was conſumed in performing thoſe referred to or related in this paper, it may be an object to employ the wind, or perhaps the power of a horſe, to turn the electrical machines, the expence of labourers being conſiderable.

[*To be continued.*]

II.

Analyſis of four Specimens of Steel; with Reflections on the new Methods employed in this Analyſis. By *Citizen* VAUQUELIN.

[Continued from page 217.]

SECTION III.

STEEL NO. 864—LARGE PIECE.

EXPERIMENT I. 1152 grains, or 61,14 grammes, of this ſteel, broken into ſmall particles, were diſſolved in the ſulphuric acid diluted with five parts of water. There remained at the bottom of the ſolution 5,5 grains, or 0,292 grammes, of black powder of the carburet of iron. In another experiment, the ſame quantity of this ſteel afforded 6,4 of carburet of iron; a quantity, the medium of which is 5,95, and conſequently the reſidue forms a fraction of the diſſolved maſs equal to 0,0051.

Experiment II. 100 grains, or 5,3 grammes, of the ſame ſteel, diſſolved in the ſulphuric acid, afforded 121 inches, or 2420 cubic millimetres of hydrogene gas. The black reſidue weighed ſcarcely 0,75 grains.

Experiment III. The 5,5 grains of carburet of the firſt experiment being ſubjected to heat under a muffle, took fire, and left three grains of a grey yellowiſh maſs, which, when treated with boiling muriatic acid, aſſumed a white colour, while the acid became yellow. It was reduced to 1,66 grains, which amounts to 0,0014 of the maſs of ſteel. This ſubſtance poſſeſſed all the characters of ſilex.

The muriatic acid uſed in this boiling contained a perceptible quantity of iron, which appeared to be the cauſe of the yellow colour it poſſeſſed previous to the operation.

Experiment IV. The ſolution of 1152 grains of iron in the ſulphuric acid having been treated as in Experiment III. Section 2, afforded 58 grains of phoſphate of iron, which makes

makes about 0,05 of the weight of the ſteel employed. The fluid likewiſe depoſited by ebullition 96 grains of a deep red matter, which did not perceptibly contain phoſphate of iron.

From this reſult it appears that the preſent ſteel contains nearly three times the quantity of phoſphorus exhibited by No. 864, the ſmall piece. For 576 afforded only 19 of phoſphate of iron, whereas 1152, or only twice the quantity, afforded 58 grains. This ſteel muſt conſequently be more brittle when cold, if, as there is no reaſon to doubt from the numerous experiments of Bergman and other chemiſts, that phoſphorus is the eſſential cauſe of the cold ſhort quality of iron and ſteel.

Experiment V. 288 grains of ſteel No. 864, the large piece, diſſolved in ſulphureous acid, as before mentioned, left 6,5 of carburet of iron, which, ſuppoſing it to contain as much ſulphur and iron as that of No. 864, the ſmall piece, would give 3,71 of carbone, or about the 0,010th part, or rather more than one-hundredth part of the ſteel employed.

Experiment VI. This ſteel was ſubjected to all the proofs calculated to ſhew the preſence of manganeſe, but without any exhibition of that ſubſtance. It may therefore be concluded that this ſteel, as well as the other examined before, does not contain any perceptible quantity of manganeſe, at leaſt ſo far as the preſent methods of chemiſtry are capable of ſhewing it *.

SECTION IV.

STEEL NO. 977.

Experiment I. 144 grains, or 7,631 grammes, of this ſteel, diſſolved in the ſulphuric acid, diluted with five parts of water, afforded 164,94 cubic inches of hydrogene gas, and left 0,941 grains, or about 0,05 grammes, of carburet of iron, or nearly 0,007 of its maſs.

Experiment II. 504 grains, or about 26,7 grammes, of the ſame ſteel, diſſolved in the ſulphuric acid, diluted with five parts of water, the ſolution being diluted with a large quantity of water, and its exceſs of acid ſaturated with carbonate of pot-aſh, depoſited, a few inſtants afterwards, a large quantity of a white matter, ſlightly inclining to grey, which acquired a ſtraw-colour by contact of the air. This matter was phoſphate of iron, and weighed 22 grains, or about 1,16 grammes.

Experiment III. The liquid whence the phoſphate of iron had been precipitated by the carbonate of pot-aſh was ſubmitted to ebullition, and again depoſited 22 grains, or 1,16 grammes, of matter, in which, however, there was ſcarcely any phoſphate of iron. So that 504 grains of the ſteel No. 977 contained 23 or 24 grains of phoſphate of iron; that is to ſay, 0,047 of its maſs.

Experiment IV. Theſe 24 grains of phoſphate of iron, treated by long ebullition with cauſtic ſoda, aſſumed a deep red colour, and after waſhing, and drying in the air, weighed only 15,33 grains. They loſt therefore 8,67 grains of phoſphoric acid, which afforded 17,54 grains of cryſtallized phoſphate of ſoda.

* The oxide of iron formed with this ſteel, ſtrongly oxided by the action of fire, and afterwards treated with the nitric acid and ſugar, afforded an oxide which threw down a very beautiful blue with the pruſſiate of pot-aſh. The ſame oxide fuſed with borax afforded a greeniſh globule; but when heated with the external flame at the extremity of a pair of forceps it acquired a ſlight purple colour, which diſappeared as the globule became cold.

Experiment V. This ſteel, diſſolved in the ſulphureous acid, afforded very nearly the ſame quantity of carbure of iron as No. 864, the ſmall piece.

SECTION V.

STEEL NO. 1024.

Experiment I. One hundred grains, or about 5,3 grammes, of filings of this ſteel afforded 108 cubic inches of hydrogene gas during their ſolution in the ſulphuric acid.

I have remarked that the hydrogene gas produced by this ſteel had an extremely fetid ſmell, infinitely ſtronger than that of the gaſes afforded by the other ſteels, which appears to ariſe from the greater quantity of phoſphorus it contains *.

Experiment II. 288 grains of filings of the ſame ſteel being diſſolved in the ſulphuric acid, diluted with five parts of water, the carburet of iron ſeparated, the ſolution diluted with water, and the exceſs of acid ſaturated with carbonate of pot-aſh, afforded a very conſiderable white depoſition of phoſphate of iron, which, when well waſhed and dried, remained white, and weighed 26 grains, or about 0,09 of the maſs of ſteel made uſe of. Theſe 26 grains of phoſphate of iron, treated with cauſtic ſoda, afforded 22 grains of phoſphate of ſoda, and the ferruginous reſidue weighed 17 grains.

Experiment III. 288 grains of ſteel, diſſolved in the ſulphureous acid, left a carbonaceous reſidue, which, treated with pot-aſh in the manner deſcribed Section II. and dried, weighed 3,5 grains. But on account of the minute loſſes which it is impoſſible to avoid in a numerous ſeries of manipulations, the quantity of this ſubſtance may be eſtimated at four grains; and that the more reaſonably, as the other kinds of ſteel gave nearly the ſame reſults. The quantity of carburet of iron in this ſteel was therefore ſomewhat leſs than 0,014 of the maſs of the ſteel.

Experiment IV. This ſteel, ſubjected to the proofs requiſite to diſcover the preſence of mangeneſe, preſented no trace thereof.

SECTION VI.

Table of the Quantities of Hydrogene Gas afforded by each Kind of Steel.

By uniting the different quantities of hydrogene gas afforded by the docimaſtic quintal of each of the ſteels diſſolved in the ſulphuric acid, we have the following reſults for each 100 grains.

	Cubic inches.
Steel, No. 864, ſmall piece	108
No. 864, large piece	121
No. 977	114
No. 1024	108

* The ſmell of putrid garlick emitted by iron and ſteel during its ſolution in acids, appears to depend immediately on the preſence of phoſphorus in the metal, of which a portion is diſſolved by the hydrogene gas. For this odour is more ſtrong in proportion to the quantity of phoſphorus. This obſervation proves that the proportion of this ſubſtance to the iron which contains it cannot be accurately determined by the proceſs of ſolution in acids which emit hydrogene gas. The ſulphureous acid may anſwer this purpoſe.

Table

Table of the Quantities of Carburet of Iron, contaminated with Silex, afforded by each of the Steels, converted into Decimal Parts.

Steel, No. 864, ſmall piece	— —	0,015
No. 864, large piece	— —	0,013
No. 977	— — —	0,012
No. 1024	— — —	0,015

As it was aſcertained by ſeveral experiments made upon the carburets of iron extracted from the different kinds of ſteel, that this ſubſtance contains at a medium 0,526 carbone, and 0,474 ſiliceous iron, it follows:

1. That the 15 thouſandth parts of No. 864, ſmall piece, are compoſed of

Carbone	— — —	0,00789
Siliceous iron	— — —	0,00711

2. That the 0,013th parts of No. 864, large piece, are compoſed of.

Carbone	— — —	0,00683
Siliceous iron	— — —	0,00616

3. That the 0,015th parts of No. 977 are formed of

Carbone	— — —	0,00789
Siliceous iron	— — —	0,00711

4. That the 0,012th parts of No. 1024, are compoſed of

Carbone	— — —	0,00631
Siliceous iron	— — —	0,00568

I do not think it neceſſary in this place to aſcertain the proportion between the iron and the ſilex which exiſts in the carburet of iron, becauſe this earthy ſubſtance being to all appearance accidentally in the ſteel, muſt vary according to a great number of different circumſtances.

The following, however, are the mean reſults obtained from ſeveral experiments on the carburet of iron:

1. Carbone	— — —	0,53
2. Iron	— — —	0,26
3. Silex	— — —	0,21
		1,00

Table of the Quantities of Phoſphate of Iron afforded by the four Kinds of Steel.

By comparing the quantities of phoſphate of iron obtained from the four kinds of ſteel here ſubmitted to examination, very remarkable differences appear; as follows:

No. 864, ſmall piece	— —	0,0210
No. 864, large piece	— —	0,0504
No. 977	— — —	0,0474
No. 1024	— — —	0,0900

 Table

Table of the Quantities of Phoſphorus contained in each of the Samples of Steel.

Analyſis having ſhewn me that phoſphate of iron is compoſed of 0,58 of oxide of iron, and 0,42 of phoſphoric acid, and this laſt being formed, according to Lavoiſier, of 0,39 phoſphorus, and 0,61 oxygene, it follows that the ſteel

No. 864, ſmall piece, contained phoſphorus	—	0,00345
No. 864, large piece	—	0,00827
No. 977	—	0,00791
No. 1024	—	0,01580

I cannot venture, however, to affirm that there really exiſts ſo great a difference between the quantities of phoſphorus contained in each of theſe pieces of ſteel as is here given by experiment. For ſo little is required to change the reſults in this reſpect, particularly when ſmall quantities are ſubjected to trial, that two or three thouſandth parts may ſoon be obtained either way. Nevertheleſs, I cannot ſuppoſe, that, with the ſame proceſſes and equal care, the difference between the maximum and minimum of theſe quantities will prove to be the conſequence of uncertainty in the methods of analyſis.

A Synoptic Table of the Proportions of Principles contained in the four Kinds of Steel before examined.

Steel, No. 864, ſmall piece	1. Carbone	0,00789
	2. Silex	0,00315
	3. Phoſphorus	0,00345
	4. Iron	0,98551
		1,00000
— No. 864, large piece	1. Carbone	0,00683
	2. Silex	0,00273
	3. Phoſphorus	0,00827
	4. Iron	0,98217
		1,00000
— No. 977	1. Carbone	0,00789
	2. Silex	0,00315
	3. Phoſphorus	0,00791
	4. Iron	0,98105
		1,00000
— No. 1024	1. Carbone	0,00631
	2. Silex	0,00252
	3. Phoſphorus	0,01520
	4. Iron	0,97597
		1,00000

SECTION

SECTION VII.

Reflections on the Causes of the variable Quantities of Carburet of Iron afforded by Steel dissolved in the Sulphuric Acid.

MANY circumstances may influence the carburet of iron by the same steel; and it is not easy to obtain equal masses of this substance, in two different experiments, from steel of the same nature.

Bergman obtained from 0,002 to 0,008 of carburet of iron from different steels. Is it probable that there was really so great a difference in the proportions of a substance to which the steels owe their qualities, without there having been likewise a very sensible difference in their usual properties?

Among the causes of uncertainty which may occasion the results to vary, we may principally remark the following: 1. The greater or less concentration of the acid employed to dissolve the iron. 2. Its mixture with water at or before the time of the experiment. 3. The more or less considerable division of the metal. 4. The length of time during which the solution is effected. 5. The time during which the deposition of carburet remains in contact with the liquor after the solution is effected.

1. It is evident that the more concentrated the sulphuric acid, the more rapid the solution, and the greater the quantity of caloric developed in a given time.

Now experiment proves that the solution of carbone by hydrogene is in the direct proportion of the caloric which penetrates it, or at least that it follows some progressive ratio of increase with the temperature. It follows, therefore, that the more elevated the temperature during the solution of steel in acids, the less of carbone will remain. It also follows that it is advantageous not to use an acid too concentrated.

2. As the too great concentration of the acid is inconvenient for the solution of steel from which the carburet is to be extracted, it is also equally inconvenient to dilute it too much. It is in fact known, that when acids are too much diluted with water, they exert a less affinity on other bodies; and this is the case with respect to very diluted sulphuric acid, and steel in a high state of division. The solution then takes place very slowly; part of the steel decomposes the water by its own attractive power, and passes to the state of the black oxide, which cannot be dissolved by diluted vitriolic acid. Hence, instead of carburet of iron, the residue consists of a mixture of this substance with the black oxide of the metal.

3. The duration of the experiment is likewise of considerable inportance. It may arise from two causes. The first is the concentration of the acid. In this case, the sulphate of iron which is formed absorbs for its crystallization the small quantity of water which remains, with which it falls down, and entirely stops the solution. The second is the too considerable weakness of the acid, the particles of which being separated too far from those of the metal by the water which retains them, cannot in this case act but very slowly.

I am well convinced that whenever the carburet of iron remains in contact with the metallic solution, particularly when it contains disengaged acid, it is subject to remarkable changes. Its black colour becomes yellow, or yellowish grey, and it diminishes in volume by the actual loss of part of its mass. From these observations it follows, that in order to obtain

obtain the greatest possible quantity of carburet of iron from any kind of steel, the acid must be neither too much nor too little concentrated. I have observed that the best proportion between the acid and the water was one of the former to five of the latter, and that coarse filings or turnings of steel were preferable to fine filings or pieces of considerable size.

Most of the inconveniences here mentioned do not exist with regard to the muriatic acid, at least in so evident a manner; because it is never so concentrated as the sulphuric acid, has a stronger affinity for the oxide of iron, and forms with this metal a very soluble salt. But it cannot be employed to determine, in one and the same operation, the proportions of carburet of iron and of phosphorus; because it decomposes the phosphate of iron, and renders it necessary to use a caustic alkali to precipitate it by double affinity, which may be attended with some inconvenience.

The method we have proposed to determine the quantity of carbone contained in steel, is therefore preferable to this in every respect.

SECTION VIII.

Reflections on the Means proposed by Bergman to discover the Presence of Phosphorus in Iron and Steel.

BERGMAN, in his Dissertation on the Analysis of Iron and Steel, does not speak of the presence of phosphorus or siderite, which he found only in cold short iron.

It is nevertheless very unlikely, that, among the very numerous samples of iron and steel which he examined, there should have been none which contained phosphorus, since the four steels which constitute the subject of this memoir all presented quantities very considerable, though they appeared of sufficiently good quality for the uses in the arts. I suspect, therefore, that this chemist's not having found it was owing to a want of attention to the probability of its existence, or else, that the means he made use of were not adapted to detect its presence.

The following is the method he proposes for this purpose, in his Dissertation on the cause of the brittleness of cold short iron:

He takes a bottle A, of the capacity of about 12 cubic inches, into which he puts 16 loths of crude iron, which affords the cold short iron. Upon the iron he pours six cubic inches of distilled water, and half an inch of concentrated sulphuric acid. As soon as the effervescence is ended, he filters the liquor, and receives it in another bottle B, of the same contents as the first. He washes the residue in the bottle A, until the filtration fills the bottle B. This being done, it is observed that the fluid in B, which at first was clear, becomes turbid and white. The powder does not settle until after several hours. He pours on the iron which remained in the bottle A a second quantity of water and acid equal to the first; when the effervescence is over, he pours the liquid into a third bottle C. He repeats this manœuvre a third and fourth time, &c. and collects the liquors in the vessels marked D, E, &c. The phenomena were the same in each of these operations; but the fifth solution in F remained clear for several weeks, at the end of which it formed a slight cloud. The sixth solution deposited nothing in the same space of time, though much iron remained undissolved.

I repeated this procefs on the fteels treated in this memoir, and did not difcover the leaft trace of phofphate of iron, though they would have afforded confiderable quantities by the method I have before propofed. This juftifies the affertion that Bergman did not find this fubftance in the different irons he analyfed, becaufe his method was infufficient.

I prove its infufficiency as follows:—Iron and phofphorus united together, and brought into contact with acidulated water, both tend to unite with oxygene by virtue of a predifpofing attraction between the acid which is added, and that which is formed, towards the oxide of iron, whence the refult is fulphate and phofphate of iron.

This laft is infoluble in water; but it has an affinity for acids, by virtue of which it combines, and becomes foluble in water. This affinity of the phofphate of iron for acids is fufficient to impede and even fufpend the folution of new quantities of iron; fo that it is not without difficulty, and after a length of time, that the phofphate of iron can be completely feparated from the fulphuric acid by means of metallic iron.

It happens therefore in every cafe when iron or fteel which contains phofphorus is diffolved in an acid, that the acid divides itfelf into two parts, one of which unites with the oxide of pure iron, and the other with the phofphate of iron as it is formed; and that when the acid is faturated by the oxide of iron and the phofphate of iron, the folution is almoft totally ftopped, though the liquor reddens blue vegetable colours. It may be hence inferred, that the fulphuric acid, united with the phofphate of iron, no longer exerts an equal power on the oxide of iron; and that the metal does not decompofe the water, nor unite to the oxygene of this fubftance but by its own peculiar force, which occafions a retardation or even a complete fufpenfion of the folution. Now, if in fuch a folution the phofphate of iron fhould be fmall in quantity, no fign of this metallic falt will appear, even by the addition of a great quantity of water: and this is what happens in the procefs of Bergman; whereas, by adding an alkaline carbonate to this folution until it ceafes to effervefce, the alkali unites with the fulphuric acid; and the phofphate of iron, whatever may be its quantity, falls down in the form of a white powder.

SECTION IX.

Reflections on the Insufficiency of the Methods hitherto propofed to difcover and feparate Manganefe from Iron.

BERGMAN is the firft who found manganefe in iron; and almoft every kind of iron which he examined afforded quantities more or lefs confiderable. The maximum of this quantity in fteel, according to this chemift, amounts to 0,30, and the minimum to 0,005, a latitude extremely great, and which appears fcarcely probable, as we fhall proceed to fhew.

In order to detect manganefe in iron or fteel, the profeffor of Upfal propofes two methods. The firft, having for its object to afcertain the prefence of this metal, and being fpeedy, cheap, and eafy to be performed, may be called explorative. It confifts in throwing into five parts of fufed nitrate of pot-afh, one part of filings of iron or fteel, and to bring the mafs into ftrong fufion after the detonation has taken place. If the crucible when cold exhibits towards its fuperior edge a vitreous circle of a greenifh blue colour, it is, according to him, a fure fign of the exiftence of manganefe in the iron. The other method may be

called

called determinative, becaufe it is not ufed until it has previoufly been afcertained that the iron contains manganefe, and that nothing remains but to find its quantity. It is performed by diffolving in the nitric acid a known weight of iron or of fteel, containing manganefe, and, after having evaporated the folution, by ftrongly calcining the ferruginous refidue, which is treated with more diluted nitric acid, into which a certain quantity of fugar has been put. By this means (fays Bergman) the manganefe will be diffolved, and the oxide of iron will remain untouched.

Thefe proceffes, carefully repeated, did not afford me the fame refults. I muft even fay that they are falfe, and capable of leading to erroneous conclufions. In fact, with regard to the firft, it is known that nitrate of pot-afh, decompofed in a crucible of pure earth or fine filver, leaves the pot-afh in the form of a mafs of a greenifh or blueifh colour. It is in vain to object, that this colour is owing to the manganefe contained in the pot-afh of commerce; for this alkali, when purified by alcohol, prefents the fame phenomena. And even fuppofing it to be a certain fign of the prefence of manganefe, it would alfo prove a fource of error, becaufe the nitrate of pot-afh, and pot-afh alone, exhibit this colour by fufion.

Pure iron itfelf might produce deception in this refpect; for, when it has been oxided by the nitrate of pot-afh, it combines by the affiftance of the alkali with the filiceous matter of the crucible, and forms a glafs which has often a greenifh colour. Common crucibles alfo are not without inconvenience; for they may contain the oxide of manganefe, which is well known to be often mixed with earthy matters.

The fecond method of Bergman is attended with ftill more danger of error than the firft, by fixing the opinion of the exiftence of manganefe, and producing a decifion with regard to its fuppofed quantity.

Bergman, who was perfuaded that the nitric acid mixed with fugar does not diffolve (the oxide of) iron, muft frequently have attributed appearances to manganefe which really arofe from iron only; for we have proved, that the vegetable acid which is formed in this operation diffolves a confiderable quantity.

III.

Extract of a Letter from Mr. HUMBOLDT to Mr. BLUMENBACH, containing new Experiments on the Irritation caufed by the Metals with Refpect to their different Impreffions on the Organs of Animals *.

MR. HUMBOLDT is one of the philofophers who has made the moft numerous obfervations on the phenomenon difcovered by Galvani concerning the irritability produced by the contact of different metals with the parts of animals in which the principle of life is apparently extinguifhed. As long ago as the year 1795 he obferved that the animal irritability was augmented by the oxygenated muriatic acid. Not having difcontinued his attention to this object, the perufal of the phyfiological writings of Reil, and his correfpondence with Scarpa and Volta, afforded him indications for new enquiries, of which he has occafionally had the courage to make himfelf the fubject.

* This Letter forms part of Gren's (German) Journal of Natural Philofophy for the month of October laft. The extract was read to the Inftitute of France, by Citizen Guyton.

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"In a converfation," fays he, "with M. Scarpa, at Pavia, on the effects which Galvanifm produced upon myfelf, nothing furprifed him more than the appearance of a lymphatic and ferous humour on my back. 'What can be the nature,' faid he, 'of this ftimulant, which in a few inftants changes the nature of the veffels to fuch a degree as to caufe them to prepare humours, which, the inftant they touch the epidermis, excite inflammation, and mark their courfe by a rednefs which lafts for whole hours?'" M. Humboldt promifed to repeat the experiment; and the account he gives of the facts conftitutes one of the moft interefting articles in his letter.

For this purpofe he caufed two bliftering plafters to be applied on the deltoid mufcle of both fhoulders. When the left blifter was opened, a liquor flowed out which left no other appearance on the fkin than a flight varnifh, which difappeared by wafhing. The wound was afterwards left to dry up: this precaution was neceffary, in order that the acrid humour which the Galvanic irritation would produce might not be attributed to the idiofyncrifis of the veffels. This painful operation was fcarcely commenced on the wound, by the application of zinc and filver, before the ferous humour was difcharged in abundance: its colour became vifibly dark in a few feconds, and left on the parts of the fkin where it paffed traces of a brown inflamed red. This humour having defcended towards the pit of the ftomach, and ftopped there, caufed a rednefs of more than an inch in furface. The humour, when traced along the epidermis, left ftains, which after having been wafhed appeared of a blueifh red. The inflamed places having been imprudently wafhed with cold water, increafed fo much in colour and extent, that Mr. Humboldt, as well as his phyfician Dr. Schalleru, who affifted at thefe experiments, entertained fome apprehenfion for the confequences.

Mr. Humboldt has not undertaken to determine the nature of the fluid which produces fuch aftonifhing effects; but he applies himfelf to circumfcribe the phenomena in the real circumftances which produce them. He judicioufly varies the preparations, and carefully notes all the refults; being perfuaded that the caufe of Galvanifm cannot be explored with fuccefs, but by obferving the proportions in which the chain of metals either irritates or has no effect: and to extend ftill more this vaft field of obfervation, he employs various means to raife or diminifh the irritable capacity of the animal organs.

What is the fenfation which the Galvanic irritation produces? Mr. Humboldt has difcuffed this queftion. "No one (fays he) can fpeak more decidedly on this fubject than myfelf, having made feveral experiments on my own perfon, the feat of which, in fome inftances, was the focket of a tooth which I had caufed to be extracted; in others, certain wounds which I made in my hand; and in others, the excoriations produced by four bliftering plafters." The following is his anfwer:

The Galvanic irritation is always painful, and the more fo in proportion as the irritated part is more injured, and the time of irritation more prolonged. The firft ftrokes are felt but flightly; the five or fix following are much more fenfible, and even fcarcely to be endured, until the irritated nerve becomes infenfible from continued ftimulus. The fenfation does not at all refemble that which is caufed by the electric commotion and the electric bath; it is a peculiar kind of pain, which is neither fharp, pungent, penetrating, nor by intermiffions, like that which is caufed by the electric fluid. We may diftinguifh a violent ftroke, a regular preffure, accompanied by an unintermitting glow, which is incomparably more active when

 the

the wound is covered with a plate of silver, and irritated by a rod of zinc, than when the plate of zinc is placed on the wound, and the silver pincers are used to establish the communication.

When the communication is made by the contact of the epidermis, it produces no effect; it appears to insulate like glass, when interposed between the wound and the metal: but if the skin be removed, by making two wounds at eight inches distance, and a plate of zinc be placed on one of them, and on the other a leg of a frog prepared, this last is seen to contract itself when it communicates with the zinc by the silver wire; which proves that the Galvanic fluid then passes beneath the epidermis.

This fluid produced in some circumstances a very sensible acid taste. The two wounds of Mr. Humboldt having been covered, one with silver, the other with zinc, an iron wire of several feet in length attached to the zinc, was conveyed between his upper lip and the spongy substance of the teeth, and thence to the tongue of another person. When the iron wire was made to touch the silver, a strong contraction of the scapular muscle took place, and at the same instant the person whose tongue formed part of the chain of communication perceived the sensation of acidity. There are also cases in which the fluid acts on the organs of taste without producing any sensible effect on the organs of motion: such is that where the epidermis serves as the conductor from zinc to the frog; for there is not then any contraction, but merely an acid taste on the tongue.

The author, having learned from Mr. Volta that he employed the solution of pot-ash *(oleum tartari per deliquium)* in order to augment the conducting power, availed himself successfully of this means to raise the capacity of the animal organs. He moistened one of his wounds with this liquor, which produced little pain; but the Galvanic irritation was more violent, and accompanied with more heat; sparks appeared and disappeared before his eyes; the tongue moistened with the same distinctly perceived the acid sensation, although the communication was established only between zinc and zinc. The thigh of the frog, moistened with the alkaline solution and laid upon a plate of glass, without touching either metal or carbonic matter, fell of itself into violent convulsions, the antagonist muscles of the legs and toes being incessantly agitated. Irritability has been re-established by this application in the animal parts, where it had been extinguished by warm solutions of the oxide of arsenic. Lastly, the irritation (which does not commonly take place when the nerve and the muscle are armed with the same metal, the different metals being between the coatings) becomes manifest after this preparation; which seems to indicate that the alkali not only irritates the nerve, but likewise adds to its irritability.

The author applied this method to amphibious animals, which he roused from their winter's sleep, and in which he perceived a peculiar symptom of irritability.

These observations led him to distinguish two states of the animal organ. The first, of irritability naturally or artificially raised or excited; the second, irritability in a less degree. These two states, which he calls *positive* and *negative*, are merely, as he remarks, different degrees, and not phenomena absolutely distinct from each other.

In individuals naturally sensible, the effects produced by alkaline solutions, by the oxygenated muriatic acid, by the solution of oxide of arsenic, are very rarely of the same intensity.

In the case of increased irritability, muscular motions are observed without metal or carbonic matter. They may be obtained with metals, though without communication between the nerve and the muſcle; that is to ſay, without the regular connection or chain. They may be alſo obtained by forming the chain of ſimilar metals.

Let the crural nerve of an animal naturally tenacious of life be placed upon glaſs. Let a ſmall piece of freſh muſcular fleſh be fixed on a ſtick of ſealing-wax, and then brought into contact with the crural muſcle. The reſult will be a violent convulſion at the inſtant when the chain of communication is completed. The ſame thing happens if, inſtead of the ſmall piece of muſcular fleſh, a detached piece of the crural nerve be fixed on the ſtick of ſealing-wax. The connection is therefore formed of two things, nerve and muſcular fibre. How in this ſimple caſe can the fluid which paſſes from the nerve into the muſcle cauſe it to be contracted? Mr. Humboldt thinks that it becomes ſtimulant, merely becauſe it returns from the nerve into the nerve by a foreign animal matter; that is to ſay, not organically connected with the nerve.

The diſparity of the metals forming the chain has hitherto appeared as a neceſſary condition to produce Galvanic irritation. This hypotheſis, however, is overturned by the experiments of Mr. Humboldt. If it be true that, in the ſtate of leſs irritability, there is very rarely contraction with ſimilar metals (as Volta affirms, contrary to Aldini), this circumſtance becomes indifferent in the caſe of increaſed irritability. Mr. Humboldt put into a china cup ſome mercury exactly purified; he placed the whole near a warm ſtove, in order that the entire maſs might aſſume an equal temperature: the ſurface was clear, without the appearance of oxidation, humidity, or duſt. A thigh of a frog, prepared in ſuch a manner that a crural nerve and a bundle of muſcular fibres of the ſame length hung down ſeparately, was ſuſpended by two ſilken threads above the mercury. When the nerve alone touched the ſurface of the metal, no irritation was manifeſted; but as ſoon as the muſcular bundle and the nerve touched the mercury together, they fell into convulſions ſo briſk that the ſkin was extended as in an attack of tetanus.

We ought not to be ſurpriſed at the precaution here taken by Mr. Humboldt to heat the mercury. This is required in conſequence of the opinion which he announces, that the parity of the metals does not depend on the homogeneity of their chemical conſtituent parts, but of their heat, poliſh, hardneſs, and form.

Gold, placed between two armatures of zinc, produces irritation only when the gold is moiſtened by ſome volatile fluid, or by the moiſture of reſpiration.

Laſtly, Mr. Humboldt has attempted to include all the caſes in the following formulæ:

1. In the State of increaſed Irritability.

Poſitive caſes.
- Frog—muſcular fleſh.
- Frog—zinc—zinc.
- Frog—zinc—muſcular fleſh—ſilver.
- Frog—zinc—ſilver—zinc.
- Frog—muſcular fleſh—ſilver—zinc.
- Frog—zinc—muſcular fleſh—ſilver—muſcular fleſh—zinc.

2. In the State of diminiſhed Irritability.

Poſitive caſes.
- Frog—zinc—ſilver.
- Frog—zinc—muſcular fleſh—ſilver—zinc.
- Frog—zinc—muſcular fleſh—ſilver—muſcular fleſh—ſilver—zinc.

Negative cafes. { Frog—zinc—zinc.
Frog—zinc—mufcular flefh—filver.
Frog—zinc—mufcular flefh—filver—zinc. }

Mr. Humboldt finifhes this letter by fome obfervations which he has collected in the courfe of his experiments on the *fthenic* or *afthenic* virtue of chemical agents; that is to fay, their energy or their inefficacy to produce irritation. Alkalis appear to be to the fenfible fibres what acids are to mufcular groups. The muriatic acid augments the iritability of the mufcle while it extinguifhes that of the nerves, which does not re-appear even after the acid has been faturated with alkali.

By continuing to bathe the nerve with an alkaline folution, an entire atony is at length produced by excefs of irritation; but if a few drops of muriatic acid be let fall on the part, the irritability is re-eftablifhed.

A thigh of a frog, irritated even to total relaxation by a warm folution of oxide of arfenic, has exhibited new convulfions, after having been immerfed for two minutes in a folution of pot-afh.

The fthenic virtue of the oxygenated muriatic acid is not lefs remarkable. Thighs of frogs naturally flaccid, and weakened ftill more by the Galvanic procefs for feven hours, which afforded no fign of motion when filver ferved as a conductor between zinc and the nerve, exhibited violent contractions when the nerve was moiftened with oxygenated muriatic acid. The author refers to this fubject the experiment which he publifhed in 1793, in his *Flora Fribergenfis*, by which it is afcertained that ordinary muriatic acid retards the germination of plants, but that oxygenated muriatic acid had caufed a plant to germinate in feven hours, which required thirty-eight in pure water in order to arrive at the fame development. This fact appears to him to indicate fome relation between the vegetable and animal organization.

A judgment may be formed from this extract of the number of important facts contained in this letter, and of the intereft they will excite when they fhall be collected, arranged, and amplified, in the large work which the author is preparing.

IV.

*Ufeful Notices refpecting various Objects**.—*Methods of clofing wide-mouthed Veffels—Prefervation of Gunpowder—Granulation of Shot—Precipitation of Magnefia.*

1. *Methods of clofing wide-mouthed Veffels.*

THE means of clofing the apertures of large jars and other fimilar veffels, fo as to render them air-tight, has long prefented, to perfons who are engaged in philofophical purfuits, an object of fome intereft. Thofe in particular who are in the habit of preferving animal fubftances in fpirit for anatomical or phyfiological cabinets, find that, notwithftanding all the care which can be taken, the evaporation of the liquor in which they are immerfed is a fubject which requires conftant attention. Perhaps nothing with which we are acquainted is

* The whole of the prefent article was received from my anonymous correfpondent, J. F—:—:—:—r, whofe former communications have, no doubt, engaged the attention of the reader.

more

more convenient and proper for ftopping fmall circular apertures, whofe interior furfaces are fmooth enough to admit of their ufe, than common phial-corks: for a certain time at leaft, where the contents of the veffel are not corrofive, they fit more clofely than moft of the glafs ftopples which are ufed for this purpofe. Where however the area of the aperture is confiderable, the porofity of cork and bladders, the neceffity of ufing veffels whofe mouths are elliptical, and the unavoidable irregularities in the form of the ground glafs covers, which are befides of confiderable expence, are circumftances which render the folution of this problem lefs eafy than might be wifhed. The clofenefs of texture which characterifes metallic fubftances feems to afford the beft means of removing thefe difficulties; and a quantity of an amalgam of tin is accordingly in fome mufeums fpread over the edges of the covers, by which means the mifchief is in a good meafure prevented: but perhaps no method of effecting this end is fo applicable to general purpofes, as that which confifts in the ufe of tin foil, which is alfo in frequent practice. It may be applied in two ways: a piece of bladder, foaked in warm water, having been ftretched tightly over the mouth of the jar, and tied, a piece of the thickeft tinfoil, previoufly examined by interpofing it between the eye and the fun, in order to detect the fmall fiffures which are frequently found in it, is to be laid fmoothly over it with the palm of the hand, without ftretching, and, that being alfo tied, a fecond piece of bladder again ftretched over it: or, where the veffel may require to be fometimes opened, the foil may be laid fmoothly over the furface of a bung, and, a piece of bladder being ftretched over it, the whole applied in the ufual way.

The following feems to be another effectual method of clofing a jar:—Let *a b* (Plate XII. Fig. 1.) be a fection of its mouth; *c d*, that of a circular rim of tin, A, an inch high, whofe internal diameter is half an inch larger than the exterior diameter of the mouth of the veffel; *e f*, that of a common tin cover, B, whofe diameter is a quarter of an inch lefs than that of the tin rim, A. The rim being cemented round the neck of the jar with fealing-wax, or the common electrical cement, and the interval between it and the neck filled to the depth of two-tenths or a quarter of an inch, with olive-oil *, it is clear, that if the jar be placed horizontally, and the cover B put on, it becomes hermetically fealed, except in the event of an elaftic fluid making its way through the oil. The only care neceffary will be, not to put fo much oil into the tin rim as to occafion it either to flow over or into the jar, on a change taking place in the preffure of the atmofphere. The mouth of the veffel fhould alfo be ftopped, to prevent any of the oil from being thrown into it on removing the cover.

In the laboratory, a thoufand ufes of thefe modes of preferving different fubftances muft occur; and it feems not improbable that, if they were extended to the purpofes of ordinary life, and glafs veffels fubftituted for the porous earthen-ware at prefent in ufe, feveral articles of culinary preparation, fuch as pickles, preferved fruits, potted meats, and the like, would receive lefs injury from the effects of time than they are now found to do.

* Lavoifier ufed mercury for a fomewhat fimilar purpofe, but its depth was confiderably greater. It feems, from fome experiments of Prieftley's, in which he found his gafes contaminated with atmofpheric air, though the tubes had been immerfed to the depth of an inch or more in this fluid, that, in a thin ftratum, oil would be more effectual. F.

If this conftruction, as I underftand it, be fuch as to admit the oil into contact with the refinous cement, this laft will foon become foft by the procefs of folution. To prevent this effect, I fuppofe it would be convenient to faften the rim by a mixture of plafter of Paris and white of egg. N.

2. *Prefervation*

2. *Prefervation of Gunpowder.*

GUNPOWDER, by reafon of the nitre which enters into its compofition, having been partially deprived of its water of cryftallization, and the known attraction of charcoal for humidity, is always fomewhat difpofed to deliquefce; and although it does not actually liquefy, or become unfit for fome of the purpofes to which it is applicable, yet, for thofe of the fportfman, to whom the quicknefs of its communication is of the higheft confequence, it is generally in a ftate very inferior to that in which it would be found, if a greater degree of care was taken in its prefervation. It is only when it has received but a very flight injury from damp, that the mifchief is capable of a remedy: when once it has become at all concreted, drying it will no longer reftore its power: the nitre will be found, on examination with a magnifier, to have cryftallized, and the ftrength and quicknefs of the powder are confiderably and permanently impaired; probably even before this fymptom has appeared. It is evident that no veffel is fufficiently clofe to prevent this circumftance from taking place, but fuch as is perfectly air-tight. There cannot perhaps be a much ftronger proof of the infufficiency of the packages in general ufe for this purpofe, than the opinion of a confiderable dealer in this article, to whom the matter was lately mentioned. He faid he was convinced that powder would be found to "give" in fome ftates of the weather, though the veffel which contained it was ever fo clofe: a notion which may perhaps have contributed to prevent the adoption of more effectual means. He added, that it is found to do fo in the tin canifters, as much as when packed in brown paper. The remedy is however extremely eafy. Nothing more is neceffary, than to cut off the communication with the atmofphere: any veffel in which falt of tartar can be preferved dry, will of courfe keep gunpowder in the fame ftate of perfection as when firft enclofed. For a quantity not exceeding a few pounds, which is not intended to be frequently removed from place to place, common ten or twelve-ounce phials anfwer extremely well; and if half a dozen of them be put into a cafe, there cannot perhaps be a more convenient magazine. They fhould be filled as full as poffible, and the powder well corked up at the mills, the corks being tied over with bladder and tinfoil. As, however, there might be fome danger of explofion from the accidental fracture of one of thefe, if this method were to be adopted for large quantities, it would in that cafe be neceffary to ufe fome other material than glafs; and if, inftead of the flider now inferted into the tin canifters, a turned brafs or pewter neck, like that of a common phial, and capable of being likewife ftopped with a fmall cork, were foldered into the top, they would alfo anfwer as well. A projection would perhaps render them inconvenient for package; and it would therefore be proper that the neck fhould be funk into the top; and, in order to get out the contents, that it fhould be let into a femi-cylindrical hollow in the fide of the canifter. When corked up, the top of the cork might be cut off, and the whole aperture covered with a plafter of thick drying paint, or wax and turpentine, fpread on a piece of tinfoil. None of the flafks, the beft of which are thofe of copper or tin, are fit for preferving the powder longer than whilft they are in ufe, during which the charger fhould be kept corked; a precaution the effects of which will be found confiderable.

There are fome, perhaps, who may not conceive thefe remarks to be very materially conducive to the general interefts of philofophy; but he to whom it has frequently happened to mifs an excellent crofs fhot, from his powder hanging fire—*quæque ipfe miferrima vidi*—will fcarcely confider this as the leaft important article in the September Journal.

3. *Gra-*

3. *Granulation of Shot.*

THE manufacture of common fowling ſhot conſiſts merely in cauſing the fuſed metal to fall in equal ſpherical drops into water. The lead is melted with the addition of a ſmall proportion of arſenic, which, being reduced to a metallic ſtate, by means of greaſe ſtirred in during the fuſion, renders it leſs fluid. An oblong ſhallow veſſel of iron, perhaps 10 inches wide, 14 long, and $2\frac{1}{2}$ deep, called a *card*, whoſe bottom is pierced with holes proportionate to the intended ſize of the ſhot, is placed at the height of from one to three inches, over the ſurface of a tub of water, covered with a thin film of oil. The card is previouſly heated to the temperature of the metal by immerging it in the cauldron; and a ſtratum of ſoft droſs or ſcoriæ, which are found on the ſurface of the fuſed alloy, is then placed on its perforated bottom, and, being ſlightly preſſed down with the ladle, forms a kind of filter, which partly chokes up the apertures, and prevents the metal from flowing through them in continuous ſtreams. The fuſed metal is then poured by ladlefulls into this veſſel, and appears notwithſtanding to run through it with conſiderable velocity; ſo that it ſeems difficult to believe that it falls in ſeparate drops, till convinced by taking up a quantity of ſhot from the bottom of the water.

The ſhot thus made is not without conſiderable imperfections. The exterior coat of the lower part of the drop becoming ſuddenly fixed by the contact of the water, its ſuperior portion, which is ſtill liquid, as it alſo cools and contracts, neceſſarily pits, like the ſurface of metal in the channel of a mould, ſo that the greater part of the ſhot are ſomewhat hollow and of an irregular form; conſequently too light for the purpoſe to which they are deſtined, and liable to unequal reſiſtance in their paſſage through the air. Theſe defects are remedied in the patent ſhot, the manufacture of which differs only from that of the preceding kind in the addition of a larger portion of arſenic, which varies according to the quality of the lead; *in dropping it from ſuch a height that it becomes ſolid before it enters the water*, which is from 40 to 100 feet; and in ſome ſubſequent operations, which are as follows: It is firſt dried and ſifted. It is then *boarded*, which conſiſts in ſcattering it on ſeveral poliſhed ſlabs or trays of hard wood, with rims, in the form of a Π, except that the ſides converge towards the lower part, to which a ſlight inclination and alternate motion in their own planes are given by boys employed in the manufacture. The ſhot whoſe form is imperfect are detected by the ſluggiſhneſs of their motion, and remain behind, whilſt the others roll off from the board. The laſt operation is the poliſhing; which is performed by agitating it, with the addition of a very ſmall quantity of black lead, not exceeding two ſpoonfuls to a ton, in an iron veſſel, turning on an horizontal axis, like a barrel-churn. It does not appear that any higher degree of perfection than that which is thus attained remains to be deſired. The argentine brilliancy of the ſhot when newly made, the beautiful accuracy of its form, and the curious inſtance of inanimate tactics which it preſents when ſcattered on a plate, render it even an agreeable object of contemplation.

4. *Precipitation of Magneſia.*

THE moſt ſtriking characters of common magneſia, and thoſe which are moſt relied on by a purchaſer who has not the means of analyſis at hand, as indicative of its purity, are its levity and impalpability: it is therefore a matter of ſome importance to thoſe who deal in

this

this article, that it be made to possess these qualities in as high a degree as is possible. It had long since occurred to the writer of this, that the effects which are attributed, by all who treat on the subject, to a very small quantity of other earths in the alkali which is used in the precipitation, were somewhat disproportionate to the assigned cause, and that a part of them were probably rather owing to a deficiency of carbonic acid *. An accidental piece of information which he received lately from a practical man, that magnesia was always to be obtained "beautifully light" by the addition of a small proportion of sal sodæ to the vegetable alkali employed, and a very loose experiment which he has since made with a view to this object, appear to corroborate such an idea. The magnesia contained in four ounces of Epsom-salt was precipitated with a filtered solution of common pearl-ash, washed, dried, and a portion of it then re-dissolved by vitriolic acid, and again precipitated with the same alkali, with the addition of one-fourth of carbonate of soda. The powder was certainly more light and impalpable after the second precipitation. An addition of carbonic acid to the alkaline solution employed, will probably operate in two ways: it will not only render the magnesia lighter, but in some degree actually purer, by precipitating the aluminous and siliceous earths before held in solution by the pot-ash in a more caustic state. In this respect, and in this only, perhaps, if a sufficiently small quantity of water be used, the aqua kali of the present Pharmacopœia is inferior to the oil of tartar per deliquium of the old ones. There is possibly a limit to the proportion of this ingredient, which can be admitted into the process with a due regard to economy; perfectly neutralised carbonate of magnesia being by no means insoluble. If an alkali, in an highly effervescent state, be added to a weak solution of any magnesian salt, it is well known that no precipitation whatever will take place. What remained in the supernatant liquor might, however, if thought of sufficient value, be afterwards precipitated with a caustic alkali, and reserved for calcination; or indeed would of itself subside during the subsequent evaporation for obtaining the vitriolated tartar. The best process in all respects may be easily ascertained by experiment, and the matter appears to deserve it.

V.

On the Elastic Fluid contained in the Air-Vessels of Fish.

DR. Francis Rigby Brodbelt, of Jamaica, in a letter to Dr. Duncan †, gives the following account of some observations and experiments which he has made on the gas contained in the air-bladder of the sword-fish:

"I will relate to you a few experiments which I made during my passage to this island. I had often wished to determine what is the nature of the gas which is contained in the air-bladder

* I find, on examination of the common magnesia with a deep magnifier, that its levity proceeds from an actual radiated crystallization, like that of snow: its form may be advantageously seen when just separating into flocks in a glass tube. Hence perhaps it would be improved by the addition of the alkali in very small portions, at intervals. Mere washing, by destroying the ramifications of the crystals, considerably augments the specific gravity of magnesia: an effect which would perhaps be best prevented by performing this operation carefully with distilled water, previously boiled on a portion of it more than sufficient for its saturation. F.

† Annals of Medicine, by Drs. Duncan, for 1796, p. 393.

bladder of fish; and I was perhaps prevented from finding it out, by hearing Dr. Monro in his lectures say, it was natural to suppose it fixed air. However, although this authority prevented me from putting it to the test of experiment for some time, yet one day, on our voyage, having caught a very large sword-fish, I collected the contents of all the air-bladders; for in that fish the bladder appeared divided into innumerable cells, which had no communication with each other. They afforded so much air that I collected a quart bottle full. My surprise was great to find that the gas contained was oxygene. A flame was brightened, an ignited stick was made to re-kindle, and it was so strong and pure, that the common experiment of a piece of steel wire, heated and put into it, succeeded well, and threw out a most vivid light when melting. I have committed to writing my thoughts on this subject at greater length, and I wish to infer that this pure air is to serve the purposes of life when the animal is far below the surface of the water."

The preceding discovery of oxygene is, I believe, perfectly new. Dr. Priestley, in his Experiments in Natural Philosophy abridged and methodized, vol. ii. page 462, mentions the commencement of a course of experiments on the state of the air which is contained in the bladders of fishes. He remarks, that when these are taken out of the fish, the air cannot be got from them by pressure through any existing aperture, but that he was always obliged to cut or burst them. The first time that it occurred to him to examine the air contained in these bladders, he found it in a great number of them to be not at all affected by nitrous air. But at another time he found air from the same kind of fish, namely, roaches, to be slightly affected by that re-agent. It appears, therefore, from these trials, that he seldom met with oxygene, and then in small quantity; but what the other portion of air might consist of was not ascertained by his experiments.

Fourcroy afterwards made experiments on the air contained in the air-vessel of the carp, which, at certain seasons, he affirms, may be had very cheap and in abundance at Paris. It was perfectly pure azotic gas, for the most part, though sometimes it contained a small quantity of carbonic acid gas. He thinks, from the nature of this fluid, that the air in the bladders of fishes is produced in the stomach. (Ann. de Chim. I. 47.) The observations of Dr. Brodbelt seem to render this general conclusion at least doubtful.

VI.

Account of certain remarkable Changes of Colour and Direction of the Clouds during a Thunder-Storm.

AMONG other circumstances enumerated by Dr. Priestley in the description of the clouds, in a thunder-storm, in his History of Electricity, mention is made of a certain luminous appearance, evidently independent of solar reflection. I have always supposed this expression to denote the opake whiteness of the upper or arched outline of certain thunder-clouds, contrasted with others apparently in contact with them, but of a dull leaden hue; and accordingly I was disposed to conclude that the whole was an optical delusion, arising from the position of the spectator, who imagined, though falsely, that the latter clouds were as much exposed to the sun's direct light as the former. But the storm which happened on Sunday morning, the 30th of July last, exhibited facts which seem to shew that the transition of electricity may cause the clouds to emit a steady permanent light, very different from the sudden flash called lightning.

 I was

I was called at five o'clock in the morning. The fky was then covered with clouds, not very denfe except to the fouth, and flying with great rapidity to the W. by S. or W. S. W. It lightened very frequently in the N. W. and S. W. quarters, by doubled and trebled flafhes of a bright illumination (for the actual flafh was not feen), with very loud thunder, ufually at the interval of 11 or 12 feconds after the flafh. The lower prominences, or ragged extremities of the clouds, were conftantly tinged with red, and I was informed that they had been very much redder before I got up.

At about ten minutes after five, no rain having fallen, but a few heavy drops, a fudden darknefs came on, and the duft rofe in Newman-ftreet, where I refide, beginning at the fouth end, about 250 yards diftant from my houfe, and proceeding to the north. It was very denfe, and rofe to the height of about fixty feet by eftimate, which is much higher than the houfes in the ftreet. At about a quarter paft five, the darknefs being then greateft, the houfes on the oppofite fide of the ftreet appeared as if feen through a deep blue glafs, particularly the white ftone work above the windows; and upon looking upwards, the clouds were feen of a deep leaden blue colour, and moving fwiftly in a direction precifely oppofite to that before obferved, namely, to the E. by N. or E. N. E. Soon afterwards, the lightning and thunder continuing during the whole of thefe changes, there fell a heavy fhower, which beat againft the weftern face of the houfe, and the darknefs gradually went off. At half paft five the clouds were much higher, and moved with a moderate angular motion to the north, while the fmoke of a chimney oppofite my window was gently driven to the fouth.

We have yet much to learn concerning the theory of thunder-ftorms. It is well known by experiments with the electrical doubler *, that almoft all bodies poffefs a certain degree of electrization, which is variable from a confiderable number of circumftances. It is alfo known from Franklin's experiments of the can and chain, as well as from numerous other facts, that the intenfity of the electric ftate will be augmented by diminifhing the furfaces of bodies. In this way, as well as from other caufes, it is inferred that clouds become highly electrified during the progrefs of their condenfation; fo that flafhes of lightning pafs between them and the earth, and between each other. From other obfervations and deductions, it has been alfo rendered highly probable that the long range of clouds in a ftorm of this nature does ferve as a conductor, through which flafhes of electric fire are conveyed from one part of the earth to another part in a different ftate. The facts above defcribed feem uncommon, and, if collated with other more ufual events, may afford fome inftruction concerning this clafs of phenomena. I do not highly efteem the conjectures which prefent themfelves to me on this occafion; but fhall communicate them, becaufe they tend to point out future objects of refearch.

The fingularity of this thunder-ftorm appears to have arifen from the mafs of aqueous vapour having been much too fmall to afford a favourable communication between the two oppofite ftates of electricity on the furface of the earth. If the mafs of clouds be fuppofed to have been at firft near the eaftern portion of the earth, and to have become electrified and repelled, they would, on the common principle of bodies in that ftate, be repelled, and pafs fwiftly to the weftern part of the furface to depofit their electricity as foon

* New Experiments on Electricity. By the Rev. A. Bennet, F.R.S. octavo. London.

as

as they came within the ſtriking diſtance. In this ſituation, it may be imagined that the ſupply or communication might continue to be made by the eaſtern part of the cloud acting in the manner of a point, while the weſtern part emitted flaſhes; a ſuppoſition which is rendered more probable by the conſideration that moſt thunder-clouds are ragged or pointed on one ſide, and round or ſwelled on the other; and alſo that the diſpoſition of any conductor to receive electricity without exploſion is much greater than to give ſilently, even when the terminations are alike. I conjecture, therefore, that the lightning came from the eaſt, and paſſed through the clouds to the weſt; that the poſterior extremities of the clouds were illuminated as points uſually are; that the electric motion of theſe low clouds at firſt cauſed an eaſterly wind; that ſome change in the general ſtate of electricity, or perhaps the mere exhauſted ſtate of the clouds, cauſed them to paſs rapidly back to the original reſervoir, as in the moſt common experiments of electricity; that this return produced firſt a ſtrong eddy in the lower air, which threw up the duſt, and afterwards a contrary ſtream of wind, by which the rain was beaten againſt the weſt front of the houſe. But why the clou 's ſhould have been illuminated with a red colour during their weſtern courſe, and afterwards with blue, does not ſeem deducible from any facts I know of. It may perhaps be analogous to the colours of the aurora-borealis. The contrary lower current, when the clouds were moving to the north, ſeems to have been a natural conſequence of their remoteneſs. It is probable that the returning current of the air, which muſt have been driven before the maſs of clouds when they were moving very near the earth, took place entirely in the upper part of the air, where, in that caſe, there was more room.

VII.

The Method of making excellent Bread without Yeaſt; as practiſed at Debretzin in Hungary. By* ROBERT TOWNSON, *LL. D. F. R. S. Edin.*

LIGHTER, whiter, and better-flavoured bread than that made here I never ate, nor did I ever ſee elſewhere ſuch large loaves. Were I not afraid of being accuſed of taking advantage of the privilege of travellers, I ſhould ſay they were near half a yard cubed. As this bread is made without yeaſt, about which ſuch a hue and cry is often raiſed, and with a ſubſtitute which is a dry maſs, that may be eaſily tranſported, and kept half a year or more, I think it may be of uſe to my country for me to detail the Debretzin art of making bread. The ferment is thus made: Two good handfuls of hops are boiled in four quarts of water: this is poured upon as much wheaten bran as can be well moiſtened by it: to this are added four or five pounds of leaven; when this is only warm, the maſs is well worked together to mix the different parts. This maſs is then put in a warm place for twenty-four hours, and after that it is divided into ſmall pieces, about the ſize of a hen's egg, or a ſmall orange, which are dried by being placed upon a board, and expoſed to a dry air, but not to the ſun; when dry, they are laid by for uſe, and may be kept half a year. This is the ferment, and it may be uſed in the following manner: For a baking of ſix large loaves, ſix good handfuls † of theſe balls are taken and diſſolved in ſeven or eight quarts of warm

* Travels in Hungary, 4to, London, 1797, page 242.

† I ſuppoſe broken into fragments; the balls themſelves being too large to be meaſured by handsful. N.

 water.

water. This is poured through a fieve into one end of the bread-trough, and three quarts more of warm water are poured through the fieve after it, and what remains in the fieve is well preffed out. This liquor is mixed up with fo much flour as to form a mafs of the fize of a large loaf: this is ftrewed over with flour, the fieve, with its contents, is put upon it, and then the whole is covered up warm, and left till it has rifen enough, and its furface has begun to crack: this forms the leaven. Then fifteen quarts of warm water, in which fix handfuls of falt have been diffolved, are poured through the fieve upon it, and the neceffary quantity of flour is added, and mixed and kneaded with the leaven: this is covered up warm, and left for about an hour. It is then formed into loaves, which are kept in a warm room half an hour; and after that they are put in the oven, where they remain two or three hours, according to the fize. The great advantage of this ferment is, that it may be made in great quantities at a time, and kept for ufe. Might it not on this account be ufeful on board of fhips, and likewife for armies when in the field?

VIII.

Defcription and Ufe of an Eudiometer with Sulphuret of Pot-Afh. By Citizen GUYTON *.

NATURAL philofophers and chemifts have long been defirous of poffeffing an eudiometer which might accurately fhew the quantity of oxygene mixed in any kind of gas. Citizen Berthollet has clearly fhewn, in his late leffons to the Normal School †, that the eudiometer of Scheele, which he with juftice confiders as the beft, has neverthelefs great defects; becaufe the abforption requires feveral hours, and becaufe there is a decompofition of water towards the end, which confequently difengages hydrogene gas, and renders the meafure of abforption doubtful.

This confideration induced me to feek a material which might immediately, and with convenience, afford a refult more to be depended on than thofe obtained by nitrous gas, hydrogene gas, phofphorus, and the mixture of fulphur and iron; the only fubftances which have been, as far as I know, hitherto ufed or propofed for that purpofe.

The fulphuret of pot-afh appeared to me to deferve trial in this refpect. I well knew that at the ordinary temperature it is only fufceptible of a combination ftill more flow and infenfible than the mixture of fulphur and iron moiftened; but I prefumed that by raifing the temperature, merely by the approach of a fmall taper, the action of chemical affinity might be fo much favoured as to determine rapidly an abforption which in that cafe would not be affected by any foreign circumftance.

The effect has completely juftified my conjecture; fo that nothing more remains than to defcribe the apparatus required to form this eudiometric inftrument. I thought that the reverfed retort, or inflected receiver (recipient cornu) as I have named it in the article *Air* of the Encyclopédie Methodique, I. 706, would unite fimplicity, convenience, and every defirable advantage. The experiment was made in the laboratory of the third divifion of the Polytechnic School, according to the following defcription:

* Journal de l'Ecole Polytechnique, II. 166.

† Séance des Ecoles Normales, &c. tom. v. p. 73.

A B, Plate

A B, Plate XII. Fig. 2. reprefents a very fmall glafs retort, with a long neck, its whole capacity being from 12 to 15 centilitres (between feven and nine folid inches Englifh). It muft be chofen of fuch a curvature, that, when the neck is fet upright, the bulb may form at its lower part a cavity to retain the matters introduced.

The extremity of the neck of this retort is ground with emery to enter the glafs tube CD, which is open at both ends, and about 20 or 25 centimetres in length (eight or nine inches Englifh). The retort then clofes the tube in the manner of a ground ftopper, and intercepts all external communication *.

A cylindrical glafs veffel, F, is provided, of the form of a common jar, in which the glafs tube CD may be entirely plunged beneath the level of the water.

Laftly, the fulphuret of pot-afh is prepared and broken into pieces fufficiently fmall to be introduced into the retort. Thefe are to be enclofed, dry and even hot, in a bottle for ufe.

Thefe conftitute the whole apparatus and preparation of materials.

When it is required to examine an aëriform fluid, by feparating its refpirable part, two or three pieces of the fulphuret, of the fize of a pea, are put into the retort. It is then filled with water, taking care to incline it fo that all the air may pafs out from the bulb. The orifice of the retort is then to be clofed, and inverted into the pneumatic tub, in order that the gas propofed for examination may be transferred into it in the ufual manner.

By an eafy manœuvre of alternately inclining the retort in different directions, all the water is made to flow out of the bulb in which the fulphuret remains.

When this is done, the retort is placed in the vertical fituation, and its extremity introduced into the tube of glafs CD, which muft always be under water. A fmall lighted taper is then to be placed under the bulb.

To fupport the retort in its pofition, the jar is provided with a wooden cover, in which there is a notch to receive it.

The firft impreffion of the heat dilates the gafeous fluid fo much that it defcends almoft to the bottom of the tube, which is difpofed exprefsly for its reception; otherwife the partial efcape would prevent an accurate determination of its change of bulk.

But as foon as the fulphuret begins to boil, the water quickly rifes, not only in the inferior tube, but likewife in the neck of the retort, notwithftanding the application and even the increafe of the heat.

If the fluid be abfolutely pure vital air, the abforption is total. In this cafe, to prevent the rupture of the veffel by too fudden refrigeration, the afcent of the water muft be rendered flower, either by removing the taper, or inclining the retort †; which will not pre-

* Citizen Chauffier had before conftructed, for eudiometric experiments with phofphorus, an apparatus little different from this, compofed of a long tube of a fingle piece, one end of which is bended and blown into a bulb, and has at e a projecting tube, which is clofed with a cork, after having caufed the water to rife within the tube to about one-third of its height. This inftrument may alfo be applied for experiments with the fulphuret of pot-afh. I muft obferve, however, that the practice is not fo eafy as it appears at firft fight: befides which, if the projecting tube renders it very convenient for operating on the air of the atmofphere, it is not the fame with regard to the other gafes, which cannot be introduced but by transferring them. G.

† It does not appear that inclining the retort would diminifh the rapidity of afcenfion. If the perpendicular height were increafed, by partly raifing the tube above the water, or if the aperture were partly clofed, this effect would follow. N.

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vent the absorption from continuing while any gas remains which is proper to support combustion.

If the fluid be common air, or vital air mixed with any other gas, the quantity of water which has entered the retort must be accurately measured after the cooling. It represents the volume of air absorbed. Care must be taken to enclose the remaining gas under the same pressure, by plunging the retort to the level of the line at which the enclosed water rests, before the orifice is stopped.

This operation of measuring, which is very easy when measuring vessels are at hand, may be habitually performed by a slip of paper pasted on the neck of the retort, upon which divisions are drawn from observation, and which must be covered with varnish, to defend it from the action of the water.

IX.

Description of an improved Electrometer, in which the Sensibility of the Gold-Leaf is considerably augmented, and the Intensities are distinguished by numerical Graduation.

IT is scarcely to be supposed that any philosopher who is conversant with electricity can be unacquainted with the electrometer of Bennet, in which two pendent slips of gold-leaf are substituted in the place of the pith-balls of Canton, and serve to indicate the nature and quality of very minute intensities of the electric state. There are two particulars in which this excellent instrument appears capable of improvement: the first, to render it portable, without danger to the gold-leaf, and the second, to express its various degrees of electrization by a scale of divisions.

I have reflected much on the probable means of securing the gold-leaf from fracture by carriage, but hitherto with little prospect of success. There was some hope that a single slip of this gold might be preserved in a sheath or box, with its sides very nearly in contact; but when I placed such a slip upon a gilded piece of wood of the same superficial dimensions, to which it was fastened at one end, its flexibility was such that the leaf very readily slided along the surface of the wood, and became full of folds, by inclining the fastened end a very few degrees lower than the other extremity. There was still less immediate expectation that the slips could be actually and repeatedly confined between two leaves or cushions, as in the book of the gold-beaters, without their being broken by occasional agitation. To this, however, my attention will probably be directed when I may again resume this object. In the mean time, I recommend it to other philosophers, as a very desirable improvement in the mineralogical apparatus, and should rejoice to be anticipated by their successful researches.

The weight of one slip of gold-leaf, in the electrometer of Bennet, is about one-six-hundredth part of a grain; but this, as well as the sensibility of the instrument, must vary, not only from the figure and dimensions of the piece, but the nature and thickness of the gold itself *. It seemed, therefore, unnecessary to endeavour to render two of these instruments comparable with each other. All that could be done was, to distinguish the different intensities as shewn by the divergencies of the leaf; or, as I have taken it, the distances at

* Philosophical Journal, I. 133.

which they ftrike a pair of uninfulated metallic bars. In Plate XII. Fig. 3, A reprefents the infulated metallic cap, from which, at C, depend the two narrow pointed flips of gold-leaf. BB is the glafs fhade, which ferves to fupport the cap, and defend the leaves from the motion of the furrounding air. DD are two flat radii of brafs, which open and fhut by means of one common axis, like a pair of compaffes. By a contrivance of fprings, they are difpofed to open when left at liberty; but the micrometer fcrew E ferves to draw a nut, which has two fteel bars, with a claw at the end of each, that enters into a correfpondent flit, in two fmall cylindrical pieces, to which the radii are fixed refpectively. This apparatus is feen in another pofition in Fig. 4. KL reprefents a piece of brafs, which ferves as the frame for the work, and fits the lower focket of the electrometer, FF, Fig. 3. In this the letters I H indicate the cylindrical pieces which carry the radii, and are feen from beneath. On the fide of the nut G, one of the fteel drawing pieces is feen; the other being on the oppofite fide, and confequently not vifible. Towards L appear the two re-action fprings. The other parts require no verbal defcription.

In the common conftruction of the gold-leaf electrometer, there are two pieces of tinfoil pafted on oppofite parts of the internal furface of BB; againft which the gold-leaf ftrikes when its electricity is at the maximum. If the radii DD be left at the greateft opening, our inftrument does not then differ from that in common ufe. But if the divergence produced by the contact of an atmofpheric conductor, or any other fource of electricity, be fo fmall as to render it doubtful whether the leaves be electrified or not, the radii may then be brought very gradually together by means of the fcrew, until the increafed divergency from their attractive force be fufficient to afcertain the kind of electricity poffeffed by the leaves. In this and all other cafes, the divifion on the micrometer head, which ftands oppofite the fixed index, at the time the leaves ftrike the radii, will fhew the greater or lefs degree of intenfity.

X.

The improved Procefs of Tanning. By Citizen SEGUIN *.

IN confequence of our knowledge of the multiplied refearches and important difcoveries of Citizen Seguin refpecting aftringent fubftances, and the happy application of thofe difcoveries to fimplify and perfect the art of tanning, we invited him to vifit the public laboratory, for the purpofe of communicating his proceffes and obfervations. Far different from thofe felfifh manufacturers, who carefully conceal under the cloak of pretended myftery, operations in themfelves fimple, frequently tranfmitted by oral tradition, and which have coft them neither trouble nor expence, the Citizen Seguin did not hefitate to comply with our wifhes, and has devoted to this object two fittings, at which all the pupils, and the greateft part of the inftitutors and agents of the fchool, were prefent. He not only exhibited without referve all that experience and meditation had difcovered to him, but he likewife came attended by fkilful workmen, with proper inftruments to execute all the proceffes. Laftly, he furnifhed gratuitoufly the fkins, and a confiderable quantity of tan,

* Reported by Citizen Chauffier; being part of the Tranfactions of the Polytechnic School of France. From their Journal, IV. 678.

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in order that each of the pupils might himſelf repeat the experiments, follow the detail of all the proceſſes, and render himſelf completely maſter of the method. We could have wiſhed to render an exact account of the two lectures delivered by Citizen Seguin at the ſchool; but circumſtances oblige us to confine our narration to the principal objects.

Want is the parent of the arts. Though man in the poſſeſſion of all his forces is formed to ſupport the difference of ſeaſons and climates; though the parts of his body which are ſubjected to preſſure, as in quadrupeds, are ſo diſpoſed as to acquire by habit and exerciſe a degree of denſity and compactneſs, which renders them little ſenſible to the action of foreign bodies: yet accidents, and circumſtances which it is eaſy to ſuppoſe, have determined him to ſeek the means of ſecuring his feet from the impreſſion of an unequal, flinty, or humid ſoil. The ſhepherd muſt have firſt made uſe of ſoft and flexible bark, mats, and ſimilar fabrications of different kinds; the hunter muſt have taken a piece of the freſh ſkin of ſuch animals as ſerved for his ſupport, which he muſt have faſhioned, modelled upon his foot, and retained with ſtraps *. Theſe ſimple means are ſufficient in a climate which is uſually dry; but they would be of little advantage on wet ground, or in a climate ſubject to the alterations of wet and dry weather.

Skins ſwell up, and become ſoft, by moiſture, which renders them permeable to water. Hence they are eaſily deſtroyed by the putrid proceſs which enſues, and they become dry and brittle when the moiſture is evaporated. Accident, no doubt, occaſioned the diſcovery of the means of preventing theſe inconveniences by the uſe of certain vegetable ſubſtances, particularly the bark of oak. It was ſeen that ſkins prepared with theſe ſubſtances acquired new properties; that without loſing their flexibility they became leſs permeable to water; more firm, more compact, and in ſome meaſure incapable of putrefaction. Theſe obſervations gave birth to the art of the tanner.

This art, no doubt of high antiquity, becauſe founded on one of the earlieſt wants of man in ſociety, comprehends a ſucceſſion of proceſſes which was executed by habit and imitation, without a knowledge of the eſſential objects. The preparation of ſkins accordingly required ſeveral years, and frequently, in ſpite of the care, expence, and ſlowneſs of the operation, the tanning was incomplete; the ſkin formed a ſoft and porous leather, which was ſoon deſtroyed by moiſture. Theſe defects eſſentially ſprung from ignorance of the true principles of this operation, becauſe no diſcovery had been made reſpecting the action of tan upon the ſkin, and the circumſtances or conditions which might accelerate or retard the proceſs.

To arrive at this knowledge in an accurate manner, it is neceſſary to conſider, firſt, the nature and properties of tan, and ſecondly, the ſtructure and compoſition of the ſkin.

We ſhall not enter into the detail of ſuch precautions as are requiſite in the choice of oak bark, the time and manner of ſeparating it from the tree, preſerving it, or pulveriſing it. It will be ſufficient for our object to remark, that water poured into a veſſel upon tan acquires, after ſome hours infuſion, at the common temperature of the atmoſphere, a brown

* According to the relations of travellers, theſe uſages are ſtill to be found among certain nations. Sparman affirms, that the Hottentots make their ſhoes with a piece of freſh ſkin, the edges of which they raiſe up, and tie with ſtraps; the hairy ſide is outwards. No other preparation is made than to beat and moiſten the ſkin. If it be ſtrong and thick (ſuch is the ſkin of the buffalo, for example), it is left for ſome hours in cow-dung, which renders it ſoft and flexible.

colour, an aftringent tafte, and becomes charged with the moft foluble fubftances contained in the tan; that by drawing off the water, and adding a fimilar quantity to the tan repeatedly, the whole of the foluble parts may be fucceffively extracted, the water ceafes to acquire colour, and there remains in the tub a mere fibrous matter, or parenchymatous texture, infoluble in water, and no longer adapted to promote the operation of tanning. This refidue is therefore always rejected in the manufactories as ufelefs. It is only ufed by gardeners for their hot-beds, but might probably be advantageoufly applied in the fabrication of coarfe paper.

It is therefore in the water of infufion, or the lixiviations of tan, that we muft feek for the foluble fubftances which alone are efficacious in tanning.

On examination of the water of the laft filtration, it is found to be not only clearer, lefs impregnated, and lefs acrid than the water of the firft lixiviation, but likewife that it poffeffes all the properties of the gallic acid. It reddens the infufion of tournfol, acts upon metallic folutions, and more particularly it precipitates a black fecula from fulphate of iron, &c. And it is alfo found that a piece of frefh fkin, divefted of its fat and fanguine humours, and macerated in this liquor, inftead of becoming compact, is foftened and fwells up.

The liquor of the firft lixiviation exhibits a very different character. It is more coloured and aftringent; it not only exhibits the properties of the gallic acid, by the alterations it caufes in the blue colours of vegetables, and the black precipitate it forms with the fulphate of iron; but it likewife poffeffes the remarkable quality of forming, with animal gelatin, or glue, a yellowifh abundant precipitate, infoluble in water, not putrefcible, which becomes hard and brittle by drying; and if a piece of fkin properly prepared be immerfed in this fluid, it becomes gradually more compact, and is converted into leather.

There exift, therefore, in the fame fluid, two very different fubftances: the one, which precipitates a black matter from iron, is the gallic acid or principle; the other, which precipitates animal gelatin or glue, is called the tanning principle, on account of its efficacy in the preparation of leather.

To leave no doubt on this important point, it was proved, by a number of experiments eafy to be repeated,

1. That the liquor of the laft lixiviation, though coloured, and of an aftringent tafte, affords no precipitate with glue; a fact, which feems to fhew that the gallic acid contained in the bark is lefs foluble than the tanning principle. In fact, as has already been remarked, when water is fucceffively poured on the tan, an infufion is at laft obtained which no longer precipitates glue, though it precipitates fulphate of iron very well.

2. The liquor of the firft lixiviation, after having been faturated with glue or animal gelatin, and forming an abundant precipitate with that fubftance, is entirely deprived of the tanning principle. It no longer differs from the liquor of the laft filtrations, and contains merely a portion of the gallic acid. Hence the addition of fulphate of iron affords a new precipitate with this liquor.

3. As the tanning principle has a ftrong attraction to the animal gelatin, with which it always forms an infoluble precipitate, this property affords a very convenient re agent to afcertain its prefence immediately in any fluid, and to determine with precifion its quantity. Accordingly, the infufion of tan poured into milk, whey, ferum, broth, &c. forms, with

thefe liquors, a precipitate more or lefs abundant, according to the quantity of gelatin they contain.

This peculiar property of the tanning principle affords an application which may become of great importance in the art of treating difeafes, to determine the nature of urine, and to afcertain fome of its changes. In the healthy fubject, all whofe functions are duly exercifed, the urine does not contain gelatin, nor afford a precipitate with the infufion of tan: on the contrary, in all the gaftric affections, the urine is more or lefs charged with gelatin, and forms, with the infufion of tan, a precipitate more or lefs abundant. The fame obfervation is applicable to acute and chronical difeafes, in which the affimilating or digeftive forces are troubled, deranged, or perverted.

4. The gallic acid, or, if other terms be preferred, the principle which precipitates the fulphate of iron, is often found alone, or at leaft without being accompanied by the tanning principle. Thus quinquina, crude or torrefied coffee, the roots of the ftrawberry-plant, fcrofularia, milfoil, arnica, the flowers of Roman camomile, and all the multitude of plants vaguely comprifed under the title of aftringents, contain the gallic acid only. All thefe form with the fulphate of iron a precipitate more or lefs coloured and abundant; but none of them produce the flighteft change in the folution of animal glue. On the contrary, the tanning principle has never been found alone, but always united or combined with the gallic principle. It was long fuppofed to exift exclufively in the oak, the nut-gall, and fumac, the only fubftances ufed at the tan-works; but it is found more or lefs abundantly in the filiquaftrum, the rofe-tree, the larix, feveral fpecies of pines, the acacias, the lotus, the fquill, the roots of biftort, of rhubarb, of parella, and feveral other plants, of which we fhall hereafter give a lift. We have alfo found this principle in the products of diftillation of different vegetable fubftances, where it was in fome meafure formed during the operation.

From thefe different confiderations, founded on experiment, the following general principles may be deduced: 1. Every fubftance of which the infufion is capable of precipitating animal jelly, poffeffes the tanning property. 2. Every fubftance which poffeffes the tanning property, likewife precipitates the fulphate of iron black. 3. Every fubftance which precipitates the fulphate of iron, but not the folution of glue, does not poffefs the tanning property *.

We fhall difmifs the confideration of the tanning principle, by fhewing fome of the remarkable changes to which its infufion is fubject.

1. A few days after its preparation a yellowifh precipitate is fpontaneoufly formed, which is more abundant in proportion as the liquor is more faturated, and the time of infufion longer. This precipitate, when feparated, is converted by drying into a very fine, light, afh-coloured powder, totally infoluble in water, oil, alcohol, and ether, even at the temperature of ebullition. When thrown on burning coals, it readily takes fire, and emits a thick fmoke. By deftructive diftillation it affords a confiderable quantity of carbonic acid gas, an aqueous acid, and a brownifh oil, leaving in the retort a light fpongy coal.

* The folution of glue is a convenient re-agent to afcertain the prefence of the tanning principle. It is therefore advantageous to have it in readinefs in the laboratory. To prevent the fpeedy putrefaction to which animal fubftances are liable, we have added one-twentieth part of alcohol, which anfwers the purpofe very well, without altering the properties of the fluid. C.

As this precipitate by its insolubility approaches to the nature of wood, and as it falls down even when the lixivium of tan is preserved in well closed vessels entirely filled, it might be supposed that it is merely a gradual and successive deposition of the remains of the woody and parenchymatous parts of the bark disseminated and suspended in the fluid; it might be imagined that the contact of air has nothing to do with this phenomenon. But we must observe, that this precipitate is found in the most limpid solution, even after it has been filtered; that it is most abundant when the infusion has been prepared from bark ground long before, and preserved without care; and that it is more readily deposited when the liquor presents a great surface to the contact of the air. In this case the surface of the fluid becomes tarnished by an extremely fine pellicle, which covers it, and becomes thicker and firmer the longer the fluid remains thus exposed and at rest. If it be divided by agitating the liquor, it is again renewed, and in this manner produces the precipitate we speak of. We may determine, in some respect, at pleasure, the formation of this pellicle and precipitate, by exposing the liquor to the contact of oxygene gas, or by pouring into it the oxygenated muriatic acid. It is sufficient even to bring a bottle containing the oxygenated muriatic acid into the vicinity of the infusion of tan. A pellicle is instantly formed, resembling a slight gauze, which floats and spreads instantly over the whole surface, assuming in a regular order the most beautiful colours of the iris, and at last becomes brownish. If the experiment be continued for a few minutes, the pellicle thickens, and the precipitate begins to fall. The formation of this precipitate, and the properties it acquires, depend, therefore, on the combination of oxygene, which operates either during the infusion and filtration of the lixivium, or after its having been drawn off clear with the contact of atmospheric oxygene. Hence are shewn the necessity and advantage of using tan recently prepared *, and of preserving the tan as well as the infusions defended from the contact of the air.

These phenomena are common to all the preparations of vegetables made by means of water, whether by infusion, decoction, or even distillation. We have seen these precipitates gradually formed in the infusion or decoction of nut-gall, sumac, gentian, quinquina, and even of the most insipid as well as of the most aromatic and acrid plants. The expressed juice of fresh plants, such as hemlock, sorrel, anil, &c. presents the same phenomena, and affords, more particularly when the action of air is promoted by agitation, a coloured pulverulent deposition which has long been distinguished by the name of fecula. Lastly, all the distilled waters which are kept for several years become turbid, by the formation of filaments and whitish flocks, more or less abundant, but always insoluble. It is to a combination of oxygene, as Fourcroy in his excellent analysis of quinquina has shewn, that we ought to attribute the formation of these precipitates, the insolubility they acquire, and their approach to the nature of the ligneous fibre.

Nevertheless, the spontaneous precipitation which is effected by the contact of atmospheric oxygene in the lixivium of tan does not change its properties, at least in any sensible manner; and as in a lixivium considerably saturated the precipitate is most abundant on the first days, it might perhaps be of advantage to wait until this first precipitate was formed before

* These observations are applicable to all vegetable substances which are reduced to powder. They not only lose their peculiar aroma, but likewise change their nature by the contact of light and atmospheric-air. Their properties being thus considerably altered, the dose at which it is proper to employ them becomes uncertain. The physician ought not therefore to prescribe in his formulæ any powders but such as are recently prepared. C.

immersing the skins. For this substance, when deposited, attaches itself to the surface of the skins, where it forms a coating more or less thick, which soils them, closes their pores, and retards in a small degree the direct action of the tan.

2. The lixivium of tan, particularly when highly concentrated, and prepared in hot weather, acquires after a few days a vinous smell, which seems to announce a commencement of fermentation, and might lead to an apprehension that its progress might impair the tanning properties. In order to ascertain how far these notions might be well founded, different vegetable acids were mixed with the tanning lixivium, but none of them sensibly altered its properties. And still more we may add, that for near two years a bottle of the infusion of tan has been kept in the laboratory, and, in spite of the alterations of temperature to which it has been exposed, and the change necessarily produced by the contact of air, its properties appear the same, and it is still daily used with advantage as a re-agent to ascertain the presence of gelatin.

As a knowledge of the properties of tan affords observations of importance, which constitute the basis of Seguin's method, we have not been apprehensive of dwelling too long on the subject; but we shall confine our remarks on the structure and chemical composition of the skin to a few general considerations.

This membrane, as is shewn by anatomists, is essentially formed of a great number of laminæ or fibres, which are white, broad, short, and closely adherent, but interwoven, and disposed in different directions, so as to leave between them an infinity of small spaces or pores. From this construction it exhibits a dense but soft spongy texture, and is susceptible of extension and contraction. In the midst of this substance are found a great number of nerves and vessels of different kinds, the very fine and multiplied ramifications of which serve to support and maintain the lamellated structure of the skin, and convey into the vacuities between the fibres those fluids which serve for the support and nutrition of this membrane. The external part is the epidermis. This thin transparent membrane is of a very different texture. It contains neither the apparatus of vessels, nor the disposition of fibres or laminæ, which by their intersection form a kind of spongy net-work. It is in some measure a simple uniform covering, which presents no distinct organization, but adheres strongly to the skin, entering into its folds and numerous porosities. Lastly, the hairs are implanted in the skin by a sort of bulb or oval root, interspersed with small vessels filled with a kind of mucus.

The composition of the skin, the changes it undergoes by different preparations, by chemical agents, and the properties it acquires in these several states, present other considerations.

If the fresh skin of an adult animal be macerated for some hours in water at the temperature of the atmosphere, and if, to accelerate the effect, agitation and pressure be used, a separation is made of the blood, the juices, and the different soluble substances contained in the vessels or vacuities; and by examining the nature of the matter thus extracted, it is found that a small portion only consists of gelatin, which has little consistence or tenacity. Subsequent macerations, at the same temperature, afford no more gelatin, the fibrous texture remains insoluble, and undergoes no further loss. It therefore appears, that in the natural state the skin contains but a small portion of gelatin perfectly formed, and soluble. It may even be apprehended, that the small quantity obtained by the first maceration was merely included in the pores of the skin adherent to the surface of the fibres; and that the vital force had not yet time to assimilate and convert it to the state of fibre.

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But if the temperature be raifed to ebullition the fibrous texture is altered, becomes foft, and fucceffively diffolves. It affumes the character of gelatin, and may be entirely converted into a vifcid and tenacious glue. The fibre which forms the folid ftructure of the fkin does not therefore effentially differ from gelatin, but in its texture, its concretion, and its infolubility in cold water; and as it is obferved that fubftances capable of abforbing oxygene deprive the fibrous matter of its folidity, and haften and determine its converfion into gelatin, we are authorifed to conclude that thefe diftinct qualities of the fibre depend only upon a proportion of oxygene which the vital action and progrefs of life combine with the gelatin. Multiplied experiments appear to leave no doubt on this point. "They have proved to me (fays Seguin) that the fibre is oxygenated glue, which in that ftate cannot combine with the tanning principle, but which acquires that property by paffing to the ftate of gelatin in confequence of the lofs of a portion of oxygene."

The effential point in the operation of tanning is therefore to afcertain, and to direct in a precife and invariable manner, the circumftances and conditions which determine the tranfition of fibre to the ftate of gelatin, and to feize the inftant in the procefs to effect a proper combination with the tanning principle. This object is too important not to recall fome of the obfervations made by Seguin.

Glue, as has before been remarked, poffeffes a great attraction for the tanning principle. It immediately forms with this fubftance an infoluble matter, which is not fubject to putrefaction; but it muft be well remarked that the precipitate is dry and brittle.

Hence it is evident that a fkin, the pores of which might contain gelatin ready formed, or of which the fibrous texture fhould have been altered and converted by certain preparations to the gelatinous ftate, would tan very fpeedily, but would afford a kind of leather which would be harfh, brittle, and difpofed to crack or peel in the wear. On the contrary, the operation will be long, incomplete, and will afford only a foft, fpongy, and putrefcible leather, if the fibre preferves its ftate of oxygenation, if it do not pafs by fucceffive degrees into the ftate of gelatin, or if the tanning principle do not penetrate its thicknefs in proportion to the converfion of the fibre into glue.

Thefe inconveniences are avoided, and the propofed object is obtained with precifion, by the ufe of the lixivium of tan. This fluid has been fhewn to contain two very different principles, of which the union and action are alike neceffary for the fuccefs of the operation. On the one fide the gallic principle, which, as is known, readily feizes the oxygene of metallic folutions, and reduces them or brings them nearer to the metallic ftate, acts nearly in the fame manner upon the fibre; it unburns it, or deprives it of oxygene, and converts it into the ftate of gelatin. On the other hand, the tanning principle, which alfo exifts in the folution, exercifes its action as foon as the fibre is fufficiently reduced to the ftate of gelatin. The tanning procefs is not, therefore, inftantly effected, like the precipitation of animal glue; but it operates gradually, and in fucceffion, by ftrata from the furfaces of the fkin to the centre; and as the action of the tanning principle immediately follows that of the gallic acid, the fibre is furprifed in its pofition, the felted texture of the fkin is totally preferved, and its compofition alone is changed. Accordingly, the leather prepared by this procefs has the advantage of being fupple, flexible, infoluble, imputrefcible, more durable, and lefs difpofed than any other to imbibe humidity.

The new method of Seguin for the preparation of leather is founded upon this feries of obfervations obſervations and experiments. We ſhall not in this place enter into the detail of the changes and improvements he has made in the different branches of the tanning art; but we muſt add, that the proceſſes have been repeated in all the laboratories of the Polytechnic School, by the greateſt number of the pupils; that the operations on different kinds of ſkins were all concluded in eight, ten, twelve, or fourteen days at moſt; that the leather thus manufactured was completely ſaturated with tan, and of a quality ſuperior to the leather prepared by the old method which is to be met with in the market; and laſtly, that to theſe advantages the method of Seguin unites ſimplicity, facility, and certainty of ſucceſs. A diſcovery of this importance, in an art ſo neceſſary to our wants, entitles the author to the eſteem and gratitude of the public; and theſe ſentiments are more eſpecially due to the citizen who ſacrifices his individual intereſt to the general proſperity, and is deſirous of communicating the fruits of his reſearches. The advantage of public utility conſtitutes his recompenſe, and enables him to diſregard the clamours which envy, ignorance, and the prejudice of old habits never fail to raiſe againſt every uſeful innovation.

After the two ſittings employed by Citizen Seguin to explain the nature and properties of tan, the order adopted in the courſe of vegetable chemiſtry was reſumed in the ſchool. The alkalis, the ligneous part, and the colouring matter were ſucceſſively examined; the laſt of which naturally led to an explanation of the principles of the art of dyeing, the different proceſſes for fixing colours, and aſcertaining their qualities. In treating of theſe different objects, ſome conſiderations were preſented reſpecting the new properties which ſtuffs acquire by tinctorial proceſſes.

The proceſs of dyeing ought not to afford ſimply a colour to pleaſe the eye, but ſhould effect a kind of colouring tannage capable of adding to the properties of the web, and to render it leſs ſoluble or putreſcent. This object is generally neglected or little known, but it cannot be indifferent. It is not only of importance with regard to economy, but we do not heſitate to ſay, equally ſo for the preſervation of health. This may eaſily be conceived if attention be paid to the circumſtances, that cloths of the ſame kind and fabric differ conſiderably according to the dye; that ſome are dry, brittle, of little durability, not adapted to preſerve or retain caloric; others are ſoft, ſpongy, capable of retaining damps and exhalations of every kind; and by the ſucceſſive action of light, air, and caloric, may either undergo a ſort of oxidation that deſtroys their texture, or elſe paſs to a kind of putrefaction, which forms a continual atmoſphere round the body, and more or leſs affects the health. It is in the midſt of armies under tents, in camps, where a great number of men are expoſed to the ſame kinds of fatigue and intemperance, and differ only in the colour of their clothing, that we may aſcertain the truth of theſe obſervations. It is in the magazines of the military hoſpitals, where the clothing of men is depoſited, that a very marked difference in the ſmell and poroſity of the ſtuffs is obſerved, according to the colour with which they are charged.

After a ſucceſſive examination of the different products of vegetables, the attention of the ſchool was directed to the alterations they undergo after death, by the action of light, caloric, air and water. Theſe ſubjects led to the theory of the vinous and acetous fermentations, and the production of alcohol and ether; and laſtly, the courſe was terminated by an examination of the phenomena of putrefaction, which reſtores to the atmoſphere and to the earth thoſe principles which the action of vitality had before extracted.

During

During the concluding months of this courſe, the pupils continued to operate in the particular laboratories appointed for their uſe, either in repeating the principal experiments which had been exhibited in the lectures, or in making others which had been pointed out or imagined by themſelves. Their attention was principally directed to the application of chemical knowledge to the progreſs of arts and manufactures. With this view they ſucceſſively prepared different kinds of ſoap, varniſhes, and pigments, and they executed in ſmall the different proceſſes of dyeing, tanning, &c. Some of the pupils attended particularly to the experimental reſearch of ſubſtances proper to form the oxalic acid; others engaged in the experimental enquiry after thoſe vegetables which contain the tanning principle, and might be uſed as a ſubſtitute for oak bark. From theſe reſearches a table has been formed, which is already very ample, wherein thoſe vegetables which afford a precipitate with the ſulphate of iron alone, are diſtinguiſhed from thoſe which afford a depoſition with the ſolution of glue. This table is already very intereſting; but it may eaſily be imagined that it ſtill preſents vacuities to be filled up by time and experiments. This will form the object of the work of another year; and when it ſhall be finiſhed, we ſhall haſten to preſent the labours of the pupils of the ſchool.

XI.

The Combuſtion of Phoſphorus in the Vacuum of the Air Pump. By DR. MARTINUS VAN MARUM.

[Concluded from page 237.]

BEING deſirous of aſcertaining the cauſe of this ſingular inflammation in air ſo rarefied, I put, on another day, a ſmall ſtick of phoſphorus, wrapped in the ſame manner in cotton powdered with reſin, under a receiver containing about four hundred cubic inches, on the plate of the air pump, in order to obſerve the phenomenon with accuracy a ſecond time. The temperature of the place where I made the experiment was nearly the ſame as on the former occaſion, namely 56 degrees of Fahrenheit's ſcale. To obſerve the degree of rarefaction more eaſily, I placed a ſhort barometer gauge under the receiver. When the air was rarefied ſo that the mercury was ſupported in the gauge to the height of about an inch, the light began to be enlarged on the ſurface of the phoſphorus, chiefly at the upper part of the ſmall cylinder. This light increaſed by degrees during the ſubſequent exhauſtion, and the inflammation took place when the mercury ſtood at the elevation of 5 lines.

The flame was much paler and weaker than is afforded by phoſphorus when burned in an atmoſphere of the uſual denſity. I obſerved the flame to become weaker and weaker, and about two minutes afterwards the phoſphorus ceaſed to exhibit any light.

IV. To aſcertain whether the cotton powdered with reſin, which was wrapped round the phoſphorus, might be the cauſe which gave place to the inflammation, I placed under the ſame receiver two ſmall ſticks of phoſphorus of the ſame ſize, one of which only was wrapped in the powdered cotton. Theſe two ſmall pieces began to ſhine at the ſame time, when the mercury ſtood at the height of about an inch in the ſhort barometer gauge. Nevertheleſs, the piece which was ſurrounded by the cotton and powdered reſin alone took fire when the rarefaction was more advanced.

I then

I then thought there was some reason to suppose that the resin might be the cause of the inflammation; and to determine this point, I put under the same receiver three similar small sticks of phosphorus, one of them simply powdered with resin, another wrapped in cotton without resin, and the third wrapped in cotton powdered with resin, in the same manner as in the former experiments. The light began to increase at the same time in all three, namely, as soon as the mercury had fallen to about one inch in the barometer. The phosphorus wrapped in cotton powdered with resin took fire first; a short time afterwards that which was wrapped simply in cotton took fire; and that alone which was simply powdered with resin did not take fire at all.

V. After these results, the question was, to know in what manner the cotton could cause inflammation in air so rarefied, in which every other combustible, though set on fire, would cease to burn; and still farther, how phosphorus could spontaneously take fire when the experiment was made in an atmosphere where the temperature does not exceed 56 or 58 degrees of Fahrenheit, though the phosphorus does not take fire in the open air unless heated to about 112 degrees of Fahrenheit. After some reflections and experiments, I found that this singular phenomenon could be very easily explained according to the principles of the modern chemistry. The increase of the light which precedes the inflammation when the air is rarefied to a certain degree, suggested to me this explanation. I shall therefore, in the first place, explain the evident cause of the increase of light in the phosphorus in the rarefied air.

VI. Exhalations continually rise from the surface of phosphorus when exposed to the atmosphere, as is proved by its speedy dissipation in that circumstance: but as soon as the air is rarefied to a certain degree, the exhalations cannot rise; for these particles will not be elevated but during the time that the surrounding air is heavier than themselves. Whenever, therefore, the air is rarefied to this degree, the exhalations must remain, and surround the phosphorus from which they came. The union of oxygene with these phosphoric exhalations must be then made only in the vicinity of the phosphorus, whence the light from the disengaged caloric must be seen there only. It is evident that this light will be much stronger when the phosphoric exhalations do not rise, because the disengagement of the same quantity of caloric is then made in a more confined space than when the phosphoric exhalations could rise and be dispersed in the receiver.

VII. The caloric which is disengaged from the oxygene, and is seen in the rarefied air round the phosphorus in the form of a stronger light than ordinary, must, also, on account of its greater density, give heat to the phosphorus. Hence may be clearly seen the reason of the combustion of phosphorus on rarefying the air when it is surrounded with cotton, as before described. Woollen and cotton stuffs have the property of preventing the dispersion of caloric *. The caloric which is disengaged round the phosphorus in the rarefied air is thus retained by the cotton, until its accumulation on the surface of the phosphorus becomes at length sufficient to set it on fire. When a piece of phosphorus is not enveloped in cotton or some similar substance, it does not take fire in the rarefied air, because the caloric which is disengaged near the phosphorus is so speedily dispersed when it is not arrested by the cotton, that the phosphorus cannot acquire the degree of inflammation necessary to inflame it.

* Because they impede the circulation of the air, by which the heat would else be conducted off. N.

VIII.

VIII. Though this explanation appeared to me to be very evidently founded upon the knowledge we already poſſeſs, I was nevertheleſs deſirous of ſhewing the truth by a direct experiment. I endeavoured to aſcertain, by means of a thermometer, that the temperature near the ſurface of the phoſphorus which is wrapped in cotton, is more elevated before the inflammation, when the light is perceptibly ſtronger. For this purpoſe, I uſed a thermometer, the bulb of which was about one quarter of an inch in diameter. I faſtened the cotton with which the ſmall ſtick of phoſphorus was ſurrounded, as in the preceding experiments, to the ball of this thermometer, ſo that it was entirely ſurrounded with it, at the ſame time that its diſtance from the ſurface of the phoſphorus was about half a line. The reſult of this experiment anſwered my expectations. I obſerved that the mercury roſe, after the light had increaſed, and that from 52 degrees, which was the original temperature, it had riſen to 67 of Fahrenheit before the phoſphorus had taken fire.

The ſize of the ball of this thermometer appeared to me to be the cauſe why the mercury did not riſe higher before inflammation. I had alſo admitted ſome diſtance between the bulb and the phoſphorus, in order that its contact might not prevent the phoſphorus from acquiring the requiſite degree of heat which is neceſſary for its inflammation. I therefore reſolved to repeat the experiment with a thermometer of the ſame kind as was uſed by Dr. Hunter in his obſervations on the heat of animals and plants, the ball of which was not more than one line in diameter. I faſtened cotton to this in the ſame manner, which was attached to a ſmall ſtick of phoſphorus; but the ball of the thermometer was placed in contact with one of the ends of the ſtick of phoſphorus, which was half a line in diameter, and four lines in length. I then ſaw that while the light round the phoſphorus became ſtronger and ſtronger, the thermometer roſe from 46 to 76 degrees before the phoſphorus took fire. The ſudden heat broke the ball of the thermometer; which prevented my uſing ſimilar thermometers in the repetition of this experiment.

Though this thermometer did not mark the degree of heat which was neceſſary to ſet fire to phoſphorus in the open air, it is ſeen, nevertheleſs, from the experiment, that the temperature riſes very conſiderably at the ſurface of the phoſphorus before it takes fire. The ball of the thermometer which I uſed for this experiment had its glaſs extraordinarily thick at bottom, which was probably one of the reaſons why the thermometer did not indicate an higher temperature before the inflammation, as there is no reaſon to ſuppoſe that phoſphorus will take fire in rarefied air at a leſs temperature than in the atmoſphere *.

IX. Laſtly, I examined whether the air could be rarefied to ſuch a degree that phoſphorus ſhould be incapable of taking fire in it. I ſaw it take fire in air ſo rarefied that the mercury in the gauge ſtood at the height of one ſingle line only. The phenomenon here deſcribed is hitherto, as far as I know, the only example of a true inflammation in air rarefied to the higheſt poſſible degree by the air pump. This fact, however, does in no reſpect prove that a true inflammation can take place in vacuo.

When the mercury in the gauge placed beneath the receiver ſtands at the height of one

* As the ſolidity of the ball of the thermometer was about half a cubic line, and the phoſphorus little more than three quarters, it ſhould ſeem that, as the heat was ſhared between the mercury and the phoſphoric vapour, the thermometer could not be expected to riſe to the point of combuſtion unleſs it had remained unbroken for a longer time. N

 line,

line, which indicates the utmost degree of rarefaction of the air which I have ever observed to take place, the rarefied air still possesses 1-300th of the density of the air of the atmosphere, in case the mercury in the barometer be supposed at 30 inches elevation.

It is certainly very singular, that the small quantity of oxygene gas which remains in air so rarefied should be sufficient for the inflammation of phosphorus; more especially as all other combustible substances are extinguished in air rarefied to a much lower degree. I have already explained the principal reason. There is another circumstance which probably favours this inflammation, of which I shall speak hereafter, when I shall have made some experiments.

X. The phenomenon exhibited by the phosphorus in rarefied air, as here described, is certainly a real combustion, as is proved by the very remarkable diminution of weight of the phosphorus after the experiment has been made in a large receiver, or has been several times repeated. The phosphoric acid produced by the combination of oxygene with the substance burned is also found on the plate of the air pump.

XI. The combustion of phosphorus in air highly rarefied is accompanied with several very singular phenomena, which I shall here mention.

1. The phosphorus usually emits, a short time after the commencement of the inflammation, fiery jets, in the form of small ignited balls, which are dispersed on all sides in the receiver, and exhibit a very curious and surprising appearance. I have not hitherto been able to explain this phenomenon.

2. The flame which surrounds the phosphorus when it burns in the rarefied air, extends farther and farther in a globular form. Its light at the same time becomes paler and paler, and at length disappears. This enlargement of the flame, and diminution of the light, are probably to be attributed to the oxygene, to which the phosphoric exhalations may unite, being gradually exhausted. The light at last entirely disappears, when all the oxygene gas which existed in the rarefied air, and could be reached by the phosphoric exhalations, is combined with them; for, as soon as there is no more oxygene to which the phosphoric exhalations may unite, there ceases to be any further separation of caloric, which causes the flame and the illumination.

3. When a small quantity of atmospheric air or oxygene gas is suffered to enter the upper part of the receiver by a cock after the combustion or light of the phosphorus has disappeared, a pale light is then seen to disperse itself through the whole capacity of the receiver. This light must certainly be attributed to the combination of the oxygene, of the gas which has entered, with the phosphoric exhalations which exist in the receiver. The exhalations which the phosphorus emits after it has been heated by the inflammation, are apparently more subtle and light than those which preceded that phenomenon; and it is probably owing to this reason that they can support themselves in the gas which remained in the receiver, though very highly rarefied.

4. When the apparatus is left untouched for some time after the combustion of the phosphorus in the rarefied air has ceased, and the whole has become cooled, then the phosphoric exhalations descend in this rarefied air. In proof of this, when the experiment is made in a receiver, on the plate of the air pump, and air is suffered to enter by a cock beneath the plate, as usual, it is then seen that the light occasioned by the entrance of the air takes place only near the plate upon which the phosphoric exhalations have fallen.

XII.

XII. When the atmospheric air or oxygene gas enters the receiver by a cock from above, a little before or the instant after the light of the combustion has disappeared, and when care is taken to admit no more air or gas than shall be sufficient to raise the gauge one or two lines, the phosphorus usually takes fire a second time. In this manner the phenomenon may be made to appear repeatedly; and accordingly as a greater or less quantity of air is suffered to enter, the circumstances are found to vary in a very remarkable manner. This communication would be too long if I were to attempt to describe the variety of appearances which I have observed in these experiments. They are not entirely the same, in circumstances which appear perfectly similar. I have observed that pieces of phosphorus obtained by different operations have exhibited different phenomena. The fiery globules emitted by the burning phosphorus are in some cases larger and more numerous than in others.

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

QUESTION V.* *Answered by the PROPOSER.*

LET x denote the weight of the salt, and s its specific gravity; w the weight of the bottle of distilled water, and W that of the solution; the magnitude, or content, of the bottle, and the specific gravity of the water, being each considered as 1.

Then, since the magnitudes of bodies of the same kind are as their weights, $w : W - x ::$ $1 : \frac{W - x}{w}$ = magnitude of the aqueous part of the solution: and, because the magnitudes of bodies of different kinds are as their weights divided by their specific gravities, $\frac{w}{1} : \frac{x}{s}$ $:: 1 : \frac{x}{ws}$ = magnitude of the salt. Whence $\frac{W - x}{w} + \frac{x}{ws} = 1$; from which equation x is found $= \frac{s}{s - 1}(W - w)$ = weight of the salt. And since $s = 2.8$, $W = 2810$ gr. and $w = 2506$ gr. by the question, we shall have $x = \frac{2.8}{2.8 - 1}(2810 - 2506) = \frac{2.8}{1.8} \times 304$ $= 472\frac{8}{9}$ gr. the quantity of salt required.

And if the case be reversed, by supposing x to be known, the value of s will be readily found from the general formula; by which means we shall be enabled to make a proper allowance for the difference in the specific gravity of the salt, arising from chemical condensation, as was done in the proposing of the question, where it is taken as 2.8 instead of $2\frac{1}{2}$, which it is supposed to be in its separate state.

QUESTION VI. *Answered by C. W.*

LET m be put for the mean temperature at the equator, n for the difference between this temperature and that of the north pole, and l for the latitude of any place, radius being 1; then the mean temperature of that latitude will be $m - n \times \text{sine}^2 l$. For the heat

 produced

produced by the direct action of the sun, is as the sine of the sun's altitude; so that if the earth were equally heated at the equator and poles, the quantity to be added to the polar heat would be $n \times$ sine of the sun's altitude, radius being 1. But it has been ascertained from observation, that the heat on the earth's surface *(cæteris paribus)*, and consequently the heat emitted by the earth itself, is always proportional to the direct solar heat; whence the quantity to be added must be $n \times$ the square of the sine of the sun's altitude: or, since the mean annual altitude of the sun, in any latitude, is equal to the complement of that latitude, the quantity to be added to the polar heat $(m - n)$ will be $n \times$ cos. $^2 l$; but cos. $l = \sqrt{1 - \text{sine}^2 l}$; consequently, $m - n + n \times$ cos. $^2 l = m - n + n - n \times$ sine $^2 l$ $= m - n \times$ sine $^2 l$. Hence, if the mean temperature of any two latitudes be known, the temperature under the equator, at the pole, and in every intermediate latitude, may be readily ascertained.

The mean temperature of lat. 40° is found to be 62.1, and of lat. 50° to be 52.9; but the square of the sine of 40° is ·413, and the square of the sine of 50° is ·586; whence

$$m - \cdot413n = 62\cdot1.$$
$$m - \cdot586n = 52\cdot9;$$

From which equations n is found $= 53$ nearly, and $m = 84$. So that, the mean annual temperature at the equator being 84°, that of the pole must be $84 - 53 = 31°$; and the mean annual temperature of every intermediate latitude will be $84 - 53 \times$ the square of the sine of that latitude.

NEW MATHEMATICAL QUESTION.

QUESTION X. *By W. THOMSON.*

IT is required to find at what time of the longest day it is the hottest in London, supposing the heat to be as the sine of the sun's altitude, and the time of its continuance above the horizon.

PHILOSOPHICAL QUESTION.

THERE is an optical appearance so frequent, that it is rather surprising that writers on that science have never mentioned it. Whenever the sun shines upon agitated water not absolutely loaded with opake matter, and the spectator is so placed that the shadow of his head may be projected upon the surface of the fluid, he will see an innumerable quantity of divergent rays within the water, of which that shadow is the centre. They are incessantly shifting their place laterally; and if more persons than one are present, each sees a system of radiations or glory round his own head, but no such appearance round the shadows of the other persons, though these also are very visible to him. As this phenomenon has a striking effect when observed, and may be accounted for upon the common principles of optics and perspective, it is offered to correspondents as an object for explanation.

To

To Mr. NICHOLSON.

SIR, *London*, 22*d Auguſt* 1797.

UPON peruſing the account given in your laſt number, of the phenomenon of the Fata Morgana, and upon conſulting the plate annexed by way of explanation, I was very much puzzled to comprehend how the ſpectator, " placed on an eminence of the city, with his back to the riſing ſun, and his face to the ſea," looking of courſe towards the weſt, could at once enjoy the ſight of this truly admirable phenomenon, a diſtant view of the *weſtern* aſpect of the city, and alſo of the mountains *behind* him. A few words in a future number, in explanation of this ſtrange combination, will much oblige,

Sir,

Your conſtant reader,

DAVUS.

⁎ THE difficulty ſtated by this correſpondent, together with ſeveral others, engaged my attention at the time the account (p. 225) was drawn up: but as they ſeemed ſcarcely capable of being cleared up by reaſoning, while the theory remained ſo very uncertain, I thought it beſt to avoid entering the ample field of conjecture which offered itſelf. It ſeems altogether improbable, that the rays of light ſhould be *reflected* immediately back to the city, and every part of the drawing directly contradicts this ſuppoſition. I am therefore inclined to conclude that the deſcription, though literally tranſlated, is faulty, ſo far as it contradicts the notion of the obſerver being ſo placed as to view the city over a portion of the bay. I imagine that the people, when they run haſtily to the ſea exclaiming Morgana! do not run to the ramparts of the town, but to the ſouthern point of the bay, at the diſtance of half a mile or leſs from the town; whence, with the ſun * behind them, they may have an oblique view of Reggio ſtrongly illuminated, and a more direct proſpect of the northern ſhore of the bay.

NEW PUBLICATIONS.

An Account of two Caſes of the Diabetes Mellitus, with Remarks as they aroſe during the Progreſs of the Cure; to which are added, A general View of the Nature of the Diſeaſe, and its appropriate Treatment; including Obſervations on ſome Diſeaſes depending on Stomach Affection, and a Detail of the Communications received on the Subject ſince the Diſperſion of the Notes on the firſt Caſe. By John Rollo, M. D. Surgeon General, Royal Artillery.——With the Reſults of the Trials with various Acids and other Subſtances in the Treatment of the Lues Venerea, and ſome Obſervations on the Nature of Sugar, &c. By William Cruickſhank, Chemiſt to the Ordnance, &c. in two Volumes Octavo, 14s. Dilly.

IN this treatiſe Dr. Rollo has conſidered the diabetes mellitus as a diſeaſe of the organs of digeſtion, and not of the kidneys; he conceives that the ſugar which paſſes off by urine

* The ſun's rays can never make an angle of 45° on the ſea at Reggio, from the azimuth of the image in the drawing.

is formed in the ſtomach, and depends chiefly upon ſome vitiated, but increaſed action of this organ. He was led to take this view of the diſeaſe from reflecting on the ſtate of the ſtomach and habits of life which preceded, the voracious appetite which always accompanies it, and the ſtate of the blood, the ſerum of which, although not ſenſibly ſweet, had not the uſual ſaltiſh taſte.

The Doctor ſuppoſes, that in this complaint the vegetable matter taken into the ſtomach has not, from ſome defect in this organ, undergone a ſufficient change to form proper chyle; that in conſequence of this much ſaccharine matter is evolved, which, when carried into the circulation, proves a general ſtimulus, producing head-aches, and quickneſs of pulſe, but that it acts more remarkably on the kidneys, occaſioning a conſtant and copious ſecretion of ſweet urine. From this hypotheſis, he was naturally led to adopt a plan of cure, which has proved completely ſucceſsful. The indications he lays down are: 1. To prevent the formation of ſaccharine matter in the ſtomach; and, 2. To remove the morbidly increaſed action of this organ, and reſtore it to a healthful condition. Theſe indications are to be anſwered by a complete diet of animal food, and by the uſe of ſuch medicines as ſhall diminiſh the action of the ſtomach, and at the ſame time counteract the formation of ſaccharine matter. The remedies employed for this purpoſe have been emetics, kali ſulphuratum, lime-water, hepatized ammonia, and vegetable narcotics. But the principal dependence is to be placed on a total abſtinence from all vegetable matter, which alone can ſupply the ſaccharine principle. By a regular perſeverance in this plan, the firſt patient was completely cured in four weeks, although the diſeaſe had been of ſeven months continuance. The urine, which at the commencement of the treatment was ſweet, and amounted to 24 pints daily, was at laſt reduced to 1½ pint, being at the ſame time free from any ſaccharine impregnation.

The ſecond patient, from his age and other circumſtances, although relieved from the diabetic affection, did not regain his wonted ſtate of health; but even in this caſe, the effects produced by the treatment, when properly attended to, were moſt decidedly in confirmation of this plan of cure.

The Doctor has received ſeveral communications in conſequence of the diſperſion of the printed notes on the firſt caſe. The moſt important are the reſult of two caſes treated in this way by Dr. Cleghorn of Glaſgow, and one by Drs. Currie and Gerard at Liverpool; all of which afford the ſtrongeſt corroboration of the efficacy of this mode of treatment.

To this account of the diabetes are added ſome experiments on ſugar, and the effects of different acids in the lues venerea. The caſes of lues venerea treated in this way, and detailed at ſome length, are: 17 by Mr. Cruickſhank, 2 by Dr. Irwin, 5 by Dr. Jameſon, and 8 by Dr. Wittman—making in all 32. Of theſe 19 were cured by the nitrous acid, 4 by the oxygenated muriatic acid, 3 by lemon-juice, or the citric acid, and 6 by the oxygenated muriate of pot-aſh. The affections in all theſe were of the primary kind, being chancres and buboes, but diſtinctly marked. The effects produced by the different remedies were nearly the ſame, and ſuch as ſeemed to indicate a general increaſed action of the ſyſtem. There was an increaſe of appetite, more thirſt than uſual, a white tongue, an augmentation in the quantity of urine, and the blood when drawn was generally ſizy: nothing, however, like ſalivation was obſerved.

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The cures in general feemed to have been performed in lefs time than would have been neceffary under the mercurial plan; and without any confinement or particular regimen. The effect is fuppofed to have been produced by the difengagement of oxygene from the different fubftances employed, inducing a new difeafe in the fyftem. Of the different remedies, the preference is given to the nitrous acid and the oxygenated muriate of pot-afh:—of the firft, from one to three drachms were given daily, diluted with about a quart of water, and of the oxygenated muriate of pot-afh from fix to fixteen grains four times a-day. No external applications were employed, but milk and water, or a very dilute folution of the ceruffa acetata, merely to keep the parts clean. At the time this publication went to prefs, no relapfes had taken place, although fome of them had been cured upwards of three months.

Annals of Medicine for the Year 1796. Exhibiting a concife View of the lateft and moft important Difcoveries in Medicine and Medical Philofophy. By Andrew Duncan, Sen. M. D. and Andrew Duncan, Jun. M. D. Fellows of the Royal College of Phyficians, Edinburgh, Vol. I. octavo, 469 pages. Edinburgh, printed for Mudie and Son, and for Robinfons, London, 1796.

The Medical Commentaries of the elder Dr. Duncan, in twenty volumes, publifhed at Edinburgh, are well known to the public. The Annals of Medicine are offered as a continuation of that work, from which the plan will not materially differ. The editors, from whofe preface I give this account, expect that, when peace fhall be again eftablifhed, their accounts of foreign medical literature will be fuperior to what the Englifh reader has been hitherto accuftomed to receive.

Reflecting practitioners are invited to ufe this work occafionally as a channel of public communication of fuch practical obfervations and facts as they may think worthy of being fo diffufed. Thefe may be tranfmitted either to Dr. Duncan of Edinburgh, or Dr. Pearfon of Leicefter Square, London.

The volume confifts of four fections. The firft contains analyfes of books, twenty in number; the fecond, medical obfervations or cafes; the third, medical news; and the fourth, a lift of new books.

Profeffional men will not require to be informed of the utility of publications of this nature; and there is no queftion refpecting the ability of the editors. If it were practicable, with confiftent brevity, to give any analyfis of the contents of fuch a work, it would on thefe accounts be the lefs neceffary to make the attempt. I fhall therefore only remark, that the number of new and interefting articles in this volume is confiderable.

A Narrative of the fuccefsful Manner of cultivating the Clove-Tree, in the Ifland of Dominica, one of the Windward Charibbee Iflands. By William Urban Buée, Efq. London, printed 1797. Quarto, 31 Pages, with an Engraving of the Clove-Tree, and fome Implements for planting it. No bookfeller's name, nor price known.

This pamphlet was printed by order of the Privy Council, in confequence of a Report from the Right Hon. Sir Jofeph Banks, Bart. to whom it was referred, as appears in a letter from that gentleman to the Earl of Liverpool, which forms part of the Appendix. From that communication it appears, that Mr. Buée is the firft perfon who has obferved that

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the pimento tree profpers beft in thofe fterile foils where trees whofe wood is of a hard texture abound, and that fugar cannot be cultivated to advantage in fuch places; and alfo, on the other hand, that where trees whofe wood is foft are naturally found, pimento trees are rarely met with, and fugar plantations will fucceed.

Mr Buée obferves, that in the Weft Indies, particularly in Dominica, moft lands facing the eaft are of a yellowifh or reddifh ftiff clay; a foil which, with few exceptions, is hardly fit for any cultivation, but is productive of the hard wood trees. Partly by the obfervation of incidents which prefented themfelves, and partly from a rational procefs of inveftigation of the fubject, the author has applied thefe facts to the clove-tree, which is of a hard clofe grain, though not fo tough as the pimento. In the Moluccas, where the clove-trees grow, the ground is covered with them, and will not admit the culture of any thing elfe. It was not, however, till after a number of experiments, during a feries of years, that he arrived at the poffeffion of feveral bearing clove-trees. The hiftorical account of his proceedings with regard to the beft method of planting the tree, and of rendering the cloves merchantable, is intelligent and clear. For this, however, I muft refer the reader to the work itfelf. Mr. Buée has alfo fucceeded in propagating the cinnamon-tree, of which he poffeffes a great number, and promifes to make it the fubject of his future remarks.

Thefe obfervations, to ufe the words of Sir Jofeph Banks, open to the cultivators of hot climates a new fource of wealth, which will not probably be confined to the growth of cloves. Other fpices may alfo profper beft in the barren foils of the Weft Indies, as lavender, thyme, and other aromatic plants are known to do in thofe of Europe.

Befides the letter of Sir Jofeph Banks, the Appendix contains a lift of the ufeful plants cultivated in the Royal Gardens at the Ifle of France in 1790; and a letter from Mr. Rutton of Charing-Crofs, expreffing the uniform opinion of feveral eminent grocers, that certain famples of Mr. Buée's cloves, forwarded to the Council by Sir Jofeph, will anfwer every culinary purpofe as well as thofe of the Spice Iflands in the Eaft Indies.

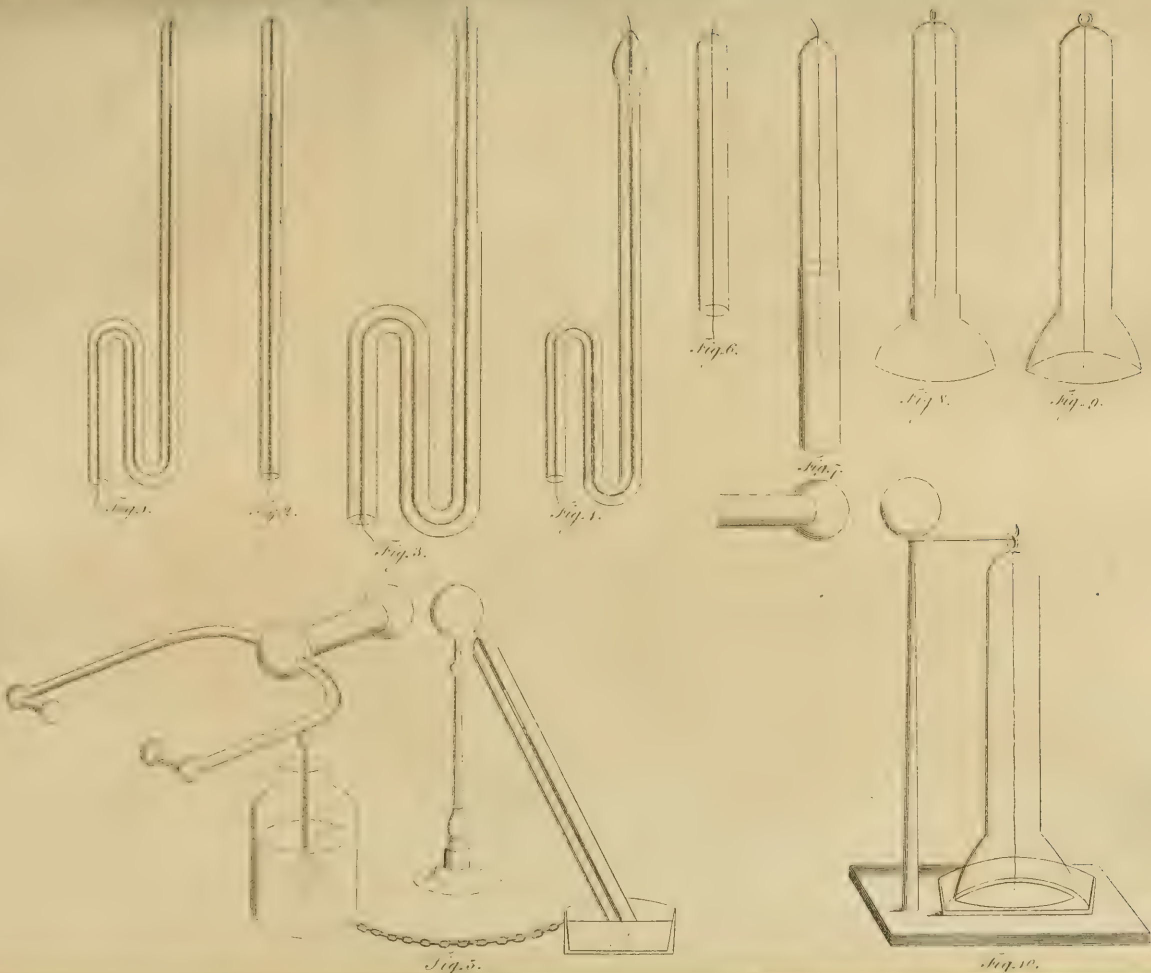


Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5. Fig. 6. Fig. 7. Fig. 8. Fig. 9. Fig. 10.

XII.

Method of closing Vessels

Fig. 1

Eudiometer
with Sulphuret
Fig. 2

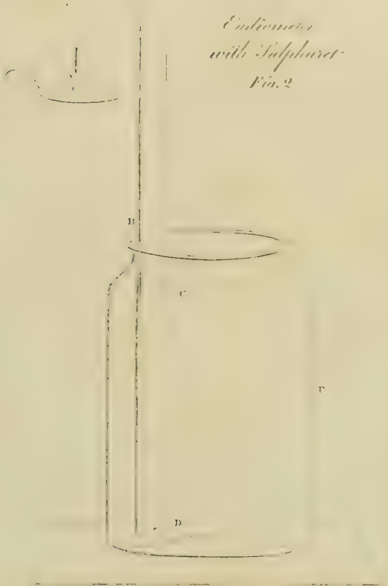
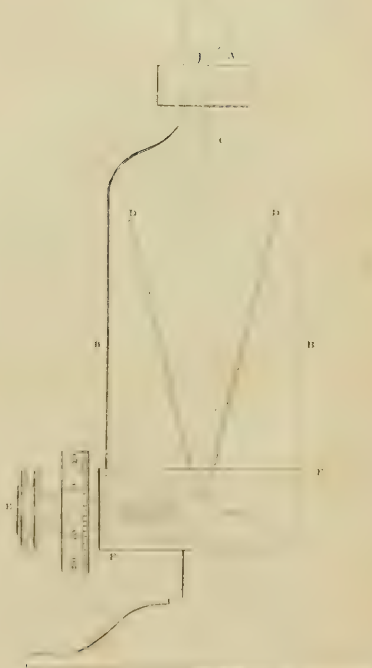
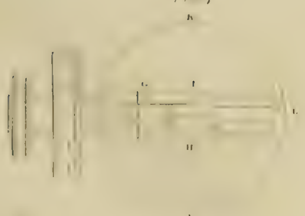


Fig. 3





A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER 1797.

ARTICLE I.

An Account of the Manner in which Heat is propagated in Fluids, and its general Consequences in the Economy of the Universe. By BENJAMIN *Count of* RUMFORD *.

THE doctrine of heat is of ſuch ſingular importance, not only in the experiments of philoſophers, but in the whole economy of animated and of inanimate beings, that at firſt ſight it appears wonderful the greateſt part of the diſcoveries relative to this natural power ſhould have been made by our cotemporaries. Not many years ago, our knowledge amounted to little more than that bodies, by the communication of heat, acquire a common temperature; a fact which, ſimply expreſſed, denotes nothing more than that there is a ſtate of equilibrium, at which two bodies may be in contact, without the communication of heat from the one to the other; and again, that the communication of heat is more rapidly effected through ſome bodies than through others. The diſcoveries of Doctors Black, Irvine, Crawford, and others, have taught us, that when bodies equal in weight or bulk, or alike in any other attribute (their intimate nature or compoſition excepted), are brought into contact, they do not, by acquiring the common temperature, occaſion an equal change in the ſenſible heat of each. In ſome inſtances the heavier body muſt part with a greater number of degrees of its temperature before an equilibrium of communication becomes eſtabliſhed between itſelf and a lighter body; and in other inſtances the contrary will be the caſe. If, therefore, water be uſed as a ſtandard, and applied in like temperatures and circumſtances to different bodies, the common temperature will differ accordingly as the bodies themſelves require a greater or leſs portion of heat to occaſion ſimilar changes

* Abſtract or abridgement from his writings, chiefly the VIIth Experimental Eſſay.

in their fenfible heat. Thefe important refults conftitute the foundation of the theory of the different capacities of bodies for heat, or their fpecific heats when at the fame temperature.

Another difcovery, of no lefs magnitude, feemed to render us acquainted with the principal caufes by which the mutations of temperature are originally effected. It was found by experiment, not only that different bodies require greater or lefs quantities of heat to be communicated in order to raife their temperature through equal numbers of degrees, but alfo that the capacity of any individual fubftance is leaft when in the folid ftate, greater in the fluid ftate, and greateft of all when converted into elaftic vapour or air. So that, as a natural confequence, a much greater portion of heat will be required to convert ice into water, and raife its temperature ten degrees, than would have been neceffary to have equally elevated the temperature of cold ice without melting it. And fo likewife the vaporization of water, without raifing its temperature beyond the boiling-water point, requires a much larger quantity of heat than would have heated the fame mafs of water from the freezing to the boiling point.

A great number of interefting deductions, from thefe facts, have been applied to explain the general mutations of heat in the univerfe around us. For, in the firft place, fince every change of chemical combination is attended with a greater or lefs change of the capacities of the aggregate, it almoft invariably happens that the temperature of the new compound is either above or below the common temperature of the atmofphere; and as the fufibilities of thefe compounds are alfo affected according to laws of which at prefent we know nothing, there is very frequently a change of temperature on this account alfo.

Such is the moft luminous and beautiful theory of heat which our cotemporaries have begun to develop, and which is at prefent explained at confiderable length in moft elementary works. But as this fubject appears to depend upon facts fcarcely capable of being extended farther by hypothefis or analogical reafoning, it has happened that inductions experiments and applications, though of the greateft value, have been but flowly made. The effect of chemical operations on the temperature of bodies has been applied with great fuccefs to explain the act of combuftion; and the general confequences of the change of capacity in the folid, fluid, and vaporous ftates have been confidered with regard to their extenfive influence upon the face of the globe. But thofe refults which originate chiefly from the differences of conducting power in bodies with regard to heat, have fcarcely conftituted an object of direct enquiry among philofophers, excepting by Count Rumford in the Philofophical Tranfactions, and in his Experimental Effays, at prefent in the progrefs of publication. On the prefent occafion I fhall not enter into the confideration of the extenfive economical ufes to which this philofopher has applied the refults of his experiments and deductions, but fhall chiefly confine the prefent memoir to the confequences which he has fhewn to arife from the imperfect conducting power of fluids with regard to heat.

The free paffage of heat, in all directions, through all kinds of bodies, has never yet been called in queftion, though the rapidity of its tranfmiffion is well known to differ exceedingly in various kinds of bodies. Under the influence of this opinion Count Rumford began his experiments on heat. His former experiments fhewed that air is a non-conductor of heat; and the late experiments contained in his feventh Effay afcertain that water is in the fame predicament. He thinks that all other fluids have the fame property. He was led

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to an experimental investigation of this curious subject by certain accidental events which he relates. These shewed, that when the circulation of water is impeded by mucilage or by fibrous matter, as in apple-pie and thick rice-soup, the heat is not only a long time in escaping, but may be very considerable in one part of the mass, while other parts are nearly cold. The baths of Baia afforded another instance of the same nature, which is very striking. When the Count was standing on the sea-shore, near the baths, where the hot steam was issuing out of every crevice of the rocks, and even rising up out of the ground, he had the curiosity to put his hand into the water. He was not surprised to find that the waves of the sea, which incessantly followed each other over the beach, should feel cold; but he was more than surprised when, on running the ends of his fingers into the sand beneath the water, he found the heat quite intolerable. The sand was perfectly wet, and yet the temperature was so very different at the small distance of two or three inches! He even found that the surface of the sand was to all appearance quite as cold as the water which flowed over it. He could not reconcile this to the supposed great conducting power of water; and then, for the first time, in consequence of his doubts respecting the existence or intensity of this power, he determined to make experiments to ascertain the fact. These however were delayed, and probably might have remained undone, if another unexpected appearance had not revived his curiosity.

In the course of a set of experiments on heat, Count R. had occasion to use thermometers of uncommon size, their globular bulbs being above four inches in diameter filled with various liquids. One of these, containing alcohol much heated, was placed in a window where the sun happened to be shining; when, casting his eyes on the tube, which was quite naked, and divided by means of a diamond, he saw the whole mass of its contents in a most rapid motion, in two opposite directions, up and down at the same time. These motions were rendered visible by some particles of fine dust that happened to be in the ball before it was filled. The tube was $\frac{43}{100}$ths of an inch in diameter; and upon examination with a lens, the rising current of spirit was seen to occupy the axis or internal part of the tube, and the descending stream was contiguous to the sides. When the tube was inclined, the rising current occupied the uppermost side, and the descending stream the lower. The velocities were perceptibly increased by wetting the tube with ice-cold water; they became gradually less as the thermometer was cooled, and ceased when the fluid had acquired the common temperature of the room; and the motion was greatly prolonged when the cooling of the bulb was impeded by wrapping it in furs or any other warm covering.

The same experiments, with motion of the same kind, and quite as rapid, were repeated with a similar thermometer filled with linseed-oil.

From these facts Count Rumford was led to conclude that the fluids he had tried, and probably all others, are in fact non-conductors * of heat; that they transmit this matter

* Throughout this most valuable essay, the author speaks of fluids, and particularly water, as *perfect non-conductors of heat*; which seems to me to be an inaccurate expression of the facts. His experiments, hereafter to be related, may prove that the direct communication of heat through a line of particles of a fluid, on the supposition of their remaining immoveable, is extremely slow; but not that it does not take place. If each individual particle were not capable of receiving and giving heat, the process by circulation could not happen; and if on the contrary they be capable, the propagation of heat from particle to particle is possible, and must doubtless happen; though from the facts it appears that the heat conveyed by the internal motion is very much greater than what passes by contiguity. Fluids then are very imperfect conductors of heat, but not perfect non-conductors. N.

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or quality, by actual motions, from the heated to the cold bodies with which they may ſucceſſively come into contact; and that every means of obſtructing or retarding thoſe motions would render the propagation of heat more ſlow and difficult. He had before found that this was actually the caſe with air, and on the preſent occaſion he proceeded to give the ſubject a thorough and careful inveſtigation.

The apparatus made uſe of for the firſt courſe of experiments conſiſted of a large thermometer, which he calls the paſſage thermometer, conſiſting of a cylindrical veſſel with hemiſpherical ends, forming the bulb, and a glaſs tube fitted into a neck by means of a good cork. Its dimenſions were as follow:—Diameter of the bulb 1.84 inches—Length 4.99—Capacity or contents 13.2099 cubic inches—External ſuperficies 28.834 ſuperficial inches—Thickneſs of the ſheet copper 0.03 inch—Weight when empty 1846 grains, and it contains 3344 grains of water at 55° of temperature. The glaſs tube is 2¼ inches long, and $\frac{4}{10}$ths of an inch in diameter, and the cylindrical neck of copper into which it is fitted by means of cork is one inch long, and $\frac{65}{100}$dths of an inch in diameter.

This thermometer, being filled with linſeed-oil, and its ſcale graduated, was fixed in the axis of a hollow cylinder of thin ſheet copper, 11½ inches long, and 2.3435 inches in diameter internally. This cylinder, which is open at one end, is cloſed at the other with an hemiſpherical bottom with its convex ſide outwards. The bulb of the thermometer was confined in the axis of this caſe, by three ſmall wooden pins, inſerted in ſockets within the large braſs tube, and the upper end was properly ſecured by cauſing the glaſs tube to paſs through a cork ſtopper adapted to the ſame metallic cylinder. The bottom of the bulb reſted on a wooden pin fixed in a ſocket in the middle of the hemiſpherical bottom of the caſe. All the pins terminated in blunt wooden points, to reduce the contacts as much as poſſible.

The ſpace between the thermometer when in its place, and the internal ſurface of the ſurrounding cylinder, was deſigned to contain the ſubſtance through which the heat was made to paſs into or out of the thermometer; the temperature of this laſt maſs being ſhewn by the graduations on the glaſs tube. The quantity of water required to fill the ſpace, and cover the bulb of the thermometer about one quarter of an inch, was found to weigh 2468 grains.

When the bulb of the thermometer was ſurrounded in its place by water or any other liquid or mixture intended to be tried, a cylinder of cork, rather leſs in diameter than the internal cavity, was ſlipped down upon the tube, not quite ſo low as the water or mixture. Above this was placed a quantity of eider-down, ſufficient to fill the remaining cavity, except what was occupied by the cork ſtopper, laſt of all to be inſerted. The thermometer was divided according to Fahrenheit's ſcale, and the whole ſcale, from the freezing point to the boiling-water point, was above the ſtopper.

The operations with this apparatus were performed by placing the prepared inſtrument in melting ice till the thermometer fell to 32°. It was then immediately plunged into a large veſſel of boiling water, and the conducting power of the ſubſtance under examination was eſtimated by the time the heat employed in paſſing through it into the thermometer; the time being carefully noted when the liquid in the thermometer arrived at the 40th degree of its ſcale; and alſo when it came to every 20th degree above it.

In the reverſe operation, the inſtrument was kept in boiling water till its temperature appeared

appeared ftationary, when it was taken out, and immediately plunged in melting ice, and the times of its defcent carefully noted.

As foon as this apparatus was completed, the Count was defirous of afcertaining whether apples which continue hot fo long in pies made of that fruit, do really poffefs a power of retaining heat greater than that of pure water, of which for the moft part they confift; but in the firft place he afcertained the quantity of fibrous matter in apples, by ftewing two ounces troy of the pulp, and wafhing off the foluble matter with a large quantity of cold water. The fibrous remainder, when thoroughly dried, weighed only 25 grains, which, by remaining for feveral days in a plate on the top of a heated German ftove, was further reduced to $18\frac{9}{10}$ths, or lefs than 1-50th part of the whole mafs.

The refults obtained by furrounding the bulb of the paffage thermometer with a quantity of ftewed apples, fo confiftent as not to exhibit figns of fluidity, and then expofing the apparatus to the heating and cooling proceffes, are tabulated in the original Effay. The conducting power of the ftewed apples proved to be little more than half that of pure water; that is to fay, the heat was nearly twice as long in paffing through the former as the latter.

As the attention of our author was fteadily fixed on the pofition that heat is communicated by fluids, only by its being tranfported by virtue of their inteftine motion produced by the change of fpecific gravity, he concluded that there may be two ways of obftructing this propagation of heat; namely, by diminifhing their fluidity, which may be done by folution of any mucilaginous fubftance; or, more fimply, by impeding the motion of their particles, which may be effected by mixing any folid fubftance with them which is an imperfect conductor of heat, and of an enlarged furface by being divided into fmall maffes.

In the experiments with ftewed apples, the paffage of the heat in the water, which conftituted by far the greateft part of the mafs, was doubtlefs obftructed in both thefe ways. The mucilaginous part of the apples diminifhed very much the fluidity of the water, at the fame time that the fibrous parts ferved to embarrafs its motions.

In order to difcover the comparative effects of thefe two caufes, water was boiled with about 1-12th part of its weight of ftarch, and examined by the apparatus. The fame weight of eider-down was alfo boiled with a like quantity of water. The intention of this laft boiling was to free the eider-down from water. From the tabulated experiments, it appears that thefe feveral additions impaired the conducting power of water nearly to a degree of equality with the ftewed apples.

In the Philofophical Tranfactions for 1792, where Count Rumford has afcertained that heat is actually propagated in air in the fame manner as it is here ftated to be propagated in water, he found that the thicknefs of a ftratum of air, which ferved as a barrier to heat, remaining the fame, the paffage of heat through it was fometimes rendered more difficult by increafing the quantity of the light fubftance, which was mixed with it to obftruct its internal motion. To fee if fimilar effects would be produced with water, he repeated the experiments with eider-down, reducing the quantity of it mixed with water to one fourth of the quantity ufed in the former experiments. The refiftance to the paffage of heat was confiderably diminifhed.

The refults of thefe experiments are extremely interefting; they not only make us acquainted with a new and very curious fact, namely, that feathers, and other like fubftances, which

which in air are known to form very warm coverings for confining heat, may ſerve the ſame purpoſe in water, but that their effects in preventing the paſſage of heat are even greater in water than in air.

This diſcovery is very happily applied by the Count to elucidate ſome of the moſt intereſting parts of the economy of nature, on which ſubject I ſhall give his inferences very nearly in his own words.

As liquid water is the vehicle of heat and nouriſhment, and conſequently of life in every living thing; and as water, left to itſelf, freezes with a degree of cold much leſs than that which frequently prevails in cold climates, it is agreeable to the ideas we have of the Creator of the world, to expect that effectual meaſures would be taken to preſerve a ſufficient quantity of that liquid in its fluid ſtate, to maintain life during the cold ſeaſon: and this we find has actually been done; for both plants and animals are found to ſurvive the longeſt and moſt ſevere winters: but the means which have been employed to produce this admirable effect have not been inveſtigated; at leaſt not as far as they relate to vegetables.

But as animal and vegetable bodies are eſſentially different in many reſpects, it is very natural to ſuppoſe that the means would be different which are employed to preſerve them againſt the fatal effects which would be produced in each by the congelation of their fluids.

Among organized bodies which live on the ſurface of the earth, and which of courſe are expoſed to the viciſſitudes of the ſeaſon, we find, that as the proportion of fluids to ſolids is greater, the greater is the heat which is required for the ſupport of life and health; and the leſs are they able to endure any conſiderable change of their temperature.

The proportion of fluids to ſolids is much greater in animals than in vegetables; and in order to preſerve in them the great quantity of heat which is neceſſary to the preſervation of life, they are furniſhed with lungs, and are warmed by a proceſs ſimilar to that by which heat is generated in the combuſtion of inflammable bodies.

Among vegetables, thoſe which are the moſt ſucculent are annual. Not being furniſhed with lungs to keep the great maſs of liquids warm which fill their large and ſlender veſſels, they live only while the genial influence of the ſun warms them, and animates their feeble powers; and they droop and die as ſoon as they are deprived of its ſupport.

There are many tender plants to be found in cold countries which die in the autumn, the roots of which remain alive during the winter, and ſend off freſh ſhoots in the enſuing ſpring. In theſe we ſhall conſtantly find the roots more compact and denſe than the ſtalk, or with ſmaller veſſels and a ſmaller proportion of fluids.

Among the trees of the foreſt, we ſhall conſtantly find that thoſe which contain a great proportion of thin watery liquids not only ſhed their leaves every autumn, but are ſometimes frozen and actually killed in ſevere froſts. Many thouſands of the largeſt walnut-trees were killed by the froſt in the Palatinate during the very cold winter in the year 1788; and it is well known that few, if any, of the deciduous plants of our temperate climate would be able to ſupport the exceſſive cold of the frigid zone.

The trees which grow in thoſe inhoſpitable climates, and which brave the cold of the ſevereſt winters, contain very little watery liquids. The ſap which circulates in their veſſels is thick and viſcous, and can hardly be ſaid to be fluid. Is there not the ſtrongeſt reaſon to think that this was ſo contrived for the expreſs purpoſe of preventing their being deprived of all their heat, and killed by the cold during the winter?

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We have seen by the foregoing experiments, how much the propagation of heat in a liquid is retarded by diminishing its fluidity; and who knows but this may continue to be the case as long as any degree of fluidity remains?

As the bodies and branches of trees are not covered in winter by the snow which protects their roots from the cold atmosphere, it is evident that extraordinary measures were necessary to prevent their being frozen. The bark of all such trees as are designed by nature to support great degrees of cold, forms a very warm covering; but this precaution alone would certainly not have been sufficient for their protection. The sap, in all trees which are capable of supporting a long continuance of frost, grows thick and viscous on the approach of winter. What more important purpose could this change answer, than that here indicated? And it would be more than folly to pretend that it answers no useful purpose at all.

We have seen by the results of the foregoing experiments, how much the simple embarrassment of liquids in their internal motions tends to retard the propagation of heat in them, and consequently its passage out of them: and when we consider the extreme smallness of the vessels in which the sap moves in vegetables, and particularly in large trees;—when we recollect that the substance of which these small tubes are formed is one of the best non-conductors of heat known *;—and when we advert to the additional embarrassments to the passage of the heat which arise from the increased viscosity of the sap in winter, and to the almost impenetrable covering for confining heat which is formed by the bark, we shall no longer be at a loss to account for the preservation of trees during the winter, notwithstanding the long continuation of the hard frosts to which they are annually exposed.

On the same principles we may, I think, account, in a satisfactory manner, for the preservation of several kinds of fruit; such as apples and pears, for instance, which are known to support, without freezing, a degree of cold which would soon reduce an equal volume of pure water to a solid mass of ice.

At the same time that the compact skin of the fruit effectually prevents the evaporation of its fluid parts, which, as is well known, could not take place without occasioning a very great loss of heat, the internal motions of those fluids are so much obstructed by the thin partitions of the innumerable small cells in which they are confined, that the communication of their heat to the air ought, according to our hypothesis, to be extremely slow and difficult. These fruits do, however, freeze at last, when the cold is very intense; but it must be remembered that they are composed almost entirely of liquids, and of such liquids as do not grow viscous with cold; and moreover that they were evidently not designed to support for a long time very severe frosts.

Parsnips and carrots, and several other kinds of roots, support cold without freezi[n]g still

* I lately by accident had occasion to observe a very striking proof of the extreme difficulty with which heat passes in wood. Being present at the foundry at Munich when cannons were casting, I observed that the founder used a wooden instrument for stirring the melted metal. It was a piece of oak plank, green, or unseasoned, about ten inches square, and two inches thick, with a long wooden handle, which was fitted into a hole in the middle of it. As this instrument was frequently used, and sometimes remained a considerable time in the furnace, in which the heat was most intense, I was surprised to find that it was not consumed; but I was still more surprised, on examining the part of the plank which had been immersed in the melted metal, to find that the heat had penetrated it to so inconsiderable a depth, that at the distance of one-twentieth of an inch below its surface the wood did not seem to have been in the least affected by it. The colour of the wood remained unchanged, and it did not appear to have lost even its moisture. R.

longer

longer than apples and pears; but thefe are lefs watery, and I believe the veffels in which their fluids are contained are fmaller: and both thefe circumftances ought, according to our affumed principles, to render the paffage of their heat out of them more difficult, and confequently to retard their congelation.

[*To be continued.*]

II.

Experiments and Obfervations on the fulminating Preparations of Gold and Silver *.

THE beft procefs for the preparation of fulminating gold being fhewn; the principles furnifhed by the acids during the folution of the gold, and thofe furnifhed by the ammoniac during the precipitation, and the order neceffary in the application of them, were confidered, and divers experiments were related, by which it appeared that this fubftance confifts of oxygen, azote and hydrogen feverally combined with caloric, and feverally attached to the gold as a common bafe.

Twelve grains of this fulminating gold being placed in a conical heap on a thin plate of brafs, were gradually heated. At a temperature between 300 and 400°, the whole exploded, with a very fharp and loud report, and the plate was pierced and torn. The round aperture was about an inch in diameter, and the lacerations extended much farther.

It was obferved that the fame effect might be produced by applying a fpark to fulminating gold lefs heated, and that the accenfion of all fuch fulminating compounds, by a fpark applied to any part, or by due augmentation of the temperature of the whole, is eafily explicable on the grounds mentioned in the minutes of the laft meeting, provided due attention be paid to the ftate of the active ingredients.

In fulminating gold, for inftance, the attractive powers tending to produce the new combinations, which take place in the inftant of combuftion, feem to be almoft equal to thofe by which the aggregation of this compound is maintained at low temperatures. For a fmall augmentation of temperature, or friction, or percuffion, or any thing which difturbs the arrangement of the gafeous principles and caloric which adhere but weakly to the gold, is fufficient for the explofion of the whole, provided the fulminating gold be pure and dry.

In refpect to the perforation of the brafs plate, it was obferved that all bodies which explode inftantaneoufly would imprefs it in a fimilar manner. For the refiftance of the air to projectiles or to expanfions of this kind, being in the ratio of the fquares of the velocities of the moving or expanding bodies, the refiftance of the air to fuch inftantaneous explofions as thofe lately mentioned, is almoft equal to that of a folid, or of the metallic plate on which the fulminating body is heated.

SMALL portions of fulminating filver, each weighing lefs than a grain, were fucceffively exploded, fome by the touch of a flender brafs wire, others by that of a feather.

* From the Minutes of a Society for Philofophical Experiments and Communications: B. Higgins, M. D. Operator. I do not hear that the fociety is continued fince the departure of the able Operator for the Weft-Indies. N.

A defcrip-

A defcription was given of many other fpecimens which had been inadvertently exploded and loft; fome by overheating them to about 90°, in the place where they were to be dried; and others by an accidental concuffion of a great iron plate, on which they were placed in feparate cups.

The experimenter faid, that he had often exploded fulminating filver, in covered veffels of the fmalleft capacity that could be ufed with fafety; and that this fubftance had frequently exploded unexpectedly in his hand, with a report louder than that of a mufket.

A luminous and momentary gleam was always vifible, but he could not difcover any other adequate effect of the emitted caloric; and therefore he concluded, that in fuch inftantaneous explofions the caloric was expelled with velocity fufficient to conftitute light.

Mr. Berthollet, the inventor of fulminating filver, having contented himfelf with a general and concife defcription of this fubject, many practical chemifts have failed in their attempts to prepare it; and others, forming their opinions from the fpecimens which they had made, have been expofed to great danger; as will appear from the following relation, which is the only part of the Minutes on this fubject that can be introduced in the prefent publication.

An ounce of fine filver was diffolved in the courfe of eight hours in an ounce of pure nitrous acid, of the London Pharmacopœia, diluted previoufly with three ounces of diftilled water, in a glafs matrafs. The folution being poured off, the refiduary black powder and the matrafs were wafhed with feven or eight ounces of warm diftilled water, and this was added to the folution. The black powder being gold was rejected; fome gold being thus feparable from any filver of commerce.

To the foregoing diluted folution, pure lime-water prepared with diftilled water was added gradually; for the folution ought not to be poured into the lime-water. When about thirty pints of lime-water had been expended, and the precipitate had fubfided, more lime-water was added, by fucceffive pints, as long as it caufed any precipitation. For it was deemed fitter that the precipitation fhould not be perfected, than that an excefs of lime-water fhould be ufed; the earthy pellicle of the exceffive lime-water being apt to mix with the precipitate. The clear liquor being poured away, the precipitate was poured off, and wafhed into a filter.

When the faline liquor had drained from it, two ounces of diftilled water were poured on the magma; and when this water had paffed, frefh portions were fucceffively added and paffed, until the whole quantity of water thus expended in wafhing away the nitrous calcareous falt amounted to a quart.

The filter being then unfolded, to let the magma of oxide of filver fpread on the flattened paper, it was placed on a chalk-ftone to accelerate the exficcation, and was gradually dried in the open air; a cap of paper being placed loofely over it to exclude the duft.

When the weather ferved, the cap was removed, to expofe the oxide to the rays of the fun; although this was not deemed neceffary; and the exficcation was promoted by cutting the oxide into thin flices. When perfectly dry it weighed 1 oz. 4 dwts. and about one-fifth of it was confidered as oxygene.

"When aqua ammoniæ puræ of any Pharmacopœia is ufed with this oxide, either in the fmall quantity which blackens it completely, or in a greater quantity; the black matter which fubfides, and which has been reprefented by fyftematic writers as the fulminating compound, has no fuch property, any farther than may be owing to the matter depofited from the alkaline folution during the exficcation.

"The alkaline liquor containing the fulminating filver ought to be poured off from the infoluble powder, and expofed in a fhallow veffel to the air. In confequence of the exhalation, black fhining cryftals form on the furface only, and foon join to form a pellicle. As this pellicle adheres a little to the fides of the veffel, or maintains its figure, the liquor may be poured off by a gentle inclination of the veffel.

"This liquor will yield another pellicle in the fame way; but the third or fourth pellicle will be paler than the former, and weaker in the explofion. The firft pellicles, when flowly dried, explode by the touch of a feather, or by their being heated to about 96°.

"The quantity of water in the ordinary aqua ammoniæ puræ renders it lefs active in the folution of the oxide, and is an impediment to the fpeedy formation and feparation of the fulminating filver; and an experimenter who has often ufed twenty grains of the oxide to produce fucceffive pellicles of fulminating filver, which may be feparately exploded with fafety, and who has perceived that the pellicles never explode whilft wet, if they be not heated, would in all probability refolve on the following improvement, and expofe himfelf to the unforefeen danger of it."

DISTILLED water was impregnated with as much pure ammoniac as it could eafily retain under the ordinary temperature of the air. A quantity of this ftrong ammoniacal liquor, equal in bulk to a quarter of an ounce of water, was placed in a fmall bottle, and 24 grains of the oxide of filver, ground to fine powder, were added. The bottle, being almoft filled, was corked, to prevent the formation of that film which ufually appeared in confequence of the exhalation of the ammoniac in other experiments.

During the folution of the oxide, bubbles of the gafeous kind arofe from it, and the folution acquired a blue colour.

As no film appeared, the bottle was agitated three or four times in the courfe of as many hours, in order to promote the folution of a fmall quantity of blackened oxide which remained at the bottom.

The experimenter confidering this as an ample provifion for twenty different charges, to be exploded in different circumftances, in the prefence of the fociety, intended to pour off the folution into as many fmall veffels, and to weigh the refiduary black powder, after allowing two hours more for the folution.

On the fixth hour he took his ufual precaution of wearing fpectacles; and obferving that a fmall quantity of black powder ftill remained undiffolved, and that no film was yet formed at the furface, he took the bottle by the neck to fhake it; knowing that it might explode by the heat of his hand, if he were to grafp it, and that the explofion in this circumftance might wound him dangeroufly.

In the inftant of fhaking, it exploded with a report that ftunned him. The bottle was blown into fragments fo fmall as to appear like glafs coarfely powdered. The hand which held it was impreffed as by the blow of a great hammer, and loft the fenfe of feeling for fome feconds; and about fifty-two fmall grains of glafs were lodged, many of them deeply,

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in the ſkin of the palm and fingers. The liquor ſtained his whole dreſs, and every part of the ſkin that it touched.

Thus it appeared that fulminating ſilver may be made which will explode even when cold and wet, by the mere diſturbance of the arrangement of its parts, in the aqueous fluid.

In ſubſequent experiments, privately and carefully conducted, it ſeemed that the property of exploding in the cold liquor, by mere commotion, depended on the unuſual quantity or proximity of the exploſive molecules in a given bulk of the liquor. And the flat bottoms, as well as the ſides, of the thick veſſels of glaſs or potters-ware, whether they ſtood on boards or on iron plates, were always beaten to ſmall fragments.

This afforded a curious inſtance of the poſſible equilibrium between the powers tending to retain the caloric, and thoſe which effect the expulſion of it; and experiments and conſiderations of this kind ſeemed to promiſe a true ſolution of the phenomena of Rupert's drops.

SMALL charges, each conſiſting of a grain of oxygenated muriate of pot-aſh, finely powdered, and mixed with an equal quantity of flowers of ſulphur, were exploded by mere trituration. And Mr. Godfrey's relation of the danger of keeping ſuch a mixture in a bottle was duly noticed; for, after he had kept it thus for ſome time, he found that it had exploded ſpontaneouſly.

III.

Experiments and Obſervations made with the View of aſcertaining the Nature of the Gaz produced by paſſing Electric Diſcharges through Water; with a Deſcription of the Apparatus for theſe Experiments. By GEORGE PEARSON, *M. D. F. R. S.*

[Continued from page 248.]

SECTION II.

EXPERIMENTS.

FROM my Journal of the numerous experiments made during the courſe of nearly two years, I ſhall ſelect thoſe which will ſerve to explain the nature of the proceſs, and ſhew the power of the plate electrical machines; and I ſhall particularly relate thoſe experiments which afforded the moſt uſeful reſults concerning the nature of the gaz obtained.

1. *With interrupted Diſcharges.*

Experiment A. About 1600 of theſe diſcharges by means of a 34 inch ſingle plate electrical machine, in nearly three hours, produced, from New River water taken from the ciſtern, and which had not been freed from air by the air pump or boiling, a column of gaz, two-thirds of an inch in length, and 1-9th of an inch wide. On paſſing through this gaz, between the two wires of the tube in which it was produced, a ſingle electrical ſpark, its bulk was inſtantly diminiſhed to two-thirds. In other experiments the bulk of gaz was only diminiſhed to about one half. And the reſult was the ſame with diſtilled water.

 B. The

B. The experiment A being repeated feveral times with diftilled, and New River water, freed from air by the air pump or long boiling, the quantity of gaz juft mentioned was obtained in about four hours.

On paffing an electric fpark through this gaz in the fituation above mentioned, its bulk was inftantly diminifhed in fome cafes 15-16ths, and in others 19-20ths.

C. 1600 interrupted difcharges, by means of a 32 inch plate machine, produced from New River water, and diftilled water, freed from their air by the air pump, a column of gaz, about 3-4ths of an inch in length and 1-9th of an inch in diameter, in the fpace of three hours. It was reduced in bulk 19-20ths by paffing through it a fingle electrical fpark.

D. 500 revolutions of the 32 inch plate machine, in three quarters of an hour, produced 600 interrupted difcharges in river water freed from air by the air pump, by which a column of gaz, half an inch in length and 1-10th of an inch in diameter, was obtained. It was diminifhed as ufual by an electric fpark 19-20ths of its bulk.

E. Nearly four days inceffant labour with the 32 inch machine produced only 56,5488 cubes of gaz, of 1-10th of an inch each, on account of the ufual accidents during the procefs. The air had been exhaufted by fetting the water under the receiver of the air pump.

F. It was found that 6000 interrupted difcharges produced about three inches in length of gaz, meafured in a tube 3-20ths of an inch in width from water out of which its air had been drawn by the air pump.

G. It appeared from many experiments, that the fame unboiled water, or water from which the air had not been exhaufted by the air pump, which had repeatedly yielded gaz by paffing through it electrical difcharges, always left a refidue of gaz which the electrical fpark did not diminifh; and this refidue was in nearly the fame quantity after fix or feven experiments, each of which afforded a column of gaz half an inch in length and 1-9th of an inch in diameter, as was left on paffing the electric fpark through the gaz afforded by the third or fourth experiment.

Hence it feems that water is decompounded by the electric difcharge, before the whole of the common or atmofpherical air is detached from the water by merely the impulfe of each difcharge. Yet I think it probable that, after the difcharges have been paffed through the fame water for a certain time, the whole of the air contained in water will be expelled, and no gaz be produced, but that compounded by means of the electric fire from water; in which cafe, fuppofing the gaz fo produced to be at leaft merely hydrogen and oxygen gaz, it will totally difappear on paffing through it an electrical fpark. But I have never been able to determine this point, becaufe the tubes were always broken after obtaining a few products, or long before it could reafonably be fuppofed the whole of the air of the water was expelled from it.

H. To the gaz obtained in the experiment E was added, over water, an equal bulk of almoft pure nitrous gaz. Fumes of nitrous acid appeared, and the gaz examined was reduced almoft one-third of its bulk. A fmall bubble more of nitrous gaz being let up, no further diminution took place. To this refidue was added half its bulk of oxygen gaz, obtained from oxy-muriate of pot-afh. This mixture of gazes having ftood feveral days over well burnt lime and boiled quickfilver, an electric fpark was paffed through the mixture over quickfilver, by which its bulk was inftantly diminifhed one-fourth. But no moifture could be perceived upon the fides of the tube or on the quickfilver. The failure of the appearance

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of moiſture was imputed to a bit of lime accidentally left in the tube which was burſt by the exploſion, and diſperſed through the tube; or elſe the quantity of water produced was ſo ſmall, comparatively with the reſiduary gaz, that the water was diſſolved by it in the moment of its compoſition. For, ſuppoſing water to have been compounded, it could not amount to the 1-100th part of a grain, and the reſiduary gaz was at leaſt 2000 times this bulk.

That a quantity of water can be compounded under the ſame circumſtances as in this experiment, and be apparently diſſolved in air, ſo as to eſcape obſervation, even with a lens, was proved by paſſing an electric ſpark through a mixture of hydrogen and oxygen gaz, well dried by ſtanding over lime.

2. *With complete or uninterrupted Diſcharges.*

THE gaz obtained by the firſt deſcribed kind of apparatus for the uninterrupted diſcharges, p. 145, and Fig. 6, and 7, always left a reſidue of at leaſt one-fourth of its bulk, on paſſing through it the electric ſpark; even when water was uſed which had been freed from air by boiling or the air pump. Nor will this reſult appear ſurpriſing, when it is conſidered how liable the water in this apparatus is to mix and abſorb air during the experiment. However, this method would have been extremely valuable, if the next other method had not been diſcovered; for gaz may be obtained by it with fewer accidents, and much more rapidly, than with the interrupted diſcharges The apparatus is alſo much more eaſily fitted up, and is more ſimple. But I think it unneceſſary to particularly relate any experiments, as they afforded the ſame reſults as thoſe already deſcribed, and as thoſe next to be related.

The following experiments were made with the apparatus deſcribed p. 146, and ſhewn by Fig. 8, 9, and 10:

Experiment I. At 0 h. 40′ P. M. began to produce diſcharges with a double plate twenty-four inch machine, in water taken from the ciſtern; and at 12 h. 6′ P. M. of the ſame day there had been written down 10200 diſcharges, each of which occaſioned air to aſcend from the bottom of the wire and braſs cup. The quantity of air obtained was now apparently about one-fourth of a cubical inch, and it occupied nearly half of the tube, the water in which was by this time very muddy.

After ſtanding till the day following at noon, when the proceſs was again commenced, it did not appear that any of the gaz had been abſorbed by the water over which it ſtood.

At 2 h. 35′ P. M. began to produce diſcharges, and at 8 h. P. M. had paſſed 6636; which, together with thoſe of the preceding day, amounted to 16,836. The tube was now 5-8ths full of gaz, and there ſeemed to be almoſt half a cubical inch; for it was obſerved that the gaz was this day yielded at double the rate it had been the day before. This was accounted for from the diminiſhed preſſure upon the electric fire, by the tube containing gaz inſtead of water.

At this time, namely at 8 h. P. M. I was ſurpriſed on the paſſing of a diſcharge by a vivid illumination of the whole tube, and a violent commotion within it, with, at the ſame time, the ruſhing up of water, inſtantly to occupy rather more than 5-8ths of the ſpace which had been occupied by gaz.

The reſidue of gaz was not diminiſhed further by an electric ſpark; and to the teſt of nitrous gaz it appeared to be rather worſe than atmoſpherical air, as it conſiſted of rather leſs than one part of oxygen, and three parts of nitrogen or azotic gaz.

It ſeemed as if the electrical diſcharge had kindled the oxygen and hydrogen gaz of the decompounded gaz, by flying from the bottom of the wire to the braſs funnel, ſo that the fire returned into the tube where it paſſed through the gaz. Or the combuſtion might be occaſioned by a chain of bubbles reaching from the braſs diſh to the ſurface of the water in the tube, which was ſet on fire in its aſcent, and thus produced combuſtion of the whole of the gaz of decompounded water.

That this phenomenon was from the combuſtion here ſuppoſed, was in ſome degree proved, by finding that the mixture of hydrogen gaz and atmoſpherical air, under the ſame circumſtances, was kindled in the ſame manner.

Experiment II. With a double plate electrical machine, 24 inches in diameter, and a ſimilar apparatus to that in the laſt experiment, 14,600 diſcharges produced at leaſt one-third of a cubical inch of gaz. While I was meaſuring with a pair of compaſſes the quantity of gaz produced, the points of them being in contact with the part of the tube occupied by gaz, I was again ſurpriſed, on the paſſing of a diſcharge, by an illumination of the whole tube, and the ruſhing up with conſiderable commotion of water, to occupy about two-thirds of the ſpace filled by gaz.

The reſiduary air was found, as in the former experiment, to be rather worſe than atmoſpherical air.

It was concluded that the points of the compaſſes had attracted electrical fire from the wire to the ſides of the glaſs, and thereby kindled the hydrogen and oxygen gaz of decompounded water. But to determine this queſtion, I introduced into the ſame tube a mixture of one meaſure of oxygen and two meaſures of hydrogen gaz, to occupy nearly the ſame ſpace in the tube as the gaz had occupied; then paſſing an electrical diſcharge through it, no combuſtion was excited; but on paſſing a diſcharge, while the compaſſes were in contact with the tube, as juſt mentioned, an illumination and violent commotion were produced, with the ruſhing up of water, to leave only 1-8th of the gaz as a reſidue. On repeating this experiment with two meaſures of atmoſpherical air, and one of hydrogen gaz, combuſtion could not be excited; nor with one meaſure of atmoſpherical air, and two of hydrogen; but on adding to this laſt mixture one meaſure of oxygen gaz, the electrical diſcharge produced the phenomena of combuſtion juſt mentioned, with the ruſhing up of water, to occupy about two-thirds of the ſpace which was occupied by the gazes.

Experiment III. Having paſſed 12,000 diſcharges through water, with the apparatus of the preceding experiment, and thereby obtained only one-fifth of a cubical inch of gaz; and having obſerved that the quantity of gaz was not greater than it was when only 8000 diſcharges had been paſſed, and yet bubbles had been ſeen to be produced on each diſcharge, as copiouſly, or more ſo, by the laſt three or 4000 diſcharges, as before; I began to ſuſpect that part of the gaz had been deſtroyed during the proceſs, or had been abſorbed. While I was conſidering how to account for this diſappearance of gaz, and was at the ſame time looking at the tube through which the diſcharges were paſſing, I obſerved one of them to be attended with a diminution, inſtantly, of about one-fifth of the gaz produced, and with a ſlight exploſion. I was now ſure, from this phenomenon, and from the unequal augmentation of the bulk of the gaz at given times during the proceſs, that combuſtion had been excited ſeveral times before, not only in the preſent experiment, but perhaps in the former ones, without obſerving it. I conceived that a gradual combuſtion alſo very probably

bably took place in this proceſs by the kindling of bubbles of gaz in their aſcent through the water. I now perceived that the diſcharges ought to be produced more ſlowly, or the tubes to be wider, to allow the bubbles to paſs quite through the water, in order to avoid the aſcenſion of gaz during the proceſs. My calculation, alſo, that 35 to 40,000 diſcharges were requiſite to produce one cubical inch of gaz from water containing its uſual quantity of common air, was rendered much more vague by this aſcenſion, ſo often liable to be occaſioned.

To the gaz which remained in the tube in this experiment was added an equal bulk of nitrous gaz; the mixture diminiſhed to 1,5; and on adding to the reſidue half its bulk of oxygen gaz, and paſſing through it the electrical ſpark, no accenſion or diminution of bulk was produced. Hence all the hydrogen gaz and oxygen gaz, produced by the decompoſition of the water, had been burnt during the proceſs; the oxygen gaz thus detected being conſidered to be only that expelled from the water.

Experiment IV. By means of electrical diſcharges with the apparatus uſed in the preceding experiment, I obtained gaz from New-River water, letting it up into a reſervoir, as ſoon as about 1-20th of a cubic inch was produced, till I had collected 1-8th of a cubic inch. To this was added an equal bulk of nitrous gaz, on which the mixture diminiſhed to 1,2; and on the addition of a little more nitrous gaz, no further diminution took place. To this reſidue half its bulk of oxygen was added; and this mixture of gazes being well dried, by ſtanding over lime and boiled quickſilver, an electric ſpark was paſſed through it, by which a diminution of one-ſixth of its bulk took place. A little dew was then ſeen upon the ſides of the tube where the quickſilver had riſen; and with the aid of a lens the ſame appearance was perceived on the part of the tube containing the reſidue of gaz.

It may now be expected, that I ſhould have made the experiment with this apparatus on diſtilled water, freed from its air, not only by long boiling or the air pump, but by ſending through it ſeveral hundred electrical diſcharges. It would alſo have been, to ſome perſons, more ſatisfactory, if the experiment had been made upon a larger ſcale, ſo as to have produced the combuſtion of a much larger quantity of gaz, and conſequently have produced a greater quantity of water. As, however, I apprehend, the experiments contained in this paper, when well conſidered by competent judges, will be found to explain the nature of the gaz procured from water by electric diſcharges; and as another very important ſubject demands my attention, the honour of more ſplendid and convincing experiments muſt be reſerved for other enquirers. If the ſame ſacrifices be made by them, as have been made in performing the preſent experiments, I think it is ſcarcely poſſible but that ſtill further light concerning the compoſition of water ſhould be procured, as well as concerning oils, alcohol, acids, &c.; to the inveſtigation of the compoſition of which, the mode of analyſis and ſyntheſis here indicated may be applied.

SECTION III.

On the Mode of Action of Electric Diſcharges.

THE mere concuſſion by the electric diſcharges, appears to extricate not only the air diſſolved in water, which can be ſeparated from it by boiling and the air pump, but alſo that which

which remains in water, notwithstanding these means of extricating it have been employed.

The quantity of this air varies in the same, and in different waters, according to circumstances. New-River water from the cistern yielded one-fifth of its bulk of air, when placed by Mr. Cuthbertson under the receiver of his most powerful air pump; but in the same situation, New-River water taken from a tub exposed to the atmosphere for some time yielded its own bulk of air. Hence the gaz procured by the first one, two, or even three hundred explosions in water containing its natural quantity of air, is diminished very little by an electric spark.

The gaz thus separable from water, like atmospherical air, consists of oxygen and nitrogen, or azotic gaz; which may be in exactly the same proportions as in atmospherical air; for the water may retain one kind of gaz more tenaciously than the other; and on this account the air separated may be better or worse than atmospherical air at different periods of the process for extricating it.

With regard to the gaz which instantly disappears on passing through it an electric spark, its nature is shewn by *(a)* this very property of thus diminishing; and by the following properties:

(b) A certain quantity of nitrous gaz instantly disappeared, apparently composing nitrous acid, on being added to the gaz *(a)* p. 303, Exp. IV.

Oxygen gaz being added to the residue after saturation with nitrous gaz, and an electric spark being applied to the mixture of gazes, well dried, a considerable diminution immediately took place, and water was produced.

(c) Combustion from hydrogen and oxygen gaz took place when the tube was about three-fourths full of gaz, p. 301, Exp. I.; which was confirmed by passing an electric discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, p. 302, Exp. II.

(d) Combustion from hydrogen and oxygen gaz took place when the points of the compasses were accidentally applied to the part of the tube containing gaz, p. 302; which was confirmed by passing a discharge, under the same circumstances, through a mixture of hydrogen and oxygen gaz, while the points of the compasses were applied to the tube.

(e) The observations made of the kindling of gaz, in small quantities, from time to time, during the process of obtaining it, particularly while it was ascending in chains of bubbles, or was adhering to the funnel of the tube, p. 302, Exp. III. confirm the evidence in favour of this gaz being hydrogen and oxygen gaz.

The evidence contained under the above heads *(a)*—*(e)* considered singly and conjunctively, I apprehend, must be admitted by the most rigorous reasoner, or severest logician, to be demonstrative that hydrogen and oxygen gaz were produced by passing electric discharges through water.

With regard to the origin and mode of production of these two gazes, our present observations and experiments do *not* afford complete demonstrative evidence; but although some hypotheses must be admitted, I conceive that the body of evidence we possess can afford a satisfactory interpretation of the phenomena.

It is demonstrable that the electric discharge and spark contain fire; and very probably they are merely a state of fire. Fire may be considered as consisting of caloric and light; but it is at least as consistent with the phenomena, and it is more philosophical, because it is more

more fimple, to confider light not as a diftinct fpecies of matter, but as a ftate of caloric, which is manifefted by its producing the fenfation termed vifion. It is demonftrable alfo, that the ponderable parts of oxygen and hydrogen gaz conftitute water. There is ftrong evidence that thefe gazes confift of a peculiar fpecies of matter which is ponderable; and of imponderable matter, which is that which is feparable from them in the ftate of fire, or flame. If fire could be applied in a fufficiently denfe ftate and quantity, it is warrantable, from a full induction of facts, to conclude, that it is able to difunite the conftituent fubftances of all the compound fubftances in nature.

[*To be concluded in the next Number.*]

IV.

Experimental Refearches to afcertain the Nature of the Procefs by which the Eye adapts itfelf to produce diftinct Vifion.

THE ftructure of the eye, and particularly its provifions for adjuftment to produce diftinct vifion, have engaged the attention of feveral philofophers during the laft five years, who have delivered papers on thefe fubjects to the Royal Society. I purpofe to give the fubftance of their difcoveries in the prefent communication.

In the year 1793, Mr. Thomas Young's Obfervations on Vifion appeared in the Tranfactions. He gives a fhort fummary of the theories of adjuftment propofed by various earlier authors. Kepler fuppofed the ciliary proceffes to contract the diameter of the eye, and lengthen its axis by a mufcular power. Defcartes imagined the fame effect to be produced by a mufcularity of the cryftalline humour, but did not attribute much to the change of figure which he fuppofed to take place in that lens. De la Hire, and alfo Haller, adopted the opinion that the eye undergoes no change to produce diftinct vifion, but the contraction and dilatation of the pupil. Pemberton fuppofed the exiftence of mufcular fibres in the cryftalline humour, by which the curvatures of its furfaces are changeable. Dr. Porterfield conceived that the ciliary proceffes draw forward the cryftalline, and render the cornea more convex. Dr. Jurin maintained the hypothefis, that the uvea, at its attachment to the cornea, is mufcular, and capable of increafing the convexity of that humour, by its contraction towards the axis of the eye. Mufchenbroek conjectures that the relaxation of his ciliary zone, which appears to be nothing but the capfule of the vitreous humour, permits the coats of the eye to pufh forward the cryftalline and cornea. And, laftly, an elongation of the axis of the eye has been fuppofed to be produced by the preffure of the external mufcles, efpecially the two oblique mufcles; and, on the other hand, the mufcular action has been fuppofed to produce a contrary effect, namely, a contraction of the axis.

In the enumeration of thefe refpective hypothefes, Mr. Young makes remarks tending to their feveral refutation. I have not tranfcribed them, principally becaufe they chiefly tend to fhew that effects of this nature deferve, if poffible, to be fubmitted to actual experiment.

From the confideration of the whole fubject, our author concluded that the rays of light emitted by objects at a fmall diftance could only be brought to foci on the retina by a nearer approach of the cryſtalline to a ſpherical form; and he could imagine no other power capable of producing this change than a muſcularity of part or the whole of its capſule.

But on cloſely examining, with the naked eye, the cryſtalline from an ox turned out of its capſule, he diſcovered a ſtructure which he thinks ſufficient to remove all the difficulties with which this branch of optics has long been obſcured. The cryſtalline lens of the ox is an orbicular, convex, tranſparent body, compoſed of a conſiderable number of ſimilar coats, of which the exterior cloſely adhere to the interior. Each of theſe coats conſiſts of ſix muſcles, intermixed with a gelatinous ſubſtance, and attached to ſix membranous tendons. Three of the tendons are anterior, three poſterior; their length is about two-thirds of the ſemi-diameter of the coat; their arrangement is that of three equal and equidiſtant rays, meeting in the axis of the cryſtalline; one of the anterior is directed towards the outer angle of the eye, and one of the poſterior towards the inner angle; ſo that the poſterior are placed oppoſite to the middle of the interſtices of the anterior; and planes paſſing through each of the ſix, and through the axis, would mark on either ſurface ſix regular equidiſtant rays. The muſcular fibres ariſe from both ſides of each tendon; they diverge till they reach the greateſt circumference of the coat; and having paſſed it, they again converge till they are attached reſpectively to the ſides of the neareſt tendons of the oppoſite ſurface. The anterior or poſterior portion of the ſix viewed together exhibits the appearance of three penniform-radiated muſcles. The anterior tendons of all the coats are ſituated in the ſame planes, and the poſterior ones in the continuations of theſe planes beyond the axis. Such an arrangement of fibres can be accounted for on no other ſuppoſition than that of muſcularity. This maſs is encloſed in a ſtrong membranous capſule, to which it is looſely connected by minute veſſels and nerves; and the connection is more obſervable near its greateſt circumference. Between the maſs and its capſule is found a conſiderable quantity of an aqueous fluid, the liquid of the cryſtalline.

He conceives, therefore, that when the will is exerted to view an object at a ſmall diſtance, the influence of the mind is conveyed through the lenticular ganglion formed from branches of the third and fifth pairs of nerves, by the filaments perforating the ſclerotica to the orbiculus ciliaris, which may be conſidered as an annular plexus of nerves and veſſels; and thence, by the ciliary proceſſes, to the muſcle of the cryſtalline; which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The diſpoſition of fibres in each coat is admirably adapted to produce this change; for ſince the leaſt ſurface that can contain a given bulk, is that of a ſphere (Simpſon's Fluxions, p. 486), the contraction of any ſurface muſt bring its contents nearer to a ſpherical form. The liquid of the cryſtalline ſeems to ſerve as a ſynovia in facilitating the motion, and to admit a ſufficient change of the muſcular part, with a ſmaller motion of the capſule.

To aſcertain whether theſe fibres can produce an alteration in the form of the lens ſufficiently great to account for the known effects, this author ſtates, that the diameter of the cryſtalline of the ox is 700 thouſandths of an inch, the axis of its anterior ſegment 225; of its poſterior 350. In the atmoſphere it collects parallel rays, at the diſtance of 235 thouſandths. From theſe data he finds, by means of Smith's Optics, article 366, and a quadratic, that its ratio of refraction is as 10000 to 6574. Haukſbee makes it only as 10000 to 6832,7; but we cannot depend on his experiment, ſince he ſays, that the image of the candle which he viewed was enlarged and diſtorted; a circumſtance that he does not explain,

explain, but which was evidently occasioned by the greater density of the central parts. Supposing, with Hauksbee and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the crystalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the distance of 1226 thousandths of an inch; but the distance of the retina from the crystalline is 550 thousandths; and that of the anterior surface of the cornea 250; hence (by Smith, art. 367.) the focal distance of the cornea and aqueous humour alone must be 2329. Now supposing the crystalline to assume a spherical form, its diameter will be 642 thousandths, and its focal distance in the eye 926. Then, disregarding the thickness of the cornea, he deduces (by Smith, art. 370.) that such an eye will collect those rays on the retina which diverge from a point at the distance of 12 inches and 8-10ths. This is a greater change than is necessary for an ox's eye: for, if it be supposed capable of distinct vision at a distance somewhat less than 12 inches, yet it probably is far short of being able to collect parallel rays. The human crystalline, he says, is susceptible of a much greater change of form.

The ciliary zone may admit of as much extension as this diminution of the diameter of the crystalline will require; and its elasticity will assist the cellular texture of the vitreous humour, and perhaps the gelatinous part of the crystalline, in restoring the indolent form.

He questions whether the retina takes any part in supplying the lens with nerves; but from the analogy of the olfactory and auditory nerves, he thinks it more reasonable to suppose that the optic nerve serves no other purpose than that of conveying sensation to the brain.

Although a strong light and close examination are required in order to see the fibres of the crystalline in its entire state, yet their direction was demonstrated, and their attachment shewn, without much difficulty. In a dead eye, the tendons are discernible through the capsule, and sometimes the anterior ones even through the cornea and aqueous humour. When the dry crystalline falls, it very frequently separates, as far as the centre, into three portions, each having a tendon in its middle. If it be carefully stripped of its capsule, and the smart blast of a fine blow-pipe be applied close to its surface, in different parts, it will be found to crack exactly in the direction of the fibres above described, and all these cracks will be stopped as soon as they reach either of the tendons. The application of a little ink to the crystalline is of great use in shewing the course of the fibres.

Mr. Young was not at first aware that the muscularity of the crystalline had ever been suspected, either by Descartes or any other person. But the laborious and accurate Lewenhoek, to whose writings he refers, has described the course of the fibres of the crystalline in a variety of animals, and has even gone so far as to call it a muscle. He did not, however, attempt to account for the focal adjustment of the eye from its muscularity.

The remaining part of Mr. Young's paper is employed in the solution of some optical queries, not immediately relating to the present object.

In Plate XIII, Fig. 1. represents a vertical section of the ox's eye, of the natural size. A, the cornea covered by the tunica conjunctiva; BCB, the sclerotica, covered at BB by the tunica albuginea and tunica conjunctiva; DD, the choroid, consisting of two laminas; EE, the circle of adherence of the choroid and sclerotica; FG, FG, the orbiculus ciliaris;

 HH, HK,

HI, HK, the uvea; its anterior surface the iris; its posterior surface lined with pigmentum nigrum; IK, the pupil; HL, HL, the ciliary processes covered with pigmentum nigrum; MM, the retina; N, the aqueous humour; O, the crystalline lens; P, the vitreous humour; QR, QR, the zona ciliaris; RS, RS, the annulus mucosus.

Fig. 2. The structure of the crystalline lens, as viewed in front, likewise of the natural size;—Fig. 3. a side view of the crystalline.

In the following year, 1794, a communication was made to the Royal Society, by Everard Home, Esq. F.R.S. of some facts relative to a preparation for the Croonian Lecture, by the late Mr. John Hunter. He states that this celebrated anatomist had for many years entertained the notion, that the crystalline humour was enabled by its own internal actions to adjust itself so as to adapt the eye to different distances; and when the tænia hydatigena first came under his observation as a living animal, he was surprised to see the quantity of contraction that took place in a membrane devoid of muscular fibres; but made use of the fact in his investigation of the structure of the crystalline humour of the eye. Some time after this, he discovered the fibrous structure of the crystalline humour in the eye of the cuttle-fish, in which it is peculiarly distinct; and thence he was led to consider the exterior part of this humour as similar in all animals. It was his intention to have ascertained by experiment, whether muscular action does in fact take place; and having found that a certain degree of heat applied through the medium of water will excite muscular action, after almost every other stimulus had failed, it was proposed to apply this to the crystalline humour, and ascertain its effects. The crystalline humour taken from animals recently killed must be considered as being still alive. Such humours were to be immersed in water of different temperatures, and placed in such a manner as to form the image of a lucid, well-defined object, by a proper apparatus for that purpose, so that any change of the place of that image, from the stimulating effects of the warm water upon the humour, would be readily ascertained. These were the experiments which Mr. Hunter had instituted and begun, but in which he had not made sufficient progress before his death to enable him to draw any conclusions.

This communication contains an unfinished letter from Mr. Hunter to Sir Joseph Banks, containing some prefatory observations leading to the statement made by Mr. Home. It appears, in fact, as that gentleman remarks, that the discovery of a fibrous appearance in the crystalline appertains to Lewenhoek; but that the discovery of an eye in which the structure is uncommonly distinct, is due to Mr. Hunter.

To this paper is annexed a plate exhibiting two sections of the crystalline humour of the cuttle-fish.

In the second part of the Transactions for the same year, I find a paper of some length on vision, by Dr. Hossack. This author shews in the first place, that the enlargement or contraction of the pupil of the eye is insufficient to produce the adjustment by which the rays of light shall converge to a point in the retina; and in fact a slight observation of what happens is sufficient to shew that the variations of the pupil are governed by different circumstances. He controverts Mr. Young's deductions with regard to the muscularity of the crystalline, and even disputes the facts. He quotes some authorities to shew that the eye is capable of accommodating itself to different distances without the assistance of the crys-

 talline,

talline, as after couching or extraction. For theſe and other reaſons his attention was more particularly directed to the external muſcles.

Upon carefully removing the eyelids, the muſcles of the eye preſent themſelves to view in number ſix; four called recti or ſtraight, and two oblique, ſo named from their direction. In Plate XIII. Fig. 4, AAAA repreſent the tendons of the recti muſcles, where they are inſerted into the ſclerotic coat, at the anterior part of the eye. B, the ſuperior oblique, or trochlearis, as ſometimes called, from its paſſing through the loop or pulley connected to the lower angle of the orbit or notch in the os frontis; it paſſes under the ſuperior rectus muſcle, and backwards to the poſterior part of the eye, where it is inſerted by a broad flat tendon into the ſclerotic coat. C, the inferior oblique, ariſing tendinous from the edge of the orbit or proceſs of the ſuperior maxillary bone, paſſing ſtrong and fleſhy over the inferior rectus, and backwards under the abductor to the poſterior part of the eye, where it is alſo inſerted by a broad flat tendon into the ſclerotic coat. DDD, the fat in which the eye is lodged. In Figure 5, the bones forming the external ſide of the orbit, with a portion of the fat, are removed, by which we have a diſtinct view of the abductor. ABC, three of the recti muſcles, ariſing from the back part of the orbit, paſſing ſtrong, broad and fleſhy over the ball of the eye, and inſerted by flat broad tendons into the ſclerotic coat, at its interior part. D, the tendon of the ſuperior oblique muſcle. E, the inferior oblique. In Fig. 6, A repreſents the abductor of the eye. B, the fleſhy belly of the ſuperior oblique, ariſing ſtrong, tendinous and fleſhy from the back part of the orbit. C, the optic nerve. D and E, the recti muſcles.

The uſe aſcribed to theſe different muſcles is that of directing the axis of the eye towards the different objects, or to expreſs the paſſions of the mind. But Dr. Hoſſack rationally infers, from the general application of the combined forces of muſcles through the whole of the animal ſyſtem, that this ſet of muſcles, which he conceives to be well adapted to produce the focal adjuſtment, may alſo be employed on that object. He aſſumes, as the neceſſary conſequence of contraction in theſe muſcles, that the axis of the eye will be elongated, and the elaſtic cornea rendered more convex; both which circumſtances would tend to preſerve diſtinctneſs of viſion with regard to near objects. For as ſuch objects afford a focal image more diſtant from the refracting ſurface, through which the light may paſs, the elongation of the axis will be of advantage, by removing the retina further back; and the increaſed convexity and ſhortened focus of the cornea will conduce to the ſame end.

How far the action of the recti muſcles might produce an elongated figure, is perhaps capable of diſpute; though this effect will probably be admitted without heſitation, as a conſequence of the contraction of the oblique muſcles. But to put the matter out of doubt, whether this organ be capable of having its focal adjuſtment conſiderably varied by external preſſure, our author applied the common ſpeculum oculi to his own eye. With a very moderate preſſure, while directing his attention to an object at the diſtance of about twenty yards, he ſaw it diſtinctly, as alſo the different i[illegible]mediate objects; but endeavouring to look beyond it, every thing appeared confuſed: h[illegible] increaſed the [illegible]ſſure conſide[illegible]ly: in conſequence of which he was enabled to ſee objec[illegible]tinctly, though [illegible] than the natural focal diſtance. For example: he he[illegible]fore his eye, at t[illegible]vo inches, a printed book. In the natural ſtate of the eye, he could neit[illegible] ner letters; but, upon making preſſure with the ſpeculum, he was enabled to diſtinguiſh both the lines and the letters of the book with eaſe.

Such being this author's conception of the action and effect of the external muſcles, he has proceeded to apply the doctrines in explanation of the changes the eye is known to undergo at different periods of life or habits of occupation, from their ſtrength in early life, their debility in old age, and their habitual action from uſe. Theſe and other general facts are accounted for with conſiderable addreſs.

Mr. Home, the brother-in-law of Hunter, and vindicator of his poſthumous fame, was appointed to read the Croonian lecture on muſcular motion, for the ſeſſion of the Royal Society beginning in the year 1794. His lecture is peculiarly valuable for the experimental reſults it exhibits. In proſecuting the enquiry projected by Mr. Hunter, he had the great advantage of the aſſiſtance of his friend Mr. Ramſden, who, in converſing upon the different uſes of the cryſtalline humour, made the following obſervations:

He ſaid that, as the cryſtalline humour conſiſts of a ſubſtance of different denſities, the central parts being the moſt compact, and from thence diminiſhing in denſity gradually in every direction, approaching the vitreous humour on one ſide, and the aqueous humour on the other, its refractive power becomes nearly the ſame with that of the two contiguous ſubſtances. That ſome philoſophers have ſtated the uſe of the cryſtalline humour to be, for accommodating the eye to ſee objects at different diſtances; but the firmneſs of the central part, and the very ſmall difference between its refractive power near the circumference, and that of the vitreous or the aqueous humour, ſeemed to render it unfit for that purpoſe; its principal uſe rather appearing to be for correcting the aberration ariſing from the ſpherical figure of the cornea, where the principal part of the refraction takes place, producing the ſame effect that, in an achromatic object-glaſs, we obtain in a leſs perfect manner by proportioning the radii of curvature of the different lenſes. In the eye the correction ſeems perfect, which in the object-glaſs can only be an approximation; the contrary aberrations of the lenſes not having the ſame ratio; ſo that, if this aberration be perfectly corrected, at any given diſtance from the centre, in every other it muſt be in ſome degree imperfect.

Purſuing the ſame compariſon: In the achromatic object-glaſs we may conceive how much an object muſt appear fainter from the great quantity of light loſt by reflection at the ſurfaces of the different lenſes, there being as many primary reflections as there are ſurfaces; and it would be fortunate if this reflected light was totally loſt. Part of it is again reflected towards the eye by the interior ſurfaces of the lenſes; which, by diluting the image formed in the focus of the object-glaſs, makes that image appear far leſs bright than it would otherwiſe have done, producing that milky appearance ſo often complained of in viewing lucid objects through this ſort of teleſcope.

In the eye, the ſame properties that obviate this defect ſerve alſo to correct the errors from the ſpherical figure, by a regular diminution of denſity, from the centre of the cryſtalline outward. Every appearance ſhews the cryſtalline to conſiſt of laminæ of different denſities; and if we examine the junction of different media, having a very ſmall difference of refraction, we ſhall find that we may have a ſenſible refraction without reflection. Now, if the difference between the contiguous media in the eye, or the laminæ in the cryſtalline, be very ſmall, we ſhall have refraction without having reflection: and this appears to be the

ſtate

ſtate of the eye; for although we have two ſurfaces of the aqueous, two of the cryſtalline, and two of the vitreous humour, yet we have only one reflected image; and that being from the anterior ſurface of the cornea, there can be no ſurface to reflect it back, and dilute an image on the retina.

This hypotheſis may be put to the teſt whenever accident ſhall furniſh us with a ſubject having the cryſtalline extracted from one eye, the other remaining perfect in its natural ſtate; at the ſame time we may aſcertain whether or no the cryſtalline is that part of the organ which ſerves for viewing objects at different diſtances diſtinctly. Seeing no reflection at the ſurface of the cryſtalline, might lead ſome perſons to infer that its refractive power is very inconſiderable; but many circumſtances ſhew the contrary: yet what it really is may be readily aſcertained by having the focal length and diſtance of a lens from the operated eye, that enables it to ſee objects the moſt diſtinctly; alſo the focal length of a lens, and its diſtance from the perfect eye, that enables it to ſee objects at the ſame diſtance as the imperfect eye: theſe data will be ſufficient whereby to calculate the refractive power of the cryſtalline with conſiderable preciſion.

Again, having the ſpherical aberration of the different humours of the eye, and having aſcertained the refractive power of the cryſtalline, we have data from whence to determine the proportional increaſe of its denſity as it approaches the central part, on a ſuppoſition that this property corrects the aberration.

An opportunity preſented itſelf for bringing the obſervations of Mr. Ramſden reſpecting the uſe of the cryſtalline lens, to the proof. A young man came into St. George's Hoſpital, with a cataract in the right eye. The cryſtalline lens was readily extracted, and the union of the wound in the cornea took place unattended by inflammation, ſo that the eye ſuffered the ſmalleſt degree of injury that can attend ſo ſevere an operation. The man himſelf was in health, 21 years of age, intelligent, and his left eye perfect: the other had been an uncommonly ſhort time in a diſeaſed ſtate, and 27 days after the operation appeared to be free from every other defect but the loſs of the cryſtalline lens.

A number of experiments were made on the imperfect eye, aſſiſted by a lens, and compared with the perfect eye. The aim of theſe trials, which were judiciouſly varied, was to aſcertain whether the eye which had been deprived of the cryſtalline lens was capable of adjuſting itſelf to diſtinct viſion at different diſtances. Among other reſults, the perfect eye, with a glaſs of $6\frac{1}{2}$ inches focus, had diſtinct viſion at three inches; the near limit was $1\frac{7}{8}$ inch, the diſtant limit leſs than 7 inches. The imperfect eye, with a glaſs $2\frac{2}{10}$ths inches focus, with an aperture $\frac{3}{40}$ths of an inch, had diſtinct viſion at $2\frac{7}{8}$ inches, the near limit $1\frac{7}{8}$ inch, and the diſtant limit 7 inches. The accuracy with which the eye was brought to the ſame point, on repeating the experiments, proved it to be uncommonly correct; and as he did not himſelf ſee the ſcale uſed for admeaſurement, there could be no ſource of fallacy. From the reſult of this experiment it appears that the range of adjuſtment of the imperfect eye, when the two eyes were made to ſee at nearly the ſame focal diſtance, exceeded that of the perfect eye. Mr. Ramſden ſuggeſted a reaſon why the point of diſtinct viſion of the imperfect eye might appear to the man himſelf nearer than it was in reality; namely, that from the imperfection of this organ, he might find it eaſier to read the letters when they ſubtended a greater angle than at his real point of diſtinct viſion. The experiments, however, appear to ſhew that the internal power of the eye, by which it is adjuſted to ſee at different

different distances, does not reside in the crystalline lens; at least not altogether; and that if any agency in this respect can be proved to reside in the crystalline, the other powers, whatever they may be, are capable of exertion beyond their usual limits, so as to perform its office in this respect.

From these considerations, and in consequence of other reflections tending to shew that an elongation of the optical axis is not probably the means of adjustment, these philosophers directed their enquiries to ascertain how far the curvature of the cornea might be subject to change. They found by trial that this part of the organ possesses a degree of elasticity which is very considerable, both for its perfection and its range; and by anatomical dissection it was found, that the four straight muscles of the eye do in effect terminate in the cornea at their tendinous extremities; that the whole external lamina of the cornea could by gentle force be separated, by means of these muscles, from the eye; so that the tendons seem lost in the cornea, and this last has the appearance of a central tendon. It was also seen that the central part of the cornea is the thickest and the most elastic.

These were considerable advances towards establishing the hypothesis of adjustment by the external curve of the eye. It remained to be shewn, by experiments on the living subject, that this curve does really vary in the due direction, when the mind perceives the distinct visible sensation of objects at different distances. For this purpose Mr. Ramsden provided an apparatus, consisting of a thick board steadily fixed, in which was a square hole large enough to admit a person's face; the forehead and chin resting against the upper and lower bars, and the cheek against either of the sides; so that when the face was protruded, the head was steadily fixed by resting on three sides; and in this position the left eye projected beyond the outer surface of the board. A microscope, properly mounted, so as with ease to be set in every requisite position, was applied to view the cornea with a magnifying power of thirty times. In this situation, the person whose eye was the object of experiment was desired to look at the corner of a chimney, at the distance of 235 yards, through a small hole in a brass plate, fixed for that purpose, and afterwards to look at the edge of the hole itself, which was only six inches distant. After some management and caution, which the delicate nature of these experiments requires, the motion of the cornea, which was immediately perceptible, became very distinct and certain. The circular section of its surface remained in a line with the wire in the field of the microscope, when the eye was adjusted to the distant object, but projected considerably beyond it when adapted to the near one. When the distant object was only 90 feet from the observer, and the near object six inches, the difference in the prominence of the cornea was estimated at 1-800th of an inch. These experiments were repeated and varied at different times and on different subjects. The observer at the microscope found no difficulty in determining, from the appearance of the cornea, whether the eye was fixed on the remote or the near object.

From these different experiments Mr. Home considers the following facts to have been ascertained:

1. That the eye has a power of adjusting itself to different distances when deprived of the crystalline lens; and therefore the fibrous and laminated structure of that lens is not intended to alter its form, but to prevent reflections in the passage of the rays through the surfaces of media of different densities, and to correct spherical aberration.

2. That the cornea is made up of laminæ; that it is elastic, and when stretched is capable of

of being elongated 1-11th part of its diameter, contracting to its former length immediately upon being left to itfelf.

3. That the tendons of the four ftraight mufcles of the eye are continued on to the edge of the cornea, and terminate, or are inferted, in its external laminæ: their action will therefore extend to the edge of the cornea.

4. That in changing the focus of the eye from feeing with parallel rays to a near diftance, there is a vifible alteration produced in the figure of the cornea, rendering it more convex; and when the eye is again adapted to parallel rays, the alteration by which the cornea is brought back to its former ftate is equally vifible.

The remaining part of Mr. Home's lecture contains fome obfervations upon the mufcular and elaftic powers, which by their oppofition produce fo curious an effect in the adjuftment of the eye. The feparate action of the mufcles produces a change in the direction of the axis of the eye. A fmall force of contraction in the whole fyftem will fteady the eye, and a greater force will comprefs the lateral and pofterior parts of the eye, and render the cornea more convex. The experiments prove that the eye-ball cannot recede in its orbit by thefe actions. Other inftances in the animal œconomy, by means of which the expenditure of mufcular action is faved by the operation of elaftic antagonifts, are alfo ftated by this author. The exertion required to adjuft the eye to near diftances, and the eafe with which it was adapted to remote objects, prove that the firft was a pofitive action, and the fecond a relief. The defect of elafticity inferred to arife from age is happily applied to explain the changes of vifion which take place in advanced life.

Two other communications to the Royal Society by this philofopher, and one by Mr. Smith on the eyes of birds, remain to be confidered; but, on account of the length of this communication, I fhall for the prefent defer them.

V.

Concerning the Properties of the Sulphureous Acid, and its Combinations with earthy and alkaline Bafes. By Citizens FOURCROY *and* VAUQUELIN *.

SEVERAL philofophers have paid attention to the properties of the fulphureous acid, and fome of its combinations; but no one has given a complete account of this acid.

Berthollet is almoft the only chemift who has opened this inveftigation. He is the firft who publifhed any accurate account of the fubject †.

We fhall not in this place defcribe either the apparatus or method of preparing this acid, becaufe both are well known to fuch as are moderately acquainted with chemiftry.

I. *Phyfical Properties of the Sulphureous Acid.*

THIS acid conftitutes a permanent elaftic fluid at the ordinary preffure and temperature of our atmofphere. Its odour is ftrong and fuffocating. It cannot maintain combuftion, nor the refpiration of animals.

Bergman, in his Treatife on Elective Attraction, affirms that the fulphureous acid, pre-

* Journal de l'Ecole Polytechnique, cahier IV. p. 445.

† Annales de Chimie, II. 54.

pared in the pneumatic apparatus over mercury, cannot be reduced to the liquid ſtate by any known means. Monge and Clouet affirm, on the contrary, that by the application of extreme cold, and a ſtrong preſſure exerted at the ſame time on this gas, they rendered it liquid.

Its ſpecific gravity, according to Bergman, is 0,00246, and 0,00251, according to Lavoiſier; which correſponds with 1.508 grain the inch cube, and 4 oz. 5 gros the foot cube (French weights and meaſures).

II. *Chemical Properties of the Sulphureous Acid.*

Action of Caloric.] Prieſtley and Berthollet affirm, that the ſulphureous acid gas, expoſed to an elevated temperature, depoſits a portion of ſulphur, and becomes converted into ſulphuric acid. Bergman relates the ſame fact. This experiment, repeated in ſeveral different ways, did not afford us the ſame reſult.

III. *Action of Oxygene Gas.*

ONE part of dry oxygene gas, and two parts of ſulphureous acid gas, prepared in the mercurial apparatus, and mixed together, ſuffered no remarkable change during the courſe of ſeveral months. If a ſmall quantity of water be added to the mixture, a ſucceſſive abſorption, after the diminution which is produced by the combination of a portion of the ſulphureous acid, is perceived; which proves the exiſtence of a true combination between theſe two bodies. In fact, when the mixture is waſhed, after the expiration of ſeveral months, the reſidue of oxygene is found to be leſs than the original quantity.

The attraction of the water for the ſulphuric acid is therefore favourable to the union of the oxygene with the ſulphureous acid. We were not able, however, to convert the whole of the ſulphureous acid into ſulphuric acid, though in all our experiments oxygene gas remained in the apparatus.

The ſulphureous acid is immediately converted into ſulphuric acid, by paſſing a mixture of this gas and of oxygene gas through an ignited tube. A very denſe white fume is formed, which becomes condenſed into the liquid form, in a bottle placed for that purpoſe at the other extremity of the tube.

It ſeveral times happened, when by accident we had uſed the exact proportions, that the two gaſes were entirely deſtroyed, and not an atom of (elaſtic) reſidue was perceived.

IV. *The Action of Water.*

WHEN the ſulphureous acid gas was paſſed into water cooled by means of ice, the combination was made with ſuch rapidity that not a ſingle bubble roſe to the ſurface of the liquor until it was ſaturated. The ice is ſpeedily melted; which proves that much caloric is diſengaged. The water acquires at this temperature the 0,15th part of its weight. Its ſpecific gravity, compared with that of pure water, is as 1020 to 1000.

Water thus ſaturated with the ſulphureous acid, and expoſed to the temperature of 15 degrees above zero (Reaumur), immediately becomes filled with an infinity of ſmall bubbles, which ſucceſſively increaſe, and riſe to the ſurface. If a bottle full of this acid be plunged in hot water, it boils with aſtoniſhing rapidity, and loſes by this operation part of its odour and acidity. It cannot, however, be eaſily deprived of theſe qualities, even by

2 boiling.

boiling. This acid freezes at a few degrees below zero, or the freezing point of water; and what may appear aftonifhing is, that not the fmalleft portion of gas is difengaged, as happens when the other gafes are diffolved in water.

Thefe experiments prove, 1. That the colder the water the more it abforbs of fulphureous acid. 2. That the combination of thefe bodies is accompanied with heat. 3. That fuch a combination cannot preferve at a more elevated temperature all the acid it was charged with at a lower degree of heat. 4. That this combination paffes to the folid ftate by cooling, without undergoing decompofition; a fact which fhews a great affinity of water for the fulphureous acid, and a weak attraction of this laft for caloric.

V. *Action of Acids upon the Sulphureous Acid.*

Action of the Sulphuric Acid.] We caufed fulphureous acid gas to pafs through the fulphuric acid cooled by a mixture of ice and falt. In proportion as the acid gas was extricated, it combined with the fulphuric acid without one fingle bubble arriving at the upper part. Soon afterwards the whole of the acid became congealed; notwithftanding which the combination continued to be made.

From time to time, the tube through which the fulphureous acid paffed was obftructed, and required to be withdrawn out of the fluid. By this treatment the matter flowed, and a quantity of gas was difengaged in the form of bubbles. The folid combination had no very perceptible fmell while it retained the form of ice; but when a portion was taken out, and laid upon a plate of glafs, it exhibited an effervefcence like that of marble when an acid is poured on it, and foon afterwards it became liquid and very odorant.

When the temperature of the mixture of falt and ice had rifen to the freezing-water point, part of the folid mafs became liquid, and bubbles were formed which rofe up in the empty part of the vafe; but as this was exactly clofed, they cryftallized on the fides in the form of the leaves of fern.

Action of the Nitric Acid.] The fulphureous acid decompofes the nitric acid, particularly when this laft is in a concentrated ftate. The fluid immediately becomes red, and foon afterwards hot, with a difengagement of nitrous gas frequently mixed with a fmall quantity of fulphureous acid. The fulphureous acid is changed by this operation into fulphuric acid, for it abundantly precipitates the muriate of barytes. Hence it is proved, that the fulphureous acid has more affinity with oxygene than the nitrous oxide. This truth is confirmed by the converfion of fulphur into fulphuric acid by means of the nitric acid.

Action of the Oxygenated Muriatic Acid.] If the oxygenated muriatic acid gas and the fulphureous acid gas be brought into contact, white fumes are afforded; the two gafes become immediately liquid, and lofe their odour, if they have been employed in fuitable proportions. The fame effects take place when thefe two acids are mixed together in the liquid ftate, namely, the lofs of odour and colour. From the action of thefe two bodies on each other, fulphuric acid and common muriatic acid are produced. The theory of this operation is nearly the fame as that of the preceding experiment.

VI. *Action of combuftible Bodies on the Sulphureous Acid.*

The Action of Hydrogene.] Thefe two fubftances have no mutual action in the cold; but if they be paffed through a tube of glafs or porcelaine well heated, in the proportion of three parts by meafure of hydrogene gas and one part of fulphureous acid gas, the latter is

 decompofed.

decompofed. Sulphurated hydrogene gas is formed; and at the extremity of the tube oppofite to that at which the gafes were introduced, very abundant cryftals of fulphur are depofited.

Hydrogene therefore, at an elevated temperature, has more affinity with oxygene than fulphur has. Confequently it is not probable, as fome perfons imagine, that fulphur may be burned by means of water to convert it into fulphuric acid.

The Action of Phofphorus.] Phofphorus does not alter the fulphureous acid. When thefe two fubftances are ftrongly heated in a glafs tube, nothing paffes which indicates the decompofition of the fulphureous acid. The phofphorus becomes fixed in cooling in the form of tranfparent drops which do not contain an atom of fulphur, and the fulphureous acid gas is ftill found to poffefs all its properties.

In this cafe, therefore, there is a ftronger adhefion between the fulphur and oxygene than between the phofphorus and the fame principle. And accordingly this combuftible fubftance, heated with fulphuric acid, does not deprive it of more oxygene than exceeded the conftitution of the fulphureous acid.

Action of Phofphorated Hydrogene Gas.] As foon as the fulphureous acid gas and phofphorated hydrogene gas come into contact, a white fume appears, and they lofe their fluid ftate. Plates of a yellow matter are precipitated on the fides of the containing veffel, which take fire on a hot iron, firft, in the manner of phofphorus, and afterwards with the characters of fulphur. It follows from this experiment, that the hydrogene is the only fubftance which burns, or combines with oxygene, in thefe circumftances, fince the gafes lofe their elafticity, and the phofphorus and fulphur are found combined together.

Action of Sulphurated Hydrogene Gas.] The fulphureous acid gas is decompofed by the fulphurated hydrogene gas. The hydrogene takes the oxygene from the fulphur, and this laft principle is feparated on both fides. It therefore forms a very abundant depofition. When thefe gafes are diffolved in water, they mutually undergo the fame decompofition, and the fulphur then precipitates to the bottom of the liquor *. If fuitable quantities of thefe folutions be taken, the odour of both difappears.

Action of Carbone.] We paffed fulphureous acid gas through a tube of glafs containing ignited charcoal. At the extremity of the apparatus cryftallized fulphur was depofited, and carbonic acid was produced, together with a fmall quantity of fulphurated hydrogene gas, from a portion of the water which was decompofed. This decompofition does not take place in the cold.

VII. Combinations of the Sulphureous Acid with Alkalis.

THE fulphureous acid readily unites with alkalis and earths. The generic name of fulphites has been given to thefe combinations. They may be prepared in two ways; either by prefenting the aqueous folution of the fulphureous acid to the bafes, or by applying the acid in the ftate of gas to thefe bodies, either diffolved or mixed with water. The latter method is preferable, for feveral reafons too long to be detailed here.

The fulphureous acid being very different from the fulphuric acid, it may eafily be conceived that its combinations ought to poffefs properties not at all refembling thofe of the fulphates. It will in fact be feen, that thefe falts poffefs peculiarities of tafte, folubility and form, which belong only to themfelves; and that they are fubject to laws of attraction and decompofition altogether different from thofe of the fulphates.

* Fourcroy, Analyfe de l'Eau d'Enghien.

The

The proportions of principles which conſtitute the ſulphites not having been yet determined, we endeavoured to aſcertain a method which might lead us as near as poſſible to the truth; but it could not be applied to all the kinds of ſulphite, on account of the diſagreement of their properties; ſo that we were under the neceſſity of employing ſeveral methods.

The Sulphite of Pot-aſh.] Sulphureous acid gas was paſſed into a ſaturated ſolution of very pure carbonate of pot-aſh, until the efferveſcence entirely ceaſed. During this combination, a ſmall quantity of caloric is diſengaged, and the ſolution cryſtallizes by cooling.

This ſalt is uſually white and tranſparent; ſometimes it is ſlightly yellow and ſemi tranſparent, if its ſolution has been very concentrated, and the cryſtallization confuſed. Its taſte is penetrating and ſulphureous, its figure that of a long rhomboidal plate; its cryſtallization often preſents ſmall needles diverging from a common centre.

When expoſed to a ſudden heat it decrepitates, and loſes its water of cryſtallization; afterwards by ignition it emits ſome vapours of ſulphureous acid; and at length a portion of ſulphur is ſeparated, and the reſidue is ſulphate of pot-aſh with a ſlight exceſs of alkali.

By expoſure to air it ſlightly effloreſces, becomes opake and hard, its penetrating ſulphureous taſte diſappears, and it acquires another, which is acrid and bitter. In this ſtate it no longer efferveſces with acids.

If this experiment be made in a cloſed apparatus with oxygene gas, it is found that the volume of the gas is diminiſhed, and that it is even totally abſorbed when the quantity is properly regulated. We ſee therefore that the ſulphite of pot-aſh may be converted into ſulphate by depriving it of a portion of its ſulphur by fire, or by introducing a quantity of oxygene at a low temperature.

The ſulphite of pot-aſh is ſoluble in a quantity of water nearly equal to its own maſs; and this ſolubility is increaſed by heat.

This ſalt is decompoſed by lime and by barytes, as may be ſhewn by pouring lime water into a ſolution of the ſulphite of pot-aſh. A white precipitate is afforded, which is the ſulphite of lime, and the pot-aſh remains diſengaged in the water.

The ſulphureous acid does not therefore follow the ſame laws of affinity as the ſulphuric acid, ſince this laſt adheres more ſtrongly to pot-aſh than to lime.

The alkalis do not change the nature of the ſulphite of pot-aſh.

Among the acids, ſome decompoſe the ſulphite of pot-aſh by ſeparating the ſulphureous acid; others change its nature without driving off its acid, but by affording a portion of oxygene, and converting it into ſulphuric acid. The firſt of theſe effects is produced by the ſulphuric, muriatic, phoſphoric and fluoric acids; the ſecond is effected by the nitric and the oxygenated muriatic acids. The acids of borax and of carbo do not occaſion any change in the cold.

When the ſulphuric acid is poured on the ſulphite of pot-aſh, a rapid efferveſcence takes place with a crackling noiſe, at the ſame time that much caloric is diſengaged.

The nitric acid, on the contrary, emits red vapours, mixed only with a ſmall quantity of ſulphureous acid, and the reſidue is compoſed of a mixture of the ſulphate and nitrate of pot-aſh.

The oxygenated muriatic acid mixed with the ſulphite of pot-aſh immediately loſes its ſmell, and the fluid is deprived of its ſulphureous taſte.

If the solution of the sulphite of pot-ash, and also that of the oxygenated muriatic acid, be both concentrated, crystals of the sulphate of pot-ash are formed immediately on the mixture.

Charcoal converts the sulphite of pot-ash into the sulphuret of pot-ash.

Salts with bases of pot-ash do not decompose this salt.

Those with bases of soda, except the borate and the carbonate, are decomposed by the sulphite of pot-ash.

All the other salts, of which the acids are stronger than the sulphureous acid, are equally decomposed by the sulphite of pot-ash.

The super-oxygenated nitrate and muriate of pot-ash, when heated with the dry sulphite of pot-ash, take fire, and are changed into sulphate.

Several metallic oxides act upon this salt. Some are entirely reduced to the metallic state; such as the oxides of gold, silver and mercury: others are brought nearer that state; such as those of lead, iron and manganese, at the maximum of oxygenation. There are others which change the nature of the sulphite of pot-ash in an opposite direction to those which take place in the foregoing cases; that is to say, which convert it into sulphate by depriving it of a certain quantity of sulphur, with which they form sulphurated oxides, such as the oxides of arsenic, and of iron slightly oxided; but in order that this operation may succeed, it is necessary to boil these substances a long time in water, and afterwards to add to the solution an acid, which occasions a coloured precipitate, at the same time that the smell of sulphurated hydrogene gas is emitted.

All the metallic solutions except the carbonates are decomposed by the sulphite of pot-ash; and as most of the metallic sulphites are insoluble, different coloured precipitates are formed, according to the nature of the metal and its state of oxidation.

[*To be continued.*]

VI.

A Memoir on the Nature of the Alum of Commerce, on the Existence of Pot-ash in this Salt, and on various simple or triple Combinations of Alumine with the Sulphuric Acid. Read before the National Institute of France. By Citizen VAUQUELIN *.

IN my memoir on the leucite or white grenate, I have announced that many natural earths and stones contain pot-ash in a state of combination; and I founded my opinion at that time on the impossibility of obtaining solid crystallized alum in octahedrons by the immediate combination of sulphuric acid and pure alumine, whatever precautions were taken to clear it of the excess of acid, without the addition of alkali.

The necessity of this addition has long been known in the alum works, more particularly in the treatment of the mother waters; and it was thought that the use of pot-ash in this circumstance was merely to saturate the excess of acid, which was supposed to prevent the crystallization of the alum. Nevertheless, the remark made by Bergman, that soda and

* This important Memoir is inserted in the Annales de Chimie, XXII. 258, whence I have translated it

lime,

fime, employed inftead of pot-afh or ammoniac in the treatment of the mother waters, do not favour the cryftallization of the alum, ought to have produced a change in the general opinion refpecting the action of pot-afh or ammoniac *. This learned chemift had likewife afcertained, that feveral alums decompofed by ammoniac afforded by evaporation the true fulphate of pot-afh, the bafis of which might be afforded, according to him, either from the argillaceous earth in which vegetables had been decompofed, or from the wood-afhes intentionally added, or, laftly, from a cafual mixture during the calcination of the ores. And he concluded from thefe obfervations, that the fulphates of pot-afh and of alumine unite together in the ftate of a triple falt †.

Though Bergman appears to fufpect that pot-afh is neceffary to the formation of alum; yet he does not venture to affirm it; as may eafily be feen in the courfe of his differtation: fo that this queftion remained ftill undecided. In fact, we fee by another paffage ‡, that Bergman falls into the common opinion, by confining the effect of the alkalis to the fimple faturation of the excefs of acid, exifting, according to him and all other authors, in the aluminous waters; and even by confidering the new falts formed by thefe alkaline fubftances as foreign bodies, lefs noxious in fact than the excefs of acid, but which cannot however be fold for alum §.

If the only effect of alkalis in the management of aluminous lixivia were to faturate the excefs of acid they are fuppofed to contain, it is evident that any other fubftance capable of abforbing this acid would anfwer the fame purpofe. But experience has proved the contrary, and it has been long known that this remarkable property belongs exclufively to pot-afh and ammoniac.

In order to explain this obfcure circumftance by experiment, I diffolved very pure alumine in fulphuric acid of equal purity. I evaporated the folution feveral times fucceffively, even to drynefs, for the purpofe of expelling the fuperabundant acid. I rediffolved the dry and pulverulent refidue in water, and reduced the folution to different degrees of fpecific gravity, with a view to feize the point moft favourable to cryftallization: but whatever precautions I took, I could not obtain any thing but a magma formed of faline plates without confiftence

* Notatu dignum eft, quod hoc cryftallifationis obftaculum alcali volatili æquè tollatur, non vero alcali minerali et calce. Bergman de Confect. Aluminis, pag. 325, tom. i. Opufcula.

† Hoc alcali quod ineft, vel ex ipfâ argillâ repetendum quæ vegetabilium putrefactorum refiduis fuit inquinata, vel ex cineribus ftudio additis, vel denique fub calcinatione et uftione fortuito immixtis. Interea hinc conftat, quod alumen et alcali vegetabile vitriolatum facilè connubium ineant, quò fal oritur triplex. Berg. ibid.

‡ Allata momenta fufpicionem movent, quod alcali vegetabile alumino perficiendo fit neceffarium, ideoque omne alumen perfectum inftar falis triplicis refpiciendum: fed hæc conjectura vacillat; nam eadem perfectio alcali volatili et fpontanea evaporatione obtinetur. Non tamen improbandam puto additionem alcali vegetabilis et depurati, nam heterogenea magis nocent quam juvant. Berg. ibidem.

§ Ut eo puriùs obtineatur alumen in alterâ cryftallizatione nonnullis in locis additamenta ufurpantur alcalina, calx et urina. Scilicet multorum annorum experientiâ compertum eft, lixivium aliquando tantam et adquirere confiftentiam (quod in officinis pinguefcere dicitur) ut et cryftalli ægrè fecernantur, et quæ prodeunt variis heterogeneis irretitæ reperiantur. His incommodis alcalinis præfertim obicem ponere tentatum fuit, quùm lixivium aciditate abundaret. Cineris clavellati et calx, five ufta five cruda, acidum abforbent, et fi juftâ addentur dofi, peregrina noxia re-vera præcipitando minuunt, quod, cognitâ lixiviorum indole, S. IX. luculentius patebit. Urina tamen nihil efficit, nifi quatenus alcali volatili prædita. Negari tamen non poteft quin novi fales peregrini immifceantur, nimirum alcali vegetabile vitriolatum, vel alii pro diverfo additamento, fine dubio, fublatis magis innocui, fed nihilominus pro alumine vendendi. Berg. ibid. p. 310.

or

or ſolidity. The ſolution here deſcribed, which had conſtantly refuſed to afford cryſtallized alum alone, afforded it immediately by the addition of a few drops of the ſolution of pot-aſh; and as I had employed theſe two ſubſtances in the requiſite proportion, the reſt of the ſolution afforded to the very end pure alum without any mixture of ſulphate of pot-aſh.

Into another portion of the ſame ſolution of pure alumine I dropped the ſame quantity of carbonate of ſoda as I had added of that of pot-aſh to the former. No cryſtallization was formed, even by the help of evaporation.

Lime and barytes produced no better effect.

Theſe experiments began to confirm the opinion I had, that the cryſtallization of alum is not prevented by an exceſs of acid, as has hitherto been thought, and that pot-aſh was not of uſe ſimply to ſaturate this acid, but that it performed an office of more importance. For I reaſoned, that if the common opinion were true, ſoda, lime, barytes, and all the ſubſtances which by a more powerful force would take this acid from alum, ought to give the ſame reſult. Another argument likewiſe preſented itſelf, which ſeemed deciſive. If the alkalis, pot-aſh and ammoniac do nothing more than unite to the ſuperabundant acid of the alum, the ſulphates of pot-aſh and of ammoniac ought not to occaſion any change in pure alum in its acidulated ſtate; but if theſe alkalis enter as a conſtituent part into the alum, and are neceſſary to its exiſtence, they ought to produce the ſame effects as pure pot-aſh or ammoniac.

I therefore added to a third portion of the ſolution of ſulphate of alumine before mentioned, ſome drops of the ſolution of ſulphate of pot-aſh; immediately upon which octahedral alum was formed. The ſulphate of ammoniac preſented the ſame effect.

This reſult gave ſtill greater confirmation to my firſt notions, though it did not yet afford a demonſtration perfectly without objection. For it might have happened that the two ſalts I made uſe of might determine the cryſtallization of the alum, ſimply by abſorbing the ſuperfluous acid, of which they are very greedy *.

To determine this poſſible fact, I mixed in the uncryſtallizable ſolution of alumine ſome of the acid ſulphate of pot-aſh, and obtained a cryſtallization no leſs abundant than with the neutral ſulphate of pot-aſh.

This laſt experiment does not therefore leave any doubt with regard to the influence and mode of action of pot-aſh and ammoniac in the fabrication of alum. This action is ſtill more ſtrongly confirmed, by the examination of the alums which have been formed by the proceſſes above related. For in this manner it is proved that they contain notable quantities of the ſulphates of pot-aſh and ammoniac.

Theſe experiments naturally led me to an examination of the different alums of commerce. Bergman had already announced, though in an indiſtinct manner, that not only the common alum, but likewiſe that of Rome, when decompoſed by ammoniac, afforded traces of the ſulphate of pot-aſh; and Scheele had remarked on his ſide, that alum which does not contain pot-aſh is not fit for making pyrophorus. Bergman, quoting this fact from Scheele, ſhews likewiſe that he conſidered the ſulphate of pot-aſh in alum as a foreign ſubſtance †.

* Ceterum alumen non tantum vulgare, ſed etiam romanum alcali volatili præcipitatum liquorem exhibet, qui haud raro alcali vegetabile vitriolatum continet. ibid.

† Alumen hoc inquinamento ſpoliatum pyrophoro generando ineptum eſt: quod facile experiri licet, nam [illegible] reſpuens cryſt[illegible], nullum [illegible] pyrophorum, modo conſuete tractatum, quamvis idem addito alcali vegetabilis pauxillo, eximium porrigat, &c.

In fact, if this chemift had thought pot-afh effential to the conftitution of alum, he certainly would not have thought it advifable to ufe a portion of the clay of Cologn to deftroy the excefs of acid in aluminous waters. For this addition is extremely vicious, and I am convinced that it was fuggefted by reafoning rather than by experiments *.

Out of all the kinds of alum which I had fubmitted to analyfis, I did not find one which did not afford fulphate of pot-afh or of ammoniac, and frequently both at once. I ufed the following method of analyfis: In the firft place, to direct my operations, I take a fmall quantity of the alum I wifh to affay, which I diffolve in the aqueous folution of pure pot-afh, and flightly heat the mixture. If it contain fulphate of ammoniac, a ftrong fmell of volatile alkali is immediately perceived. I then put into a tubulated retort a given quantity of this fulphate of alumine; I adapt a receiver containing a fmall quantity of water, and then pour on the alum a folution of pot-afh in a proper quantity to decompofe the fulphate of ammoniac and alumine at once. I boil this mixture for a quarter of an hour, at the end of which all the ammoniac is volatilized, provided no more than three or four docimaftic quintals be operated upon. I combine this ammoniac to faturation with the fulphuric acid, and the quantity of falt which I obtain indicates that which was contained in the ammoniacal fulphate of alumine.

When the pot-afh does not indicate the prefence of ammoniac, which is very feldom, I follow another method to feparate the fulphate of pot-afh. I decompofe the alum by means of ammoniac, and, after having wafhed the precipitated alumine, I evaporate the liquor to drynefs, and heat the remaining falt in a crucible, till it emits no more white vapours of the fulphate of ammoniac. The remainder is the pure fulphate of pot-afh. By thefe analytical methods, I found that one pound of cryftallized alum contains about one ounce 64 grains of fulphate of pot-afh; but as the alum contains about 0,44 water of cryftallization, this raifes the quantity of the fulphate to one ounce 7 gros 17 grains, for a pound of alum, or otherwife for cryftallized alum, about 0,070, and for dry alum, 0,125. When the alum has been formed with volatile alkali, it is found to contain the fulphate of ammoniac, nearly in the fame proportion as the fulphate of pot-afh. Whence it follows, that a quintal of alum prepared with pot-afh contains,

1. Sulphate of alumine — — —	49
2. Sulphate of pot-afh — — —	7
3. Water — — — —	44
	100

When the alums contain both the falts here mentioned at once, which frequently enough happens, I ufe lime inftead of pot-afh to difengage the ammoniac, and proceed with the refidue as before.

It may therefore be afcertained, by thefe fimple effays, whether pot-afh or ammoniac or both together have been ufed in a manufactory for the preparation of alum. This proof

* Hujus (lixivii magiftralis) cantharo duas addidi drachmas argillæ Colonienfis in fubtilem comminatæ pulverem, et pauci aquæ humectatæ; calore ebullitionem provocavi, qua per decem minutæ continuata, et poftea, refrigeratione peractà, refiduam feparavi argillam; lotam ficcavi; tandemque ponderatione inveni 25.5 grana foluta, quæ aluminis augmentum 141 granorum indicant. Berg. ibidem

may be of some utility; for Bergman pretends, I don't know on what foundation, that urine communicates to alum properties which are hurtful in dyeing: it is not probable, however, that this chemist asserted the fact without proof. In all the works where putrid urine is employed for the treatment of alum waters, the alum contains the sulphates of pot-ash and of ammoniac, because the combustibles which serve to roast the ores deposit a certain quantity of alkali, which unites with the sulphuric acid, and contributes, according to its proportion, to the formation of a greater or less quantity *.

It is known, from the experiments of Bergman and several other chemists, that by boiling a solution of ordinary alum with pure alumine in a very divided state, this last combines with the alum, and renders it insoluble in water; that is to say, it converts it into the state of neutral sulphate of alumine, or saturated with its earth. I have repeated this experiment with the design to ascertain whether the sulphates of pot-ash and ammoniac are precipitated with the alum; and I immediately observed, that the combination does not take place but by means of heat, though I used alumine recently precipitated from a solution, and still humid; and that at the end of a certain time the alum was entirely precipitated, and scarcely any signs of it left in the water. I re-dissolved the alum thus precipitated, in diluted sulphuric acid, and this solution afforded very fine crystals of octahedral alum by cooling; whence it follows that the pot-ash and ammoniac did fall down with the sulphate of alumine, and joined in the formation of this quadruple, earthy, tasteless salt. I at first imagined that this fact might serve to explain how it sometimes happens that aluminous waters, passed over materials less rich than those from which they were obtained, diminished in density by the loss of a portion of the acid necessary to the solution of the alum; but as the combination is not made without heat, and it requires a great division in the alumine, I presume that this effect is owing to another cause: nevertheless, it might be possible, in the process of time, especially in hot weather, that something of this nature might happen. It was interesting to determine what happens with regard to the sulphates of pot-ash and ammoniac in the precipitation of alum by its own base. I therefore boiled a solution of pure sulphate of alumine, that is to say, which contained no alkali, and was not crystallizable, with a certain quantity of the earth in a very divided state. It dissolved a small quantity, lost its slight acidity, but did not become insoluble. Having afterwards dropped a very small quantity of the solution of pot-ash into this liquor, a precipitate was formed in a short time, which was of the same nature as that of the foregoing experiment; that is to say, what is called alum saturated with its earth. It is therefore proved that the sulphates of pot-ash or ammoniac are necessary to render the alum capable of being precipitated by its earth, or to cause it to pass, as it were, to the earthy state. It is also proved, that the aluminous waters which do not contain pot-ash may remain as long as may be desired on their materials, without becoming saturated with too great a quantity of earth, or suffering alum to precipitate.

From the whole of what we have thus far explained, it will be easy to draw a number of consequences of importance to the arts, chemistry, and natural history.

1. It is not, at least in the greatest number of circumstances, the excess of acid which impedes

* When alums contain at the same time the sulphate of ammoniac and of pot-ash, the quantity of the latter is less; and in this respect there must be great varieties in the proportions, according to the dose of urine or of pot-ash which is added. V.

pedes the cryftallization of alum, but it is the want of pot-afh or ammoniac. For it is difficult indeed to imagine that the fulphuric acid could remain difengaged after fo long remaining upon alumine in a ftate of extreme divifion, and always fuperabundant. It is true that the aluminous waters redden the vegetable tinctures, but this property is not owing to a difengaged acid. This portion of acid is a conftituent part of thefe waters, and it appears to have more affinity with the neutral fulphate of alumine than with a new quantity of this earth at the temperature of the atmofphere.

2. The fulphate of pot-afh may be ufed, as well as pure pot-afh, to caufe the cryftallization of alum. It even has the advantage over the latter falt, becaufe, if the aluminous waters do not really contain a difengaged acid, the pot-afh, in its combination, will precipitate a portion of alumine, and diminifh the product of the boiling; whereas the fulphate of pot-afh does not produce the fame effect: but if the lixiviums contain difengaged acid, which muft very feldom be the cafe, it is not converted into alum by the fulphate of pot-afh, and is loft with regard to the product. I think, therefore, that with regard to fuch waters as really contain an excefs of acid, or a very oxided fulphate of iron, the ufe of pot-afh may be preferable to that of the fulphate of pot-afh. But with regard to the price of thefe fubftances, I think that in many places it would be profitable to ufe the fulphate of pot-afh, becaufe it is a falt indirectly produced in a great number of manufactories, where it may of courfe be obtained at a very moderate price. The refidues of the diftillation of aqua fortis by the fulphuric acid would be excellent for this operation. I mean that they would be preferable to the neutral fulphate of pot-afh. For I have remarked that this laft precipitates a portion of aluminous earth, which the other does not. This falt would more efpecially poffefs a decided preference before pot-afh, in thofe cafes where the aluminous waters contain at the fame time a great quantity of fulphate of iron intended to be ufefully employed, becaufe it would act immediately on the fulphate of alumine, without touching that of the iron; whereas the pot-afh does not begin to form alum until the whole of the ferruginous falt is decompofed. It would be in particular of much greater advantage than putrid urine, becaufe this fluid always contains phofphoric falts, which decompofe a portion of the fulphate of alumine, and confiderably diminifh the product.

This inconvenience might, however, be avoided by adding a certain quantity of lime to the urine, to precipitate the phofphoric acid.

3. Alumine cannot be ufed in the treatment of mother waters, as Bergman propofes. This earth is incapable of favouring the cryftallization of alum, befides which, it decompofes a portion of alum by the affiftance of ebullition; in which circumftance it feizes the acid neceffary to its folution, and precipitates it in the form of that powder which is called alum faturated with its earth.

4. Many alum ores muft naturally contain pot-afh, becaufe perfect alum is often obtained from the firft cryftallization of new alum waters without the addition of this alkali. It is true that an objection may be made with regard to the wood ufed in calcining thefe ores, which may be fuppofed to have furnifhed the alkali; but it is not probable that the fmall quantity of wood employed, in comparifon to the quantity of ore and the alum it affords, could fupply enough of pot-afh for the cryftallization.

5. All the earths and ftones which have given, or fhall hereafter afford, by analyfis with the fulphuric acid, perfect alum without addition of pot-afh, muft contain this alkali na-

 turally.

turally. For it is well proved, by the experiments related above, that alum cannot exist without pot-ash or ammoniac; and as there is little probability that this last should be found combined in earths or stones, unless perhaps in very rare cases, we may almost constantly be assured, when alum is obtained from any of these substances, that its formation was effected by pot-ash. The quantity of alum will immediately shew in what proportion this alkali existed in the substances analysed. I have announced to the Institute in the communication of my experiments on the leucite, that I had begun a series of experiments on several earths and stones which I presumed to contain pot-ash. At present I can give the results of some of my experiments.

The crude alum ore of La Tolfa afforded me 2,3 per cent.; but as it is difficult to extract more than two-thirds of this substance from the stones which contain it, we may, without fear of mistake, estimate the quantity at 3,4 per quintal *. The zeolite of the Isle of Ferro afforded 1,78, which makes according to our estimation 2,37 per cent. The argillaceous earth of Forges in Normandy, of which the pots of the glass works at Seves are made, likewise afforded it, but in a very small quantity †. The adamantine spar, on the analysis of which Citizen Guyton is at present employed, must contain a considerable quantity; for it affords much alum when treated immediately with the pure sulphuric acid. In reading over the analyses of stones which have hitherto been made, an almost certain proof will be found, no doubt, either by the loss in the sum of their products, or by the alum they afforded with the sulphuric acid, that they contained pot-ash, though no mention either was made or could be made respecting it.

6. The alum of commerce ought not to be considered as a simple salt, but as a combination in the state of a triple and sometimes quadruple salt of sulphate of alumine, sulphate of pot-ash, or of ammoniac. Among these last we may distinguish two species; the one without excess of acid, insoluble in water and insipid, being what is improperly called alum saturated with its own earth; and the other, which contains an excess of acid soluble in water, very sapid and astringent, is the common alum.

There is likewise a pure sulphate of alumine, very astringent, very difficult of crystallization, in the form of brilliant pearl-coloured plates without consistence, and which cannot be rendered insoluble by the addition of a new quantity of its base. This last salt may with the greatest propriety be called the sulphate of alumine.

7. It follows from the comparative analysis, and the knowledge acquired respecting the

* 100 parts of the ore of Tolfa contain

1. Alumine - - - -	43,92
2. Sulphuric acid - -	25,00
3. Pot-ash - - - -	3,08
4. Water - - -	4,00
5. Silex - - - -	24,00
	100,00

† 100 parts of the earth of Forges, calcined, contain:

Alumine - - -	40
Silex - - - -	60
	100

different

different ſtates of the combination of alumine with the ſulphuric acid united at the ſame time with other baſes, that we muſt diſtinguiſh ſeven ſtates in this combination, and that it is neceſſary to expreſs them according to the rules of the methodical nomenclature. Here follow the ſeries, the nature, and the names of theſe ſeven ſulphates of alumine.

1. Sulphate of alumine, or the artificial combination of ſulphuric acid and alumine. This ſalt is aſtringent ; it cryſtallizes in laminæ or flexible leaves, ſoluble in water. It has never been deſcribed nor named by chemiſts.

2. Acid ſulphate of alumine. It is the foregoing ſalt, with exceſs of acid, from which it differs by reddening blue vegetable colours. It is eaſily made by diſſolving that ſalt in the ſulphuric acid, but it is not eaſy to convert this into the neutral ſulphate of alumine but by boiling it a long time with its earth. This ſalt, like the firſt, has not been deſcribed.

3. Saturated ſulphate of alumine and of pot-aſh. It is the alum of the chemiſts ſaturated with its earth. I have deſcribed the manner of making it. It is pulverulent, inſipid, inſoluble, not cryſtallizable, and is eaſily converted into true alum by the addition of ſulphuric acid.

4. The acid ſulphate of alumine and of pot-aſh. It is eaſy to prepare it chemically. It greatly reſembles common alum ; but I have found none but that of La Tolfa which is of the ſame nature.

5. The acid ſulphate of alumine and of ammoniac. It is eaſily made in our laboratories. I have not yet found it pure in commerce. It has all the properties of alum, and may be uſed for the ſame purpoſes.

6. The acid ſulphate of alumine, pot-aſh, and ammoniac. It is remarkable enough, that this ſhould be the nature of the alum moſt frequently made in the arts, and that to expreſs its combination ſo many words ſhould be neceſſary. This, however, may be avoided by reſerving the name of alum to this ſubſtance, which will be ſufficient to diſtinguiſh it perfectly.

7. The acidulous ſulphate of alumine and of pot-aſh. I am leſs acquainted with this than the preceding ſpecies. The name which I propoſe to characteriſe it has been ſuggeſted to me, becauſe by adding a ſmall quantity of pot-aſh to the ſolution more than is neceſſary to obtain octahedral cryſtals, it manifeſtly paſſes to the cubic form.

8. Laſtly, the phyſician, the chemiſt, and the manufacturer, with whom the uſes of alum are greatly multiplied, will hereafter poſſeſs a knowledge of the ſubſtance they employ, and may appreciate its effects on the animal economy, and other bodies to which it is ſo frequently applied.

VII.

Deſcription of an Inſtrument proper to meaſure the Volume of a Body without plunging it in any Liquid. By H. SAY, Capitaine du Génie.*

THE ordinary method of determining the ſpecific gravity of ſolids by immerſion in a liquid is inconvenient for powders and fluids, and impracticable with regard to ſuch bodies

* Abſtract of a Memoir printed in the Annales de Chimie, XXIII. 1.

as may be acted upon by the ſtandard liquid. The learned author of the memoir from which the following extract is taken, has propoſed to remove this difficulty by plunging the ſubject of experiments in common air or a gaſeous fluid, by means of an inſtrument he calls a ſtereometer.

If the capacity of a veſſel, or, which is the ſame thing, the volume of air contained in that veſſel, be meaſured, when the veſſel contains air only, and alſo when the veſſel contains a body whoſe volume is required to be known, the volume of air aſcertained by the firſt meaſurement, deducting the volume aſcertained by the ſecond, will be the volume of the body itſelf. Again, if it be admitted as a law, that the volume of any maſs of air be inverſely as the preſſure to which it is ſubjected, the temperature being ſuppoſed conſtant, it will be eaſy to deduce, from the mathematical relations of quantity, the whole bulk, provided the difference between the two bulks under two known preſſures be obtained by experiment.

Let it be ſuppoſed, for example, that the firſt preſſure is double the ſecond, or, which follows as a conſequence, that the ſecond volume of the air be double the firſt, and that the difference be fifty cubic inches, it is evident that the firſt volume of the air will likewiſe be fifty cubic inches. The ſtereometer is intended to aſcertain this difference at two known preſſures.

The inſtrument is a kind of funnel, AB, Fig. 1, Plate XIV. compoſed of a capſule A in which the body is placed, and a tube B as uniform in the bore as can be procured. The upper edge of the capſule is ground with emery, in order that it may be hermetically cloſed with a glaſs cover M ſlightly greaſed. A double ſcale is paſted on the tube, having two ſets of graduations; one to indicate the length, and the other the capacities, as determined by experiment.

When this inſtrument is uſed, it muſt be plunged in a veſſel of mercury with the tube very upright, until the mercury riſes within and without to a point C of the ſcale, ſee Fig. 2. The capſule is then cloſed with the cover, which being greaſed will prevent all communication between the external air and that contained within the capſule and tube.

In this ſituation of the inſtrument, in which the mercury ſtands at the ſame height within and without the tube, the internal air is compreſſed by the weight of the atmoſphere, which is known and expreſſed by the length of the mercury in the tube of the common barometer.

The inſtrument is then to be elevated, taking care to keep the tube conſtantly in the vertical poſition. It is repreſented in this ſituation Fig. 2, ſecond poſition. The mercury deſcends in the tube, but not to the level of the external ſurface, and a column DE of mercury remains ſuſpended in the tube, the height of which is known by the ſcale. The interior air is therefore leſs compreſſed than before, the increaſe of its volume being equal to the whole capacity of the tube from C to D, which is indicated by the ſecond ſcale.

It is known therefore that the preſſures are in proportion to the barometrical column and to the ſame column diminiſhed by the ſubtraction of DE. And the bulks of the air in theſe two ſtates are inverſely in the ſame proportion, and again the difference between theſe bulks is the abſolute quantity left void in the tube by the fall of the mercury; from which data by an eaſy analytical proceſs the following rule is deduced: Multiply the number which expreſſes the leſs preſſure by that which denotes the augmentation of capacity, and divide the product by the number which denotes the difference of the preſſures. The quotient will be the bulk of the air when ſubject to the greater preſſure.

To

To render this more eafy by an example, fuppofe the height of the mercury in the barometer to be 78 centimetres, and the inftrument being empty to be plunged in the mercury to the point C. It is then covered, and raifed until the fmall column of mercury DE is fufpended, for example, at the height of fix centimetres. The internal air, which was at firft compreffed by a force reprefented by 78 centimetres, is now compreffed only by a force reprefented by 78 — 6, or 72 centimetres.

Suppofe it to be obferved at the fame time, by means of the graduations of the fecond fcale, that the capacity of the part CD of the tube which the mercury has quitted is 2 cubic centimetres. Then by the rule $\frac{72}{6} \times 2$ give 24 cubical centimetres, which is the volume of the air included in the inftrument when the mercury rofe as high as C in the tube.

The body of which the volume is to be afcertained, muft then be placed in the capfule, and the operation repeated. Suppofe, in this cafe, the column of mercury fufpended to be eight centimetres, when the capacity of the part CD of the tube is equal to 2 centimetres cube. Then the greateft preffure being denoted by 78 centimetres, as before, the leaft will be 70 centimetres, the difference of the preffures being 8, and the difference of the volumes two cubical centimetres. Hence $\frac{70}{8} \times 2$ gives the bulk of the included air under the greateft preffure 17,5 cubic centimetres. If therefore 17,5 centimetres be taken from 24 centimetres, or the capacity of the inftrument when empty, the difference 6,5 cubic centimetres will exprefs the volume of the body which was introduced. And if the abfolute weight of the body be multiplied by its bulk in centimetres, and divided by the abfolute weight of one cubic centimetre of diftilled water, the quotient will exprefs the fpecific gravity of the body in the common form of the tables where diftilled water is taken as unity, or the term of comparifon.

After this defcription and explanation of the ufe of his inftrument, the author proceeds with the candour and acutenefs of a philofopher to afcertain the limits of error in the refults; an object feldom fufficiently attended to in the inveftigation of natural phenomena. From his refults it appears, that with the dimenfions he has affumed, and the method prefcribed for operating, the errors may affect the fecond figure. He likewife gives the formulæ by means of which the inftrument itfelf may be made to fupply the want of a barometer in afcertaining the greateft preffure. He likewife adverts to the errors which may be produced by change of temperature. To prevent thefe as much as poffible, the actual form of the inftrument and arrangements of its auxiliary parts are fettled as in Fig. 3, by which means the approach of the hand near the veffel and its tube is avoided. In this figure the vertical pofition of the tube is fecured by the fufpenfion of the veffel, and a perforation in the table through which the tube paffes. The table itfelf fupports the capfule in its firft pofition, namely, that at which the cover is required to be put on.

It feems probable, from the author's general reflections immediately preceding the explanation of the figures, that he had not entirely finifhed his meditations on this fubject. If he had, I think it likely that he would have determined his preffures as well as the meafures of bulks by weight. For it may be eafily underftood, that if the whole inftrument were fet to its pofitions by fufpending it to one arm of a balance at H Fig. 3, the quantity of counterpoife when in equilibrio might be applied to determine the preffures to a degree of accuracy much greater than can be obtained by linear meafurement.

VIII.

Useful Notices respecting various Objects.—Styrian Steel—Elastic Strings for Musical Instruments and other Purposes—Wheels without Cogs.

1. *Styrian Steel.*

SOME time ago I was favoured with a visit from Mr. Ramondini, an Italian gentleman on his travels. He assured me that the Styrian steel is made very well in Hungary, and can be obtained from any iron originally smelted with charcoal, and not with coke. He says that the secret of the process consists in the management upon the hearth of the blast or refining furnace. If the iron be stirred about while in fusion, it will afford bar iron; but if the scoria be simply raked off as it rises, and the metal be left for a due time undisturbed in fusion, and then taken to the hammer, it proves to be steel. He affirms, that the iron made with coke will not cement into steel, but forms a slag. Perhaps this may arise from a redundancy of plumbago.

2. *Elastic Strings for Musical Instruments and other Purposes.*

THE elastic string known by the name of catgut, and very useful for making the bands of lathes, when its diameter is large, or the strings of musical instruments, when small, possesses peculiar properties which are well known. As it is composed of animal fibre, it is subject to putrefy by accidental moisture, and on this account the strength of such as appears to be perfect is very uncertain. I understand that musical strings of catgut are not made in this country. An intelligent friend of mine made an experiment many years ago with silk to form this last article. He wound the single thread or fibre of the silkworm, without twisting, till the mass of fibres formed a body equal to his wish. He smeared this mass with white of egg, which he rendered immediately consistent by passing it through heated oil. The string was exquisitely true, or uniform in its thickness. When tried upon an instrument, the tone was not satisfactory to an accustomed ear: the performer said it was tubby, a word which I suppose has reference to the sound emitted by a tub, and will, as I imagine, convey a correct though perhaps undefinable notion to a musician. Musical strings produce a sound more grave the greater their diameter or their length, and the less their tension. It is required that this sound should not only be intense and perfect, but also that it should endure for a certain short time without considerable diminution. The maximum of these qualities, when the length is limited, is aimed at by regulating the diameter and the tension. If the diameter be too great, the sound will not last; if too little, it will be deficient in intensity. There is an artifice by which the permanence of a grave sound from a given length of string is secured, without requiring the tension to be much diminished. It consists in adding to the weight of the string, without adding to its strength or elastic force. For this purpose a slender wire is wrapped round the string, which with this load gives a very different and more lasting tone than a simple string of the same length. I believe it is admitted that the tone of these strings is less brilliant or clear, and I suppose the strings made by my friend had the same quality, the brittle indurated white of egg adding to the weight but not to the elasticity. The caoutchouc or elastic gum was at that time unknown. I think it worth trying with silk for musical strings, and still more in the fabrication of strong bands for lathe work, to be used instead of catgut.

3. *Wheels*

3. *Wheels without Cogs.*

VERY much of the difpofal or tranfmiffion of mechanic force is effected by means of wheels acting upon each other. The connection of the moving parts is performed either by the mutual action of teeth, or by ftraps, or by the friction of one face of a wheel againft another. I have feen a drawing of a fpinning-wheel for children, at a charity-fchool, in which a large horizontal wheel with a flip of buff leather glued on its upper furface, near the outer edge, drove twelve fpindles at which the fame number of children fat. The fpindles had each a fmall roller likewife faced with leather, and were capable by an eafy and inftantaneous motion of being thrown into contact with the large wheel at pleafure. Each child could therefore throw her own part of the apparatus into work, or caufe it to ftop, as often or as long as fhe pleafed. The winding bobbins for yarn at the cotton-mills operate on the fame fimple and elegant principle, which poffeffes the advantage of drawing the thread with an equal velocity, whatever may be the quantity on the bobbin, and cannot break it. I do not know that this principle has been applied in large work, except in a faw-mill by Mr. Taylor of Southampton. In this the wheels act on each other by the contact of the end grain of wood inftead of cogs. It makes very little noife, and at the time it was mentioned to me had been in wear about fourteen years. I fuppofe, of courfe, that there is a contrivance for bearing the wheels firm againft each other by wedges at the fockets, or rather by levers.

IX.

An Economical Procefs to obtain Pure Cauftic Alkali in the Large Way, with Fufed Pot-afh, or the Lapis Caufticus. By Citizen BOUILLON LE GRANGE *.

THE proceffes for obtaining the cauftic alkali and fufed pot-afh, called lapis caufticus in the difpenfatories, being either defective or tedious, the Citizen Welter and myfelf have endeavoured to abridge this operation, by ufing a procefs which requires lefs time and expence, and is more certain and ufeful, particularly for obtaining the cauftic alkali in the large way, which is fo neceffary in the arts and in chemical experiments.

We believe, therefore, that practical men will receive with pleafure the defcription of a method by which no part of the pot-afh is loft, and which affords it in a ftate of purity, and very cauftic, without much expence or apparatus.

Our apparatus confifts of feveral veffels of white wood, or, which is better, calcareous ftone, whofe dimenfions may be varied according to the quantities intended to be prepared. Thofe which we have eftablifhed for the Polytechnic School are of ftone, of the internal capacity of one foot cube, (fee Fig. 4, Plate XIV.) having their bottoms grooved by channels of an inch in depth and the fame width, and fo far diftant from each other that there are five or fix parallel, which terminate at an end in a fimilar groove, which croffes the whole, and ferves as a gutter to collect the faline folution. In the middle of this laft an

* Annales de Chimie, XXII. 137.

 hole

hole is pierced, to infert a tube of glafs, which comes out under an angle of 45 degrees with the horizon.

The channels are covered with tubes of glafs ranged acrofs, upon which is placed a cloth in fuch a manner as to cover them completely. Upon this a thin layer of wood-afhes is fprinkled, and afterwards the mixture which we fhall proceed to mention is put into the veffels.

For want of veffels of ftone, we may operate by ufing fmall tubs of white wood, as in Figure 5; and inftead of the grooves, river fand muft be fpread on the bottom, after being firft carefully wafhed; over this a finer ftratum muft be fpread, and the whole then covered with a cloth, and wood-afhes ftrewed thereon. Thefe veffels, like the others, muft each have a tube to convey the fluid which filters through.

It is not neceffary to remark, that the firft method is to be preferred. For the cauftic alkali always feizes a portion of the colouring matter of the wood, and may even, according to its degree of concentration, take up a fmall quantity of filex by folution. But thefe inconveniencies are of little confequence, when merely the cauftic alkali or lapis caufticus is wanted.

For the arts, and for the delicate experiments of chemiftry, the cifterns of calcareous ftone are to be preferred: with thefe the liquor is obtained perfectly limpid.

Things being thus difpofed, equal parts are to be taken of quicklime and of pot afh, more efpecially when the lime is very cauftic. In the contrary cafe, twenty parts of lime may be taken to fifteen of pot-afh. Water is then to be heated in an iron pot till it nearly boils; in this ftate the lime is to be added, which by its extinction brings it to the boiling heat. When the extinction is complete, the pot-afh is mixed, and the whole formed into a thick mafs, which is left to cool a little.

The mixture is afterwards poured into the cifterns, and immediately covered with water. In order to avoid making perforations in the mixture by pouring the water, a fmall board is laid to receive the ftream, which rifes with the fluid.

Care muft be taken to place pitchers or other veffels to receive the liquor which flows through the tube; and in order to prevent the lixivium from abforbing carbonic acid from the atmofphere, the veffels muft be flightly clofed to prevent the circulation of the external air.

It is likewife neceffary that a fupply of water fhould be kept up over the mixture. When it comes out of the tube in an infipid ftate, it is unneceffary to draw off any more.

The folutions obtained are all to the laft very nearly of the fame ftrength; for they become weak all at once: from this circumftance the very aqueous folutions are avoided.

Iron pots may be ufed to evaporate the waters. The laft run muft be taken firft, becaufe it is rather weakeft; and by this means the ftronger folutions are not fo long expofed to the contact of the air. The evaporation muft be performed by a ftrong ebullition.

When the concentration has arrived to a certain point, the fulphate of pot-afh cryftallizes and falls down. It may eafily be taken out by placing at the bottom of the pot a perforated iron ladle, in which the falt is collected. Strong ebullition is neceffary to keep the atmofpheric air at a diftance from the furface, and towards the end the motion of boiling ferves to carry the fulphate of pot-afh into the ladle.

If

If the lapis cauſticus be wanted, the concentrated liquor is to be poured into a ſmaller pot, and the evaporation carried ſo far that the ſalt may congeal when poured out on a plate of iron or marble *.

If a purer alkali be required for the delicate experiments of chemiſtry, inſtead of pot-aſh, the tartareous acidule or cream of tartar may be calcined and made uſe of; or otherwiſe the fuſed pot-aſh before mentioned may be purified by alcohol after the manner of Berthollet. Experiment has proved to us, that very pure alkali may by this means be obtained †.

In this caſe, the lixivium is to be evaporated to the conſiſtence of a thick ſyrup in a ſilver baſon ‡, or preferably in cloſe veſſels. This matter is then to be diſſolved in alcohol. The pot-aſh alone combines with the fluid: the ſulphate and muriate of pot-aſh, the portions of earth and even of carbonic acid which it obſtinately retains, or which it has reſumed from the air during the evaporation, remain at the bottom of the ſolution. If the alcohol has been poured on the matter whilſt ſtill hot, and a leſs quantity of this ſolvent has been employed than was requiſite for diſſolving the pot-aſh, it cryſtallizes by cooling in white blades, which are ſometimes ſeveral inches long. If it be required to ſeparate the pot-aſh from the alcohol, and to obtain it in the dry ſtate, the ſolution muſt be evaporated in a ſilver baſon, and not in a glaſs veſſel; for the pot-aſh often diſſolves a portion of ſilex, which alters its purity.

We ſee by this operation, of which the details deſerve to be conſulted, in the memoir of Berthollet, that the cauſtic pot-aſh is deprived of ſilex, of carbonic acid, of all the foreign ſalts, and of the ſmall portion of iron it may have taken up from the veſſel in which the evaporation was performed.

* It is proper to remark, that the ſolid cauſtic alkali thus prepared is much more active than that uſually made by chemiſts for ſale. For this reaſon it is neceſſary to be very prudent in its application. G.

† See, however, the remark of Lowitz on this ſubject. " Philoſ. Journ. I. 165. N."

‡ What follows is extracted from the memoir of the celebrated Berthollet, for the advantage of thoſe who may not poſſeſs the means of conſulting the original. G.

X.

A TABLE for reducing the Unities of the Metre, Litre, and Gramme into English Inches, Gallons, and Grains. (See page 199).

Fr. Measures	Metres in Inches.	Litres in Cubic Inches.	Litres in Ale Gallons of 282 Inches.	Litres in Wine Gallons of 231 Inches.	Grammes in Grains.
1	39.383	61.083	0.2166	0.2644	22.966
2	78.766	122.17	0.4332	0.5289	45·932
3	118.15	183.25	0.6498	0.7933	68.898
4	157.53	241.33	[illegible]	[illegible]	91.864
5	196.91	305.41	1.0830	[illegible]	114.83
6	236.30	366.50	1.2996	[illegible]	137.79
7	275.68	427.58	1,5162	[illegible]	160.76
8	315.06	488.66	1.7328	[illegible]	183.73
9	354.45	549.75	1.9195	[illegible]	[illegible]
10	393.83	610.83	2.1661	2.6443	229.66
11	433.21	671.91	[illegible]	[illegible]	[illegible]
12	472.60	733.00	[illegible]	[illegible]	275.59
13	511.98	794.08	[illegible]	[illegible]	298.56
14	551.35	855.16	[illegible]	[illegible]	321.52
15	590.74	916.24	[illegible]	[illegible]	344.49
16	630.13	977.33	3.4657	[illegible]	367.46
17	669.51	1038.4	3.6823	[illegible]	390.42
18	708.89	1099.5	[illegible]	4.7597	413.39
19	748.28	1160.6	4.1155	[illegible]	436.35
20	787.66	1221.7	4.3321	5·2386	459.32
21	[illegible]	1282.7	4.5487	5.5530	482.29
22	866.43	1343.8	4.7653	5.8174	505.25
23	905.81	1404.9	4.9819	6.0819	528.22
24	945.19	1466.0	5.1985	6.3463	551.18
25	984.57	1527.1	5.4151	6.6107	574.15
26	1023.1	1588.2	5.6318	6.8752	597.12
27	1063.3	1649.2	5·8484	7.1396	620.08
28	1102.7	1710.3	6.0650	7:4040	643.05
29	1142.1	1771.4	6·2816	7.6684	666.01
30	1181.5	1832.5	6·4982	7 9329	688.98
31	[illegible]	1893.6	6.7148	8.1973	711.95
32	1260.3	1954.7	6.9314	8.4617	734.91
33	[illegible]	2015.7	7.1480	8.7262	757.88
34	[illegible]	2076.8	7.3646	8.9906	780.84
35	1378.4	2137.9	7.5812	9.2550	803.81
36	[illegible]	2199.0	7.7978	9.5194	826.78
37	[illegible]	2260.1	8.0144	9.7839	849.74
38	[illegible]	2321.2	8.2310	10.048	872.71
39	1535.9	2382·2	8.4476	10.313	895.67
40	1575.3	2443.3	8.6642	10.577	918.64
41	1614.7	2504.4	8.8808	10.842	941.61
42	1654.1	2565.5	9·0975	11.106	964.57
43	1693.5	2626.6	9.3141	11.370	987.54
44	1732.9	2687.7	9.5307	11.635	1010.5
45	1772.2	2748.7	9.7473	11.899	1033.5
46	1811.6	2809.8	9.9639	12.164	1056.4
47	1851.0	2870.9	10.180	12.428	1079.4
48	1890.4	2932.0	10.397	12.693	1102.4
49	1929.8	2993.1	10.614	12.957	1125.3
50	1969.1	3054·1	10.830	13.221	1148.3
51	2008.5	3115.2	11.047	13.486	1171.3
52	2017.9	3176.3	11.264	13.750	1194.2
53	2087.3	3237.4	11.480	14.015	1217.2
54	2126·7	3298.5	11.697	14.279	1240.2
55	2166.1	3359.6	11.913	14.544	1263.1
56	2205.4	3420.6	121.30	14.808	1286.1
57	2244.8	3481.7	12.347	15.072	1309.1
58	2284.2	3542.8	12·563	15.337	1332.0
59	[illegible]	[illegible]	12.780	15.601	1355.0
60	2363.0	3665.0	12.996	15.866	1378.0
61	2402.4	[illegible]	13·213	16.130	1400.9
62	2441.7	3787.1	13·430	16.395	1423.9
63	2481.1	3848.2	13.646	16.659	1446.9
64	2520.5	3909.3	13.863	16.923	1469.8
65	2559.9	3970.4	14.079	17 188	1492.8
66	2599.3	4031.5	14.296	17 452	1515.8
67	2638.7	4092.6	14.513	17.717	1538.7
68	2678.0	4153.6	14.729	17.981	1561.7
69	2717.4	4214,7	14.946	18.246	1584.7
70	2756.8	4275.8	15.162	18.510	1607.6
71	2796.2	4336.9	15.379	18.774	1630.6
72	2835.6	4398.0	15.596	19.039	1653.6
73	2875.0	4459.1	15.812	19.303	1676.5
74	2914.3	4520:1	16.029	19.568	1699.5
75	2953.7	4581.2	16.245	19.832	1722.4
76	2993.1	4642.3	16.462	20.097	1745.4
77	3032.5	4703.4	16.679	20.361	1768.4
78	3071.9	4764.5	16.895	20.625	1791.3
79	3111·3	4825.6	17.112	20.890	1814.3
80	3150.6	4886.6	17.328	21.154	1837.3
81	3190.0	4947·7	17.545	21.419	1860.2
82	3229.4	5008.8	17.762	21.683	1883.2
83	3268.8	5069.9	17.978	21·948	1906.2
84	3308.2	5131.0	18.195	22.212	1929.1
85	3347.6	5192.1	18.412	22.476	1952.1
86	3386.9	5253.1	18.628	22.741	1975.1
87	3426.3	5314.2	18.845	23.005	1998.0
88	3465.7	5375.3	19.061	23.270	2021.0
89	3505.1	5436.4	19.278	23.534	2044.0
90	3544.5	5497.5	19·495	23.799	2066.9
91	3583.9	5558.6	19.711	24.063	2089.9
92	3623.2	5619.6	19.928	24.327	2112.9
93	3662.6	5680.7	20.144	24.592	2135.8
94	3702.0	5741.8	20.361	24.856	2158.8
95	3741.4	5802.9	20.578	25.121	2181.8
96	3780.8	5864.0	20.794	25.385	2204.7
97	3820.2	5925.1	21.011	25.650	2227.7
98	3859.5	5986.1	21.227	25.914	2250.7
99	3898.9	6017.2	21.444	26.178	2273.6
100	3938.3	6108.3	21.661	26.443	2296.6

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

QUESTION VII. *Anſwered by J. F—:—:—:—R.*

THE order in which the firſt 51 cards are diſtributed being of no importance with reſpect to the chance for trumps, the ſolution of this queſtion will be ſomewhat ſimplified by conceiving the hands of the dealer's two adverſaries to conſiſt of the firſt 26 cards dealt off. The chance that theſe may be all of them of the 39 which are not trumps will evidently be, $\frac{39 \cdot 38 \cdot 37 \cdot 36 \ldots \&c. \ldots\ldots 14}{51 \cdot 50 \cdot 49 \cdot 48 \ldots \&c. \ldots\ldots 26} = \frac{25 \cdot 24 \cdot 23 \cdot 22 \ldots \&c. \ldots\ldots 14}{51 \cdot 50 \cdot 49 \cdot 48 \ldots \&c. \ldots\ldots 40} = \frac{19}{580027} = \frac{1}{30527\frac{14}{19}}$, ſo that the odds againſt it are $30526\frac{14}{19}$ to 1; and yet it is 5200300 times more probable that the dealer and his partner ſhould have the thirteen trumps between them, than that the dealer himſelf ſhould hold them all in his own hand.

De Moivre, in his "Doctrine of Chances," (2d edit. cor. to prob. xix.) gives a theorem reſpecting the chance of drawing any certain number of black and white counters, which is applicable in ſuch an infinite variety of caſes, that it is perhaps worth mentioning. It may be thus generaliſed:—If a and b be the reſpective numbers of things of two different kinds, heaped promiſcuouſly together, the following formula will expreſs the value of the chance, that out of c of them, taken at hazard from amongſt the whole number $n\,(=a+b)$ there be found the exact number p, and no more, of that kind whoſe whole number is a:

$$\frac{c.c-1.c-2..\&c.(p \text{ terms}) \times d.d-1.d-2\ldots\&c.\,(a-p \text{ terms}) \times \frac{a}{1}.\frac{a-1}{2}.\frac{a-2}{3}\&c.\,(p \text{ terms})}{n.n-1.n-2.n-3..\&c.\,(a \text{ terms}).}$$

wherein d is $= n - c$. A proper ſubſtitution in this expreſſion will alſo give us the ſame reſult as above.

QUESTION VIII. *Anſwered by J. F—:—:—:—R.*

LET a and b be the hyperbolic logarithms of a and b. Then will $\mathrm{a}x$ be $= \mathrm{h.l.}\,a^x$ and $1 + \mathrm{a}.x + \frac{\mathrm{a}^2}{2}x^2 + \frac{\mathrm{a}^3}{2.3}x^3 + \frac{\mathrm{a}^4}{2.3.4.}x^4 \ldots\ldots \&c. = a^x$; and, in like manner, $1 + \mathrm{b}.x + \frac{\mathrm{b}^2}{2}x^2 + \frac{\mathrm{b}^3}{2.3}x^3 + \frac{\mathrm{b}^4}{2.3.4.}x^4 \ldots\ldots \&c. = b^x$; the ſum of which $2 + (\mathrm{a}+\mathrm{b})x + \frac{\mathrm{a}^2+\mathrm{b}^2}{2}x^2 + \frac{\mathrm{a}^3+\mathrm{b}^3}{2.3}x^3 + \frac{\mathrm{a}^4+\mathrm{b}^4}{2.3.4.}x^4 \ldots \&c. = c$, by the queſtion. Hence, putting $d = c - 2$, and $\alpha, \beta, \gamma, \delta, \varepsilon$, &c = the above co-efficients of the ſeveral powers of x in the latter ſeries, we get $x = \frac{1}{\alpha}d - \frac{\beta}{\alpha^3}d^2 + \frac{2\beta^2 - \alpha\gamma}{\alpha^5}d^3 + \frac{5.\alpha\beta\gamma - 5\beta^3 - \alpha^2\delta}{\alpha^7}d^4 \ldots\ldots \&c.$ As, however, this ſeries does not, when d is great in reſpect of α, begin to converge till after a conſiderable number of terms, it ſeems better to uſe the common tentative proceſs, repeatedly aſſuming two values of x, and applying the following proportion:—Difference of reſults : difference of aſſumed values :: leaſt error : correction for the neareſt value.—This method, though an indirect one, has certainly great practical advantages in a variety of caſes wherein the expreſſions are ſo entangled with ſurds or unknown exponents as not to be otherwiſe reducible without a great deal of trouble.

Another method of obtaining the value of x is by means of a table of artificial sines, as follows:—Let r be the logarithmic radius, a, b, and c the common logarithms of a, b, and c, and $d = r - \frac{1}{2}c$. Find an artificial sine, and its correspondent cosine, f and s, in the tables, so that $\frac{f - d}{\frac{1}{2}a}$ may be $= \frac{s - d}{\frac{1}{2}b}$, each of which quantities will then be $= x$. The demonstration becomes obvious by considering, that if one of three quantities be equal to the sum of the other two, their square-roots are the sides of a right-angled triangle.

Nearly a similar answer was given to this question by ANALYTICUS.

NEW MATHEMATICAL QUESTIONS.

QUESTION XI. *By W. SIMPSON.*

IF H, h, be the heights of any two signals above the horizon of an observer, A, the angle which they subtend, and a, the same angle reduced to the plane of the horizon, then will $\text{cos. } A = \text{cos. } a \times \text{cos. } H \times \text{cos. } h + \text{sine } H \times \text{sine } h$. Required the demonstration.

QUESTION XII. *By W. C. of Greenwich.*

ON the 1st of May 1797, the sun's declination being 15° 9′, it was observed that his altitude, azimuth, and the latitude of the place were all equal. Required the hour and place where the observation was made.

*** I SHOULD be glad to give a description and drawing of Mr. Varley's machine for producing perpetual motion, as requested by Mr. Notlem of Wisbech, if an attentive perusal of the specification enrolled in Chancery had shewn me any thing tending to improve the theory or practice of mechanics. The description in the periodical work he mentions is not sufficiently clear to shew the whole of what the writer meant to explain, and I found the original equally imperfect. Mr. Varley's notion, obscured by some extraneous and unimportant circumstances, appears to be, that if an exhausted cylinder be fixed to one part of the periphery of a wheel, and a piston fitted therein, the pressure of the atmosphere on this last, supposed also to be attached to the wheel by a spring and chain (parallel to a tangent), will tend to drive it into the vacuum, and, if prevented by the shortness of the chain, will draw the wheel round. It is obvious to any person moderately acquainted with statics, that the pressures on his wheel must counterbalance each other, and cannot produce motion.

It has always been easy to shew the fallacy of schemes for perpetual motion in the particular instances; but I have met with no clear enunciation of this project so general as to include every possible scheme, and evince its own absurdity. The difficulty of performing this seems to arise from a want of direct and concise demonstrations of the fundamental principle of the lever, and of the equal pressure of fluids in all directions.

THE

*** THE correspondent who desires an explanation whence the sulphate of pot-ash mentioned in the last paragraph of page 218, was derived, is reminded, that this salt is the most abundant impurity in the common vegetable alkali. Kirwan, in the Irish Memoirs for 1789, states its amount in Dantzick pearl-ash at 505 grains in the pound troy, which is upwards of one seventh of the weight of the pure alkali. As Professor Siegling made his liquor of flints *by the ordinary process*, it is not to be supposed that his alkali had undergone any very laborious purification.

PUBLICATIONS ON GALVANISM.

THE celebrated Aldini, Professor of Natural Philosophy at Bologna, has forwarded to the first class of the National Institute a collection of works published in Italy, since the discovery made by Galvani, of the irritation produced in animal matters by the metals, and which treat on that subject. These works having been hitherto little circulated, Citizen Guyton, from whose annonce in the 22d volume of the Annals of Chemistry, page 323, this extract is taken, naturally concluded that the list would be acceptable to the world. The paper of Mr. Humboldt, of which I have given a translation *; another memoir by the same author, on the chemical process of vitality, together with certain truly philosophical remarks upon the same by Citizen Fourcroy, all which are inserted in the 22d volume of the same Annals, have shewn the great importance of a phenomenon which has engaged the attention of all Europe. The first class of the Institute of France has considered this as an object most worthy of investigation, and has for that purpose nominated a commission of seven members, who for five months past have been engaged in experiments proper to determine the circumstances, and develop its nature.

The following is the list. The annexed remarks are made by Citizen Guyton.

Aloysii Galvani in Bononiensi Archigymnasio Professoris, etc. de Viribus Electricitatis in Motu musculari, Commentarius. Cum Joan. Aldini Dissertatione et Notis. Mutinæ, 1792. Small folio, 80 pages, with three engraved plates.

This work contains a letter of Bassani Carminesi, which exhibits the opinion of Volta concerning the seat of animal electricity.

Joannis Aldini de Animali Electricitate Dissertationes duæ. Bononiæ, 1794. Small folio, 41 pages, with figures.

Dell' Uso e dell' Attività dell' Arco Conductore nelle Contrazioni dei Muscoli. In Bologna, 1794. Small quarto, 168 pages. With a Supplement of 23 pages.

Memoria intorno all' Electricità Animale, del Sig. Dott. Gio. Aldini, Prof. di Fisica nell' Universtià di Bologna, etc. 10 June, 1794. In quarto, 12 pages.

* Philos. Journ. I. 256.

Prima

Prima Brunonis Theoriæ Rudimenta. Bononiæ, 1797. Octavo, 16 pages.

This paper of Aloyfius Zanottus is written in the form of a letter addreffed to Charles Mundini.

It contains a development of Dr. Brown's opinion, that irritability is one and indivifible relative to the fyftem of the nerves; that is to fay, that this force is diftributed through every point of the fyftem, in the fame manner as velocity, impreffed on any body or fyftem of bodies, appertains equally to all the elements of this body or fyftem.

Irritants are diftinguifhed into extrinfic and intrinfic.

The extrinfic are heat, light, found, food, &c. The blood and other animal fluids, though appertaining to the fyftem of the animal, appear to act on its economy like extrinfic objects.

The intrinfic irritants are the functions of the fyftem, fuch as the mufcular contraction, the exercife of the fenfes, the action of thought, the movements of the brain, &c.

The common effect of thefe forces is motion. Each poffeffes a certain activity called ftimulus. There cannot be fuch an agent as a fedative; for, when the irritants produce weaknefs, this weaknefs muft be attributed to a defect of the ftimulating power in degree, &c. &c.

Fig. 5.

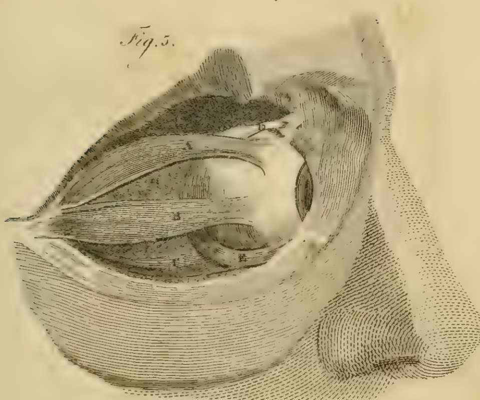


Fig. 2.

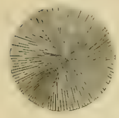
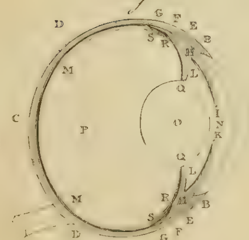


Fig. 3.

Fig. 1.



Focal adjustment of the Eye.

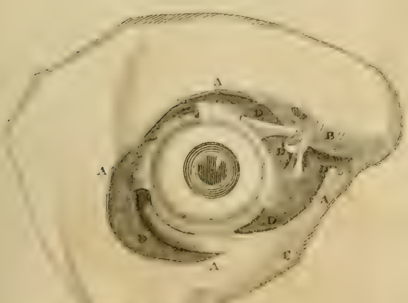
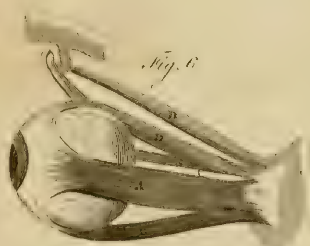
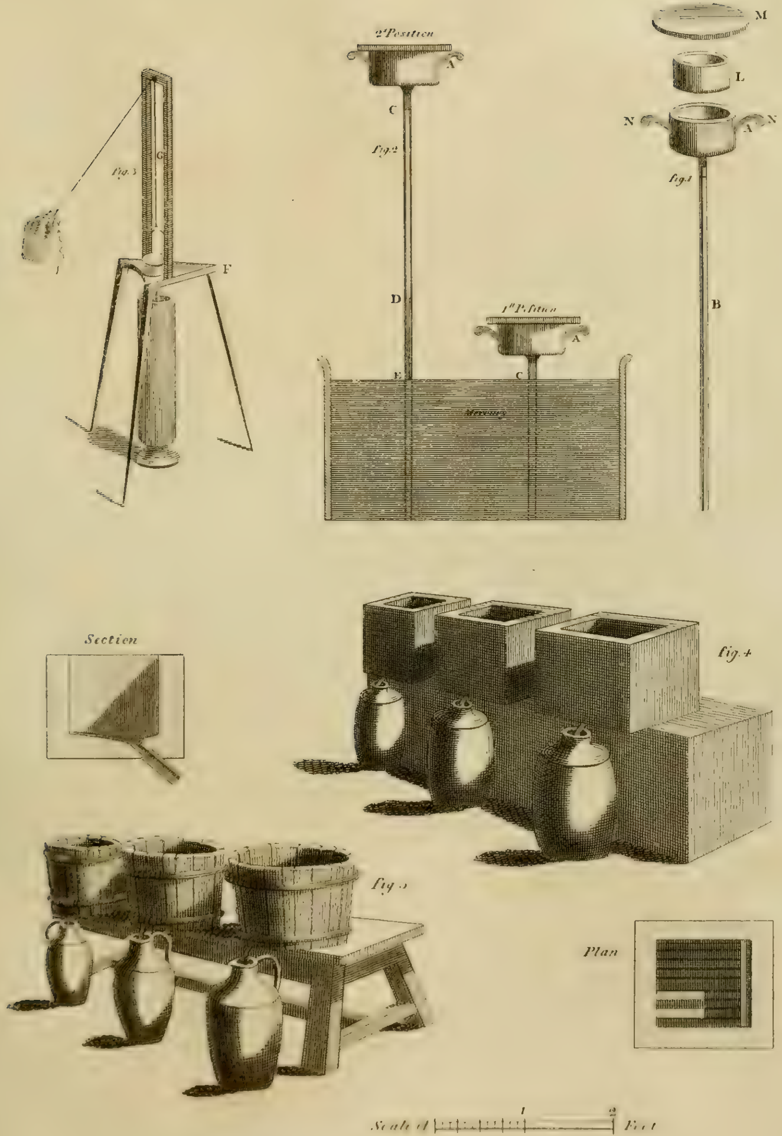


Fig. 4.

Fig. 6.




2d Position
1st Position
Mercury
fig.1
fig.2
fig.3
fig.4
fig.5
Section
Plan
A
B
C
D
E
F
G
L
M
N
Scale of 2 Feet

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER 1797.

ARTICLE I.

Experiments and Observations on the Nature of Sugar. By WILLIAM CRUICKSHANK, Chemist to the Ordnance, and Surgeon of Artillery.

SUGAR has been supposed to be a substance intermediate between mucilages and vegetable acids, containing more oxygene than mucilage, and less than the acids. To ascertain this and some other circumstances, the following experiments were instituted:

Two ounces of refined sugar were introduced into a retort, and exposed to a heat gradually increased until its bottom became red-hot: there came over into the receiver seven drachms of a sharply acid liquor, which required 132 grains of a solution of pot-ash to saturate it: this liquor was mixed with a little empyreumatic oil: the charry residuum which remained in the retort weighed seven drachms; the quantity of gas which escaped during the operation must therefore have amounted to two drachms: some of this was examined, and found to consist of a mixture of carbonic acid gas and hydro-carbonate.

Two ounces of gum-arabic were introduced into a retort at the same time, and exposed to a heat in every respect similar: the quantity of acid liquor which came over into the receiver amounted to seven drachms and 15 grains; this contained a little more empyreumatic oil, but was not so sharp as that obtained from the sugar, and required only 117 grains of the same solution of pot-ash to saturate it: the charry residuum which remained in the retort weighed five drachms and 45 grains; the quantity of elastic fluid or gas which escaped during this process must therefore have amounted to three drachms: it consisted, like the former, of a mixture of hydro-carbonate and carbonic acid gas; but towards the end of the operation the proportion of hydro-carbonate was more remarkable. From these experiments it would appear that sugar yields by distillation more pyromucous acid than

gum,

gum, in the proportion of 132 to 117. The refiduary charcoal of the fugar alfo exceeded that of the gum by 1-7th; but this may in fome meafure be accounted for from the greater quantity of the hydro-carbonate yielded by the latter. As oxygene is now allowed to be the univerfal acidifying principle, and as the acid yielded in both inftances (viz. the pyro-mucous) was exactly of the fame kind, it may be reafonably inferred, that the fugar which afforded the greateft quantity of acid contained likewife the greateft proportion of oxygene; for it is probable that both the carbonic acid and the hydro-carbonate were formed from the decompofition of the water by the carbone of thefe fubftances, as neither was produced in any quantity until near the end of the operation: the oxygene, therefore, contained in the former fhould not be confidered as entering effentially into the compofition of either the gum or fugar.

It is well known that vegetable mucilages and fœcula are fomehow converted into fugar by malting: we conceived, therefore, that it would throw confiderable light on this fubject, to obferve with more attention than had hitherto been done, the particular changes and decompofitions which take place during this procefs: it was with this view that the following experiments were made:

December 1, 1796. A quantity of barley, after being foaked in water for 24 hours, was put into a wine-glafs, and introduced into a jar containing common air, and inverted over water: the temperature in this and the following experiments was preferved between 60 and 70 as nearly as poffible. At the end of five days it began to grow, and on the 28th the greateft part had thrown out fhoots at leaft half an inch in length. On January 7, vegetation was ftill going on, and the air in the jar had fomewhat diminifhed: the barley being now withdrawn was found to be very fweet, and nearly converted into the ftate of malt. The air in the jar was found to confift of azotic and carbonic acid gas in the proportion of 20 to 6, the whole of the oxygene being either abforbed, or converted into carbonic acid.

January 19, 1797. A quantity of barley, previoufly fteeped in water for 48 hours, was introduced, as in the laft experiment, into a jar containing oxygene gas, and inverted over water to which fulphuric acid had been added. At the end of three days it began to grow, and this procefs went on to the 29th. The water had now rifen confiderably in the jar, the gas having fuffered a diminution of about one-third. The barley being withdrawn fmelled completely of malt, and tafted fweet. The gas in the jar, on examination, was found to confift of 64 parts carbonic acid, 32 azote, and 4 oxygene; from which it would appear that the air employed in this experiment had contained originally about 20 per cent. of azotic gas.

To be more certain of the nature of the change which the pure air undergoes in this procefs, the experiment was repeated as follows:

January 23. A quantity of barley, foaked in water for two days, was introduced into a jar containing 46 meafures of very pure oxygene gas, and inverted over mercury. At the end of three days the barley began to grow, and this procefs continued for ten days, although very flowly: the column of gas remained exactly of the fame height, fo that it had undergone no apparent diminution or increafe: the barley being withdrawn, the air in the jar was examined, and found to confift of carbonic acid gas, mixed with only 1-50th of its bulk of oxygene gas. The barley was partly converted into malt, the quantity of oxygene being infufficient to produce this change upon the whole.

Another

Another experiment with common air was made at the ſame time, and exactly under ſimilar circumſtances. In this caſe the barley did not begin to grow until the end of the fourth day; and at the end of ten days had made much leſs progreſs than that in the oxygene gas. It was now withdrawn, and the air in the jar, which had increaſed a little, examined; when it was found to conſiſt of carbonic acid and azotic gas, in the proportion of one to two very nearly, mixed with a very ſmall quantity of oxygene gas. A little of the barley taſted ſweet.

Being now ſatisfied that during the evolution of the ſaccharine principle from vegetable mucilage, a quantity of oxygene was either abſorbed or converted into carbonic acid, we wiſhed to know if this proceſs could take place in any degree without the preſence of this gas.

In order to determine this point the following experiments were made:

January 20. A quantity of barley, ſoaked as in the former experiments, was introduced into a jar filled with, and inverted over, mercury. At the expiration of 12 days a very conſiderable quantity of gas was produced, at leaſt five or ſix times the bulk of the barley; but nothing like vegetation was perceivable. The gas, on examination, was found to conſiſt of carbonic acid, being entirely abſorbed by lime-water. The barley had not the leaſt ſweet taſte, nor did it appear to have undergone any ſenſible change.

On January 20th, another portion of the ſame ſoaked barley was introduced into a wine-glaſs, and placed in a jar containing nitrous gas inverted over water. At the expiration of ten days the gas had undergone a ſlight diminution, but there was not the ſmalleſt appearance of vegetation. The barley being withdrawn and examined was found to have undergone no apparent change. The gas contained about one-ninth of its bulk of carbonic acid, the remainder being pure nitrous gas, as was manifeſt from the diminution it underwent when mixed with pure air. The nitrous gas which diſappeared in this inſtance muſt have been abſorbed either by the barley or the water. The carbonic acid which was found mixed with it is accounted for by the laſt experiment.

Two other portions of ſoaked barley were introduced into jars, the one containing hydrogenous and the other azotic gas, and inverted over mercury. At the expiration of 12 or 14 days there was not the leaſt appearance of vegetation in either, but the gas in both had increaſed in bulk about one-fifth. The barley being withdrawn and examined, that in the hydrogenous gas taſted muſty, but not in the leaſt ſweet; the portion in the azote appeared to have undergone no change. The gas in both jars contained from one-third to one-fourth of its bulk of carbonic acid, the remainder being the original gaſes not ſenſibly changed.

From theſe experiments, therefore, it is manifeſt that oxygene is abſolutely neceſſary for the converſion of vegetable mucilage into ſugar; as in no one inſtance was ſaccharine matter formed where this was not preſent, and the quantity of the former was always in proportion to that of the latter; for we found in all the experiments, that when the oxygene was conſumed this proceſs immediately ceaſed.

It may ſtill remain doubtful, whether the oxygene is abſorbed by the barley, or merely converted into carbonic acid: we are inclined to think that it is chiefly abſorbed, although part may alſo be conſumed in the formation of this acid; for we have ſeen that carbonic acid is formed without the preſence of oxygene gas, and that in very conſiderable quantity;

 which

which we conceive must proceed from the decomposition of the water, whose oxygene unites with the carbonaceous principle of the barley, whilst its hydrogene is fixed, and may be necessary to the production of the saccharine principle. We suppose, therefore, that vegetable mucilage is converted into sugar by being deprived of part of its carbone, whilst at the same time it is combined with a greater proportion of oxygene, and probably also with hydrogene from the decomposition of the water. Thus, then, both from analysis and synthesis, it would appear that sugar contains more oxygene than gum or mucilage. From this hypothesis it should follow, that if sugar be deprived of part of its oxygene, it must lose its sweetness, and form something like a gum. To see how far this might be accomplished was the object of the following experiments:

A quantity of syrup was introduced into a jar, filled with, and inverted over, mercury: to this was admitted about an equal quantity of the phosphuret of lime: a considerable production of phosphoric gas almost immediately took place, and the mercury descended in the jar. At the expiration of eight days the syrup was withdrawn and examined: it had no sensibly sweet taste, but rather a bitter astringent one: when filtered, alcohol produced a copious white precipitate in flakes, very much resembling mucilage separated from water by the same substance.

This experiment was somewhat varied, as follows: A little refined sugar was dissolved in alcohol, and to this solution a little phosphuret of lime was added: no phosphoric gas was disengaged, nor was there any apparent action produced. More phosphuret being added, the mixture was allowed to remain in an open phial for several days. The alcohol having now evaporated, some distilled water was added; but this produced no disengagement of gas, as the phosphuret had been decomposed, and converted principally into phosphate of lime. The mixture being filtered, and the clear liquor evaporated, there remained a substance extremely tenacious, and which had much the appearance of gum-arabic: its taste was bitter, with a very slight degree of sweetness: when squeezed between the teeth it had exactly the feel of gum, but more tenacious. It did not appear to be soluble in alcohol, or at least in any considerable quantity: when thrown upon a red-hot iron it burned like gum, and left a bulky and insipid charcoal.

It would appear that the saccharine principle had been destroyed in these experiments, and converted into something resembling a gum. That this was effected by the abstraction of oxygene is rendered highly probable from the nature of the substance employed, and the change which it was found to have undergone; for there are few substances which have so strong a tendency to combine with oxygene as the phosphuret of lime.

Some other trials of a similar nature were made by mixing solutions of sugar with the different sulphurets, and by agitating them with nitrous gas in close vessels. The sulphurets, more especially that of pot-ash, manifestly destroyed the saccharine taste; but on account of the solubility of the different products, the particular change produced could not be so easily and accurately ascertained. The action of the nitrous gas was more doubtful.

In order to be satisfied how far the effects produced on the sugar in the former experiments might be owing to the abstraction of oxygene, I added to solutions of this substance in water both lime and pure pot-ash, and boiled the mixtures for some time: the lime appeared manifestly to combine with the sugar, to which it communicated a very bitter astringent taste, but it was still sweet: a little alcohol, added to the filtered solutions, pro-

duced

duced a precipitate in white flakes, fomewhat fimilar to that in the experiments with the fulphuret of lime, and which appeared to be a combination of fugar with lime. Some of the filtered folution being evaporated by a gentle heat, there remained a femi-tranfparent fubftance, much more tenacious than the thickeft fyrup, but not equal to that produced by the phofphuret of lime; and it had a rough bitter tafte, mixed with a certain degree of fweetnefs. The pot-afh, likewife, appeared to combine with the fugar, the fweet tafte being more completely deftroyed than by the lime: but on the addition of fulphuric acid, fulphat of pot-afh was formed; and this being precipitated by alcohol, the fweetnefs appeared to be completely reftored. It may likewife be proper to obferve, that when alcohol was added to a portion of the folution of fugar and pure pot-afh, after it had been boiled to the confiftence of a fyrup, no union took place; but the alcohol, notwithftanding the mixture was completely and repeatedly agitated, ftill fwam pure on the top: a circumftance which would feem to prove that a new compound is formed by thefe fubftances, which is not foluble in this fluid, although they are both completely fo in a feparate ftate.

Having found that fugar might be converted into a fpecies of gum by depriving it of part of its oxygene, we conceived that gum might, by the addition of oxygene, be changed into a fubftance refembling fugar: but although feveral trials were made with a view of combinining oxygene in different proportions with gum-arabic, no remarkably fweet tafte was ever perceived; on the contrary, in every experiment, it feemed to run very readily into the acid ftate, particularly when it was expofed to the action of the oxygenated muriatic acid gas.

Indeed, when we reflect on the change which vegetable mucilage muft undergo in the procefs of malting, the fimple addition of oxygene does not appear to be fufficient; for it is probable, from the decompofition of the water, that fome of its hydrogene is fixed, whilft its oxygene difengages and unites with a certain portion of charcoal, forming the carbonic acid. Although, therefore, fugar and mucilage confift of the fame principles, viz. carbone, hydrogene, and oxygene; yet unlefs thefe are combined in certain determinate proportions, the former, which when pure is no doubt always a fubftance of exactly the fame nature, cannot be produced: the hydrogene and carbone muft be accurately proportioned, as well as the oxygene *.

II.

An Account of the Manner in which Heat is propagated in Fluids, and its general Confequences in the Economy of the Univerfe. By BENJAMIN *Count of* RUMFORD.

[Continued from page 296.]

FROM the internal motions of the particles of fluids during the propagation of heat, and from this propagation being impeded by every thing which obftructs thofe motions, refults amounting to a theory of confiderable perfection were obtained. But the fubject in the hand of our author naturally produced other interefting confequences. It was by

* This paper is, with the author's confent, extracted from Dr. Rollo's account of two cafes of Diabetes Mellitus, of which an account is given in this Journal, p. 235.

the motions of very fine particles of dust, accidentally mixed with the spirits of wine in his large thermometer, and strongly illuminated by the sun, that he first discovered the internal motions which take place in that fluid during the time of its cooling. Reflecting on this fact, he immediately concluded that the internal motions of water might be rendered equally visible if he could find any solid body of the same specific gravity as water, and not liable to be dissolved by it; but such a substance was not to be found. On reflection, it occurred to him, that it is very fortunate that such substances do not abound; for otherwise we should find great difficulty in procuring water in a pure state.

As the solid could not be found, he determined to adapt the specific gravity of the fluid to that of a solid very little heavier than the water in its pure state. The solid he made choice of was transparent yellow amber, of the specific gravity 1,078, and he increased the density of the water by the addition of a certain quantity of pure alkaline salt.

The amber was broken into pieces about the size of mustard-seeds, and put into a glass vessel with water, to the bottom of which it sunk. Upon the gradual addition of an alkaline solution some of the pieces rose to the top, and others subsided to the bottom. The former were removed, and the fluid was carefully adjusted with regard to density, until the latter, at 60° of Fahrenheit, remained permanently suspended in the different parts of the fluid.

A glass vessel, consisting of a globe about two inches in diameter, with a cylindrical neck three quarters of an inch in diameter and twelve inches long, was filled with this prepared liquid. The first experiment made with this instrument was to plunge it into a tall glass jar, nearly filled with water almost boiling hot. Two currents, in opposite directions, began to move at the same instant with great celerity in the liquid in the cylindrical tube, the ascending current occupying the sides of the tube, while that which moved downwards occupied its axis.

As the saline liquor grew warm, the velocity of these currents gradually diminished; and at length when the liquor had acquired the temperature of the surrounding water in the jar the motion ceased entirely. On taking the glass body out of the water, the internal motions of the liquor recommenced: but the currents had changed their directions; that which occupied the axis of the tube being now the ascending current. When the cylindrical tube was inclined a little, the ascending current occupied the upper side, and the descending current the lower. Both ceased when the instrument had acquired the common temperature of the room.

In all cases when the instrument received heat, the current in the axis of the tube, or on its lower side if inclined, moved downwards; but when it parted with heat this motion was in the opposite direction. A change of a few degrees was sufficient to set the contents of the instrument in motion. By the application of an heated body, the fluid nearest the place of contact was made to ascend; but the contrary effect took place if the body were colder than the instrument. And in either case the motion thus produced was attended with a contrary motion in some other part of the instrument.

Among the applications made by this enlightened philosopher to an instrument so calculated to detect the secret operations of nature, one consisted in applying a lighted candle to the middle of the tube while it was inclined in an angle of about 45 degrees from the perpendicular. In this situation the motion of the fluid in the upper part of the tube became excessively rapid, while that in the lower end, where it was united to the globe, as well as that

that in the globe itself, remained almoft perfectly at reft. He even found that he could make the fluid in the upper part of the tube actually boil, without that in the lower part of it appearing to the hand to be fenfibly warmed. This fact not only ferved to fhew that heat is propagated in fluids chiefly, if not altogether, by virtue of thefe motions which arife from change of denfity, but alfo that this tranfportation cannot be effected in a downward direction.

It is an opinion generally received among philofophers, that water cannot be heated in contact with ice. Our author faw that this pofition muft be in a great meafure true with regard to water upon which ice floats; becaufe the hot water muft afcend and be cooled by caufing part of the ice to melt, while the cold water from this laft would defcend to the bottom, and tend to preferve the uniformity of temperature. But he faw likewife that the cafe would be very different if the ice were at the bottom. The inferences which this courfe of argument would point out are fingular and ftriking, and they are not lefs fo for being announced to us under the incontrovertible fanction of experiment. Natural as the induction is, from the valuable experiments of Count Rumford, it ftill feems in fome meafure aftonifhing to affert, that water may be actually made to boil, and kept in that ftate without melting a piece of ice, plunged in it, with more rapidity than could be effected by cold water at 40 degrees.

The direct experiments with ice and hot water are related at large in the Effay. A circular cake of ice, three inches and an half thick, weighing $10\frac{1}{8}$ ounces, and nearly as large as the internal diameter of a cylindrical glafs jar, which was 4,7 inches (its height being 14 inches), was gently put into the jar containing fix pounds $1\frac{1}{4}$ ounce troy of boiling-hot water. It was entirely melted in two minutes and fifty-eight feconds.

The fame experiment was repeated; but inftead of the ice being fuffered to fwim at the furface, it was faftened down in the bottom of the jar, and the hot water poured upon it. The ice was retained in the jar by means of two flender and elaftic pieces of deal about 1-8th of an inch thick, and 1-4th of an inch wide, which, being a very little longer than the internal diameter of the jar, were of courfe flightly bended when they were introduced into it horizontally; and on being put down to the ice at right angles to each other, they confined it from rifing to the furface when the water was added. A circular piece of ftrong writing-paper was laid upon the ice to protect it from the action of the boiling-hot water while poured, which was afterwards removed, as gently as poffible, by means of a ftring faftened on one fide; and to prevent the glafs jar from being cracked by the fudden application of the boiling-hot water, a fmall quantity of cold water was firft poured in, to cover the ice to the height of about a quarter of an inch. The hot water was poured againft the middle of the circular piece of paper.

The jar with the ice and the hot water in it being placed on a table near a window, the paper which covered the furface of the ice was gently drawn away, and the Count prepared himfelf to obferve at his eafe the refult of this moft interefting experiment.

A very few moments were fufficient to fhew that his expectation with regard to it would not be difappointed. In the former experiment a fimilar cake of ice had been entirely melted in lefs than three minutes; but in this, after more than twice that time had elapfed, the ice did not fhew any apparent figns of beginning to melt. Its furface remained fmooth and fhining, and the water immediately in contact with it appeared to be perfectly at reft;

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though the internal motions of the hot water above it, which was giving off its heat to the ſides of the jar and to the air, were very rapid, as was diſtinctly perceived by means of ſome earthy particles or other impurities which this water happened to contain.

The ice was examined with a very good lens, but it was a long time before any ſigns of its melting could be perceived. The edges of the cake remained ſharp; and the minute particles of duſt, which by degrees were precipitated by the hot water as it grew colder, remained motionleſs as ſoon as they touched the ſurface of the ice.

As the hot water had been brought from the kitchen in a tea-kettle, it was not quite boiling-hot when it was poured into the jar. After it had been in the jar one minute, a thermometer was plunged into it, and its temperature found to be at 180.

After 12 minutes had elapſed, its temperature at the depth of one inch under the ſurface was 170°. At the depth of ſeven inches, or one inch above the ſurface of the ice, it was at 169½; while at only 3-4ths of an inch lower, or 1-4th above the ſurface of the ice, its temperature was 40°.

When 20 minutes had elapſed, the heat in the water at different depths was found to be as follows:

	Degrees.
Immediately above the ſurface of the ice	40
At the diſtance of half an inch above it	46
At 1 Inch	130
At 3 Inches	159
At 7 Inches	160

When 35 minutes had elapſed, the heat was as follows:

At the ſurface of the ice	40
Half an Inch above it	76
1 Inch above it	110
2 Inches	144
3 Inches	148
5 Inches	148½
7 Inches	149

At the end of one hour the heat was as follows:

At the ſurface of the ice	40
1 Inch above it	80
2 Inches	118
3 Inches	128
4 Inches	130
7 Inches	131

After one hour and 15 minutes had elapſed, the heat was found to be as follows:

	Degrees.
At the ſurface of the ice	40
1 Inch above it	82
2 Inches	106
3 Inches	123

The heat of the water had hitherto been taken near the ſide of the jar:—in the two following trials it was meaſured in the middle or axis of the jar.

When

When one hour and 30 minutes (reckoning always from the time when the boiling water was poured into the jar) had elapsed, the heat of the water in the middle of the jar was found to be as follows:

	Degrees.
At the surface of the ice — — —	40
1 Inch above it — — —	84
2 Inches — — —	115
3 Inches — — — —	116
7 Inches — — — —	117

When two hours had elapsed, the heat in the middle of the jar was found to be as follows:

	Degrees.
At the surface of the ice — — —	40
1 Inch above it — — —	76
2 Inches — — — —	94
3 Inches — — — —	106
4 Inches — — —	108
6 Inches — — — —	$108\frac{1}{4}$
7 Inches — — — —	$108\frac{1}{2}$

An end being now put to the experiment, the hot water was poured off from the ice; and on weighing that which remained, it was found that five ounces six grains troy (= 2406 grains) of ice had been melted.

Taking the mean temperature of the water at the end of the experiment at 106°, our author remarks that the mass of hot water (which weighed $73\frac{1}{4}$ ounces) was cooled 78 degrees, or from the temperature of 184° to that of 106° during the experiment. Now as it is known that one ounce of ice absorbs just as much heat in being changed to water as one ounce of water loses in being cooled 140 degrees, it is evident that one ounce of water which is cooled 78 degrees, gives off as much heat as would be sufficient to melt $\frac{78}{140}$ths of an ounce of ice; consequently the $73\frac{1}{4}$ ounces of hot water which in this experiment were cooled 78 degrees, actually gave off as much heat as would have been sufficient to have melted $\frac{73\frac{1}{4} \times 78}{40} = 40\frac{3}{10}$ ounces of ice.

But the quantity of ice actually melted was only about five ounces; and hence it appears that less than one-eighth part of the heat lost by the water was communicated to the ice, the rest being carried off by the air.

As the same quantity of hot water was used in this experiment and in that which immediately preceded it, and as this water was contained by the same vessel, it appears that ice melts more than eighty times slower at the bottom of a mass of boiling-hot water than when it is suffered to swim on its surface. For as in the one experiment $10\frac{5}{8}$ ounces of ice were melted in two minutes and 58 seconds, five ounces at least must have been melted in one minute and 29 seconds; but in the other experiments two hours or 120 minutes were employed in melting five ounces.

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The ice however was melted, though very slowly, at the bottom of the hot water; and that circumstance alone would have been sufficient to have overturned the hypothesis respecting the propagation of heat in fluids solely by means of the intestine motion. This consideration demanded an attentive enquiry into the circumstances of the experiment. One of the most striking among these was, that at the end of the first half-hour, on examining the surface of the ice it was seen that it had been melted, excepting only where it had been covered, or as it were shadowed, by the flat slips of deal which secured the ice in its place. It was singular that the ice was defended, not only by the undermost piece of wood, but by the other, which, lying across the under piece, did not touch the ice any where except at its ends. It was natural to imagine, from this event, that the ice had been melted by radiant heat or calorific rays from the water, and that a portion of these had been intercepted by the slips of deal. This supposition pointed out another course of experimental enquiry.

Into a cylindrical glass jar, $6\frac{1}{2}$ inches in diameter, and eight inches high, was put a circular cake of ice, as long as could be made to enter the jar, and about $3\frac{1}{2}$ inches thick; and on the flat and even surface of the ice was placed a circular plate of the thinnest tin that could be procured, near $6\frac{1}{2}$ inches in diameter, or sufficiently large just to cover the ice. This plate of tin (which, to preserve its form or keep it quite flat, was strengthened by a strong wire, which went round it at its circumference) had a circular hole in its centre just two inches in diameter; and it was firmly fixed down on the upper surface of the cake of ice, by means of several thin wooden wedges which passed between its circumference and the sides of the jar.

A second circular plate of tin, with a circular hole in its centre, two inches in diameter, and in all other respects exactly like that already described, was now placed over the first, and parallel to it, at the distance of just one inch, and, like the first, was firmly fixed in its place by wooden wedges.

These perforated circular plates being fixed in their places, the jar was placed in a room where Fahrenheit's thermometer stood at 34°, and ice-cold water was poured into it, till the water just covered the upper plate; and then the jar was filled to within half an inch of its brim with boiling water, and, being covered over with a board, was suffered to remain quite two hours.

At the end of this time, the water, which was still warm, was poured off, and, the circular plate being removed, the ice was examined.

A circular excavation, just as large as the hole in the tin plate which covered the ice (namely, two inches in diameter), and corresponding with it, perfectly well defined, and about 2-10ths of an inch deep in the centre, had been made in the ice.

This was what our author expected to find; but there was something more which he did not expect, and which for some time he was quite at a loss to account for. Every part of the surface of the ice which had been covered by the tin plate appeared to be perfect, level, and smooth, and shewed no signs of its having been melted or diminished, excepting only in one place, where a channel about an inch wide, and a little more than 2-10ths of an inch deep, which shewed evident marks of having been formed by a stream of warm water, led from the excavation just mentioned in the centre of the upper part of the cake of ice to its circumference. As the edge or vertical side of the cake of ice was evidently worn away where this stream passed, there could be no doubt with respect to its direction.

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It certainly ran out of the circular excavation in the middle of the ice; and though it might at firſt appear difficult to explain the fact, and to ſhew how this hot water could arrive at that place; yet it was quite evident that the immediate cauſe of the motion of this ſtream of water could be no other than its ſpecific gravity being greater than that of the reſt of the water at the ſame depth; and that this greater ſpecific gravity was at the ſame time accompanied by a higher degree of heat, is evident from the deep channel which this ſtream had melted in the ice, while other parts of the ſurface of the ice at the ſame level were not melted by the water which reſted on it.

This experiment, though attended with remarkable circumſtances, did not clearly ſhew that the fuſion of the uncovered part of the ice had been effected by radiant heat. It occurred to the author, that radiant heat, like light, might probably be reflected downwards in part from the internal ſurface of the water; and conſequently, that it might be expected that a light black body, namely, a circular piece of deal board, covered over with black ſilk, would abſorb a portion of the whole body of rays which were directed to the ice. This trial was made, but with no ſenſible effect.

As it is uncertain whether heat in the radiant ſtate can be reflected in this manner, the experiment may perhaps be conſidered as likely to afford a leſs equivocal concluſion than might have been obtained from the application of a poliſhed metallic ſurface to the ſuperior termination of the cylinder of water. But the appearance of a channel worn in the plate of ice in the preceding experiment gave riſe to meditations which clearly pointed out the manner in which the ice was melted, and rendered the attempts to detect the ſuppoſed operation of latent heat unneceſſary. The Count's explanation, in his own words, is as follows:

Though it is one of the moſt general laws of nature with which we are acquainted, that all bodies, ſolids as well as fluids, are condenſed by cold, yet, in regard to water, there appears to be a very remarkable exception to this law. Water, like all other known bodies, is indeed condenſed by cold at every degree of temperature which is conſiderably higher than that of freezing; but its condenſation, on parting with heat, does not go on till it is changed to ice; but when in cooling its temperature has reached to 40 degrees of Fahrenheit's ſcale, or eight degrees above freezing, it ceaſes to be farther condenſed; and on being cooled ſtill farther it actually expands, and continues to expand as it goes on to loſe more of its heat, till at laſt it freezes; and at the moment when it becomes ſolid, and even after it has become ſolid, it expands ſtill more on growing colder. This fact, which is * noticed by M. de Luc, in his excellent Treatiſe on the Modifications of the Atmoſphere, has ſince been farther inveſtigated and put beyond all doubt by Sir Charles Blagden. (See Philoſophical Tranſactions, vol. lxxviii.)

Now, as water in contact with melting ice is always at the temperature of 32°, it is evident that water at that temperature muſt be ſpecifically lighter than water which is eight degrees warmer, or at the temperature of 40°: conſequently, if two parcels of water at theſe two temperatures be contained in the ſame veſſel, that which is the coldeſt and lighteſt muſt neceſſarily give place to that which is warmer and heavier, and currents of the warmer water will deſcend in that which is colder.

* The ſimple fact was alſo obſerved by the celebrated Robert Boyle.

In the two last experiments, as the circular tin plate which covered the surface of the ice served to confine the thin sheet of water which was between the plate and the ice—as this water could not rise upwards, being hindered by the plate—and as it had no tendency to descend—it is probable that it remained in its place; and as it was ice-cold, it was not capable of melting the ice on which it reposed. But as the tin plate had a circular hole in its centre, the surface of the ice in that part was of course naked; and the ice-cold water in contact with it being displaced by the warmer and heavier water from above, an excavation in the form of a shallow basin was formed in the ice by this descending warm current.

The warm water contained in this basin overflowed its banks as soon as the basin began to be formed; and issuing out on that side which happened to be the lowest, opened itself a passage under the tin plate to the edge of the ice, over which it was precipitated, and fell down to the bottom of the jar. The water of this rivulet being warm, it soon formed for itself a deep channel in the ice; and at the end of the experiment it was found to be everywhere deeper than the bottom of the basin where it took its rise.

This manner of accounting for the appearances in question seemed to the Count to be quite satisfactory; and the more he meditated on the subject, the more he was confirmed in his suspicions that all liquids must necessarily be perfect non-conductors of heat.

On these principles he was now enabled to account for the melting of the ice at the bottom of the hot water, as also for the slowness with which that process went on; and encouraged by this success he proceeded with confidence to plan and to execute still more decisive experiments; from the results of which he considers the important facts in question to have been put beyond all possibility of doubt.

If water be in fact a perfect non-conductor of heat, that is to say, if there be no communication whatever of heat between neighbouring particles or molecules of that fluid (which is what he supposes); then, as heat cannot be propagated in it, but only in consequence of the motions occasioned in the fluid by the changes in the specific gravity of those particles which are occasioned by the changes of their temperature, it follows that heat cannot be propagated downwards in water, as long as that fluid continues to be condensed with cold; and that it is only in that direction (downwards) that it can be propagated after the water has arrived at that temperature where it begins to be expanded by cold; which has been found to be at about the 40th degree of Fahrenheit's scale.

Reasoning on these principles, he was led to this remarkable conclusion: namely, that *water which is only eight degrees above the freezing point, or at the temperature of* 40 *degrees, must be able to melt as much ice in any given time, when standing on its surface, as an equal volume of water at any higher temperature, even though it were boiling-hot.*

The experiments by which this unexpected result was confirmed and established must be deferred to the concluding part of this abstract.

III. *Experi-*

III.

Experiments and Observations made with the View of ascertaining the Nature of the Gaz produced by passing Electric Discharges through Water; with a Description of the Apparatus for these Experiments. By GEORGE PEARSON, *M. D. F. R. S.*

[Concluded from page 248.]

THE fire of the electric discharge, in a very condensed state, passes with inconceivable velocity through the whole length of the upper wire, in the case of the interrupted explosion, p. 243—247, and of the single wire in the case of the complete explosion, p. 247, 248; so that it neither exerts its energy on the wire, nor on the water, till it arrives at the extremity of the wire. There it is momentarily interrupted and accumulated; and, in the moment before its diffusion through the water, it is so dense and in such quantity as to manifest itself by a spark at, or nearly in contact with, the extremity of the wire. In the moment of its diffusion, a small part of this condensed fire interposes betwixt the constituent elements of the ultimate and invisible particles of water, that is, betwixt the hydrogen and oxygen, of which water is compounded, so as to place them beyond the sphere of their chemical attraction for one another; and each ultimate particle of hydrogen and of oxygen uniting with a determinate quantity of fire, new compound ultimate particles, consisting of hydrogen and caloric, and of oxygen and caloric, that is, hydrogen gaz and oxygen gaz, are compounded. This mode of action of electric fire on water is confirmed by the effect of electric fire and common fire, of a due degree of density, on oxide of quicksilver, which is by them resolved into oxygen gaz, and quicksilver in the vapour state. All calculation must needs be extremely vague; yet, perhaps, some elucidation will be obtained by considering that as it probably requires 70 or 80 thousand discharges to produce a cubic inch of gaz from the supposed decompounded water, the gaz produced by each discharge cannot amount to one 200,000dth of a grain weight. The quantity of condensed fire at the extremity of the wire must be immensely great, comparatively with the quantity of it which enters into the composition of the gazes from decompounded water; otherwise it is not easy to conceive how even the minutest particle of water could be decompounded, the electric fire being in contact with a large body of water, and passing through it with a velocity which is incalculable.

The reason of the metal wire not uniting to the oxygen of the decompounded water, as in the experiment of passing water through red-hot iron tubes, might be assigned from the intensity of the fire only; but it is also on account of the rapid motion of the discharge, as well as, partly, from the great quantity of light. In a very low temperature, light decompounds oxide of silver, and of several other metals; also oxy-muriatic acid, nitric acid, &c. Hence light both decompounds bodies, and prevents oxygen from coming within the sphere of chemical attraction of the metal.

It is supposed that but a very minute proportion indeed, of the electric discharge, is consumed at the end of the wire in the composition of gazes, during a momentary interruption, as above said; for it diffuses itself through the water, between the wires, yielding a volume of vivid light, till it arrives at the extremity of the under wire or point of the

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convexity of the metal cup, where it is again condensed by the superior conducting power of the metal to that of water, and where, if it be in due quantity, and of sufficient density, it manifests itself by a spark, and infallibly again decompounds water: hence bubbles are seen to rise from the end of the lower wire, and from the metal cup, as well as from the point of the upper wire.

From these interpretations it will not be difficult to explain the reason of a spark appearing in some cases at the points of both the upper and under wire; why in other cases it appears at the point of the upper wire only; why in others it appears at the point of the lower wire only; namely, according to the density of the fire of the discharge there accumulated.

Concerning the agency of electrical fire in causing the hydrogen and oxygen gaz of the supposed decompounded water to undergo combustion and produce water, it is well known that the smallest visible spark, or particle of flame, or fire, can kindle as rapidly a very large quantity of hydrogen and oxygen gaz, as the greatest quantity of flame, sparks, or fire, can kindle the smallest quantity of these two gazes; while, on the other hand, the largest mass of matter, heated most intensely, but short of ignition, cannot produce combustion of oxygen and hydrogen gaz.

Although, as hath been above explained, caloric and light, in a sufficiently dense state, may decompound every compound substance in nature; it is also well ascertained, that caloric, in certain states, universally promotes chemical combinations. The mode of agency, in this latter case, I apprehend to be in a different way from that commonly accepted; for I think it is unnecessary to suppose that it operates by increasing the power of chemical attraction, and I conceive its agency to be merely diminishing or destroying the powers which counteract chemical union; especially diminishing cohesive attraction, and exciting motion among the particles of the different substances: hence these substances are applied to one another, within their spheres of chemical attraction; or the chemical attraction acts between a greater number of points of the different bodies, as when caloric renders solids and inelastic fluids into the elastic fluid state.

Accordingly, when an electric spark, or the smallest particle of flame, or of an ignited substance, is applied to the gaz produced in the above process, or to the mixture of hydrogen and oxygen gaz, the ultimate particles of these gazes nearest to the flame are driven from it in all directions, as from a centre, by the interposition of fire, or of caloric and light; so that they are brought within the sphere of their chemical attraction for the ultimate particles of the gazes at a certain distance from the centre of application of fire; which therefore unite, and the caloric and light, disengaged by that union, act in a similar manner in producing union among the next set in order of proximity, of the ultimate particles of the gazes; the disengaged caloric and light of which act in producing union of the next set of ultimate particles in order; and so on successively, but with incalculable velocity, the greatest bulk of ultimate particles of the two gazes unite with one another; the known products of which union are fire and water; or light, caloric, and water. According to this hypothesis, if caloric or fire, or merely caloric of sufficient intensity and quantity, be applied to a given bulk of hydrogen and oxygen gaz, no combustion should be produced; as the caloric will be interposed in such quantity that all the ultimate particles must be at the same instant driven from one another in all directions, so that they are beyond their spheres fpheres of chemical attraction for one another; and in various inftances common experience verifies this hypothefis.

If light be confidered as a different fpecies of fubftance from caloric, then the theory of its agency, lately publifhed by Dr. Parr, M. D. may be applied very happily to explain the explofion from the combuftion of oxygen with hydrogen gaz. From a very large induction of facts, Dr. Parr infers that, although light and caloric fubfift together very commonly in the fame compound fubftance, they fimultaneoufly repel each other on the decompofition of the fubftances with which they were united; and from this repulfion, and alfo the repulfion of oxygen and light of one another, he chiefly accounts for the combuftion of oxygen gaz.

On my principles above ftated, we can explain why quickfilver of the temperature of 1000°, or more, of Fahrenheit's fcale, cannot unite to the oxygen of oxygen gaz; but why at this temperature caloric feparates oxygen in the gaz ftate from oxide of quickfilver: and why at the temperature of between 600 and 1000° quickfilver does unite to the oxygen of oxygen gaz; but at which temperature oxygen gaz is not feparable from the oxide of this metal: and again, why at below 600° oxygen of oxygen gaz can neither unite to quickfilver, nor be feparated from its oxide.

Thefe principles may be applied varioufly to the interpretation of the phenomena of combuftion, and other cafes of chemical combination, according to the ftate of aggregation of the fubftances which have a chemical attraction for one another. For example; the oxygen of oxygen gaz cannot unite to the conftituent fubftances of a wax or tallow candle, in a low temperature; becaufe the cohefive attraction of the wax or tallow, as well as the chemical attraction between their conftituent fubftances—hydrogen and carbon—counteract the chemical attraction between the oxygen of oxygen gaz, and the ultimate particles of hydrogen and carbon of the candle. But at a pretty elevated temperature, when the wax and tallow are in the vapour ftate, the cohefive attraction no longer fubfifting among the ultimate particles of thefe fubftances, and the motion excited by the ignited portion of wax or tallow bringing the ultimate particles of thefe fubftances within the fphere of chemical attraction of the particles of oxygen gaz, thefe latter unite firft to the carbon, by virtue of the ftronger attraction between the oxygen and carbon, than that between the oxygen and hydrogen; but, in the inftant of this difengagement of the carbon from the hydrogen, a portion of this hydrogen unites alfo to oxygen, and thus not only carbonic acid gaz but water is produced: hence the *blue flame*, as appears on other occafions, in which there is combuftion of hydro-carbonate gaz. The remainder of the hydrogen of the decompounded wax or tallow, afcending in the gaz ftate, it unites to the oxygen of oxygen gaz; and, as in other cafes of combuftion of oxygen gaz with hydrogen, a *white* or *ftraw coloured flame* is produced. The wick anfwers the purpofe, by means of capillary attraction, of applying the wax and tallow in fuch quantities as can be decompounded and combine with the oxygen of atmofpherical air; hence the combuftion is gradual and equal. The wick itfelf contains hydrogen and carbon; hence, in combining with oxygen of oxygen gaz, it alfo produces a *blue flame*. Hydro-carbonate gaz being fpecifically heavier than hydrogen gaz, is another reafon for the blue flame appearing diftinct from the white, and at the inferior part of the *fruftum* of a cone of flame from a burning candle. It is fcarcely neceffary to fay that hydrogen and carbon are conftituent fubftances of wax and tallow; and

and that when they combine with oxygen, as above explained, the products are water and hydro-carbonate gaz.

If I did not use the term *demonstration* in a more strict and precise sense than is usual, except in mathematics, I would venture to affirm that this theory is almost demonstrated by the agency of fire and carbon on black oxide of manganese. If colourless vitrifiable matter, and that oxide be melted together, by means of the *white* part of the flame of a candle, purple-coloured glass will be produced; for then the hydrogen of this part of the flame carries off little or none of the oxygen from the oxide: but if these substances be melted together by the *blue* part of the flame, colourless glass will be produced; for then the carbon of this part of the flame carries off oxygen from the black oxide, and produces white oxide. By combining oxygen anew with colourless glass, containing manganese, it will become purple; and this is effected by melting such colourless glass with a little nitrate of pot-ash, or by melting it in open vessels by the yellow flame of a candle. By separating oxygen from glass rendered purple by manganese, colourless glass is produced; and this is effected by melting such coloured glass with a little carbon, or by the blue flame of a candle. I have not thought it necessary to distinguish between the indigo and violet rays; or to notice the extremely thin film of violet flame, which an attentive observer may perceive surrounding the inferior part of the white, extending as high but scarcely higher than the wick; because the explanation is perfectly obvious from the preceding distinction of the two kinds of gazes afforded by the candle. If I have explained more satisfactorily than former members of this society the above phenomena of combustion, I owe this advantage to some experiments of combustion of inflammable gazes which I have made for some years past, in my chemical lectures; by which the colours are shewn to be very different, and correspond to the above theory. I apprehend the theory of an ingenious member of this society cannot explain adequately the phenomena, and does not appear supported by any facts: for there is no evidence that the white flame is not equally the effect of immediate decomposition as well as the blue; or that the blue becomes white flame by ignition; and I presume that the experiments which I have mentioned shew that these differences depend upon different decompounding substances contained in the candle.

With regard to the evidence afforded by the foregoing experiments concerning the composition of water and of hydrogen and oxygen gaz. These substances are now accounted for in two ways only; namely, 1. By saying that these two gazes consist of water and imponderable matter; and that during combustion the water is precipitated. 2. By saying that the two gazes consist of a peculiar basis, one of which is named oxygen and the other is hydrogen, each of which is rendered into the gaz state by uniting to caloric, and perhaps also to light; and that during combustion these bases unite with one another, thus compounding water and discharging caloric and light. If complete demonstration could be given, there would not be two opinions; for its proofs, if understood, command universal assent: but the case being otherwise, that opinion must be adopted on the side of which the evidence preponderates according to the laws of reasoning in physical science. Now with regard to the former of these opinions, I can perceive but two facts in support of it. The first of these is supposed to afford a sort of synthetical proof: it is the instance of water being required to obtain, by fire, the whole of the carbonic acid from carbonate of baryt. Here the fact, if admitted, is true only of carbonic acid; but even in this case it has not been

been shewn, that the water enters into the composition of carbonic acid; and until that is proved, it is warrantable to suppose that the water serves to disengage the carbonic acid, by attracting and uniting with the baryt; especially as this acid gaz may be wholly separated from other substances without the intermediation of water, and as it can be compounded by uniting the driest carbon to the driest oxygen gaz. Further, although carbonic acid gaz be very apt to contain water, it may be obtained in so dry a state as not to indicate moisture to the most delicate tests.

The next fact affords a sort of analytical proof; which is the existence of water in air or gaz in general, as shewn by muriate of lime, acetite of pot-ash, sulphuric acid, quicklime, pot-ash, &c. But the absorption of water by these substances only shews that it may be suspended or dissolved in gazes; but not that it enters into their composition. Gaz may be obtained or rendered quite free from water, as just said; and as the compounds by the combination of the gazes with various different substances are quite different from the compounds by the combination of water with the same various different substances, there does not appear to be in this case any admissible evidence of water being an essential constituent substance in the composition of gazes.

The experiments of separating oxygen and nitrogen gaz from water by boiling, and by exposing it in vacuo and applying caloric, only shew how much more tenaciously, and in how much greater quantity, these gazes are contained in water than is commonly supposed; for however long a given quantity of water yielded gaz, so that there seemed no limit to the quantity, the water which afforded it was still very considerably greater than the weight or quantity of gaz.

With respect to the other opinion, that hydrogen and oxygen gaz are compounded of a peculiar basis and caloric, and that water is compounded of these two bases—1. It has been shewn by an experiment, the accuracy of which is not questionable, that when these two gazes produce water by combustion, the amount of the weight of the water is exactly the weight of the gazes consumed, and that no ponderable matter but water is produced when nothing but the bases of these two gazes unite; but that when other substances, such as nitrous acid and carbonic acid, were produced, as well as water, then nitrogen and carbon were present. 2. It has been shewn, that when oxygen in a concrete state, as in metallic oxides, unites to the hydrogen of hydrogen gaz, the water thus compounded is equal in weight to that of the hydrogen gaz consumed, and the deficient weight of the oxide. 3. It appears that when hydrogen in a concrete state, as in alcohol, is united to the oxygen of oxygen gaz, the water compounded is equal in weight to the weight of the alcohol and oxygen gaz consumed, provided the addition be made of the carbonic acid also produced by the carbon of the alcohol. The carbon of alcohol can be shewn by analysis; and its existence in carbonic acid can be shewn both by analysis and synthesis. 4. When water is applied to certain substances, nothing but hydrogen gaz is obtained, and new compounds, the constituent parts of which are oxygen and the substance applied; for these new compounds have the same properties as the compounds of the same substances so applied to water, and of the oxygen of oxygen gaz. And in these cases the additional weight derived from the water, together with the weight of the hydrogen gaz separated from the water, is equal to the water destroyed. 5. It has been rendered at least very probable, that when oxygen and hydrogen are rendered into the gaz state, they absorb or unite with a large quantity of caloric, or of both caloric and light. I might add, that befides the pofitive evidence juft ftated, there does not appear to be any evidence to contravene the conclufion that water is compounded of hydrogen and oxygen; and that thefe gazes are compounded of a peculiar bafis, called hydrogen and oxygen, united to caloric: for it would be eafy to fhew, but it would be digreffing too far, that the remaining partizans of the phlogiftic fect have advanced erroneous evidence in fome cafes, and in others have neglected the confideration of oppofing but well authenticated evidence.

In the experiments contained in this paper no fupport can be found for the former opinion concerning water, and hydrogen and oxygen gaz; but they confirm the latter opinion. For, 1. the combuftion of thefe gazes, rendered perfectly dry, afforded water, p. 303, Exp. IV. 2. The evidence from the procefs above defcribed is peculiar; for no one other procefs affords oxygen and hydrogen gaz from water. In all other proceffes for decompounding water, either the decompounding concrete fubftance receives fomething from the water applied, and at the fame time hydrogen gaz is produced; as in the inftance of paffing water through a red-hot iron tube: or the decompounding concrete fubftance receives fomething from the water; and this compound unites to that in water, which produces hydrogen gaz with caloric; as in the inftance of applying water to red-hot carbon: or two decompounding fubftances being applied, each of them receives a different thing from the water, namely, hydrogen and oxygen. Hence, by the procefs above defcribed, the objection is removed, that the hydrogen gaz might be feparated from the decompounding fubftance itfelf, by water taking its place. 3. The production of hydrogen and oxygen gaz, and the production of water by the combuftion of them, in the above procefs, afford an additional evidence of the decompofition and compofition of water, and of hydrogen and oxygen gaz; as the mode of their production by this procefs is perfectly confiftent with the rationale of their production in all other cafes.

Thus it appears that the grandeft difcovery ever made in chemical philofophy, by an illuftrious member of this Society, in 1781, has been confirmed by a number of fubfequent experiments. The body of evidence is indeed fo numerous, and of fuch a nature, that, in the minds of thofe who underftand its import, and who rely on the accuracy of the weights and meafures employed, it produces as much conviction concerning the compofition of water as can be obtained by the evidence of almoft any other cafe of compofition. I muft, however, beg leave to proteft againft thofe able philofophers, who have maintained, that the compofition of water, and feveral gazes, has received full and complete demonftration; by fuch unwarrantable pretenfions their adverfaries have obtained over them fome advantages. For in the chain of caufes and effects there are fome links which cannot be explained by the direct evidence of fenfe; and there, in fo far as we admit hypothefes, although confiftent with the phenomena, we may be faid to quit day-light, notwithftanding we at the next ftep emerge into light derived from the perceptions of fenfe. For inftance, if I combine a certain known weight of hydrogen and oxygen by combuftion of their gazes, and produce water equal in weight to that of thefe gazes; and if I again refolve this water, by means of the electric fire, or red-hot iron, into the original quantity of hydrogen gaz and oxygen, I cannot give the full and complete demonftration of the compofition of water and thefe gazes: for, as I proceed in the interpretation, I at length come to demonftrate the mode of agency of the particles of the hydrogen and oxygen gaz on one another

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when they produce water, caloric, and light. But here I muft call in the aid of the imagination: accordingly I imagine that the gazes confift of hydrogen and oxygen, which are ponderable—united to caloric, and perhaps light, which are imponderable; that thefe ponderable particles unite with one another, and their caloric and light are fet at liberty. Now here I have not any evidence of fenfe; for I cannot perceive, by the fenfes, the exiftence of the compofition of the gazes juft ftated, nor of their decompofition, and union of their ponderable parts. This being the cafe, other kinds of imponderable matter may unite, or efcape, befides caloric and light; confequently I cannot give the full and complete demonftration in thefe inftances. But the fame objections may be made to the pretenfions to demonftrate fully and completely the compofition of fulphate of foda, fulphate of pot-afh, or any double falt, perhaps, whatever; for caloric is feparated, and poffibly other imponderable matter, when thefe fubftances are compounded; and caloric, and perhaps other matter, may unite when the fulphuric acid and alkali are difunited.

Hence chemiftry, in its prefent ftate, ought not to pretend to vie with mathematical philofophy in its demonftrations. But it does not appear improbable, that the fame certainty as in mathematics may hereafter be attained in chemiftry. We are encouraged to entertain this hope, from finding that the art of obfervation, and the invention of artifices for rendering the properties of matter evident to the fenfes, have been proportionate to the advances of knowledge of facts; as was predicted by Chancellor Bacon.

IV.

Obfervations on the Electrophore, tending to explain the Means by which the Torpedo and other Fifh communicate the Electric Shock.

THOSE who are converfant with electrical experiments know that the electrophore confifts of a flat metallic plate with an infulating handle, and another feparate plate of non-conducting matter, either varnifhed glafs, or fome refinous fubftance, coated with metal beneath, and placed with its uncoated fide uppermoft. When this uncoated face is electrified by friction or otherwife, and the plate of metal placed upon it, this laft is found to give a fmall fpark to the finger, which is of the fame nature as the electricity of the non-conducting furface; but when the plate is lifted by its infulating handle, it will emit a fpark to the finger which is much ftronger, and of the oppofite kind to that of the non-conducting plate. This phenomenon, which was at firft confidered as very difficult to be explained, was afterwards found to be produced throughout by the fame energies, whatever they may be, which govern the electric charges. For, as I have elfewhere obferved *, the charge confifts of two electricities, ufually called plus and minus, which compenfate each other; and alfo of a portion of electricity on the infulated fide, which in equal charges is greater, the greater the diftance between the two electrified furfaces, and in unequal moderate charges is nearly in proportion to the charge itfelf. When the non-conducting furface of the electrophore is rubbed, it acquires both thefe electricities, namely, the charge by virtue of the compenfating power of the uninfulated coating beneath, and alfo the portion of fimple electricity requifite to maintain the charge. If the metallic plate be then

* Philofophical Tranfactions, 1789.

placed upon it, and touched by the finger, a portion of the electricity may really paſs to the metal at the few places of actual contact; and another part, if the intenſity be ſtrong, may ſtrike through the thin plate of air interſperſed between the metal and the reſinous face. What remains on the reſinous ſurface will loſe by far the greateſt part of its intenſity, in conſequence of its becoming a charge to that ſmall plate of air, while the metallic plate, poſſeſſing the oppoſite part of the charge, will have its intenſity equally low. During theſe charges, the ſpark which paſſes from the metal muſt therefore be of the ſame character as that of the reſinous plate; in the ſame manner as the outer coating of the jar emits poſitive ſparks during its tranſition to the negative ſtate. The commencing intenſity of this ſpark cannot exceed that which belonged to the charge between the reſinous ſurface and the lower coating: but as the intenſity requiſite to maintain a charge on the thin plate of air is extremely minute, by far the greater part of the electricity will be employed in conſtituting that charge, and the ſpark given off by the plate will be much ſhorter and more gradual than if the ſame quantity had paſſed at once from the plate in a ſtate of ſimple electrization; that is to ſay, the ſpark will be apparently very ſmall. But when the plate is raiſed up by its inſulating handle, part of the charge on the reſinous ſurface will begin to acquire its former ſtate with regard to the lower coating; and that which continues to be compenſated by the upper moveable plate, will require a greater portion of uncompenſated electricity to maintain it. The intenſity of the plate will rapidly increaſe as it riſes. It will throw out ſparks and ramifications to the lower plate, and to the ſurrounding bodies; and at a certain diſtance the whole remaining portion of its former charge may be conſidered as ſimple electricity, which will ſtrike the finger at a greater diſtance, and with more ſuddenneſs and brilliancy. This ſpark, though in fact conſiſting of leſs electricity than the former, will nevertheleſs be more perceptible both to the eye and the ear, and conſequently will be thought larger: the firſt is the gradual exploſion of a charge; the ſecond conſiſts of the ſudden eſcape of a portion of ſimple electricity.

If we ſuppoſe the non-conducting matter of the electrophore to be extremely thin, and the ſpark when in contact to be made to paſs from the upper to the lower metallic plate, the effect will be nearly the ſame as the tranſition of the ſhock from ſuch a coated electric with a charge of the quantity of electricity which paſſes; and when the moveable plate is raiſed, the effect will be ſimilar to that of a ſpark from the prime conductor. Or rather, perhaps, by a compariſon familiar to electricians, the firſt may be conſidered as the ſhock from a large battery charged by only one turn of the handle of the machine, and the latter as the ſhock from a ſmall jar capable of being charged high by the very ſame quantity of electric matter. It is certain that the ſhock, as well as the ſpark from the battery, would in theſe circumſtances be inconſiderable, though the effects of the jar might be very ſtriking.

When the electrophore is thus compared with the jar, its charge will be found to be wonderfully ſmall, as it conſiſts of no more electricity than, when uncompenſated, would paſs off in the ſimple ſpark which was obtained.

I found that two ſquare inches of Muſcovy talc, about one-hundredth part of an inch thick, which is an exceedingly good non-conductor, required, when coated with tinfoil, one turn of a ſmall cylinder to diſcharge through one-tenth of an inch; and one turn of the ſame cylinder charged a ſimple conductor of about ſix ſquare feet ſurface, ſo as to give a ſpark about nine inches long. Now if we aſſume the quantities of electricity in conductors to be as the length of the fpark, which, though a doubtful pofition in great intenfities, may ferve the prefent occafion, and that a talc electrophore of two fquare inches furface on each fide have the intenfity to give a fpark of one-tenth of an inch, its whole electricity will be expreffed by $4 \times 0{,}1 = 0{,}4$. But the whole electricity of the conductor which gave the nine inch fpark will be expreffed by $6 \times 144 \times 9 = 7776$. And this number doubled, or 15552, for a double electrophore, alfo expreffes fuch a charge of the talc as gives a fpark of explofion or difcharge of 0,1 inch. The length of the explofive fpark, on applying fuch a fimple electrophore to its plate, will therefore be fhorter than 0,1 in the proportion of 0,4 to 15552, or 1 to 38880; the fpark would therefore be the ,000002 of an inch long. And as Mr. Cavendifh has found that electric fhocks nearly equal are produced when the quantities of electricity are inverfely as the lengths of the fpark of difcharge, and alfo that the quantities of electricity in charges are as the furfaces, it will follow, that the fhock of two fquare inches of the talc with a fpark of 0,1 inch, will be equal to the fhock of 77760 fquare inches with a fpark of 0,000002 inches. This furface is equivalent to an electrophore of 279 inches fquare, or meafuring 23 feet by the fide; or 19440 of the double electrophores of two inches furface.

To elucidate this, I conftructed a fmall electrophore adapted to perform the experiments which Beccaria called vindicating electricity. It confifted of two metallic plates with infulating handles. Each was a fquare whofe fide meafured two inches, and the face of each was coated with the thin talc. When the two uncovered furfaces of the talc were applied together, and the whole charged as one plate, the difcharge gave a denfe fhock of confiderable feverity. The fparks, on feparation, were about one-eighth of an inch long, and very weak. No perceptible fpark was exhibited on bringing them together. The operation appeared capable of being repeated almoft inceffantly, as in the electrophore.

It may perhaps be fimpler to confider the electrophore as a compound jar or plate variable in its thicknefs. When the plates of the inftrument juft defcribed are together, the two electricities on the contiguous furface compenfate each other, and the external coatings may be fuppofed nearly in the natural ftate. But when they are feparated, the portion of compenfated electricity will be lefs, and the intenfities would rife if it were not for the external coatings, which in a great meafure prevent it; each plate becoming charged like a fimple jar, and the equilibrium being poffible to be reftored by communication between the external coatings. This fecond charge, intenfity, and explofion will be greater the further the plates are removed. It appears, therefore, that in a deduction of the manner in which electric fifh may communicate the fhock, we may fafely avoid the more complicated confideration of the electrophore, and compare the action immediately to that of a fimple jar.

Mr. Hunter, in the 63d volume of the Philofophical Tranfactions, page 434, defcribes the electric organ of the torpedo to confift of a number of columns varying in their length from an inch and an half to a quarter of an inch, and their diameters about 2-10ths of an inch. The number of columns in each organ of the torpedo he prefented to the Royal Society was about 470; but in a very large torpedo the number of columns in one organ was 1182. Thefe columns were compofed of films parallel to the bafe of each, and the diftance between each partition of the columns was 1-150th of an inch. If we fuppofe thefe films to be charged with electricity, and to be 1-300th of an inch thick; and a middling fize torpedo to contain in both organs on the whole 1000 columns of an inch long, and 0,03

fquare

ſquare inches area at the baſe; then $1000 \times 150 \times 0{,}03 = 4500$ ſquare inches. Now I found that Muſcovy talc, of 0,01 inch thick, has twelve times the capacity of the glaſs of a jar of 421 ſquare inches, which, from former experience, I know to be as thin as ſuch jars can be without danger of breaking by exploſion; and the torpedinal membrane, being leſs than one third the thickneſs of the talc, will have three times the capacity; that is to ſay, its capacity will be thirty-ſix times that of ſtout glaſs; or both organs will be equivalent to $4500 \times 36 = 162000$ ſquare inches, or 1125 ſquare feet.

My large jar, with Lane's electrometer, meaſuring a ſpark about 1-200th of an inch long, gave very ſenſible and rather unpleaſant ſhocks acroſs the hand, and alſo the tremulous ſenſation cauſed as I ſuppoſe by the imperfect conducting power of the ſkin with ſo low an intenſity. When the ſpark was 1-100th of an inch long, the ſhock was ſtrong enough to convulſe the hand, and at leſs than 1-50th it was painfully ſtrong. This laſt was probably ſtronger than the ſhock of the torpedo. If we therefore avail ourſelves of Mr. Cavendiſh's deduction, that, the quantity of electricity being increaſed in proportion as the length of ſpark is diminiſhed, the ſhock will be rather greater than before, we may compute the length of the ſpark in the torpedinal ſhock of the magnitude laſt mentioned. For the organs of the torpedo, compared as to their capacity with that of the jar, will be equivalent to $\frac{162000}{421} = 375$ times as great, and they will give ſuch a ſhock when charged as to afford a ſpark $\frac{1}{375}$ of $\frac{1}{50}$ of an inch $= \frac{1}{18750}$ inch. No wonder, therefore, that the ſhock will not paſs through an interrupted circuit, and that the ſpark is not exhibited.

Reſpecting the manner of operation, there are no facts which ſhew how this charge is actually produced, maintained, and communicated. Whether electricity be actually collected, compoſed, or decompoſed in the organs of the fiſh, or whether it ſimply exiſts in thoſe organs, as perhaps it may in all bodies, in the ſtate of what is called compenſation, are queſtions concerning which we in fact know nothing. It has appeared to me, from the obſervation of the high electric ſtate which talc naturally poſſeſſes, and from the innumerable ſhocks the electrophore is capable of giving by mere change of arrangement, that a machine might be conſtructed alſo capable of giving numberleſs ſhocks at pleaſure, and of retaining its power for months, years, or to an extent of time of which the limits can be determined only by experiment. I will not here deſcribe the mechanical combinations which have occurred to me in meditating on this ſubject, but ſhall ſimply ſhew that the dimenſions of the organs of the torpedo are ſuch as by certain very poſſible motions, and the allowable ſuppoſition of conducting and non-conducting powers, may produce the effects we obſerve. How far it may be probable muſt undoubtedly be left to future experimental reſearch.

In new talc, which had never been excited nor electrified, and exhibited no ſigns of electricity when applied to Bennet's electrometer, I found that the laminæ were naturally in ſtrong, oppoſite, electric ſtates, counterbalancing each other. When they were torn aſunder in the dark, they gave flaſhes at leaſt 1-10th of an inch long to each other. This is 1875 times the intenſity of the torpedinal electricity, as before deduced. If, therefore, one or more columns of talc, or other thin electric plates 1-300th of an inch thick, and making up the ſurface of the electric organs of the torpedo, were ſo conſtructed as that the plates might touch each other by pairs only, naturally in oppoſite ſtates, and coated on the outſide;

fide; if, moreover, there were one common conductor communicating with the upper plate of every pair, and another in the fame manner with the lower;—then a feparation of all the pairs to the diftance of only $\frac{1}{15750}$ inch would produce the torpedinal intenfity; the equilibrium would be reftored by the two conductors if made to communicate, and whatever living creature was in the circuit would receive a fhock: and on reftoring the original fituation of the apparatus, a fhock might alfo be given. The force of thefe fhocks would differ according to the quantity of the apparatus made ufe of at the time, or the diftance to which the plates were drawn afunder. If different columns were exploded in rapid fucceffion, the quick repetition of fmall fhocks would produce the tremulous fenfation.

If we were to conjecture that the torpedo actually operates like a machine of this kind, we fhould find our fuppofition to include the following fubordinate parts:—1. The membranes may be non-conductors, and the fluid between them a conductor. 2. They may act as electrophores. 3. The white reticular matter between the columns may confift of conductors feparately leading to the two oppofite furfaces. 6. Thefe feparate conductors, in all their fubdivifions, may be well kept afunder by a covering of non-electric matter. If this be of the fame kind as the membranes, and $\frac{1}{1000}$th of an inch thick, it would be fufficient for the purpofe, becaufe the intenfity of the electric ftate is deduced from its power of breaking through a much more permeable electric, namely, air, at nearly twice that interval. 7. The effects may alfo be produced by the motion of conducting plates in a non-conducting fluid.

V.

A Letter from Mr. VON HUMBOLDT to M. H. VAN MONS on the Chemical Procefs of Vitality; together with the Extract of a Letter from Citizen FOURCROY to Citizen VAN MONS on the fame Subject.*

I HAVE lately addreffed feveral letters to Meffrs. Dolomieu and Fourcroy at Paris, and perceive by thofe I have received from the former that mine have mifcarried. Permit me, Sir, to addrefs myfelf to you. By your means I may perhaps fucceed in forwarding to Paris fome explanations refpecting facts which, as I underftand, employ part of the time of the National Inftitute. Be pleafed to accept my affurances of the great refpect which your zeal and your chemical difcoveries have infpired me with. The natural philofophers of Europe ought to form a fingle family. They are in purfuit of the fame interefting objects; and this is a fufficient motive to produce that ufeful degree of intimacy which is calculated to promote their refearches.

You are probably acquainted with my Effays on the Vegetable Philofophy, fuch as my *Aphorifmi ex doctrinâ phyfiologiæ chimicæ plantarum*, annexed to my *Flora fubterranea Fribergenfis*, and feveral memoirs which I have prefented to the National Inftitute.——The memoir on the action of oxygenated muriatic acid upon the vegetable and animal fibre, which is printed in the *Magazin Encyclopédique* of Millin, Noël, and Warens, feems to have

* Annales de Chimie, XXII. 64.

had

had more fuccefs. I am happy to hear that Meffrs. Vauquelin and my friend Dolomieu have begun to repeat my experiments. As the memoir which was read to the National Inftitute related principally to the germination of vegetables, I have thought it my duty to announce to you certain facts more ftriking refpecting the animal fibre. The ftrongeft ftimulus of the nervous fibre is that of the alkalis. It appears that thefe falts affect the irritable and fenfible fyftem by means of their azote. Let the thigh of a frog be thrown into the oxygenated muriatic acid, or the nitric acid, and it will remain motionlefs. Let it be put into a folution of pot-afh, or of foda, and it will undergo contractions no lefs ftrong than when irritated by the metals. Thefe motions always commence at the lower extremities. The toes move firft, afterwards the *mufculus gaftrocnemius*, and then the thigh. If the nerve be very fenfible (for nothing more is required than fimply to immerfe the extremity of the crural nerve in the oleum tartari per deliquium), the contractions will end in an univerfal tenfion or rigidity. The leg rifes up perpendicularly, the membrane of the feet extends itfelf, and the tetanus appears. In this fituation all the irritability of the fibre appears to be extinguifhed; and if an electric ftroke be paffed through the limb, the exhauftion becomes real. It is a ftriking phenomenon to fee the laft remaining figns of tetanus difappear in an inftant. But there is another method by which the tenfion difappears, and by which I am able to reftore the irritability to the organs. It feems that the acidifiable bafes of the alkali, principally the azote, have confumed all the oxygene contained in the fibre. The chemical procefs of vitality ceafes. If I pour an acid, for example the nitric acid, upon the nerve, an effervefcence will take place; part of the alkali becomes latent, and the reft will have a proper proportion with refpect to its oxygene. From this moment the contraction with zinc and filver is again produced. Increafe the quantity of acid, and the movements are again weakened. In this manner it is, that by forming an equilibrium between the azote of the alkali and the oxygene of the acid applied to the animal fibre, the irritability of the organs may be taken away or reftored three or four times in fucceffion. You may eafily perceive, Sir, that thefe experiments require fteady attention. The degree of infenfibility to which the nerve is reduced by repeating them may be very different. It is poffible to determine exactly the quality of the chemical agents, their weight and temperature; notwithftanding which, many experiments do not fucceed. The reafon is, that there are conditions which depend on the individuality of the organization, and concerning which we muft ftill confefs our total ignorance. The influences of the oxygenated muriatic acid upon the animal fibre are lefs marked than thofe of the alkalis; but they are neverthelefs of much importance. I fteeped the feet of a frog (I mention this animal by preference, though I have made the fame experiments on other fpecies) in a folution of opium in alcohol. The metals, or galvanifm, excited no motion. I threw one leg into pure water, and the other into the oxygenated muriatic acid; the firft remained motionlefs, the fecond gave very ftrong contractions, and fhewed that its irritability was reftored. The common acids deprefs the irritability of the nervous fibre. A crural nerve, rendered infenfible by the ordinary muriatic acid, remains fo though it has been fteeped in the folution of pot-afh: but the mineral acids exhauft the forces of the mufcles, by condenfing the elements of the mufcular fibre. Thefe acids act in the fame manner as cold, which depreffes the nerves, and is beneficial to the mufcles. The mufcles and the nerves have fpecific ftimuli, agreeable to the diverfity of the elements. The terrible action which the alkalis exercife on the nerves

nerves appears to explain the effect of the fecretion of the feminal liquor on the blood. It is this alkali which, diftributed throughout the fyftem, anfwers the purpofe of a ftimulus beneficial to the animal fibre. By this action I account for the ferocity of the ichthyophagi.

My eldeft brother, who is very fkilful in the ftudy of anatomy, applied zinc and filver to the mouth and the brain of a dead fifh; it afforded no motion. I poured oxygenated muriatic acid on the nerves, and at that inftant the contractions became very ftrong. Mr. Herz and feveral learned men of Berlin were prefent at thefe and many other experiments. The heart of the fame fifh, which had entirely ceafed to palpitate, began to perform this movement with regularity when I threw it into the oxygenated muriatic acid. The fame experiment fucceeded very often with the hearts of frogs: when a heart is immerfed in a folution of pot-afh, it lofes its irritability for ever; fo that azote is not the fpecific ftimulus of the heart.

Mr. Pfaff, while employed in my experiments refpecting germination in the oxygenated muriatic acid, has difcovered that frogs fuffocated in the oxygenated muriatic acid gas exhibit a very high degree of irritability after their death. I beg you will fix the attention of Mr. Vauquelin on the action of fulphate of pot-afh upon the nerves. I have been aftonifhed at every thing I beheld. Two legs of frogs in a very lively ftate were fteeped in the folution of the fulphate of pot-afh. I tried them three or four minutes afterwards with the metals. The contractions had increafed in force, and were even convulfive. It appeared that the three acidifiable bafes contained in the folution hydrogene, azote, and fulphur, acted ftrongly on the oxygene conveyed by the arterial blood. This action revives the procefs of vitality. After fourteen or fixteen minutes the whole thigh became of a blackifh brown. All the oxygene of the blood was abforbed, aud the carburet of hydrogene appeared in a difengaged ftate. The zinc and the filver are not then capable of exciting the fmalleft motion.

Yet it would be a great miftake to conclude that all irritability is exhaufted in this cafe. I have feen the contractions re-appear feveral times on reftoring oxygene to the fibre by means of a folution of the oxide of arfenic. The flame is thus renewed which feemed ready to expire. The oxide of arfenic produces a tetanus and perfect infenfibility if the nerve remains long immerfed. It feems then that the too great quantity of oxygene abforbs as it were the acidifiable bafes which fupport the chemical procefs of vitality. I have thrown the whole thigh into the folution of pot-afh, and I obferved that galvanifm afterwards had the power of exciting motion.

You fee, Sir, what an immenfe number of experiments remain to be made on thefe objects of vital chemiftry. It is enough that a method has been pointed out of meafuring the degree of irritability of the organic parts by means of galvanifm. I fhall have the honour to fend you my work on the nervous and mufcular fibre, and on the chemical procefs of vitality. I collect facts, and miftruft my own hypothetical ideas. You will perceive with me how miftaken the notion is that oxygene performs the principal part in this procefs. My experiments prove that the irritability or tone of the fibre depends only on the mutual equilibrium between all the elements of the fibre, azote, hydrogene, carbone, oxygene, fulphur, phofphorus, &c. The chemical combinations of phofphorus and of azote, for example, appear to be in no refpect lefs important than thofe of oxygene with the acidifiable bafes. How much light may we not expect from the advances of yourfelf, Fourcroy, and Vauquelin on thefe objects!

VON HUMBOLDT.

ADDITION to the foregoing LETTER.

HAVING preferved fome frogs for the winter, I have this morning repeated fome experiments, of which I venture to fend you an account. In the preceding letter I have remarked that, as we are only fuperficially acquainted with the principles of vital chemiftry, we ought not to be furprifed if we do not always obtain the fame refults. A negative experiment proves nothing againft another of an affirmative nature.

I am very fure that a nerve rendered infenfible by alcohol will not recover its irritability by fulphate of pot-afh. But it may very well happen that a thigh, of which the tetanus has been caufed by the oxide of arfenic, fhould remain in a ftate of tenfion notwithftanding the action of the folution of pot-afh.

I have feen the following facts within this quarter of an hour. I took the four extremities of a very lively frog. The right arm and the right leg leaped on zinc and filver. I fteeped them for four minutes in alcohol. The hydrogene acted ftrongly on the fibre. The toes of the foot trembled during the firft minute Soon afterwards a total rigidity came on; the mufcle became white, the blood having apparently loft its oxygene. I replaced the arm and the leg on the zinc and filver, but there was not the flighteft contraction. I then quickly threw them into the oxygenated muriatic acid, which I had fhaken ftrongly before it was poured out; the limbs remained in it for three minutes. A flight tremulous motion fhewed, even in the cup, that the vital forces were reftored. I replaced the arm and the leg on the metals; the contractions were again produced, not only with zinc and filver, but with zinc and iron.

Here I think is a very fimple and decifive experiment. I then changed the method in order to obferve the effect. I took the left thigh, and immerfed it for nine minutes in alcohol. It loft all irritability, and the oxygenated muriatic acid was no longer capable of reftoring the vital force. The left arm had remained untouched for fifteen or eighteen minutes. I prepared its nerve, but it fhewed only very weak and flow contractions with zinc and filver. I threw it into alcohol. After the firft minute its irritability was increafed, the galvanifm acted more ftrongly; but after three minutes all the irritability was exhaufted, and I applied in vain the remedy of oxygenated muriatic acid. I fteeped the arm in the folution of the oxide of arfenic, and it then afforded contractions, though very weak.

Here are four experiments, two of which fucceeded, and in the two others the vital forces were not reftored. I think, neverthelefs, that in good logic we ought to admit the affirmative experiments. Examine the conditions, and you will fee they are very different. The left leg remained too long, nearly nine minutes, in the alcohol. The right arm was already very weak when the experiment began. Who can boaft of reviving the dead?—If of two chemifts the one fhould obtain oxygene gas by heating the red oxide of mercury, while the other did not obtain it, we fhould always believe that the apparatus of the latter was not hermetically clofed. I never faw an organ rendered infenfible by alcohol which recovered its irritability by being left to itfelf. It neceffarily follows, therefore, that in the experiments I have ventured to relate, and of which my work contains a very great number, the oxygene of the muriatic acid muft have been a principal agent. The art of medicine will be infinitely benefited if we fhould fucceed in obferving the phenomena which the feveral elements produce in contact with the irritable fibre. It is proper to begin with fimple combinations, and afterwards proceed to combinations of two, three, and four principles.

I have fent to the National Inftitute a memoir on the nature of light, and its chemical combinations. Mr. Wedgwood pretends that the phofphorefcence of calcined bodies is not altered in hydrogene and azotic gas. I think he did not purify thefe gafes by means of phofphorus, as I did. I have feen luminous wood extinguifhed in the azotic and hydrogene gafes. A fmall quantity of oxygene, being admitted into the veffel, caufes the whole of the phofphorefcence * to revive. I have alfo converted morilles (Phallus Efculentus) into a fubftance which refembles tallow, by means of the fulphureous acid. I have made foap of it.

Bayreuth, December 29, 1796. VON HUMBOLDT.

Extract of a Letter from Citizen FOURCROY *to Citizen* VAN MONS *on the Subject of that of Mr.* HUMBOLDT.

I THINK Mr. Humboldt proceeds with rather too much expedition in his folutions. It is to be feared that he may find it neceffary to retract. I am apprehenfive that he admits too many hypothefes †: that he does not repeat each experiment fufficiently before he forms a conclufion. This in particular is much more important with regard to the philofophy of animal bodies than any other branch of fcience, becaufe it is furrounded by numberlefs difficulties and multiplied fources of error and illufion. I fear that if certain chemifts continue to advance with fuch rapidity, the phyficians will foon have reafon to exclaim againft this encroachment. If applications be too fuddenly made, and arbitrary fuppofitions accumulated, it may perhaps come to pafs that this fcience may a fecond time be rejected from the healing art, as it was formerly by Stahl and Boerhaave, in confequence of the exceffive abufe of hypothefes committed before their time in this refpect by Tachenius, Willis, and others. Too much earneftnefs may be equally pernicious to chemiftry as well as medicine, and impede the progrefs which the firft is capable of producing, and ought to produce, in the fecond.

Meffrs. Girtanner and Valli appear to me to make an ill ufe of their abilities and knowledge in this refpect. They fuffer themfelves to be carried away by the ingenious notions they derive from modern chemiftry.

All this, however, does not prevent my being of opinion that the experiments of Mr. Humboldt are extremely interefting, and that he ought to continue them with affiduity; but I fhould wifh that he would vary them more, repeat each in particular more frequently, and be moderate in his conclufions. I cannot, for example, repeat to you how many new ideas and chemical explanations, very probable in themfelves, have occurred to me during

* Spallanzani obferved the fame phenomena; and, what is more remarkable, he obferved the phofphoric or fhining animals ceafed to emit light in the azote, hydrogene and carbonic gafes, and that they emitted a light infinitely more vivid in oxygene gas than in the atmofpheric air. Chimico Efame degli Efperimenti de Goettling. Modena, 1796.

† For example, in the preceding memoir he fpeaks of the azote of alkalis as if it were demonftrated that azote is one of the principles of thefe bodies. I firft announced or fufpected this eight years ago, but it is not yet proved. F.

ſeveral years paſt, in conſequence of my reſearches into the animal analyſis. They would be ſufficient to change the aſpect of phyſiology and medicine; but I have been careful not to publiſh them until well matured and proved by experience, leſt I ſhould otherwiſe embarraſs two ſciences at once. I wiſh to riſk nothing of this kind, but proceed gently, and hope to arrive at ſolid concluſions in the courſe of time. I am earneſtly deſirous of ſeeing the work of Mr. Hildebrandt, as well as that of M. Humboldt. Notwithſtanding the ſpeed they appear to ſhew in their chemical explanations of vegetable and animal life, I know not why I am perſuaded that they are leſs advanced than we in the analyſis and true intimate knowledge of the materials of theſe two kingdoms. I very much commend their zeal, and admire their bold advances; but they cannot blame our well-grounded caution and prudent ſlowneſs. It is admirable to proceed with expedition, and make great advances in the paths of nature; but it is ſtill better to obſerve well, to ſee clearly, and to communicate with accuracy what we obſerve in our progreſs. I am ſtill on my journey, and confeſs that I am very far from having arrived at the place I am deſirous of reaching.

The young men have attended with ardour my courſe of Animal Chemiſtry at the School of Medicine. Nothing can equal their wiſh to learn. The twenty lectures I give on this part of chemiſtry, which is ſo new, produce, as I ſee, a great movement in this branch of the ſtudy of nature. But I moderate their enthuſiaſm as much as I can. I fear leſt by too much precipitation this beautiful machine ſhould be broken in my hands. It would be much to be regretted if expectations ſo rich and ſo happy were to be diſſipated in ſmoke; and this will not fail to happen if the edifice be built on hypotheſes, or too much haſte be made in conſtruction before the materials are ready. I collect them by degrees, but they are ſtill too ſcanty to riſk the formation of a ſyſtem. Yet, to ſpeak plainly, I think there are few chemiſts who poſſeſs more facts than myſelf on the animal analyſis: but they are not yet ſufficiently connected in their relations to each other to form an entire work. If the attempt were made, it certainly would not be *Ære perennius*, &c. &c.

VI.

Concerning the Properties of the Sulphureous Acid, and its Combinations with Earthy and Alkaline Baſes. By Citizens FOURCROY *and* VAUQUELIN.

Concluded from page 318.

Sulphite of Soda.] THIS ſalt is white, and perfectly tranſparent. Its figure a four-ſided priſm, two of the ſides being very broad, and two narrow, terminating in dihedral pyramids. Its taſte is cool, and afterwards ſulphureous.

Its habitudes in the fire are abſolutely the ſame as thoſe of the ſulphite of pot-aſh, excepting only that it commences its alterations by the aqueous fuſion.

By expoſure to the air it effloreſces, and is afterwards converted into ſulphat, but leſs ſpeedily than the ſulphite of pot-aſh.

It requires four parts of water for its ſolution; is more abundantly ſoluble in hot water, and readily cryſtallizes by cooling.

Barytes,

Barytes, lime, and pot-afh decompofe the fulphite of foda.

It is not foluble in alcohol.

Salts with bafes of pot-afh, except the carbonate, do not decompofe this falt; the other genera decompofe it like the fulphate of pot-afh.

The metallic oxides, and their folutions in acids, have the fame effects with the fulphite of foda as with that of pot-afh.

It contains per quintal—1. Soda 18,8;—2. Sulphuric acid 31,2;—3. Water 50.

Sulphite of Ammoniac.] This falt has the form of a prifm of fix fides, terminated by fix-fided pyramids. It fometimes affumes the figure of a fquare tablet, the borders of which are floped fo as to form a folid of fix irregular faces. Its tafte is cool and penetrating, like that of the ammoniacal falts; but it leaves a fulphureous impreffion in the mouth.

By expofure to the air it attracts moifture, and foon afterwards paffes to the ftate of fulphat.

It is very foluble, and requires at moft its own weight of water for its folution.

Heat increafes its folubility, and it cryftallizes by cooling.

On the fire it is volatilized without decompofition.

Barytes, lime, pot-afh, and foda decompofe it in the cold; magnefia produces the fame effect by the affiftance of heat.

The acids act on the fulphite of ammoniac in the fame manner as on thofe of pot-afh and foda; but the refults are different.

Charcoal does not convert it into fulphuret, becaufe it rifes too fpeedily by heat.

It does not decompofe falts with bafes of pot-afh or of foda, but it decompofes thofe of lime, magnefia, barytes, and alumine, with which it forms infoluble precipitates.

Its habitudes with the metallic oxides and falts are nearly the fame as thofe of the fulphites of pot-afh and of foda, excepting that it forms with feveral of them triple falts, as we fhall more amply explain in another memoir on the metallic fulphites.

Its component parts in the quintal are—1. Ammoniac 29,07;—2. Sulphuric acid 60,06;—Water 10,87.

Sulphite of Lime.] The form of the fulphite of lime is that of a fix-fided prifm, terminating in a very long pyramid. Its tafte at firft is fcarcely perceptible; but when it has been kept for fome time in the mouth, it communicates to the tongue a tafte which is manifeftly fulphureous.

This falt, when well neutralifed, is very fparingly foluble in water; but it becomes foluble by an excefs of acid. In this way it may be obtained in cryftals; that is to fay, by expofing its folution in the fulphureous acid to the air. The acid is diffipated, and leaves the falt in a ftate of purity. Barytes alone, among the earths, is capable of decompofing it. This may be afcertained by mixing a folution of that earth with a folution of the neutral fulphite of lime, when a light precipitate is formed.

Heat converts it into fulphate, by depriving it of a portion of fulphur.

The mineral acids decompofe it, like the other fulphites.

The alkalis produce no change in this falt; for the alkaline fulphites are decompofed by lime.

Among the neutral falts, the alkaline carbonates, as well as the alkaline phofphates and fluates, alone decompofe it.

It does not acquire the ftate of fulphat by contact of the air, but very flowly.

Metallic

Metallic solutions decompose it, at least for the most part.

The quintal contains—1. Lime 47;—2. Sulphuric acid 48;—3. Water 5.

Sulphite of Magnesia.] This salt is white and transparent. Its form a depressed tetrahedron; its taste is mild and earthy at first, and afterwards sulphureous.

By exposure to heat, it softens, swells up, and becomes ductile, like gum; it loses about 0,45 of its weight by the desiccation.

If the heat be kept up after it has lost its water of crystallization, the sulphureous acid flies off, and the residue in the retort is magnesia, nearly pure. In this way we found that a quintal of this salt is composed of 16 parts magnesia, 39 sulphureous acid, and 45 water.

This salt is sparingly soluble in water, but very soluble in an excess of acid; and this solution, exposed to the air, crystallizes very readily, by losing its excess of acid.

It becomes opake in the air, and changes by degrees into sulphate; but for this purpose much time is required.

The fixed alkalis, lime and barytes, decompose it completely; ammoniac decomposes it in part only, and a triple salt is formed.

The alkaline and earthy salts, except those of alumine, decompose it likewise.

The mineral acids and metallic solutions produce the same effects on this salt as on the other sulphites.

Sulphite of Barytes.] This salt does not crystallize, has no perceptible taste, and is perfectly insoluble in water. For this reason the sulphureous acid carries down a precipitate from the aqueous solution of barytes. It is not rendered soluble by an excess of acid, like the other earthy sulphites.

Heat changes it into sulphate; but it does not pass to this state by exposure to the air, but with extreme difficulty.

No earth nor alkali decomposes it.

The acids decompose it; whence the sulphureous acid does not precipitate the barytic salts, as the sulphuric acid does.

Among the neutral salts there are none but the alkaline carbonates which decompose it.

The other properties of this salt are such as are common to the sulphites.

It is composed of—1. Barytes 59;—2. Sulphureous acid 39;—3. Water 2.

Sulphite of Alumine.] It is insoluble in water, but becomes abundantly so by excess of acid. Its dissolution does not crystallize by the contact of air, and becomes converted into a softish ductile mass.

Fire disengages the sulphuric acid without alteration.

All the alkalis and earths decompose it, as do also the mineral acids.

Its component parts are—44 alumine, 32 sulphureous acid, and 24 water.

From the facts described in this memoir it is evident that the sulphites possess very different properties from those of the sulphates; and that they follow peculiar laws of solution, crystallization, affinity, and decomposition.

In fact they possess—1. A sulphureous taste, similar to that of their acid. 2. They are decomposable by fire, either by the escape of their acid without alteration, or by losing a portion of sulphur and becoming converted into sulphates. 3. They are converted into sulphates by the contact of air, or of any other substance capable of affording oxygene; and their weight is increased by this conversion. 4. They are decomposed by most acids; which expel the

the ſulphureous acid with effervescence, and the production of a ſtrong penetrating odour. 5. They burn rapidly, and with flame, when heated with ſuper-oxygenated muriate of pot-aſh, or with ſaltpetre, and become ſulphates. 6. The alkaline ſulphites are more ſoluble than the ſulphates, and the earthy ſulphites are much leſs ſo. 7. Laſtly, the ſulphite of lime is not decompoſed by the alkalis, like the ſulphate.

VII.

*An Account of the Great Copper Works in the Isle of Anglesey. By Mr. ARTHUR AIKIN *.*

Auguſt 13, 1796.

THIS has been a moſt intereſting and entertaining day, being ſpent in viſiting the vaſt copper works connected with the Parys Mountain. We breakfaſted at Amlwch, a conſiderable town on the coaſt, about two miles from the mine, and almoſt entirely peopled by the miners and their families.

We had no difficulty in diſtinguiſhing this celebrated mountain, for it is perfectly barren from the ſummit to the plain below; not a ſingle ſhrub, and hardly a blade of graſs, being able to live in this ſulphureous atmoſphere.

"No graſſy mantle hides the ſable hills,
No flowery chaplet crowns the trickling rills;
Nor tufted moſs nor leathery lichen creeps
In ruſſet tapeſtry o'er the crumbling ſteeps."

DARWIN.

The nearer we approached the ſcene of buſineſs, the more penetrating was the fume of the ſulphur; but we had very ſoon too many objects of attention to regard this inconvenience. The mountain is about a mile in length, and is the property of Lord Uxbridge and the Rev. Mr. Hughes, and the fortunate diſcovery of the copper took place a little more than thirty years ago; thus converting a piece of ground, originally of very little value, into one of the moſt profitable eſtates in the kingdom.

The ſubſtance of the mountain being ore, the work is carried on in a very different manner from the cuſtom of other mines: here are, comparatively, few ſhafts or levels, the greater part being quarried out, ſo as to leave a vaſt excavation open to the day. There are two of theſe quarries or mines, which are worked by two different companies: the firſt goes by the name of the Mona Mine, and is the ſole property of Lord Uxbridge; the other, called the Parys Mine, is ſhared between the Earl and Mr. Hughes. The view down this ſteep and extenſive hollow is ſingularly ſtriking. The ſides are chiefly of a deep yellow or duſky ſlate colour, ſtreaked, however, here and there, by fine veins of blue or green ſhooting acroſs the cavern, mingled with ſeams of greyiſh yellow. The bottom of the pit is by no means regular, but exhibits large and deep burrows in various parts, where a richer vein has been followed in preference to the reſt. Every corner of this vaſt excavation reſounds with the noiſe of pickaxes and hammers: the edges are lined with workmen drawing up the ore

* Tour through North Wales.

from

from below; and at short intervals is heard, from different quarters, the loud explosion of the gunpowder by which the rock is blasted, reverberated in pealing echoes from every side.

The exterior covering of the mountain is an aluminous slate; the matrix black-grey chartz; the ore *copper*, chiefly

I. *The yellow sulphurated:* of which the richest contains, according to miners' computation, that is, in the proportions of the ounce troy,

Sulphur	——	——	5 dwt. (25 per cent.)
Copper	——	——	Ditto.
Refuse	——	——	10 dwt. (50 per cent.)

The worst ore yields nearly the same quantity of sulphur, but of metal no more than six grains ($1\frac{1}{4}$ per cent.); this inferior kind, however, is chiefly worked for the sulphur. The other species and varieties of ore that the mine produces are,

II. Black ore, containing copper, mixed with galena, calamine, and a little silver.

III. Malachite, or green and blue carbonate of copper.

IV. Native copper, but in very small quantity.

V. Sulphate of copper, crystallized and in solution.

VI. Sulphate of lead in considerable quantity, containing a pretty large proportion of silver.

VII. Native sulphur.

Process.—The ore is got from the mine by blasting; after which it is broken into smaller pieces by the hammer (this being chiefly done by women and children), and piled into a kiln, to which is attached by flues a long sulphur chamber. It is now covered close; a little fire is applied in different places, and the whole mass becomes gradually kindled; the sulphur sublimes to the top of the kiln, whence the flues convey it to the chamber appointed for its reception. This smouldering heat is kept up for six months, during which the sulphur chamber is cleared four times, at the expiration of which period the ore is sufficiently roasted. The poorest of this, that is, such as contains from $1\frac{1}{4}$ to 2 per cent. of metal, is then conveyed to the smelting-houses at Amlwch-port; the rest is sent to the company's furnaces at Swansea and Stanley near Liverpool. The greater part of the kilns are very long, about six feet high; and the sulphur chambers are of the same length and height, connected by three flues, and on the same level with the kilns; four new ones, however, have been built at Amlwch-port, by which much sulphur is preserved that would have been dissipated in the old kilns. The new ones are made like lime-kilns, with a contrivance to take out the roasted ore at the bottom, and thus keep up a perpetual fire. From the neck of the kiln branches off a single flue, which conveys the sulphur into a receiving chamber, built on the rock, so as to be on a level with the neck of the kiln, that is, above the ore.

The two smelting-houses, of which one belongs to each company, contain 31 reverberatory furnaces, the chimnies of which are 41 feet high; they are charged every five hours with 12 cwt. of ore, which yields $\frac{1}{2}$ cwt. of rough copper, containing 50 per cent. of pure metal; the price of rough copper is about 2l. 10s. per cwt. The coals are procured from Swansea and Liverpool, a great part of which is Wigan slack. From experiment it appears, that though a ton of coals will reduce more ore than the same quantity of slack, yet, owing to the

the difference of price, the latter is, upon the whole, preferable; the prices of the two at Liverpool being—coals 8s. 6d. per ton—flack 5s. per ditto.

The fulphate of copper, however, is the richeft ore that the mine yields, containing about 50 per cent. of pure metal. This is found in folution at the bottom of the mine, whence it is pumped up into cifterns, like tanners' pits, about two feet deep: of thefe pits there are many ranges, each range communicating with a fhallow pool of confiderable extent. Into thefe cifterns are put caft-iron plates, and other damaged iron veffels procured from Coalbrook Dale; when the fulphuric acid enters into combination with the iron, letting fall the copper in the form of a red fediment very flightly oxided. The cifterns are cleared once in a quarter of a year, when the fulphate of iron in folution is let off into the fhallow pool, and the copper is taken to a kiln, well dried, and is then ready for exportation. The fulphate of iron remaining in the pool partly decompofes by fpontaneous evaporation, and lets fall a yellow ochre, which is dried and fent to Liverpool and London.

The fulphur produced in the roafting, after being melted and refined, is caft into rolls and large cones, and fent to London. The cones are ufed chiefly for the manufactory of gunpowder and fulphuric acid.

Green vitriol and alum are alfo made in fmall quantities by a feparate company; but to thefe works ftrangers are not admitted.

The number of men employed by the two companies is 1200 miners, and about 90 fmelters; the miners are paid by the piece, and earn in general from a fhilling to twentypence per day.

The depth of the mine in the loweft part is 50 fathoms, and the ore continues as plentiful as ever, and of a quality rather fuperior to that which lay nearer the furface.

With regard to the annual quantity of ore raifed, little certain can be mentioned. The Parys Mine has furnifhed from 5000 to 10,000 tons per quarter, exclufive of what is procured from the fulphate of copper in folution; and as the two mines employ nearly equal numbers of workmen, they probably afford about the fame quantity of ore.

Adjoining to the fmelting-houfes is a rolling-mill, upon the fame conftruction as maltmills, for grinding the materials for fire-bricks; thefe confift of fragments of old fire-bricks, with clunch (a kind of magnefian clay found in coal-pits) procured from near Bangor-ferry.

The port of Amlwch is chiefly artificial, being cut out of the rock with much labour and expence, and is capable of containing 30 veffels of two hundred tons burthen; it is greatly expofed, and dangerous of accefs during high northerly winds, which drive a heavy fea up the neck of the harbour. The two companies employ 15 brigs from 100 to 150 tons burthen, befides floops and other craft, all of which lie dry at low water.

The various articles, the produce of the mines, which are exported, are the following:

I. Coarfe regulus of copper from the fmelting-houfes.
II. The richer copper ore roafted.
III. The dried precipitate of copper from the vitriol pits.
IV. Refined fulphur.
V. Ochre.
VI. Alum.
VII. Green vitriol.

 The

The town of Amlwch, which about 30 years ago had no more than half a dozen houfes in the whole parifh, now fupports a population of four or five thoufand inhabitants; and was at prefent, being market-day, thronged with miners and country-people. After dinner we walked along the fea fhore, climbing the fteep flate rocks, whence the water below appeared of a beautiful green, and fo tranfparent as to fhew the fhelving rocks to a great depth beneath.

Having heard that at Camlyn Bay, about eight miles weft of Amlwch, there were fome marble quarries, and that it furnifhed afbeftos, we refolved to fpend this day in vifiting it: the road lay in general about half a mile from the coaft; the fubftratum was waved green magnefian flate. When we arrived at Camlyn Bay, we looked in vain for marble or afbeftos, and proceeded homewards along the coaft. The fhore of Camlyn Bay confifts entirely of green and purple waved magnefian flate rock, with large veins of quartz. Having arrived at a promontory that feparates Cemmaes Bay from the former, we found it to confift of a fine blue-veined limeftone, or common marble. Some way on, near the village of Cemmaes, this limeftone is cut through by a ftratum defcending to the water, about 40 yards wide, of black flate, containing iron pyrites; and in the caverns dug in this, probably in a fruitlefs fearch after metals, are efflorefcences of fulphate of iron and chalybeate fprings. To this fucceeds a beautiful water-grey fand, mixed with lime but of little coherence, on expofure to the air, taking an ochrey ftain. Adjoining to this are a few yards of calcareous free-ftone, and then a cliff of very hard white and water-grey marble; a range of fand and loofe free-ftone fucceeds, and the bay terminates with a marble promontory. The foil of the land furrounding the bay is for the moft part, efpecially near the village, a deep fand. The limeftone terminates fhortly after, and the green-waved magnefian flate continues the boundary of the ifland. This ridge of lime is in general higher than the flate, defcribing an irregularly indented line of coaft, about four miles long: its breadth varies from a quarter to half a mile; and a narrow valley, forming its outline towards the land, feparates it entirely from the afbeftine flate, thus preventing any intermediate ftrata.

The whole of this coaft is cut out into bays or receffes of various forms and dimenfions, with lofty projecting promontories, which are for the moft part fine fheep-walks. A number of iflands alfo are formed by ledges of rock, many of them a good way out at fea, and at high water juft appearing like black fpots in the midft of the waves: many of thefe creeks are fecure havens for fmall veffels, which are protected from weft and fouth-weft winds by the rocks. The village of Cemmaes ftands upon a little creek opening into a moft beautiful bay about a mile acrofs; its entrance into the main fea is guarded on each fide by a craggy promontory, the one of grey, the other of fnow-white marble, gliftening above the green fea, fmooth as the furface of a mirror, and whofe fparkling tranfparency baffles defcription. In the interior recefs of the bay, the bank of black flate, mentioned above, was finely contrafted with a lofty irregular projecting arch of white marble, pierced by the conftant dafhing of the waves; while the founds of laughter and merriment, proceeding from two boats' crews of young people, that had juft pufhed out of the creek on a party of pleafure, added double life and intereft to this lovely fcene. The land adjoining the cliffs, that overlook the fea, produces a great deal of corn, chiefly oats and barley. A golden tinge already

already begins to appear, that will ufher in the harveft, as foon as the crop of hay with which the farmers are now bufied is fafely houfed.

As we approached Amlwch, we were much pleafed with feeing the fcars of rock between the town and fea occupied by numerous groupes of men, women and children, all neat and in their beft clothes, it being Sunday, who were enjoying the mild temperature of a fummer evening rendered refrefhing by the neighbourhood of the fea. In one place we obferved a circle of men gathered round a point of rock, in which was feated the orator of the party reading a newfpaper aloud, and commenting upon it; on other little eminences were feen family parties, the elder ones converfing, and the younger children gamboling about them, or running races with each other: in a new-mown meadow clofe to the town, we paffed by a large company of lads and laffes feated on a green bank, chatting, laughing, and full of mirth and frolic. To one who had been a fpectator of the grofs and riotous delight too frequent on holyday-evenings in the outfkirts of the metropolis, or any large town in England, the contraft could not fail of being very ftriking, and much to the advantage of the inhabitants of Amlwch: out of the whole number we did not fee one drinking party; the pleafures of fociety and mutual converfe needed not the aid of intoxication to heighten their relifh.

> Mean time the fong went round, and dance and fport,
> Wifdom and friendly talk, fucceffive, ftole
> Their hours away: while in the rofy vale
> Love breath'd his infant fighs, from anguifh free,
> And full replete with blifs; fave the fweet pain
> That inly thrilling but exalts it more.
>
> Harmonious Nature too look'd fmiling on:
> Clear fhone the fkies, cool'd with eternal gales,
> And balmy fpirit all. THOMSON.

I am acquainted with no place the manners of whofe inhabitants are fo unexceptionable (as far at leaft as a ftranger is enabled to judge of them) as Amlwch; and the favourable opinion which I was led to entertain of them, on vifiting the town laft year, is confirmed by what I have obferved at prefent. Not a fingle inftance have I known of drunkennefs; not one quarrel have I witneffed during two very crowded market days, and one of them a day of unufual indulgence, that I paffed at this place; and I believe no gaol or bridewell, or houfe of confinement, exifts in the town or neighbourhood. Moft of the miners are methodifts, and to the prevalence of this religious fect is chiefly to be attributed the good order that is fo confpicuous. Men who have been long confirmed in habits of vice and irregularity, need arguments the moft potent that can be offered to counterbalance the affociated power of habit and inclination: were it poffible forcibly to tear them from their connections, and to place them in an entirely different fituation, reafon might then be called in gradually to perfect the cure; but where this cannot be done, (and in moft cafes it is impracticable,) what argument can be urged of fuch overbearing force as to combat with and overthrow the moft rooted propenfities, even upon their own territory, unaffifted by external coercion, except a ftrong and impreffive appeal to their hopes and fears; and, by prefenting both exaggerated and in full contraft, to overwhelm the mind by furprife and alarm ?

After fupper we ftrolled up to the mountain, which now no longer refounded with the confufed noife of pickaxes and hammers; all was hufhed in profound filence; and the moon-beams, which were reflected bright from the fides of the vaft excavations, could fcarcely penetrate the deep abyfs below. As we returned we were ftruck with the clear red vivid flames iffuing in a large body from the long range of fmelting-houfes on the coaft, and cafting their rays to a great diftance.

VIII.

A Method of difpofing GUNTER'S *Line of Numbers, by which the Divifions are enlarged, and other Advantages obtained.*

OF the many ingenious inftruments for computation which were in ufe during the laft century, among mathematicians, fcarcely any are to be found at prefent except the fector and the logarithmic line of Gunter. The eafe and accuracy of computation by thofe admirable numbers have rendered the others of little importance; but the fector has maintained its ftation from its utility in graphical operations, and the Gunter's line is not only of great value in nautical and other proportions which do not exceed three places of figures, but alfo as a check to affure the truth of the leading figures in more extended calculations. About ten years ago I communicated to the Royal Society a method of extending the range of this laft inftrument, which I ftill confider as lefs generally known than its utility may perhaps claim. The principles depend on the following confiderations:

1. If two geometrical feries of numbers, having the fame common ratio, be placed in order with the terms oppofite each other, the ratio between any term in one feries and its oppofite in the other will be conftant *.

2. And the ratio of a term in one feries to any term in the other, will be the fame as obtains between any other two terms having the fame relative pofition and diftance †.

3. In all fuch pairs of geometrical feries as have the fame common ratio, the laft-mentioned property obtains, though the firft antecedent and confequent be taken in one pair, and the fecond in any other pair ‡.

4. If the differences of the logarithms of numbers be laid in order upon an arrangement of equi-diftant parallel right lines, in fuch a manner as that a right line drawn acrofs the

* Geom. feries $\begin{cases} a \;\; an \;\; an^2 \;\; an^3 \;\; an^4 \\ b \;\; bn \;\; bn^2 \;\; bn^3 \;\; bn^4 \end{cases}$

Then $a : b :: an : bn :: an^2 : bn^2$, &c.

† In the foregoing feries $a : bn^2 :: an^2 : bn^4 :: an : bn^3$, &c.

‡ Geom. feries $\begin{cases} a \;\; an \;\; an^2 \;\; an^3 \;\; an^4 \\ b \;\; bn \;\; bn^2 \;\; bn^3 \;\; bn^4 \end{cases}$

Geom. feries $\begin{cases} d \;\; dn \;\; dn^2 \;\; dn^3 \;\; dn^4 \\ \frac{bd}{a} \;\; \frac{bdn}{a} \;\; \frac{bdn^2}{a} \;\; \frac{bdn^3}{a} \;\; \frac{bdn^4}{a} \end{cases}$

Then $a : bn^2 :: dn : \frac{bdn^3}{a}$, &c.

whole ſhall interſect it at diviſions which denote numbers in geometrical progreſſion; then, from the condition of the arrangement, and the property of this logarithmic line, it follows, firſt, that every right line ſo drawn will, by its interſections, indicate a geometrical ſeries of numbers *; ſecondly, that ſuch ſeries as are ſo indicated by parallel right lines, will have the ſame common ratio †; and thirdly, that the ſeries thus indicated by two parallel right lines, ſuppoſed to move laterally without changing either their mutual diſtance, or paralleliſm to themſelves, will have each the ſame common ratio; and, in all pairs of ſeries indicated by ſuch two lines, the ratio between an antecedent on one parallel and the oppoſite term on the other, taken as a conſequent, will be conſtant ‡.

5. In the foregoing paragraphs the logarithmic line has been conſidered as unlimited. On ſuch a line, therefore, any antecedent and conſequent being given, it would be poſſible to find both on the arrangement, and to draw two parallel lines, one over each number: and if the lines be then ſuppoſed to move without changing either their diſtance or abſolute direction, ſo that the line, which before marked an antecedent, may in the ſecond ſtation mark

* Let AB, CD, EF (Plate XVI. Fig. 1.) be portions of the logarithmic line arranged according to the condition; let GH be a right line drawn acroſs, ſo as to paſs through points of diviſion e, c, a, denoting numbers in geometrical progreſſion; then will any other line IK, drawn acroſs the arrangement, alſo paſs through points f, d, b, denoting numbers in geometrical progreſſion.

Demonstration. From one of the extreme points of interſection f, in the laſt named line IK, draw the right line fg parallel to GH, and interſecting the arrangement in the points i, h; and the ratios of the numbers $e:f$, $c:i$, and $a:b$, will be equal, becauſe the intervals on the logarithmic line, or differences of the logarithms of theſe numbers, are equal:

$$\text{Or } \frac{e}{c} = \frac{f}{i} \text{ and } \frac{c}{a} = \frac{i}{h}.$$

$$\text{But } \frac{e}{c} = \frac{c}{a} \text{ by the condition.}$$

Therefore $\frac{f}{i} = \frac{i}{h}$; or the nnmbers f, i, h, are in the ſame continued ratio as the numbers e, c, a.

Again, the point f, the line id, and the line hb, are in arithmetical progreſſion, and denote the differences of the logarithms of the numbers f and f, i and d, h and b.

The quotients of the numbers themſelves are therefore in geometrical progreſſion, that is,

$$\frac{f}{f} : \frac{d}{i} : \frac{b}{h}, \text{ or } \frac{i}{d} = \frac{dh}{bi}.$$

$$\text{Or } \frac{i}{d} = \frac{di}{bf}, \text{ by ſubſtituting } \frac{i}{f} \text{ for its equal } \frac{h}{i}.$$

$$\text{Whence } \frac{f}{d} = \frac{d}{b} \text{ or } f:d:b. \qquad \text{Q. E. D.}$$

† In the ſame manner, as it was proved that the line fg parallel to GH paſſes through points of diviſion denoting numbers in the ſame continued ratio as thoſe indicated by the line GH, it may alſo be ſhewn, that the line LM, parallel to any other line IK, will paſs through a ſeries of numeral points having the ſame continued ratio as the ſeries indicated by that line IK to which it is parallel.

‡ Becauſe the lines preſerve their paralleliſm to their former ſituation, they will indicate geometrical ſeries having the ſame common ratio as before; and, becauſe their diſtance meaſured on the logarithmic line remains unchanged, the differences of the logarithms of oppoſite numbers, and conſequently their ratio, will be conſtant.

mark a new antecedent, the other (by 2. and 3.) will mark a number at the ſame relative poſition and diſtance, which will be the conſequent to this laſt antecedent, after the ſame ratio.

6. Suppoſe a logarithmic line to contain no more than a ſingle range of numbers from 1 to 10, it will not be neceſſary, for the purpoſes of computation, to repeat it; for, if a ſlider or beam have two fixed points at the diſtance of the interval between 1 and 10, and a moveable point be made to range between theſe (always to indicate the antecedent), in this caſe, if the conſequent fixed point fall without the rule, the other fixed point will ſhew the diviſion it would have fallen on if the rule had been prolonged. This may be eaſily applied to the arrangement deſcribed, N° 4.

7. If the arrangement conſiſt only of the logarithms from 1 to 10, and the parallel croſs lines interſect that geometrical ſeries whoſe ſucceſſive ratios altogether, with that of the laſt to the firſt, make by compoſition the ratio $\frac{1}{10}$, the contrivance N°. 6. may be applied to ſhew ſuch conſequents as fall, laterally, without the rule.

8. It will be convenient that the arrangement of the lines ſhould be diſpoſed ſo as to occupy a rectangular parallelogram; or, in other words, that the croſs line, cutting the ſeries laſt mentioned, may be at right angles to the length of the rule.

The conſtruction of an inſtrument on the principles here explained will admit of various diſpoſitions of the graduated lines and apparatus for meaſuring intervals upon them. In the Tranſactions I gave a figure of a rule conſiſting of ten parallel lines, equivalent to a double line of numbers, upwards of twenty feet in length, with a beam compaſs for meaſuring intervals.

Fig. 2. Plate XVI. repreſents a Gunter's ſcale, equivalent to that of $29\frac{1}{2}$ inches in length, publiſhed by the late Mr. Robertſon. It is, however, but one eighth part of the length, and contains only one-fourth of the quantity of diviſion. In the ſlider GH is a moveable piece AB, acroſs which a fine line is drawn; and there are alſo lines CD, EF, drawn acroſs the ſlider, at a diſtance from each other equal to the length of the rule. The ſketch No. 1. repreſents one face or ſide of the inſtrument, and No. 2. repreſents the oppoſite face. Each contains one-fourth part of the line of numbers. When it is uſed, the ſlider muſt be ſet ſo that the line on the piece AB may be placed at the antecedent, and one of the end marks CD, or EF, may be oppoſite the conſequent. After this adjuſtment of the ſlider, the whole may be moved at pleaſure, till the piece AB is ſet at any other required antecedent; and then the ſame line CD or EF, as before, will indicate the conſequent at the ſame diſtance or poſition as before. But if the conſequent mark of the ſlider ſhould fall without the rule, the other line will indicate the required conſequent upon the rule, though at the diſtance of one line on the rule farther off in poſition than the other conſequent mark would elſe have ſhewn it. The operations are obvious and familiar upon the rule itſelf.

Another inſtrument was deſcribed in the Tranſactions, which was equivalent to the ſame rule, but of a circular figure, one inch and a-half diameter. The graduations were made upon three concentric circles, and a ſector was applied inſtead of the ſlider. On account of the larger figure I have here given, it becomes unneceſſary to deſcribe this.

I approve

I approve of this conftruction as fuperior to every other which has yet occurred to me, not only in point of convenience, but likewife in the probability of being better executed, becaufe fmall arcs may be graduated with very great accuracy, by divifions transferred from a larger original.

The circular inftrument is a combination of the Gunter's line and the fector, with the improvements here pointed out. The property of the fector may be ufeful in magnifying the differences of the logarithms in the upper part of the line of fines, the middle of the tangents, or the beginning of the verfed fines. It is even poffible, as mathematicians will eafily conceive, to draw fpirals, on which graduations of parts, every where equal to each other, will fhew the ratios of thofe lines by means of moveable radii fimilar to thofe on fuch an inftrument.

After the publication of the account in the Tranfactions of the Royal Society, the mention of circular and fpiral inftruments brought to the recollection of the late Mr. George Adams, of Fleet-ftreet, (who had feen the ftraight inftrument fome years before it was communicated to the Society,) that he had a fpiral engraved on a brafs plate by his father. He made me a prefent of the plate, and fhewed me, by a manufcript, that it was conftructed in the year 1748. The fpiral has ten turns, and its external diameter is twelve inches. It contains the numbers, fines and tangents, the latter being twice repeated, and may with eafe be ufed to compute to four places and an eftimate figure; which is to the full as much as could be done with a common Gunter's fcale of fixty feet in length.

Fig. 3. Plate XVI. reprefents the line of numbers drawn according to this fyftem. The fector ACB is ufed to meafure the ratios. One thread muft be fet to the antecedent, and the other to the confequent. If the antecedent thread be then removed without altering the angle to any other antecedent, the other thread will mark a confequent at the diftance of the fame number of turns of the fpiral in the fame direction. In cafe the number of turns fhould proceed without the fyftem, it will be neceffary to return and reckon onward from the oppofite extremity of the fpiral to complete the number.

IX.

On the Mechanical Projects for affording a Perpetual Motion.

IN confequence of the notice * taken of Mr. Varley's attempt to produce a perpetual motion, I have been requefted by feveral correfpondents to ftate how far the mechanical fcheme for which Dr. Conrad Shiviers took out a patent in the year 1790, for the fame object, may be worthy of attention. I have, on that occafion, mentioned the difficulties which have prevented any clear general demonftration of the abfurdity of this purfuit from being produced, though it has not been difficult to fhew the fallacy of the individual plans. It does not indeed feem eafy to enunciate the fcheme itfelf. What in univerfal terms is

* Philof. Journ. I. 334.

the

the thing propofed to be done? Is it to caufe a body, or fyftem of bodies, to act in fuch a manner that the re-action fhall be greater than the action itfelf, and by that means generate force by the accumulation of the furplus? Or, can the motion communicated be greater than that loft by the agent? Since thefe pofitions are evidently contrary to the phyfical axioms called the Laws of Nature, and frictions and refiftances would fpeedily deftroy all motions of fimple uniformity, it may be prefumed that s'Gravefande, who thought that all the demonftrations of the abfurdity of fchemes for perpetual motion contained paralogifm, would have ftated the propofition under different terms. But without entering into this apparently unprofitable difquifition, it may be ufeful, as well as entertaining, to make a few obfervations on the mechanical contrivances which depend on a miftaken deduction from the general theorem refpecting the balance, among which that of Dr. Shiviers muft be claffed.

There is no doubt but numerous arrangements have been made, and ftill are laboured at by various individuals, to produce a machine which fhall poffefs the power of moving itfelf perpetually, notwithftanding the inevitable lofs of force by friction and refiftance of the air. Little, however, of thefe abortive exertions has been entered upon record. The plans of Bifhop Wilkins, the Marquis of Worcefter, and M. Orfyreus, are all which at this time occur to my recollection.

There is no doubt but the celebrated Wilkins was a man of learning and ability. His Effay towards a real character and a philofophical language is fufficient to render his name immortal. Twenty years before the appearance of that work he publifhed his Mathematical Magic, namely in the year 1648, containing 295 pages fmall octavo, which, from the number of copies ftill in being, I fuppofe to have been a very popular treatife. It is in this work that I find, among other contrivances for the fame purpofe, a wheel carrying fixteen loaded arms, fimilar to that delineated in fig. 4, plate xv. in which, however, for the fake of fimplicity, I have drawn but fix. Each lever A B C D E F is moveable through an angle of 45°, by a joint near the circumference of the wheel, and the inner end or tail of each is confined by two ftuds or pins, fo that it muft either lie in the direction of a radius, or elfe in the required pofition of obliquity. If the wheel be now fuppofed to move in the direction EF, it is evident that the levers A B C D, by hanging in the oblique pofition againft the antecedent pins, will defcribe a lefs circle in their afcent, than when on the other fide they come to defcend in the pofitions EF. Hence it was expected that the defcending weights, having the advantage of a longer lever, would always predominate. Dr. Wilkins, by referring the weights to an horizontal diameter, has fhewn that in his machine they will not. A popular notion of this refult may alfo be gathered from the figure, where there are three weights on the afcending, and only two on the defcending fide; the obliquity of pofition giving an advantage in point of number, equal to what the other fide may poffefs in intenfity. Or if this contrivance were to be ftrictly examined, on the fuppofition that the levers and weights were indefinitely numerous, the queftion would be determined by fhewing that the circular arcs AK, HI, are in equilibrio with the arcs AG, GL.

The fimpleft method of examining any fcheme of this kind with weights, confifts in enquiring whether the perpendicular afcents and defcents would be performed with equal maffes in equal times. If fo, there will be no preponderance, and confequently no motion. This is clearly the cafe with the contrivance before us.

5 The

The Marquis of Worcefter, who will ever be remembered as the inventor of the fteam engine, has defcribed a perpetual motion in the 56th number of his Century of Inventions, publifhed in the year 1655, and fince reprinted in 1767 by the Foulis's at Glafgow. His words were as follow:

"To provide and make, that all the weights of the defcending fide of a wheel fhall be perpetually further from the centre than thofe of the mounting fide, and yet equal in number, and heft to the one fide as the other. A moft incredible thing if not feen, but tried before the late King (of bleffed memory) in the Tower by my directions, two extraordinary ambaffadors accompanying his Majefty, and the Duke of Richmond and Duke Hamilton, with moft of the Court attending him. The wheel was 14 feet over, and 40 weights of 50 pounds a piece. Sir William Balfour, then Lieutenant of the Tower, can juftify it with feveral others. They all faw, that no fooner thefe great weights paffed the diameter line of the lower fide, but they hung a foot further from the centre; nor no fooner paffed the diameter line of the upper fide, but they hung a foot nearer. Be pleafed to judge the confequence."

Defaguliers, in his courfe of Experimental Philofophy, vol. I. page 185, has quoted this paffage, and given a fketch of a pretended felf-moving wheel, fimilar to that of fig 5, plate xv. as refembling the contrivance mentioned by the Marquis of Worcefter. The defcription of this laft engineer agrees, however, fomewhat better with the contrivance fig. 4. It muft of courfe be a miftake in terms, when he fays the weight receded from the centre at the lower diameter, and approached towards it at the upper: the contrary being in fact neceffary to afford any hope of fuccefs; and accordingly in the quotation it is fo ftated. I am therefore difpofed to think that fig. 5 reprefents the wheel of Orfyreus at Heffe Caffel, much talked of about the year 1720, and which probably was made to revolve, during the time of exhibition, by fome concealed apparatus. It confifts of a number of cells or partitions diftinguifhed by the letters of the alphabet, which are made between the interior and exterior furfaces of two concentric cylinders. The partitions being placed obliquely with refpect to the radius, and a cylindrical or fpherical weight placed on each, it is feen from the figure, that thefe weights will lie againft the inner furface of the large cylinder, whenever the outer end of the bottom partition of any cell is loweft; and on the contrary, when that extremity is higheft, the weight will reft on the furface of the interior cylinder. Let the wheel be made to revolve in the direction A B C; the weights in C D E F G H I being clofe to the external circle, and the weights K L M A B clofe to the inner, for the reafons laft mentioned. As the cell B defcends, its weight will likewife run out, at the fame time that the weight in the cell I will run in, in confequence of its partition being elevated. By the continuation of this procefs, fince all the weights on the defcending fide pafs down at a greater diftance from the centre, while thofe on the afcending fide rife for a confiderable part of their afcent at a lefs diftance from the fame point, it is concluded that the wheel will continue to maintain its motion. On this, however, it is to be remarked, that the perpendicular afcent and defcent are alike, both in meafure, and in time of performance; and that the familiar examination, even to thofe who know little of fuch fubjects, is fufficient to fhow that the preponderance is not quite fo palpable as at firft it appears. For the weights G and F, H and E, I and D, are evidently in equilibrio, becaufe at the fame horizontal diftance from the centre; and if the favourable fuppofition that the

weight

weight B hath already run out be admitted, it will then remain a queftion whether thefe two exterior weights, B and C, can preponderate over the four inner weights K L M A. The more accurate examination of this particular contrivance will lead to the following theorem. **In two concentric circles, if tangents be drawn at the extreme points of a diameter** of the fmaller, and continued till they interfect the larger, the common centre of gravity of the arc of the greater circle included between the tangents, and of the half periphery of the fmaller circle on the oppofite fide of the diameter, will be the common centre of the circles. If, therefore, the balls were indefinitely numerous and fmall, the fuppofed effective parts of the wheel, fig 5, would be in equilibrio, as well as the parts beneath the **horizontal tangent of the inner circle.**

Fig. 6 reprefents the contrivance of Dr. Shiviers, which, in a periodical publication, in other particulars refpectable, has been faid to continue in motion for weeks, and even months together. There is not the fmalleft probability that it fhould continue in motion for half a minute, or nearly as long as a fimple wheel would retain part of its firft impulfe. The external circle denotes a wheel carrying a number of buckets, A B I L, &c. C reprefents a toothed wheel, on the fame axis, which drives a pinion D; and this laft drives another pinion E upon the axis of a lanthern, or wheel intended to work a chain pump with the fame number of buckets as in the large wheel A B I. The lanthorn G is made of fuch a fize as to raife the buckets a b i l with a due velocity. K reprefents a gutter, through which a metallic ball, contained in the bucket m may run and lodge itfelf in the bucket A of the wheel. Each of the buckets of the wheel B I L M, which are below the gutter, is fupplied with a metallic ball, and fo likewife are the afcending buckets a b i l m of the chain-pump. As the pump fupplies the wheel, it is itfelf again fupplied at M, where the balls fall into its afcending buckets. Now it is prefumed that the balls in the wheel, I fuppofe on account of their diftance from the centre of motion, will defcend with more than fufficient force to raife thofe on the chain, and confequently that the motion will be perpetual.

The deception in this contrivance has much lefs feduction than in the two foregoing, becaufe it is more eafily referred to the fimple lever. This, like the others, exhibits no profpect of fuccefs, when tried by the fimple confideration of the equality of the afcent and defcent in the whole time of the rotation of a fingle ball. It may alfo be fhewn from the principles of wheel work, which are familiar to artifans, that whatever is gained by the excefs of the diameter of the great wheel beyond that of the wheel C, is again loft by the excefs of the lanthorn A beyond the pinion E.

The fundamental propofition of the fimple lever or balance, that equal bodies at an equal diftance from the fulcrum will equiponderate, but that at unequal diftances the moft remote will defcend, has in thefe and numberlefs other inftances led mechanical workmen and fpeculators to purfue this fruitlefs enquiry with labour and expence often ill-afforded, and with a degree of anxiety and infatuation which can hardly be conceived by thofe who have never fuffered the pain of hope long deferred. For this reafon chiefly, it has appeared defirable and ufeful to treat the fubject in a familiar way, without defcending to thofe expreffions of contempt, which ignorance, harmlefs to all but itfelf, is furely not entitled to. If fuch reafoners were well convinced that the power of a machine is to be eftimated by the excefs of motion referred to the perpendicular, without any regard to the apparent centre

centre of the machine, and that in machines very little compounded it is poffible to produce effects directly contrary to the rule which is true of the fimple lever, they would probably renounce many flattering projects, grounded only on the fuppofition of its univerfality. Defaguliers contrived an apparatus in which two equal weights may be placed at any diftance whatever from the centre of motion, and ftill continue in equilibrio. Fig. 3 reprefents this inftrument, AD denotes a balance with equal arms, and EF another of the fame dimenfions. Thefe move on the centres B and C, and are connected by the inflexible rods AE and DF; the motion being left free by means of joints at the corners. Acrofs the rods AD, EF, are fixed two bars IK, LM. Now it is unneceffary to fhew that the weight G will defcribe exactly the fame line or circular arc, when the levers are moved into the pofition a d f e, or any other pofition, as it would have defcribed in cafe it had been fufpended at A, or K, or E; and that it is of no confequence in this refpect at what part of the line AE, or IK, it be fixed. The fame obfervations are true of the weight H on the other fide. And accordingly it is found, that thefe equal weights may be fufpended any where on the lines IK and LM without altering their equilibrium.

By this contrivance it is moft evidently proved, to thofe who are totally unacquainted with the theory, that weights do not preponderate in compound engines, on account of their diftance from the centre. Several other contrivances may be made to the fame effect. The following combination of wheel-work prefented itfelf to me as one which would moft probably be miftaken for a perpetual motion. Fig. 2. Plate XV. The five circles reprefent the fame number of wheels, of equal diameter and number of teeth, acting together. The middle wheel A is fixed between two upright pillars, fo that it cannot revolve. The other four wheels are pinned in a frame HI, in which they can revolve, and through which the axis of A likewife paffes. From the extremity of the axis of D, and alfo of d, proceed the horizontal levers HK and IL, which are equal, and point in the fame direction parallel to the plane of the wheels. At the extremity of thefe arms hang the equal weights P and p. Let it now be imagined that the end I of the frame is depreffed; the wheel B will turn round by the re-action of the fixed wheel A in the fame direction as HI, and it will make one revolution, in the fame time, relative to the frame, or two with regard to abfolute fpace by reafon of its being carried round. The action of B upon D will produce a rotation relative to the frame in the oppofite direction during the fame time. Inftead therefore of two revolutions, like the wheel B, this wheel D, with regard to abfolute fpace, will not revolve at all, and in every pofition of the apparatus the arm IL will continue horizontal, and point the fame way. For fimilar reafons the arm HK will retain its pofition. Confequently, it is feen that the defcending weight will move at a great horizontal diftance from the centre N, while the afcending weight rifes very near that centre. But there will not on this account be a perpetual motion: for the actions of the levers HK and IL upon the frame HI, by means of the toothed wheels, will in the detail be found precifely alike, and in the general confideration of the motions of P and p, the oppofite motions in the circle E F G will be accurately the fame.

It has always been confidered as effential to a perpetual motion, that it fhould be derived from fome energy which is not fuppofed to vary in its intenfity. Such are the inertia, the gravity or magnetifm of bodies. For an occafional or periodical variation of intenfity in any force is evidently productive of motion, which requires only to be accumulated or

 applied,

applied, and the apparatus for applying it cannot be confidered as a machine for perpetual motion. Neither in ftrictnefs can any machine, whofe motion is derived from the motion or rotation of the earth, and the confequent change of feafons and rotation of events, be fo confidered, becaufe it does not generate, but only communicates. The perpetual flow of rivers, the viciffitudes of the tides, the conftant, periodical and variable winds, the expanfions and contractions of air, mercury, or other fluids, by daily or other changes of temperature, the differences of expanfion in metals by the fame change, the rife and fall of the mercury in the barometer, the hygrometric changes in the remains of organized beings, and every other mutation which continually happens around us, may be applied to give motion to mills, clocks, and other engines, which may be contrived to endure as long as the apparatus retains its figure.

X.

Ufeful Notices refpecting various Objects.—Silver alloyed with Crude Platina.—Tempering of Steel.—Rifled Shot.

1. *Silver alloyed with Crude Platina.*

BERGMAN, in his Treatife on the Blow-Pipe, in one of the earlier editions, directed that the fpoon for blow-pipe experiments fhould be made of filver alloyed with one tenth of platina, of which the purification was at that time little known. Dr. Lewis, in his Philofophical Commerce of Arts, mentions the fufion of filver with crude platina. The metals united but imperfectly, with a remarkable projection of particles of the metal, as if by a kind of ebullition over the infide of the crucible. Several years ago, being defirous of making fuch a fpoon, I felected ten grains of crude platina, in particles poffeffing very little magnetifm, and fufed the fame with one hundred grains of pure filver in a blaft furnace, ufing a large proportion of nitre, with the intention of fcorifying any of the bafer metals. The effect which Lewis mentions took place; and the compound, when poured out, had a fcabrous or unfound appearance. I thought the grains of platina might have been merely furrounded by the adherent filver; but this did not feem to be the cafe, for it bore laminating between two fteel rollers very well. After this laft procefs, I fubjected it to fufion again with a ftronger heat, and again laminated it into a thin plate. This operation of fufing and rolling was repeatedly performed, but ftill the metal appeared rough in certain parts of the furface. As a laft effort, I therefore expofed it to the moft violent heat I could urge, and determined to leave it to cool in the crucible beneath the nitre which flowed above it. The crucible in the ignited ftate was taken, from fome motive I cannot now recollect, to a window, and fet down upon a tile. As I ftood attentively obferving the appearance of the metallic globule through the tranfparent and tranquil bath of nitre, the ignition gradually went off, fo as to be fcarcely vifible in that clear light. But on a fudden it recovered itfelf in an inftant, and the nitre boiled up fo as to fill the vacant fpace of the crucible. The button of metal, when cooled, was fcabrous; but I neverthelefs formed it into a fpoon, by rolling, hammering, and afterwards polifhing it. It was then expofed to a low red heat, in a common fire, and became bliftered all over.

The above facts poffefs the utility which attends unfuccefsful experiments, namely, that the narration may fave others from a repetition of the labour. But, philofophically fpeaking, there are no new experiments which would be unfuccefsful if we thoroughly underftood them. I think the decay and recovery of ignition in this is a curious inftance of what feems to be a general law of the congelation of fluids. It is probable that all fluids, as well as water, are capable of being cooled below their freezing point, and afterwards become hotter by the efcape of latent heat when they congeal. Thus water cooled below 32 degrees is fuddenly raifed to that temperature the inftant ice is formed, becaufe the ice gives out the difference of the heat which was latent in the fluid ftate. And fo it appears to have happened with the fluid metallic compound in thefe experiments. As its temperature was diminifhed, it became lefs luminous or ignited; but, at the inftant of congelation, that portion of heat which had been employed in maintaining the fluid ftate, was extricated, and became employed in raifing the temperature. The effect of this increafe was feen in the greater emiffion of light, and the boiling of the nitre.

2. *Tempering of Steel.*

INSTRUMENTS of fteel are required to be hard, in order that they may penetrate and divide the fubftances intended to be cut; and tenacious, that they may not break during the operation. The hardeft fteel is the moft brittle; for which reafon, though hardnefs would in every cafe be a defirable quality, yet, for the fake of tenacity, it is in many inftances neceffary to diminifh it. We fee, therefore, that there muft be a precife mean between too foft and too brittle, which will be beft fuited for the refpective purpofes to be accomplifhed. A fpring muft be tenacious, and need not be very hard. A knife for cutting leather, and foft fubftances, muft be fomewhat harder than a fpring. Pen-knives and razors muft be ftill harder; and files and tools for working metal muft be hardeft of all, though even in thefe care muft be taken not to deftroy their tenacity by making them too hard. Steel is hardened by ignition, and fubfequent plunging in water. The chief art of this procefs confifts in covering the fteel with fome mixture, which fhall prevent its being degraded in the fire to the ftate of iron. The file-makers ufe the grounds of beer mixed with common falt. Others ufe the cementing mixture. No greater heat is to be ufed with any fteel, than by experience is found fufficient to produce hardnefs at leaft equal to that of a file. More heat would render the grain coarfe and open. Urine is thought to be better for quenching the fteel than water, probably becaufe it may be a better conductor of heat, and perhaps on account of its phofphoric ingredient, which is now with juftice fuppofed * to be an effential part of fteel. When fteel is not intended to poffefs the utmoft hardnefs, it is afterwards foftened by the application of a lower degree of heat. This operation is called tempering. The greateft difficulty confifts in applying the proper degree of heat uniformly over the whole mafs. The common method is to judge by the colour affumed by the clean furface of the fteel when thus heated. The heat may be applied by the fire, or a pan of charcoal, or the flame of a candle or lamp, or by laying the

* Philofophical Journal.

piece-

piece upon ſand to be gradually heated, or upon melted lead. The ſaw-makers, and ſome makers of ſprings, heat the article, rub it with greaſe, and then heat it ſtill farther till the fumes take fire; this is called blazing, and affords a temper nearly the ſame as when the ſteel by heat has acquired a deep blue colour. When the temper is given from the colour, the firſt tinge which appears is a faint ſtraw colour, which is ſuitable to pen-knives and hard cutting-tools. The next colour, which is purple, is rather too ſoft for a knife, and too brittle for a ſpring. After this follows the blue, of which there are ſeveral ſhades. The deepeſt is very ſoft, and is ſucceeded by a whitiſh yellow, which indicates too great a degree of ſoftneſs for any cutting-tool.

Mr. Hartley, in the year 1789, took out a patent for a method of tempering ſteel. His ſpecification, which is ſo general as perhaps to include no method at all in the way of monopoly, indicates that the heat is to be meaſured by a pyrometer or thermometer applied near the article. The actual practice of this method appears to conſiſt in uſing oil and a mercurial thermometer. In this way many dozens of razors or tools may be tempered at once with the utmoſt facility. The different degrees of heat for various kinds of ſteel, and their ſeveral uſes, may ſpeedily be determined by experiment. For want of a thermometer graduated to the higher degrees, I have not yet made any experiments. The only fact I have at preſent to communicate is, that Mr. Stodart, who uſes this method, ſtates that the requiſite temper on ſteel for a pen-knife is 450 degrees of Fahrenheit's ſcale *. I find, on trial, that a good pen-knife is as hard as any tool which can admit of tempering. Hard gravers, for turners' uſe, muſt not be ſoftened at all.

3. *Rifled Shot.*

IN the latter end of the year 1789, I was, by various conſiderations, induced to think, that the effect which is produced by rifling muſquetry might be produced in artillery by giving a ſuitable figure to the ſhot. It is almoſt needleſs to explain this effect. When a bullet is driven along the bore of a piece, it muſt be acted upon by the internal ſurface ſo as to cauſe a rotation, the axis of which motion will lie acroſs the line of direction. In conſequence of this, the re-action of the air will be ſtronger on one ſide of the bullet than on the other, and it will deviate from the intended courſe according to no certain rule. The method of rifling conſiſts in cutting one or more ſpiral grooves in the hollow ſurface of the muſquet, into which the ball is either forcibly rammed down, or elſe conveyed to its place by an aperture at the breach, or near the chamber. The lead is thus made to fit the internal ſcrew, and uſually takes about half a turn during its courſe through the barrel. The axis of this rotation being parallel to the line of direction, it muſt follow that the reſiſtance of the air will be equal on all ſides of the bullet, and it will fly with more certainty to the object of aim. It ſeemed to me, that if a cylindrical ſhot, with hemiſpherical ends, were thrown out of a common barrel, it might be poſſible, by means of certain ſpirals cut on the end ſurface, to cauſe the blaſt of the powder and the reſiſtance of the air to concur in producing the ſame rotation.

For this purpoſe I took a wooden pattern, and cut the ſpherical ſurface into twelve ſpiral planes, by dividing the equator into the like number of equal parts, and drawing ſpirals from

* Philoſophical Tranſactions 1795, p. 326.

the

the points of divifion obliquely towards the poles. The wood between every pair of contiguous fpirals was then taken away, by cutting from the one line parallel to the axis, and from the other perpendicular to a plane paffing through it. By this procefs, when the axis was fet upright, there appeared, as it were, twelve roads floping upwards from the equator towards the pole, bounded on the fide next the wood by upright walls; and the fhot, when fufpended on an axis or centre point, could be blown round very fwiftly by the breath directed towards the pole.

Shot of this kind were made and tried at a foundery in North Wales. By an experiment with a brafs gun newly bored, it was afcertained that the fhot did really revolve in its courfe along the bore; but the trials with fhot of different weight and dimenfions did not promife more accuracy of effect than was obtained by common fpherical fhot ufed at the fame time. Particular notice was taken of the manner in which the fhot ftruck the butt: the greateft number of times, it ftruck with the anterior end; fometimes the ftroke was made with the broad fide, and, in a few inftances, the end which came laft out of the gun arrived firft at the mark. Hence it appears, that the very flight angular deviation at the mouth of the piece is more than fufficient to counteract any effect which might elfe have been derived from the fubfequent action of the air upon a projectile duly figured.

It feems, neverthelefs, that this principle might be applied to advantage in bar fhot. If the ends of this projectile were chamfered or floped with refpect to the axis, it would pafs through the air with a revolution of its extremities, inftead of one end following the path of the other, as may fometimes be fuppofed to happen.

With regard to the execrable practice of war, I think it a decided queftion, that increafe of power is, on the whole, in favour of rectitude and virtue; and that wars are likely to be fewer, lefs durable, and lefs pernicious, the more fcientifically they are conducted.

MATHEMATICAL CORRESPONDENCE.

QUESTION IX. *Anfwered by J. F—:—:—:—R.*

IN order to a folution of this problem, we have only to find the point in the plane of the horizon, which is perpendicularly under the elevated object; the diftances of which point from the three ftations are as the cotangents of the angles of elevation taken at each refpectively. This will be effected by the following

CONSTRUCTION.

LET A, B, and C be the three ftations (Plate XV. Fig. 1.) and a, b, and c the cotangents of the refpective angles of elevation taken at each. Produce AB and CB towards E and F, and make BF = BA, and BE = BC. Take FG to FB as c to b, and EG to EB as a to b, and about the centres F and E with the refpective radii FG and EG defcribe arcs interfecting in G. Draw FG, BG, EG, and in GB produced take BD to AB as BE to BG. I fay the point D is the required point in the horizontal plane.

For,

For, draw AD and CD.—Then the triangle GBE being fimilar to ABD, and GBF to CBD, we have AB (= FB) : $\frac{c}{b}$ AB (= FG) :: b : c :: BD : CD and BC (= BE) : $\frac{a}{b}$ BC (= EG) :: b : a :: BD : AD. Therefore AD, BD and CD being as a, b, and c refpectively, the point D is rightly found. Q. E. D.

Hence alfo, if with AD (: AB :: EG : BG) and CD (: CB :: FG : BG) as radii, arcs be defcribed about A and C, they will interfect each other, and each of them will interfect GB produced in the required point.

CALCULATION.

FROM the values of FB, BE, FE, FG, and EG, find BG trigonometrically, and from thence AD, BD, or CD.

THIS queftion was anfwered in nearly the fame manner by Analyticus, who obferves, that if the three ftations are at unequal diftances from the object, its apparent altitude will vary at each of them, according to the effect of refraction, and of courfe occafion a threefold error in the refult; which may be avoided by taking the ftations fo that the angles of elevation fhall be all equal, in which cafe a fingle refraction only will be concerned, which indeed is fufficient to render this, or any other fimilar method of determining the heights of terreftrial objects, very inaccurate. If, befides the angles of elevation, thofe formed by the bafe and hypothenufes drawn from each extremity of it, to the top of the object, be taken, there is no occafion for more than two ftations; which being chofen in fets, in the way laft mentioned, will enable us to afcertain the difference of refraction at different diftances.

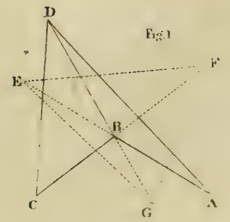


Fig 1

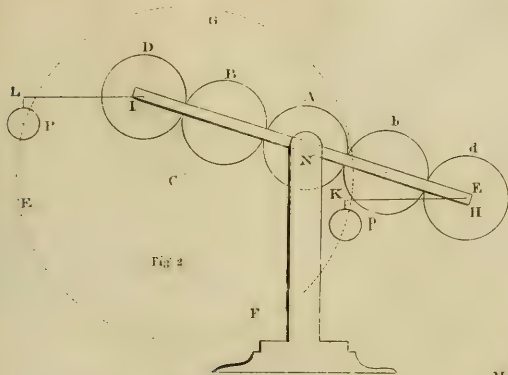


Fig 2

By Bishop Wilkins

Compound Ballance

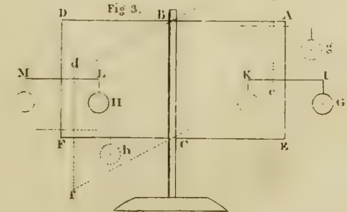


Fig 3.

Schemes for

Perpetual Motion

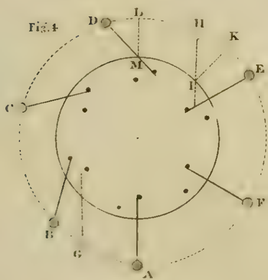


Fig. 4

By the Marquis of Worcester

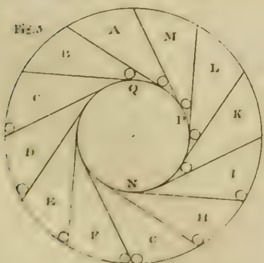


Fig. 5

By Dr. Shiviers

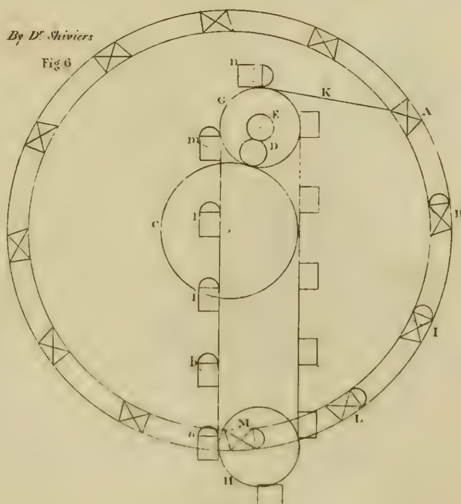
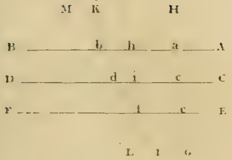


Fig 6

Scale equivalent to Gunter's Rule of 29 ½ Inches long.

Fig. 1.



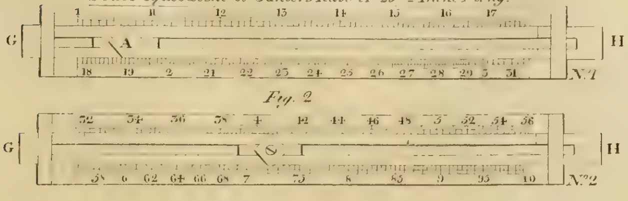


Fig. 3. Spiral Instrument equivalent to Gunter's Rule of 40 feet long.

A

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AND

THE ARTS.

DECEMBER 1797.

ARTICLE I.

Concerning the Spontaneous Action of Concentrated Sulphuric Acid on Vegetable and Animal Subſtances; its Action upon Alcohol, and the Formation of Ether. By Citizens FOURCROY *and* VAUQUELIN.

IT has been long known that the concentrated ſulphuric acid deſtroys the texture of organic matters, by converting them into coal, in conſequence of an action which reſembles that of an elevated temperature; but this action has not yet been properly explained, ſo as to determine in what it may conſiſt, or the products it affords. The ancient theory of chemiſtry attributed this effect, in a looſe manner, to the affinity of phlogiſton for the ſulphuric acid, and was content to compare it to combuſtion. The authors of the pneumatic theory diſcerned ſome of the data of this phenomenon, and arrived ſomewhat nearer the truth: but they did not comprehend the whole. They have even committed ſome miſtakes, doubtleſs becauſe they did not ſtudy all the details of this remarkable action, nor obſerve with ſufficient care every thing that paſſes in the ſubjects of the experiments. My aſſociate Vauquelin has long been employed, in conjunction with myſelf, upon various methods of analyſing vegetables; and having ſubmitted a great number to the energy of the principal re-agents among which the acids occupy the firſt rank, a long ſtudy of the action and curious effects of the concentrated ſulphuric acid has led us to diſcover what had hitherto eſcaped the obſervation of chemiſts reſpecting its manner of action upon organic ſubſtances. Though the reſult of our enquiries in this reſpect belongs directly to the vegetable analyſis, which we ſhall offer to the ſcientific world as ſoon as our labour ſhall have acquired all the maturity we are deſirous it ſhall poſſeſs, we have nevertheleſs thought proper to detach this ſmall part, becauſe it appears worthy to intereſt the cultivators of chemiſtry, and at the ſame

 time

time proper in some respects to explain and ameliorate their daily processes, particularly those which relate to several pharmaceutical operations.

When a dry vegetable substance, such as wood, straw, or gum, is plunged in the concentrated sulphuric acid, this matter becomes coloured and soft, appears to dissolve in the acid, and forms with it a magma or kind of brown or black imperfectly fluid matter, well known to every chemist as the product of their experiments. If, after the mutual action of these bodies is exhausted, the mixture be diluted with a sufficient quantity of water, two phenomena equally interesting are observed. The one is the precipitation of a black powder, possessing all the characters of carbone nearly pure; the other is the slight degree of heat which the mixture produces with the water. It is far beneath the temperature which an equal quantity of the sulphuric acid, originally employed, would have exhibited with a like quantity of water.

The vegetable matter is certainly much altered, and has undergone a great decomposition in this experiment; since the carbone which entered into its composition is separated, nearly pure, from the other principles with which it was combined. It is well proved at present, that vegetable substances are composed of carbone, hydrogene, and oxygene united in various proportions, and remaining in a constant equilibrium of composition, so long as new attractions do not come to destroy it. In the present case the equilibrium is manifestly broken, because the carbone is separated from the two other principles. It was at first imagined, and in this manner it is that Citizen Berthollet has particularly determined the cause of this phenomenon in several of his works; namely, that the hydrogene of the vegetable substance united with the oxygene of the sulphuric acid, and, by thus forming water and sulphureous acid, the carbonic principle was separated and thrown down. We have ourselves adopted and admitted the same explanation, until repeated experiments shewed us that this opinion is erroneous. In fact, in the case here mentioned, and observed for a great number of times with the utmost attention and care, we have not perceived that an atom of sulphureous acid was produced, but that the sulphuric acid remains entire without any alteration or change in the proportion of its constituent parts. Since, therefore, this acid is not decomposed in the cold by vegetable matters, it is necessary to conclude, that the changes produced in those bodies are the consequence of a re-action between their proper principles; a re-action of which the sulphuric acid is only the occasional or subsidiary cause.

But in order to determine by what energy this acid produces such an alteration in vegetable matters, it is requisite to examine with accuracy what the alteration may consist in; and this examination, to which we have a great number of times directed our attention, has shewn the error adopted with regard to the pretended decomposition of the acid, and the true cause of the changes thus produced in organic bodies. When the carbonic precipitate of the experiment above described is separated, it is found that the sulphuric acid which floats above it, is singularly weakened, and contains the acid of vinegar, which may be separated by distillation. Here therefore we see the vegetable matter converted into acetous acid and carbone. If the quantity of this last, together with that of the vinegar, be compared with the quantity of vegetable matter employed, a considerable loss is perceived in the mass of these products, compared with that of the substance which afforded them. Nothing can have been lost in the experiment, because no elastic fluid is disengaged; and as the sulphuric acid has lost much of its density, and becomes much less heated with water,

water, ſince it is, in a word, extremely diluted, it is evident that the water it has acquired could not have been formed but at the expence of the principles of the vegetable, and that it contains the weight which is wanting to the carbone and acetous acid to repreſent the whole of the vegetable matter ſubmitted to experiment. For it is very certain that an action perfectly the ſame, and a reſult accurately identical, takes place in cloſed veſſels, as well as in ſuch as are open; and that the water which ſaturates the ſulphuric acid does not come from the atmoſphere. We are therefore forced to conclude, from this examination, that the vegetable ſubſtance treated with the concentrated cold ſulphuric acid, has ſuffered a decompoſition or analyſis by ſeparating a portion of its carbone nearly pure, and that another part of carbone has united with hydrogene and oxygene to form the acetous acid, at the ſame time that a ſecond portion of oxygene has united with a ſufficient quantity of hydrogene to form the water which dilutes the acid. All theſe changes in the vegetable ſubſtance are therefore made at the expence of its own proper principles. Nothing has come to paſs but a change of equilibrium between them, a ſeparation and unequal combination of their component parts, which produce the three new products obtained.

In what manner does the concentrated ſulphuric acid determine this change of equilibrium? How can it convert an homogeneous organic matter into acetous acid and coal? What is the force or forces whoſe energy or concurrence deſtroys the connection which united the principles of the vegetable ſubſtance, while it has no influence on the ſulphuric acid? A ſingle argument well applied, from the reſult of experiments, will lead us to the ſolution of this problem. Since a vegetable ſubſtance requires a new equilibrium of principles after the total action of the concentrated and cold ſulphuric acid, and remains in the triple ſtate of carbone, acetous acid, and water, at which period the ſulphuric acid has become much leſs denſe and concentrated than before;—we muſt conclude, that the affinity of this acid for water, as well as that of the oxygene for hydrogene and carbone, at the ſeveral proportions required to form vinegar, prevail over the attractions which held theſe three principles united in other proportions under the original form of homogeneous vegetable matter. But, in this new ſum of elective attractions, the only cauſe which determines the change that takes place is the affinity of the ſulphuric acid for water, of which the principles do not unite nor the compoſition take place, till the moment at which this affinity acts. When once this tendency of the acid for water is ſatisfied, the equation between the ſeveral principles is effected, an equilibrium is eſtabliſhed, and no further alteration follows.

It ſeems, however, at firſt ſight, that the imagination refuſes to admit, in the ſulphuric acid, an affinity for a body which does not exiſt completely formed, or to conceive the formation of the water as reſulting from the attraction which the acid exerts upon it, though it do not yet exiſt in the vegetable matter, but in its principles. But whatever repugnance a reſult of this kind ſeems at firſt to produce, it does not appear leſs certain, ſince it is evident, as matter of fact in this experiment, that the acid originally concentrated becomes feeble and aqueous; that the water did not exiſt ready formed in the vegetable matter; that it was not taken from the atmoſpheric air; and that, when once the acid has become diluted, every ſubſequent action ceaſes. Theſe four points are equally eſtabliſhed, and there can be no doubt of their certainty. Beſides which, a great number of facts are at preſent well known to chemiſts, which compel them to admit ſuch affinities as are productive of fubfequent events, and have been denoted for fome years by the name of difpofing affinities. Thus it is that the attraction of acids for the metallic oxides favours the decompofition of water by means of the metals, which do not in fact become oxided to unite to acids, but at the expence of the oxygene which they take from the hydrogene. Thus likewife it is, that the affinity of the cauftic earths and alkalis for the fulphuric acid determines the combuftion of the fulphur of fulphurets by means of water, and the feizure of oxygene from the latter, which would not take place without the exiftence of this difpofing affinity. When the long confideration of chemical phenomena, the habit of beholding them, and of appreciating their refults, has deftroyed this firft repugnance in the mind of the chemift, he foon conceives that it is not more difficult to admit and explain this difpofing affinity, than it is in natural philofophy to adopt, and admit as proved, the attraction at great diftances which takes place between the planetary bodies.

We may exprefs the refults of the chemical effect as it has here been at length explained by a fimple formula, which in reality is nothing more than an argument difpofed in a clofe arrangement, like all the algebraic formulæ; and this method, by prefenting chemical phenomena in a concife language, will poffefs the advantage, when more generally employed, of reprefenting, with clearnefs and precifion, every thing that happens in the moft complicated operations. Thus we may fay,

Aqueous fulphuric acid + liquid acetous acid + precipitated carbone = concentrated fulphuric acid + entire vegetable matter. In this manner it was that Lavoifier exhibited the whole myftery of the vinous fermentation in this fimple form of an equation:—Juice of grapes = carbonic acid + alcohol; becaufe, in fact, the refult of this interefting obfervation, formerly fo obfcure, but at prefent fo perfpicuous, is the converfion of muft, or liquid faccharine matter, into alcohol and carbonic acid.

If the effects be truly fuch as we have here explained, it muft be neceffary for their exiftence that the fulphuric acid fhould be concentrated, and the vegetable matters in the dry ftate: for, if they contained water ready formed, this, by its immediate application to the fulphuric acid, would faturate it, and prevent its re-action upon the principles of thefe fubftances. The fame thing would happen if the acid employed was already weakened by the previous addition of water. This, in fact, takes place in both cafes, as the experience of every chemift muft fhew. It muft, however, be obferved in this place, that in order that the action between vegetable matter and fulphuric acid combined with water fhould be nothing, the water ought to be enough to faturate the acid: for, if its tendency to combination with water be not entirely fatisfied, its decompofing action on vegetable fubftances muft be in proportion to its want of faturation. In fact, the decompofition here mentioned, namely the formation of water and vegetable acid with the precipitation of carbone, cannot be obtained but by increafing the quantity of acid. This alfo is confirmed by experience, which fhews that the quantities of fulphuric acid neceffary to produce the required decompofition, do not follow the fimple ratio of the quantities of water they contain, but are at leaft in proportion to the fquares of thefe quantities. So that if we fuppofe, for example, that the moft concentrated fulphuric acid contains a quantity of water equal to two, and that another acid contain a quantity equal to four, there will be required not only the double of this laft to produce the fame effect upon a vegetable fubftance, but it will be found neceffary to ufe at leaft fixteen times as much. And this agrees very well with reafoning, fince it is known

5 that

that the affinity of the ſulphuric acid for water diminiſhes in proportion to the ſquare of the quantity of water it contains.

Though we have reduced the general action of the concentrated ſulphuric acid upon vegetable matters to the formation of water, of acetous acid, and the ſeparation of carbone, we muſt here remark, that in ſome caſes the action becomes more complicated; that ſometimes, alſo, vegetable acids are formed; that the formation of a ſmall quantity of alcohol, even in ſubſtances not ſaccharine, ſuch as gum, unſized paper, &c. likewiſe occaſionally accompanies the foregoing phenomena; and, laſtly, that there is in many circumſtances a diſengagement of carbonated hydrogene gas more or leſs abundant. But theſe multiplied products do not take place ſo frequently as the compoſition of water, of vinegar, and the precipitation of carbone, the conſtancy of which is invariable during the action of concentrated ſulphuric acid upon vegetable matters. They are in ſome meaſure phenomena neceſſary to the former. They depend upon the different proportion of the principles which compoſe vegetable ſubſtances. They take place more eſpecially in thoſe which contain a greater quantity of hydrogene. The deſcription of theſe, and the conſiderations to which they may lead chemiſts, belong to the general and complete hiſtory of vegetable analyſis which we propoſe to publiſh when our work ſhall be finiſhed. Laſtly, theſe productions of alcohol, of various acids at the ſame time, and of carbonated hydrogenous gas, indicate a more profound alteration in the vegetable matters treated by the ſulphuric acid; a complication of effects, which, though it originates in the change of equilibrium occaſioned by the powerful attraction of this acid for water, cannot be well developed until after the explanation of the new proceſſes of analyſis, the methods and reſults of which we ſhall hereafter preſent to the chemical world.

Beſides this multiplication of effects and complicated combinations which are ſometimes obſerved in vegetable matters treated with the concentrated ſulphuric acid, another alteration ſometimes takes place in this laſt. When the ſubſtances placed in contact with it contain too ſmall a proportion of oxygene to form the water required for its ſaturation, their hydrogene then unites with a portion of the oxygene of the acid itſelf, and the ſulphureous acid is formed. This effect is more particularly remarked in oily ſubſtances. It takes place only till the quantity of water neceſſary for the ſaturation of the acid is afforded, and ceaſes as ſoon as this ſaturation is effected.

This action of the concentrated ſulphuric acid is ſtill more complicated on animal than on vegetable matters, becauſe the animal matters themſelves are more compounded. Though the affinity of the acid for water be the cauſe, the formation of this liquid is not only accompanied with that of a vegetable acid and the precipitation of carbone, but ammoniac is likewiſe formed. No eſcape of any volatile principle is perceived. The azote, which forms part of theſe ſubſtances, unites with a portion of hydrogene, and conſtitutes the volatile alkali, which combines with the ſulphuric acid, or with the acid which is at the ſame time compoſed, while another part of the hydrogene forms with the oxygene of the ſame ſubſtances that water which is requiſite to ſaturate the ſulphuric acid. But in theſe caſes, as in the experiments with vegetable ſubſtances, the quantity of carbone, which exceeds that required to compoſe the new acid, together with a portion of hydrogene and oxygene, will be ſet at liberty, and falls down in the form of magma, of flocks, or of black powder. In this manner it is that in the caſe of burns, frequently ſo dangerous, which are produced by the concentrated

ſulphuric

ſulphuric acid, there is formed, at the expence of the ſkin, water, vegetable acid, and ammoniac, while carbone is precipitated, and a complete deſtruction of the burned parts is the reſult of theſe ſimultaneous effects, as appears by the brown or black ſcar which afterwards falls off, and is ſeparated as a foreign body. Hence may be deduced how great the diſorganizing power of this concentrated acid muſt be on the coats of the ſtomach or the œſophagus, and the utility of preſenting to it, at the moment it is ſwallowed, a mucous liquid ſubſtance, on which its action may be directly and ſpeedily exerciſed.

From the whole of what is here ſtated concerning the reciprocal action of concentrated ſulphuric acid, and the ternary or quaternary compounds which belong to the vegetable and animal kingdoms, we may perceive that its tendency to deſtroy theſe complicated compoſitions, and convert them into others more ſimple, muſt open a new path in the vegetable analyſis. We may perceive that, ſince its effects are confined to thoſe here deſcribed, while the ſubſtances and the ſulphuric acid are permitted to act ſpontaneouſly and without foreign aſſiſtance, the energy of the acid will proceed much further in this decompoſition when it is increaſed by the addition of new quantities of this fluid, or the application of accumulated heat. But the new reſults thus produced would carry us from the ſubject here treated, and to which we at preſent mean to confine ourſelves.

In this accurate deſcription of the ſpontaneous action of ſulphuric acid upon vegetable and animal matters, our intention was to ſhew more particularly that this knowledge may have a direct influence upon pharmaceutic operations. Our aſſociate Vauquelin has ſhewn in another memoir, which forms the continuation and uſeful reſult of the preſent, that the principles here explained are alone capable of developing the theory of the formation of ether, hitherto ſo vague and uncertain; and at the ſame time to perfect the operations proper to afford theſe important products. This firſt work, which we offer jointly to the ſchool of pharmacy, will be a confirmation of the truth conſigned in the diſcourſe on the indiſſoluble union of chemiſtry and pharmacy, namely, that the former cannot make the ſlighteſt advance, without the latter receiving immediately a proportional degree of perfection.

Concerning the Action of the Sulphuric Acid upon Alcohol, and the Formation of Ether.

THE preparation of ether is a complicated pharmaceutic operation, the reſults of which are no leſs known than the theory is obſcure. This is proved by the different explanations given by the chemiſts who have treated of this operation before the eſtabliſhment of the pneumatic doctrine.

The greater number of modern philoſophers are in general of opinion, that the whole mechaniſm of the formation of ether conſiſts in the decompoſition of the ſulphuric acid, and the tranſition of part of its oxygene to the elements of the alcohol. According to this hypotheſis, water, carbonic acid, and ſulphureous acid are formed. This opinion contains the ſame error as was pointed out in the foregoing memoir. In order to explain the operation with perſpicuity and effect, we ſhall deſcribe its circumſtances from the beginning to the end. And while we thus treat on a ſubject familiar to every chemiſt, we hope to prove that this operation, ſo common, and ſo often repeated, does nevertheleſs offer, in the ſeries of its phenomena, certain facts which have not yet been properly deſcribed, and of which the accurate knowledge alone can afford a ſolid theory of etherification.

Experiment

Experiment I. Equal parts of concentrated fulphuric acid and rectified alcohol being mixed together, difengage a quantity of caloric capable of elevating the temperature of the mafs to 70 degrees (190 Fahrenheit). Bubbles of elaftic fluid are formed, the liquor becomes turbid, affumes an opal colour, and at the end of feveral days becomes of a deep red colour.

Experiment II. A combination of two parts of fulphuric acid and one part of alcohol elevates the temperature to 75 degrees (20. Fahrenheit), becomes immediately of a deep red colour, which changes to black a few days afterwards, and emits a fmell perceptibly ethereal.

Experiment III. When we carefully obferve what happens in the combination of equal parts of alcohol and concentrated fulphuric acid expofed to the action of caloric in a proper apparatus, the following phenomena are feen:

1. When the temperature is elevated to 78 degrees (208 Fahrenheit), the fluid boils, and emits a vapour which becomes condenfed by cold into a colourlefs, light and odorant liquor, which, from its properties, has received the name of ether. If the operation be properly conducted, no permanent gas is difengaged until about half the alcohol has paffed over in the form of ether. Until this period there paffes abfolutely nothing but ether and a fmall portion of water, without mixture of fulphureous or of carbonic acid.

2. If the receiver be changed as foon as the fulphureous acid manifefts itfelf, it is obferved that no more ether is formed, but the fweet oil of wine, water, and acetous acid, without the difengagement hitherto of a fingle bubble of carbonic acid gas. When the fulphuric acid conftitutes about four-fifths of the mafs which remains in the retort, an inflammable gas is difengaged which has the fmell of ether, and burns with a white oily flame. This is what the Dutch chemifts have called carbonated hydrogene gas, or olefiant gas, becaufe, when mixed with the oxygenated muriatic acid, it forms oil *. At this period the temperature of the fluid contained in the retort is elevated to 88 or 90 degrees (230 or 234 of Fahrenheit).

3. When the fweet oil of wine ceafes to flow, if the receiver be again changed, it is found that nothing more paffes but fulphureous acid, water, carbonic acid gas; and that the refidue in the retort is a black mafs, confifting for the moft part of fulphuric acid thickened by carbone.

The feries of phenomena here expofed will juftify the following general inductions:

1. A fmall quantity of ether is formed fpontaneoufly, and without the affiftance of heat, by the combination of two parts of concentrated fulphuric acid and one part of alcohol.

2. As foon as ether is formed, there is a prodnction of water at the fame time; and while the firft of thefe compofitions takes place, the fulphuric acid undergoes no change in its intimate nature.

3. As foon as the fulphureous acid appears, no more ether is formed, or at leaft very little; but then there paffes the fweet oil of wine, together with water and acetous acid.

4. The fweet oil of wine having ceafed to come over, nothing further is obtained but the fulphureous and carbonic acids, and at laft fulphur, if the diftillation be carried to drynefs.

The operation of ether is therefore naturally divided into three periods; the firft, in which a fmall quantity of ether and water are formed without the affiftance of heat; the

* Philofophical Journal, I. 44.

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fecond, in which the whole of the ether which can be obtained is difengaged without the accompaniment of fulphureous acid; and the third, in which the fweet oil of wine, the acetous acid, the fulphureous acid, and the carbonic acid are afforded. The three ftages have no circumftance common to all, but the continual formation of water, which takes place during the whole of the operation. The principles eftablifhed in the preceding memoir, and the comparifon of the phenomena here defcribed, would in ftrictnefs be fufficient to explain how the fulphuric acid acts upon alcohol in the formation of ether; but as this operation includes feveral particular circumftances which are not comprifed in the expofition of the general principles fketched out in the firft memoir, we fhall in this place enter into fome details neceffary for the folution of the propofed problem.

The ether which is formed without the affiftance of caloric, and the carbone which is feparated without decompofition of the fulphuric acid, prove that this acid acts on alcohol in a manner totally different from what has hitherto been fuppofed. It cannot, in fact, be affirmed, that the acid is altered by the carbone, becaufe daily experience fhews that no fenfible attraction takes place between thefe two bodies in the cold; neither can it be affected by the hydrogene, for in that cafe fulphureous acid would have been formed, and it is known that no trace is exhibited during this firft period. We muft therefore have recourfe to another fpecies of action, namely, the powerful attraction exercifed by the fulphuric acid upon water. It is this which determines the union of the principles which exift in the alcohol, and with which the concentrated acid is in contact: but this action is very limited if the acid be fmall in quantity; for an equation of affinity is foon eftablifhed, the effect of which is to maintain the mixture in a ftate of repofe.

Since it is proved that ether is formed in the cold by the mixture of any quantities of alcohol and fulphuric acid, it is evident that a mafs of alcohol might be completely changed into ether and vegetable acid by ufing a fufficient abundance of fulphuric acid. It is equally evident that the fulphuric acid would not by this means undergo any other change than that of being diluted with a certain quantity of water. This obfervation proves, at the fame time, that alcohol contains oxygene, becaufe water cannot exift without this principle, which muft be afforded by the alcohol only, fince the fulphuric acid fuffers no decompofition. It likewife fhews that the fulphuric acid here, inftead of favouring the decompofition of water as in many other cafes, does, on the contrary, determine its formation; fo that its action on the oxides of carbone and hydrogene, which conftitute vegetable matters in general, is abfolutely the reverfe of that which it exercifes upon the fimple combuftible bodies, fulphur and metals, for example, of which it effects the oxidation by the decompofition of water.

We muft not however imagine, from thefe facts, that ether is alcohol minus oxygene and hydrogene. Its properties alone would contradict this, for a quantity of carbone proportionally greater than that of the hydrogene is at the fame time feparated. It may in fact be conceived, that the oxygene, which in this cafe combines with the hydrogene to form water, not only faturated that hydrogene in the alcohol, but likewife the carbone. So that, inftead of confidering ether as alcohol minus hydrogene and oxygene, we muft, by keeping an account of the precipitated carbone and the fmall quantity of hydrogene contained in the water which is formed, regard it as alcohol plus hydrogene and oxygene. We muft obferve, that the comparative analyfis of alcohol and of ether, of which the details would

would be out of place in this memoir, correfpond perfectly with the refult afforded by the theory.

The foregoing are the effects produced by a combination of alcohol and fulphuric acid, fpontaneoufly produced without foreign heat. Let us in the next place obferve how this combination is effected when caloric is added. The phenomena are then very different, though fome of the refults are the fame.

In the firft place we muft obferve, that a combination of fulphuric acid and alcohol in equal parts does not boil at lefs than 78 degrees of temperature, while that of alcohol alone boils at 64. Now fince ebullition does not take place till the higher temperature, it is clear that the alcohol is retained by the affinity of the fulphuric acid, which fixes it more confiderably. Let us alfo confider that organic bodies, or their immediate products, expofed to a lively brifk heat, without the poffibility of efcaping fpeedily enough from its action, fuffer a partial or total decompofition, according to the degree of temperature. Alcohol undergoes this laft alteration when paffed through an ignited tube of porcelain. By this fudden decompofition it is converted into water, carbonic acid, and carbone. The reafon, therefore, why alcohol is not decompofed when it is fubmitted alone to heat in the ordinary apparatus for diftillation, is, that the temperature at which it rifes in vapours is not capable of effecting the feparation of its principles; but when it is fixed by the fulphuric acid or any other body, the elevated temperature it undergoes without the poffibility of difengagement from its combination is fufficient to effect a commencement of decompofition, in which ether and water are formed, and carbone is depofited. Nothing more therefore happens to the alcohol in thefe circumftances than what takes place in the diftillation of every other vegetable matter in which water, oil, acid, and coal are afforded.

Hence it may be conceived that the nature of the products of the decompofition of alcohol muft vary according to the different degrees of heat, and this explains why at a certain period no more ether is formed but the fweet oil of wine and acetous acid. In fact, when the greateft quantity of the alcohol has been changed into ether, the mixture becomes more denfe, and the heat which it acquires previous to ebullition is more confiderable. The affinity of the acid for alcohol being increafed, the principles of this acid become feparated; fo that, on the one hand, its oxygene feizes the hydrogene and forms much water, which is gradually volatilized; while, on the other, the ether retaining a greater quantity of carbone, with which at that temperature it can rife, affords the fweet oil of wine. This laft ought, therefore, to be confidered as an ether containing an extraordinary portion of carbone, which gives it more denfity, lefs volatility, and a lemon-yellow colour.

During the formation of the fweet oil of wine, the quantity of carbone which is precipitated is no longer in the fame proportion as during the formation of ether.

What we have here ftated concerning the manner in which ether is formed by the fimultaneous action of the fulphuric acid and heat, appears fo conformable to truth, that nearly the fame effects may be produced by a cauftic fixed alkali. In this cafe alfo a kind of ether and a fweet oil of wine are volatilized, and coal is precipitated. It is therefore only by fixing the alcohol that the fulphuric acid permits the caloric to operate a fort of decompofition. It may alfo be urged as a proof of this affertion, that the fulphuric acid, which has ferved to make ether as far as the period at which the fweet oil of wine begins to appear, is capable of faturating the fame quantity of alkali as before its mixture with the alcohol.

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With regard to the formation of the oil of wine by the augmentation of temperature from the concentration of the ſulphuric acid, it is ſo true, that if water and alcohol be added in the ſame proportion as the volatile product comes over, oil of wine will never be formed, and all the alcohol will be converted into ether.

It is to the ſame cauſe, namely the heat at a higher degree of intenſity, that we muſt attribute the development of carbonated hydrogene gas, or the oleſiant gas, which ſeems to be nothing elſe but the ſweet oil of wine, with a ſmall portion leſs of oxygene and more of caloric.

CONCLUSION.

From the facts and obſervations contained in this memoir, it follows:

1. That the formation of ether is not owing, as has been thought, to the immediate action of the principles of the ſulphuric acid upon thoſe of the alcohol, but to a true reaction of the elements of the alcohol upon each other, and particularly the oxygene and hydrogene, occaſioned merely by the ſulphuric acid.

2. That we might in ſtrictneſs change any quantity whatever of alcohol into ether, without the aſſiſtance of heat, by ſufficiently increaſing the proportion of ſulphuric acid.

3. That the operation is divided into two principal ſtages, in one of which ether and water are only formed; and in the other, the ſweet oil of wine, water, and the acetous acid.

4. That ſo long as ether is formed, the ſulphuric acid is not decompoſed, and there is no product of ſweet oil of wine; but as ſoon as this laſt appears, little, if any, ether is afforded, and the ſulphuric acid is at the ſame time decompoſed.

5. That the formation of ſweet oil of wine may be prevented, by keeping the temperature of the mixture between 75 and 78 degrees; which may be eaſily done, by ſuffering a few drops of cold water to fall from time to time into the retort.

6. That alcohol differs from ether by containing more carbone and leſs of hydrogene and oxygene, and that the ſweet oil of wine is to ether nearly what alcohol is to the oil*.

II.

On the Multiplier of Electricity. By the Inventor, Mr. T. CAVALLO; with Obſervations.

To Mr. NICHOLSON, Editor of "The Journal of Natural Philoſophy, &c."

SIR,

HAVING a few days ago ſeen for the firſt time your Journal of Natural Philoſophy, Chemiſtry, &c. I found in the third article of No. 1. an erroneous account of my Multiplier of Electricity, a deſcription of which is contained in the third volume of my Treatiſe on Electricity; and truſting that your candour will allow a correction of this error to come before the Public, I have taken the liberty to ſend you the following explanation, with a requeſt that you will be pleaſed to inſert this letter in your next number.

In the firſt paragraph of the above-mentioned article, you compare an electrical inſtrument of your contrivance (which you deſcribe in the ſequel) with my multiplier, in the following words:—"I then (viz. in the year 1787) mentioned my intention to conſtruct an inſtrument on that principle, which ſoon afterwards I did. It was ſhewn to Sir Joſeph Banks and his friends,

* Theſe Memoirs are tranſlated from the Annales de Chimie, XXIII. 186.

friends, at his house, nearly at the same time, and in the same year transferred to the celebrated Mr. Van Marum of Haerlem, who now possesses it. From various other avocations I was prevented from causing any others to be made. It is not therefore wonderful that the same thought should since have occurred to so great a master of the subject as Mr. Cavallo, who, in the third volume of his Electricity, published in 1795, gives a description and engravings of an instrument very different in form, but the same in principle, &c."

Now the error I allude to consists in the assertion *that the two instruments are the same in principle*; and I flatter myself that the following explanation will be sufficient to shew that they are so far from it as to be totally different in principle and in effect.

Your instrument does nothing more than collect a considerable quantity of diffused electricity into a small compass, which is exactly the same office as is performed by Mr. Volta's condenser, or by my collector of electricity, which is described in the Philosophical Transactions for the year 1788; whereas my multiplier renders an absolutely small quantity of electricity manifest, by accumulating a considerable quantity of the contrary electricity. Or, to be more explicit, your instrument cannot communicate to the electrometer a greater quantity of electricity, or rather not nearly so much of it, as is contained in the electrified body which is to be examined; whereas my multiplier accumulates a quantity of electricity many times greater than that of the body in question, and of course much more perceptible.

Suppose, for instance, that a certain body, having its surface equal to that of the electrometer which belongs to your instrument, contain 100 parts of electricity; and suppose, likewise, that the said electrometer requires 200 such parts of electricity in order to shew any divergency. Now your instrument will be found incapable of manifesting the electricity of the given body; for, when the upper tinfoil segments have conveyed the greatest portion, or we may even say all the 100 parts of electricity from the given body to the hook with the electrometer, they cannot, in the subsequent rotations of the instrument, collect any more. The electrometer, therefore, being at most loaded with 100 parts of electricity, cannot shew any divergency; since, according to the supposition, at least 200 parts of electricity are required in order to produce any divergency in it.

But if the above-mentioned electrified body be applied to the plate A of my multiplier, and the instrument be worked, the plate C, and of course an electrometer which is connected with it, will soon acquire 300 or 400 parts, or, in short, a much greater quantity of electricity than is sufficient to cause a divergency in the electrometer; because, by working the instrument, the original quantity of electricity in the plate A is not diminished, or removed from it; but the contrary electricity (which is repeatedly and unlimitedly induced upon the plate B by the plate A) is accumulated upon the plate C, &c.

Sir, I remain your obedient humble Servant,

Wells-Street, Oct. 30, 1797. T. CAVALLO.

AFTER returning my sincere thanks to Mr. Cavallo for the detection of the error he mentions, into which I certainly should not have fallen if I had consulted his Treatise at the time the paper he alludes to was written, I shall take the present opportunity of making some remarks on the instruments we at present possess for manifesting the presence and kind of weak electricities. These are: the condenser, the simple doubler, the doubler with mechanism,

chanism, two instruments by Mr. Cavallo, and the instrument described at p. 17 of this Journal.

After the discovery of the method of insulation by Stephen Gray, the first form in which compensated electricity was observed appears to have been in the combination called the electric jar; in the explanation of which so many of the earlier electricians have shewn their ingenuity. The next instance consisted in the application of conductors to each other; by which it was shewn, that electricity is not equally diffused over the surface of a conducting body, while any part of the electrified mass which differs in intensity is separated from the rest by the interposition of air or other non-conductors. The cann and chain of Franklin, and the electrical well of Beccaria, exhibited the principle of compensation in another light; and the subject was rendered still more difficult when the strong opposite electricities of ribbands and silk stockings, placed in contact, exposed to friction in that situation, and then separated, were shewn in the experiments of Cigna and Symmer. The vindicating electricity of Beccaria appeared to be still more remote from the simple general laws which had been applied to explain the effect of the jar; and the electrophore of Wilcke presented difficulties scarcely less considerable.

The first process for accumulating electricity, which differs from that of mere friction, appears to have been invented by Professor Lichtemberg *, and also by Dr. Klincock †, of Prague. It was performed by means of two resinous plates like those of the common electrophore, and one metallic plate with an insulating handle. When one of the resinous plates had been slightly rubbed, it was used to produce the opposite electricity in the metallic plate. This plate was then conveyed to the other resinous plate, and made to deposit its electricity by applying its edge, or otherwise, to the non-conducting surface. The last-mentioned surface could then be used in the ordinary manner to give an electricity to the metallic plate of the same kind as that of the original resinous surface, to which it was again applied by the edge. The electricity of the first resinous plate became thus increased, and, by the first-mentioned process, the metallic plate was returned to the other resinous surface in a higher state of electrization. By a repetition of these manœuvres, the two plates were speedily put into as high a state of electricity as they were capable of retaining.

An accidental circumstance directed the attention of Professor Volta to the advantages which might be derived from the increased electrical capacity of the metallic plate of the electrophore, when placed not upon the resinous plate, but on an imperfectly conducting substance ‡. If the last-mentioned substance were a good conductor, it would at some part of its surface be so far in contact with the upper plate as to carry off any electricity communicated to this last; and if it were a non-conductor, it might, and in most cases probably would, possess an electric state of its own which would influence that of the upper plate. But the imperfect conductor will not readily produce the first effect, and is incapable of the latter. It therefore serves only to assume the contrary state of the upper plate whenever electricity is communicated to this last, and renders it susceptible of absorbing a much greater quantity than it else could have done; the nature and intensity of which becomes per-

* Journal de Rozier, January 1780, p. 20.

† Philosophical Transactions, LXVIII. p. 1029.

‡ Journal de Physique for May and August 1783, and also the Philosophical Transactions, Vol. LXXII. p. 237.

ceptible when the plate is lifted up. Mr. Cavallo improved this inftrument, by communicating the electricity of the metallic plate, when raifed up, to another plate of the fame kind, but much fmaller, and in contact with an imperfectly conducting furface. And the Rev. Mr. Bennett adapted the fame to his electrometer *. This is the condenfer of Volta; and the imperfectly conducting plate may be a piece of dry marble, or a common wooden table, or any kind of plate covered with thin filk.

Mr. Bennett was, I believe, the firft who improved the condenfers of Volta, by making ufe of a manipulation fimilar to that of the Profeffors Lichtemberg and Klincock. His inftrument, called the doubler, confifts of three metallic plates, capable of being applied to each other with their flat furface, but prevented from contact in this fituation by a thin coating of varnifh. They have infulating handles at the fide, and may be brought into actual contact edgewife. Let the three plates be called A, B and C; and the manipulation for increafing the intenfity of one of them, for example A, will be as follows:

The plate A being infulated, place B upon it. In this fituation communicate electricity to it, while B is touched with the finger. The confequence will be, that A will receive a much larger quantity of electricity than it would elfe have abforbed if B had not been prefent.

Remove the communication from A, and the finger from B, and raife the latter by its infulating handle. A and B will then exhibit the oppofite electric ftates more ftrongly than while together.

Place C upon B, and touch C with the finger. It will affume the oppofite ftate to B, or, in other words, the fame ftate as A.

Place B upon A, as before, touching B with the finger; and at the fame time apply C edgewife to A. In this fituation, C, being without compenfation, will give the greateft part of its electricity to A.

Remove C; take the finger from B, and feparate B from A. The oppofite electricities will be ftronger than before, on account of the increafe afforded by C.

Place C upon B again, as in the third ftage of the original procefs, and repeat the fubfequent manipulations. In each of them the intenfities of A and B will be nearly doubled.

Though this procefs is fimple and evident, yet it requires to be learned, and takes a certain time for the performance. It therefore appeared a defirable object to form an inftrument which fhould by fome very fimple movement complete this fmall feries of operations. In the month of December 1787 Mr. Partington lent me an inftrument contrived by Dr. Darwin, and confifting of four metallic plates, two of which were moveable by wheel-work into pofitions which required them to be touched with the hand in order to produce the effect. It appeared to me that the whole operation, including the touching, might be done by a fimple combination without wheel-work by the direct rotation of a winch. This was foon afterwards effected, and communicated by me to the Royal Society in the year 1788. The inftrument is alfo defcribed in Mr. Bennett's *New Experiments on Electricity*, publifhed in 1789, and in other books.

Mr. Bennett and Mr. Cavallo obferved, foon after the difcovery of the doubler, that it never fails to exhibit an electric ftate by the mere operation, without any communication

* Philofophical Tranfactions, LXXVII. p. 32.

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of electricity having been previoufly made. The former made feveral valuable obfervations and experiments to inveftigate the caufes and remove the uncertainty produced by this fpontaneous electricity; and the latter prefented to the Royal Society * an account of a new inftrument called a collector of electricity, in which, as he afferts, this imperfection does not take place. It confifts of a plate of tin, fupported by two upright fticks of glafs; on each fide of which plate are two frames of wood, covered with gilt paper, which do not touch the tin plate, but ftand parallel to it at a fmall diftance. Thefe frames are faftened to the platform of the inftrument by hinges; fo that if electricity be communicated to the plate, it will receive a large quantity without any confiderable intenfity, becaufe its capacity is much augmented by the vicinity of the plane of gilt paper on each fide. But if thefe planes be thrown back into the horizontal pofition, which is eafily done by means of their hinges, the electricity, which before was compenfated in the plate, will have its intenfity greatly increafed. An electrometer connected with this plate will therefore fhew figns of electricity by means of a communication made between a large ftock of electricity, and the tin plate in its firft pofition, though the intenfity of that ftock may have been too fmall to have affected the electrometer without this contrivance.

It does not appear, in the author's defcription of this inftrument, that it removes the equivocal effect of the doubler; for it is evident that it does not, in its fimple procefs, enter the province of the doubler in which this effect takes place. The doubler requires fix or feven turns before it will exhibit fpontaneous electricity; at which period the firft charge is magnified above twelve thoufand times: but his fimple inftrument will fcarcely exceed one hundred times, and therefore requires the electricity to be one hundred and twenty times as ftrong as that which caufes the uncertainty of the doubler. Whence it may be inferred, that the doubler would have acted unequivocally with all fuch electricities as this inftrument is capable of exhibiting.

But the fpinning inftrument, as Mr. Cavallo in the foregoing letter juftly obferves, does nothing more than collect a confiderable quantity of diffufed electricity into a fmall compafs, though it does not appear to be exactly the fame office as is performed by M. Volta's condenfer, or his collector, as defcribed by their refpective inventors. For thofe inftruments, if very fmall, may exhibit very little intenfity in conjunction with an electrometer of confiderable furface; whereas the fpinning inftrument, befides its facility in performing the operations, may be confidered as poffeffing a furface indefinitely great. Thefe advantages, fuch as they are, conftitute all the difference I perceive between them.

Mr. Cavallo's multiplier, defcribed in the third volume of his Complete Treatife on Electricity, which I had miftakenly compared with the fpinning inftrument, confifts of four metallic plates, A, B, C, and D. The plate A is fupported on a ftick of glafs fixed in the platform of the inftrument. The plate B is alfo fupported on a ftick of glafs which is fixed on a radius or arm, by means of which it can be brought very near and parallel to A, and again removed, fo as to touch the third plate C, by means of a wire projecting from B. The plate C is fupported, like A, by a ftick of glafs fixed in the platform. And, laftly, the plate D is fixed by a metallic ftem upon a fliding-piece, by means of which it can be brought very near and parallel to C, and occafionally removed.

* Philofophical Tranfactions for 1788.

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The operation is this:—Let B be brought as near to A as the construction will admit. In this case it must be remarked that the wire proceeding from B will touch another wire that communicates with the earth. Let electricity be then communicated to A. The plate A will receive about one hundred times as much electricity when B is thus applied at about 1-40th of an inch distance, than it would have otherwise acquired at a like intensity, and B will possess the contrary state nearly equal. Let B then be removed so as to come in contact with C, of which the capacity is no less increased by the vicinity of D. The electricity of B will be shared between itself and C in proportion to their capacities; that is to say, about 99 parts in C, and 1 in B. At the next repetition B will bring 100 parts, which, together with the 99 parts in C, will be shared in the same manner; that is to say, 197 in C, and 2 in B. In this manner, without entering into the consideration of series where the data cannot pretend to minute accuracy, it is clear that the process, indefinitely prolonged, can never produce a greater charge in C than 100 times the quantity conveyed at each single motion by B; that is to say, 100 × 100 times, or 10,000 times the intensity of the original opposite electricity given to A, provided D be drawn back to leave C in a disengaged state.

It has always appeared to me that the doubtful results of the doubler must arise from the natural or spontaneous state of the plates A and B, at their first conjunction. If this charge be too strong to be destroyed by the energy of the communicated electricity, the doubler must in the result shew the same electricity as it would have shewn without such communication, though somewhat later. And if the same circumstance were to obtain in the plates A and B of Mr. Cavallo's last instrument, or any other contrivance operating by a charge, I do not see but that the results must be equally uncertain. If the electricity of B, instead of being accumulated in C of the multiplier, had been employed to produce the contrary state in D, and thence by communication to A, as in the doubler, it does not appear that any uncertainty could follow, provided only that the first effect of the conjunction of A and B had been governed by the communicated electricity. And if this reasoning be true, it will follow from that and the facts, that the uncertainty of the doubler exists also in the condenser, the collector, the spinning instrument, and the multiplier, though the first-mentioned instrument alone is delicate enough to shew it; and that in all electricities which are strong enough to affect any of the latter, the doubler may be used without fear of an equivocal result.

III.

A Memoir on certain Methods of Economy and Improvement in the Manufacture of Hats. By Citizen CHAUSSIER *.

THE progress of the arts has been long retarded by prejudice, and the servile disposition which attaches itself without enquiry to the ancient practices of business; and more particularly by the spirit of individual interest and suspicion which have prevailed among artists, and induced them to conceal their operations under a kind of mysterious veil. By this means

* Journal Polytechnique, cahier I. p. 160.

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the knowledge of the arts was in a great meaſure confined to thoſe who actually practiſed them. Error was propagated under the ſhade of ſecrecy, at the ſame time that uſeful proceſſes and improvements, which fortunate experiments had diſcovered, were alſo confined to the limits of individual works, beyond which they were never extended. At preſent, however, the arts cannot but participate in the ſalutary reform which has taken place on the ſoil of equality. Every effort ought to be directed to the ſame object, and every petty intereſt ought to be ſacrificed to the public welfare. Thoſe myſterious reſervations, thoſe practices ſo carefully concealed under the name of ſecrets, ought to be diſcloſed. The means of amelioration and improvement ought not to remain concentrated in any particular workſhop. As ſoon as experience ſhall have confirmed their efficacy, they ought to become the public property *. Theſe conſiderations induce me to point out certain proceſſes relative to the art of the hatter, which for ſome time paſt have been employed with much ſucceſs in a large manufactory in the department of La Côte d'Or.

The art of the hatter is well known to conſiſt of a method of forming, with the wool or fur of different animals, a kind of ſtuff of a denſe compact texture, capable of aſſuming and preſerving any figure which may be given to it. To obtain this object a variety of proceſſes are employed, partly chemical and partly mechanical, which may be reduced to four principal operations; namely, felting, fulling, dyeing, and preparation.

I ſhall not here deſcribe the operation of felting, nor the previous preparations which ſeveral kinds of hair demand in order that they may be uſed in manufactures. This would lead us too far from our preſent object, and would be merely a repetition of what is generally well known; eſpecially ſince the publication of the intereſting memoir † of Citizen Monge, in the ſixth volume of the Annales de Chimie.

I ſhall

* The learned author has been carried a little too far by attending to one ſide of the queſtion only. No private property can be better grounded than a man's right to the reſults of his own intellect; and there is none that he can better defend. How far the public may acquire a right of the kind here mentioned, by propoſing an adequate equivalent for individual exertions and expence, is a queſtion of great difficulty, whether it be morally or politically conſidered. N.

† As this memoir is not generally known in England, it will be uſeful to inſert an abſtract in this place. Annales de Chimie, VI. 300.

1. When a ſingle hair is inſpected by the microſcope, under whatever magnifying power, it appears ſmooth and poliſhed: 2. Yet it is certain that the ſurface is not equally ſmooth when rubbed in different longitudinal directions, but is compoſed either of ſcales like thoſe of fiſh, or imbricated zones like the horns of animals. For, 3. when a hair is held by the root, and drawn between the finger and thumb, there is little friction, and no noiſe; but, on the contrary, when it is held by the point, and drawn in the oppoſite direction, the reſiſtance is more conſiderable, the motion tremulous, and attended with a chirping noiſe. And again, 4. If an hair be held between the finger and thumb, and rubbed, by alternately moving them in the direction of its length, a progreſſive motion will be produced, which is always with the root end foremoſt. 5. The ſame ſtructure may be ſhewn by tying two hairs together, and then giving the knot a few blows between the palms of the hands; for the knot will either untie itſelf, or draw cloſer, accordingly as the aſperities of the ſurfaces are placed in the tying. 6. From this mechaniſm, which is common to wool and every other kind of hair, may be deduced the harſh feel of woollens againſt the ſkin compared with linens, and their irritating effect upon wounds. 7. It is the diſpoſition to progreſſive motion endwiſe which cauſes hair to entangle and felt itſelf together, when preſſed by the hatter between two linen cloths; to which laſt they do not unite, becauſe its fibres are ſmooth. 8. Cut hair is better than ſuch as is plucked, becauſe the bulbous roots of the latter prevent the progreſſive motion. 9. The fibres of wool, being crooked, muſt naturally move in curve lined directions; but thoſe of the hare, the rabbit,

I ſhall confine myſelf to the obſervation, that a workman, by means of the bow, beats, ſpreads, and diſtributes equally in ſmall flocks upon a plane ſurface the quantity of wool he has ſelected: that he then covers it with a linen cloth ſlightly moiſtened, and by means of a gradual preſſure in every direction he connects the different fibres, which, by intertwining and croſſing each other, form a piece of ſtuff of a ſoft and ſpongy texture. Upon this firſt piece he applies another formed in the ſame manner, and ſometimes a third or fourth, according to the nature of the materials, and the intended thickneſs and conſiſtence of the work. Theſe different pieces are ſucceſſively brought together, and diſpoſed in a form ſuitable to the article* he propoſes to fabricate; and, in order to effect the coheſion, he uſes a number of preſſures and alternate motions in different directions, during which he preſerves the ſuppleneſs and flexibility of the material by ſlight aſperſions of warm water.

This work, which is entirely mechanical, forms the felt, which is a kind of ſoft ſpongy ſtuff, of greater or leſs thickneſs, and in its firſt ſtate of a looſe imperfect texture. The fibres it is compoſed of, are yet weakly connected, and would ſoon diſunite again. In order to give the requiſite denſity and conſiſtence, it is neceſſary that it ſhould undergo the operation of fulling.

This operation, which is in a certain reſpect the completion of felting, has for its objects the intimate connection of the fibres, and a more perfect and durable coheſion of the whole maſs. But for this purpoſe the mere mechanical act of preſſure is inſufficient. In this way the reſult would be a formleſs maſs, without conſiſtence. Experience has long ſhewn, that for the fulling it is neceſſary to make uſe of a bath of water heated nearly to ebullition, into which are put ten or fifteen pounds of lees of wine *(lie)* for each hundred pounds of water. The heat is kept up during the whole time of working, and every three or four hours a new quantity of lees is added.

Into this bath the workmen plunge their felt, and begin their ſecond proceſs. The felt

rabbit, and the beaver, being ſtraight, cannot be uſed alone in felting till after a previous operation, which conſiſts in rubbing them with a nitrous ſolution of mercury, by means of a bruſh, before they are ſeparated from the ſkin. The ſolution, acting on one ſide of the hairs only, renders them crooked. 10. The ſtraight hairs are uſed for facing hats, in which operation the action muſt be continued for a determinate time only, otherwiſe they would paſs through and come out on the oppoſite ſide. 11. The diſpoſition of wool to felt itſelf is an impediment to the carding and ſpinning proceſſes; in which the operators therefore uſe oil, which diminiſhes the power of the hairs to act on each other. 12. But at the fulling-mill ſoap and marle are uſed, which carry off the oil, and reſtore the wool to its former ſtate; in conſequence of which the fulling proceſs takes place, and the cloths are rendered narrower, ſhorter, cloſer, ſtronger, and thicker. 13. The balls of hair in the ſtomachs of certain animals which lick one another are felted by the action of the ſtomach. 14. To which I may add, that a ſimilar cauſe is the chief reaſon why beds of cock and hen feathers are inferior to thoſe of gooſe feathers. The latter being much ſtraighter do not form the hard [illegible] which [illegible] in the former. If a machine were conſtructed either to cut off the quill part of cock and hen feathers, or to break the ſtem in three or four places, it would probably render the article of much more value. This laſt operation might be done by two fluted cylinders. N. [illegible]

* Felt may be uſefully employed in a variety of objects. Some time ago an artiſt imagined it might be applied to the ſoles of ſhoes, and made ſeveral pair. They were of good uſe in dry weather; but, notwithſtanding his having placed a [illegible] between the inner and outer ſole, they eaſily [illegible] the moiſture, and became heavy and unpleaſant. But it may be [illegible], by a further attention to the ſubject and [illegible] to the felt. The trial deſerves to be made; for which reaſon I have mentioned it in this place. C.

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is dipped in, and immediately again taken out and ſqueezed bended and rolled, by preſſure in different directions, ſometimes with the hand defended by leather, and ſometimes by means of a roller or other ſimilar inſtrument. The immerſion and working of the felt is repeated, and the operation continued until the ſtuff is well condenſed, and has acquired the requiſite ſolidity.

Such are the proceſſes uſed in manufactories, and they conſtitute the whole of what is explained in the *Deſcription des Arts*, and in the new edition of the *Encyclopédie Methodique*.

Since the operation of fulling is employed to form a denſe and compact ſtuff with the fibres or hairs, and to determine the intimate coheſion of its component parts; and ſince the mere mechanical operation is not ſufficient for this purpoſe, even with the aſſiſtance of a water-bath at the boiling-heat, without the addition of the lees as a neceſſary condition;—this laſt muſt be conſidered as a chemical ſolvent, which acts directly on the ſubſtance of the hairs themſelves, and produces, either by ſoftening or ſwelling them, an alteration neceſſary to inſure the coheſion of the different fibres of the ſtuff. But the lees being compoſed of the mucilaginous and colouring parts which are ſeparated, together with a great quantity of tartar, or the acidulous tartrite of pot-aſh, it became neceſſary to aſcertain, in a poſitive manner, what might be the principle of its action.

The editor of the Encyclopédie has not heſitated to affirm, that it is the alkali or pot-aſh of the lees which determines the fulling. But in order to ſhew how erroneous this aſſertion is, nothing more is neceſſary than to dip a piece of blue paper into the bath, by which the former becomes inſtantly red; and if, after ſeveral hours work, the ſtate of the bath be again examined, it is found that the acidulous tartrite of pot-aſh is partly exhauſted, and the workmen ſoon perceive, from the difficulty of continuing their work, that a new quantity is required to be added. And again, if we conſider the ſparing ſolubility of the acidulous tartrite of pot-aſh in cold water, it is eaſily ſeen why in this proceſs the water muſt be kept nearly boiling. Whence it is evident that it muſt act by the portion of acidule it contains.

This firſt obſervation induced me to think that the ſulphuric acid might be advantageouſly ſubſtituted in the place of the lees; and as twelve pounds of lees are uſually added to one hundred of water, I eſtimated by approximation that one gros of ſulphuric acid would be equivalent to at leaſt one pound of the lees, and conſequently that twelve gros of ſulphuric acid would be ſufficient for one hundred pounds of water.

Experiment ſoon confirmed my conjectures; and after a firſt trial, which the manufacturer did not conſent to make without trepidation, it was aſcertained that the uſe of the ſulphuric acid is greatly to be preferred to that of wine-lees; that it is not only much more economical, but ſtill more convenient in the uſe; and, what is ſtill more important, the health of the workman is not injured by the exceſs and duration of the heat, the thick vapours, and the diſguſting odour which exhales from the bath, particularly when the lees have been altered by mouldineſs and putrefaction, which is very common in theſe manufactories *.

* I cannot avoid mentioning a ſmall inconvenience which was experienced the firſt time the ſulphuric acid was uſed. A bottle was filled with the quantity requiſite for the bath intended to be made uſe of; and as the acid was poured out from ſome height without any precaution, ſome drops were diſperſed, which, falling on the hands or clothes of the operator, occaſioned ſpots by corroſion. To avoid this, it is only required that the bottle ſhould be plunged in the bath, and its contents poured out, ſo as to occaſion no ſcattering of drops. C.

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In fact, when the fulphuric acid is employed, it is ufelefs to keep the bath nearly boiling, as was formerly done. A degree of heat of twenty-five or thirty degrees (90° or 100° of Fahrenheit) is fufficient for good fulling. The faving of fuel is an object of importance in manufactories; and as very little fire is neceffary when fulphuric acid is ufed, cauldrons of lead may be fubftituted inftead of copper boilers, the firft coft and annual repair of which are very confiderable.

The felts prepared by the new procefs are alfo of a very fuperior quality to thofe which have been worked in the bath with wine-lees. In fact, the mucilaginous and colouring matters of the lees, which are fufpended in the bath, penetrate the texture of the ftuff, and adhere with more or lefs force; and when after having paffed the hats through the dye they are beaten, a fine black duft flies off in great abundance, which not only weakens the texture of the felt, but by diffufing itfelf through the manufactory greatly incommodes the workmen, and frequently occafions coughs and diforders of the throat.

The manufacturer, accordingly, who made this firft trial, continues to employ it in preference to the old procefs; and, according to his report, the advantages are incalculable: " Hats felted in this manner," fays he, in one of his letters, " are not only clear of the powder which abounds in the others, but they take the dye better, *ils s'ejarrent mieux*, and are cleaner. The workman has not his bath fo hot, which is to him a convenience no lefs valuable than the faving of fuel to the mafter.

Befides thefe advantages, the wine-lees formerly made ufe of by the hatters in large quantities will remain for other ufes, in the fabrication of faltpetre, foft foaps, and for other defirable purpofes*.

When I had afcertained that the fulling of hats was principally determined by the portion of acid which exifts in the lees, I determined to fubftitute the oil of vitriol, or fulphuric acid, becaufe it is found in the market at a reafonable rate. But though all the acids have common properties in which they refemble each other, they have alfo others by which each is peculiarly diftinguifhed. It would therefore be of importance to examine, among the numerous clafs of acids, whether fome one might not have a more direct action upon the ftuff than any of the others, fo as to fhorten the labour, without prejudice to the article or the economy of its preparation. Could not acid liquors be prepared in the manufactory itfelf, by fermenting barley, or bran, or fome fuch material? This is an object of refearch, to which we may invite manufacturers, and others who are attached to the cultivation of the arts.

I fhall not here defcribe the proceffes of dyeing. In the manufactory of La Côte d'Or, the nut-gall is not ufed; and oak-bark has been fubftituted with advantage: but thefe methods are known, and have been already publifhed. I proceed to the fourth and laft operation of hat-making.

It confifts in lining the inner furface of the crown, as well as of the brim of the hat, with a glutinous fubftance, which in drying gives firmnefs to the work, and preferves its form. The ufual compofition is made of gum arabic, common gum, and Flanders glue, which are

* The fulphuric acid, more or lefs diluted with water, might be advantageoufly ufed in many of the arts. Thus, the braziers ufually employ lees to clean and brighten their work; and moft certainly they would obtain the fame effect by fimply dipping the piece in water flightly acidulated with fulphuric acid, which would alfo fave them the trouble of rubbing the work for a long time, as they are obliged to do when they ufe lees, &c. C.

diffolved together in a fufficient quantity of water, and brought to the requifite thicknefs by boiling.

This preparation, fimple and eafy as it appears, is not indifferent with regard to the beauty and duration of the work. If it be too tenacious, it renders the fluff dry and brittle, and after fome months' ufe, a kind of greyifh incruftation is formed on the furface, which alters the texture. It appeared to me, that this effect was caufed by the gum arabic which is added to the glue. I therefore fought among the plants of our own country for a fimple preparation, which might be fubftituted inftead of thefe natural and friable gums. The mucilaginous principle abounds in a great number of plants, and may be eafily extracted by ebullition; and a gum may even be formed by evaporation, which preferves its fupplenefs and flexibility. Thefe confiderations induced me to recommend, inftead of the ufual preparation, a folution of glue in a decoction loaded with the mucilage of linfeed-oil. This preparation has long been ufed with economy in the manufactory, and advantage in the excellence of the work.

Since that time Citizen Margueron having communicated to me his obfervations on the mucilage which may be extracted from the leaves of the horfe-chefnut-tree *(marronier d'Inde)*, and having afcertained how great a portion of mucous and adhefive matter thefe leaves afford, efpecially when the foliation is in its vigour; a ftrong decoction of thefe leaves has been ufed with much fuccefs to make the preparation with glue.

There are a great many other native plants, which would be equally proper to afford factitious gums, and of which the ufe would be very advantageous in the arts. We are at prefent bufied upon this object, and hope in the courfe of time to prefent the refults of our refearches.

P. S. Since this memoir was written, a philofopher attached to the arts, and who has obferved much, has informed me that oil of vitriol or fulphuric acid is ufed in fome foreign manufactories in the fulling of hats, and that the procefs is confidered as a great fecret. I was ignorant of this. The publications I have examined make no mention of it, and the workmen I have confulted had no knowledge of its ufe. Befides which, truth is always new while it continues unknown, and ufeful obfervations require to be repeated in order to make them generally adopted.

IV.

Doubts concerning the Exiftence of a new Earth in the Mineral from New South Wales examined by WEDGWOOD in the Year 1790.

A PORTION of the fame mineral from Sydney Cove in New South Wales as was formerly analyfed by the late Mr. Wedgwood, and had been prefented to M. Haidinger, Counfeller of Mines, by Sir J. Banks, has been lately examined by the celebrated M. Klaproth*. As the refult feems to have inclined this able chemift to reject the new earth of Wedgwood, the fubject appears fufficiently interefting to admit of being ftated to the public.

* Beytrage zur Chemifchen Kenntnifs der Mineral. Kœrper, b. ii.

M. Klaproth

M. Klaproth had two famples; the firft confifting for the moft part of black brilliant fcales, which might have been taken for plumbago or carburet of iron, but appeared to him to be ferruginous mica; the fecond, which contained much lefs of this fubftance, was confidered by him as purer, and fubjected to experiment.

Thirty grains were reduced to fine powder. The greyifh colour it naturally poffeffed became blueifh by levigation. Upon this powder the concentrated muriatic acid was boiled, and decanted off when cold. New acid was boiled upon the refidue, and this operation was three times repeated.

The acid after filtration through double paper was not rendered turbid by water gradually added in fufficient quantity. The mixture, when expofed to heat, preferved its tranfparency.

By faturating the acid with carbonate of pot-afh, a fmall quantity of precipitate was afforded in light flocks, which, being collected on the filtre, edulcorated and dried, weighed 3.25 grains. This precipitate being added to diluted fulphuric acid, afforded cryftals of alum, and left a fmall portion of filiceous earth.

The refidue, which was not acted upon by the muriatic acid, was treated in the dry way with three parts of pot-afh, and afterwards with the muriatic acid. The gelatinous infoluble portion, feparated by the filtre, edulcorated and dried, weighed 19.5 grains. It was filex.

The muriatic folution, effayed by pruffic acid, afforded a blue precipitate, correfponding with about 0.25 grains of iron.

From this folution, when decompofed by carbonate of pot-afh, a quantity of alumine was feparated, which, when edulcorated, and dried by heat, weighed 8.5 grains, and upon being combined with fulphuric acid was totally converted into alum.

The whole of what this chemift obtained from the fand of Sydney Cove was alumine, filex, and iron, without the leaft indication of any other principle. Though he operated upon thirty grains only, and it was impoffible for him to repeat his analyfis, he obferves that the refult is fufficient to render the prefence of a new earth doubtful. He refers the folution of this doubt to time; and on the fuppofition of error in Mr. Wedgwood's experiments, he is difpofed to think he did not filtre his acid folution before he added water to it, and that the earth depofited by the diluted acid was probably nothing but filex chemically united to alumine.

Previous to a difcuffion of the probability how far Wedgwood could be miftaken in his facts and deductions, and on what the difference between his refults and thofe of M. Klaproth might depend, I had purpofed to give an abridgment of the paper of the former chemift from the eightieth volume of the Philofophical Tranfactions. But after having made the fame, and begun my comparative remarks, I found that fo much of thefe laft would depend upon the ftrict ftatement of experiments, that I determined to give fo much of the entire paper as relates to the new earth. And this I do the more willingly, as it is not, as I am aware, to be found in any work of general circulation. What follows, therefore, is in the words of Mr. Wedgwood *.

Analytical Experiments on a Mineral from Sidney Cove in New South Wales.

THIS mineral is a mixture of fine white fand, a foft white earth, fome colourlefs micaceous particles, and a few black ones, refembling black mica or black lead, partly loofe, or detached from one another, and partly cohering together in little friable lumps.

* Phil. Tranf. LXXX. 307.

None-

None of theſe ſubſtances ſeem to be at all acted upon by the nitrous acid, concentrated or diluted, nor by oil of vitriol diluted with about equal its meaſure of water. In the cold, or in a boiling heat, the mineral remained unaltered in its appearance, and the acids had extracted nothing from it that could be precipitated by alkali.

Oil of vitriol boiled upon the mineral to dryneſs, as in the proceſs of making alum from clay, produced no apparent change in it; but a lixivium made from this dry maſs with water, on being ſaturated with alkali, became ſomewhat turbid, and depoſited, exceeding ſlowly, a white earth in a gelatinous ſtate, too ſmall in quantity for any particular examination, but which, from its aſpect, from the manner in which it was obtained, and from the taſte of the lixivium before the addition of the alkali, was judged to be the aluminous earth.

The marine acid during digeſtion ſeemed to have as little action as the other two; but on pouring in ſome water, with a view only to dilute and waſh out only the remaining part of the acid, a remarkable difference preſented itſelf; the liquor became inſtantly white as milk, with a fine white curdly ſubſtance intermixed; the ſtrong acid having extracted ſomething which the ſimple dilution with water precipitated.

The white matter being waſhed off, more ſpirit of ſalt was added to the remainder, and the digeſtion repeated with a long tube inſerted into the mouth of the glaſs, ſo as nearly to prevent evaporation. The acid, when cold, and ſettled fine, was poured off clear, and on diluting it with water the ſame milky appearance was produced as at firſt.

The digeſtion was repeated ſeveral times ſucceſſively with freſh quantities of the acid, till no milkineſs appeared on dilution. The quantity of mineral employed was 24 grains, and the reſiduum, after the operations, waſhed and dried, weighed ſomewhat more than 19 grains, ſo that about one-fifth of it had been diſſolved. In ſome parcels of the mineral, taken up promiſcuouſly, the proportion of ſoluble matter was much leſs, and in none greater. It is only the white part, and only a portion of this, that the acid appears to act upon; the white ſand, much of the white ſoft earth, and all the black particles remain unaltered.

To try whether this tedious proceſs of ſolution could be expedited by triture or calcination, ſome of the mineral was rubbed in a mortar; and in doing this, it appeared pretty remarkable, that though the black part bore but an inconſiderable proportion to the reſt, yet the whiteneſs of the other was ſoon covered and ſuppreſſed by it; the whole becoming an uniformly black, ſhining, ſoft, unctuous maſs, like black lead, rubbed in the ſame manner with a few gritty particles, perceptible on preſſing hard with the peſtle. A pennyweight of this mixed, ſpread thin on the bottom of a porcelain veſſel, was calcined about an hour, with a fire between 30 and 40 degrees *; it became of an uniform, dull, white or grey colour, excepting a very few, and very ſmall, ſparkling, black particles, ſuſpected to be thoſe which had eluded the action of the peſtle; it loſt in weight ſix grains, or one-fourth.

The mineral thus ground and calcined was found to be juſt as difficult of ſolution as in its crude ſtate, with this additional diſadvantage, that the undiſſolved fine particles are indiſpoſed to ſettle from the liquor.

* By degrees of *fire* or heat *above ignition*, I mean thoſe of my thermometer; and ſome idea may be formed of their value by recollecting that they commence at viſible redneſs; and that the extreme heat of a good air-furnace of the common conſtruction is 160° or a little more. W.

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In all the experiments of diffolution, as often as the heat was at or near the boiling point of the acid, frequent and pretty fingular burfts or explofions happened, though the matter lay very thin in a broad-bottomed glafs. They were fometimes fo confiderable as to throw off a porcelain cup with which the glafs was covered, and once to fhatter the glafs in pieces. In a heat a little below this, the extraction feemed to be equally complete, though more flow; but a heat a little below that in which wax melts, or below 140° of Fahrenheit's thermometer, appeared infufficient.

To determine the degree of folution neceffary for the precipitation of the diffolved fubftance, and whether the precipitation by water be total, a meafure of the folution was poured into a large glafs, and the fame meafure of water added repeatedly. The third addition of water occafioned a flight milkinefs, which increafed more and more to the fixth. The liquor being then filtered off, another meafure of water produced a little frefh milkinefs, and an eighth rather increafed it; a ninth and a tenth had no effect. The liquor being now again paffed through a filter, folution of falt of tartar did not in the leaft alter its tranfparency; fo that after the folution has been diluted with eight or nine times its meafure of water, there is nothing left in it that alkali can precipitate.

From the manner in which the folution is neceffarily prepared, it cannot but contain a great redundance of acid; for the fmall quantity of acid fufficient for holding the foluble part fufpended, would be foked up or entangled by the undiffolved part, fo as fcarcely to admit of any being poured off; and it cannot be diluted or wafhed out but by the ftrong acid itfelf. The folution with which the above experiment was made, was reckoned to have only about fix grains of the foluble matter to three ounces of fpirit of falt, having been prepared by digefting that quantity of the fpirit, by half an ounce at a time, on thirty grains of the crude mineral.

A faturated folution was obtained by digefting, on a fmall portion of the folutions thus prepared, the precipitate thrown down by water from the larger portions till the acid would take up no more. A folution thus faturated cannot bear the fmalleft quantity of water; a fingle drop, on the firft contact, producing a milky circle round it.

Examination of the above Subftance, extracted from the Mineral by Marine Acid, and precipitated by Water.

THIS fubftance wafhed and dried is indiffoluble in water, as indeed might be expected from the manner of its preparation.

Nor is it acted upon by the nitrous or vitriolic acids, concentrated or diluted, cold or hot, nor by alkaline folutions, mild or cauftic, of the volatile or fixed kind.

It is diffolved by ftrong marine acid, but not without the affiftance of nearly the fame degree of heat that is neceffary for its extraction from the mineral. From this folution it is precipitated by water; and after repeated diffolutions and precipitations it appears to have fuffered no decompofition or change.

Spirit of nitre, added to the faturated folution, makes no precipitation; and if the quantity of nitrous acid exceeds, or at leaft does not fall much fhort of, that of marine acid in the folution, the mixture fuffers no precipitation from water. Nor does any precipitation

happen,

happen, though the nitrous spirit be previously mixed with even a large quantity of water, provided the quantity of solution added to it does not exceed that of the nitrous spirit in the mixture. The appropriate menstruum for this substance (that is, for keeping it in a state of dilute solution) appears, therefore, to be aqua regia; and the due proportion of the two acids, of any given strength, might be determined, if necessary, with greater accuracy and facility for this than for any other body I know of; because, if there be even a very minute surplus of marine acid in the solution, that surplus will instantly betray itself, on dropping a little into water; all that was dissolved by it, and no more, being precipitated by the water. It may be observed, however, that where an addition of nitrous acid is used, a saturated solution cannot be obtained (unless by subsequent evaporation), the same quantity of marine acid being necessary with as without that addition: the change or modification which the nitrous acid produces in the marine, serves, in the present instance, not for effecting the solution, as in the case of gold and some other metals, but merely for enabling it to bear water without depositing its contents.

Oil of vitriol dropped into the saturated marine solution, occasions no change till its quantity comes to be about equal to that of the solution; a considerable effervescence and heat are then produced, the liquor becomes milky, and the marine acid is extricated in its usual white fumes. The mixture, heated nearly to boiling, becomes transparent, and afterwards continues so in the cold. This vitriolic solution is precipitated by water, and the precipitate is redissolved by marine acid.

The saturated marine solution is indisposed to crystallise. By continued evaporation in gentle heat it becomes thick and butyraceous, and in this state it soon liquefies again on exposure to the air. The butyraceous mass, in colour whitish or pale yellow, is not corrosive, like the similar preparations made from some metallic bodies, nor is it more pungent in taste, but rather less so, than the combination of the same acid with calcareous earth. In a heat increased nearly to ignition, the acid is disengaged, and rises in white fumes, which, received in a cold phial, condense into colourless drops, without any appearance of sublimate. From the remaining white mass, spirit of nitre extracts so little as to exhibit only a slight milkiness on adding alkali; a proof that nearly all the marine acid had been expelled; for, while that acid remains, the whole is dissoluble by the nitrous.

The substance in question is not precipitated by Prussian lixivium. A drop or two of the lixivium do indeed occasion a little white or blueish white precipitation in the saturated marine solution; but in the more dilute no turbidness appears till the quantity of lixivium is such as to produce that effect by its mere water; and when the precipitate has at length been formed, it redissolves in marine acid as easily as that made by water; whereas the precipitates resulting from the union of the Prussian matter are not acted upon by acids till that matter has been extracted from them by an alkali. For further satisfaction, in this important point, the experiment was repeated with a solution in aqua regia. Here the Prussian lixivium, in whatever quantity it was added, occasioned no precipitation at all (only the usual blueishness arising from the iron always found in the common acids); and pure alkali added afterwards precipitated the original white substance unchanged.

The following experiments of precipitation by alkalis were made with the marine solution before the effect of an addition of nitrous acid had been discovered; and they were made with so much care and attention, that it was not thought necessary to repeat them afterwards.

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afterwards. To obviate as much as poſſible the equivocal reſults that might ariſe from water contained in the precipitants, the different alkalis were applied in the drieſt ſtate I could reduce them to; viz. pure ſalt of tartar, kept for ſome time in a heat juſt below redneſs; cryſtals of marine alkali melted and dried in the ſame manner; volatile alkali in cryſtals, a little ſurplus acid being in this inſtance previouſly added to the ſolution to counteract the water of cryſtallization in the alkali; *ſalt of tartar cauſticated* by quicklime, and haſtily evaporated to dryneſs; the *marine alkali cauſticated* in like manner; and the vapour of cauſtic volatile alkali, ariſing with a very gentle heat from a retort into a phial containing the ſolution. All theſe alkalis occaſioned copious precipitations. All the precipitates, after waſhing and drying, were found to rediſſolve in marine acid; and from all theſe ſolutions the original ſubſtance was precipitated unaltered on diluting them with water.

In ſtrong fire, from 142 to 156 degrees, this ſubſtance diſcovers a much greater fuſibility than any of the known ſimple earths. In a ſmall veſſel made of tobacco-pipe clay, it melted, and glazed the bottom; and on a bed of powdered flint, preſſed ſmooth in the manner of a cupel, it did the ſame. Magneſia or chalk would indeed vitrify in the clay veſſel; but on flint no one of the known earths ſhews any tendency to vitrification in that heat *. In a cavity ſcooped in a lump of chalk, this ſubſtance, in the heat above mentioned, ran into a ſmall round bead, ſmooth, whitiſh, and opake, not in the leaſt adhering to the calcareous maſs. On a bed of powdered quick-lime, it formed a browniſh ſcoria, which in great part had ſunk into the lime, and ſeemed to have united with it. On Mr. Henry's magneſia, uncalcined, it melted and ſunk in completely, leaving only a ſlight browniſh ſtain on the ſurface where it had lain. On beds of the baro-ſelenite, and barytic quick-lime, it likewiſe melted and ſunk in, leaving a diſcoloured ſpot behind; but whether it really united with the ſubſtrata, or only penetrated into their interſtices, could not be determined with certainty, on account of the ſmallneſs of the quantity of the mineral I had to work upon.

On a bed of powdered charcoal, in a crucible cloſely luted, this ſubſtance likewiſe melted: and therefore it may be preſumed not to have owed its fuſion in the above experiments to the ſame cauſe to which ſome of the common ſimple earths, in certain circumſtances, owe theirs, namely their union with the matter of the veſſel or ſupport; that is, with an earth or earths of a different kind from themſelves; but to poſſeſs a fuſibility ſtrictly its own, which takes place in a fire of 150 degrees, or perhaps leſs.

As charcoal in fine powder aſſumes a kind of fluidity in the fire, ſimilar to that which powdered gypſum exhibits in a ſmall heat, its ſurface had changed from concave to horizontal, and the bead had ſunk to the bottom; it was rough and black on the outſide, and whitiſh within. On repeating the experiments in a cavity ſcooped in a piece of charcoal,

* It may be proper juſt to mention that I find this to be a very commodious and ſure method of trying, in ſmall, whether any given earthy body be fuſible with other earths. If the body is diſpoſed to vitrify with any proportion of clay or flint, for inſtance, it will equally vitrify when a little of it is applied, or even duſted only, on the bottom of a ſmall cup made of clay, or on a ſmooth cloſe bed of finely powdered flint. The body, in this mode of application, ſeems to unite with only juſt ſo much of the matter of the ſubſtratum as is requiſite for their moſt perfect fuſion together, and has nothing elſe in contact with it, ſo that no deception can ariſe; whereas, if mixed with the ſame matter, there might be no appearance of fuſion, unleſs certain favourable proportions of the two ſhould chance to be hit upon; and even then, if the quantity be ſmall, it would not be certain but that the fuſion might have originated from the matter of the crucible. W.

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the result was a blackish bead like the former, only smooth on the outside, with something of metallic brightness, not unlike that of black-lead. Both beads were very light, and had a considerable cavity within. All the internal part was whitish, without the least metallic aspect; and the external glossy blackness appeared to be only the stain which charcoal powder communicates in strong fire to some earthy bodies that have a tendency to vitrify. By boiling in concentrated marine acid, a part of the beads was dissolved, precipitable, as at first, by water; but an accident prevented the process from being continued sufficiently to determine whether the whole could be dissolved or not.

By this fusibility in the fire, solubility in one only of the common mineral acids, and parting with the acid in a heat below ignition, precipitability by water, and non-precipitability by Prussian lixivium, this substance is strongly discriminated from all the known earths and metallic calces. And as it suffers no decomposition from any of the alkalis in any of the usual modes of application, I presume it cannot be considered as a combination of any of those earths or calces with any of the known acids; for all the combinations of this kind would, in one or other of the above methods of trial, have had the earth or metal disengaged from the acid.

Whether this substance belongs to the *earthy* or *metallic* class, I cannot absolutely determine; but am inclined to refer it to the earthy; because, though brought into perfect fusion in contact with inflammable matter and in close vessels, it does not assume the appearance which metallic bodies do in that circumstance.*.

The black substance, which seems to have composed about one-fifth part of the crude mineral, was found to resemble plumbago in its leading properties, but its residue did not appear to be iron. The remaining three-fifths of the mineral, which resisted the humid attacks in Mr. Wedgwood's experiments, was probably silex; but he does not speak of any direct examination of its properties by fusion with alkalis, the sparry acid, or otherwise.

On the preceding experiments of M. Klaproth and the late Mr. Wedgwood, the question which in the first place appears to demand solution, is, whether the same mineral was examined by both chemists? If this should not appear probable, it will be unnecessary to enter upon the subsequent enquiry, which set of experiments is most likely to be erroneous?

On the historical evidence, as far as regards the sameness of the parcels of mineral, I think there is little to be said. Both were obtained from Sir Joseph Banks; the first directly, and the latter with the intervention of the respectable, and as it may be concluded careful, M. Haidinger. Nevertheless, if the obvious description and decided experiments should point out a difference, it will afford more than a presumptive proof that they were not the same. 1. M. Klaproth's mineral was greyish, and became blueish by levigation. Mr. Wedgwood's was a fine white sand, a soft white earth, with some colourless micaceous particles, and a few resembling plumbago. When levigated, it became black, shining, soft, and unctuous. 2. Klaproth's marine solution, after filtration, afforded no precipitate by the addition of water. Wedgwood's solution in the same acid, when cold, settled fine; and, decanted off clear, afforded a white precipitate with water to the whole amount of its contents. After six measures of water had caused much of the precipitate to fall, the liquor was filtered,

* Here ends the extract in Mr. Wedgwood's own words.

and

and more water threw down a little more. 3. Klaproth's marine folution afforded a precipitate by the addition of an alkali which was foluble in vitriolic acid, and afforded alum. Wedgwood's precipitate by means of water was infoluble in the nitrous or vitriolic acids. The indirect folution of this matter in vitriolic acid was precipitated by water. 4. M. Klaproth's experiments fhewed nothing but filex, alumine, and a minute portion of iron. He thinks it probable that Wedgwood's precipitate by water was filex combined with alumine. But in Wedgwood's experiments this matter was fufed without addition in an hole fcooped in charcoal, and alfo in an hole in a lump of chalk, to which it did not adhere. Kirwan * found equal parts of alumine and filex infufible at 160° of Wedgwood; and Achard found them infufible in a porcelain furnace, in all proportions; not to mention that very refractory veffels are made out of the fame two earths mixed together. 5. I do not fee how Wedgwood's method of fubfidence and decantation could have left any thing fufpended which Klaproth's filtre could have detained.

Hence it feems fair to conclude that the two minerals were not the fame, however this may have happened; and that the exiftence of the new fufible earth of Wedgwood ftands on the fame evidence as before, namely, his experiments, which have not yet been repeated, that I know of.

V.

A Philofophical Memoir, containing—1. *Experiments relative to the Propagation of Sound in different Solid and Fluid Mediums.—And* 2. *An Experimental Enquiry into the Caufe of the Refonance of Mufical Inftruments. By M.* PERROLE †. *With Annotations.*

AS the tranfmiffion of found through water ‡, through air of greater or lefs denfity §, and through different gafeous fubftances ‖, has added to the amount of our philofophical knowledge, I have thought an abundant harveft of new facts might be obtained by caufing found to pafs through a great number of bodies of different kinds, as well folid as fluid, and comparing their effects together.

Thefe are the views which have directed the experiments of which I fhall give an account in the firft part of this memoir; and in the fecond I fhall avail myfelf of the refults in an endeavour to afcertain the caufe of the refonance of bodies.

PART I.

AS all the trials depend upon the following experiment, it is neceffary to give particular attention to its detail.

Experiment I. and chief. Clofe the ears with chewed or mafhed paper; fufpend a watch by an hook. Place the ear at the diftance of two lines from the watch, and you will not hear its

* Mineralogy, i. 58.

† Mem. de l'Acad. Royale de Turin, v. 195.

‡ Nollet, Mem. de l'Acad. Royale de Paris, 1743.

§ Mufchenbroek, n. 1442.—Nollet, Leçons de Phyfique, iii. 355.

‖ Prieftley's Experiments and Obfervations, &c. and my Experiments in the Turin Memoirs for 1786, 1787. P.

vibrations.

vibrations. Then take a ſolid body, ſuch as a ſmall cylinder of wood, of one foot or one foot and a half in length, and one or two lines in diameter. Place it in contact at one extremity with the watch, and apply the other end to one of the numerous parts of the head which propagate ſound by the touch *; for example, with the cartilaginous parts of the ear; you will hear the ſound much better than if the ear had not been cloſed, and the ſonorous body had been placed in the air at the ſmalleſt diſtance from the organ.

As the ſound was not perceptible at the diſtance of two lines in the firſt diſpoſition, and was very ſtrongly perceived in the ſecond; it is evident that the ſmall cylinder propagated ſound better than atmoſpheric air.

When we reflect on this experiment, and the reſult it affords, we may, without difficulty, perceive, that, in order to aſcertain the reſpective force of the propagation of ſound through ſolid bodies, nothing more is required to be done than to procure ſubſtances of different natures, to give them the ſame form, and ſubject them to an experiment of this kind. In this manner I made the following experiments:

Experiment II. I procured ſmall cylinders of the dry wood of fir, oak, box, cherry-tree, cheſnut-tree, and logwood, each one line in diameter, and one foot in length. The ears being then cloſed, I placed them ſucceſſively in contact with the watch, and at the ſame time with the cartilaginous part of the ear, as in the preceding experiment.

The different cylinders tranſmitted the ſound very well; but its tone † ſeemed to vary whenever a new cylinder was uſed, and the intenſity appeared to be never exactly the ſame. We had no means of aſcertaining the difference of the tone *(timbre)*, but the intenſity appeared in the following order, beginning with thoſe cylinders which appeared to propagate ſound with the greateſt activity: fir, logwood, box, oak, cherry-tree, cheſnut.

Experiment III. I determined to extend my reſearches to the metals, and conſtructed metallic cylinders ſimilar to the former. When ſubjected to the ſame trial, they tranſmitted the ſound in general rather worſe than the cylinders of wood.

The nature of the ſound appeared likewiſe to differ in the cylinders of wood and thoſe of metal. The tone was not exactly the ſame in the different metals, and the intenſity followed this order: iron, copper, ſilver, gold, tin, lead.

Experiment IV. I hung my watch ſucceſſively to ſtrings of ſilk, of wool, of hemp, of flax, hair, and gut, which were nearly of the ſame diameter and exactly of the ſame length as the ſolid cylinders. One extremity of the ſtring was applied by the hand in contact with the cartilage of the ear, while the watch reſted on the oppoſite extremity of the ſtring, which touched no part of the body. The ſtrings thus extended propagated the ſound with leſs

* Almoſt every part of the head propagates ſound when in immediate contact with a ſonorous body. This may be eaſily proved by applying a watch to the ſeveral parts after having cloſed the ears. See my Diſſert. Anat. &c. and my Recherches ſur l'Organe de l'Ouie et la Propr. des Sons, tom. iii. des Mem. de la Soc. R. de Medec. et le Journal de Phyſique, 1773, tom. ii. P.

† *Son timbre*, which does not, as I apprehend, imply any difference of acuteneſs or gravity in ſound, but another relation for which I know no Engliſh word but tone. Thus the tone *(timbre)* of the hautboy differs greatly from that of the flute when both are in uniſon, and ſo likewiſe do the different tones *(tons)* of the ſame kind of inſtrument. It does not appear from the text, whether the acuteneſs or gravity of theſe tranſmitted ſounds did differ from each other. N.

force

force than solid bodies, and modified it in a remarkable manner. The tone in each of the strings appeared to be different, and the intensity followed this order :—gut, hair, silk, hemp, flax, wool, cotton.

From the foregoing experiments it follows :—1. That hard bodies and stretched strings transmit sound much better than atmospheric air. 2. That each of these mediums transmits it in a manner which is peculiar to itself, so that the kind and intensity of the sound are never exactly the same, as far as can be judged on trials which do not always present very striking results *. 3. That in general wood transmits sound very well; that the metals transmit it with somewhat less energy, and that stretched strings occupy the third place in the scale of power in this respect.

Experiment V. Being determined to extend my researches, I caused the sound of the watch to pass through pieces of zinc, antimony, glass, sal gem, gypsum, dried clay, and marble. As I could not give the same form to these different substances, I was unable to determine with precision their respective powers of transmission; but I observed that all these bodies transmitted sounds better than air, and that it was modified in a particular manner by each of them. Marble was remarkable for the little force with which it transmitted the sonorous movements. Two pieces of this substance, of different form and size, propagated it in a weak and almost insensible manner.

These are the experiments I have made with solid bodies. To complete the series, it remained to submit fluids to similar trials.

I have already published my experiments on aëriform substances †. In this place I shall relate those I have made upon liquids. This part of my operations required a different process.

Experiment VI. I closed all the joints of my watch with soft wax, and then suspended it by a silk thread. In this state I hung it by an iron branch fixed in the wall, so that the watch remained suspended in the middle of a glass vessel five inches in diameter, and seven inches high, taking care that neither the watch nor the thread touched the vessel in any part. I remarked the kind of sound afforded by the watch, and the distance at which I ceased to hear it. After having marked this point, I then filled the vessel with water, into which I again suffered the watch to descend, with the same precaution of not suffering it or the thread to touch the vessel.

The tone *(timbre)* was changed in the water in a striking manner. The sound was propagated in so lively a manner that the glass, and a small table of wood, on which it stood at a distance from the wall, seemed to undergo direct percussions from a solid body. But what appeared still more astonishing was, that in the midst of all these agitations the fluid in which the watch was plunged was perfectly tranquil, and its surface not in the slightest degree agitated.

By substituting different liquids in the place of the water, I had results in general analogous to those I had obtained with that fluid; but each medium gave a different modification to the sound, of which the intensity was noted as follows:

* Such chiefly are those which were obtained with the cylinders of wood, and stretched strings. P.

† Mem. de l'Acad. Roy. des Sc. de Turin, 1786, 1787.

Intensity of Sound observed in different Fluids.

		Feet.
1. In the air serving as the term of comparison, it ceases to be heard at the distance of	——— ——— ——— ———	8
2. In the water, at that of	——— ——— ——— ———	20
3. Oil-olive	——— ——— ——— ———	16
4. Oil of turpentine	——— ——— ——— ———	14
5. Spirit of wine	——— ——— ——— ———	21

It is proper to observe, that on repeating these trials I observed some variations in the intensity, which appeared to depend on the organ of sense or accidental noises.

From the experiments made upon liquids, it follows:—1. That these as well as solids do transmit sounds much better than air, and that even the fat oils are not to be excepted *.

2. That each fluid upon trial is found to modify the sound in a peculiar manner.

3. Philosophers maintain the opinion that sound is propagated in the air by means of certain motions or undulations, which the transparence of that fluid prevents our seeing. My experiments with fluids which do not elude the sight, and in which no motion was perceived, notwithstanding the very effectual transmission of sound, may render this in some respect doubtful.

4. Lastly, the experiments on solids, fluids, and those I have published on the gases †, afford the probable conclusion that all mediums produce particular modifications in the tone (*timbre*) and the intensity of sound; or otherwise, that the same sound varies as often as it passes through a different medium. I shall now pass to the experiments which constitute the second part of this memoir.

PART II.

EVERY one has observed, that if a watch be placed upon a table, the sound is very evidently increased. The difference between the sound of a tuning-fork, when it vibrates without being placed in contact with any solid body, and that which it affords when its handle is pressed against a solid body with a large surface, is also well known. The experiments related in the former part of this memoir having led me to expect that the increase of force and harmony in these circumstances was owing to the property possessed by wood of propagating sound better than the external air, and modifying its tone; I determined to ascertain the truth by experiment.

The different powers I had observed in wood and marble, with regard to the propagation of sound, appeared proper to throw some light on this important question. In fact, if the modifications of the sound of the tuning-fork and the watch when placed on a table of wood, be owing to the energy with which this substance transmits the sound, and the marble propagating it very weakly, it would follow that a table of marble ought not to fortify the

* Morhof, *Steut.* pag. 104. affirmed that fat oils could not transmit sonorous modulations. P.

† Loco citato.

found, or at leaft to produce very little effect on thefe fonorous bodies. On thefe confiderations I made the following experiments:

Experiment I. I applied the tuning-fork upon a table of wood, and when it had ceafed to vibrate I applied my watch to the fame furface. The found in each was increafed in proportion to their refpective intenfities. I then removed the wooden top of the table, and fubftituted in its place another of marble, of the fame dimenfions. The tuning-fork and watch were applied as before. The found of the former was augmented, though much lefs than when it was applied on the wooden covering. The found of the watch was fcarce perceptibly increafed. I could perceive little difference between its found, whether it touched the marble or hung in the air at the fame diftance from the ear.

Though this experiment ftrongly confirms my conjecture, I determined, neverthelefs, to fubject it to another proof:

Experiment II. I placed my watch on the wooden table, and clofed my ears with chewed paper. When the ear was at a few lines diftance from the table, I could not hear the vibrations of the watch. I then placed my ear in contact with one of the fmall wooden cylinders ufed in the experiments already defcribed. The oppofite end of the cylinder was placed in contact with the table.

The found of the watch immediately and forcibly ftruck the ear. I made this application to every part of the table, not excepting even the feet; and the found was in every cafe diftinctly heard. The experiment was then repeated with the marble covering inftead of the wood. The vibrations of the watch were heard in an imperfect or indiftinct manner, and only when the wooden cylinder was applied at a fhort diftance from the fonorous body. I did not ufe the tuning-fork in this experiment, becaufe, in fpite of every precaution, it was not poffible to clofe the ears fo perfectly but that the found ftill remained perceptible.

In order to eftablifh my conjecture on the moft folid bafis, it remained to repeat the experiments upon tables of all the fubftances ufed in the experiments related in the firft part of this memoir, and to fhew that the refonances follow the proportion of the conducting powers of the bodies. But the difficulties which oppofed the execution of this plan induced me to confine my enquiries to afcertain whether the refonance does not vary in different bodies, as well as the power of tranfmitting found.

Experiment III. I therefore placed the tuning-fork fucceffively upon plates of earthenware, of porcelain, on plates of glafs, and upon thin ifolated plates of copper and tinned iron. The found was fortified by thefe bodies, and the tone never appeared to be exactly the fame in any two experiments.

Thefe trials naturally led me to examine the fame founds upon mufical inftruments. With this view I applied firft the tuning-fork and afterwards the watch, upon bafes, violins, mandolins, guitars, harpfichords, and horns. Both founds were proportionally increafed.

They appeared to acquire more force and melody by means of the mufical inftruments than on the other bodies. The intenfity feemed directly in proportion to the volume of the inftrument.

From thefe experiments it follows:—1. That all the fubftances which were tried, which poffeffed extended furfaces, fortify the weak founds produced by bodies which touch them, and modify the tone in a manner peculiar to each.

2. That

2. That these effects arise from the transmission of sound by solid bodies being in general better than by the air, and the peculiar modification of the tone by each.

3. That the resonance of musical instruments is more particularly to be attributed to this cause *.

4. The experiments with musical instruments afford reason to conclude that the volume of bodies has an influence on their resounding properties.

5. M. de Maupertuis † has affirmed that the resonance of musical instruments is owing to the instrument containing fibres of every possible length; each sound puts those in motion which are either in unison or concord with itself, while the others remain motionless ‡. But the second experiment, in which it is shewn that there is no part of any resounding body which does not transmit the sound, will not admit of the adoption of the ingenious thought of that celebrated author.

6. As marble in some degree extinguishes sound, and bears the same rank among solid bodies as inflammable air among fluids, it is not advisable to use it in the construction of churches, concert-rooms, theatres, or other edifices in which the propagation of sound is desirable.

Such are the principal results of experiments which, on the whole, have engaged my attention for a number of years; notwithstanding which, I have not been yet able to extend them, particularly those belonging to the second part, as far as they are capable of being carried. I shall not, however, have the mortification to think my endeavours useless, if the society to whom my works are addressed shall think they may constitute an addition to the sum of the discoveries in science which they continually present to the world.

Annotations to the Paper of M. PERROLE on Sound.

THE philosophy of sound is still exceedingly imperfect. Mathematicians have established theories upon the simple data that sonorous bodies vibrate, and that these vibrations are communicated to the surrounding elastic fluid, of which the density and spring are measurable. M. Perrole's experiments shew that the object of research comprehends much more than this. The following queries and remarks may perhaps prove of some advantage to the curious enquirer:

1. There can be no doubt but that undulation in the air accompanies its propagation of sound, and that sonorous bodies are put into a vibrating state by percussion. The proofs of this are too numerous to be detailed. Sound propagated through fluids seems to diminish in proportion to the distance, and so probably it does in solid bodies. Its intensity must depend upon the elasticity and density jointly. The military practice of lying down on the ground to listen for footsteps is well known; and Dr. Franklin relates that he heard the blow of two stones struck together at the distance of near a mile, smart and strong as if close at the ear §. We have no information respecting the velocities of sounds propagated through solid bodies.

* The numerous surfaces which these instruments present must likewise render them more sonorous. P.

† Acad. Paris, 1724.

‡ The memoir of M. de Maupertuis relates to stringed instruments only. P.

§ Experiments and Observations. London, 1774, p. 445.

2. As ſound paſſes through the air with a velocity of 1142 feet in a ſecond, and the ſwifteſt wind does not paſs with a velocity of 90 feet in the ſame time; it might naturally be inferred that the velocity of ſound with or againſt the wind ought to be ſomewhat affected, but its intenſity very little. The contrary, however, is the caſe; from which, and other circumſtances, ſome philoſophers have been diſpoſed to conclude that the medium by which ſound is tranſmitted is not air, but another fluid of greater ſubtilty.

3. Sounds ſeem more intenſe, and are heard to a greater diſtance, by night than by day. In a ſtill night, the voices of the workmen at the diſtillery at Batterſea may be heard at Weſtminſter Bridge, over an interval of about three miles; the watch-word at Portſmouth may be heard at Ride in the Iſle of Wight, the diſtance of which is between four and five miles; and numerous inſtances are related of the propagation of weak ſounds to much greater diſtances, without mentioning thoſe of which the intenſity is greater. It is a practical queſtion of ſome importance to aſcertain whether this difference may ariſe from the different ſtate of the air, the greater acuteneſs of the organ, or the abſence of the ordinary noiſes produced in the day. By attentive liſtening to the vibrations of a clock in the night, and remarking the difference between the time when no other noiſe was heard, and when a coach paſſed along, it has appeared clear to me that this difference ariſes from the greater or leſs ſtillneſs only, and that no voluntary effort or attention can render the near ſound much more audible, while another noiſe acts upon the organ. In this ſituation the ear is nearly in the ſtate of the eye, which cannot perceive the ſtars in the day time, nor an object behind a candle.

4. It would be impoſſible to uſe optical inſtruments in the day time, if the light of other objects beſides thoſe to which the attention is directed were ſuffered to mix itſelf with the rays admitted into the field of view. In the ſame manner it ſeems that we have little to hope with regard to the improvement of acouſtic inſtruments for day uſe, if the firſt requiſite, namely, the excluſion of unneceſſary ſound, cannot be obtained.

5. Numerous experiments have ſhewn that ſound can be reflected, and that the impreſſion on the ear is greater or leſs, according to the diſpoſition of the reflecting bodies. Optical inſtruments are diſpoſed in tubes of ſuch a length, that the rays of light which ariſe from a ſmall portion of the viſible hemiſphere can alone reach the organ of perception. All the others ſtrike the ſurface of the tube, and after one or more reflections are almoſt totally abſorbed or loſt. It remains to be aſcertained from reaſoning and experiment, how far the ſame effect may be produced with regard to ſound. With a cylindrical wooden pipe, three inches in diameter and eight feet in length, at the diſtance of two miles from London, I liſtened to the noiſes which came from that capital. I think I did not deceive myſelf by any prepoſſeſſion, when I diſtinctly heard the noiſe and agitation of wheels on the pavement much more ſtrongly than any other kind of ſound. Nearer ſounds, not in the direction of the tube, were leſs perceived; and ſuch as were loud aſſumed a muſical tone; moſt probably from the reiterated reflections under the ſeveral angles of its reception. I conſider this experiment as of little other value than as ſerving to convey an hint, that a tube lined with cloth might defend an acouſtic inſtrument from ſounds out of the line of its direction, while the inſtrument itſelf might magnify and render diſtinct the ſound required to be heard.

6. M. Perrole conjectures, at the end of the firſt part of his memoir, that ſonorous bodies

bodies do not vibrate during the propagation of found, becaufe the water in his curious experiment of the watch was not feen to move. He has overlooked the very great number and minutenefs of the vibrations required to produce found. They cannot be vifible but in cafes of extreme fimplicity and intenfity. The ftring of a mufical inftrument emits found long after its vibrations have ceafed to be vifible. He likewife, at the end of his fecond part, objects to the theory of Maupertuis, which fuppofes that fonorous and refonant bodies fubdivide themfelves fo as to vibrate differently in different parts. But the body in queftion may be confidered under different points of view. 1. As the medium of found, it may conduct or tranfmit every found indifferently. 2. If its fimplicity of figure and texture be fuch as to produce very few founds at a time, it will, upon the whole, emit a mufical tone. For I confider a mufical tone as one or more fimple founds in concord with each other; and a noife, as a greater number not poffeffing the fame relation. Thus, the noife produced by preffing down an indifcriminate number of contiguous keys in the organ, is fo far from being mufical, that it feems aftonifhing that the aggregate of pipes fo melodious fhould produce a found fo harfh. 3. Or the body in queftion may affift or impede the found of another fimple body, fuch as a ftring with which it is in contact. When it affifts that found, it does not feem improbable that the effect pointed out by Maupertuis may really take place. For the divifion of bodies, his fuppofition requires, is known to take place in the fingle ftring which gives the trumpet notes, and has on that account been called the trumpet marine; and alfo in bells, which not only give a fet of diftinct contemporaneous tones differing in acutenefs, but to a certain extent are found to alter the fyftem according to the plan of percuffion. And again, it will readily be imagined by thofe who take fo much pains in experiments for fixing the found poft of the violin, that the refonance which is fo much affected by this difpofition, is more probably of the whole inftrument than merely of its parts as conductors of found.

7. The figure of the external ear, which is made up of a feries of concavities with ftops or bridges interpofed, is an object which, as far as I know, has never yet been explained or enquired into. In the cat and other animals this ftructure appears to be very complicated. After the found or aërial undulation has been modified and conveyed into the ear through this apparatus, it is received upon the ftretched membrane called the tympanum. This, as well as the other membranous parts of the internal ear, feems evidently adapted to vibrate by the action of found; and moft probably after the manner that a mufical ftring vibrates when another ftring is made to found in its vicinity, and emits a tone in concord not too remote from unifon. From this fact and the formation of the ear, a queftion may be propofed of fome confequence with regard to the conftruction of acouftic inftruments: Whether the found produced by a remote body or that which is emitted by a correfpondent vibrating body, at a lefs diftance from the organ, be the moft perceptible? For example, if two ftrings, A and B, be tuned in unifon, and the ftring A be ten feet from the ear, while the ftring B is placed at the diftance, for example, of one inch from the fame organ;—whether the fecondary found of B, when A is ftruck, may not be more perceptible than the original found of A? It is unneceffary to enter into confiderations of the mechanical circumftances under which, from theory, this or the contrary effect may take place. It may be fufficient in this very remote view of the fubject to remark, that in point of fact the influence of founds may extend further than the organ can perceive them. I was once in a room facing the

the ftreet, in London, when I perceived the window to be agitated with certain tremulous motions, attended with a confiderable found. The reiterations were fhort and diftinct, three or four at a time; and then ceafed till a fecond and a third repetition of the fame effect took place. This remarkable procefs continued to increafe in intenfity of found, and engaged my attention for about a quarter of an hour; after which the nearer approach of one of thofe inftruments called a tambourin, compofed of parchment ftretched on a hoop, and played upon by rubbing the extremity of the finger upon its furface, evinced that the agitation and found emitted by the window had been caufed by the vibrations of that inftrument. Hence it fhould appear, that the judicious conftruction of an inftrument for receiving and magnifying founds would not only require a fcientific arrangement of an external part to exclude foreign founds, and reflecting furfaces to modify and augment the direct undulations; but that the laft effect fhould be received on a tympanum capable of adjuftment in its tenfion, and thence conveyed by a proper veftibule to the organ of perception itfelf. If the fcience of receiving and augmenting founds were once improved to the degree here fketched out, there would probably be no difficulty in magnifying founds intended to be conveyed from one place to another.

VI.

Concerning the Steam-Engine as originally invented by the Marquis of WORCESTER, *and the Improvements fince made in Steam-Engines without the Pifton or Lever. With a Defcription of an Engine of this Kind conftructed by Mr.* PETER KIER, *of Kentifh Town.*

THE Marquis of Worcefter is the undoubted inventor of the fteam-engine, which is defcribed in his Century of Inventions. From the title, he appears to have conftructed one before the year 1655. His words, No. 68, are as follow:

"An admirable and moft forcible way to drive up water by fire, not by drawing or fucking it upwards; for that muft be, as the philofopher calleth it, intra fphæram activitatis, which is but at fuch a diftance. But this way hath no bounder, if the veffels be ftrong enough; for I have taken a piece of a whole cannon whereof the end was burft, and filled it three quarters full of water, ftopping and fcrewing up the broken end, as alfo the touch-hole; and making a conftant fire under it, within 24 hours it burft and made a great crack; fo that, having a way to make my veffels fo that they are ftrengthened by the force within them, and the one to fill after the other, I have feen the water run like a conftant fountain ftream forty foot high. One veffel of water rarefied by fire driveth up forty of cold water. And a man that tends the work is but to turn two cocks, that, one veffel of water being confumed, another begins to force and re-fill with cold water, and fo fucceffively; the fire being tended and kept conftant; which the felf-fame perfon may likewifely abundantly perform in the interim between the neceffity of turning the faid cocks."

Plate XVII. Fig. 1, reprefents the fteam-engine which was made by Captain Savery, and generally fuppofed to be the fame as the Marquis of Worcefter's. A reprefents a boiler containing water, the fteam of which may be tranfmitted into either of the veffels B and C by means of the cocks D E. The veffels have a communication at bottom with the vertical

tical pipe F G. These communications are made between two valves H I and K L opening upwards; and the effect is as follows: Imagine the upper part of the pipe to be filled with water for the purpose of rendering the valves tight, and let the cock D be turned. The steam from the boiler, being lighter than the air included in the vessel B, expels that air, which, not being permitted to pass through the valve H, issues upwards through I. In this situation, let the cock D be shut, and cold water be thrown upon B, or, which is much better, spouted into it; and the steam will become condensed, and leave an empty space into which the pressure of the atmosphere at F will force the water upwards through the valve H. While this is performing, the cock E is to be opened, and the air from the vessel C expelled by the steam in the same manner. This is the regular series of operations, and is repeated a second time by shutting the cock E, cooling the vessel C, and opening the cock D. By these means the water rises from F into the vessel C, while the steam from the boiler presses the water out of B, through the valve I, to G. The whole process therefore, simplified by attending to one of the vessels only, is found to consist in expelling the air by steam; forming a vacuum by condensation into which the water rises, and then forcing that water upwards by new steam, which, being condensed as before, is replaced by new water; and so forth.

To what practical extent the experiments of the Marquis of Worcester were carried, does not appear; but it was near the end of the seventeenth century, when Captain Savery proposed this engine as an invention of his own, for raising water and draining mines. Prony, in his Architecture Hydraulique, i. 564, says, he published his *Miner's Friend* about 1699. Desaguliers, in his Course of Experimental Philosophy, 3d edition, ii. 465, does not scruple to affirm, that Savery had this invention from the Marquis of Worcester's book; but has not, it must be confessed, given to his assertion all the proof it might require. He says, that Savery bought up all the copies of the Century of Inventions which he could procure, and burned them; and that he used to relate a story as the first hint of his invention: that having drank a flask of Florence at a tavern, and thrown the empty flask upon the fire, he called for a bason of water to wash his hands; and perceiving the little wine left in the flask had filled it with steam, he took it by the neck and plunged its mouth in the water, which was immediately driven up by the pressure of the air. On this incident, Dr. D. remarks, that it could not have so happened; because when he himself had repeated the experiment by boiling half a glass of wine in a flask, and, putting on a thick glove to defend his hand, had plunged its mouth beneath the surface of water, the pressure of the atmosphere was so strong as to beat the flask out of his hand against the cieling—a circumstance with which he does not doubt but that the Captain would have embellished his tale if he had made the experiment.

It appears to me, nevertheless, that somewhat more than these incidents ought to be required to establish the charge of deception against Savery, who, in the course of probabilities, might have invented the steam-engine half a century after the Marquis of Worcester, whose description, clear as it now seems to us who know the engine, was not perhaps enough so as to attract much attention, in company with one hundred other enigmatical proposals. Captain Savery being a patentee, and having probably a considerable interest depending, might be as strongly urged to suppress the Marquis of Worcester's book on that account, as for the reason asserted by Desaguliers. And with regard to the experiment of the flask, it is hardly allowable

allowable for a philofopher to conclude that the experiments of another are falfe or impoffible, becaufe they do not agree with his own. I fuppofe Defaguliers made his experiments with more wine and more heat than Savery; and if I were difpofed to reafon againft the relation of a pofitive fact, I fhould be inclined to doubt the projection of the flafk againft the ceiling by an exertion of atmofpherical preffure, which muft have been as ftrong on the outer as on the inner furface of the veffel. And in this doubt I fhould be ftill more fettled, from having myfelf very often repeated the experiment without any fuch refult. But the integrity of Defaguliers forbids any fuch infinuation. Under certain circumftances the flafk might have been beaten out of his hand; but it does not follow that thofe circumftances were prefent in Captain Savery's experiment.

The Marquis of Worcefter was the firft inventor of the fteam-engine. Savery was either the fecond inventor, or he had the ingenuity to difcern a valuable invention in the midft of loofe hints, and to give it organization and effect.

We do not know how the Marquis of Worcefter condenfed his fteam, or, indeed, whether he condenfed it at all; for it certainly does not require condenfation unlefs for what is called fucking. It is faid, that the condenfation in the firft engines was effected by water on the outfide of the veffel; but in Savery's engines it was performed within the veffel. Several of his engines were made with only one fteam-veffel, and Defaguliers found by experiment, that this kind is confiderably better than fuch as have two; becaufe the action of the fteam is rendered more fudden, and the chemical condenfation during the procefs of forcing is lefs. This excellent philofopher and practical engineer made feveral other improvements in this engine and its parts, which may be feen in his work laft quoted: but the fubfequent invention of the fteam-engine with a pifton and lever, and the improvements which have been made therein, feem to have greatly retarded the progrefs of the original fimple machine.

Dr. Papin, well known for his invention of the digeftor, was bufied in experiments on fteam. Prony, in his Architecture Hydraulique, i. 566, mentions a work of his, printed at Caffel in 1707, under the title of *Nouvelle Maniere d'élever l'Eau par la Force de Feu*, in which a fteam-engine is defcribed which differs from that of Savery, but may equally accord with the defcription of the Marquis of Worcefter. From the engraving, fig. 268, in his fecond volume, it appears to have confifted of a fpheroidal boiler, a cylindrical fteam-veffel into which was fitted a float or pifton, and an air-veffel which received the water from each ftroke previous to its being forced by re-action to a greater height. The water flowed through a pipe with a valve opening downwards, whence it paffed beneath the pifton which floated upon it. The defcent of the pifton was effected by the fteam, and its afcent by the action of the water from the original ftock; at which period the fteam was fuffered to efcape into the air by means of a cock, and the communication between the boiler was fhut off.

In thefe engines it may readily be apprehended that the action of the direct fteam on any definite furface, fuch, for example, as a fquare inch, will be accurately equal to the re-action of the water which is forced up; and confequently, that Savery's engine will require fteam more elaftic than the air of the atmofphere, in every cafe except that wherein the water is raifed by fuction, and afterwards fuffered to flow out of the bottom of the veffel into a channel or ciftern. But if this action of fteam be, by the intervention of fome mechanifm, tranfmitted to the place where it is intended it fhall operate, it will be poffible to regulate the proportions in any defired manner. It was very early in the prefent century

 that

that Newcomen and Cawley began their trials of improvement of the ſteam-engine with a cylindrical ſteam-veſſel and piſton depreſſed by the weight of the atmoſphere, by which means they obtained a ſafe application of power for raiſing water to heights far beyond any in which the old engine could be truſted, on account of the extreme ſtrength required in the boiler and ſteam apparatus. This event is probably the cauſe why ſo little has ſince been done to remedy that leading imperfection, or to adapt the original machine to work itſelf without an attendant. It is certain, however, that the weight of the apparatus, and frictions of the parts, in all ſteam engines with a piſton and lever, have prevented their being extenſively uſeful in ſmall undertakings, ſuch as the blowing of ſmall furnace-bellows, the drawing of boats and carriages, with numberleſs other operations where forces not exceeding one, two, or three horſes are wanted. In theſe operations it is highly probable that the engine of Savery would be very uſeful, if it were made to work without an immediate attendant; for Deſaguliers found the advantage greatly in favour of Savery's engine, on a ſmall ſcale, compared with Newcomen's lever-engine. He had an engine of the former conſtruction in his garden, which raiſed ten tons of water an hour, about thirty-eight feet high; and a friend of his erected a working model of the lever-engine on the ſame ſpot. The boiler of this laſt was exactly of the ſame ſize as that of Deſaguliers, and his cylinder was ſix inches bore and about two feet in length. It raiſed four tons per hour into the ſame ciſtern. It coſt three hundred pounds; but the engine on Savery's conſtruction, having all copper pipes, coſt but eighty pounds.

It muſt not be overlooked, however, that this account affords no ſtatement of the quantity of fuel conſumed under each boiler. It might perhaps have been the caſe, that Savery's boiler could have fed a larger cylinder. But at all events there is no doubt of the fact, that the beſt lever-engines cannot be advantageouſly uſed when ſmaller than a determinate ſize; and that, on account of the charge of attendance to open and ſhut the cocks, no trials have been made to ſhew upon how ſmall a ſcale Savery's engine might be rendered uſeful.

An engine upon Savery's principle, with various judicious improvements, was erected four years ago by Mr. Kier, at his manufactory of coach axle-trees near Pancras, where it has almoſt conſtantly been worked without repair. It is perhaps on too large a ſcale to afford much information reſpecting this queſtion; but is certainly of conſiderable value, not only becauſe it works without an attendant, and regulates its own motions, but alſo becauſe, as might naturally be expected, the wear and tear is altogether inconſiderable. He has permitted me to give an account of this engine.

The ſketch Fig. 2. Plate XVII. repreſents this engine, without extreme pretenſions to accuracy, but upon a ſcale of a quarter of an inch to a foot. R repreſents an oval boiler ſeven feet long, five inches wide, and five deep. The proprietor conſiders it as being of dimenſions ſufficient to work a larger engine; a circumſtance which muſt, in a certain degree, diminiſh the effects of the preſent. It feeds itſelf in the uſual manner, with water conveyed through a pipe at the end of which is a valve. This valve does not open until the fall of the water within the boiler has ſuffered a float to ſubſide, which by its actual weight aſſiſts to draw it open, but by its tendency upwards, as the water in the boiler riſes, ſerves effectually to cloſe it. The boiler, therefore, remains conſtantly at or near the ſame degree of fullneſs. The ſteam is conveyed by a pipe TAV to a box B, through which, by the opening and ſhutting of a valve, it can be conveyed to the working chamber E. The axis C ferves as a key to open and fhut the valve. NO is a ciftern of water, from which the fupply is made through the vertical pipe in which the valve Q is placed; and GG is another ciftern into which the water is delivered through the pipe F, which is provided with a valve H opening outwards. IM reprefents an overfhot wheel eighteen feet in diameter, moving on the axis KL, and communicating its motion to the lathes and other rotatory engines of the manufactory. The water in both the cifterns becomes warmer than the hand after working a fhort time; for which reafon the injection-water is forced up by a pump from a well fupplied by the fmall ftream on which thefe works are eftablifhed. A leaden pipe paffes from this forcing pump to the upper or conical part of the chamber E, for the purpofe of injecting cold water at the proper time. Neither of thefe could be reprefented with convenience in the prefent fection.

The manner in which the fteam and cold water are alternately admitted into the chamber E, remains to be explained. Upon the extremity K of the axis of the overfhot wheel there is fixed a folid wooden wheel about four feet in diameter, reprefented in fig. 3, as feen in front, and alfo in profile, where the fmall letters denote the fame things as are marked by the large letters of the alphabet in the front view. ABCD are four cleats, all or any number of which may be fixed on the wheel at a time. Each cleat has its correfpondent block EFGH on the oppofite furface of the wheel. The ufe of thefe is to work the engine. Suppofe the wheel IM or any part of the revolving apparatus be drawn round by hand, one of the cleats meets in its rotation with a lever which opens the fteam-valve by a bar of communication reaching to the handle of the axis C, fig. 2. The fteam confequently paffes into the chamber E, and the fteam-valve fhuts again as foon as the cleat has paffed. Speedily after this the correfpondent block on the other fide of the wheel meets another lever, which is fimilarly attached to the handle of the forcing pump, and therefore throws a jet of cold water into the chamber, and condenfes the fteam. The preffure of the atmofphere then forces the water from the ciftern NO through the valve Q towards the chamber E. When the engine has been long out of work, I fuppofe two or three ftrokes may be neceffary to raife the water to the top of the chamber. As foon as this is the cafe, the injection of the fteam fuffers the whole body of water above the valve H to overcome the preffure of the atmofphere and rufh out. The water which is raifed is fuffered to flow upon the overfhot wheel through a fluice, and by that means keeps the work in motion, and replenifhes the lower ciftern.

Hence we fee that in effect this engine is the fame as figure 1. excepting that it is not applied for the immediate purpofe of raifing water, but gives motion to other apparatus. It is therefore unneceffary to infift more largely upon the mere operation; but the peculiarities of Mr. Kier's engine are deferving of notice. In the firft place it may be obferved, that he ufes no refervoir for his injection water, but drives the requifite quantity up at each ftroke. The advantage of this is, that the pump fuftains the action of a very fhort column of water, though it apparently forces to the height of about 26 feet. For the column in the pipe and chamber PE by its re-action takes off the effect of an equal length of column which would elfe have acted againft the forcer, and thus renders the injection more eafy and quick. In the fecond place it may be obferved, that he has proceeded directly oppofite to the firft remark of the Marquis of Worcefter, who rejects drawing or fucking upwards. For this engine does not force at all. The water merely falls out of the chamber, and confequently never

never requires steam stronger than the atmosphere. From the effect of this engine under circumstances of such advantage, it may fairly be concluded that the action of steam against water in forcing can never be beneficial except at a place where fuel could be had extremely cheap. Thirdly, it was found at the first construction of this engine, that the consumption of steam by contact with the water was so great that it could not be worked with the smallest advantage. This defect was remedied in the present engine, as well as in another at Norwich, by fixing a small air-valve in the steam-box, which was struck for an instant immediately before the admission of the steam. It may be presumed that the air occupied a space above the water, and prevented their coming together. Mr. Kier, however, is disposed to think that the effect does not take place in this manner, but by some mixture and sudden dilatation of the two fluids; because he imagines the mischief from the wet cylinder would be the same upon descending steam. Much, however, may be said in defence of the opposite opinion. The air-valve is not at present used, because the engine does very well without it; which is supposed to arise from its being less air-tight than it was at first. Fourthly, the motion of the overshot wheel is regulated by an apparatus called a governor, invented, as I think, by Mr. Watt, and represented in figure 4. The bar HI revolves by communication with the engine, and carries round the balls A and B, which move on a fixed joint C. When the rotation is very quick, the balls fly out, and draw down the points DE, and consequently F, which is so constructed as to slide upon the bar or axis HI. A lever, FG, connected with the sluice of the upper cistern GG, fig. 2. is therefore made to fall or rise accordingly as the velocity is greater or less. By this disposition, when the wheel moves very speedily, from lightness of work or any other cause, the quantity of water thrown down from the upper cistern is immediately diminished; and, contrarywise, the quantity of water is rendered greater when the slowness of the movement shews that it is wanted. When I saw the engine at work, it had but one cleat and block upon its wheel, and two men were at work at strong lathes, roughing out certain pieces of iron about two inches in diameter. Whether it were the unsteadiness of this work, or the want of sufficient celerity of communication, I know not: but the variations in this governor were upon the whole more considerable than I have remarked in a lever-engine with a fly and this apparatus also. Fifthly, as the injection-water makes a constant addition to the sum of the water contained in the cisterns NO and GG, the surplus is allowed to overflow. If it were to overflow the lower cistern, it would warm the water in the injection well; for which reason a contrivance becomes necessary to prevent that effect. Mr. Kier's contrivance is this:—The water of the upper cistern, when at a certain elevation, overflows into a communication which conveys the water to the lower; for which reason its contents can never rise above a certain level. There is also a pipe from the bottom of the upper cistern, which is recurved upwards to communicate with a gutter a little lower than the place of overflowing just mentioned. It is clear, therefore, that the upper cistern would always overflow at this pipe sooner than at the place of communication with the lower cistern, if there were not a valve within the upper cistern, over the mouth of the recurved pipe. Now it is so managed by means of a float in the lower cistern, that this valve in the upper shall be opened whenever the water below rises to a certain level. And consequently, as soon as ever the lower cistern has received this portion, it can receive no more, because all the subsequent addition of water will pass away by the recurved pipe.

This

This engine confumes fix bufhels of good coals in twelve hours' work when in its beft ftate, or feven bufhels when at the worft. Under thefe circumftances it gives ten ftrokes per minute, each throwing out feven cubic feet of water, at an aperture twenty feet above the water beneath. This quantity, namely 70 cubic feet per minute, will weigh 4345 pounds, which being doubled, to reduce it to Defaguliers's ftandard height of ten feet, will amount to 8690. And this divided by 580, the number of pounds in a hogfhead, will give a quotient of 15, reprefenting fo many men, according to the eftimate of that author, which he reckons equivalent to three horfes. This refult is not more than half what is performed by the improved engines with a pifton of fuch a fize as to be equal to 20 or 25 horfes. But how far thofe engines would prove effective upon the fmall fcale of three horfes, or the ftill fmaller fcale of one or even lefs, remains to be decided.

The engine here defcribed has been at work four years, and from the fimplicity of its conftruction has yet exhibited no proofs of wear. Mr. Kier thinks it a profitable engine to himfelf, and that it would be ferviceable for raifing water where coals are cheap. A contrivance he made in the year 1783 might, perhaps, with proper modifications, be ufed to obviate the neceffity of forcing where the heights are confiderable. It is liable, however, to fome ftrong objections, and certainly requires to be maturely weighed with regard to dimenfions and effects before it fhould be attempted to be put in practice.

Fig. 5. reprefents the contrivance for forcing, as I find it in the original drawing; and I thought it unneceffary to make any alteration to adapt it to what is called fucking, which might, if required, be eafily done by any perfon flightly acquainted with the fubject. A is a boiler, and B a fteam-veffel. This laft communicates with the veffels M L K T, each of which, except the lower one, confifts of two veffels; an interior veffel, clofed on all fides excepting where it communicates with B; where pipes P, O, N, I, enter the upper part of each; and alfo where there is a valve at the bottom, opening upwards: the pipes I N O likewife communicate with the three exterior veffels K, L, M. If fteam be let pafs from A to B, it is inferred that the air will be driven from B, and prefs upon the water in the lower veffel T, which will be driven into the exterior veffel of K, but not into the interior veffel, becaufe the preffure of the air through the tube H is more than fufficient to keep the valve of the apparatus K fhut. The next ftep in the operation confifts in clofing the cock C, and opening D, out of which the re-action of the water forcing itfelf into T to its natural level, and into the interior veffel of K on the fame account, will drive a portion of fteam. D is then to be clofed, and C opened; in confequence of which, the contents of T will be forced up to K, as before; and the interior veffel K will evacuate its contents into the exterior of L. The fteam being fhut off at C, and the cock D being opened as before; the veffel T, the interior veffel K, and the interior veffel L will fill as before, and a larger portion of fteam will iffue from D. A third repetition of the procefs will drive the contents of thefe three interior veffels a ftep higher; and a fourth repetition will caufe the contents of the upper interior veffel M to flow out at P; after which every alternation of the work with the cocks C, D will throw out the fame quantity from P.

I fhall only remark on this contrivance, that the veffel B muft neceffarily contain a quantity of air capable of occupying the whole interior fpace contained in the clofed veffels T K L M, with an allowance for the lofs of bulk in condenfation under the preffure of a column of water equal to one of the lifts, and that the quantity of steam to be discharged at each stroke must occupy a space equal to that of all the water moved at each stroke, and must in all cases be considerably stronger than the atmosphere.

VII.

On the Mechanism by which the Mariner's Compass is suspended.

If a bar of iron be rendered magnetical, by rubbing it on a natural magnet, or by any of the well-known processes for that purpose; it acquires the property of disposing itself nearly in a north and south line, whenever it is suspended so much at liberty that the energy of this power is sufficient to overcome the friction or other impediments to its motion. The mariner's compass is an apparatus in which a bar of this kind, called the needle, is supported, for the highly useful purpose of determining the position of the meridian at sea, and consequently of enabling ships to steer their course by day or night, without observation of the stars, or any other external objects, as was necessary before the discovery of this instrument. In a well-constructed mariner's compass the needle is defended from the impulse of the air, and is little subject to be disturbed by the external motions or agitation of a ship at sea. As this disturbance is, however, the chief impediment to the convenient use of the compass in a boat, where the motions are sudden and short, or in a ship, when the waves are very turbulent; and as the artists in this branch have endeavoured to persuade the world that certain pieces of mechanism were much superior in their use to others differently disposed;—I thought it might be of some utility to explain the simple principles of a good suspension.

When the needle of the compass disposes itself in the magnetical meridian, there is a certain line within the piece of steel, which joins its two poles, and may be considered effectively as the needle itself. But as this line is not visible, the admeasurements of position must be made with regard to some marks on the extremity of the needle; which marks will be truly placed when the needle is found to occupy the same position with respect to a fixed point, upon being reversed, so that the lower side shall become the upper. If the magnetical power had been found on experience to occupy the same parts of the needle with proportional intensity, during its decay, to that it possessed immediately after the touch, these marks, once made, would continue to shew the true magnetical points as long as the needle possessed any directive force. But it is well known that soft steel loses its magnetism sooner than hard; and consequently it may be inferred that, unless both sides of a needle were equally hard, the magnetic power would deviate in process of time towards the harder side. These considerations lead to an obvious method of diminishing such growing error. It consists in making the needle flat and thin, and suspending it with its edge, and not its flat side, uppermost, as is more commonly done.

The needle is usually supported on a steel point, which occupies the axis of a cylindrical box called the compass box. For this purpose there is formed in the needle itself a cap or hollow conical centre of brass, steel, or hard stone, which is applied over the point. The tendency of the needle to be disturbed by agitation will greatly depend upon the position of the

the vertex of the conical cavity. It is neceffary that it fhould be above the centre of gravity; but this diftance muft be fo fmall as that the libration of the needle when one end is depreffed fhall be very flow, and yet fpeedy enough to recover the horizontal pofition in a reafonably fhort time. In fact, the whole of the fteadinefs of the compafs and its box appears to depend on this principle of flow vibration. For, if a needle perform its vertical vibration in eight feconds, it will be very little difturbed by an alternate action that lafts but a fecond or two.

The greater number of workmen have imagined that the agitation of the compafs is communicated by friction at the points or edges of fufpenfion, and have accordingly exerted their ingenuity to diminifh this friction, by contrivances fimilar to that of a conical cap balanced on a point, and itfelf affording another point to fupport the needle. But it is very readily proved by experiment, as well as argument, that the greateft difturbance of the needle is produced by the quantity of horizontal progreffive motion, and not by the mere inclination or angular motion. A compafs-needle fupported on a fimple point will fuffer very little agitation from any angular motion or moderate deviation from perpendicularity in the pin; but it will inftantly begin to vibrate if moved horizontally. Thus the common experiment of tilting the compafs-box in all pofitions while its centre remains immoveable is fallacious, and there are very few compaffes indeed which will bear to be flided backwards and forwards upon a table.

It appears therefore that the fteadinefs of a needle which vibrates flowly, is the confequence not only of the length of time it allows for alternate actions to operate, and deftroy each other; but alfo of the difficulty with which it yields to fuch impreffions. If the centres of fufpenfion and of gravity in the needle were coincident, no angular motion would be produced by any action of the pin, excepting by the effects of friction; and the angular motion produced in other cafes will be lefs, the fhorter the diftance between thefe two centres, or the lever by which it is propagated.

The fimple fufpenfion of the needle on a point has been applied to the compafs-box, for which it is little fuited, not only becaufe of the wear upon fo fmall a furface, but alfo becaufe it admits the box to traverfe horizontally; an effect which is inconvenient, and cannot be remedied by any means not calculated in fome refpect to increafe the effects of agitation. The method moft generally received, and in fact the beft adapted to this inftrument, are the gimbals.

This well-known contrivance confifts of an hoop fupported upon two pins diametrically oppofite each other, and iffuing from the external furface of the ring in fuch a direction that both lie in the fame diametrical line. When the hoop is fufpended on thefe pins, it is at liberty to turn freely round the diameter of which they conftitute the prolongation. The notches or holes of fupport are difpofed horizontally. The compafs-box itfelf is placed in a fimilar ring with two projecting pivots; and thefe pivots are inferted in holes made in the former ring at an equal diftance from each of its pivots. If, therefore, we fuppofe the whole to be left at liberty, the compafs-box may vibrate upon the diametral line of the outer ring, and alfo upon a line formed by its own pivots, at right angles to that diametral line. The confequence of this arrangement is, that the centre of gravity of the compafs-box will difpofe itfelf immediately beneath the interfection of both lines on which it is at liberty to move :—that is to fay, if the weight of the box or its parts be properly difpofed, the compafs will affume a pofition in which its upper furface fhall be horizontal.

The fame principles which were applied to the fingle centre of the magnetic needle will alfo apply to the axis of the gimbals. If the centre of gravity of the compafs-box be fo placed with refpect to either axis as that its vibrations fhall be quick, every horizontal action will greatly difturb it, and it will not fpeedily fettle. The moft favourable pofition of the pivots or edges of fupport in the gimbals will be when they all lie in the fame plane, and the centre of gravity of the compafs-box is very little below that plane.

The practical application of thefe inferences appears to be, that, without pretending, as has been done, that any peculiar fecret or great difcovery is required to give ftability to this ufeful inftrument, nothing more is required than good workmanfhip, and a proper adjuftment of the weight with regard to the centres or axes of fufpenfion. The needle ought to be adjufted either by means of its cap, or by proper filing away, or elfe by additional pieces to the card, fo that it fhall vibrate very little, and that flowly, when placed upon a point and moved horizontally, whether in the direction of the needle or at right angles to that direction. The card is then ready for the compafs-box. The box itfelf muft be adjufted with the card in its place, fo that it fhall exhibit the fame fteadinefs when moved in the line of direction of the outer pivots. And laftly, the fame difpofition muft be made with regard to the motion in the direction of the inner pivots. It is fcarcely neceffary to add, that the means of this adjuftment confift in fhifting the pivots themfelves, or, which is much better, in altering the difpofition of weight about the compafs-box. An external ring of metal, encircling the box, and raifed or lowered until the proper place for fixing it is found, would perhaps afford the moft convenient method.

Upon the whole, the reader will perceive, that the leading aim of the prefent paper is to enforce the truth, that the compafs is very little difturbed, at fea or elfewhere, by tilting the box on one fide, but very much by fudden horizontal changes of place; and, confequently, that a fcientific provifion againft the latter is the chief requifite in a well-made inftrument of this kind. And again, that nothing is more eafy than to afcertain whether a compafs poffeffes ftability; fince nothing more is requifite than to flide it upon a table in the feveral directions above-mentioned, and remark how far it is difturbed. The good workmanfhip of the cap and pin of the needle may be afcertained by infpection with a magnifier, and alfo by drawing the card with a fmall key or other piece of iron, a very little quantity, for example a quarter of a degree, out of its ftation or pofition, and remarking whether it returns accurately to its original ftation.

Before I quit this fubject, I muft take notice, that, as the fufpenfion on a point has been applied to the compafs-box, fo, on the other hand, the gimbals have been applied to the needle. This was done by the late Dr. John Lorimer, who difpofed a dipping needle on its own axis between the cheeks of a frame parallel to the diameter of a circle of brafs graduated to fhew the angle of dip. This circle or meridian had pivots at its zenith and nadir, which moved in holes diametrically perforated in another circle. The needle was by this means not only permitted to difpofe itfelf in a line forming the angle of dip with the horizon, but was enabled to carry the meridional circle into the line of the magnetic meridian. It does not appear from the Doctor's paper and drawing in the 65th volume of the Philofophical

phical Tranfactions, that he had added the fimple expedient of an horizontal gimbal-hoop to the crofs circle which fupported the meridional pivots. Such a circle, divided, inftead of the fixed horizon in his figure, would have rendered it a complete inftrument for general purpofes. It is probable however, that for ordinary naval ufe the fimpler compaffes of the prefent conftruction would be preferable.

VIII.

On the Maintaining Power in Clocks and Watches.

IN a former paper on the compenfations for change of temperature in pendulums, a flight view was taken of the methods by which a train of wheels might be made to move either uniformly or by equal intervals of progreffion, fo as to afford a meafure for the lapfe of time. The pendulum and the balance are at prefent the regulators which for juft reafons are preferred to every other contrivance. If we fuppofe a body to be fufpended at the extremity of a ftring, its gravity, as is well known, will caufe the ftring to point towards the centre of the earth's gravitation at that part of the furface. If the body be then drawn afide out of this perpendicular pofition, it is equally clear that it will be removed further from the earth's centre, towards which, when again fet at liberty, it will fall through the arc of a circle, which is the only line the ftring will permit it to defcribe. At the inftant the perpendicular pofition is thus regained, the body will not be at reft, but will poffefs a portion of velocity in a direction at right angles to that of gravity. It will not, therefore, be in the power of gravitation to affect that velocity; and confequently the body will proceed onwards on the oppofite fide of the perpendicular, in a fecond circular arc, which elementary writers demonftrate to be in theory perfectly fimilar, and equal to the arc of defcent:—that is to fay, the action of gravity will more and more oppofe the afcending motion, as its direction becomes more remote from the horizontal line, and will at length not only deftroy the motion of afcent, but again generate a falling motion precifely the fame as the firft. Such a pendulum would, therefore, if no other circumftances prefented themfelves than have yet been ftated, vibrate for ever; and its vibrations would be performed in times precifely equal. But the truth is, that fuch a body muft in actual experiment give motion to the air through which it paffes; and muft alfo overcome a certain degree of rigidity to which the ftring, or any other fubftitute in nature which might be propofed, is fubject. On thefe accounts, the quantity of motion in the pendulous body will be continually diminifhed, and at length become infenfible. It may alfo be eafily apprehended, that a pendulum, though conftructed with fuch delicacy as to move for many hours before it comes to apparent reft, could not be of much practical utility without fome ready means to keep an account of its vibrations. For both reafons, therefore, a pendulum without any other addition can fcarcely be ufed as a time-keeper.

A train of wheels, urged by a weight or fpring, is therefore attached to the pendulum for thefe two purpofes, namely, to maintain its motion by a fmall impulfe given at each vibration, or at moft at each fecond vibration, and to keep account of the number of times. The principle of the balance is, that its fimilar vibrations are performed in equal times, like thofe of the pendulum. The methods of adapting the train of wheels to one of thefe regulating organs will not therefore effentially differ from thofe of connecting it with the other.

other. In most of these contrivances, the pendulum or balance is, during some part of its motion, brought in the way of one of the teeth of the last wheel of the train, at the time when the whole system of wheels are at liberty to move. The tooth, therefore, strikes either the pendulum, or a part of the movement connected with it, and then slips off, or escapes from that obstacle. The whole set of parts in a time-piece by which this alternate action is effected, is for that reason called the escapement.

[*To be continued.*]

MATHEMATICAL AND PHILOSOPHICAL CORRESPONDENCE.

Answer to the Philosophical Question, p. 284.

To Mr. Nicholson.

SIR,

FROM the description of the divergent rays seen in agitated water surrounding the shadow of the observer's head, which forms part of the question in your sixth number, I did not fail to look for the appearance on the first opportunity of sunshine which offered. I observed the fact as you describe it, and found that the radiations appear without being modified in any respect by the distance of the observer from the surface of the water. This circumstance indicated a probability, that the cause which I could not investigate by direct reasoning, might be developed by a nearer inspection than could with convenience be made on the bank of a river. With this view I placed a vessel of water, which was three feet deep, in an out-house, in such a manner that the sun shone upon its surface. When I placed myself so that the shadow of my head fell upon the water, I perceived no radiations whatever; but upon agitating the water with my hand, the water appeared full of the lines called sun-beams, parallel to each other no doubt, but which, on account of the smaller optical distance between their remoter than between their nearer extremities, had the appearance of convergence towards the shadow. As it is perfectly intelligible from the well-known truth in perspective, that all lines parallel to the line of sight will vanish in the point of sight; it remains only to be shewn how these distinct parallel beams are produced.

When the surface of the water is at rest, and forms one plane, the sun's light forms one uniform mass in which no lines are distinguishable; but when the agitation takes place, a number of convexities and concavities are produced. These scatter the rays in all directions, except at that small portion where the contrary curvatures join. The same physical effect is produced as from so many small planes; that is to say, the rays pass straight through each surface, and afford the parallel distinct beams of light. And though these small planes are continually forming and disappearing, yet there are always a certain number in existence, and the effect of their transient individual variations is nothing more than to produce an undulation in the general system of radiations.

And now, Sir, after having resolved your question, as I think, satisfactorily; I shall take the liberty to propose another in the same department of science.

When the sun shines, if two bodies, for example hands or fingers, be gradually moved, so that their shadows may meet; it is observed that the shadow of the body farthest from the

the sun will swell out, and join the other shadow when the distance becomes diminished to a certain quantity, while the shadow of the body nearest the sun undergoes no alteration. What is the nature of the inflection of light which occasions this appearance?

I am, Sir, yours, &c.

A. Y.

*** The fact mentioned in the last paragraph of this letter has been stated and explained elsewhere, though I cannot now recollect where I have seen it. It does not depend upon inflection, but upon the different widths of the penumbræ. When the penumbræ first interfere, a faint lenticular shade is produced, which is nearer the shadow of the most remote body than that of the other; and as soon as the remoter body itself comes in contact with the bounding ray of the greater penumbra, it begins totally to intercept the sun's light on the side next its own shadow. The time employed in passing from one extreme ray of the penumbra to the other will be less the nearer the bodies are to each other, because the distance of those rays is less the nearer the vertex. The shadow of the remoter body will consequently run over the penumbra with greater speed than that of its former progress; and if any part be convex, it will advance beyond the others.

To Mr. NICHOLSON.

SIR,

HAVING observed in the 4th Number of your Journal *, and likewise in the Annales de Chimie, an extract of Mr. Prevost's Memoir on the means of rendering visible the emanations of odorant bodies; I take the liberty of communicating an observation which occurred to me a few years ago, while making experiments with different substances exposed to the oxigenated muriatic acid-gas. That which afforded the curious phenomenon to which I allude, was some highly-rectified animal oil. This matter, immediately on being exposed to the gas (which was in a very dry state), was seen to emit a copious steam, the particles of which rising to the height of about four inches above the small phial in which the oil was contained, were then observed gradually to descend, forming a very curious and pleasing appearance.—Although struck at the time with the singularity of this phenomenon, and endeavouring to account for the formation of the vapour by a supposed union of the hydrogen of the emanating matter with the super-abundant oxigen of the gas; yet I omitted at the time, and have since neglected, to endeavour to verify my conjectures by further trials.—The circumstance, however, revived in my recollection on reading your Journal; and conceiving it to be an experiment that affords a nearer approach to the absolute *perceptibility* of an odorant vapour, than those stated in the Memoir of Mr. Prevost, I impart it for your further investigation, if thought worthy your attention, and am

Your obliged reader,

Nov. 24, 1797. W. HOWARD.

N. B. The oil was the only odorant substance tried: perhaps many others might afford a similar or varied appearance.

*** The philosophical consideration of odorant bodies is somewhat obscured by the old method of generalising, or referring the properties of bodies to some distinct principle or

* Page 153. See also on this subject, p. 205. N.

thing

thing ſuppoſed capable of being ſeparated from the body itſelf. Thus the odors of bodies have been ſuppoſed to depend on a ſubſtance imagined in a looſe way to be common to them all and ſeparable from them. Hence the terms, principle of ſmell, ſpiritus rector; and even in the modern nomenclature we find *aroma*. There does not in effect ſeem to be any more reaſon to infer the exiſtence of a common principle of ſmell than of taſte. The ſmell of ammoniac is the action of that gas upon the organ of ſenſe; and this odorant inviſible matter is exhibited to the ſight when combined with an acid gas. But in the ſame manner as ammoniac emanates from water and leaves moſt part of that fluid behind, ſo will the volatile parts of bodies be moſt eminently productive of this action; and very few, if any, natural bodies will be found which riſe totally. The moſt ſtriking circumſtance in the effect is, that an act of ſuch power ſhould be attended with a loſs by exhalation which is ſcarcely to be appreciated by weight, or in any other method during a ſhort interval of time. But we know ſo little of nervous action, and of other phenomena of electricity, of galvaniſm, or even of heat, which ſtrongly affect the ſenſes but elude admeaſurement by gravitation, that the difficulty of weighing the effluvia of odorant bodies becomes leſs aſtoniſhing.

QUESTION XI. *Anſwered by J. F—:—:—:—R.*

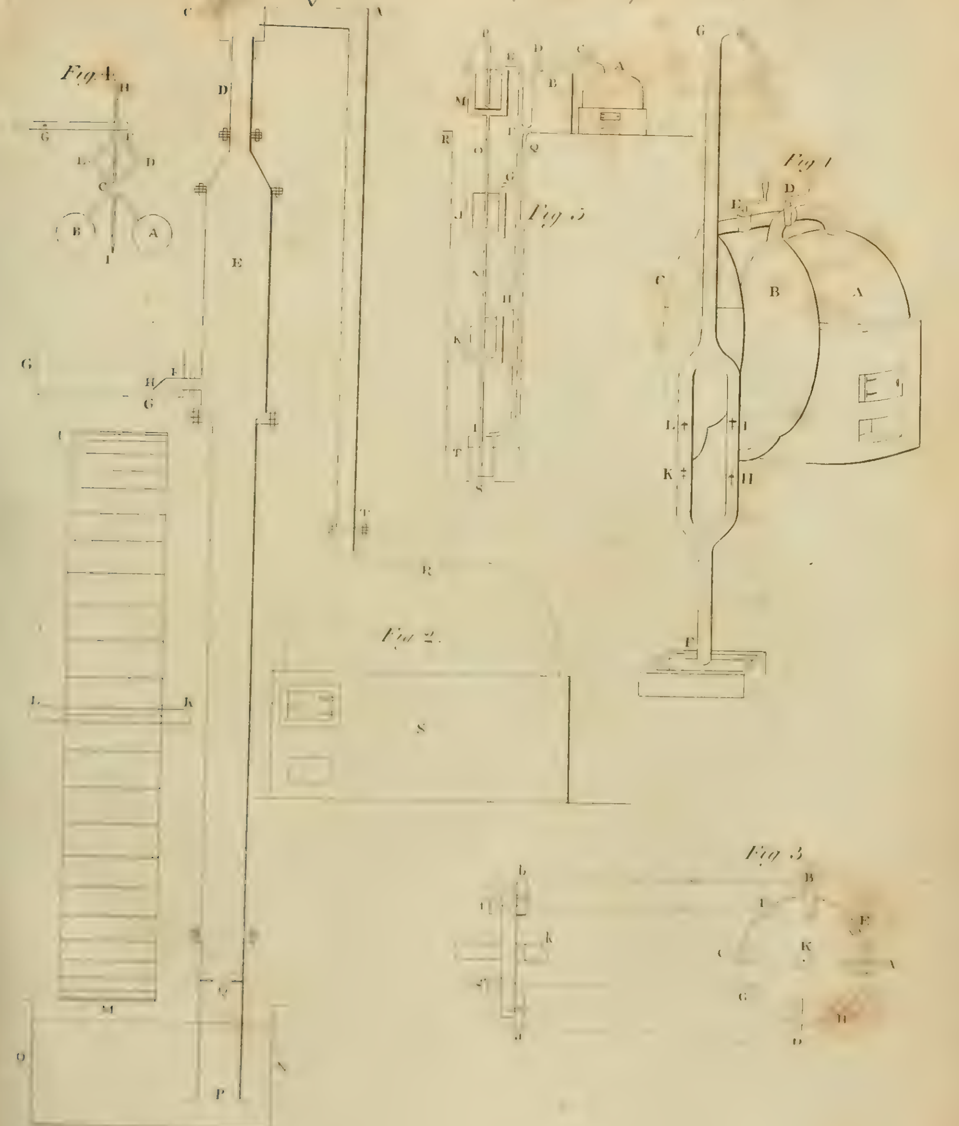
LET H h be the altitudes of the two ſignals or other objects, A their direct angular diſtance, and a their difference of azimuth, as ſtated in the queſtion. It is proved by the writers on ſpherics, that, in any ſpherical triangle, Coſ. any angle : radius : : radius × coſ. oppoſite ſide — rectangle coſines including ſides : rectangle ſines including ſides; which analogy, applied to the triangle formed by the zenith-diſtances and direct angular diſtance of the elevated objects, gives, Coſ. a : radius : : radius × coſ. A — ſine H × ſine h : coſ. H × coſ. h; ſo that coſ. A × radius ^{2} = coſ. a × coſ. H × coſ. h + ſine H × ſine h; or, putting radius = unity, Coſ. a = coſ. a × coſ. H × coſ. h + ſine H × ſine h. Q. E. D.

QUESTION XII. *Anſwered by Mr. WILLIAM CASTILAU, of Uffington, Salop.*

LET Z P ⊙ repreſent the zenith, pole, and ſun reſpectively; x the coſ. of ∠ PZ ⊙: then, per data and a well-known theorem in ſpherics, SZ ⊙ × S PZ × coſ. ∠ ⊙ ZP + coſ. Z ⊙ × coſ. PZ = coſ. P ⊙ (rad being 1); that is (putting d for ſine of the ſun's dec. 15° 9′ or .26135) $x \times x \times x + \overline{1 - xx} = d$: this equation ordered is $x^3 - x^2 = d - 1 = -.73865$, whence $x = -.66588$ = coſ. 131° 45′ ∴ 48° 15′ will be the azim. when equal to the lat. and alt. on the given day. The ∠ ZP ⊙ alſo = $2^h\ 3'\ 54''$. Per Naut. Alm. the ſun's declination for noon, on the given day (at Greenwich), is 15° 18′ 28″, and its change for 24^{hrs} = 18′ 7″ ∴. As 18′ 7″ : 24^h : : $\overline{15°\ 18'\ 28''} - 15°\ 9'$: $12^h\ 32'\ 27''$ the time from noon at Greenwich when the obſervation was made. The latitude of the place of obſervation, then, is 48° 15′ N. and its longitude $(\overline{12^h\ 32'\ 27''} - \overline{2^h\ 3'\ 54''})$ $10^{hrs}\ 28'\ 33''$ E. of Greenwich.

☞ The ſame was anſwered by *J. F—:—:—:—R.*

The original Steam engine; with Improvements.



A

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JANUARY 1798.

ARTICLE I.

Experiments made with a View to ascertain the Cause of Buildings, which have Metallic Conductors belonging to them, being struck by Lightning. By Lieutenant-Colonel HALDANE.

IT is not necessary, in the present communication, to enquire what may be the nature of electricity; whether it exist in the form of one fluid only, and all its phenomena depend upon the presence of a greater or less quantity than naturally belongs to all bodies; whether its effects may depend upon the influence of opposite polarities, and the operations of attractive and repellent powers in a single fluid; or, lastly, whether it exists in the form of two different, though always co-existing, fluids, produced by the union or the separation of certain gases, which belong to electric and non-electric substances, according to their various elective attractions, and which may be brought into action by the attrition of those substances, either in the form of heat or of electricity, according to their different combinations.

These, it must be admitted, are points of curiosity and importance; but at present it is only necessary to admit, what was clearly ascertained by Dr. Franklin, and since confirmed by the observations of many other ingenious and able philosophers, that there is a sameness between the matter of lightning and the electrical fluid. And electrical experiments, although the results of them cannot be compared with the effects produced in the vast and extensive operations of nature; yet, by exhibiting to our view similar appearances, they may furnish us with notions of, at least, the proximate, though we may for ever remain ignorant of the more remote, causes which produce those most wonderful, and sometimes most dreadful, effects.

Electricity, attached to the same kind of substances, shews itself in two different forms, which

which have been diftinguifhed by the terms *pofitive* and *negative* electricity. Thefe algebraical terms are well fuited to defcribe either two different fluids, or two different ftates of the fame fluid; for it is well known by experiment, that electricity, under thefe different forms, has the property (like pofitive and negative quantities in algebraical computations) of deftroying the effects of each other, when united in equal quantities.

The enquiries hitherto made upon the fubject of this paper, have in general been confined to what relates to the operation of one fluid only; but in thefe experiments the joint operations of the pofitive and negative electricities are confidered, to which probably many appearances, that have hitherto occafioned much controverfy amongft electricians refpecting the forms of conductors, may be afcribed.

It is a known fact in electricity, that if the furfaces of two metallic bodies be placed oppofite and near to each other, the one infulated, and the other connected with the earth; if a thin plate of any electric fubftance be introduced between thefe furfaces, and extending beyond their extremities, if electricity be then communicated to the electric by means of the infulated metallic fubftance, the other metallic fubftance connected with the earth has the power of putting the fide of the electric with which it is in contact into the oppofite ftate of electricity; and the pofitive and negative electricities on the oppofite furfaces of the electric, by their attractive powers, retain each other in their refpective fituations. And, in folid electrics, even after the removal of the metallic fubftances, if the fame, or other conducting fubftances, having a conducting communication between them, be applied again to the oppofite fides of the charged electric, all electricity will difappear, filently, or with an explofion. Thefe are the phenomena of the electric jar.

It is alfo well known, that if a body of atmofpheric air, included between metallic furfaces, be the electric employed in the experiment above defcribed, fimilar effects will be produced.

Hence it may be inferred, that the effects of lightning may arife from the difcharge of large bodies of electrified air; the upper furface of which will be in one ftate of electricity, and the furface adjoining the earth and terreftrial bodies will be in the oppofite ftate; and when thefe bodies of charged air pafs over lofty buildings capable of forming a conducting communication between their oppofite furfaces, the explofion of lightning takes place.

The lower furface of a body of charged air may extend over the whole, over a part, or over no part, of a building, at the time it is ftruck by lightning; the building forming only a part of the conducting communication.

Buildings in general have many metallic fubftances belonging to them: it is to thefe fubftances, and to their imperfect connection with the earth, that moft of the mifchief that arifes from the effects of lightning is generally afcribed. To prevent this mifchief, metallic conductors have been attached to buildings.

The advantage expected from thefe conductors is founded upon a fuppofition, that the matter of lightning will always prefer, in its paffage to the earth, a continued conducting fubftance to one that is broken or interrupted by non-conducting fubftances; and therefore it is fuppofed that a conductor, conftructed of a perfectly continued metallic fubftance, and extending from above the upper furface of a building into the earth, below its foundation, will receive and convey the matter of lightning into the earth, and prevent the mifchief that

that might happen by its ftriking any metallic fubftance which is not continued to the earth.

But buildings furnifhed with fuch conductors have been ftruck by lightning: it is obvious, therefore, that the lightning muft either have paffed to the damaged part without approaching the conductor, or it muft have ftruck from the conductor to the damaged part: for, although all metallic fubftances belonging to a building may be placed at a diftance from the conductor, yet it cannot be fuppofed that any diftance within the limits of a building can exceed the *ftriking diftance* of lightning.

The apparatus with which this experiment was performed, confifts of an electrical machine having a glafs cylinder of about 18 inches diameter, conftructed by Mr. Nairne, in the form of his patent electrical machines.

Two hollow cylinders *, made of thin wood covered within and without with tinfoil, are placed, in a vertical pofition, upon two infulated circular boards; the glafs pillars which fupport the infulated boards are about a foot in height, and are fixed into thick circular boards, covered alfo with tinfoil, and refting upon the floor of the room.

The interior diameter of each of thefe cylinders is 18 inches; the height from the infulated board upon which it ftands, eight feet fix inches, including an hemifpherical top; and the diftance between them is about five feet.

From the centre of each of the boards, that reft upon the floor, rifes a thick glafs pillar, which paffes through a hole of a foot diameter in the centre of the infulated board into the hollow of the cylinder. This pillar of glafs extends about fix inches above the infulated board, and fupports, in a vertical pofition, another cylinder made of wood, and covered with tinfoil.

This interior cylinder is one foot in diameter, and its height is fix feet fix inches, including an hemifpherical top. It is placed exactly parallel to the exterior cylinder, the interval in every part being three inches. To this interior cylinder is fixed a metallic chain, by which it can be connected with the exterior cylinder, fo as to form only one infulated body covered with tinfoil; or can be connected with the earth, whilft the exterior cylinder remains infulated.

The apparatus has alfo a moveable infulated body, made of cork covered with tinfoil, to reprefent a cloud moving in the air.

And alfo two metallic rods placed upon infulated ftands, that they may be connected with, or detached from, the earth at pleafure. Thefe rods are ufed to reprefent the conductors, or any metallic fubftances that belong to buildings; their forms are varied by fixing balls or points upon their upper extremities; and the ftand in which they are placed being hollow, they can be placed at any height by means of a fcrew.

To fhew the effects of the paffage of the electrical fluid, two infulated metallic balls are placed within the *ftriking diftance* of each other; and, in the interval between them, is put fome inflammable fubftance, fuch as dried powdered rofin mixed with cotton.

* It may be of advantage to the reader to confult the literal references at the end of this memoir before he proceeds. N.

EXPERIMENT.

THAT this experiment might refemble, as much as an electrical apparatus is capable of exhibiting, the operations of nature, it was performed by charging and difcharging plates of air.

Arrangement of the Apparatus.

THE metallic chains affixed to the interior tinfoiled cylinders are connected with the tinfoiled boards which reft upon the floor of the room. A metallic communication is then made between thefe boards, and another metallic communication between the exterior cylinders, which remain infulated.

The exterior cylinders, thus united, are connected with one of the conductors of the electrical machine (in this experiment it is with the pofitive); the other conductor (or rubber) being connected with the earth.

The electrical machine being put in action, the exterior tinfoiled cylinders become electrified; and, the interior cylinders being connected with the earth, the body of air between them becomes charged; and, if a circuit be made between the exterior and interior cylinders, an explofion takes place, and a ftrong electrical fhock may be felt.

The exterior cylinders reprefent the upper furface, and the interior cylinders reprefent the lower furface of a body of charged air in the atmofphere.

The moveable body, reprefenting a cloud moving in the air, is connected with the exterior cylinders, by which means it becomes a part of the upper electrified furface of the body of charged air, and can be made to pafs over the building at pleafure.

One of the metallic rods, placed in perfect communication with the earth (but feparate from the communication of the interior cylinders with the earth), reprefents a metallic conductor attached to a building.

The infulated metallic balls, with the rofin placed in the interval between them, are fuppofed to reprefent any metallic fubftances belonging to the building.

A glafs tube filled with water, and about 18 inches in length, is placed between the conductor and one of thefe balls, to reprefent a conducting communication, which muft frequently occur during a thunder-ftorm, between the conductor and other metallic fubftances belonging to the building.

Since a conductor may be raifed to any height at pleafure, it is, in this experiment, fuppofed to be raifed rather beyond the diftance at which any pointed metallic fubftances belonging to the building might, by their influence on the operations of the electrical fluid, endanger the building.

This experiment may be divided into three cafes. 1ft, The electricity oppofite to that of the cloud may extend over the conductor, and all other metallic fubftances belonging to the building. 2dly, It may extend over fome metallic fubftances, but not over the conductor. 3dly, It may not extend over the conductor, or over any part of the building.

CASE

CASE I.

THE electricity oppofite to that of the cloud, that is, the lower furface of the body of charged air, is fuppofed to extend over the conductor, and over all metallic fubftances belonging to the building.

The conductor is therefore connected with the communication between the interior cylinders; and one of the infulated balls being connected by means of the glafs tube with the conductor, let the other ball be connected with the interior cylinders.

RESULT.

WHEN the conductor was terminated by a point, and the cloud paffed near it, an explofion took place upon the point, and the electrified air between the cylinders was inftantly difcharged. When the cloud paffed at a greater diftance, no explofion took place; but the electricity difappeared from the tinfoiled cylinders, and the plate of air between them was filently difcharged, as was fhewn by the electrometer.

When the conductor was terminated by a ball, and the cloud paffed near it; the ball was ftruck, and the plate of air between the cylinders difcharged as before: but when the cloud paffed beyond the *ftriking diftance* to the ball, the tinfoiled cylinders continued electrified, as was fhewn by the electrometer; and the plate of air remained in a charged ftate.

In this cafe no fparks paffed between the balls where the rofin was placed.

CASE II.

THE lower furface of the body of charged air is now fuppofed to extend over fome of the metallic fubftances belonging to the building, but not over the conductor.

The communication between the conductor and the interior cylinders being removed, the remainder of the apparatus was arranged as in the firft cafe.

RESULT.

WHEN the conductor was terminated by a point, and the cloud paffed near to it; a fpark took place on the point, and the rofin in the interval between the balls was inftantly fet on fire. When the cloud paffed at a greater diftance, the electricity of the charged air difappeared filently, as in the firft cafe.

When the conductor terminated by a ball, and the cloud paffed near to it; it was ftruck, and the rofin between the balls inftantly fet on fire: but when no fpark paffed between the cloud and the ball, no fparks paffed where the rofin was placed, and no decreafe of electricity appeared upon the tinfoiled cylinders.

CASE III.

THE lower furface of the body of electrified air is now fuppofed not to extend over any part of the building, and confequently not over the conductor, or over any other metallic fubftance belonging to it.

Therefore the metallic rod, which repreſents the conductor, remaining as in the laſt caſe; the metallic ball which was connected with the communication between the interior cylinders has now a ſeparate communication with the earth only.

RESULT.

WHEN the conductor was terminated by a point, and the cloud paſſed near it; ſome weak ſparks paſſed from it to the point on the conductor. If the cloud paſſed at a greater diſtance, no ſparks appeared, but the electricity on the tinfoiled cylinders was diminiſhed as in the former caſes.

When the conductor was terminated by a ball, weak ſparks paſſed between the cloud and the ball, and the electricity of the tinfoiled cylinders decreaſed; but when the cloud paſſed at a diſtance above the point at which no ſparks paſſed, there was no variation to be obſerved in the electricity of the charged air.

In this caſe no ſparks paſſed in the interval between the balls where the roſin was placed.

To the three caſes already exhibited, a fourth may be added, to ſhew that the lightning may have paſſed to the damaged part of the building without having ſtruck the conductor.

Although a conductor may be extended to a conſiderable diſtance above the building, yet ſome part of the upper ſurface of the body of charged air may approach as near to ſome of the metallic ſubſtances belonging to the building as to the conductor; and which may be exhibited in this caſe in two parts.

CASE IV.—Part I.

THE metallic rod, which repreſents the conductor, has a perfect communication with the earth only; the glaſs tube filled with water, which connected it with one of the inſulated balls, being removed. The other metallic rod, which is to repreſent any pointed metallic ſubſtance, is placed upon an inſulated ſtand, and is connected by a metallic communication with the inſulated ball to which the tube of water had been attached; the other inſulated ball remaining connected with the communication between the interior cylinders as before.

The lower ſurface of the body of charged air is here ſuppoſed not to extend over the conductor, or over the other metallic body, but to extend over ſome ſubſtance within ſtriking diſtance of it.

That the cloud might paſs at nearly the ſame diſtance from the point on the metallic rod as from the conductor, the point on the metallic rod was fixed nearly at the ſame height as the upper extremity of the conductor.

RESULT.

WHETHER the conductor was terminated by a point or by a ball, and the cloud paſſed near to it as well as to the point upon the metallic rod, ſometimes the conductor was ſtruck, ſometimes the point upon the metallic rod was ſtruck, and which inſtantly ſet fire to the roſin between the balls.

Sometimes, when a ſpark paſſed between the cloud and conductor, and no ſpark upon the point of the other metallic rod; ſparks would appear between the inſulated balls, and of ſufficient ſtrength ſometimes to light the roſin.

Theſe

Thefe fparks were the weakeft when the conductor was terminated by a point, and much ftronger when it was terminated by a ball.

This circumftance feemed to fhew, that the paffage of the electrical fluid is not confined to one circuit only.

CASE IV.—Part II.

THE lower furface of the body of charged air is now fuppofed to extend over the conductor; the other metallic fubftances remaining in the fame ftate as before.

The conductor is now connected with the communication between the interior cylinders.

RESULT.

WHEN the cloud paffed near to the conductor, and alfo within ftriking diftance of the point upon the other metallic rod; no explofion or fpark appeared in any of the intervals; but the body of air was filently difcharged, as was fhewn by the electrometer: and the fame effect was produced, when, by lowering the point upon the conductor, the cloud paffed nearer to the point on the metallic rod than to the conductor.

When the conductor was terminated by a ball, the ball was ftruck when the cloud paffed near to it; and if the ball was fituated as the point was, fo that the cloud paffed nearer to the point upon the metallic rod than to the ball on the conductor, the point on the metallic rod was ftruck, and the rofin inftantly lighted.

CONCLUSION.

FROM this experiment it appears to be manifeft, that the advantages arifing from metallic conductors, erected for the purpofes of fecuring buildings from the effects of lightning, depend more upon the lower furface of the body of charged air extending over them, than upon their form or conftruction.

It is not eafy to decide, from the refult of the experiment, whether they fhould be terminated by *points* or by *balls*. Probably, with refpect to the large operations of nature, there may be no difference. The ball ufed in this experiment is three inches diameter; the largeft furface of charged air fcarcely exceeded 70 fquare feet: but if fome acres of charged air were brought into action, the ball of three inches would foon be reduced to a point. However, as the fubject has occafioned much controverfy, we may take the opportunity of inveftigating it from the data afforded by this experiment.

If we reafon upon what is exhibited in the firft and third Cafes, it is obvious that pointed conductors are to be preferred to thofe terminated by balls. They have the property of acting at a much greater diftance than balls, and have the power of deftroying, filently, the effects of lightning, when balls can only accomplifh it by means of an explofion, which muft always be attended with fome danger.

But if we reafon upon the refult in the fecond Cafe, we fhall find that the power which points have of acting at a greater diftance than balls, will make them more liable to produce the mifchief fhewn in that experiment.

Under these considerations, the conductor terminated by a ball might be thought preferable to one terminated by a point: but when we recollect that most of the metallic substances which belong to buildings are generally terminated in edges and points; that they have the same influence as the point upon the conductor; that their operation may be attended with more danger, and that they may extend their influence beyond that of a conductor terminated by a ball; we may therefore conclude, that the height to which a pointed conductor is generally raised above the other metallic substances belonging to a building, when compared with the vast distances at which lightning can act, will not increase, in any great degree, the danger to which a building may be exposed under the circumstances exhibited in the second case of this experiment: and therefore conductors that are terminated by points are more likely to produce the good effects expected from them than those which are terminated by balls.

These considerations upon the effects that may be produced by the joint operations of positive and negative electricity, have, thus far, been confined to what may happen within a space occupied by a building; but they may be extended to much greater spaces upon the surface of the earth. A conductor may be struck by lightning, and (if the lower surface of the body of charged air extends over no part of the building) may convey the lightning into the earth, without the least mischief being done to the building; but when the lightning arrives at the lower extremity of the conductor, there is no proof that by its union with the earth it becomes decomposed. That decomposition cannot happen until it becomes united with its opposite electricity, which may be attached to a body of earth or substances far removed from the conductor. The lightning will therefore continue its operations until it arrives at the place where the opposite electricity is present, and in its passage will occasion much mischief if it does not meet with good conducting substances.

The accidents which have been known to happen at a distance from the place which was seen to be struck by lightning, have been ascribed (by reasoning according to the theory of Dr. Franklin) to what is called a *returning stroke*; but it might, with an equal degree of probability at least, be ascribed to the object to which the accident happened being within the circuit which the lightning makes between the opposite surfaces of a body of charged air.

London, Nov. 29, 1797. HENRY HALDANE.

In addition to what has been already written two figures are annexed (Plate XVIII). The first is a drawing to represent a building furnished with a conductor, which is supposed to be struck by lightning.

The second is a plan of a part of the electrical apparatus, arranged as described in the second Case of these Experiments.

References to Fig. I.

A A represents the upper surface of a body of charged air.

B B B the lower surface of the same body of charged air, which is attached to the earth and terrestrial bodies, and in an opposite state of electricity to that of the upper surface A. A.

This lower surface may extend to the places marked 1, 2, 3; that is, the limit of charged air may be represented by the dotted lines A 1, A 2, A 3, as described in the three Cases of the Experiment.

C the conductor.

C F The

C F, the paſſage which the lightning may take, according to the ſecond Caſe of this Experiment, and damage the ſame building at F, if the limit of charged air be at A 2; or the building near to it, if the limit of charged air be at A 3.

References to Fig. II.

A A, the exterior cylinders.

B B, the interior cylinders.

The interval between them is ſhaded, and contains the air which is charged.

A A, the metallic rod which connects the exterior cylinders.

B B, the metallic rod which connects the interior cylinders.

C, the conductor.

D and E, the two inſulated metallic balls.

C D, the glaſs tube of water which connects the conductor C with the ball D.

E R, a metallic rod, which connects the ball E with the communication B B, between the interior cylinders.

F, ſome inflammable ſubſtance, as roſin mixed with cotton, placed between the balls D E.

G, a metallic rod, to repreſent any metallic ſubſtance belonging to the building, to be placed or removed at pleaſure. (It was uſed in the fourth Caſe, but was removed from the apparatus in the ſecond Caſe.)

H H, circular pieces of cork covered with tinfoil, fixed to a metallic rod P H.

This rod P H is fixed at O, into a glaſs pillar of about ten inches in height; the lower end of the glaſs pillar is fixed into the centre of a wheel S, which turns upon a pivot that is fixed into the table T.

The edges of the wheel S being grooved, two cords are applied to the two grooves. Theſe cords, paſſing through pulleys in the table and alſo in the ſtand of it, are conveyed to the hand of the perſon who is turning the cylinder of the electrical machine, by which means he can make the body H revolve at pleaſure.

From the rod at O riſes a metallic ſtem, with a ſwivel upon the top of it. To this ſwivel is fixed K L, which connects it with the exterior cylinder A.

Over the top of this ſtem below the ſwivel paſſes a cord, fixed at one end to the centre of H, and at the other to the balance weight P, which ſerves to keep P H in an horizontal poſition.

M N, a metallic rod which connects the exterior cylinders A A with the poſitive conductor of the electrical machine.

The metallic rod M N is about eight feet above the floor of the room, and the moveable rod P H traverſes at about ſix feet above the floor. H. H.

II.

New Conſtruction of the Air-Pump. By JAMES SADLER, Eſq. Chemiſt to the Admiralty.

THE conſtruction of the air-pump, with the improvements of Mr. Cuthbertſon and the Rev. J. Prince, have been explained in a former communication *. In order that the effect of this inſtrument may be as perfect as its theory will allow, it is neceſſary that its valves

* Philoſophical Journal, i. 119.

ſhould infallibly open at the requiſite periods of the operation, and that the ſpace between the piſton and the upper or the lower valve, when at its extreme poſitions, ſhould be the leaſt poſſible, or rather that there ſhould be no vacuity remaining. It has been ſhown that in the air-pump of Prince the lower ſpace is abſolutely out of the queſtion, becauſe there is no lower valve; and the upper ſpace may be, and probably is, much diminiſhed by the effect of the oil. The air-pump of Cuthbertſon, in which the conſtruction of the valves is moſt admirable, and their action abſolutely ſecured, ſeems to poſſeſs a ſtill greater portion of this laſt advantage from the circulation of the oil, though not directly intended to anſwer this purpoſe. Mr. Sadler has directed his attention immediately to the good effects which a due application of that fluid is calculated to produce, and has conſtructed an air-pump, poſſeſſing the deſirable requiſites of ſimplicity, cheapneſs and power, of which Figure 1. Plate XIX. is a ſketch.

A B repreſents the barrel, Q a ſolid piſton or plunger fitted into the barrel, and leathered ſomewhat looſely. O is the piſton-rod, capable of being raiſed and depreſſed in the uſual way by a handle and toothed wheel which acts upon the ſtraight rack-work. The barrel communicates, by means of the pipe C, with a chamber D, at the upper part of which is a valve K, opening upwards into a ciſtern L. P is the plate for the receiver, through which a communication-pipe N E paſſes to the interior part of the veſſel, D. At the lower extremity E of this pipe there is a valve, connected with the lever F H, by a wire F E paſſing through a collar of leathers. G is the axis of the lever F H, upon which, by means of a weight at H, an action is produced which preſſes the valve E upwards into its place. H I is another wire or tail, by means of which the extremity H of the lever may be raiſed, and conſequently the valve E opened.

From theſe particulars the effect of the machine may be explained. Let the piſton be drawn out and the barrel filled with oil, which will of courſe riſe to the ſame level in D. Let the piſton then be put in and preſſed downwards by turning the handle. The oil will be driven from the barrel into D, driving the air before it through the valve K ſo completely, that a portion of the oil itſelf will follow into the ciſtern L. Let the piſton be again drawn upwards, and the oil will follow, becauſe D is higher than the valve, and will conſequently leave an empty ſpace in this veſſel D. As ſoon as the piſton-rod has arrived at a certain height, it will act by means of a prominence upon the catch I, and open the valve E. A portion of the air in the receiver will therefore ruſh into D. Upon the return of the piſton the valve E will in the firſt place ſhut, and the air in D will be extruded through K. This extruſion will be as complete as before, becauſe at every ſtroke a portion of oil from the ciſtern L will paſs through the pipe L M to the upper part of the barrel, and eſcape beſide the piſton while riſing, on account of the looſe manner in which it is leathered. From this contrivance it is provided that the internal part of the machine ſhall contain oil ſufficient not only to diſplace the air, but even to paſs through in a ſmall quantity after it by means of the valve K; and conſequently, by working the apparatus the exhauſtion may be carried to any deſired degree. Fig. 2. repreſents the ſame machine viewed in another direction.

In the preſent pump the valve K may be conſidered as repreſenting the valve which cloſes the upper part of the barrel in Cuthbertſon's pump, and the valve E as the lower fixed valve. We ſee therefore from this combination that the ſolid piſton is made to act both ways, in the

the ſame manner as if a ſide valve had been made in Cuthbertſon's barrel, opening outwards beneath the piſton, and the piſton itſelf had been made ſolid. For in ſuch an arrangement the riſing ſtroke would have drawn the air out of the receiver, and the deſcending ſtroke would have driven it out ſideways. But Mr. Sadler has removed the objections which might be made to ſuch a diſpoſition, by making his piſton in effect fluid, and therefore capable of adapting itſelf to every cavity or irregularity in the veſſel D.

How far it might be found, in proceſs of time, that the oil might become changed by the circulation, and leſs fit for the purpoſe; whether it might carry minute bubbles of air along with it, and whether mercury or any other fluid in veſſels of ſuitable materials might be preferable, I know not. But thus much is certain, that the means of conſtructing an inſtrument of this kind, with a few ſlight variations to facilitate the execution, are within the power of many ingenious philoſophical men who have neither the ability nor the ſkill to procure a good air-pump of the other conſtructions.

Fig. 3 repreſents a barrel in which the imperfection of a ſpace between the piſton and lower valve is obviated, and the action of this laſt valve ſecured. A B repreſents the barrel; O, the piſton rod; C, the valve in the piſton; I I, the leathers; L L, a hollow cylindrical piece of metal, which nearly fits the cavity M M, which ſurrounds the lower valve D and its tube. The piece L L does not touch the barrel, and there are two holes, K K, communicating with the central cavity in the piſton, from the ſpace between L and the barrel. This piſton alſo is not leathered very tight, becauſe it is intended that the preſſure of the atmoſphere ſhall force a portion of oil into the lower cavity of the barrel during the aſcent of the piſton. P is the tube which communicates with the receiver. N is a ſtopping-ſcrew ſerving to cloſe a hole through which any ſuperfluous oil may be drawn. G E is a lever applied to open the lower valve D, when the piſton has riſen to a certain height, and by a proper communication acts upon the extremity G by the wire H. Let it be ſuppoſed that the piſton is down, and that oil is poured into the barrel above it. While the piſton is drawn up, a portion of the oil will inſinuate itſelf paſt the leathering into the partial vacuum below. As ſoon as the piſton has riſen nearly to its limit, the valve D will be opened, and part of the air in the receiver will paſs into the barrel through P. Upon the return of the piſton the valve D will in the firſt place cloſe; and when the piſton ſhall arrive at its lower ſtation, the oil in M M, being diſplaced by the piece L L, will riſe through all the cavities of the piſton, and drive out whatever portion of air may not have eſcaped through C during the deſcent. And ſince every riſing ſtroke of the piſton will be attended with the tranſition of a portion of the oil from the upper to the under part of the barrel, the ſpace M M will, at the end of every deſcending ſtroke, be found to contain not only a ſufficient quantity to fill the place which would elſe have been occupied by air, but alſo another portion, which will paſs through C at the termination of the ſtroke, and complete the effect.

III.

Observations on Phosphorus. By Citizen BRUGNATELLI, *Professor of Chemistry, &c. at Pavia* *.

IN one of the public meetings of the Laboratory of the University, when the subject related to the peculiar properties of Kunckel's phosphorus, and required these experiments to be repeated upon which Gœttling has built his theory in opposition to that of the French chemists; I repeated the valuable experiments of Fourcroy and Vauquelin, which are described in the present volume †, which shew the error of the German chemists; and as I have observed some peculiar circumstances not mentioned by the Parisian chemists, I flatter myself that a detail of these experiments will not displease my readers.

EXPERIMENT I.—*The rapid Solution of Phosphorus in pure Oxygene Gas* ‡; *and its Combustion, which takes place by the Mixture of other Gases.*

I introduced a cylindrical piece of phosphorus supported on a tube of glass into a long jar, the upper part of which contained six cubic inches of oxygene gas obtained from nitre. The rest of the jar was full of water. It was formed of thin white glass, three inches in diameter; and the experiment was made in the most perfect darkness with the hydro-pneumatic apparatus. The thermometer of Reaumur stood at 12 degrees above 0, (59, of Fahrenheit). After the phosphorus had remained in this position for one minute, it was withdrawn, and a few bubbles of inflammable gas were introduced. This gas was obtained by dissolving iron in the sulphuric acid § diluted with water, which was constantly used in these experiments. As soon as the bubbles of this gas had entered the phosphorated oxygene gas, the whole mass of elastic fluid beneath the glass appeared phosphorescent. Azotic gas, obtained from animal fibre by means of nitric acids, produced the same effect.

In some instances the phosphorus introduced into the oxygene gas produced a slight phosphoric vapour, which immediately ceased; but the same phenomenon was produced by introducing inflammable gas into this gas. But when by accident the phosphorus introduced into the oxygene gas began to shine; the gas itself, which had thus been placed in contact with the phosphorus, no longer shone, even after the expiration of some time by the addition of the afore-mentioned gases, in whatever proportion they were added, unless the white vapour was condensed and the phosphorus extinguished.

* Annali di Chimica e Historia Naturale, tome xiii. ann. 1797, page 275. I do not possess this work, but have followed the translation of Citizen Van Mons in the 24th volume of the Annales de Chemie, whose accuracy and knowledge of the subject afford the most perfect assurance of his fidelity.

† Ibid. page 3. ‡ The author calls this gas *therm-oxygene.*

§ The author calls this acid *oxi-sulphuric.* He prefixes to the name of each acid the syllables *oxi,* such as oxi-nitric, oxi-phosphoric, &c.

M. Van Mons has preferred the nomenclature presented by the French chemists; and justly remarks, that, if every chemist were to make one, it would soon become impossible to distinguish the substances concerning which they might write.

EXP.

EXP. II.—*Phosphorated Oxygene Gas becomes luminous by the Contact of the Oxygenated Muriatic Acid Gas.*

AFTER having left a cylinder of phosphorus for one minute in several cubic inches of pure oxygene gas, in which it did not shine at all, nor affect the transparency by white vapours when brought into the light; a bubble of oxygenated muriatic acid gas, obtained by distillation of the ordinary muriatic acid from the black oxide of manganese, was introduced. The mixture of the two gases produced a soft light through the whole mass of elastic fluid contained in the vessel.

EXP. III. *Phosphorated Oxygene is also luminous with Nitrous Gas.*

NITROUS gas is very greedy of oxygene, in order to convert itself into nitrous acid. The nitric acid suddenly takes this gas from the oxygene gas, while heat is developed, and the combination is shewn in red vapours. I was desirous of ascertaining what would happen when nitrous gas was introduced into phosphorated oxygene gas, according to the usual process. As I performed my experiment in the dark, I could not observe the moment in which the nitrous gas arrived at the upper part of the jar in contact with the phospherated gas. A very perceptible azure light was seen, which was more considerable than had been afforded by the use of any other kind of gas. A second introduction of nitrous gas produced no further effect. The vessel being afterwards inspected by the light of a candle, the mixture of the two gases appeared so opake, and the glass so loaded with white vapours, that it appeared as if filled with milk.

EXP. IV.—*Phosphorus is soluble in Hydrogene Gas.*

INFLAMMABLE gas dissolves phosphorus as speedily as oxygene gas in the first Experiment, and the whole volume of this gas is impregnated and becomes phosphorescent by the contact of oxygene gas, or the oxygenated muriatic acid gas. But, in order to observe this phenomenon in perfection, it is necessary to operate with pure inflammable gas, which has not long been in contact with water, because this would prevent its acting upon the phosphorus with the same energy; and as this gas absorbs a certain portion of pure air from the water, it would follow that the phosphorus would produce more or less of white vapour, and readily emit light. It is therefore necessary to operate over mercury.

EXP. V. VI. and VII.—*The Appearances of Phosphorus in the Oxygenated Muriatic Acid Gas.*

A PIECE of phosphorus was introduced into a cylinder of glass filled with oxygenated muriatic acid gas over water. It did not burn perceptibly in the dark; but in the light, white vapours were seen through the whole of the glass in such plenty as to render it opake. A short time afterwards the phosphorus was fused and ran like wax by heat, or camphor by the nitric acid. The phosphorus in this state floated on the water, instead of falling to the bottom as before its fusion. The temperature of the atmosphere at the time of making this experiment, was about ten degrees above o. I put a large cylindrical piece of phosphorus into a crystal decanter, placed on a table, and full of very pure oxygenated muriatic acid gas. The capacity of the bottle was eight cubic inches, and it was entirely full. The phosphorus took fire with flame, and threw out a great number of very brilliant sparks against the sides of the vessel, which it heated, and itself became entirely fused. The vessel was

filled

filled with white vapours. Afterwards, in proportion as the veffel, which was not broken in this experiment, became cool, the phofphorus congealed, but continued to fhine, though without emitting fparks. When the veffel was opened under mercury; the metal entered, and filled one third of its capacity. The gas which remained after the rapid combuftion of the phofphorus was tried over mercury; firft, by mixing oxygene gas, and afterwards the hydrogene and azotic gafes: but no light or other phenomenon appeared, to deferve attention.

I covered the ball of a thermometer with two grains of phofphorus melted in hot water, and immediately conveyed this inftrument, which marked 10 degrees above 0, into a fmall bottle full of oxygenated muriatic acid gas. I foon obferved white vapours, by the affiftance of a light; for the experiment was performed in the dark. The phofphorus became foft, flowed, and took fire; and the mercury in the thermometer was feen to rife from 10 to 50 degrees.

EXP. VIII.—*The Phenomena of Phofphorus in the Carbonic Acid Gas.*

AS the carbonic acid gas, at a low temperature, paffes readily enough through water, without being abforbed, to be fubjected to experiment even in the hydro-pneumatic apparatus; I difengaged this gas from the carbonate of lime by means of the diluted fulphuric acid. I collected the pureft gas, and included fix cubic inches in a large tube clofed above, and having its lower end plunged in a veffel of water. A cylinder of phofphorus which was introduced in the dark did not immediately fhine, but after about two minutes it began to emit a glimmering light, which increafed to a greater degree of brilliancy than would have been exhibited in atmofpheric air. The temperature was about ten degrees above zero.

Little fatisfied with thefe trials, I repeated the fame experiments over mercury, and obtained the following refults. The phofphorus did not fhine in the pure carbonic acid gas, and did not appear to me to be diffolved in the fame manner as in fome of the preceding gafes; for the carbonic acid gas mixed with oxygene gas did not afford any light, though it had been long in contact with a large cylinder of phofphorus. Confequently the light which appeared in the carbonic acid gas over water depended on a portion of pure air which was difengaged from the water itfelf, and mixed with the carbonic acid gas, while this laft combined with the water, and diminifhed the affinity of that fluid for the portion of oxygene it ufually holds in folution.

EXP. IX.—*Some Phenomena of Phofphorus with Atmofpheric Air.*

I PUT feveral pieces of phofphorus into two phials of cryftal glafs, of the capacity of about four cubic inches, half filled with pure water, and the other half containing atmofpheric air. Thefe phials were agitated from time to time: after three days, the temperature of the air being at 18 degrees above zero, the water in the bottles emitted a very perceptible fmell of phofphorus, and was a little turbid. The phials being afterwards clofed and gently agitated in obfcurity, I obferved that the included air, which was at firft obfcured, immediately fhone by a flame which ftruck the infide of the phial. By the affiftance of light it was feen that the tranfparency of the air was affected by white vapours. When the vapours had ceafed, I agitated the phial again in the dark, and the fame phenomenon appeared,

appeared, and was frequently renewed, by brilliant points from the furface of the water; fome of which produced the illumination through the air. I faw the fame thing feveral times, as did likewife a number of fpectators, who were much furprifed at the effect.

I at firft fuppofed that the water of the phial in which the phofphorus was placed might hold fufpended certain fmall particles of the phofphorus, which were afterwards diffolved by the atmofpheric air of the veffel; and that fome part of this phofphorus, afterwards taking fire in confequence of the agitation of the water, might produce through the whole of the air, the flame I have here fpoken of. But the ready combuftion of phofphorus in atmofpheric air at the temperature of 18 degrees contradicted this explanation; not to mention that it was difficult to conceive that the air fhould occafion the folution rather than the combuftion of this fubftance. I could not even fuppofe that the atmofpheric air of the veffel was decompofed by the phofphorus diffolved in the water, or by very fmall particles of this fubftance which might float upon the liquid; fo that the azotic gas remaining alone might act on the phofphorus, partly diffolving it, and by this means conftituting a phofphorated gas, which might fhine by the agitation of the water, in confequence of the difengagement of a quantity of oxygene fufficient to occafion combuftion. For I had often obferved that, when the phial was uncorked with the greateft care, in order to avoid any agitation, there was no production of flame notwithftanding the accefs of atmofpheric air; but the phofphorefcence appeared immediately upon agitating the phial, whether the phofphorus had or had not a communication with the air of the atmofphere.

On one occafion, when I had fix phials prepared in this manner, and obferved them for fome time in the dark, I faw that fome of them afforded a flame fpontaneoufly, and without agitation.

I attributed this flame which appeared in the air of the phials, containing water charged with particles of phofphorus, to a combuftion and inflammation of the phofphorus, produced, at the inftant of the agitation of the furface of the water, by the immediate folution of this combuftible in the atmofpheric air. I imagined that this phenomenon might be reproduced as long as there was any oxygene gas in the phial. It might, in fact, take place for a number of times fucceffively, becaufe a very fmall quantity of oxygene is required for the purpofe. The agitation of the water appeared to me to be neceffary to detach the covering of phofphoric acid with which the parcels of phofphorus might eafily be covered, and which might hinder their combuftion, more particularly when the phofphorus was very much divided; that, if this covering fhould be fpontaneoufly detached, either by elevation of temperature or any other circumftance, the phofphorefcence muft appear in the end of the bottles even without agitation; and that the fpontaneous flafhes are produced in this manner.

I took a piece of phofphorus, which I caufed to burn in the air of a phial of the fame capacity as thofe employed in the foregoing experiment, at the bottom of which was a little water. The phofphorus, fufpended by a filk thread in the upper part of the bottle, burned vifibly (the temperature being 15 degrees above zero), and, as ufual, emitted white vapours, which defcended to the furface of the water. I left the phofphorus in the air of the bottle until it no longer fhone, which effect happened in the courfe of the day.

The

The bottle, when observed in the dark, exhibited no light; the water agitated in every direction produced no light; but it was seen to inflame immediately upon the contact of the atmosphere by taking out the cork; and when the light disappeared, it was capable of being produced again by agitating the same water, which immediately before did not exhibit any light by the strongest agitation.

In this experiment the flame likewise arose from the phosphoric particles on the surface of the water, with the vapours of the phosphorus which it condensed by burning. The total want of light in the included air before it was open arose from the total want of pure air, which is necessary for the production of this phenomenon.

EXP. X.

I DISSOLVED a small piece of phosphorus in pure oil of turpentine. The oil became slightly turbid, afterwards clear, assumed a yellowish colour, and emitted a slight smell of phosphorus. A crystal phial was half filled with this solution. I was desirous of observing whether agitation would afford a flame in the air, as in the former experiment; but I did not perceive the least light. I observed the same thing with the prepared oil of thyme, containing phosphorus. The temperature of the atmosphere was 15 degrees above zero.

EXP. XI.—*Observation on Phosphorated Alkohol.*

PHOSPHORUS dissolves in alkohol when it is very pure. In proportion as the solution is effected, the alkohol loses its proper smell, and assumes that of phosphorus, which is very disagreeable. The alkohol takes up very little of the phosphorus, and continues entirely limpid. I attempted, but constantly in vain, to observe the phenomenon of the flame in the air of the vessels, which I had partly filled with phosphorated alkohol. Its habitude was in every respect similar to that of phosphorated oil of turpentine at the ordinary temperature of the atmosphere.

EXP. XII. *Water causes Phosphorated Alkohol to shine very strongly.*

IF in perfect darkness a drop of phosphorated alkohol be poured into a crystal decanter at the bottom of which is a small quantity of pure water, a very singular phenomenon is observable. At the moment when the phosphorated alkohol comes into contact with the water, a brilliant light is disengaged, which passes with a serpentine motion, and rapid, crackling along the surface of the water. The air within the vessel then becomes entirely luminous, and remains for some time in this state. If the bottle be unclosed, and air blown into it; the white vapours disappear, the air becomes transparent, and the phosphorescence disappears: but if the mixture be again agitated, several luminous points are seen at its surface, the air of the bottle becomes again inflamed; and this may be renewed until the whole of the phosphorated alkohol has been consumed.

If a pen be dipped in water, and in this state it be plunged in phosphorated alkohol; at the moment of contact of the two fluids a sudden light spreads through the air, and very perceptible flames are seen, in a dark place, to issue from time to time out of the mouth of the phial.

EXP. XIII. *Water is not the only Substance which sets fire to Phosphorated Alkohol.*

I HAVE observed that the phenomenon of the inflammation of phosphorated alkohol not only takes place with cold and with boiling water, but also with concentrated sulphuric acid,

acid, with the folution of pot-afh, with the liquid nitrate of lime, and with the folution of various alkaline falts.

EXP. XIV. *Sulphuric Ether diffolves Phofphorus.*

I LEFT, during fixteen days, in a well clofed cryftal decanter, two cylinders of good phofphorus, with fome ounces of fulphuric ether.

This liquid did not appear to have loft its ufual fmell, as was the cafe with alkohol. It was very tranfparent; and, upon being decanted into another bottle and agitated in obfcurity, there were no figns of phofphorefcence. I afterwards poured fome drops into water which I had put in another bottle; but here alfo I obferved no appearance of light, though the place of operation was perfectly dark.

The phofphorated ether was feen in the light floating upon the water, and very limpid. I added a fmall quantity of alkchol to this mixture, which immediately rendered it turbid like milk. This phenomenon may afford a fure means of afcertaining the purity of ether. The mixture agitated in every direction afforded no light; but light appeared with much vivacity and at feveral repetitions after the mixture had been a little heated. A great number of luminous points appeared on the furface of the water after it had been long agitated. I made this experiment in a bottle of a certain fize, clofing and unclofing it fometimes in order that the white vapours might iffue out, in proportion as they were formed.

The following are the confequences which I have deduced from the foregoing experiments:

1. Phofphorus is very readily diffolved in pure oxygene gas at a mean temperature *(Exp. I.)* without emitting light; and the phofphorated oxygene gas fhines when it is diffufed in any other azotic or mephitic gas. *(Exp. II. and III.)* This phenomenon appears to me to depend on the agitation which the integral parts of the oxygene gas and phofphorus undergo when another gas is mixed with them, and from the diminution of affinity between the integral parts even of the oxygene gas, effected by the addition of the new gas. By this diminution of affinity, if phofphorus is placed in the fphere of chemical attraction with the oxygene, the oxygene gas becomes decompofed, and the phofphorus burns.

2. Phofphorus is likewife fpeedily diffolved in inflammable gas *(Exp. IV.)* and this phofphorated gas burns when it is brought into contact with any oxygene gas.

3. The oxygenated muriatic acid gas fuddenly burns phofphorus. The oxygene gas which contains this acid is decompofed by the phofphorus, which rapidly combines with the oxygene *(Exp. V. VI. VII.)* to form phofphoric acid; and the concrete caloric, or other component principles of the oxygenated muriatic acid gas, being fet at liberty, raifes the temperature, and melts the phofphorus which is not yet oxygenated.

4. Phofphorus is not foluble in pure carbonic acid gas *(Exp. VIII.)*; neverthelefs, at the temperature of 12 degrees above 0 of the thermometer of Reaumur, it burns better in this gas mixed with a fmall portion of oxygene gas, than when in atmofpheric air at the fame temperature.

5. Water does not diffolve phofphorus, but it holds very fmall particles in a ftate of fufpenfion, more efpecially thofe which are impregnated with a principle of combuftion,

 and

and have been fufed by caloric *(Exp. V. VI. and VIII.)*. The air of the atmofphere diffolves phofphorus at the moment it burns it, and the folution is fenfibly phofphorefcent when the experiment is made in a cryftal glafs bottle. This caufes a flame more or lefs brilliant, in proportion to the greater or lefs quantity of phofphorus which is burned at the fame time.

6. Phofphorus which is burned in a decanter with water at the bottom, not only decompofes the oxygene of the air in which it burns, but likewife that of the air which is found mixed with water *(Exp. X.)*.

7. Oil of turpentine and oil of thyme, containing phofphorus, do not burn in contact with atmofpheric air at the ordinary temperature *(Exp. XI.)*.

8. Alkohol diffolves phofphorus, and lofes its agreeable fmell. Phofphorus fhines in the atmofpheric air of clofed veffels, by pouring phofphorated alkohol upon fome fubftance which fhall combine with the liquid and feparate the phofphorus; fuch are water, fulphuric acid, &c. *(Exp. XII. XIII. XIV.)*

Sulphuric ether likewife diffolves phofphorus, and this folution is not decompofed by water, upon which it floats; but when phofphorated ether is diluted with alkohol, or is changed into anodyne liquor, it then combines with water, and quits the phofphorus, which, being divided into very fine white particles, gives the mixture the colour of milk.

IV.

On the Advantage of inverting the Slider in many Operations on the Common Sliding Rule. By the Rev. W. PEARSON, of Lincoln.

To Mr. NICHOLSON, Author of the "Journal of Natural Philofophy," &c.

SIR,

ON perufing the eighth number of your Journal, I was much pleafed with the ingenious inftruments mentioned in the VIIIth article, upon which the graduations of Gunter's Line of Numbers are fo difpofed as to be very confiderably enlarged, whilft yet the whole range of a fingle line is contained in a fmall compafs. If an inftrument of either the parallel, circular, or fpiral conftruction, of a convenient fize, were manufactured for fale, with a proper degree of accuracy, I am fully perfuaded that confiderable advantage would be derived therefrom by purchafers; particularly by thofe perfons whofe profeffion is to make practical calculations. But as the public are in poffeffion of a fliding rule, in the operations of which long ufage has made every mechanic expert, it is to be apprehended that prejudice would in this, as in other inftances, prove an obftacle to the general fale of fuch an inftrument, were the introduction of one into general ufe attempted. This obfervation is, I believe, verified by the flow fale of the patent, and alfo of the new improved fliding rule, in comparifon of that of the common one, amongft artificers, who conftitute the moft numerous clafs of purchafers. I have been informed by a reputable tradefman, who retails fliding rules of the different conftructions, that for every patent, or new improved fliding rule that is purchafed, more than twenty of Cogfhall's, or the common fliding rules, are fold.

fold. By the fame prejudice, probably, any *new method* of working by the *old rule*, though more expeditious and equally accurate, would be rejected in practice if fuch method were offered to the public. But, be this as it may, I think the confideration that the probability is but fmall of any particular improvement being generally adopted in practice, is not a fufficient reafon why that improvement fhould be withheld from the public eye, whilft it is confidered as fuch. There are always individuals whofe minds are unfettered by prejudice *, and to whom any hint tending to produce an improvement always proves acceptable.

The logarithmic line of numbers, known by the name of Gunter's line, is fo well underftood by every one who is acquainted with the properties of logarithms, as to render any obfervation on the conftruction of it unneceffary. The ufe of two fuch lines, ufually denominated A and B, working together by means of a direct flider, fuperfedes the application of compaffes, and is taught in moft practical books of arithmetic.

There is however another, and I think more commodious method of applying the flider, than the one generally practifed (except in working direct proportions), which I have not feen explained, nor practifed by any perfon; and which, therefore, I fhall beg the favour of you to communicate to the public through the medium of your interefting Journal, provided you deem it of fufficient importance. The method I have to communicate lays no claim to fuperior accuracy when compared to the common method, the powers of the inftrument being the fame in both; but, befides novelty, I truft it will be found to have facility to recommend it.

The method is fimply this: Invert the flider B on any common fliding rule, whereby the numerical figures will afcend on it, and on the fixed line A, in contrary directions: now, as the diftance from unity to any multiplier, on Gunter's line, will invariably extend from any multiplicand to their product, it follows, that if any particular number on the inverted flider B be placed oppofite to any other given number on A, the product of thofe numbers will ftand on the flider B, againft unity on A; for, in any pofition of the inverted flider, the diftance from unity to the multiplier on A, inftead of being carried forward on B, as when the flider is in a direct pofition, is brought back thereby to unity again; fo that unity (or *ten* on fingle lines where the flider is too fhort for the operation) is invariably the index for the product of any two coincident numbers throughout the lines.

In divifion, by the fame procefs, if the dividend on B be put to the index, or unity on A, the divifion and quotient will coincide on the two oppofite lines; fo that when one is given, and fought for on either line, the other is feen on its oppofite line at the fame time.

* This prejudice is the effect of habit, and can feldom be eradicated from the minds of fuch individuals as confider the ready occurrence of a propofition as a teft of its truth. To eftablifh a new philofophical theory has, in every inftance, required time fufficient to educate an entire generation of men. The rejection of the Ariftotelian philofophy, the adoption of experimental refearch, the fubftitution of the doctrine of gravitation inftead of that of vortices, and the rejection of phlogifton by our contemporaries, are fufficiently illuftrative of this affertion. New practices are ftill more difficult to be introduced. The new grammar, the new rudiments of fcience, or the new inftrument, however fuperior to the old in fimplicity, facility, and truth, muft be lefs valuable to the ordinary teacher or artifan, whofe memory is familiarized with the precepts of the latter, and whofe only ambition is to earn his fubfiftence with the leaft poffible exertion. N.

The next operation which offers itself here is reciprocal proportion, which can be effected by no other method than by inverting the flider, but which is rendered as eafy by this application, as direct proportion is in the common way; for if any antecedent number on B inverted be fet to its confequent on A, any other antecedent on B, in the fame pofition, will ftand againft its confequent on A, fo as that the terms may be in a reciprocal ratio. In fquaring any number, it will appear from what has been already faid, that if the number to be fquared be placed on B, inverted againft the fame on A, the fquare will ftand on B, againft unity on A. Therefore, to extract the fquare root of any number, let that number on B ftand againft unity on A; and then wherever the coincident numbers are both of the fame value, that point indicates the root. If two dividing lines of the fame value do not exactly coincide, the coincident point will be at the middle of the fpace contained between thofe two which are neareft a coincidence; and as there is only one fuch point, there can be no miftake in readily afcertaining it. The finding of a mean proportional between any two numbers is extremely eafy at one operation; for if one of the numbers on B inverted be fet to the other on A, the coincident point of two fimilar numbers fhews either of thofe to be the mean, or fquare root of their product, according to the preceding procefs. Thus have we a fhort and eafy method of multiplying, dividing, working reciprocal proportion, fquaring and extracting the fquare root, at one pofition of the inverted flider, whereby the eye is directed to only one point of view for the refult, after the flider is fixed: whereas, by the common method of extracting the fquare root by A and B direct, the flider requires to be moved backwards and forwards by adjuftment, the eye moving alternately to two points, till fimilar numbers ftand, one on B againft unity on A, and the other on A againft the fquare number on B; which fquare number, in the cafe of finding a mean proportional, muft be found by a previous operation. Hence, for more convenience in the extraction of roots, and meafuring of folids, an additional line called D, has been added to the rule, which renders it more complex, and confequently feldom underftood by an artificer. Upon examining the patent rule with a graduated piece of brafs, to be attached to, or detached from the flide at pleafure; and alfo Mr. Horton's new improved rule with two fliders; I perceive that the method of multiplying length, breadth, and depth into one another at one pofition, in which their excellence confifts, is nothing more than a combination of the two methods on the common rule; for the product of the two firft numbers, effected by means of an inverted and a direct line, forms the multiplier to be ufed with the third number, by a third direct line, at the fame pofition of the fliders. Hence, a gauger's rule will anfwer the fame purpofe, if an additional flider be held in a proper fituation inverted. No reafon, however, has been given for the effect of this procefs in any book that I have yet feen, nor of the application of an inverted flider to any other fpecies of calculation *.

* In reply to the obliging poftfcript of the author of this paper, I have only to remark, that the approbation of the intelligent cultivators of fcience is the moft eftimable reward of my endeavours to diffufe knowledge; and that inclination, as well as duty, will induce me to pay every attention to his future communications. N.

V. *Abftract*

V.

Abstract of a Memoir entitled "Enquiries concerning the Nature of Prussian Blue" By *Mr.* PROUST *.

IF iron were fufceptible, as chemifts imagine, of uniting with oxygene in all the intermediate proportions between $\frac{27}{100}$ and $\frac{48}{100}$, which appear to be the two extreme terms of its union with that principle, ought it not to afford, with any given acid, as many different combinations as it is fuppofed capable of producing oxides? Why, for example, does not this metal, which affords, with the fulphuric acid, a falt conftant in its properties when oxided no farther than $\frac{27}{100}$, exhibit different combinations, equally conftant in their refpective properties, when it contains $\frac{34}{100}$, $\frac{38}{100}$, or $\frac{45}{100}$ of oxygene?

A great number of facts prove, on the contrary, that iron does not reft indifferently at all the different degrees of oxidation between the two terms abovementioned; and, notwithftanding the different degrees of oxygenation through which iron is fuppofed to pafs when its fulphate is expofed to the air, we are acquainted with no more than two fulphates of this metal.

The firft is the green, or cryftallizable fulphate, in which Lavoifier has proved that the iron contains $\frac{27}{100}$ of oxygene. This falt, when pure, is infoluble in fpirit of wine; its folution in water is of a very pale fea green colour. It is not altered by the gallic acid, affords no blue with alkaline pruffiates, &c.

The fecond kind of fulphate, no lefs conftant in its properties, is that red deliquefcent incryftallizable combination which is foluble in alkohol, and is known by the name of mother water of vitriol; but is not really fuch, unlefs when it produces no alteration in the oxygenated muriatic acid; that is to fay, when its oxide contains $\frac{48}{100}$ of oxygene.

This fulphate is eafily obtained by treating it with nitric acid until its folution no longer difengages nitrous gas, by the addition of a new quantity of acid.

This fulphate, befides, poffeffes exclufively the property of forming a black precipitate with the gallic acid, and of affording Pruffian blue with the alkaline pruffiates, as will hereafter appear.

Between thefe two fulphates, which Mr. Prouft calls the green fulphate and the red fulphate, there is no intermediate point. If the green fulphates, by expofure to the contact of air, affume a colour which appears to belong to neither of thefe fpecies, it may be decidedly fhown that they are fimply a mixture of the two, by feparation by means of alkohol. Each falt will then exhibit all its diftinctive properties. The green fulphate will conftantly produce a green precipitate with the cauftic alkalis; which precipitate will foon become black if preferved under water without the contact of the air, becaufe its particles continually approach each other, and render the colour more and more intenfe.

The red fulphate, on the contrary, will afford, with the fame alkalis, a yellow red pre-

* This abftract is tranflated from the Annales de Chimie XXIII. 85. The editors do not fay where the original is to be found.

cipitate,

cipitate, which is an oxide, incapable of attracting any thing from the air of the atmoſphere, nor from the oxygenated muriatic acid.

This oxide, according to the experiments of Mr. Prouſt, contains $\frac{48}{100}$ of oxygene, and is capable of forming the baſis of a ſeries of combinations, which bear the ſame relation to theſe which afford the black oxide, as the red ſulphate of iron bears to the green ſulphate.

So that in whatever degree of alteration an ordinary ſulphate may be examined, it will be found to conſiſt merely of theſe two ſalts, mixed in different proportions.

Whence it follows, that a ſulphuric or muriatic ſolution of this metallic ſubſtance is nothing but a mixture of the two ſalts, one of which has for its baſis the oxide of iron, containing $\frac{27}{100}$ of oxygene, and the other the ſame oxide, containing $\frac{48}{100}$ of oxygene. And as little attention has hitherto been paid to the diſtinctive properties of theſe ſalts; effects have frequently been attributed to the one, which belong excluſively to the other.

We muſt therefore diſtinguiſh two ſulphates, two muriates, two arſeniates, two pruſſiates, &c.: the latter ſalts more particularly conſtitute the object of Mr. Prouſt's Memoir.

The White Pruſſiate.

IN order to obtain this ſalt, a ſolution of the perfectly pure green ſulphate muſt be taken and mixed with a well ſaturated ſolution of the pruſſiate of pot-aſh. This laſt ſolution, if perfect, affords cryſtals of a beautiful orange-yellow in tetrahedral pyramids, truncated near their baſe.

The green ſulphate of iron is obtained and preſerved in a ſtate of purity, by keeping it in a well cloſed veſſel quite full, in which a ſmall piece of iron or tin is put. The ſame purpoſe is anſwered by reducing the red oxide, which may exiſt in the ſolution, to the ſtate of black oxide, by a mixture of water charged with ſulphurated hydrogene gas. This ſulphate, after purification, ought not to change by the addition of the gallic acid. In this ſtate, its colour is an extremely weak ſhade of ſea-green. The green colour does not appear, unleſs the ſolution be in contact with the atmoſpheric air.

After having poured the ſolution of the pruſſiate upon the ſulphate of iron, the bottle muſt be immediately cloſed. A plentiful white precipitate immediately falls down, which ſoon afterwards aſſumes a green tint, occaſioned by the air contained in the bottle, and likewiſe by another cauſe. But if the veſſel remain cloſed this ſhade does not become deeper, and light alone does not produce any change.

It is adviſable to pour an exceſs of the pruſſiate upon the ſulphate, in order to complete the decompoſition. After ſeveral hours repoſe, this pale pruſſiate is covered with a yellow liquor, which is a mixture of the pruſſiate and the ſulphate of pot-aſh. This liquor retains in ſolution a ſmall quantity of white pruſſiate, which acquires a blue colour by abſorption of oxygene, when it is placed in contact with the atmoſpheric air. By this union, it becomes inſoluble, and falls upon that which is at the bottom. The laſt-mentioned precipitate receiving in its turn the impreſſion of the atmoſphere, ſoon becomes blue at its ſurface. The intenſity of colour ſucceſſively increaſes, and proceeds downwards, until at length the white pruſſiate becomes converted into Pruſſian blue.

The action of the oxygene of the atmoſphere, in theſe circumſtances, is ſtill more confirmed by the phenomena which are ſeen when the white pruſſiate is expoſed to the action of of the air on a filter. It becomes blue by abſorbing oxygene; and the colour does not acquire its greateſt intenſity until the oxide of iron has obtained $\frac{43}{100}$ of that principle.

Of all the ſaline combinations of iron, there is none of which the baſis becomes ſaturated with oxygene ſo rapidly as the pruſſiate. The carbonate of iron is not to be compared with it.

The ſulphuric and muriatic acids poured on the white pruſſiate of iron occaſion no change. The oxygenated muriatic acid, on the contrary, enlivens the pruſſiate in an inſtant, and loſes its odour. The nitric acid likewiſe converts it to blue, but more ſlowly, becauſe it does not abandon its oxygene with the ſame facility as the oxygenated muriatic acid.

The ſolution of ſulphurated hydrogene, which does not alter the green ſulphate, as has already been remarked, has likewiſe no action on the white pruſſiate. It merely reduces to white the ſmall quantity of pruſſiate which had become blue by the acceſs of air; or, in other words, it deprives it of that portion of oxygene which, it may already be ſeen, conſtitutes the difference between the white and the blue pruſſiates.

It may perhaps be an anticipation of the facts, to diſtinguiſh that pruſſiate by the name of white, which in ſtrictneſs is ſuch only at the moment of its formation, and afterwards becomes covered with a greeniſh white; but it is very probable that it acquires this ſhade merely from the air contained in the veſſel, and in the liquids made uſe of. There exiſts likewiſe another cauſe in the ſmall portion of red oxide, from which the alkaline and even the calcareous pruſſiates derive their yellow colour. This oxide becomes converted into blue pruſſiate, and mixes with the white pruſſiate at the moment wherein the pruſſic acid, being diſengaged from the alkalis, is at liberty to unite with the different oxides it meets with. Theſe oxides are, as we have ſeen, a great quantity of the black oxide mixed with the red oxide of the alkaline pruſſiate. It muſt therefore neceſſarily follow, that a ſmall portion of the blue pruſſiate will be mixed with a great quantity of the white pruſſiate; whence this laſt acquires its greeniſh tinge.

From the whole of theſe facts, it is evident that the oxide, which conſtitutes the baſe of the white pruſſiate, poſſeſſes the ſame degree of oxidation as when it formed the baſis of the green ſulphate. This oxide in effect paſſes from the ſulphate to the white pruſſiate, without the interference of any cauſe which can either increaſe or diminiſh the proportion of oxygene. Hence it follows, that ſince the alkalis ſeparate the oxide of the green ſulphate of a graſs-green colour, they ought alſo to ſeparate it from the white pruſſiate under the ſame tinge.

This in fact happens at the inſtant when the cauſtic alkali, or ammoniac, is poured on the white pruſſiate. But in order to judge better of the ſhades, it is adviſable to uſe theſe ſolutions diluted with water. The green oxide is not completely deprived of pruſſic acid till after the repeated application of alkalis. This may be ſhewn by taking up the green oxide, by an acid which does not diſſolve the white undecompoſed pruſſiate.

The Blue Pruſſiate.

AFTER what has been ſaid, it is eaſy to foretel the habitudes of the nitrate, and all the ſolutions of which the oxide is at its maximum of oxidation, when alkaline pruſſiates are applied. No interval is perceived between the precipitation and the moſt lively blue. The

colour

colour is perfect at the inftant of precipitation, and expofure to the atmofphere adds nothing to its intenfity.

Pruffian blue is, in a word, that pruffiate, whofe bafis contains $\frac{48}{100}$ of oxygene. It is the fame with refpect to the white pruffiate as the red fulphate is to the green. Thefe two pruffiates do not differ with regard to their acid. Their diftinctive characters arife from the different oxidation of their bafes.

The blue pruffiate is not altered by acids. The oxygenated muriatic acid changes it, by rendering it green, at the fame time that itfelf undergoes alteration, as Berthollet has difcovered. But the action of this deftructive acid is exercifed on the pruffic acid, and not on the oxide, which can receive no greater dofe of oxygene than it has already acquired from the nitric acid, the air, &c.

The acids which are ufed to brighten fuch Pruffian blues as are imperfect, are of no other ufe than to diffolve the great quantity of carbonate of iron, precipitated by the pot-afh which is not faturated with pruffic acid, and fuper-abounds in ill-prepared lixiviums. If white pruffiate were to exift in fuch precipitates, it would be in no refpect changed by the acid, and would acquire a blue colour only by abforbing from the atmofpheric air the quantity of oxygene neceffary for that purpofe.

In order to fhew that oxygene is the principle which in the blue pruffiate of iron affords the diftinctive colour, nothing more is neceffary than to remark the colour of the oxide when precipitated by alkalis. In the green fulphate the precipitate was black; after the precipitation from Pruffian blue it is red. No other principle but oxygene could have occafioned this difference in thefe oxides.

Mr. Prouft having fpoken indifferently of the yellow and the red oxide of iron, as being completely faturated with oxygene, obferves, that he ufes thefe expreffions without diftinction, becaufe a number of facts have convinced him that there is no difference between them. Every red oxide, fays he, when diffolved in any acid whatever, is precipitated of a yellow colour by alkalis, whether pure or faturated with carbonic acid. This laft acid occafions no difference in the precipitate, becaufe it has no tendency to unite with iron at that degree of oxidation. The red oxides when dried are brown, obfcure and often black, according to the degree of drynefs or denfity they may have acquired. But if they be pounded in a mortar, the characteriftic colour foon appears. It is affirmed, that thefe oxides have the power of decompofing ammoniac; but our author kept them for feveral years in ammoniac, without obferving that they were in any refpect changed. He had no better fuccefs with the oxide of manganefe at the ordinary temperature of the air.

The folution of fulphurated hydrogene gas, when kept in a veffel with the blue pruffiate, is decompofed. It feizes from the oxide that portion of oxygene which conftitutes the difference between the blue and the white pruffiates; and the pruffiate thus rendered white exhibits the fame phenomena with alkalis, as if it had been obtained immediately from the green fulphate. The white pruffiate kept under hepatic water undergoes no alteration; in which refpect it refembles the green fulphate. Either of thefe readily yield to the hydrogene diffolved in this water all the oxygene they poffefs, exceeding 27 per cent.

The fame theory explains, why the red fulphate and the nitrate of iron decompofe fulphurated hydrogene. The oxide of iron burns the hydrogene, the fulphur is depofited, and the fupernatant liquor, inftead of affording a red precipitate by alkalis, exhibits a green.

This

This obfervation points out a method of reftoring the copperas of the fhops to the ftate of green fulphate. When brown depofitions are formed, it contains copper.

The folution of fulphurated hydrogene is not the only practicable means to reduce the blue pruffiate to the ftate of white pruffiate. The requifite abftraction of oxygene is made by keeping Pruffian blue with water and plates of iron or tin in a well clofed veffel. By this treatment it acquires all the properties of the white pruffiate.

The diftribution of oxygene between a metal and its oxide is not an uncommon fact in chemiftry. By keeping a red fulphate or muriate with iron, they are enabled to recover their firft ftate. Mercury kept in a folution of corrofive fublimate becomes changed, as does likewife the metallic falt itfelf, into the mild muriate of calomel. Mercury undergoes the fame change in a folution of the red muriate of iron, but is not altered in a folution of the green muriate of the fame metal. In the red fulphate, the mercury is converted into that kind of fulphate which does not become yellow by the affufion of water, that is to fay, in which the oxide is at its minimum. Many other facts of the fame kind might be mentioned.

It has been afferted in this paper, that the oxygenated muriatic acid does not act upon the oxide of Pruffian blue. The following affords a proof of the truth of this affertion: All the known red oxides, whether natural or artificial, colcothar, the iron ore of Elba*, undergo no alteration in this acid; but the native brown oxides, which for the moft part are mixtures of the black and red oxides, are affected by fuch treatment.

It is found by means of the oxygenated muriatic acid, that the oxides of the nitrate, the acetite and the muriate of lead are not at their maximum of oxidation. All thefe falts, when kept beneath this acid, are decompofed. A brown or puce-coloured oxide is foon depofited, and even cryftallized round the fides of the veffels: and the nitric acid no longer acts upon the new oxide. In procefs of time, however, that acid affumes a fine rofe colour, bubbles of air rife from the mixture at the bottom, and at length the nitrate is produced, as foon as the oxide, continually folicited to union by the nitric acid, has parted with the dofe of oxygene which oppofed that union.

The muriatic acid does not diffolve fuper-oxygenated lead without producing at the fame time abundance of the oxygenated acid; but in order to procure this oxide in greater quantity, it is only neceffary to treat the minium of commerce with a weak nitric acid, which feparates 13 or 14 per cent. of brown oxide, difcovered, as is well known, by Scheele. The red lead of Siberia is nothing elfe, as Mr. Macquart has fhewn, but a natural fuper-oxidation of this metal. It would be interefting to know, whether by carrying the calcination beyond the point which affords minium, it might not be poffible to render the oxide brown. Such a procefs might perhaps fupply the want of manganefe, for preparing the oxygenated muriatic acid.

Mr. Prouft defers to a future communication an account of the nature of lead, oxided in a lefs degree than conftitutes the bafis of the nitrate of this metal.

From the foregoing facts Mr. Prouft draws the following conclufions;

The oxide which alkalis feparate from Pruffian blue is red, though it originally was black in the green fulphate which afforded this blue.

* The ore of Elba often contains fulphate of iron. It may be extracted by treatment with nitric acid, and fubfequent precipitation with ammoniac or cauftic pot-afh.

The

The white prussiate is a salt which is affected by atmospheric air in no other manner than the green sulphates, muriates and carbonates, or, in a word, most of the saline combinations which contain iron oxided to the minimum. There is no other difference with respect to their super-oxidation, but the greater or less time these metallic salts demand; for he observed that the muriates and phosphates of which the oxide is at the minimum are not perceptibly altered by exposure to the air.

The prussiate of iron is not the only combination of this metal which owes its blue colour to atmospheric oxygene. That which is called native Prussian blue is merely the phosphate of iron oxided to a certain point. Mr. Proust intends to shew artificial phosphates which are grey, blue, and white, according to the degree of oxidation. We are at present well acquainted, adds the author, with the cause of those dull greenish tinges which frequently appear in the newly-made prussiates. It often happens that they are not brightened well by acids, and do not acquire their peculiar lively colour but by exposure to the air. This fact is well known to manufacturers, who accordingly take care not to waste their acid in attempting to render such blues perfect.

It has been observed, that the pure green sulphate does not afford a black with the gallic acid. This is very true of it at the first moment. But the contact of air soon colours the mixture at its surface. A few drops of the oxygenated muriatic acid immediately produce the same effects throughout the fluid. We see therefore that iron does not form ink with the gallic acid, but in proportion as it is oxided. This black colour may likewise be destroyed by including the black mixture in a bottle with a certain quantity of hepatic water.

From these facts it also very evidently appears why it is the established practice to expose stuffs to the air after the black dye; and why ink newly made and pale becomes black very speedily after it is spread upon paper, &c. For in all these mixtures the sulphate of commerce is used, which contains little of the red sulphate, and much of the green. When the gallic acid is poured into solutions of the sulphate and muriate, containing the red calx of iron, ink is instantly produced. The basis of ink and of every black dye is therefore merely the gallate of iron, of which the iron is oxided to the maximum. Lastly, we cannot but observe in all these facts that chemists have hitherto erred with respect to the property of the ordinary sulphate of iron to become black with the acid of galls, to afford a blue with alkaline prussiate, &c. These properties belong exclusively to the combinations of which the oxide contains $\frac{48}{100}$ of oxygene, and not $\frac{27}{100}$ only. I shall conclude, says Mr. Proust, by deducing from these experiments the principle I have established at the commencement of this memoir; namely, that iron, like many other metals, is subjected to the law of nature, which presides at every true combination; that is to say, that it unites with two constant proportions of oxygene. In this respect it does not differ from tin, mercury, lead, and, in a word, almost every known combustible. He intends shortly to explain the nature of that kind of oxide which results from the union of oxygene with carbone, in a less proportion than is required to constitute the carbonic acid.

VI. An

VI.

An Account of some Experiments to determine the Force of Fired Gunpowder. By BENJAMIN *Count of* RUMFORD, *F. R. S. M. R. I. A.* *

AFTER obferving that no human invention, except perhaps the art of printing, appears to have produced fuch important changes in civil fociety as the invention of gunpowder; that notwithftanding the extenfive ufes to which this wonderful agent is applied, it feems not hitherto to have been examined with the attention it merits, and that probably this want of inveftigation may have arifen from the danger attending the experiments; our author proceeds to relate his own experiments and obfervations.

Several eminent philofophers and mathematicians have, from time to time, directed their attention to this fubject. The modern improvements in chemiftry have greatly elucidated the caufe and circumftances attending the explofion of gunpowder. But the great defideratum, namely, the real meafure of the initial expanfive force of this agent, has not yet been determined. Robins, from his experiments, concluded that the elaftic force of the fluid generated in the combuftion of gunpowder, is one thoufand times greater than the mean preffure of the atmofphere; but the celebrated Daniel Bernouilli determines its force to be not lefs than ten thoufand times the fame mean preffure. Count Rumford, being ftruck with the great difference in thefe refults, has occafionally, for many years, endeavoured to afcertain the truth by experiment. In a paper printed in the year 1781, in the Tranfactions, he gave an account of an experiment, No. 92, by which it appeared that, calculating even upon Mr. Robins's own principle, the force of gunpowder muft be at leaft 1308 times greater than the mean preffure of the atmofphere; and from that, and many other experiments, he became convinced that it was neceffary to abandon the methods of that philofopher, and make others with a very different apparatus.

His firft attempts were to fire gunpowder in a confined fpace, thinking that when he had accomplifhed this he fhould find means of meafuring its elaftic force without difficulty. A very ftrong fhort gun-barrel was prepared, and attempts made to fire gunpowder in the fame, by means of a fmall vent, provided in one inftance with a valve, and in another experiment lined with gold to prevent corrofion. Thefe attempts were unfuccefsful; the force of the explofion proving fufficiently great to enlarge the hole, and fo rapid as not to allow time for clofure of the valve. It became neceffary, therefore, to endeavour to fire the gunpowder by means of heat conveyed through the mafs of the metal itfelf. This was fuccefsfully performed in a barrel of the beft forged iron, 3,45 inches long, the diameter of its bore $\frac{7}{10}$ths of an inch, and its ends clofed up by two fcrews, each one inch in length, which were firmly and immoveably fixed in their places by folder. The vacuity between them in the barrel was confequently 1,45 inch in length, and conftituted the chamber of the piece, whofe capacity was nearly $\frac{6}{10}$ths of a cubic inch. The thicknefs of the metal was equal to the bore of the piece. An hole 0,37 of an inch in diameter was bored through both fides of the barrel, through the centre of the chamber, and at right angles to its axis. Two tubes of iron, 0,37 of an inch in diameter, the diameter of whofe bore was $\frac{1}{4}$th of an inch, were firmly fixed in thefe holes with folder, in fuch a manner, that, while their internal openings

* Abridged from the Philofophical Tranfactions for 1797, p. 222.

were exactly opposite to each other, and on opposite sides of the chamber, the axes of their bores were in the same right line. The shortest of these tubes, which projected 1,3 inch beyond the external surface of the barrel, was closed at its projecting end; or rather it was not bored quite through its whole length, $\frac{3}{10}$ths of an inch of solid metal being left at its end, which was rounded off in the form of a blunt point. The longer tube, which projected 2.7 inches beyond the surface of the barrel on the other side, and which served for introducing the powder into the chamber, was open; but it could occasionally be closed by a strong screw furnished with a collar of oiled leather which was provided for that purpose. The method of making use of this instrument was as follows: The barrel being laid down, or held in a horizontal position with the long tube upwards, the charge, which was of the very best fine-grained glazed powder, was poured through this tube into the chamber. In doing this, care must be taken that the cavity of the short tube be completely filled with powder; and this can best be done by pouring in only a small quantity of powder at first, and then, by striking the barrel with a hammer, cause the powder to descend into the short tube. When, by introducing a priming-wire through the long tube, it is found that the short tube is full, it ought to be gently pressed together, or rammed down by means of the priming-wire, in order to prevent its falling back into the chamber, upon moving the barrel out of the horizontal position. The short tube being properly filled, the rest of the charge may be introduced into the chamber, and the end of the long tube closed up by its screw.

More effectually to prevent the elastic fluid generated in the combustion of the charge from finding a passage to escape by this opening after the charge was introduced into the chamber, the cavity of the long tube was filled up with cold tallow, and the screw that closed up its end (which was $\frac{1}{2}$ an inch long, and a little more than $\frac{1}{10}$th of an inch in diameter) was pressed down against its leather collar with the utmost force. The manner of setting fire to the charge was as follows: A block of wrought iron, about $1\frac{1}{2}$ inch square, with a hole in it capable of receiving nearly the whole of that part of the short tube which projects beyond the barrel, being heated red hot, the end of the short tube was introduced into this hole, where it was suffered to remain till the heat, having penetrated the tube, set fire to the powder it contained, and the inflammation was from thence communicated to the powder in the chamber.

The result of this experiment fully answered the expectation of the author. The generated elastic fluid was so completely confined that no part could make its escape. The report of the explosion was so very feeble as scarcely to be heard. It certainly could not have been heard at the distance of 20 paces. It resembled the noise occasioned by breaking a very small glass tube. The quantity of powder made use of in this experiment was not more than 1-8th of what the chamber was capable of containing.

The next attempt was to measure the force of the elastic vapour thus confined. A hole was bored in the axis of one of the screws, or breech pins, which closed up the end of the barrel just described, and a piston of hardened steel was fitted into this hole, which was $\frac{2}{10}$ths of an inch in diameter. The end of the piston, which projected beyond the end of the barrel, was then caused to act upon a heavy weight, suspended as a pendulum to a long iron rod. It was hoped that a deduction of the pressure of the elastic vapour might be made from the length of the arc described by this pendulum; but the experiment was not found to answer, though various alterations and improvements were made in the apparatus before the

the method was abandoned. It was found almoſt impoſſible to prevent the eſcape of the elaſtic fluid by the ſides of the piſton; and the reſults of apparently ſimilar experiments were exceedingly different, and ſo uncertain, that the Count was often at a loſs to conjecture the reaſons of theſe extraordinary variations.

In order to elucidate theſe in a diſcurſive way, the Count proceeds to remark, that Mr. Robins's two aſſumed poſitions, namely, that the operation of gun-powder is performed by the rarefaction of a permanently elaſtic fluid, and that the whole of any charge is ſet on fire before the ball is ſenſibly moved from its place, are not to be admitted. On the contrary, from his own experiments, the Count is diſpoſed to refer the prodigious expanſion of fired gun-powder to the action of water in the ſtate of ſteam at a very elevated temperature; and he ſhews by various obſervations, ſeveral of which muſt be familiar to every one who is habituated to uſe this agent, that the accenſion of gun-powder is ſo far from being performed in an extremely minute portion of time, that it is in every inſtance gradual and progreſſive. But having found it impoſſible to meaſure the elaſtic force of fired gun-powder with any degree of preciſion by the methods already mentioned, he totally changed his plan of operations, and, inſtead of endeavouring to determine its force by cauſing the generated elaſtic fluid to act upon a moveable body through a determinate ſpace, he contrived an apparatus in which this fluid was made to act by a determinate ſurface againſt a weight, which being increaſed at pleaſure ſhould at laſt be ſuch as would be juſt able to confine it, and in that caſe would juſt counterbalance, and conſequently meaſure its elaſtic force.

A ſolid block of very hard ſtone, four feet four inches ſquare, was placed upon a bed of ſolid maſonry, which deſcended ſix feet below the ſurface of the earth. Upon this block of ſtone, which ſerved as a baſe to the whole machinery, was placed the barrel in which the exploſions were made. It was made of hammered iron 2,78 inches long, and 2,82 inches in diameter at its lower extremity, which was flat in order to reſt upon its ſupporter. Its bore was one quarter of an inch in diameter, 2,13 inches long, meaſured from the upper ſurface of the barrel, and it ended in a very narrow opening below, not more than 0,07 of an inch in diameter, and 1,715 inch long, which formed the vent or paſſage by which the fire is communicated to the charge. This paſſage, however, was not open below, but terminated in a projection from the centre of the bottom of the barrel about 0,45 of an inch in diameter, and 1,3 inch long, which formed the vent tube cloſed below. When this barrel is placed upon its ſtand, which is of gun metal, the vent tube paſſes, through a hole, into a cavity in the ſtand which has a lateral opening. Into this lateral opening a ball of red hot iron is introduced, having a proper cavity within for the reception of the vent tube, which it ſpeedily ignites, and ſets fire to the powder through the ſolid ſubſtance of the tube itſelf. The opening of the bore of the barrel, which is placed in the vertical poſition when in uſe, is cloſed by a ſolid hemiſphere of hardened ſteel, whoſe diameter was 1,16 inch, its plain ſide being downwards. It is confined laterally by three upright cylindrical pins, which allow it to riſe only in the vertical direction. Upon this hemiſphere is placed the weight made uſe of for confining the elaſtic fluid generated from the powder in its combuſtion. This weight, which in ſome of the experiments was a heavy twenty-four pounder placed vertically upon its caſcabel, being fixed to certain timbers, was capable of ſliding up and down in a vertical frame, and could be raiſed and lowered in the intervals between the experiments by a ſtrong lever.

The end of the barrel was covered with gold, in order to prevent as much as poſſible its being corroded by the elaſtic vapour, which, when the weight is not heavy enough to confine it, eſcapes between the end of the barrel and the flat ſurface of the hemiſphere; but even this precaution was not found to be ſufficient to defend the apparatus from injury. The ſharp edge of the barrel, at the mouth of the bore, was worn away almoſt immediately; and the flat ſurface of the hemiſphere, notwithſtanding it was of hardened ſteel, and very highly poliſhed, was ſenſibly corroded. The corroſion of the mouth of the bore, by which the dimenſions of the ſurface, upon which the generated elaſtic fluid acted, were rendered very uncertain, would alone have been ſufficient to have rendered all the Count's attempts to determine the force of fired gun-powder abortive, had he not found means to remedy the evil. The method he purſued for this purpoſe was as follows: Having provided ſome pieces of very good, compact ſole-leather, he cauſed them to be beaten upon an anvil with a heavy hammer, to render them ſtill more compact; and then, by means of a machine made for that purpoſe, cylindrical ſtoppers of the ſame diameter preciſely as the bore of the barrel, and 0,13 of an inch in length, (that is to ſay, the thickneſs of the leather) were formed of it; and one of theſe ſtoppers, which had previouſly been greaſed with tallow, being put into the mouth of the piece after the powder had been introduced, and being forced into the bore till its upper end coincided with the end of the barrel, upon the exploſion taking place, this ſtopper (being preſſed on the one ſide by the generated elaſtic fluid, and on the other by the hemiſphere loaded with the whole weight employed to confine the powder) ſo completely cloſed the bore, that when the force of the powder was not ſufficient to raiſe the weight to ſuch a height that the ſtopper was actually blown out of the piece, not a particle of the elaſtic fluid could make its eſcape. And in thoſe caſes in which the weight was actually raiſed, and the generated elaſtic fluid made its eſcape, as it did not corrode the barrel in any other part but juſt at the very extremity of the bore; the experiment by which the weight was aſcertained, which was juſt able to counterbalance the preſſure of the generated elaſtic fluid, was in no wiſe vitiated either by the increaſed diameter of the bore at its extremity, or by any corroſion of the hemiſphere itſelf; for as long as the bore retained its form and its dimenſions in that part to which the efforts of the elaſtic fluid were confined, that is, in that part of the bore immediately in contact with the lower part of the ſtopper, the experiment could not be affected by any imperfection of the bore either above or below.

The powder made uſe of in theſe experiments was of the beſt quality, being that kind called *poudre de chaſſe* by the French, and very fine grained, and it was all taken from the ſame parcel. Care was taken to dry it very thoroughly, and the air of the room in which it was weighed out for uſe was very dry. The weights employed for weighing the powder were German apothecary grains, 104,8 of which make 100 grains troy. The weights employed to confine the elaſtic vapour generated in the combuſtion of the powder, are reduced from Bavarian pounds, in which they were originally expreſſed, to pounds avoirdupois. The meaſures of length were all taken in Engliſh feet and inches. The experiments were all made in the open air, in the court-yard of the arſenal at Munich, and they were all made in fair

fair weather, and between the hours of nine and twelve in the forenoon, and two and five in the afternoon; but the barrel was always charged, and the extremity of the bore closed by its leather stopper in the room where the powder was weighed. In placing the barrel upon the block of stone, great care was taken to put it exactly under the centre of gravity of the weight employed to confine the generated elastic vapour. Upon applying the red-hot ball to the vent tube, and fixing it in its place by its lever, which supported it, the explosion very soon followed.

When the force of the generated elastic vapour was sufficient to raise the weight, the explosion was attended by a very sharp and surprisingly loud report; but when the weight was not raised, as also when it was only a little moved, but not sufficiently to permit the leather stopper to be driven quite out of the bore, and the elastic fluid to make its escape, the report was scarcely audible at the distance of a few paces, and did not at all resemble the report which commonly attends the explosion of gunpowder. It was more like the noise which attends the breaking of a small glass tube, than any thing else to which it could be compared. In many of the experiments in which the elastic vapour was confined, this feeble report attending the explosion of the powder was immediately followed by another noise totally different from it, which appeared to be occasioned by the falling back of the weight upon the end of the barrel after it had been a little raised, but not sufficiently to permit the leather stopper to be driven quite out of the bore. In some of these experiments a very small part only of the generated elastic fluid made its escape; in these cases the report was of a peculiar kind; and though perfectly audible at some considerable distance, yet not at all resembling the report of a musket. It was rather a very strong sudden hissing, than a clear, distinct, and sharp report.

Though it could be determined with the utmost certainty by the report of the explosion, whether any part of the generated elastic fluid had made its escape; yet for still greater precaution, a light collar of very clean cotton wool was placed round the edge of the steel hemisphere, where it reposed upon the end of the barrel, which could not fail to indicate, by the black colour it acquired, the escape of the elastic fluid, whenever it was strong enough to raise the weight by which it was confined, sufficiently to force its way out of the barrel.

Though the end of the barrel at the mouth of the bore was covered with a circular plate of gold, in order the better to defend the mouth of the bore against the effects of the corrosive vapour; yet, this plate being damaged in the course of the experiments (a piece of it being blown away), the remainder of it was removed, and it was never after thought necessary to replace it by another. When this plate of gold was taken away, the length of the barrel was of course diminished as much as the thickness of this plate amounted to, which was about $\frac{1}{400}$th part of an inch; but in order that even this small diminution of the length of the barrel might have no effect on the results of the experiments, its bore was deepened $\frac{1}{400}$th of an inch when this plate was removed, so that the capacity of the bore remained the same as before.

After making use of a great variety of expedients, the best and most convenient method of closing the end of the bore, and defending the flat surface of the steel hemisphere from the corroding vapours, was found to be this: First, to cover the end of the bore with a circular plate of thin oiled leather; then to lay upon this a very thin circular plate of hammered brass, and upon this brass plate the flat surface of the hemisphere. When the elastic fluid made its escape, a part of the leather was constantly found to have been torn away, but never in more places than one; that is to say, always on one side only.

What was very remarkable, in all those experiments in which the generated elastic vapour was completely confined, was the small degree of expansive force which this vapour appeared to possess after it had been suffered to remain a few minutes, or even only a few seconds, confined in the barrel; for upon raising the weight by means of its lever, and suffering this vapour to escape, instead of escaping with a loud report, it rushed out with a hissing noise, hardly so loud or so sharp as the report of a common air-gun; and its effects against the leathern stopper, by which it assisted in raising the weight, were so very feeble as not to be sensible. Upon examining the barrel, however, this diminution of the force of the generated elastic fluid was easily explained; for what the Count thinks was undoubtedly in the moment of the explosion in the form of an elastic fluid, was now found transformed into a solid body as hard as a stone. It may easily be imagined how much this unexpected appearance excited his curiosity; but, intent on the prosecution of the main design of these experiments, the ascertaining the force of fired gun-powder, he was determined not to permit himself to be enticed away from it by any extraordinary or unexpected appearances or accidental discoveries, however alluring they might be; and faithful to this resolution, he postponed the examination of this curious phenomenon to a future period, and since that time he has not found leisure to engage in it. He thinks it right, however, to mention such cursory observations as he was able in the midst of his other pursuits to make upon this subject.

This matter was very hard, and so firmly attached to the inside of the barrel, and particularly to the inside of the upper part of the vent tube, that it was always necessary, in order to remove it, to make use of a drill, and frequently to apply a considerable degree of force. It was of a black colour, or rather of a dirty grey, which changed to black upon being exposed to the air, had a pungent acrid alkaline taste, and smelt like liver of sulphur. It attracted moisture from the air with great avidity. Being moistened with water, and spirit of nitre being poured upon it, a strong effervescence ensued, attended with a very offensive and penetrating smell. Nearly the whole quantity of matter of which the powder was composed seemed to have been transformed into this substance *; for the quantity of elastic fluid which escaped upon removing the weight was very inconsiderable. But this substance was no longer gun-powder; it was not even inflammable.

What change had it undergone? demands our author,—What could it have lost? It is very certain the barrel was considerably heated in these experiments. Was this occasioned by the caloric, disengaged from the powder in its combustion, making its escape through the iron? And is this a proof of the existence of caloric considered as a fluid *sui generis*; and that it actually enters into the composition of inflammable bodies, or of pure air, and is necessary to their combustion? He dares not take upon him to decide upon such important questions. He once thought that the heat acquired by a piece of ordnance in being fired arose from the vibration or friction of its parts, occasioned by the violent blow it received in

* It is much to be regretted that no experiment was made of the weight of this substance afforded by a given weight of gunpowder. N.

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the explosion of the powder, but acknowledges fairly that it does not seem to be possible to account in a satisfactory manner for the very considerable degree of heat which the barrel acquired in these experiments, merely on that supposition.

That this hard substance found in the barrel, after an experiment in which the generated elastic vapour had been completely confined, was actually in a fluid or elastic state in the moment of the explosion, is evident, as he thinks, from hence: that in all those cases in which the weight was raised, and the stopper blown out of the bore, nothing was found remaining in the barrel. It was very remarkable, that this hard substance was not found distributed about in all parts of the barrel indifferently, but there was always found to be more of it near the middle of the length of the bore than at either of its extremities, and the upper part of the vent tube in particular was always found quite filled with it. It should seem therefore, says he, that it attached itself to those parts of the barrel which were soonest cooled; and hence the reason, most probably, why none of it was ever found in the lower part of the vent-tube, where it was kept hot by the red-hot ball by which the powder was set on fire.

He found by a particular experiment, that the gunpowder made use of, when it was well shaken together, occupied rather less space in any given measure than the same weight of water; consequently when gunpowder is fired in a confined space which it fills, the density of the generated elastic fluid must be at least equal to the density of water. The real specific gravity of the solid grains of gunpowder, determined by weighing them in air and water, is to the specific gravity of water as 1,868 to 1000. But if a measure whose capacity is one cubic foot hold 1000 ounces of water, the same measure will hold just 1077 ounces of fine-grained gunpowder, such as was used in these experiments, that is to say, when it is well shaken together. When it was moderately shaken, together its weight was exactly equal to that of an equal volume, or rather measure, of water. But it is evident that the weight of any given measure of gunpowder must depend much upon the forms and sizes of its grains. He adds one observation more relative to the particular appearances which attended the experiments, in which the elastic vapour, generated in the combustion of gunpowder, was confined, and that is with regard to a curious effect produced upon the inferior flat surface of the leathern stopper where it was in contact with the generated elastic vapour. Upon removing the stopper, its lower flat surface appeared entirely covered with an extremely white powder, resembling very light white ashes, but which almost instantaneously changed to the most perfect black colour upon being exposed to the air.

The sudden change of colour in this substance, upon its being exposed to the air, led him to suspect that the solid matter found in the barrel was not originally black, but that it became black merely in consequence of its being exposed to the air. The dirty grey colour it appeared to have, immediately on being drilled out of the cavity of the bore where it had fixed itself, seems to confirm this suspicion. An experiment made with a very strong glass barrel would not only decide this question, but would most probably render the experiment peculiarly beautiful and interesting on other accounts.

The Count thinks a barrel of glass might be made strong enough for the experiment, if it could withstand the sudden heat, and on the whole seems disposed to think the trial worth making.

All the parts of the operation being ready, it was in the autumn of the year 1792 that the first experiment was made.

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The barrel being charged with 10 grains of powder, its contents when quite full amounting to about 28 grains, and the end of the barrel being covered by a circular piece of oiled leather, and the flat fide of the hemifphere being laid down upon this leather, and a heavy cannon, a twenty-four pounder, weighing 8081 pounds avoirdupois, being placed upon its cafcabel in a vertical pofition upon this hemifphere, in order to confine by its weight the generated elaftic fluid, the heated iron ball was applied to the end of the vent-tube; and after waiting but a very few moments in anxious expectation of the event, Count Rumford had the fatisfaction of obferving that the experiment had fucceeded. The report of the explofion was extremely feeble, and fo little refembling the ufual report of the explofion of gunpowder, that the by-ftanders could not be perfuaded that it was any thing more than a cracking of the barrel, occafioned merely by its being heated by the red-hot ball: yet, as the Count had been taught by the refult of former experiments not to expect any other report, and as he found, by putting his hand upon the barrel, that it began to be fenfibly warm, he was foon convinced that the powder muft have taken fire; and after waiting four or five minutes, upon caufing the weight which refted upon the hemifphere to be raifed, the confined elaftic vapour rufhed out of the barrel. Upon removing the barrel and examining it, its bore was found to be choked up by the folid fubftance already defcribed, and from which it was with fome difficulty that it was freed, and rendered fit for another experiment. The extreme feeblenefs of the report of the explofion, and the fmall degree of force with which the generated elaftic fluid rufhed out of the barrel upon removing the weight which had confined it, had infpired the affiftants with no very favourable idea of the importance of thefe experiments. It was feen indeed from the beginning by their looks, that they thought the precautions to confine fo inconfiderable a quantity of gunpowder as the barrel could contain, perfectly ridiculous; but the refult of the following experiment taught them more refpect for an agent, of whofe real force they had conceived fo very inadequate an idea.

In this fecond experiment, inftead of 10 grains of powder, the former charge, the barrel was now quite filled with powder, and the fteel hemifphere, with its oiled leather under it, was preffed down upon the end of the barrel by the fame weight as was employed for that purpofe in the firft experiment, namely, a cannon weighing 8081 pounds. The barrel (which, though fimilar to it in all refpects, was not the fame that has already been defcribed) was made of the beft hammered iron, and was of uncommon ftrength. Its length was 2¾ inches; and though its diameter was alfo 2¾ inches, the diameter of its bore was no more than ¼ of an inch, or lefs than the diameter of a common goofe quill. The length of its bore was 2.15 inches. Its diameter being 2¾ inches, and the diameter of its bore only ¼ of an inch, the thicknefs of the metal was 1¼ inch; or it was 5 times as thick as the diameter of its bore. The charge of powder was extremely fmall, amounting to but little more than $\frac{1}{10}$th of a cubic inch; not fo much as would be required to load a fmall pocket piftol, and not one-tenth part of the quantity frequently made ufe of for the charge of a common mufket. This inconfiderable quantity of gunpowder, when it was fet on fire by the application of the red-hot ball to the vent tube, exploded with fuch inconceivable force as to burft the barrel afunder in which it was confined, notwithftanding its enormous ftrength, and with fuch a loud report as to alarm the whole neighbourhood. The fpectators turned pale with affright and aftonifhment, and it was fome time before they could recover themfelves. The barrel was

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not only completely burst asunder, but the two halves of it were thrown upon the ground in different directions; one of them fell close by the Count's feet, as he was standing near the machinery to observe more accurately the result of the experiment.

From some former experiments with small iron wire, Count Rumford deduces, that a cylinder of that metal, whose transverse section is one inch, would be able to sustain 63466 pounds without being broken. But for greater accuracy, he caused several small pieces to be cut out of the solid half of the barrel which was broken. From four experiments with pieces whose diameters were respectively in thousandth parts of an inch, fifty, sixty, sixty-six and seventy-six parts, which were broken by a direct pull, it was found that the medium weight required to break one inch of this iron was in pounds avoirdupois 63173. The variations in the result were such as to render the second figure (expressing thousands) uncertain. Our author computes the resistance of the barrel from the area of the surface of fracture, namely, $6\frac{1}{2}$ inches, by using this as a simple multiplier, which gives $410624\frac{1}{2}$ pounds. And this force being considered as applied to a surface of half an inch, which is the area of a longitudinal section of the bore of the barrel, and reduced into atmospheres, by allowing 15 pounds avoirdupois for the medium pressure of the air upon a square inch, gives 54750 atmospheres for the measure of the force exerted for overcoming such a resistance. This force, enormous as it may appear, is supposed by the Count to be short of the real initial force of the elastic fluid generated in the combustion of gunpowder, because he thinks it probable that the barrel was in fact burst before the generated elastic fluid had exerted all its force. On this head of probabilities I would venture to make a remark, that the iron in the barrel may perhaps be considered as not exactly in the situation of the pieces which our author broke in his engine by a direct pull. For in these last it may reasonably be supposed, that the whole pressure of cohesion was equally acted on till the moment of fracture; whereas in the barrel the fracture may, from the spring of the metal and other circumstances, be supposed to have proceeded from the inner to the outer surface by a progression or tearing asunder, which, however swift, must have rendered the division more easy. But at all events, whatever may be the force of this remark, it can in no respect invalidate such conclusions as were drawn from the actual lifting of great weights by raising the hemispherical cover of the barrel.

A set of experiments were instituted with an apparatus of the kind here described, for the purpose first of determining the expansive force of the elastic vapour generated in the combustion of gunpowder in its various states of condensation, and the ratio of its elasticity to its density; and secondly, of measuring by one decisive experiment the utmost force of this fluid in its most dense state; that is to say, when the powder completely fills the space in which it is fired, and in which the generated fluid is confined. A numerous series of experiments are tabulated in the Count's Memoir.

The dimensions of the barrel made use of in those experiments were as follow:

Diameter of the bore at its muzzle = 0,25 of an inch.

Joint capacities of the bore and of its vent-tube exclusive of the space occupied by the leathern stopper = 0,08974 of a cubic inch.

Quantity of powder contained by the barrel and its vent-tube when both were quite full, exclusive of the space occupied by the leathern stopper, 25,641 German apothecaries grains, = $24\frac{1}{2}$ grains troy.

The Table, in the original, contains a great number of experiments, in which the weight or piece of ordnance was either not raiſed, or was thrown up with a loud report. Thoſe experiments, in which the weight juſt moved without a report, are obviouſly ſuch as indicate the elaſtic force to have been equal to the preſſure. I have accordingly ſelected theſe only in the following table as ſuitable to form the baſis of computations. The day, hour, and minute when each experiment was made, are likewiſe to be found in the original, though it did not ſeem neceſſary to inſert them in this abridgment. The expreſſion of the weight on atmoſpheres is grounded on the aſſumption, that the mean preſſure of the atmoſphere is equal to fifteen pounds avoirdupois upon a ſquare inch.

TABLE I.—*Experiments on the Force of Fired Gunpowder.*

State of the Atmoſphere.		The Charge of Powder.		Weight employed to confine the elaſtic fluid.		State of the Atmoſphere.		The Charge of Powder.		Weight employed to confine the elaſtic fluid.	
Thermometer. F.	Barometer.	German Apot. Grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmoſpheres.	Thermometer. F.	Barometer.	German Apot. grs.	In 1000 parts of the capacity of the bore.	In lbs. avoirdupois.	In atmoſpheres.
Deg.	Eng.Inch.	Grs.	Parts.	lbs.		Deg.	Eng.Inch.	Grs.	Parts.	lbs.	
37	28,56	6	234	504,8	685,6	32	28,2	10	390	1387,5	1884,3
57	28,37	1	39	57,4	77,86	32	28,2	11	429	1634	2219
34	28,1	2	78	134,2	182,3	36	28,34	12	460	1895,1	2573,7
48	28,31	3	117	212,24	288,2	42	28,3	13	507	2422	3288,3
50	28,36	4	156	281,57	382,4	43	28,31	14	546	2951	4008
48	28,35	5	195	413,27	561,2	43	28,31	15	585	3477	4722,5
59	28,34	7	273	597,66	811,7	70	28,2	16	624	5220	7090
50	28,32	8	312	857,64	1164,8	68	28,3	18	702	8081	10977
50	28,32	9	351	1142,3	1551,3						

In the laſt experiment, with near eleven thouſand atmoſpheres, the weight was raiſed with a very ſharp report, louder than that of a well-loaded muſket; and in the experiment immediately following with the ſame charge, but with the addition of ſix hundred and nineteen pounds to the weight, the vent-tube of the barrel burſt. This experiment was the 85th of the Table, probably the 85th time of heating the tube.

(To be concluded in the next Number.)

VII.

Obſervations and Experiments on Steel, reſembling that of Damaſcus; with an eaſy Teſt for determining the uniform Quality of Steel before it is employed in Works of Delicacy or Expence.

IN the infancy of ſociety the hardeſt bodies, ſuch as ſtones, and certain kinds of wood, were ſelected and uſed for cutting inſtruments, and ſtill are applied to that purpoſe in ſeveral parts of the world. Theſe materials were ſucceeded by copper, hardened by a mixture of tin, of which numerous weapons yet remain in the cabinets of the curious. And laſtly, ſteel, fteel, whether obtained directly from the ore, or by cementation of malleable iron, has defervedly taken place of every other article, on account of the united qualities of tenacity and hardnefs *. When the fword was the chief weapon of war, it muft have been an object of grea intereft and demand to give to its blade a durable keen edge, and a degree of firmnefs or ftrength, which, without rendering it unwieldy, fhould enfure the warrior againft expofure to the fatal accident of its breaking in the act of combat. The fabres of Damafcus have been famous for ages, and ftill bear a great price in the Eaft; but we have no decided account of the manner in which this fteel is manufactured or made up. Some years ago I was favoured with the poffeffion of a true blade of this kind for a few days, which, if my recollection be accurate, had coft the poffeffor twelve guineas at Conftantinople. I know the fum was not lefs than this. As I was not permitted to make any experiments upon it, I could only ground my procefs upon reafoning from its external appearance and obvious qualities.

It had a dull grey or blueifh appearance, was fcarcely harder than common fteel from the forge, was not eafily bended, and when bended had no fpring to recover its figure. Its back was fmooth, as were alfo two narrow floped furfaces which formed its edge under an angle of about 40 degrees; but its flat fides were every where covered with minute waving lines in maffes in all directions, not croffing each other, and, for the moft part, running in the direction of its length. The lines were in general as fine as harpfichord wire, not extremely well defined nor continued; and their diftinction from each other was effected by no perceptible indentation of the furface, but rather by the fucceffion of parts differing in the degree of polifh or brightnefs. No one, upon infpection of this furface, would for a moment have imagined or allowed that it could have been done by engraving or etching, as the falfe blades are damafked. I was informed that if any part of this blade were made fmooth by grinding or whetting, the wavy appearance, called the water, could be again produced by means of lemon juice; and that its excellencies were, that it could be depended upon not to break, and that it would cut deeper into a foft fubftance, fuch as a pack of wool, or into flefh, than any other kind of blade.

From thefe circumftances, as well as from the price, I was induced to think that the blade was compofed of fteel and iron, and that the procefs of forging was fuch as greatly to enhance the coft, by the labour and management it might require. For if we fuppofe the pieces to be united together at the welding heat, and then forged or drawn out, it is certain that no fmall degree of fkill and care would be required to render all the parts found, and at the fame time preferve the fteel and iron in poffeffion of their characteriftic properties. Too great a heat would probably render the whole mafs more uniform than is confiftent with the fubfequent production of the water or wavy appearance. In my attempt to imitate this fteel, I endeavoured to fubftitute a mechanical contrivance in the place of this fuppofed careful forging.

I caufed a cylindrical hole of about one inch in diameter to be bored through a piece of caft iron, the lower part of which could be fo placed upon an anvil as to clofe one end of the hole. A forged iron plug was made nearly to fit the cylindrical hole, but confiderably longer. Equal weights of German fteel and Swedifh iron, both in filings, were then well

* Philofophical Journal, I. 381.

mixed

mixed with oil, and wrapped in a paper, which had before been rolled upon the plug, and consequently fitted the cylinder. The ends of the paper were neatly folded; and the whole mass being then put into the cast-iron cylinder placed upon the anvil, a few blows were given by driving the plug into the hole with a heavy hammer. By this means the mass of filings, when thrust out of the cylinder, was compact and manageable. It was then placed in a charcoal fire, and urged to a welding heat by the double bellows. Thence it was taken with the tongs; again hastily put into the cylinder, and hammered by means of the plug and the heavy hammer. When it was taken out, the whole was found to be consolidated; but upon forging it into a plate, a considerable portion flew off in a crumbly form. The plate, however, was filed up, smoothed, and examined.

Its colour presented nothing remarkable. When weak nitrous acid was poured upon it, it became mottled in consequence of the numerous small black spots which appeared upon the particles of steel, while those of iron remained clean. On the nitrous acid being washed off, the surface appeared wavy like the Damascus steel, but scarcely at all fibrous; doubtless because the solid had not been drawn out by forging. An attempt was made to harden it by ignition and cooling in water; but it still remained soft enough to be cut with the graving tool, the point of which did not indicate any difference in that respect between the parts of iron and of steel, though it is very probable such a difference did really exist.

I infer, therefore, that the Damascus steel is in fact a mechanical mixture of steel and iron; that it is incapable of any considerable degree of hardness, and consequently is in no danger of breaking from its brittleness; that its tenacity is ensured not only from the admixture of iron, but likewise from the facility with which its soundness may be ascertained throughout, by the same process which exhibits the water or fibrous appearance: and, lastly, that the edge of a weapon formed of this material must be rough, on account of the different resistance which the two substances afford to the grindstone, in consequence of which it will operate as a saw, and more readily cut through yielding substances than such cutting tools as are formed of a more uniform substance.

This experimental enquiry directed my attention to a method of ascertaining the uniformity of texture in iron or steel, which perhaps may have been noticed by others, but is certainly unknown in most manufactories, though I have found it of great utility. If a weak acid, for example the nitrous, which I have usually taken in a very diluted state, be applied to the face of iron or steel previously cleaned with the file, or with emery paper, the parts which contain the greatest portion of carburet of iron (or plumbago) immediately shew themselves by their dark colour. It very frequently happens that articles of considerable value, intended to be fabricated in iron or steel, are not known to be defective until much expence has been laid out in manufacturing them. A piece of iron, which has a vein of steel running through it, as is too often the case, will require at least three times the labour and care to turn it in the lathe, which would have been demanded by a piece of greater uniformity. Steel which abounds with spots, or veins, or specks called pins, may be fashioned completely, and will not shew its defects, until the final operation when the attempt is made to polish it. Other articles, such as measuring screws, blades of sheers, fine circular cutters, &c. either bend in the hardening, from the difference of expansion, or resist the tool when wrought in the tempered state, or exhibit other incurable defects when they come to be tried; which the test by nitrous acid would have indicated before

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any expence had been incurred. In thefe, and in numberlefs other inftances, it would have been incomparably more advantageous to have rejected the material upon the firft trial, rather than have proceeded to the very expenfive procefs of manufacturing the article, and then finding it of no value. By this fimple expedient I have feen bars of fteel as full of veins and irregularities as wood, and have been enabled to felect the beft and moft uniform pieces for works of the greateft delicacy; whereas, before I thought of this mode of trial, I have very often had the mortification to fail in the laft ftage of experimental proceffes, upon which much coft and labour had been beftowed.

VIII.

On the Irritability of the Pollen of Plants. With an Account of a Compofition for clofing Wide-mouthed Veffels.

To Mr. NICHOLSON, Editor of the Philofophical Journal.

SIR,

A FEW fummers ago a friend of mine chanced to make a curious difcovery relative to the irritability of the pollen or fecundating duft of plants; which, as I know not that it has yet met the eye of the public, you are at liberty, if thought fufficiently interefting, to communicate through the channel of your Journal.

Whilft making obfervations with one of Adams's compound microfcopes on the figure of the particles of pollen collected from different plants, he applied a drop of water to a fmall group (which was placed on the glafs plate of the ftage), in order to increafe the bulk; and, by that means, more accurately determine the figure of the particles. Happening, at the fame time, to have a fmall phial of fpirits of wine at hand, he next tried a drop of that liquid to frefh pollen, when he was agreeably furprifed with feeing it produce a quick gyration, as well as a darting of the particles backward and forward in the drop: this motion continued for a few minutes, and then gradually fubfided; the particles unravelling into a continuous filmy thread, and at other times appearing to burft and emit a multitude of particles * infinitely fmaller, and which conglomerated together, leaving the capfular veffel empty, or only filled with the fpirit.

The fpeedy evaporation of the fpirit appearing at times to prevent the completion of the phenomenon, he afterwards tried, and with better fuccefs, a little common brandy. The motions, in this cafe, were continued longer, and the appearances were more complete. It will appear that thefe do not proceed from the mere evaporation of the liquid, becaufe thofe particles which have been once faturated, though from fome caufe they may not have burft or unravelled, are incapable of excitation, at leaft in any fimilar degree, by the application of more fpirit.

Different kinds of pollen were made ufe of for this experiment; but that of the "Cactus flagelliformis" was moftly employed, as affording the moft ftriking appearance, on account of the magnitude of its particles.

* At leaft it prefented this appearance at times to my eye; which, I doubt not, has been the occafion of its obtaining from fome botanifts the appellation of the fpermatic aura of pollen.

I avoid entering into any theoretical obfervations on the aforefaid phenomenon, until more fully illuftrated by experiment, and content myfelf with merely announcing it to the curious inveftigators in the paths of philofophical botany.

I take this opportunity of communicating another method, or at leaft another compofition for cementing wide-mouthed veffels, in addition to thofe mentioned in your Journal for September. It is a mixture of fpermaceti and caoutchouc: the former to be melted in a ladle, and the latter added in fmall bits, which will be gradually but effectually diffolved, and the compound forms a cement perfectly air-tight when poured on fluid and fuffered to cool. It is alfo, I believe, very little, if at all, attacked by acids, and on that account might be of confiderable ufe in philofophical laboratories. The quantity of caoutchouc may be varied according to the intended purpofe, or any hardening fubftance, as maftic, added if required. If this latter communication fhould prove of any ufe to you perfonally, or to your chemical friends, it will amply gratify

Your obliged reader,

Dec. 1ft, 1797. AMICUS.

IX.

Experimental Refearches to afcertain the Nature of the Procefs by which the Eye adapts itfelf to produce diftinct Vifion.

[Continued from page 313.]

IN the former part of this article, refpecting the adjuftment of the eye, a confiderable number of original experiments and remarks were related from communications to the Royal Society. Thefe have for their objects the determination, whether the cryftalline humour by its fuppofed mufcularity, the external mufcles by their greater or lefs preffure upon the orbit, or the cornea by a variation in its curvature, be each the fole or chief agent in caufing the pencils of light from vifible objects to arrive in all cafes at thofe foci which diftinct vifion requires. I fhall now proceed to give the fubftance of the other Memoirs publifhed by the Royal Society upon the fame fubject.

In the volume for 1795, page 263, I find a paper of Obfervations on the Eyes of Birds, by Mr. Pearce Smith. This gentleman relates, that, in the year 1792, he obferved, while diffecting the eyes of birds, an irregular appearance of the fclerotica in that part which furrounds the cornea, and which in them is general y flat. On a more minute examination, it appeared to be fcales lying over each other, and which appeared capable of motion on each other. On inveftigating this ftructure, the fcales were found to be of bony hardnefs, at leaft much more fo than any other part of the fclerotica. On the infide of the fclerotic coat there was no appearance of thefe fcales. Tendinous fibres were detected fpreading over the fcales, terminating in the four recti mufcles of the eye, fo that upon the contraction of thofe mufcles the fcales would be moved.

By reflecting on the probable ufes of this conformation, Mr. Smith deduced, that the internal capacity of the eye will be greateft when the recti mufcles are not in action; and the feveral circles of fcales being fuffered to repofe upon each other in fucceffion, are upon

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the whole, nearer the anterior extremity of the optic axis, than at any other time; that when by voluntary effort thofe mufcles are made to act, the feveral circles of fcales will be drawn back, and diminifh the periphery of the eye at their refpective pofitions in this laft fituation; and that confequently the internal capacity of the organ being rendered lefs, the moft elaftic part, namely, the cornea, will yield to the preffure of the included humour, and become more convex. This he concludes muft render fmall objects, near the animal, very diftinct.

On the relaxation of the mufcles, the original flatnefs of the cornea will be reftored by the elafticity (as he affirms) of the fclerotica; and he proceeds to make remarks on the advantage the animal fyftem derives from elafticity being oppofed to mufcular force, giving nearly the fame inftances as were offered with regard to the elafticity of the cornea a few months before by Mr. Home *. He does not explain in direct terms the peculiar excellence this imbricated fyftem of fcales may be imagined to poffefs, but thinks it particularly neceffary to birds, which, without it, would, as he fays, be continually expofed to dafh themfelves againft the trees of a thick foreft. He conceives alfo, that the extraordinary alteration in the focus of the eye of an eagle in almoft an inftant of time when it darts from the upper air and feizes an object on the ground, and the purfuit and feizure of a gnat or fmall fly by a fwallow, muft require the aid of this apparatus, which in them is very diftinct. But with regard to thefe particulars, or final purpofes, fomewhat more of inveftigation feems to be wanting. For it is certain, that no bird will dare to fly rapidly through a thick wood, but either pafs leifurely from branch to branch, or foar above the tops of the trees when fpeed is the object of their aim. And however confiderably the bony circles may be fhewn to add to the range of adjuftment for near objects, it does not appear that any greater fuddennefs of change, in the eagle and other birds of the fame fpecies, is wanted, than is within the power of animals not poffeffing that ftructure; becaufe the adjuftment from remote to near objects is effected in the human, and probably every other eye, in a time incomparably fhorter than that of an eagle's defcent upon his prey. Neither do fwallows purfue gnats or flies, but fcud through the air with at leaft twelve times the velocity of thofe fmall creatures, which they probably catch by opening their mouths without the leaft ufe of fight.

The mention of fwallows, and the probability of winged animals ftriking themfelves againft trees in their flight, brings to my recollection fome particulars which do not feem altogether foreign to the prefent fubject. In a certain ftate of the atmofphere, it is ufual for fwallows to fly rapidly along near the face of a row of houfes, at the diftance of three or four inches from the wall. Now I take it for granted, that they cannot poffibly diftinguifh the houfes during their courfe, on account of the rapid angular motion; and I doubt very much whether they fee objects before them with any precifion—not to mention the probability that the eye itfelf would be immediately dried if the nictitating membrane were not almoft conftantly upon it. With thefe reflections, on an occafion of this kind, it did not feem furprifing that many of thefe birds came fo near my face, while obferving them from a window, that they fcarcely feemed to have noticed me. But the moft remarkable circumftance was, that they did in fact turn afide fo as to avoid ftriking me; though in fome inftances fo near that I

* Philof. Tranf. 1795, p. 20; or this Journal, p. 313.

both heard and felt the action of the air to which they gave motion. On more attentive conſideration, however, it appeared to me, that this motion of the air is the very inſtrument which by its re-action may warn them of the preſence and poſition of an obſtacle the inſtant they approach it; which they may habitually and ſpeedily avoid. For, as the intelligent blind are ſaid to know, by the motion of air and its echo, the ſize of rooms, the width of ſtreets, and other local attributes, which are commonly aſcertained by the ſenſe they are deprived of; and as boats move more ſluggiſhly in ſhallow or contracted channels; and air itſelf cannot be forced with rapidity through long tubes;—in all which circumſtances the re-action muſt be very conſiderable; ſo, on the other hand, it can ſcarcely be doubted, that the object of ſuch re-actions, if capable of perception, would be ſufficiently aware of their preſence or abſence. I ſuppoſe the re-action of the air daſhed againſt my face by the ſwallow, and the noiſe I heard, were as perceptible and as impreſſive to the bird as to myſelf; and from this reaſoning I am leſs diſpoſed to wonder at the reſult of the cruel experiment performed in Italy a few years ago upon bats. As I have ſeen the notice only in a voluminous periodical publication to which I cannot now turn, I muſt ſimply relate, that bats have been thought to poſſeſs a ſixth ſenſe, becauſe, when the eyes of one of theſe animals were diſſected out, it flew round a room with the ſame ſpeed and preciſion as before, avoiding the walls and obſtacles as readily as if ſtill poſſeſſed of ſight. If my conjectures have any foundation, theſe creatures did never uſe the eye to warn them of impediments to their flight, but had conſtantly attended to the re-action of the fluid in which they moved; which would be very different in the vicinity of an obſtacle compared with that in open ſpace. From the re-action of a direct obſtacle, they would turn to the ſide where the preſſure was leaſt, or the ſpace moſt open; and if the obſtacle were oblique, it may eaſily be imagined that the requiſite deviation from their courſe would be equally obvious.

To return to Mr. Smith's Memoir. He was led from the examination of the eyes of birds to thoſe of quadrupeds, and found by tearing and diſſecting that the recti muſcles terminate in the cornea; in which, and in his inferences, he entirely agrees with Mr. Home in the paper before-mentioned *.

The Croonian Lecture, read before the Royal Society in November 1795, was written by Everard Home, Eſq. and contains a proſecution of the enquiry reſpecting viſion, of which an abridgement has already been given.

The explanation of the adjuſtment of the eye by an aſſumed change in the radius of the cornea being different from the theories before formed on that ſubject, it was thought right to put it to the teſt of every experiment which might appear likely to refute or confirm the obſervations already made. The reader has ſeen that a perceptible variation in the figure of this part of the organ of ſight, was aſcertained by obſerving its outline by means of a microſcope. Another method ſuggeſted itſelf; namely, that if the convexity of the cornea were increaſed to a certain degree, it could be meaſured by applying an achromatic microſcope, with a divided eye-glaſs micrometer, to view the image in the virtual focus of its ſurface. The firſt ſtep in this experimental proceſs was to aſcertain, by experiments upon convex mirrors, what difference of curvature could be decidedly obſerved by an apparatus of this kind. Two convex mirrors, one of $\frac{4}{10}$ths of an inch focus, the other $\frac{5}{10}$ths, had their

* Philoſ. Tranſ. or this Journal as laſt quoted.

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flat furfaces made rough and blacked, to prevent an image being feen from both furfaces. One of thefe was ftuck upon a piece of wood, directly oppofite a window, at 12 feet diftance from it. A board, four feet long, and fix inches broad, was placed perpendicularly againft the fafh of the window, and its image reflected from the mirror upon the object glafs of an achromatic microfcope, with a divided eye-glafs micrometer.

The two images were feparated by means of the divided eye-glafs till their furface of contact, which appears like a black line, was rendered as fmall as poffible. When this effect was produced on the images from the mirror of $\frac{4}{10}$ths of an inch focus, that mirror was removed and the other put in its place. The contact of the two images, which before appeared like a line, had now acquired confiderable breadth, correfponding exactly to the difference between the convexities of the mirrors.

When the fame experiment was repeated upon the eye, there was a perceptible, though extremely fmall, change in the micrometer at firft when the eye was frefh. This was not however feen afterwards, and the eye very foon became fo much fatigued, that it was neceffary to defift. It was found that every time the eye adapted itfelf to different diftances it became neceffary to alter the diftance of the object-glafs of the microfcope from the cornea.

This experiment was repeated on four different days with the fame refult; namely, a change in the micrometer at firft, which in the fubfequent trials could not be detected. Two fuppofitions offered themfelves as likely to account for this effect, namely, a motion of the head forwards, or an action of the mufcles of the head itfelf; but, as the author remarks, this effect ought in the firft cafe to have been more frequent, the greater the fatigue of the eye; and the latter circumftance would have produced a contrary refult. It appeared, therefore, that the effect on the micrometer did really arife from a change in the cornea, though too fmall to be detected with certainty in this way.

With two other mirrors, of which the focal diftances were $\frac{4}{10}$ths and $\frac{408}{1000}$ths of an inch, the difference between the fize of the images was juft vifible in the micrometer; but it did not appear probable that the fame difference would have been vifible had the mirror not been perfectly at reft. From the unfteadinefs of the eye, it might therefore be reafonably fuppofed that a change of this magnitude might take place in the cornea without being diftinctly feen.

To give an idea of the fhort time that a part can remain nicely adjufted by mufcular action, the author points out an experiment which any one may make upon himfelf. Let him take a glafs fpirit level, and reft one end of it on a table, fupporting the other with his hand, and endeavour to keep the air bubble in the middle. If the hand is very fteady, the bubble may be kept nearly in its place, but not exactly fo; for it will undulate in correfponence with the action of the mufcles, making up for want of fteadinefs, by fhort motions, in contrary directions.

From thefe experiments, the change in the curvature of the cornea could not be more than $\frac{1}{125}$th part of an inch, as any greater quantity would probably have been diftinctly feen in the micrometer. This, however, is ftill more than was afcertained by the former experiments, which made it to exceed $\frac{1}{800}$th part of an inch.

This change in the cornea, at firft view of the fubject, appeared fufficient to account for the adjuſtment of the eye, and may probably be ſufficient when the lens is removed, but not when the eye is entire. The author therefore proceeded to examine what alteration the figure of the human eye might be ſuſceptible of when air was thrown into its cavity, through the optic nerve, ſo as to diſtend its coats. From the experiments it was found that the axis of viſion was lengthened a ſmall degree in the eye of a boy ſix years old, forty-five minutes after death, while the tranſverſe diameter and axis from the optic nerve were ſhortened. A ſimilar though leſs effect was obſerved in the eye of a man 25 years old, one hour after death; but no alteration took place, by the like treatment, in the eye of a man 50 years old, [illegible] minutes after death. The object of theſe experiments does not clearly appear, as there is no ſuſpicion of an action of this kind in the living ſubject; but they appear to ſhew that the cornea is the part moſt variable from elaſticity; and when the preſſure is made laterally, and from without, the elongation muſt be ſtill greater, the action of the ſtraight muſcles being the moſt advantageous that could be imagined for this purpoſe.

This lateral preſſure, Mr. H. obſerves, will not only elongate the eye, and increaſe the convexity of the cornea but will produce an effect upon the cryſtalline lens and ciliary proceſſes, puſhing them forward in proportion as the cornea is ſtretched. For, as theſe proceſſes form a complete ſeptum between the vitreous and aqueous humours, the cavity of the aqueous humour will be always of the ſame ſize, and the cornea and lens at the ſame diſtance from each other; in the accurate production of which effects, he ſuppoſes it likely that the ciliary proceſſes may operate by muſcular action; an opinion which other facts in the courſe of the lecture tend to confirm.

The reſult of this enquiry, which was not carried on, as he remarks, in ſupport of any particular theory, but with the ſole view of diſcovering the truth, appears to be, that the adjuſtment of the eye is produced by three different changes in that organ; an increaſe of curvature in the cornea, an elongation of the axis of viſion, and a motion of the cryſtalline lens. Theſe changes, in a great meaſure, depend upon the contraction of the four ſtraight muſcles of the eye. Mr. Ramſden, from computations grounded on the principles of optics and general ſtate of the facts, eſtimates, that the increaſe of curvature of the cornea may be capable of producing one-third of the effect, and that the change of place of the lens, and elongation of the axis of viſion, ſufficiently account for the other two-thirds of the quantity of adjuſtment neceſſary to make up the whole.

After this explanation of the mode by which the axis of viſion can be elongated, and the convexity of the cornea increaſed in the human eye, for the purpoſe of its adjuſtment, Mr. Home was deſirous of applying theſe obſervations to the eyes of other animals.

Quadrupeds in general muſt have their eyes fitted to ſee very near objects, as many of them collect their food with their mouths, in which action the objects are brought very cloſe to the eye. Birds are under the ſame circumſtances in a ſtill greater degree with reſpect to their food; but from their mode of life, they alſo require the power of ſeeing objects at a great diſtance from the eye. Fiſhes, from the nature of the medium in which they live, muſt have ſome other mode of adjuſting the eye than that of a change in the cornea, becauſe that ſubſtance is poſſeſſed of the ſame refractive power as the ſurrounding fluid.

Quadrupeds have three modes of procuring their food; one by their fore-paws only, which they uſe like hands, as in the monkey tribe; the ſecond by their fore-paws and mouth,

mouth, as the lion and cat tribe; the third by the mouth only, as all ruminating animals. These three different modes require the food being brought from different distances from the eye, and it is curious that the muscles of the eye are different in all the three tribes.

In the monkey tribe the muscles of the eye are exactly the same as in the human. In the lion tribe they are double in number, and the four intermediate muscles are lost in the sclerotic coat at a greater distance from the cornea than the others. In the ruminating tribe there are four muscles, as in the human eye; but there is also a muscle surrounding the eye-ball attached to the bottom of the orbit, round the hole through which the optic nerve passes, and lost upon the sclerotic coat immediately before the broadest diameter of the globe of the eye. The upper portion of this muscle is rather the longest, the insertion being nearly in a circular line at right angles at the axis of vision, but not to the axis of the eye from the entrance of the optic nerve.

In quadrupeds in general, the ball of the eye is broader in proportion to its depth than in the human subject. In the bull the proportion is 1 [illegible] inch to $1\frac{6}{10}$. The cornea is larger and more prominent, its real thickness not easy to be ascertained, because, like that of the human eye, it readily imbibes moisture after death. When dried, it is thinner than the sclerotic coat in the same state. In ruminating animals, the cornea, though circular, has an oval appearance, arising from an opake portion which is covered by a membrane. The ciliary processes, as in the human eye, are connected with the choroide coat; but they are larger, and are united at their origin with the iris.

This structure of the eye in quadrupeds, as far as it differs from that of the human eye, appears calculated to increase the power of adjusting it to near objects; and from the mode of life which these animals pursue, such additional powers appear necessary to enable them to procure their food with ease.

That birds, procuring their food by their beak, must require an adjustment to see very near objects, and that a degree of precision and facility, with regard to remote objects, is no less necessary from their situation during flight, are obvious circumstances. Mr. Home mentions some instances, in which birds are supposed to have seen distant objects with peculiar distinctness. It is related, that vultures and other birds very speedily repair to the place where a dead animal or other prey is exposed to them; and though there is nothing, perhaps, in these facts which suppose an acuteness of sight greater than that of other animals, yet it cannot be doubted, upon the whole, that these animals see with very great precision at vast distances.

The eyes of birds are larger in proportion than those of other animals, and broader in proportion to their depth, with a more prominent cornea than in the quadruped. The cornea in the goose is not united to the sclerotic coat by the terminating of one abruptly in the other; but the two edges are bevelled off and laid over each other for nearly $\frac{1}{[illegible]}$th of an inch. This circumstance, as also the bony rim or apparatus surrounding the basis of the cornea, which is peculiar to this class of animals, and described by Mr. Smith, was well known to Haller. Mr. Home describes it more particularly, and states its use in the focal adjustment to be nearly the same as that described by Mr. Smith. The ciliary processes are larger and longer in birds than in other animals whose eyes are of the same size; and the marsupium or membranous process peculiar to the class of birds, is shewn by him to be very similar to that of the ciliary processes; but stronger in all its parts, and, like them, connected with the cryſtalline lens at the one extremity, and at the other with the bottom of the eye. By experiments on this membrane he aſcertained, that it is capable of muſcular contraction: for, when the marſupium and cryſtalline lens of a gooſe's eye were expoſed immediately after death, and the lens was puſhed forwards ſo as to elongate that membrane from $\frac{7}{40}$ths to $\frac{10}{40}$ths of an inch, it repeatedly recovered its original dimenſion when the preſſure was taken off. But when the parts had been left until all remains of life were gone, and the tenſion was then made as before to $\frac{10}{40}$ths of an inch, it contracted only to $\frac{8}{40}$ths; whence it was concluded, that the difference between the contractions was the effect of muſcular action. It was obſervable, however, that in ſo minute a quantity it was eaſy to be deceived. Another experiment was therefore inſtituted upon the well-known fact, that in the act of dying, the muſcles are found to contract to their utmoſt where there is no reſiſtance to prevent ſuch action; and that this is alſo found to take place in the greateſt degree when the animal is killed by any violence committed upon the brain or ſpinal marrow. The cryſtalline lens of a turkey's eye was extracted, and immediately afterwards the turkey was killed by wounding the ſpinal marrow. The two eyes were taken out and put into ſpirits. In the one the marſupium had nothing to prevent its contracting to the utmoſt; while in the other the lens being in its natural ſituation could not allow of any unuſual contraction. Some days afterwards the two eyes were examined: in the perfect eye the marſupium meaſured $\frac{4}{20}$ths of an inch, and its different folds were ſemi-tranſparent; in the imperfect eye the marſupium meaſured $\frac{3}{20}$ths of an inch, and the folds were much more opake. Here then was a difference of $\frac{1}{20}$th of an inch in the length of the two marſupiums: which could ariſe from no other cauſe than the one having contracted ſo much more than the other, which contraction muſt be conſidered to be muſcular.

On a review of the peculiarities in the eyes of birds the author infers, that they tend to facilitate the lengthening of the axis of viſion, and increaſing the convexity of the cornea. The bony rim to which the muſcles are attached, confines their effect to the broadeſt part of the eye; and as their action throws the cornea forward, the anterior rim of the bony edge yields to adapt itſelf to that change. The ciliary proceſſes are long, and admit of the lens being moved forward during the adjuſtment for very near objects, which is performed with more facility than in other animals, while their action ſerves to bring it back to its place. The marſupium ſerves alſo to draw it backwards, and, by ſuſtaining part of the preſſure from behind, renders the cornea flatter, while the anterior edge of the bony rim is adapted to it in this ſtate. It may be ſaid that no ſuch great change is neceſſary for viſion with parallel rays; but Mr. Home remarks, that where viſion is to be very diſtinct, a certain nicety of adjuſtment becomes neceſſary, and the action of the marſupium is probably intended for that purpoſe.

The ſubject of viſion in birds is concluded by an obſervation, that one of the moſt beautiful illuſtrations of the combination of muſcular and elaſtic ſubſtances is ſeen in the motion of the nictitating membrane. This membrane is elaſtic, and is connected by means of a tendon with two muſcles ſituated upon the poſterior part of the eye-ball. The action of theſe muſcles brings the membrane over the cornea; and the inſtant they ceaſe to contract, the elaſticity of the membrane draws it back again.

The eyes of fiſhes have ſeveral peculiarities, and in many reſpects their ſtructure differs from that which is obſerved in the quadruped and bird.

The

The muscles of the eye, that correspond to the straight muscles in the quadruped, are four in number: they are however differently placed; they do not surround the eye-ball, but two of them are on that side of the orbit next to the nose of the fish, the other two on the opposite side; their attachment to the eye is close to the edge of the cornea; they do not pass round the eye-ball towards the posterior part, as in other animals, but are connected with the bones of the head at some distance from the eye on each side, so that they cannot at all compress the eye laterally; they can only pull it backward by the combined effect of their actions.

The bottom of the orbit on which the eye-ball rests is solid and adapted to it, there being no fat interposed between them as in other animals; and where the eye is removed to a great distance from the skull, and that cannot be the case, there is a strong cartilage projecting from the skull to the bottom of the eye, and that end of it next to the eye is concave, and fitted to the portion of the eye-ball directly opposite the cornea, just above the entrance of the optic nerve. This is considered as a fixed point upon which the eye moves; but it will also, from the situation of the muscles, allow the eye to be forced back upon it, and the whole eye to be flattened.

The shape of the eye differs considerably in different fishes; but in all of them the transverse diameter is the longest. In the haddock the proportion is $\frac{11}{10}$ths to $\frac{8}{10}$ths of an inch, and in some fishes it differs much more.

The size of the eye does not correspond with that of the fish; the salmon's eye being smaller than the haddock's.

The sclerotic coat is in some fishes membranous*, in some partly bone †, in others entirely so ‡; but in general the posterior part is membranous, although the lateral parts are bone §.

The cornea is in general flat, not always circular in its shape, is very thin, made up of laminæ, and does not lose its transparency in spirits, appearing like talc ‖. In others it is more convex, as in fish of prey; this appears to adapt it to the spherical crystalline lens, which in them lies directly behind it ¶. The tunica conjunctiva forms the anterior layer of the cornea **, and in some fishes is quite detached.

(The Substance of Mr. Home's Concluding Lecture upon the Eye will be given in a future Number.)

NEW PUBLICATIONS.

Philosophical Transactions of the Royal Society of London, for the Year 1797, Part II. Quarto, 541 pages, exclusive of the title, contents, list of presents, and index, with nine plates. Sold by Elmsly, London.

THIS part contains the following papers: 1. On the Action of Nitre upon Gold and Platina, by Smithson Tennant, Esq. F.R.S. 2. Experiments to determine the Force of Fired Gun-powder, by Benjamin Count of Rumford, F. R. S. M. R. I. A. (see Philos. Journal, i. p. 459). 3. A third Catalogue of the comparative Brightness of the Stars; with an in-

* Haddock. † Sword-fish. ‡ Devil-fish. § Mackarel. ‖ Sword-fish. ¶ Pike. ** Haddock.

troductory

troductory Account of the Index to Mr. Flamsted's Observations of the fixed Stars contained in the Second Volume of the Historia Cœlestis; to which are added several useful Results derived from that Index, by William Herschell, LL. D. F. R. S. 4. An Account of the Means employed to obtain an overflowing Well, in a Letter to the Right Hon. Sir Joseph Banks, K. B. P. R. S. from Mr. Benjamin Vulliamy. 5. Observations of the changeable Brightness of the Satellites of Jupiter, and of the Variation in their apparent Magnitude; with a Determination of the Time of their rotatory Motions on their Axis. To which is added a Measure of the Diameter of the Second Satellite, and an Estimate of the comparative Size of all the four, by William Herschell, LL. D. F. R. S. 6. Farther Experiments and Observations on the Affections and Properties of Light, by Henry Brougham jun. Esq. 7. On Gouty and Urinary Concretions, by William Hyde Wollaston, M. D. F. R. S. 8. Experiments on Carbonated Hydrogeneous Gas, with a View to determine whether Carbon be a Simple or a Compound Substance, by Mr. William Henry. 9. Observations and Experiments on the Colour of Blood, by William Charles Wells, M. D. F. R. S. 10. An Account of the Trigonometrical Survey carried on in the Years 1795 and 1796, by Order of the Marquis Cornwallis, Master General of the Ordnance, by Colonel Edward Williams, Captain William Mudge, and Mr. Isaac Dalby.

A Lecture introductory to a Course of Popular Instruction on the Constitution and Management of the Human Body, by Thomas Beddoes, M. D. octavo, 72 pages, price 1s. 6d. Printed for Cottle in Bristol, and sold in London by Johnson, 1797.

The author of this pamphlet was accidentally informed, by a practitioner in surgery, in the course of the year just expired, that he was desirous of giving a course of anatomical lectures in Bristol. To furnish individuals with so much knowledge of themselves as should enable them to guard against habitual sickliness, and a variety of serious disorders, had long been an object of Dr. Beddoes's contemplation. He, therefore, proposed that the course should be modelled according to this idea. He remarked that a distinct exhibition of the larger lines of anatomy and physiology would be also the mode of instruction best adapted to young students in medicine; much observation of lectures having convinced him that extreme minuteness is only perplexing to beginners. This, joined to some other considerations, prevailed. Mess. Bowles and Smith undertook the course, the intention of which is, to exhibit the structure of the body in a manner neither superficial nor tedious; to explain the functions of the parts as far as they have hitherto been investigated; to illustrate, by specimens, the principal deviations of these parts from their healthy conformation, and to intersperse such reflections as may be useful in physical education, and the whole conduct of life. Dr. Beddoes purposes to contribute his utmost assistance to the design, in whatever way that assistance shall, upon reflection, appear most likely to be effectual. This introductory lecture constitutes part of his exertions in a plan so truly calculated to promote individual happiness, and public welfare. He is desirous that its publication may produce similar undertakings elsewhere, and justly supposes that a communication of the fact, that these lectures are attended by an audience more than twice as numerous as the friends of that design expected, will tend to promote this purpose.

After this concise statement of an undertaking to which every friend of mankind must wish success, I must decline analysing the lecture, and content myself with observing, that the subject, which itself is highly interesting to all descriptions of men, is treated in a very perspicuous and impressive manner.

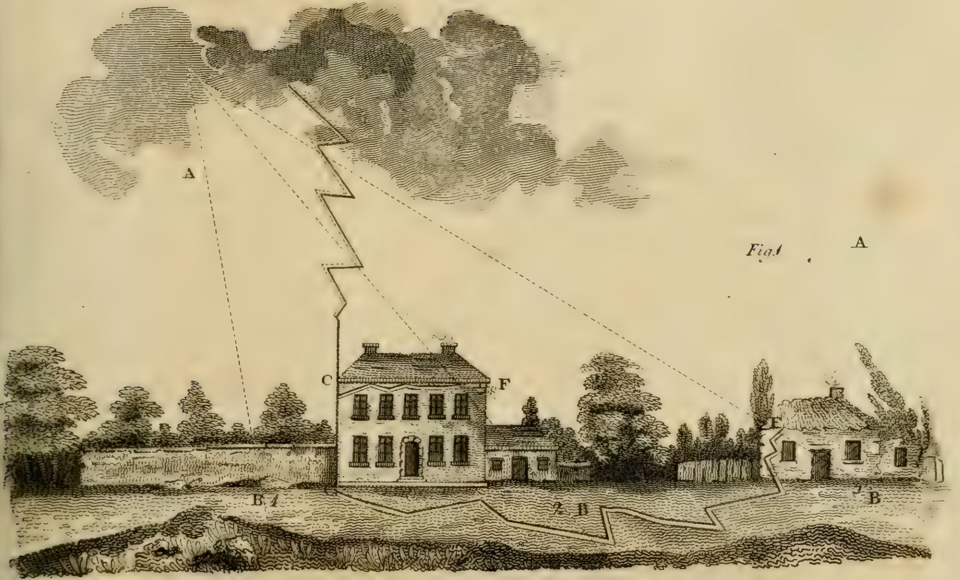


Fig. 1

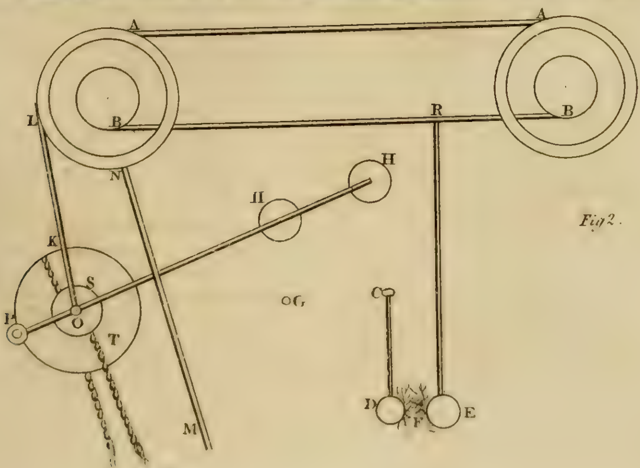
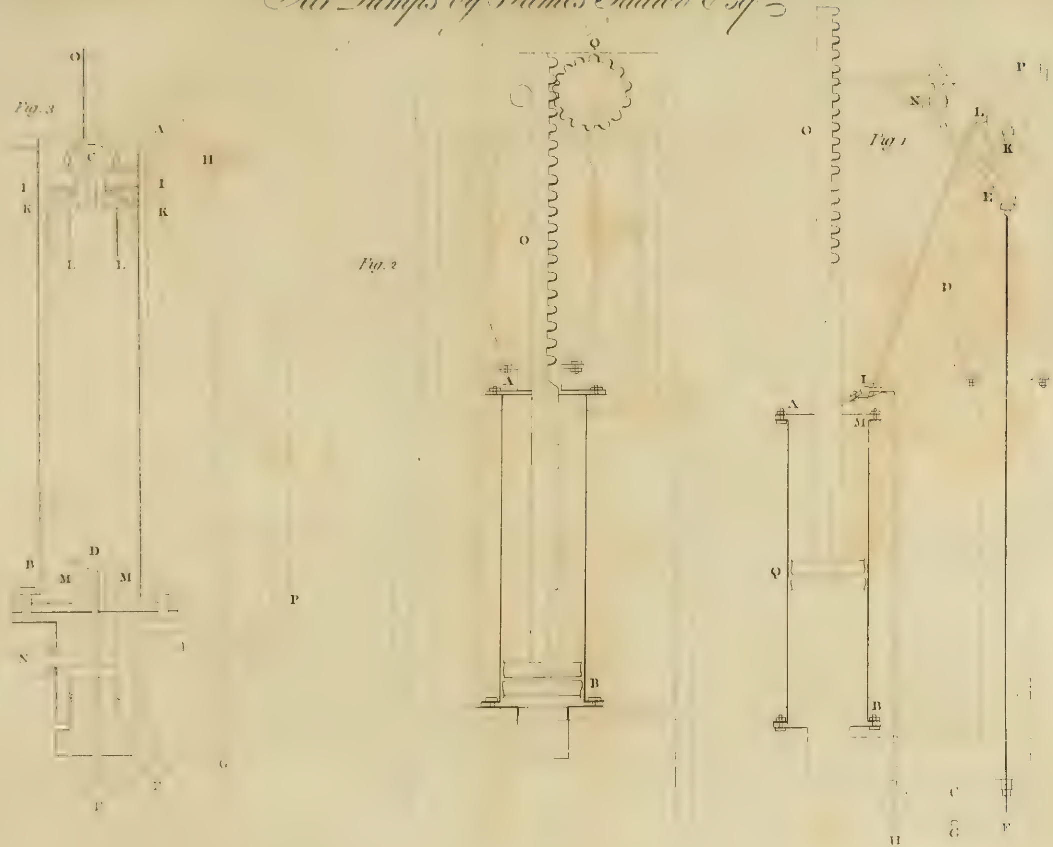


Fig. 2.

Air Pumps by James Sadler Esqr.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY 1798.

ARTICLE I.

An Attempt to accommodate the Diſputes among the Chemiſts concerning Phlogiſton. In a Letter from Dr. MITCHILL of New York to JOSEPH PRIESTLEY, LL.D. F. R. S. &c. &c. Dated November 20, 1797 *.

ON reviewing the ſtate of philoſophical controverſy as carried on both in Europe and America between the Phlogiſtians and their opponents; it has of late appeared to me, that much of the difficulty which attends the ſubject ariſes, as in abundance of other caſes, from the want of a preciſe language and of a right underſtanding of each other's meaning. This was ſo evident to me in the preſent caſe, that I informed my audience of it in one of my public lectures in Columbia College, and added my belief, that due attention to terms, their application and uſe, would have great influence in bringing the diſpute to a termination.

Having ſubjected water heated to the temperature of ſteam in an eolipile, and directed the ſteam iſſuing from it to the ſurface of red-hot charcoal; the coal brightened, and a greater flame was obſerved near the ſpot againſt which the ſteam was made to play. Here was an occurrence oppoſing the common obſervation of mankind, that water will always extinguiſh fire by reaſon of its own incombuſtibility. Water kept at or below a certain temperature will extinguiſh fire, and ſo will oil; but if water be raiſed to a heat ſufficiently high, it will alſo burn or undergo decompoſition like oil. As far as I could judge from the phenomena before me, water in proper circumſtances underwent a true combuſtion, and was inflammable for the ſame reaſon that oil was, becauſe it contained a ſomething that would burn; and this ſomething ſeemed to be exactly ſimilar to that which made oil capable of exhibiting flame. It ſtruck me inſtantly, that the inflammability of

* Communicated by the Author. N.

this vapour proceeding from burning fat, from heated alcohol, from camphor, ether, coal, and a multitude of other fubftances, gave evidence of their poffeffing a principle enabling them to burn with flame after the fame manner that water did. If there was this fimilarity or indeed identity of the inflammable radical among them, there appeared to be no more propriety in calling that radical *hydrogene* than in terming it *oleogene*, *alcoholigene*, *etherigene*, *carbogene* *, &c. To give the radical fubftance enabling oil, alcohol, ether and coal to burn with flame a name derived from water, becaufe it enabled water to burn with blaze too, appeared to me partial, illogical and wrong, inafmuch as it conftantly and unneceffarily brought water and its properties to mind whenever any thing was thought of that contained hydrogene; and by this unhappy affociation, befides the difficulty which attends the fubject in point of fact, vaftly greater difficulty was made to furround it by reafon of the ill chofen and badly afforted terms employed in talking about it.

I had entertained no doubt, for two years, that *hydrogene* was an improper word for a nomenclature of fcience, and deferved to be ftruck out of the lift: but as I was engaged in reforming another article of that arrangement, I chofe not then to meddle with it; and I am glad I did not; fince the prolonged difputes between the parties afford more weighty caufes for an alteration of terms at this day than exifted at any former time.

The circumftance common to all the proceffes I have mentioned, is "burning with flame or blaze," which, wherever it occurs, feems to indicate the prefence of what has been called hydrogene. According to my prefent conception of the matter, this principle or fubftance common to fo many bodies, and enabling them to undergo inflammation, may in ftrict propriety be called *phlogifton*. I always thought *phlogifton* a well conceived word, and have objected to it, not on account of the impropriety of the term as fuch, but becaufe of the vague and unfatisfactory way in which it was ufed. If a definite fignification can be affixed to it, I think the adoption of it will be ftill a great acquifition to philofophical language, and have a tendency to fettle at leaft half the controverfy which divides the chemifts.

I propofe, then, to expunge *hydrogene* and fubftitute *phlogifton* in its place. *Phlogifton* will thus be the radical term, and ftrictly mean the thing in combuftible bodies which forms blaze or ignited vapour. The union of this with mere caloric will make *phlogiftous* or inflammable air, the air which burns with blaze. The combination of phlogifton with oxygene will conftitute water or the *oxyd of phlogifton*, one of the products of inflammation, and, like fixed air and other compounds formed during the fame procefs, incombuftible in common temperatures and circumftances afterwards. And the caufe of this flownefs to burn, of water and the other compounds which combuftion furnifhes, is owing to the large dofe of oxygene with which they are charged, giving them little or no appetite for more. If this bafe be united to a yet larger quantity of oxygene, it will form the acid of phlogifton, or water foured by excefs of oxygene, as *perhaps* (though I do not believe it) in what is termed the pyro-lignic and pyro-mucic acids, and perhaps in fome other cafes: but the readinefs with which phlogifton parts with its furplufage of oxygene, turns back to water, and preferves itfelf in that oxydated form (as proved by the operation of fharp-pointed filaments under water in effecting the feparation) fhews that nature, in enabling the princi-

* So in the original: doubtlefs by overfight. N.

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ple of inflammability to combine with oxygene, disqualifies the latter in most cases from becoming an acid with the former; unless it should be found (and in this I have no faith) that the formation of the native acids of vegetables is a process of this kind. Should this latter conjecture turn out to be the fact, there would be instances enough of *phlogistic* and *phlogistous acids*.

The nomenclature will then stand thus:

1.	2.	3.	4.
Phlogiston for Hydrogene.	Phologistous gas for Hydrogeneous or inflammable air.	Oxyd of phlogiston for Water or oxyd of hydrogene.	Gaseous oxyd of phlogiston for Aqueous gas, or watery steam.

5.	6.	7.
Phlogistous and phlogistic acids. Probably no such things; the hydrogene or phlogiston not being capable of combining with oxygene beyond the degree of oxyd.	Phlogistates and phlogistites. Probably no such combination.	Phlogisturcs and phlogisturets; of sulphur, coal, phosphorus, iron and zinc, instead of The common sulphur, &c. of the laboratories and shops, &c. &c.

On the decomposition of fat and oil by fire, it is known that a large quantity of water is formed, and this probably by an union of the base of vital air with phlogiston or hydrogene. The like obtains in the inflammation of alcohol, camphor, ether and coal, part of the phlogiston or hydrogene of which apparently turns to water by junction with oxygene. And the principle which in the first instance readily exhibits the blazing appearance is changed by and during that operation to a something much more difficult to inflame by any after-process.

If hydrogene or phogiston is the material which inflames in the substances already mentioned, there is presumptive evidence, upon the face of the subject, of its existence also in the common sulphur, phosphorus, zinc, and iron of the laboratories. I do not mean to say it is a *necessarily* constituent part of either of those bodies; for I believe they may exist without it, or at least they may be conceived to exist abstractedly from it. They therefore stand very well in the nomenclature as simple substances. But if these substances, such as we commonly get them after exposure to the common atmosphere in ordinary temperatures, are taken for simple or pure elementary bodies, the persons who consider them so fall into a great mistake. In their usual forms they are all incorporated with hydrogene or phlogiston, and from it derive their capacity to burn with flame. This will be the more clearly seen by considering them more particularly one by one.

1. Of Sulphur. The Phlogistians say sulphur is composed of phlogiston united to vitriolic acid; consequently if any thing takes away that ingredient from the acid, this will turn to brimstone. The antiphlogistians affirm sulphur to be a simple body, uncombined chemically with any thing; and that it becomes sulphuric acid by junction with oxygene. Now both parties have reasoned in a manner that does not by any means satisfy me. They have viewed the combustion of sulphur in the abstract, rather than taken it up as it is. The fact is, that the acid formed in the combustion of sulphur is not the solid crystallized mat-

ter of the glacial oil of vitriol, but a folution of thefe cryftals in water. The exiftence of vitriolic acid in a fluid form implies neceffarily the co-exiftence of water. The formation of the water in the inflammation of fulphur appears to have been paffed over by both parties, though the interpretation of this part of the procefs feems to me to furnifh the means of reconcilement. Thus, while the pure fulphur combines with one portion of oxygene to make the acid, the hydrogene or phlogifton unites with another parcel of it to form the water in which the acid diffolves. Common brimftone then is not a fimple fubftance, but is a *phlogifture of fulphur*. And this is confirmed by the fact that, where combuftion is reftrained, the fulphur may be refolved into hepatic gas; the phlogifton turning with caloric into inflammable air and diffolving fome of the fulphur. The formation of common hepatic gas feems to evince the fame thing; for while the potafh feizes the fulphur, the hydrogene or phlogifton is fet loofe, turns with caloric to hepatic gas, and fnatches, as it departs, a portion of the fulphur from the alkali. Thus it appears that the two fyftems are reconcileable with each other. When the Old Chemifts talk of phlogifton, they fhould define it to be that thing which burns with flame, and when united to oxygene forms water. When the new ones make experiments on fulphur, they fhould remember that the common material called by that name is not the abftract, pure, uncombined elementary thing they intend in their nomenclature.

2. In like manner the phenomena attending the inflammation of phofphorus feem to have been as negligently interpreted. Phlogifton added to phofphoric acid was faid by fome to conftitute phofphorus; while oxygene added to phofphorus made phofphoric acid in the opinion of others. But thefe were a kind of chemical theorems, true only in the abftract experiments. We find that phofphorus burns *with flame*, and *water* is exhibited during the procefs. All that needs be faid about it is, that in common circumftances phofphorus, though capable of exifting *per fe*, has a very ftrong attraction for hydrogene or phlogifton; and in ordinary cafes attaches more or lefs of it to itfelf. During its inflammation part of the oxygene, as in the cafe of the fulphur, combines with the phlogifton into water; and another part of it joins the oxygene to conftitute the acid. In eftimating the *whole* of the procefs, the candid partifans of both fides will allow that the fubftance under confideration parts with its phlogifton and borrows oxygene, and thus water and the acid diffolved in it are formed.

Where is the harm of owning that common phofphorus contains a portion of hydrogene united with it? It does not invalidate the modern theory: but it fhews that the objections of the ancient doctrine were not frivolous, as they have by fome been deemed to be; but on the contrary very fubftantial, and not capable of reconcilement upon any other plan, that I know, than the one herein fuggefted.

3. Zinc may be abftractedly confidered as a fimple body, and with propriety placed as fuch in the catalogue. Commonly, however, it is prefented to us in clofe connection with hydrogene, for which its attraction is fo ftrong that they commonly appear in the form of a *phlogifture of zinc*. When that compofition is employed for experiments, it is very eafy to conceive how, when fuch zinc is expofed to a fufficient heat in an open fire, the phlogifton diflodged, and immediately becoming phlogiftous gas or inflammable air, fhall take upon itfelf the form of flame and conftitute water; while the oxygene combines with the metal into a white oxyde, the flowers of zinc. So if the fame compound be diffolved in fulphuric acid,

acid, the phlogiſton diſplaced will turn to inflammable air, while the acid and the zinc form white vitriol. In this way ſome of the phlogiſtous or inflammable gas may be accounted for, as extricated from the metallic preparation: and at the ſame time I ſee no objection to deriving the reſt of the great quantity afforded by this proceſs, from the decompoſition of part of the water or oxyd of phlogiſton. To accommodate matters then, the advocates of the Lavoiſierian theory ſhould concede that zinc, in common circumſtances, is aſſociated with hydrogene or phlogiſton. And the diſciples of Stahl ſhould on their ſide allow that zinc cannot be conſidered as a pure metal, while alloyed or blended with phlogiſton or any other foreign ingredient. The material they have all worked upon is not the uncombined metal, but a phlogiſture of zinc.

4. Some ſorts of iron treated by heat alone afford phlogiſtous or inflammable air. The ſame metal may be made to burn with flame, and, when treated with ſulphuric acid, affords much phlogiſtous air. What then is the thing commonly called iron? Is it a pure and unmixed ſubſtance? Or is it a compound of elementary iron with hydrogene or phlogiſton? The facts enumerated lead concluſively to the latter opinion. The Phlogiſtians are right then, when they ſay common iron is a compound: and they are right when they ſay the inflammable air obtained from it is nearly pure phlogiſton. And the Antiphlogiſtians are juſtifiable in placing in their enumeration of ſimple bodies, ſuch a thing as elementary iron is or may be imagined to be; and in aſcribing the production of hydrogenous or phlogiſtic gas to a decompoſition of part of the water. The compound called iron then gives out ſomething, and takes in ſomething, in all the common proceſſes. And the modern chemiſts ſhould correct the miſtake they appear generally to have fallen into, of taking it for granted, that was a ſimple ſubſtance which in fact is a chemical compoſition of iron with hydrogene. And thus finery cinder, which evidently differs from hæmatites or any pure oxyd of iron, may be a triple compound of iron, hydrogene or phlogiſton, and oxygene; which juſt about correſponds with your idea that it conſiſts of iron and water.

It will not follow from all this, that becauſe phlogiſton or hydrogene ſo generally exiſts in combination with zinc and iron, it muſt be an ingredient in all metals. For gold, arſenic, ſilver, platina, mercury, copper, tin, lead, biſmuth, cobalt, antimony, and mangaſe are capable of exiſting without it, and accordingly do not commonly burn with flame, nor afford inflammable air by ſolution in acids; though if ever they exhibit in any of their ſtates blaze by burning, or phlogiſtous air with acids, this will only evince the exiſtence of hydrogene in them in ſuch caſes. Both parties may thus allow that ſome metals contain phlogiſton, and ſome do not. Nor will charcoal, as has ſometimes happened, be confounded with phlogiſton according to this view of the matter; though hydrogene is often blended with it. If coal at any time affords inflammable air, this is no evidence of the converſion of that ſubſtance into phlogiſtous gas, but merely a proof that the coal when ſubmitted to experiment was combined with the baſis of inflammable air, which it could part with and ſtill remain coal, though in that caſe incapable of burning with flame; but, in a ſufficient expoſure to heat and oxygenous air, taking on without blaze the form of carbonic acid gas. It may be conceded then on both ſides, that, though phlogiſton or hydrogene may exiſt with coal, nevertheleſs coal can exiſt without phlogiſton.

I know not in what manner theſe conſiderations may impreſs your mind. I have flattered myfelf they have a tendency to accommodate and reconcile the gentlemen of both fides to each other, and to the truth. But in this if I have deceived myfelf, I have only to appeal to your known experience and candour. It does however feem to me capable of getting through with much of the difcuffion not yet folved between Mr. Kirwan and the French philofophers; and between yourfelf and Meffrs. Adet and Maclean; and I fufpect the celebrated experiments of Mrs. Fulhame are in no wife repugnant to this mode of interpretation.

Permit me to exprefs my fatisfaction, before I conclude, that you have not given up the points before the philofophical world, without due examination. I no more like unanimous decifions in hafte upon a queftion of fcience than of legiflation. The fure way to introduce errors both into laws and into philofophical performances, is to affent to new bills and projects upon the credit and authority of the propofer, without compelling them to undergo the amendments of debate. And I cannot help confidering your refufal to agree to *all* that the New Nomenclature Men have afferted and recommended, as a fortunate event in philofophy; as I am fure, with refpect to myfelf, the oppofition you have made has caufed me to confider the fubjects in difpute with greatly more attention than I otherwife fhould have done. With much refpect I beg leave to affure you that I am, &c.

SAM. L. MITCHILL.

ANNOTATIONS *upon the preceding* LETTER.

Steam in an eolipile.] It is well known that ignited charcoal in the gun-barrel decompofes water, and that the products are hydrogene and carbonic acid gas. In this cafe the carbone is burned, and the hydrogene unburned. Are we to fuppofe that the water was firft decompofed in Dr. Mitchill's experiment, and then recombined by a fecond combuftion of the hydrogene? or was the effect any thing more than an increafed energy of combuftion by the greater afflux of atmofpheric air mechanically impelled by the fteam? Dr. Lewis * found that the fteam from the eolipile always extinguifhed his fires, unlefs the vapour was made to pafs through a portion of the atmofphere. Whence I infer that the quantities of heat carried off and rendered latent in giving elafticity to the hydrogene and carbonic acid are fo great as fpeedily to diminifh the temperature, and terminate the combuftion of the charcoal; unlefs, as in the experiment of the gun-barrel, there be a fufficient fupply of heat from without.

by this unhappy affociation.] The argument of affociation of ideas feems to militate ftrongly againft the reception of the word phlogifton in a new fenfe.

Burning with flame or blaze.] Though the author's arguments in favour of hydrogene, as the exclufive caufe of flame, may be hypothetically applied to all the inftances in which this appearance is feen; yet the contrary pofition, namely, that bodies, whether mechanically divided, fuch as the powder of refin or coal, or chemically, as zinc in the elaftic ftate during fublimation, may be fet on fire and exhibit the appearance of an ignited tranfparent fluid, fuch as flame, feems at leaft equally probable. It cannot therefore be ftated as matter of fact, that where flame is there muft neceffarily be hydrogene.

* Philofoph. Commerce of Arts, p. 21.

In

In the decomposition of fat and oil by fire, &c.] The difference between the leading assumption in Professor Mitchill's letter, and the theory formerly maintained by Mr. Kirwan, and stated by him in the Introduction to his Work upon Phlogiston, appears to be, that the former does not suppose his phlogiston, or principle which affords flame, to be essential to the metallic state, but merely, as I conceive it, accidental in that class of combustible substances. He follows Mr. Kirwan, by first directing his attention to the universality of hydrogene in organized bodies, and then endeavouring to shew that it exists in sulphur, phosphorus, zinc, and iron, in their ordinary combustible state. It has not yet been shewn that aqueous sulphuric acid can be produced by the combination of sulphur and oxigene, without the presence of water ready formed. The same observation more strongly applies to phosphorus, which Lavoisier converted into the solid acid by the burning glass in a vessel of oxigene gas over mercury. In the observations on zinc and on iron, we look in vain for the experimentum crucis, which must be exhibited before the supposed fact that metals contain hydrogene can be admitted.

I have ventured to make these remarks on the Professor's ingenious communication, without aiming at the establishment of positions contrary to those he has offered. The questions he treats must be decided by plain facts, if such can be found. Till this be done, they remain among the mass of unknown propositions, concerning which, we ought neither to affirm nor deny.

II.

Experiments on the Composition and Proportion of Carbon in Bitumens and Mineral Coal. By RICHARD KIRWAN, *Esq. F. R. S. L. & E. M. R. I. A. &c.*

AN exact knowledge of the component parts of the different species of mineral coal, and also of bitumens (substances which most of them contain) forms an object of some importance, not only to the naturalist whose views are merely speculative, but to the practical œconomist, who wishes to extract from each species all the advantages it is capable of yielding, and to be enabled to compare the various kinds afforded by different countries, in order to obtain and employ that which shall on the comparison appear to him best suited to his intentions.

In effect, coals are not only applicable to the more usual purposes of combustion—an use, simple as it may appear, attended, according to their various species, with a considerable difference of calefactive power both in intensity and duration, but also to the production of varnishes, much more advantageously applicable in many instances than those extracted from the Vegetable Kingdom, as Lord Dundonald has discovered, and abundantly proved*, and also of that charred residuum called coak, the only one that can be resorted to in many cases, and, in most, superior to vegetable charcoal.

* Upon the most minute enquiry why coal-varnish is not more commonly employed in paying the bottoms of ships, I have been informed the principal reason is, that it succeeds too well; the ships not requiring such frequent repair. K.

Coals

Coals and bitumens are, however, fubftances that refift the ufual modes of analyfis; they elude the action of aqueous, acid, alkaline, or fpirituous menftruums; and diftillation, the only mode hitherto ufed, confounds and varies their natural contents.

Reflecting on thefe obftacles to an exact difcrimination of bitumens and coals, and of the various kinds of thefe laft, it occurred to me, that partly by combuftion and partly by their efficacy in decompofing nitre, the fecret of their internal compofition might poffibly be unveiled.

1. Combuftion. I have obferved that all the fpecies of folid bitumen properly fo called, when laid on a red hot iron, burn with a large bright flame, fmoke and foot, leaving none or fcarce any coaly refiduum, and only a little afhes:

That the fofter bitumens, as maltha, burn in the fame manner, leaving no coal, but only a little afhes, and requiring no increafe of heat for their entire confumption:

That afphalt burns with flame and foot, but melts and fwells, and requires for its entire confumption an increafe of heat, leaving fcarce any coal, and but little afhes.

It is moreover well known that liquid bitumens contain inflammable air and carbon: that they abforb atmofpheric air when long expofed to it and light: that, in confequence of this abforption, they are thickened, blackened, and condenfed, firft into mineral tar, then into mineral pitch or maltha, and laftly into afphalt: that almoft all fpecies of mineral coal yield more or lefs of both fpecies of bitumen in diftillation, leaving a fhining coaly refiduum; but that the proportion is variable in every fpecies according to the degree of heat applied; that the refiduum always obftinately retains a proportion of bitumen, and that confequently diftillation, in addition to its other imperfections, is an infufficient medium whereby to difcern the proportion of carbon and bitumen, and confequently to difcriminate the various forts of mineral coal from each other.

2. Decompofition of nitre. It has long ago been remarked by the juftly celebrated Macquer *, that nitre detonates with no oily inflammable matter, until fuch matter is reduced to a coal, and then only in proportion to the carbonaceous matter it contains. An obfervation the truth of which will fully appear in the fubfequent experiments.

Hence it occurred to me that, fince in the act of detonation nitre is always totally or partially decompofed, and fince, where carbonaceous compounds are employed, this decompofition arifes folely from the mere carbonaceous parts, and, every thing elfe being equal, is proportioned to the quantity of mere carbon they contain; and fince moft fpecies of coals are compounds of mere carbon and bitumen, as appears by the products of their diftillation; it fhould follow, that, by the decompofition of nitre, the quantity of mere carbon in a given quantity of every fpecies of coal may be difcovered; and this being known, that of bitumen may be inferred; and, the other unefiential ingredients being detected by incineration, the whole contents of coaly fubftances might be afcertained.

The compofition of bitumens alfo, as far as relates to their proportion of carbon and oil, may be evidenced in the fame manner; and here it is to be obferved, that the bitumens I here confider are thofe that are found in a dry or folid ftate, and that thefe contain a larger proportion of carbon than the liquid bitumens; for, though thefe laft alfo contain carbon, it being an effential component part of all oils, yet this portion does not extricate or educe

* Diction. Chym. fecond edition, 481.

any

any air from nitre, nor confequently contribute to its decompofition, as the fubfequent experiments fufficiently evince; but is confumed partly by the pure air fpontaneoufly emitted by nitre during its ignition, and partly by the ambient atmofpheric air.

Nay, when mineral coal is employed in the decompofition of nitre, the fhare which the mere carbonaceous part of the bitumen contained in it contributes to the decompofition will be found fo fmall that it merits no confideration in the general account.

The firft ftep towards carrying this analytic plan into execution muft therefore be, to determine the quantity of pure carbon neceffary to decompofe a given quantity of pure nitre. But here many practical difficulties occur, which fhall prefently be mentioned. The moft perfect method of obviating them was that employed by the ever memorable Lavoifier: he mixed the pureft nitre with charcoal alfo purged of the inflammable as well as other airs and water which it ufually abforbs, in the proportion which, after feveral trials, he found requifite for the entire decompofition of that falt, rammed them into a copper tube, fired them, and continued the inflammation under water; by which means the charcoal was acted on folely by the air educed from the nitre to the entire exclufion of the external air, and this air was educed folely by the ignited charcoal to the entire exclufion of external heat; advantages that cannot be procured by the ufual mode of effecting this decompofition. Thus he found the proportion of charcoal neceffary for the entire decompofition of nitre, to be as 1 to 7,57, or, in other words, that 13,21 parts charcoal decompofe 100 of nitre *: and yet even in this experiment I find a fmall inaccuracy, as he did not take the water employed in mixing the nitre and charcoal into the account; and hence, and for fome other reafons, the detail of which would lead me too far, I think the proportion fhould be as 1 to 7,868 nearly, or that 12,709 charcoal decompofe 100 of nitre; but the difference is of little importance.

This mode of experimenting, however, is inapplicable on the prefent occafion; the different fpecies of mineral coal being not fo readily inflammable as to carry on the combuftion in this manner. Hence I contented myfelf with the common manner only, ufing fuch precautions as to render its refults tolerably uniform, and repeating each experiment feveral times.

I examined the purity of the nitre I employed by nitrated filver, and found by the quantity of falited filver produced, that 480 grains of the nitre contained 3,5 grains of common falt (135 grains of muriated filver, indicating 100 of common falt): hence the conftant quantity of nitre I ufed was 483,5 grains, except in the experiments on bitumens, as I had not enough of them to expend in fo large a quantity of nitre.

The nitre was heated barely to rednefs, before any coal was projected on it, in a wind furnace and a very large crucible; upon this uniform degree of heat much of the uniformity of different experiments on the fame fpecies of coal depends.

In my firft experiments the coals were reduced to a very fine powder, and then projected on the ignited nitre; but I obferved that by this method much more of each fpecies of coal was requifite to alkalize the ftandard quantity of nitre, than when it was reduced to a coarfe powder about the fize of a pin's head, or fomewhat larger; and the reafon is, that by the force of the explofion much of the finer powder is carried off, without having been in

* 11 Mem. Sçav. Etrang. 626.

contact with the nitre. Hence in the experiments of Mr. Hielm on the quantities of charcoal of different woods requisite to alkalize 100 parts of nitre, we find these quantities to bear for the most part some analogy to their specific gravities, being generally smaller when the specific gravity of the charcoal is lighter; thus*:

Grains requisite to alkalize 100 grains of nitre.

	Specific gravity.	First experiment.	Second ditto.
Oak-coal	0.332	35	30
Birch-coal	0.542	22	22
Pine-coal	0.280	29	20
Fir-coal	0.441	33	25
Coak	0.744	19	

Another circumstance of great importance towards procuring just and uniform results is, that the projections of coal should succeed each other without delay as soon as the flame ceases; for, as ignited nitre gives out pure air spontaneously, and so much the more as it is more heated, the acid will be decomposed, and the nitre alkalized, by a quantity of coal so much the smaller as the intervals of projection are longer. From inattention, perhaps, to this and the last-mentioned particular, as well as from various conditions of common charcoal, which seldom contains less than $\frac{1}{12}$, and often $\frac{1}{8}$ of its weight of moisture and absorbed air, proceeded the various results of different chemists, with respect to the proportion of it necessary to alkalize nitre.

It is almost superfluous to add, that the charcoal should be projected in very small portions. I seldom projected more than one or two grains at a time: each operation lasted from 20 to 25 minutes nearly.

There is always some portion of nitre undecomposed, being protected by the surrounding alkali. This error is unavoidable, but very small. Even the position of the crucible in the furnace is not indifferent; for, if it be near the flue, more coal must be employed, which I attribute to the torrent of air, which, in that case, affects and carries away more than when the crucible is nearer to the anterior part of the furnace.

It may, perhaps, be suspected that this and some other incidental errors may be avoided by previously mixing the nitre and coal, and projecting the mixture in small portions into a red-hot crucible; but not to mention that this method supposes the due proportion of these two substances to be known, which cannot be till after the experiment, and that also every atom of these substances is in perfect contact with the other substance, else they cannot act on each other—independently, I say, of these unfounded suppositions, this mode of experimenting is still more fallacious than the former, as during these projections a considerable proportion of the nitre is scattered and dispersed, and may be seen adhering to the sides of the crucible. This loss being repeated at every projection, becomes at last intolerable.

I now proceed to relate the experiments themselves, conducted in the manner I have mentioned. The different species of coal and bitumen, whose composition I have thus examined, were Kilkenny coal, Maltha, Asphalt, Lancashire cannel, Slaty, Scotch cannel, Whitehaven, Wigan, Swansey, and Leitrim; selecting of each sort the purest specimens, free from pyrites and visible stony matter.

* Schwed. Abhandl. 1781, 188.

5 *Kilkenny*

Kilkenny Coal.

ITS colour is black, and, when fresh broken, frequently violet.

Lustre 4, Metallic. Transparency 0 *.

Fracture foliated, the course of the lamellæ variously and confusedly directed. Fragments rather sharp, and often discovering between the distinct concretions whitish illinitions.

Hardness 7. Specific gravity 1,526.

Does not burn until wholly ignited, and then slowly consumes without caking or emitting flame or smoke. 266 grains of it exposed to a heat of 27° Wedgwood in a crucible for five hours, did not lose their lustre until almost $\frac{9}{10}$ of them had disappeared, and at last left reddish ashes amounting to 7,13 grains, nearly 2,7 per cent. Projecting this coal in fine powder on 480 grains of pure ignited nitre, I found the salt required 65 grains of the coal to alkalize it, but only 50 grains when in coarse powder; and in a third experiment, when the crucible was farther from the flue of the surface, only 49 grains; so that I look upon 50 grains as being in round numbers nearest to the truth. That is, in the proportion of one part of Kilkenny coal to 9,6 of nitre: or, 100 parts of nitre require for their decomposition 10,416 of Kilkenny coal.

This proportion of coal is much smaller than that of charcoal in Mr. Lavoisier's experiment, which we have seen to be as 1 to 7,57, or as 13,21 to 100, which I attribute to the advantageous mode in which his experiment was instituted, as already explained; whereas in mine, and the usual way, the decomposition of nitre is promoted by the external heat applied, as well as by the coal, and consequently less of the coal is employed.

From the experiments of Scheele one might be led to infer, that the proportions of charcoal and nitre necessary to the alkalization of this latter, approach still nearer to each other

* The figures made use of by this author to denote degrees of lustre, transparency, hardness, and sharpness of fragments, are explained in the following Table, which I have constructed from his section upon the distinctive characters of minerals.

Numbers or Degrees.	Lustre.	Transparency.	Fragments.
0	None.	None.	Perfectly blunt.
1	Exceedingly weak; or a few shining particles.	Only at the edges.	Less blunt.
2	Glossy; not more than of silk.	Transmits light, but not sufficient to discern objects.	Sharp.
3	Shining; as of crystals or of metals not much polished.	Transmits light enough to shew objects imperfectly through the mass.	More sharp.
4	Brightest; as of diamonds or polished metals.	Objects clearly distinguished through the mass.	Most sharp; as of glass.

On account of the higher range of numbers, degrees of hardness could not conveniently be included in the above Table. No. 3, denotes the hardness of chalk; 4, a superior hardness, but yet what yields to the nail; 5, that which will not yield to the nail, but easily and without grittiness to the knife; 6, that which yields more difficultly to the knife; 7, that which scarcely yields to the knife; 8, that which cannot be scraped by a knife, but does not give fire with steel; 9, that which gives a few feeble sparks with steel, as basalt; 10, that which gives plentiful lively sparks as flint. N.

than

than in Lavoiſier's ſtatement, and conſequently much nearer than in mine: for, in his Eſſay on Plumbago, he tells us that five parts nitre are ſufficient to conſume one of charcoal, and conſequently it ſhould ſeem that one part charcoal ſhould decompoſe no more than five of nitre. The conſequence however is not juſt, for undoubtedly five parts nitre would conſume one of charcoal, but it does not thence follow that they would not conſume ſtill more. On the other hand, he found that ten parts nitre were neceſſary for the conſumption of one part of plumbago; whence it follows, that one part of plumbago decompoſes ten of nitre, otherwiſe nine parts nitre would ſuffice to conſume it, and the tenth would have been unneceſſary, as it acts only as it is decompoſed. Now this proportion approaches very nearly to my reſults; namely, one of charcoal to 9,6 of nitre.

Hence, and ſince Kilkenny coal in the preceding experiments ſhewed no ſign of its containing any thing bituminous, I take it for granted that it conſiſts almoſt entirely of pure carbon; and ſince 50 grains of it alkalize 480 grains of pure ignited nitre, that in all the ſubſequent experiments on other ſpecies of coals or bitumens free from ſulphur and iron, the decompoſition of this ſtandard quantity of nitre will indicate, in the quantity of coal neceſſary for that decompoſition, the preſence of 50 parts of mere carbon.

Before I proceed to the recital of other experiments, I muſt mention another circumſtance that occurs in making them, which is, that, after the inflammation ceaſes, a hiſſing noiſe is perceived for a long time, and is increaſed on adding freſh quantities of coal, even when the nitre is ſeemingly decompoſed. This ſeemed to me to ariſe from the decompoſition of the nitrous air, or mephitized nitrous acid, of which a portion is always retained by the alkali; and conſequently I paid no attention to it, but always ceaſed adding coal when the inflammation totally ceaſed.

Maltha.

ITS colour is dark-brown, or black.

Luſtre 0. Tranſparency 0. Fracture uneven, tough. Specific gravity 2,070.

It feels ſomewhat greaſy, yields to compreſſion, has a heavy ſmell, acquires a poliſh when ſcraped, does not adhere to the tongue, or ſtain the fingers; its flame high and bright, leaving no coal, but only a little aſhes.]

Having but a ſmall quantity of this ſubſtance, I on this occaſion uſed only 240 grains of nitre: when it was heated to redneſs, I threw on it one grain of vegetable pitch; it immediately inflamed, but floated quietly on the ſurface of the nitre, and decrepitated like common ſalt from the moiſture it contained: the flame was partly white from the action of the air ſpontaneouſly emitted by the nitre, and partly yellowiſh from the action of the ambient atmoſpheric air, but ſteady, and unattended with thoſe turbulent guſhes that attend the decompoſition of nitre by carbonaceous ſubſtances.

I then gradually projected on it 55 grains of maltha, which was all I had: this burned juſt as the pitch, but attended with a blacker ſmoke; yet the nitre was ſo far from being alkalized, that, to produce this effect, I was obliged to throw on it 29 grains of cannel coal.

Now 33,5 grains of cannel coal, if it alone had been uſed, would ſuffice to alkalize 40 grains of nitre: it will preſently be ſeen, therefore, the 55 grains of maltha and the one grain of pitch contained no more carbon than $33,5 - 29 = 4,5$ grains. Therefore 100 grains of maltha contain no more than 8 grains of carbon. And as theſe 8 grains of carbon provoked no turbulent eruption of air from the nitre, it is plain they did not contribute

to

to its decompoſition, but were taken up by the air it ſpontaneouſly emitted, and partly by the ambient atmoſpheric air.

Aſphalt.

ITS colour is greyiſh black. Luſtre 2.3. greaſy. Tranſparency o.

Fracture perfectly conchoidal. Hardneſs from 7 to 8, very brittle. Specific gravity from 1,07 to 1,165 by my trials. It feels ſmooth, but not greaſy; has no ſmell, except while pounding; does not ſtain the fingers; when heated it melts, ſwells, and at laſt inflames; but it requires, for inflammation, a higher heat than maltha does, and leaves no coal, and ſcarce any aſhes.

Of this bitumen I found 161 grains requiſite to alkalize the ſtandard quantity of nitre: it viſibly educed air from the nitre; for there were eruptions from time to time. I ſuppoſe when the more oily part was conſumed, and the carbonaceous laid bare, much of the flame was alſo yellowiſh. Hence 161 grains of aſphalt contain only 50 of mere carbon, that is, nearly 31 per cent.

Mr. Thory, burning it in a low heat, found it to leave about of its weight of coal after melting, ſwelling and inflaming as uſual *: however, his aſphalt was not perfectly pure, as he obtained ſulphur from it.

Cannel Coal.

ITS colour is black. Luſtre 2. Common when freſh broken, often barely 1. Tranſparency o.

Croſs fracture conchoidal. Fragments rather ſharp. Hardneſs from 7 to 8. Specific gravity by my trials 1,232, by Dr. Watſon's 1,273; does not ſtain the fingers, eaſily kindles without melting, and burns with a large bright flame, but of ſhort duration, leaving a large coaly reſiduum; does not cake. 240 grains of it, heated until all the coaly part was conſumed, left 7,5 grains of reddiſh brown aſhes, moſtly argillaceous, that is, 3,12 per cent.

66,5 grains of it were ſufficient to alkalize the ſtandard quantity of nitre. It burned with a large bright flame, except the laſt portion, which was yellowiſh, the pure air of the nitre being then exhauſted. Hence 66,5 grains contained 50 of pure carbon, and 2,08 of aſhes; then deducting 52,08 from 66,5, we find the quantity of bitumen equal 14,42. Then 100 parts of it contain 75,2 of carbon, 21,68 bitumen of the ſort called maltha, and 3,1 of aſhes.

I take this bitumen to be maltha, from its quick inflammability, and the ſhort duration and brightneſs of its flame, both which properties indicate the moſt inflammable of the bitumens, and whoſe flame is leaſt durable from its refuſal to cake (caking being a property ariſing from the fuſion of aſphalt), and the difficult combuſtibility of the carbonaceous ſubſtance that remains after the ceſſation of its flame—qualities that counter-indicate aſphalt.

Slaty Cannel Coal.

THAT which I employed was from Ayrſhire in Scotland, the only one of this ſort imported to Dublin.

Its colour is black.

* 6 Crell's Chym. Jour. 62.

Its lustre 2. Common. Transparency 0.

Its fracture partly slaty, partly imperfectly conchoidal; fragments sharp.

Its hardness from 5 to 8. Specific gravity 1,426 by my trials.

It burns like the compact cannel, but ceases sooner to flame; does not cake—leaves a stony residuum. 240 grains of it, treated as before mentioned, leave 50 of reddish grey ashes, equal 20,83 per cent. From the smell that issues from it during ignition, I am led to think it contains some portion of sulphur.

To alkalize 480 grains of nitre, 105 grains of this coal were employed. It burned like the former with a large white continued flame, except the last portions. Hence this quantity contained 50 grains of mere carbon; and since it also contained 20,83 of ashes, the remainder, namely 34,15, must have been bitumen. Then 100 parts of it contain 47,62 of carbon, 32,52 of bitumen, and about 20 of ashes. Some deduction however from these quantities of carbon and bitumen may be made by reason of the small proportion of sulphur contained in it. This bitumen I take to be maltha, and not asphalt, for the reasons I mentioned in treating of compact cannel.

It is from a coal of this sort that Lord Dundonald extracts his tar, as maltha easily distils; but it is probably of a better kind, as this stony kind exists mostly in Ayrshire.

By his Lordship's mode of distillation, however, much seems to be lost during the internal combustion. I should think the Prince of Nassau Saarbruck's method in this respect more advantageous. Mr. Sage tells us, that, by distillation, he obtained from cannel coal $\frac{1}{4}$ of its weight of tar *; but Mr. Faujas, who uses Lord Dundonald's method, obtains from the coal of Decire, which seems to be of this kind, only 4 per cent. of tar †. Faujas also observed, that this tar is gradually converted into asphalt by long exposure to the air, which confirms the difference I have established between the two bitumens.

Whitehaven Coal.

ITS colour is black.

Its lustre 3. Greasy. Transparency 0.

Its fracture plane foliated. Its fragments 2, often discovering quadrangular or cubic distinct concretions, sometimes interfected with brownish red flakes.

Its hardness 6, very brittle. Specific gravity 1,257 by my trials. Stains the fingers, particularly when moist.

It burns at first with a clear flame, and for a long time; but at last cakes. 240 grains of it, after five hours strong heat, left only 4 grains of reddish ashes, or about 1,7 per cent.

The standard quantity of nitre was alkalized by 88 grains of this coal. Hence 100 grains of it contained nearly 57 of mere carbon, 41,3 of a mixture of maltha and asphalt, and 1,7 of ashes. That it contains both maltha and asphalt is evident from its flame and caking. The proportion I cannot exactly ascertain, but most probably the asphalt predominates.

Wigan Coal.

ITS colour is black.

Its lustre 3. Greasy. Transparency 0.

Its fracture plane foliated. The lamellæ, some uniformly, some promiscuously directed. In the gross often slaty; forms separate concretions, often with bright yellowish illinitions.

* Roz. Journ. p. 387. † Roz. Journ. p. 188.

Its

Its hardness 6. Specific gravity 1,268 by my trials. It burns with a bright flame and quicker than the foregoing, and is less apt to cake. 328 grains of it, exposed as the former to a strong heat, left 5,13 grains of ashes, that is, 1,57 per cent. 81 grains of it decomposed 480 grains of nitre. Hence 100 grains of it contain 61,73 of carbon, 36,7 of a mixture of maltha and asphalt, and 1,57 of ashes.

It seems to contain a larger proportion of maltha, with respect to its quantity of asphalt, than Whitehaven coal does.

Swansey Coal.

ITS colour is black.

Its lustre 2. Transparency 0.

Its fracture foliated; but some lamellæ being at right angles with the other, give it a fibrous or striated appearance. Fragments 2.

Its hardness 5, very brittle. Specific gravity 1,357 by my trials.

It burns more slowly than the former, and cakes.

240 grains of it, treated as the former kinds, left 8 grains of yellowish red ashes; that is, equal 3,33 per cent.

Of this coal 68 grains were requisite to decompose 480 grains of nitre. Then 100 grains of it contain 73,53 of carbon, 23,14 of a mixture of maltha and asphalt, and 3,33 of ashes. The asphalt seems to predominate.

Leitrim Coal.

ITS colour is black.

Its lustre when fresh broken 3. Transparency 0.

Its fracture foliated. Its fragments 2.

Its hardness 6, very brittle. Specific gravity 1,351 by my trials. It slightly cakes.

240 grains of it left, after three hours exposure to heat, 12,5 grains of reddish grey ashes, that is, equal 5,2 per cent.

The decomposition of the standard quantity of nitre required 70 grains of this coal. Hence 100 grains of it contain 71,43 of carbon, 23,37 of a mixture of maltha and asphalt, and 5,2 of ashes.

Newcastle Coal.

I HAD none of this kind of coal; but, according to Dr. Watson's experiment, it left on distillation a coaly residuum amounting to 58 per cent. and hence contained about 40 of a mixture of asphalt and maltha, in which the former appears to predominate. Hence it much resembles the Whitehaven coal; but it evidently contains sulphur also, which that of Whitehaven seldom does.

A synoptical View of the Contents of Bitumens and different Sorts of Mineral Coal.

100 parts	Carbon.	Bitumen.	Afhes.	Specific gravity.
Maltha	8	—	—	2,070 *
Afphalt	31	68	—	1,117
Kilkenny	97,3	—	3,7	1,526
Compact cannel	75,2	21,68 maltha	3,1	1,232
Slaty cannel	47,62	32,52 maltha	20	1,426
Whitehaven	57	41,3 mixt	1,7	1,257
Wigan	61,73	36,7 mixt	1,57	1,268
Swanfey	73,53	23,14 mixt	3,33	1,357
Leitrim	71,43	23,37 mixt	5,20	1,351
Newcaftle	58	40 mixt	—	1,271

To thefe refults I fhall add a few more taken from a Treatife on Pit Coal, lately publifhed by Signior Fabroni. The Italian coals were examined by himfelf; the French and German by other chemifts—All by diftillation.

100 parts	Carbon.	Bitumen.	Afhes.	Specific gravity.
Coals of Halles	86	12	—	—
of Tudertino	25	75	—	—
of Cortolla	45	43	12	1,403
of Macinaia	60	37	3	1,411
Stony of ditto	12,5	37,5	50	1,666
of Mocaio	32	35	33	1,403

Thefe coals contain very little afphalt, but chiefly maltha.

Moft coals afford a volatile alkali by diftillation :—this feems to me to be rather a product of the operation arifing from the union of hydrogene and mephitic air, and thus the alkaline bafis of the ammoniacs found on volcanos feems to have been formed. Coals alfo afford an acid, commonly the marine, or if pyritous, alfo the vitriolic, more rarely the fuccinous.

According to Mr. Jars, 100 parts of the beft Englifh coal give when charred 63 of coaks †; but Hielm found the refiduum of the beft Englifh coals diftilled, to amount to 73 per cent. and Dr. Watfon found the refiduum of Newcaftle coal to amount only to 58 per cent. Thefe refults neceffarily differ according to the degree of heat applied, the duration of the combuftion, and the variable admiffion of air. It is plain the bitumen is never totally expelled, at leaft not until moft of the carbon is confumed; but much more of it is expelled by combuftion than by diftillation. Watfon, p. 27 and 28 ‡.

* Probably 1,07.

† 1 Jars, 329.

‡ This paper is taken from the Second Volume of Elements of Mineralogy, lately publifhed.

III. *Obfervations*

III.

Observations on the best Methods of producing Artificial Cold. By Mr. RICHARD WALKER *.

HAVING already investigated the means of producing artificial cold, and at the conclusion of my last paper (on the Congelation of Quicksilver) dismissed that part of the subject, the best method of making use of those means naturally becomes a desideratum; to that therefore I have lately given my attention, and flatter myself that the following observations may be considered as an useful Appendix to my former papers. The freezing point of quicksilver being now as determined a point on the scale of a thermometer, viz. 39°, as the freezing point of water; and as this metal, exhibited in its solid state, affords an interesting as well as curious phenomenon, I shall apply what I have to say principally to that object.

Frequent occasions having occurred to me of observing the superiority of snow in experiments of this kind to salts, even in their fittest state, that is, fresh crystallised, and reduced to very fine powder, I resolved upon adopting a kind of artificial snow.

The first method which naturally presented itself was by condensing steam into hoar frost. This answered the purpose, as might be expected, exceedingly well; but the difficulty and expence of materials in collecting a sufficient quantity determined me to relinquish this mode for another, by which I can easily and expeditiously procure ice in the fittest form for experiments of this kind; the method I mean is, by first freezing water in a tube, and afterwards grinding it into very fine powder. Thus possessed of the power of making ice, and afterwards reducing it to a kind of snow, the congelation of quicksilver becomes a very easy and certain process; for, by the use of a very simple apparatus, (Plate XX. fig. 1.) quicksilver may be frozen perfectly solid in a few minutes, wherever the temperature of the air does not exceed 85°: thus, one ounce of nitrous acid is to be poured into the tube *b* of the vessel, observing not to wet the side of the tube above with it; a circular piece of writing-paper, of a proper size, is to be placed over the acid, resting upon the shoulder of the tube, and the paper brushed over with some melted white wax. Thus prepared, the vessel is to be inverted, and filled with a mixture of diluted nitrous acid, phosphorated soda, and nitrous ammoniac, in proper proportions for this temperature *, and tied over securely, first with waxed paper, and upon that a wet bladder.

The vessel being then turned upright, and placed in a shallow vessel, viz. a saucer or plate, an ounce and a half of rain or distilled water is to be poured into the tube, which is to be covered with a stopper or cork, and, as soon as frozen solid, ground to very fine powder, an assistant holding it firmly and steadily the while; observing occasionally to work the instrument in different directions up and down, that no lumps may be formed. When the whole of the ice is thus reduced to powder, and the lumps, if any, broken, the frigorific mixture is to be let out quickly, by cutting or untying the string, and removing the bladder, &c. which confines it; a communication made by forcing a rod of glass or wood through the partition; and the whole mixed expeditiously together.

* Philosophical Transactions, 1795.

† I have, by a very accurate preparation of this mixture, sunk a thermometer from 85 degrees (temperature of the vessel and materials) to + 2 degrees.

In this climate a mixture much leſs expenſive will be ſufficient; viz. that compoſed of diluted nitrous acid, Glauber's ſalt, ſal-ammoniac and nitre; a mixture of this kind ſinking a thermometer in the warmeſt weather to near 0°. At the temperature of 70 degrees, or a little higher, the quantity of diluted nitrous acid may be about one-fourth leſs than is mentioned in the table for 50 degrees.

Theſe methods are the moſt expeditious, and attended with the leaſt trouble; but as ice may be uſed with equal certainty, and with much leſs expence, I ſhall give a particular detail of an experiment made with the uſe of it, firſt mentioning a preparatory experiment, to which I was immediately led by the recollection that Sir Charles Blagden, in his paper "on the Point of Congelation," (Phil. Tranſ. vol. lxxviii.) had found that common ſal-ammoniac and common ſalt mixed with ſnow produced a cold of — 12 degrees, whereas the latter uſed alone with ſnow produces only — 5 degrees. I uſed a mixed powder of equal parts of common ſal-ammoniac and nitre with the common ſalt, by which the thermometer ſunk to — 18 degrees; and when I uſed nitrous ammoniac with common ſalt, to — 25 degrees. This cold I could not increaſe by the addition of any other ſalts, nor could I equal it by any other combination of ſalts. Thoſe I tried were, Glauber's ſalt, ſalt of tartar, ſoda, and ſal catharticus amarus. By ſeveral trials, I found the beſt proportions to be, ſnow or pounded ice twelve parts, common ſalt five parts, and of nitrous ammoniac, or a powder of equal parts, ſal-ammoniac and nitre mixed, five parts; or one-third of common ſalt, when I uſed that alone with ſnow or pounded ice.

My apparatus then (Dec. 28. laſt) conſiſted of two veſſels (fig. 3 and 4.); an inſtrument (fig. 6.) to grind or rather ſcrape the ice to powder; a kind of ſpatula (I uſe a marrow ſpoon) to ſtir the powder occaſionally; a thermometer (fig. 8.) and a ſmall thermometer glaſs, with the bulb three-fourths full of quickſilver (fig. 7.) I filled the veſſel fig. 3, holding when inverted two pints, *ſtratum ſuper ſtratum*, with pounded ice, common ſalt, and a powder conſiſting of equal parts ſal-ammoniac and nitre mixed together; by firſt putting in ſix ounces of pounded ice, then two ounces and a half of common ſalt, and, after ſtirring theſe well together, two ounces and a half of the mixed ſalts, mixing the whole well together: this was repeated in the ſame manner until the veſſel was quite full. It was then tied over ſecurely with a wet bladder, turned upright, and one ounce and a half of rain water poured into the tube through a funnel, the tube covered with a cork, and the veſſel left undiſturbed till the water was frozen perfectly ſolid. The inſtrument for grinding it was then put in to acquire cold, whilſt the veſſel fig. 4, holding a pint, was filled in the ſame manner, with the ſame proportions of materials; a bladder tied over it, ſet upright, and one ounce of fuming nitrous acid poured in to be cooled. The ice was then ground to powder; and when finiſhed, the nitrous acid being found to have acquired a ſufficient degree of cold, viz. — 13 degrees, the frigorific mixture of ice and ſalts was let out of the veſſel which contained the nitrous acid, and the powdered ice (ſtill ſurrounded by its frigorific mixture) added to the acid as quick as poſſible; when the thermometer ſunk to near — 50 degrees, and the mixture ſoon froze the quickſilver in the glaſs bulb. In this experiment 18 minutes were required to freeze the water perfectly ſolid, and 15 to reduce the ice by moderate labour to very fine powder. The experiment was over in 55 minutes, and the temperature of the preparatory cooling mixture then found to be — 10 degrees.

I had

I had a ſpirit thermometer by me; but a mercurial thermometer being much more ſenſible, and conſequently deſcending much quicker, I prefer it in experiments made merely to freeze quickſilver—knowing from experience how the congelation is going on, from the irregular deſcent of the mercury when a few degrees below its freezing point, and from having uſually found that the quickſilver in the thermometer glaſs begins to freeze as ſoon as the mercurial thermometer reaches — 40°.

Whenever I have occaſion to uſe ice in ſummer for this purpoſe, I uſually pound together firſt ſome ice and ſalt in a ſtone mortar, about two parts of the former to one of the latter; throw this away, and wipe the peſtle and mortar perfectly dry: the mortar being thus cooled, the ice may afterwards be pounded ſmall without melting.

And as a mixture made of ſnow or ice in powder and ſalts does not give out its greateſt cold till it is become partially liquid by the action of the ice and ſalts on each other, it is neceſſary that the whole be ſtirred well together, till it is become of an uniformly moiſt pulpy conſiſtence; eſpecially ſince, in becoming liquid, the mixture ſhrinks ſo much, that, if this be not attended to, the veſſel will not be near full, and conſequently the upper part of the tube not ſurrounded, as it ought to be, by the frigorific mixture. The diſſolution of the ice and ſalts may, if required, be haſtened, by adding occaſionally a little water; but then the cold produced will be leſs intenſe, and not ſo durable.

That particular form of the veſſel in which the ice is made and reduced to powder is choſen, becauſe it ſubjects the powdered ice in the tube to the conſtant action of the freezing mixture, without which it would be leſs fit, particularly in warm weather, for the intended uſe; and becauſe in it the ice is not liable to be impregnated with the ſalts of the mixture, by which it would be utterly ſpoiled; and that for cooling the nitrous acid, and making the ſecond mixture in, becauſe it is ſteady, and is beſides inſulated, as it were, from the external warm air, and ſurrounded in its ſtead by an atmoſphere much colder.

It is ſcarcely neceſſary to add, that when ſnow which has never thawed can be procured, it may be cooled in this apparatus by a mixture of ſnow (inſtead of the pounded ice) and the ſalts, and the trouble of reducing the ice into powder ſaved.

I prefer the red fuming nitrous acid, becauſe, as I have obſerved in a former paper, it requires no dilution. Being under the neceſſity at one time of uſing the pale nitrous acid, I found it required to be diluted with one-fifth its weight of water. The beſt and only way of trying or reducing any acid to the proper ſtrength, is by adding ſnow, as Mr. Cavendiſh directs, or the powdered ice to it, until the thermometer ceaſes to riſe; then cool the acid to the ſame temperature of the ſnow again; add more ſnow, which will make the thermometer riſe again, though leſs; cool it again, and repeat this until the addition of ſnow or powdered ice will not make the thermometer riſe: to be very accurate, it ſhould be reduced in this manner to the proper ſtrength, at the temperature, whatever it be, at which the nitrous acid and ſnow, or powdered ice, are to be mixed together when cooled.

In the courſe of my experiments I have endeavoured to aſcertain the comparative powers of ice to produce cold with nitrous acid in the different forms I have had occaſion to uſe it. The reſult is, that freſh ſnow ſunk a thermometer to — 32 degrees, ground ice to — 34 degrees, and the moſt rare frozen vapour to below — 35 degrees; the veſſel and materials each time being + 30 degrees.

The

The veſſels for theſe mixtures, particularly that in which the quickſilver is to be frozen, ſhould be thin, and made of the beſt conductors of heat; firſt, becauſe thin veſſels rob the mixture of leſs cold at mixing, that is, if two mixtures of the ſame kind are made, one in a thin, the other in a thick veſſel, the former will be coldeſt; ſecondly, becauſe the air is a ſufficiently bad conductor; and thirdly, for the very obvious reaſon that the cold is tranſmitted through them quicker.

For theſe reaſons, and from the difficulty I have found in procuring veſſels of glaſs, which are undoubtedly fitteſt for experiments of this kind, I have uſed tin, which is readily had in any form, and, if coated with wax, is ſufficiently ſecured againſt the action of the acids.

I give the inſide ſuch a coating by pouring melted white wax into the veſſel, previouſly clean and dry, and turning it about by hand, ſo as to leave no point of the metal uncovered for the acid to act on, pouring the ſurplus away.

In the experiments above deſcribed, I uſed a ſingle veſſel for cooling the nitrous acid; a cupping-glaſs (repreſented by the dotted line at *b* fig. 4.) being cemented into the tin, and thereby forming that part in which the nitrous acid was firſt cooled, and the mixture afterwards made in which the quickſilver was frozen; but from the trouble and impediments ariſing from letting out the mixture, and clearing the bottom from the lumps of ice, &c. adhering to it, I was led to the addition of the other part (fig. 5.) by which all theſe difficulties are got rid of: and it is beſides a much more comfortable and neat way of conducting it; the upper part, which contains the nitrous acid, being lifted off, and placed on the table immediately before the powdered ice is added.

The whole of this apparatus may be of tin; that part only (when the cooling mixtures are made without uſing any corroſive acid) in which the acid mixture is to be made being previouſly coated in the manner above mentioned; or a thin glaſs tumbler, of a proper ſize, may be cemented in.

I have occaſionally uſed a thin glaſs tumbler for the mixture in which the quickſilver is to be frozen, immerſing it, with the acid, in a frigorific mixture, till the acid is ſufficiently cooled; then adding the ground ice to it, previouſly removing the tumbler out of the frigorific mixture, as in the experiment above mentioned: this ſimplifies the apparatus, but is leſs convenient on many accounts.

The ſcale of this apparatus may be diminiſhed or increaſed at the will of the operator; for there is no doubt that a ſmall quantity of quickſilver may be frozen at any time with one-fourth of this quantity, with an apparatus of this kind, by any one converſant in ſuch experiments.

I have frequently frozen quickſilver by mixing together at 0°, three drams of ground ice with two drams of nitrous acid.

Whenever the intention is, as in theſe experiments, to cool the materials to nearly the ſame temperature with the frigorific mixture in which they are immerſed, the proportion of the frigorific mixture to the intended mixture (or materials to be cooled) ſhould not be leſs than twelve to one; a greater diſproportion is ſtill better.

By attending to the directions particularly mentioned in the experiment made on December 28, a thermometer may be always diſpenſed with; the proportions of the materials

terials to be cooled being exactly adjuſted, and, when they are to be mixed, preciſely determined by the time employed in grinding the ice to powder. The proportions of ſnow or pounded ice and ſalt or ſalts may be gueſſed ſufficiently near without weighing, unleſs in very nice experiments.

Imagining that a recapitulation of the different mixtures deſcribed in my former paper for producing artificial cold, brought into one view, might not be unuſeful, I have ſubjoined a table of the ſalts, their powers of producing cold with the different liquids, and the proportions of each, according to a careful repetition of each; the temperature being 50 degrees.

SALTS.	Liquor.		Temperature of Cold produced.
* Sal-ammoniac 5, nitre 5 — —	Water	16	— + 10°
Sal-ammoniac 5, nitre 5, Glauber's ſalts 8 —	—	16	— + 4
* Nitrous ammoniac 1 — —	—	1	— + 4
Nitrous ammoniac 1, ſal ſoda 1 — —	—	1	— 7
Glauber's ſalt 3 — — —	D. nitr. acid	2	— 3
Glauber's ſalt 6, ſal-ammoniac 4, nitre 2 —	—	4	— 10
Glauber's ſalt 6, nitrous ammoniac 5 — —	—	4	— 14
Phoſphorated ſoda 9 — — —	—	4	— 12
Phoſphorated ſoda 9, nitrous ammoniac 6 —	—	4	— 21
Glauber's ſalt 8 — — —	Marine acid	5	— 0
Glauber's ſalt 5 — — —	D. vitr. acid	4	— + 3

N. B. I have choſen the temperature of 50 degrees, becauſe the materials may at any time, by immerſion in water drawn from a ſpring, be cooled nearly to that temperature, and the experiment for freezing with any of theſe mixtures commence there.

At a higher temperature than 50 the quantity of the ſalts muſt be increaſed, and the effect will be proportionably greater: at a lower temperature diminiſhed, when the effect will be proportionably leſs.

It muſt be obſerved, that, to produce the greateſt effect by any frigorific mixture, the ſalts ſhould be freſh cryſtallized †, not damp, and newly reduced to very fine powder; the veſſel in which they are made, very thin, and juſt large enough to contain the mixture; and the materials mixed intimately together as quickly as poſſible, the proper proportions at any temperature (thoſe in the table being adjuſted for the temperature of 50° only) having been

* The ſalts from each of theſe may be recovered by evaporating the mixture to dryneſs, and uſed again repeatedly.

N. B. The figures after each ſalt, and after the liquor, ſignify the proportion of parts by Troy weight to be uſed; the trouble of weighing the water may be ſaved by obſerving, that a full ounce of it by wine meaſure, correſponds exactly with one ounce of it by Troy weight: likewiſe it muſt be noticed, when more kinds of ſalts than one are uſed, to add them to the liquor one after the other in the order they ſtand in the table, beginning on the left hand, and ſtirring the mixture well between each addition. D. nitr. acid is red fuming nitrous acid two parts, and rain or diſtilled water one part by weight, well agitated together and become cool. D. vitr. acid is ſtrong vitriolic acid, and rain or diſtilled water equal parts by weight, thoroughly mixed (very cautiouſly) and cooled.

† Soda, phoſphorated ſoda, and Glauber's ſalt, are beſt cryſtallized afreſh, becauſe their effect, eſpecially the two laſt, in the acids, depends upon the quantity of water they contain in a ſolid ſtate.

previouſly

[illegible] tried, by adding the powdered ſalts gradually to the liquid till the thermometer ceaſes to ſink; obſerving to produce the full effect of one ſalt before a ſecond is added, and likewiſe of the ſecond before a third is added. Neither ſoda, phoſphorated ſoda, nor Glauber's ſalt, ſhould be mixed with nitrous ammoniac, or the powder compoſed of ſal ammoniac and nitre, unleſs at a low temperature, that is, below 0; but pounded and kept apart.

In the experiments alluded to in the table, the precaution of freſh-cryſtallizing the ſalts was not obſerved, becauſe I choſe to give the ordinary effects only. I therefore then uſed ſalts in their common ſtate, taking care, however, to chooſe ſuch as had not in the leaſt effloreſced.

Since it is always uſeful, and generally abſolutely neceſſary, to know how much room in a veſſel the ſeveral materials take up ſeparately; and when mixed, it will be right to obſerve, that ſnow or ice in powder, at near 0°, occupy in meaſure nearly two thirds more than their weight; that is, one ounce weight of water will, when in the form of ſnow, or ice ground to powder, nearly fill a veſſel which holds three ounces wine meaſure; powdered ſalts, nearly double their weight; ſtrong nitrous acid, about three-fourths its weight; and a mixture made of ſalts and diluted nitrous acid; meaſures rather leſs than two-thirds of the weight of the ingredients. Without a previous knowledge of this, it is impoſſible to adjuſt the ſize of the veſſels to the mixtures which are to be made; becauſe, in moſt nice experiments of this kind, the height to which a veſſel will be filled is indiſpenſably neceſſary to be known beforehand.

The long continuance of the late froſt having afforded me opportunities of repeating theſe experiments in various ways, I ſhall mention briefly the reſult of ſuch as appear to me to be material.

I have found, that ice may be ground ſo fine as to be equal to frozen vapour; and the harder it is frozen, the finer it is ground, but with more labour.

That quickſilver may be frozen by cooling the nitrous acid only, ſaving the trouble and inconvenience of cooling the ſnow likewiſe; either by adding ſnow at + 32 degrees, to nitrous acid at — 29 degrees; or ſnow at + 25 degrees, to nitrous acid at — 20 degrees; or ſnow at + 20 degrees, to nitrous acid at — 12 degrees. Moſt winters offer an opportunity of doing it in this way; the nitrous acid may be cooled in a mixture of ſnow and nitrous acid.

That it may likewiſe be frozen by mixing expeditiouſly together ſnow and nitrous acid, when the temperature of each is + 7 degrees.

Or by mixing ground ice and nitrous acid at + 10 degrees.

Hence it follows, that the cold of this climate offers occaſionally opportunities of freezing quickſilver, without previouſly cooling by art the materials to be mixed; for I have once ſeen the thermometer at + 6 degrees—and others, I believe, have ſeen it lower.

I expected an opportunity would have offered this winter, but the loweſt point I ſaw my thermometer at this ſeaſon was only + 10 degrees. At this temperature I mixed nitrous acid (cooled out-of-doors to the temperature of the air) and ſnow on January 23d laſt; but the cold produced was not quite ſufficient to freeze the quickſilver, although very near it, as indicated by a thermometer. From what I have obſerved ſince theſe latter experiments were made, I think it may be reaſonably expected that powdered ice and nitrous acid at + 14 degrees, or ſnow at + 10 degrees, will ſucceed, if mixed expeditiouſly.

Strong

Strong ſpirit of vitriol, whoſe ſpecific gravity is 1,848, required to be diluted with half its weight of water, and produced with ſnow at the temperature of + 30 degrees, about eight degrees leſs than with nitrous acid, ſinking the thermometer to — 24 degrees; four parts of the diluted vitriolic acid required at that temperature ſix parts of ſnow.

It perhaps will be remarked, that I have taken no notice before of the vitriolic acid. The reaſon is, becauſe, the freezing point of quickſilver being 39, it may be frozen tolerably hard by a mixture of nitrous acid with ſnow or ground ice, though the utmoſt degree of cold this acid can produce with ſnow is — 46°, which degree of cold may be produced by mixing the ſnow or ground ice and nitrous acid at 0°.

If it be required to make it perfectly ſolid and hard, a mixture of equal parts of the diluted vitriolic acid and nitrous acid ſhould be uſed with the powdered ice; but then the materials ſhould not be leſs than — 10° before mixing.

If a ſtill greater could be required than a mixture of this kind can give, which is about — 56 degrees, the diluted vitriolic acid alone ſhould be uſed with ſnow or powdered ice, and the temperature at which the materials are to be mixed not leſs than — 20 degrees.

Select, according to the intention, either of the three following mixtures:

Firſt, ſnow or pounded ice two parts, and common ſalt one part, which produces a cold of — 5 degrees.

Second, ſnow or pounded ice twelve parts, common ſalt five parts, and a powder, conſiſting of equal parts of common ſal-ammoniac and nitre mixed, five parts, which produces a cold of — 18 degrees.

Third, ſnow or pounded ice twelve parts, common ſalt five parts, and nitrous ammoniac in powder five parts, which produces a cold of — 25 degrees.

The proportions which I have found to be the beſt for mixing the ſnow or powdered ice with the different acids at different temperatures are theſe: viz. at + 30 degrees, ſeven of of the former to four of the nitrous acid; at + 5 degrees, (with a trifling allowance, if any, for a few degrees above or below) three to two; at — 12 degrees, four to three, with the mixed acids, and at — 20 degrees, with the diluted vitriolic acid, equal parts.

If it be required to prepare the materials in a frigorific mixture without the uſe of ice, a mixture of the proper ſtrength may be choſen from the table.

It is immaterial, when the exact proportions of each are known, whether the powdered ice be added to the acid, or the acid poured upon that, provided the powdered ice be kept ſtirred to prevent lumps forming, and the materials be mixed as quick as poſſible. But when the proportion is not known, it is better to be provided with more powdered ice than is expected to be wanted, and add it to the acid by degrees, until the greateſt effect is produced, as ſhewn by a thermometer.

The conſiſtence is a pretty ſure guide to thoſe accuſtomed to mixtures of this kind, viz. when freſh additions of ſnow or ice do not readily diſſolve in the acid, though well ſtirred, and the mixture acquires a thickiſh flocculent appearance.

Snow or powdered ice that have ever been ſubjected to a cold leſs than freezing are ſpoiled, or rendered much leſs fit for experiments of this kind.

I prefer the method of adding the powdered ice or ſnow to the acid in a ſeparate veſſel, principally becauſe the ſize of that veſſel may be exactly adjuſted to the quantity of mixture it is to contain.

A mix-

A mixture made of diluted nitrous acid, phoſphorated ſoda, and nitrous ammoniac, (by much the moſt powerful of any compounded of ſalts with acids,) prepared with the greateſt accuracy, is not quite equal to a mixture of ſnow and nitrous acid, each mixed at + 30 degrees, although very nearly ſo.

Though quickſilver may be frozen by ſalts diſſolved in acids, it is neceſſary that the materials be cooled previouſly to mixing, much lower than when ſnow or ground ice are uſed.

If it be required to mix the powdered ſalts and acids at a low temperature, the beſt method is this:—Put firſt the nitrous ammoniac into the tube of ſuch an apparatus as fig. 1, ſhaking it down level, gently preſſing the upper ſurface ſmooth; then the phoſphorated ſoda or Glauber's ſalt: cover this with a circular piece of writing paper, and pour a little melted white wax upon it, and when cold pour upon this the diluted nitrous acid: immerſe this in a frigorific mixture till it is ſufficiently cold, as found by dipping the thermometer into the liquor occaſionally; force a communication through, and ſtir the whole thoroughly together, contriving that the upper ſtratum of ſalt, that is, the phoſphorated ſoda or Glauber's ſalt, be mixed with the liquor firſt, and then the nitrous ammoniac; the powdered ſalts do not require ſtirring whilſt cooling, like ſnow, for, however hard they are frozen, they will readily diſſolve in the acid: care muſt be taken that the partition be perfect between the ſalts and the liquor, and that in this and every inſtance where the materials are to be cooled they be immerſed below the ſurface of the frigorific mixture. The ſtrength of the red fuming nitrous acid uſed in theſe experiments I found to be 1,510, and that of the vitriolic acid 1,848.

I have thought it better, for the ſake of brevity, not to uſe in this, as in my former papers, the new chemical names, eſpecially as the old ones are more generally known.

Theſe experiments were chiefly made in a warm room, not far from the fire-ſide.

I have now finiſhed my propoſed plan reſpecting the beſt modes of conducting experiments on cold; in which it will appear, that I have reduced the congelation of quickſilver, in any climate, at any ſeaſon, to a certain and almoſt as eaſy a proceſs as that I originally ſet out with for the freezing of water, (Phil. Tranſ. vol. LXXVII.) viz. by previouſly cooling the materials in one mixture, to produce the effect in a ſecond. It may very likely appear to ſome, that I have been too minute in a few particulars; yet, as perhaps experiments of this kind, all circumſtances conſidered, are inferior to few in the delicacy required to make them ſucceed completely, I truſt I ſhall be excuſed by thoſe who chooſe to repeat them, particularly ſuch as are not in the habit of making experiments of this kind—eſpecially if it ſecure them from an unſucceſsful attempt, and that perhaps without being able to account for it.

Oxford, March 1, 1795.

It is very well known, that vitriolic ether will produce ſufficient cold by evaporation to freeze water. This circumſtance is noticed by many; and ſeveral different methods have been propoſed, particularly one by Mr. Cavallo, with a very ingenious apparatus for the purpoſe (Phil. Tranſ. vol. LXXI.); nevertheleſs, as I am upon the ſame ſubject, and the following experiments differ, as well in the effect produced, as in the particular mode of conducting them, from any I have met with, I have ventured to mention them.

June

June 29, 1792, temperature of the air 71 degrees, I funk a thermometer (the bulb being covered with fine lint tied over it and clipped clofe round) by dipping it in ether, and fanning it to 26 degrees; then by expofing the thermometer to the brifk thorough air of an open window, to 20 degrees; and again, by ufing fome of the fame ether, but which had been purified by agitating it with eight times its weight of water, applied exactly as in the laft experiment, the thermometer funk to 12 degrees. Water tried in the fame manner at the fame temperature, funk the thermometer to 56 degrees.

A whirling motion was given the thermometer during each experiment.

The lint was renewed for each experiment, and the bulb required to be dipped into the ether thrice; the firft time fufficiently to foak it; after which the thermometer was held at the window till it ceafed to fink; then a fecond quick immerfion, and likewife a third, expofing the thermometer in like manner after each immerfion.

In this manner, a little water in a fmall tube may be frozen prefently, by good ether not *purified*, at any time, efpecially if a fmall wire be ufed to fcratch or fcrape the fides of the tube below the furface of the water.

During the warmeft weather of laft fummer, I frequently froze water in this way.

Explanation of Plate XX.

FIG. 1. is a veffel in one piece, open at the bottom; *a a* the body, holding, when inverted, two pints; *b* the tube, holding five ounces; the lower or fmaller part (formed by a contraction or leffening of the tube in diameter, merely for the purpofe of leaving a fmall fhoulder for a temporary partition), holding rather lefs than one-fifth of the whole.

Fig. 2. is a veffel confifting of two parts; *a a* the body, holding two pints; *b* the tube, holding five ounces; which, together with the lid *c*, forms a cover to take off and on the veffel.

N. B. This veffel may, if preferred, be ufed inftead of fig. 1. the parts correfponding with it, except in not being open at bottom, and the continuation of the tube upwards juft fufficient to ferve for a handle.

Fig. 3. is a veffel in one piece, open at the bottom, holding, when inverted, two pints; *b* the tube, holding four ounces and a half.

Fig. 4. a veffel open at bottom, holding, inverted, one pint.

Fig. 5. a cover to fig. 4. *a a* the body, fitting exactly over, and *b* the cup-part (holding three ounces) fitting exactly within the correfponding parts of fig. 4.

Fig. 6. the inftrument for grinding the ice into powder; it works upon a fhort centre-point, and has the edge bevilled contrary ways on each fide the point, fo as to follow. The finenefs of the powder is regulated by the degree of preffure ufed. The handle is wood, the reft metal; *a* is a fliding cover fitting on the tube in which the ice is ground, to exclude the external air, and to keep the inftrument fteady; *b* is the fhoulder or guard, to prevent the point of the inftrument from touching fo as to endanger injuring the bottom of the tube. It fhould be made fo as to fit without grating the infide of the tube in ufing.

The tubes of each of the veffels fhould be fomewhat fhorter than the veffel, fo as not quite to reach the bottom of it.

Fig. 7. a thermometer-glafs, with the bulb three-fourths full of quickfilver.

Fig. 8. a thermometer with the lower part of the fcale-board turned up with a hinge,

 for

for the convenience of taking the temperature of fmall quantities, or of mixtures in which mineral acids form a part.

N. B. Thefe veffels are reprefented as in glafs, that being undoubtedly fitteft for purpofes in which corrofive acids are to be ufed.

IV.

The Defcription of a New Portable Electrical Machine. Invented by the Rev. W. PEARSON, *of Lincoln.*

AN electrical machine fufficiently portable, and at the fame time poffeffing power adequate to all the purpofes of medical electricity, has long been a defideratum among thofe profeffional gentlemen who attend the infirm, and are perfuaded that electricity has a fanative influence in many diforders of the human body. The electrophore and medical bottle, as alfo the prepared ribbon and apparatus attached thereto, do both poffefs the former property, but are found to be greatly deficient in the latter. Mr. Colman *, in his ingenious treatife on the means proper to be ufed for reftoring fufpended refpiration, propofes an electrical machine (fuch as is both portable, and likewife poffeffes power fufficient to give a fhock of 35 inches of coated furface at about a quarter of an inch ftriking diftance) to conftitute one important article of his improved apparatus for the recovery of perfons apparently drowned or fuffocated ; but feems to lament that we have no machine anfwering fuch a defcription. A confideration of fuch magnitude as the want of an inftrument which promifes to benefit fociety in fo critical a moment as is that of the *apparent extinction of the vital principle*, has turned the mind of the writer of this article frequently upon the fubject, as having fome relation to his clerical function : he has in theory projected various conftructions of a portable machine, which mature confideration convinced him would be objectionable in practice. For to anfwer all the purpofes requifite for conftituting an electrical machine fuitable for Mr. Colman's purpofe, it appears neceffary not only that it have the two defired properties of portability and power, but alfo that its power be guarded fo as not to be annihilated by the moifture when ufed in the open air ; that it be capable of being ufed without a table or ftand in cafes of neceffity ; that it be rendered fit for ufe upon the fhorteft notice ; that when ufed it charge the coated furface without confiderable lofs of time ; and laftly, that it be not liable to be eafily broken.

The writer, however, flatters himfelf he has at length hit upon a conftruction which comprehends all thefe properties, and which he recommends to the notice of inftrument-makers, from a conviction that it is practicable. Indeed he has conftructed more than one machine, which anfwer his moft fanguine expectation, and which therefore he will the more circumftantially defcribe.

Fig. 1. Plate XXI. reprefents a view of the machine with the eye placed directly over it at a fmall diftance ; A B C D is a fmall mahogany box $9\frac{1}{2}$ by $7\frac{1}{2}$ inches within, and 6 inches

* As it is two years fince I perufed this treatife, and I have not feen it fince, I cannot be certain of the exact words of the title, nor whether the name be Colman or Coleman. W. P.

deep,

deep, made of half-inch board: upon the bottom of the box lies a plate of glafs, alfo $9\frac{1}{2}$ by $7\frac{1}{2}$ inches, cemented to the wood at the four corners by common electrical cement; this plate is coated on both furfaces with tin-foil 7 inches by 5, as reprefented at Q in figure 2 (except that the flips of tinfoil at H and I are pafted or gummed upon the *under* fide only), and is ufed as a fubftitute for a jar of 35 inches of coated furface, it being of no confequence what the fhape of the glafs be, provided it have a proper quantity of coating. M E N is a cylinder of glafs of four inches diameter, the body of which is $7\frac{1}{2}$ inches long: at M and N are brafs or * boxen caps cemented as in other machines, upon the fmall cylindrical ends of which the cylinder revolves by means of a fimple winch C, which may be taken off or put on at pleafure, either by fcrewing, or by being inferted upon a fquare fhoulder. The central points of revolution of N and M are at about $2\frac{1}{2}$ inches from the top of the box, the firft inferted into a circular hole in one end of the box, and the other *let down* in an open place made down through the other, which has a detached piece of fimilar wood to fit it, and to keep the cylinder in its place. L is the cufhion, and E the filk placed in the ufual way; D is a fcrew with a milled head, which, by the affiftance of a tapped nut, placed faft in the infide of the box at 4 inches from the bottom, preffes againft the elaftic part of the *fupport* of the cufhion which is hid from the eye: this fupport, which may be of elaftic wood, coated above D with tinfoil, or of any elaftic metal, is fcrewed faft to the back of the box near the bottom in the infide. The chain at D is hung on the fcrew at pleafure, as the wood is found to be in a good or bad conducting ftate.

F is a piece of light wood turned very fmooth in a lathe, and neatly covered with tinfoil, its ends being rounded. This piece, which I fhall call the collector, is furnifhed with about a dozen fixed pins of brafs, projecting againft the fide of the cylinder, as reprefented in the figure, as near as may be without touching it. The collector is 6 inches long, and fomewhat more than an inch in diameter, and is fupported by two folid glafs pieces, C C, of an inch and three quarters in length, and nearly half an inch in diameter, cemented into the fide of the box at $4\frac{1}{2}$ inches from the bottom, and each at an inch from the end of the collector: by this means the collector becomes infulated, and has a chain to be hooked on an eye of wire fixed on any part thereof, fo as to fall occafionally upon the upper infulated coating of the plate of glafs in the bottom of the box: the chain, however, muft not be fo long as to extend beyond the coating by any motion of the box.

B is a glafs tube † of about one-eighth of an inch bore, and about 3 or $3\frac{1}{2}$ inches long, with a ring of brafs or horn, that has a male fcrew, cemented upon the middle of it, to fcrew into another ring that has a female fcrew, and is cemented or otherwife faftened into the

* When caps are made of wood, they muft be turned fo as to admit the glafs neck of the cylinder into a fpace or cavity which it fhall nearly fit. By this difpofition, the glafs is cemented to the wood at both furfaces, and the cap itfelf will be ftronger. W. P.

† Mr. Nicholfon may have been aware, that glafs-tubes of a fmall bore, however thick, are liable to be broken by a fhock paffing through them, when unguarded; and alfo that a lining of any kind will ufually prevent fuch an effect. Mr. Fell of Ulverfton, in Lancafhire, an excellent electrician, has tubes placed over the wires of a very large battery, lined with paper or tinfoil, which do not break with a difcharge that melts pretty thick brafs wire. The tubes are ufed as a guard againft any accidental touch when the battery is charged. W. P.

front side of the box at $4\frac{1}{2}$ inches from the bottom. Through this tube (when coated with varnish or substance of any kind within, to prevent its breaking by a discharge) passes a brass wire as thick as will easily move in it, about $4\frac{1}{2}$ inches long, tapped about an inch at each end and a little smoothed; on each end of this wire is a tapped nut which screw back or forwards to or from the ends of the tube, so as to hold the wire in any situation that may be required with regard to the distance of its ends from those of the tube. The wire has also two brass balls, one of which has its diameter less than that of the tube, that it may move through the circular hole into the box, to prevent its falling on the glass plate, which it might do if screwed off and on within the box.

O O are the directors with handles of glass, or baked wood varnished, each six inches long, besides the balls and wires; the balls are fast upon the wires, and the wires screw into sockets on the ends of the handles.

There is yet one part of the apparatus which it was not necessary to exhibit in the plate, viz. the insulating stool; this is made exactly of a size to cover the box, namely $10\frac{1}{2}$ inches by $8\frac{1}{2}$, and has four feet of glass, or baked wood varnished, almost 6 inches long. Underneath that part of the stool which covers the collector slides a little drawer, fig. 3. by means of the end-pieces P P, which drawer contains the tube, with its appendages, milled nut. chains, handles of the directors, and amalgam, and must be taken from the stool when used. The three compartments of the drawer, by being made to fit the directors and tube, prevent their shaking in carriage. When packed, the four feet of the stool go exactly into each corner, and the edge of the drawer just within the side of the box, by which means the stool is kept in its place as a cover; and lastly the wires of the directors pass through the stool, which is of inch plank, at the middle near the two ends, and, by screwing into the fixed nuts in the edges of the box's ends at K and K, until the balls touch the wood of the stool, fix the whole so firm that a handle, such as is used in a chest of drawers, fixed on the centre of the stool, serves for carrying the whole apparatus by, which has the appearance of a lady's small travelling-box, and weighs eight pounds. After having been thus prolix in the description of the separate parts of this small machine, I come next to say a few words about the use of it.——As it is necessary before using any machine to have it perfectly dry, and as the separate parts require to be successively placed before the fire, this machine will be found to possess an advantage even in the preparation, which greatly conduces to expedition; for after the box is disengaged from the stool, and has the tube screwed into its place (both which may be done in one minute), the box may be held in the hand before the fire, where all its parts will be alike dried at the same time; or it may be placed on the end A or side D at any convenient distance from the fire on the floor, as occasion may require, whilst the amalgamated leather is preparing.

The machine being freed from moisture and dust, and amalgam applied to the cylinder, the shock is administered in the following manner, viz. Let the small chain fall from the collector upon the upper coating of the glass plate, and place the inner ball at the required striking distance, suppose a quarter of an inch, as the case may require, and fix it there by means of the adjusting nuts, and the quantity of the charge will be limited in the same manner as by Lane's electrometer; then connect one chain with the conducting wire which passes through the tube, and the other with a brass ring connected with the slip of the under coating at H (as appears in figure 1, or at I, as is most convenient), and hook the oppofite ends upon the wires of the directors, whereby a fhock may be fent through any particular part of the body, as well as if the medical jar were ufed with Lane's electrometer. It is immaterial whether the machine, in this operation, be held on a table or ftand, the operator's knee when fitting, the bed of a patient, or even the ground by the fide of a river in cafe of neceffity where the patient is apparently drowned: nay, it may be held, by a perfon ftanding, in one arm, whilft the other hand turns the winch.

When the fpark only is required, the fmall chain is removed from the collector, and alfo the long chain, connected with the hook at H, is taken away; the inner ball is then fixed fo as to touch the collector, by means of the adjufting nuts; in which cafe the collector, the wire in the tube, and the fecond long chain connected therewith, form together one conductor. After the exterior end of this chain is attached to the wire of one of the directors, the fpark may be directed *into* any particular part of the body; when the eye is the part affected, a pointed wire muft be ufed inftead of the wire carrying the ball, which muft be made to fcrew into the focket of the director.

When a fpark is taken *out of* the body, the patient muft ftand on the ftool, and hold the chain, connected as before, in his hand; and then the operator, or affiftant, may take fparks from him, as in other machines.

If the patient be nervous, or afraid of the fpark, it would be fafer to hook the chain to the collector for fear of breaking the tube, and hold it at a diftance from the edge of the box in a perpendicular direction. The fparks taken from an infulated body are ftronger than thofe fent into any perfon by reafon of the fmallnefs of the conductor, which is perhaps the greateft defect of the machine: this defect may, however, be remedied by placing any rounded metallic fubftance, fuch as a large candleftick, upon the ftool near the machine, and connecting the chain with it, fo as to make it a part of the conductor, and then the fparks will become more denfe.

There is yet another way of adminiftering the electric fluid, which cannot properly be called adminiftering either the fpark or the fhock; in which cafe the fenfation is not fo momentary as is that caufed by the fhock, but more pungent than that of the fimple fpark; it may be called the interrupted fhock, and is thus effected: Let the fhort chain fall from the collector upon the coating of the plate, and connect one of the long ones with the hook at H or I, and fuffer it to be extended on the table; then, the inner ball touching the collector, turn half a dozen rotations before the finger be prefented to the outer ball; and becaufe the circle is not completed between the two coated furfaces of the plate by *perfect* conductors, a continuous ftream of denfe fparks will iffue into the finger, caufing a more pungent fenfation than would be felt from a fimple fpark of a much more powerful machine. I fhould fuppofe the ftimulant power of this mode of electrifying any particular limb to be very great, and I underftand it has been practifed by fome electricians, by means of a charged jar where the circle is not completed by perfect conductors.

If a fmall pocket-jar, in a cafe, be added to this machine, it will be capable of producing either a negative or pofitive charge in it; for if the jar held by the ball connected with its inner coating be prefented fo as to receive a pofitive charge on its outer coating from the outer ball of the machine, the inner coating will of courfe be negatively charged; and if it be placed upon the ftool, or inverted goblet, till the hand has taken hold of the outfide, the negative charge will be retained.

Thus

Thus this little machine is capable of affording much amuiement, and even of trying experiments with, befides its being adapted for medical purpofes; and, which is not the fmalleft recommendation, may be manufactured at a fmall expence, compared with the larger machines. But, fmall as it is, I have frequently fired ardent fpirits with it, and have feen its power fuch as to caufe 35 inches of coating to difcharge at a quarter of an inch ftriking diftance at every third revolution; and at fourteen or fifteen revolutions it will, when in good order, difcharge at half an inch. A fecond machine of this conftruction, which I lately made for a friend, contains 42 inches of coated furface, with a cylinder of the fame fize as the one already defcribed, and is better adapted for making experiments, the box being larger, and the infulation more perfect; but is not fo portable.

Before the former machine was completed, I apprehended that the proximity of the wood to the collector would take off much of the fluid; but it does not appear that this is the cafe at all in giving fhocks, nor even in taking fparks, unlefs the diftance of the body to be electrified from the ball ufed be greater than the diftance from the collector to the wood, or rather from the adjufting fcrews to the wood, for here the tendency to efcape feems to be the greateft. On this account the parts of the box where the fluid has the greateft tendency to efcape, are varnifhed with feveral coats of fealing-wax melted in fpirits of wine. How far baked wood would be preferable to mahogany in its working ftate for the front and ends of the box, I have not attempted to afcertain.

As this machine is calculated to keep out the humidity of the air, when the hole for the tube is corked, and all the glafs, or infulating parts, well varnifhed with proper electrical varnifh; the exciting power of the cylinder, when packed in good order, will remain but little impaired for feveral hours, and fometimes days if kept in a dry room, without frefh amalgam. And what may appear rather remarkable, as the ftool is near the cylinder when packed, if the chains and tube be firft properly fixed, and the drawer removed, a fhock may be adminiftered with the ftool fcrewed on, as well as when off, and to all appearance in as powerful a manner: hence the ftool may be ufed as a prefervative againft humidity in the open air, in cafes of neceffity.

After what has been faid of the conftruction and ufes of this little machine, it may be afked: "Has its efficacy ever been brought to the teft?" I anfwer, Yes. A gentleman, who is a near relative of the writer of this article, was feized with a paralytic affection, during breakfaft, on September 15, 1797, which totally deprived him of the ufe of any part of his left fide, but which was not attended with pain. The beft medical affiftance was recurred to for nearly a fortnight, without producing any apparent alteration. The appetite and tendency to fleep remained nearly as ufual; but the diftortion of one fide of the mouth rendered the fpeech fo inarticulate as to be nearly unintelligible. On the 27th of the fame month I was permitted to adminifter partial fhocks through different parts of the affected fide, which I began at the ftriking diftance of one-eighth of an inch with the machine already defcribed, while the patient was extended on a fofa; the firft two or three fhocks fent through the hand, were fcarcely if at all felt; by and by a fenfation was perceived; a few larger fhocks at one-fixth were then adminiftered, which convulfed the hand a little, and were fenfibly felt; a great many more, at the fame ftriking diftance, were then fent through moft of the joints of the difeafed fide; laftly, a few more, at the diftance of one-fourth one-fourth of an inch, were directed from the ſhoulder to the hand, and from the knee to the foot. After being thus electrified, the patient felt ſome pain in the knee, where the largeſt ſhocks entered, and upon trial could raiſe the leg a little from the ground; but the hand and arm were motionleſs. On the morning of the 28th, the patient, after a good ſleep, could raiſe the leg as before; and, to my great ſatisfaction, ſuch an alteration had taken place, that by a motion proceeding apparently from the ſhoulder, the arm was carried back and forward, as if ſuſpended by a pin; but the hand appeared no more than a dead weight. Partial ſhocks of one-fourth of an inch were adminiſtered as on the preceding evening, after which a pain ſimilar to what had been before felt in the knee was now felt in the ſhoulder; the knee became ſomewhat ſtronger, and the arm more active in its motion, but the hand remained apparently dead. In the evening of the 28th, the patient was in bed, and electrified as before, after which the thumb and firſt finger could be moved in a ſmall degree. This rapid advance on the firſt application of electricity, could not, I think, be attributed to the effects of medical aſſiſtance, which had been omitted to be uſed a few days before, except that a ſeton remained (which ſtill remains); nor yet to what is called the "vis medicatrix naturæ," for this uſually operates ſlowly. Shocks have continued to be uſed ſometimes from one extremity to the other, in general twice a day, ever ſince, to the preſent time, January 13, 1798, whilſt the ſtriking diſtance has been gradually enlarged to nearly 4-10ths of an inch, and the increaſe of ſtrength and activity has been hitherto regular. The patient at this time can walk into the ſtreet, or up and down ſtairs, without the aſſiſtance of a ſtick, and can handle any thing of ſmall weight in his weaker hand: in ſhort, he is happily become capable of ſuperintending the affairs of his family, and, I truſt, has reaſon to hope for ſtill greater ſtrength by perſevering in the means hitherto uſed. Other inſtances could be adduced in which this machine has been uſed with as good effect as a larger one in diſeaſes of the human body; but a particular detail would enlarge the bounds of this article, which perhaps will be deemed already ſufficiently long.

Lincoln, January 13, 1798.

V.

A Memoir concerning a remarkable Phenomenon in Meteorology. Read to the Society of Naturaliſts of Geneva. By M. DE SAUSSURE, *October* 1797 *.

AS ſoon as I had rendered my hygrometer ſufficiently perfect to compare one inſtrument with another, and at the ſame time ſo delicate as to immediately ſhew the changes in the atmoſphere, I was in hope that I might avail myſelf of it to foretell the changes of the weather. I expected that the hygrometer would move towards *dry* on the approaching fine weather, or towards *moiſt* when wet weather was at hand; and it is true that, in general, the north-eaſt wind, which uſually with us accompanies fine weather, does cauſe it to move towards *dry*; and, on the other hand, that it indicates humidity during the ſeaſons of rain. But I have ſince obſerved a very remarkable exception; namely, that the times of greateſt dryneſs are generally the precurſors of rain.

* This paper was communicated to the Editor of La Decade Philoſophique, &c. by Felix Desportes, Reſident of the French Republic at Geneva. I have tranſlated it from this laſt work, No. 4. N.

Ever

Ever since I made this obfervation, my thoughts have been frequently directed to account for it; and it was not till lately at Plombieres that I found a fatisfactory caufe, which I fhall make the fubject of the prefent Memoir.

In order to give every poffible degree of certainty to my obfervations, I kept my inftruments defended not only from the direct rays of the fun, but from every reflection; and I obferved their ftation daily at the fame hours, but more particularly at four in the afternoon, when the greateft drynefs ufually prevails.

During the two months that I remained at Plombieres, the greateft drynefs I obferved was on the 2d of Auguft. The hygrometer indicated 68,5°, the thermometer being at 22,5°. Three or four days before, the hygrometer at the fame hour had ftood higher or nearer to humid, that is to fay, 86°, or 87°, though the thermometer was near a degree higher, namely 23,1°, which muft have proportionally raifed the hygrometer. It rained in the evening of the fame day on which it had been drieft, namely, on the 2d of Auguft. At the fame time that the hygrometer defcended, the barometer alfo fell near two lines. Now I have proved, in my Effays on Hygrometry, and by feveral experiments, that in an air which is rarefied the hygrometer defcends, and denotes a greater degree of drynefs. I therefore attributed this extraordinary drynefs to the rarefaction of the air.

On my return from Plombieres, I purfued my obfervations with the fame care. On the 29th of Auguft of this prefent year, at 20 minutes after 4 in the evening, I obferved the hygrometer at 74°, while the thermometer ftood at 22,5°. On the following day, at 50 minutes after one, I found the thermometer exactly at the fame degree as the evening before, namely, 22,5°, the hygrometer being at 59,5°, that is to fay, 14,5° higher than the evening before. I noted this moment, as affording a valuable obfervation, on account of the identity of height in the thermometer. It was therefore evident that the drynefs of the air had been increafed, not by its heat, but by fome other agent, fuch as its rarity. In fact, the barometer had defcended more than half a line. A wind from the fouth-weft prevailed at the fame time, and it rained early the next morning.

But the moft ftriking inftance is to be found in my Effay on Hygrometry. I made this obfervation at Chamouni on the 23d of July 1781. The hygrometer ftood at 41,2°, the thermometer indicating at the fame time 20,2°. But this degree of heat was far from fufficient to produce fuch a degree of drynefs; for, by calculating from the table at page 87 of my Effay on Hygrometry, we find that the difference of $4\frac{1}{2}$ degrees of heat between that day and the preceding could not deprefs the hygrometer more than 9 degrees, inftead of 20, which it really defcended. The excefs, namely 11 degrees, muft therefore be attributed to another caufe, which I conclude to be the rarefaction of the air. The air may be rarefied either by the fall of the barometer, or the direction of the wind. In fact, the fouth and fouth-weft winds coming from lower countries than ours, and from the fea, muft neceffarily rife, and thence become rarefied, and confequently, as I have faid, caufe the hygrometer to advance towards *dry*. It is probable likewife that the fingular elevation of the valley of Chamouni above the level of the fea, was one of the principal caufes of the drynefs which predominated in that valley on the 26th of July 1781.

We may therefore conceive the reafon of this phenomenon, which at firft fight appears fo ftrange; namely, that extraordinary drynefs fhould precede rain, and that the hygrometer fhould become, in this manner, an affiftant to the barometer, and afford one of the moft certain

tain indications of change of weather. It is to be obſerved, that it rained at Chamouni the next morning.

Nature has exhibited ſigns of dryneſs which do not deceive the inhabitants of the country, and ſerve to predict ſtorms long before hand; ſuch as the flaccidity or drooping of plants with large and thin leaves; ſuch as the gourd and the beet in our gardens, the petaſite in the fields, and the cacalia in the mountains. It is obſerved, that the leaves of theſe plants droop and incline towards the ground on the approach of ſtormy weather; and that on the contrary they ſpring up, and aſſume an appearance of vigour, when the dew or rain has reſtored the elaſticity and natural freſhneſs to their fibres. I muſt add, that the heavy rain in the month of September laſt was preceded by a wind of extraordinary dryneſs: and as we are deſirous of knowing the purpoſe, or at leaſt the uſe, of each of the laws of nature, I would remark, that theſe great droughts which precede ſtorms ſeem intended to put vegetables into a proper ſtate to obtain the greateſt advantage from the rains, on which their growth is to depend. A dry air relaxes and empties their veſſels, and gives them the power of abſorbing the rain-water which ſucceeds. This water finds the air through which it falls loaded with carbonic acid and other exhalations which give fertility to plants.

It is in fact obſerved, that the rain of ſtorms which ſucceed uncommonly dry weather gives to vegetables a peculiar growth and ſtrength, much greater than are found to ſucceed other rains or long continued wet weather.

Hence we perceive, that the more attention we pay to natural phenomena, the more reaſon we find to admire the order and uniformity which prevail in the laws to which they are ſubject.

VI.

On the various Denominations given to the Alkali of Tartar. By a Correſpondent.

To Mr. NICHOLSON, Editor of "The Journal of Natural Philoſophy, &c."

SIR,

AMONGST the numerous innovations which have taken place within theſe few years, nothing perhaps has undergone ſuch an infinite variety of modifications as the chemical nomenclature: ſo far indeed has the rage for novelty been carried in this reſpect, that every perſon who, without a demonſtrable neceſſity for the change, propoſes at preſent either a new ſyſtem, or an alteration in any one already received, muſt juſtly incur the cenſure of increaſing the confuſion which ſuch an endleſs mutation of names has already produced. I ſhall however beg leave to mention a circumſtance which has frequently appeared to me to be a remarkable one. We have no tolerable appellation univerſally received in our own language for a ſubſtance which is indiſputably in more ordinary uſe in the laboratory, and more frequently ſpoken of in chemical writings, than any other—the pure alkali of tartar. Let us only ſee what are the appellations which have been given to it.

Kali, the name applied to this ſalt by the London College of Phyſicians, is not only improper, as being already that of a genus of plants, but has in fact been long uſed as the diſtinctive appellation of the other fixed alkali, in consequence of its extraction from those vegetables.

Fixed vegetable alkaline salt, the designation of this substance in the Edinburgh Pharmacopœia, to say nothing of its being rather a description than a name, is no less objectionable on account of its impropriety. This alkali does not seem to be any more a product of vegetation than soda.

Potasse, the barbarous corruption of our word *pot-ash* by the French chemists, has with them at least one advantage; it does not signify any thing else: but when re-translated by us into *pot-ash*, it certainly becomes in the highest degree objectionable, as tending to confound a simple substance in a state of chemical purity, with a heterogeneous compound of very different qualities.

Spodium, the appellation proposed by Dr. Hopson, is also inappropriate. This salt is certainly not formed by the process of incineration any more than the other fixed alkali is: indeed barilla, from which the greater part of the soda used in this country is obtained, is as immediately in the state of a cinder as any substance in commerce.

Tartarin, or, better perhaps, *tartarine*, is a word, which, if not every thing we could wish, has several great advantages. 1st. It signifies nothing else. 2dly. It is perfectly appropriate. Tartar is a substance which has been long and universally known, and which contains this alkali only in combination with an acid which is destructible by fire; so that the purest specimen of its carbonate, which is commonly met with in the shops, is obtained from it by mere combustion; on which account it has been long called by several compound names analogous to the one here spoken of, as *salt of tartar*, *alkali of tartar*, &c. 3dly. The name itself recalls to the mind one of the readiest and most ordinary means of distinguishing the solution of this salt from that of soda by the test of the acid of tartar. And 4thly. It has already been used throughout the whole of a work of some importance, which is in the hands of every chemist;—a circumstance perhaps essential to the establishment of any new chemical appellation. As the Latin, on account of its saving the necessity of auxiliary particles, and for some other reasons, seems better adapted to some of the purposes of a chemical nomenclature than the modern languages, the feminine *tartarina* may serve well enough in this case. The word proposed in the French nomenclature will not by this means become absolutely useless:—the classical chemist may still label his common pot-ash "*Potassa Russica*," and its titles, if not of equal antiquity, will not perhaps be inferior in elegance to those of its next-door-neighbour, the "*Cineres Perlati*."

But as it is rather my intention to point out the difficulty than to propose the remedy, I shall add no more on the subject. The wish of every one must be, that by the universal reception of some unobjectionable appellation, we may be enabled to speak of this substance without impropriety, or the appearance of affectation.

I am, Sir, your obedient servant,

PHILONOMUS.

January 18, 1798.

VII. *An*

VII.

An Account of ſome Experiments to determine the Force of fired Gunpowder. By BENJAMIN *Count of* RUMFORD, *F.R.S. M.R.I.A.*

[Concluded from page 468.]

AFTER tabulating the experiments, of which an abſtract was given in our laſt number, the Count proceeds to aſcertain the law, according to which the elaſticities increaſe in proportion to the quantities of powder uſed for the charge. For the ſake of brevity, as well as becauſe the ſubject appears to require ſtill further inveſtigation than he has beſtowed upon it, I ſhall refer the reader to the paper itſelf, inſtead of attempting to give an abridged account of this part, and ſhall proceed to relate the other experimental reſults contained in his paper.

After having ſhewn the extreme force of fired gunpowder, he adverts to an objection which may be made againſt his deductions. How does it happen that fire-arms and artillery of all kinds, which certainly are not calculated to withſtand ſo enormous a force, are not always burſt when they are uſed? Inſtead of anſwering this queſtion, by aſking how it happened that the extremely ſtrong barrel uſed in his experiment could be burſt by the force of gunpowder, if this force be not in fact much greater than it has ever been ſuppoſed to be, he proceeds to ſhew that the combuſtion of gunpowder, inſtead of being inſtantaneous, as Mr. Robins's theory ſuppoſes, is much leſs rapid than has hitherto been apprehended; an obſervation, which, if eſtabliſhed, is certainly ſufficient to anſwer the objection.

He remarks, that it is a well known fact, that on the diſcharge of fire-arms of all kinds, cannon and mortars as well as muſkets, there is always a conſiderable quantity of unconſumed grains of gunpowder blown out of them; and what is very remarkable, as it leads directly to a diſcovery of the cauſe of this effect, theſe unconſumed grains are not merely blown out of the muzzles of fire-arms, but come out alſo by their vents or touch-holes, where the fire enters to inflame the charge, as many perſons who have had the misfortune to ſtand with their faces near the touch-hole of a muſket, when it has been diſcharged, have found to their coſt.

It appears extremely improbable to our author, if not abſolutely impoſſible, that a grain of gunpowder actually in the chamber of the piece, and completely ſurrounded by flame, ſhould, by the action of that very flame, be blown out of it without being at the ſame time ſet on fire. And, if this be true, he conſiders it as a moſt deciſive proof not only that the combuſtion of gunpowder is leſs rapid than it has generally been thought to be, but that a grain of gunpowder actually on fire, and burning with the utmoſt violence over the whole of its ſurface, may be projected with ſuch a velocity into a cold atmoſphere, as to extinguiſh the fire, and ſuffer the remains of the grain to fall to the ground unchanged, and as inflammable as before.

This extraordinary fact was aſcertained beyond all poſſibility of doubt by the Count's experiments. Having procured from a powder-mill in the neighbourhood of the city of Munich a quantity of gunpowder, all of the ſame maſs, but formed into grains of very different ſizes, ſome as ſmall as the grains of the fineſt Battle powder, he placed a number of vertical ſcreens of very thin paper, one behind another, at the diſtance of 12 inches from each other; and loading a common muſket repeatedly with this powder, ſometimes without and ſometimes with a wad, he fired it againſt the foremoſt ſcreen, and obſerved the quantity and effects of the unconſumed grains of powder which impinged againſt it.

The ſcreens were ſo contrived, by means of double frames united by hinges, that the paper could be changed with very little trouble, and it was actually changed after every experiment.

The diſtance from the muzzle of the gun to the firſt ſcreen was not always the ſame; in ſome of the experiments it was only 8 feet, in others it was 10, and in ſome 12 feet.

The charge of powder was varied in a great number of different ways; but the moſt intereſting experiments were made with one ſingle large grain of powder, propelled by ſmaller and larger charges of very fine grained powder.

Theſe large grains never failed to reach the ſcreen; and though they ſometimes appeared to have been broken into ſeveral pieces by the force of the exploſion, yet they frequently reached the ſcreen entire; and ſometimes paſſed through all the ſcreens (five in number) without being broken.

When they were propelled by large charges, and conſequently with great velocity, they were ſeldom on fire when they arrived at the firſt ſcreen, which was evident not only from their not ſetting fire to the paper (which they ſometimes did), but alſo from their being found ſticking in a ſoft board, againſt which they ſtruck, after having paſſed through all the five ſcreens; or leaving viſible marks of their having been impinged againſt it, and being broken to pieces and diſperſed by the blow. Theſe pieces were often found lying on the ground; and from their forms and dimenſions, as well as from other appearances, it was often quite evident that the little globe of powder had been on fire, and that its diameter had been diminiſhed by the combuſtion before the fire was put out, on the globe being projected into the cold atmoſphere. The holes made in the ſcreen by the little globe in its paſſage through them, ſeemed alſo to indicate that its diameter had been diminiſhed.

That theſe globes or large grains of powder were always ſet on fire by the combuſtion of the charge, can hardly be doubted. This certainly happened in many of the experiments; for they arrived at the ſcreens on fire, and ſet fire to the paper: and in the experiments in which they were projected with ſmall velocities, they were often ſeen to paſs through the air on fire; and when this was the caſe, no veſtige was to be found.

They ſometimes paſſed on fire through ſeveral of the foremoſt ſcreens without ſetting them on fire, and ſet fire to one or more of the hindmoſt, and then went on and impinged againſt the board, which was placed at the diſtance of 12 inches behind the laſt ſcreen.

The Count then proceeds to mention another experiment, in which the progreſſive combuſtion of gunpowder was ſhewn in a manner ſtill more ſtriking and not leſs concluſive.

A ſmall piece of red-hot iron being dropped down into the chamber of a common horſe-piſtol, and the piſtol being elevated to an angle of about 45 degrees, upon dropping down into its barrel, one of the ſmall globes of powder (of the ſize of a pea), it took fire, and was projected into the atmoſphere by the elaſtic fluid generated in its own combuſtion, leaving a very beautiful train of light behind it, and diſappearing all at once like a falling ſtar.

This amuſing experiment was repeated very often, and with globes of different ſizes. When

When very ſmall ones were uſed ſingly, they were commonly conſumed entirely before they came out of the barrel of the piſtol; but when ſeveral of them were uſed together, ſome, if not all of them, were commonly projected into the atmoſphere on fire.

As the ſlowneſs of the combuſtion of gunpowder is undoubtedly the cauſe which has prevented its enormous and almoſt incredible force from being diſcovered, our author deduces, as an evident conſequence, that the readieſt way to increaſe its effects, is to contrive matters ſo as to accelerate its inflammation and combuſtion. This may be done in various ways; but, in his opinion, the moſt ſimple and moſt effectual manner of doing it would be to ſet fire to the charge of powder, by ſhooting (through a ſmall opening) the flame of a ſmaller charge into the midſt of it.

He contrived an inſtrument on this principle for firing cannon three or four years ago, and it was found, on repeated trials, to be uſeful, convenient in practice, and not liable to accidents. It likewiſe ſuperſedes the neceſſity of uſing priming, of vent-tubes, port-fires, and matches; and on that account he imagined it might be of uſe in the Britiſh navy. Whether it has been found to be ſo or not he has not yet heard.

Another infallible method of increaſing very conſiderably the effect of gunpowder in fire-arms of all ſorts and dimenſions, would be to cauſe the bullet to fit the bore exactly, or without windage, in that part of the bore at leaſt where the bullet reſts on the charge; for, when the bullet does not completely cloſe the opening of the chamber, not only much of the elaſtic fluid, generated in the firſt moment of the combuſtion of the charge, eſcapes by the ſide of the bullet; but, what is of ſtill greater importance, a conſiderable part of the unconſumed powder is blown out of the chamber along with it in a ſtate of actual combuſtion, and, getting before the bullet, continues to burn on as it paſſes through the whole length of the bore; by which the motion of the bullet is much impeded.

The loſs of force which ariſes from this cauſe, is in ſome caſes almoſt incredible; and it is by no means difficult to contrive matters ſo as to render it very apparent, and alſo to prevent it.

If a common horſe-piſtol be fired with a looſe ball, and ſo ſmall a charge of powder that the ball ſhall not be able to penetrate a deal board ſo deep as to ſtick in it when fired againſt it from the diſtance of ſix feet; the ſame ball, diſcharged from the ſame piſtol with the ſame charge of powder, may be made to paſs quite through one deal board, and bury itſelf in a ſecond placed behind it, merely by preventing the loſs of force which ariſes from what is called windage, as he found more than once by actual experiment.

He has in his poſſeſſion a muſket, from which, with a common muſket charge of powder, he fires two bullets at once with the ſame velocity that a ſingle bullet is diſcharged from a muſket on the common conſtruction with the ſame quantity of powder. And, what renders the experiment ſtill more ſtriking, the diameter of the bore of his muſket is exactly the ſame as that of a common muſket, except only in that part of it where it joins the chamber, in which part it is juſt ſo much contracted, that the bullet, which is next to the powder, may ſtick faſt in it. He adds, that though the bullets are of the common ſize, and are conſequently conſiderably leſs in diameter than the bore, means are uſed which effectually prevent the loſs of force by windage; and to this laſt circumſtance, he concludes, it is doubtleſs owing, in a great meaſure, that the charge appears to exert ſo great a force in propelling the bullets.

I That

That the conical form of the lower part of the bore where it unites with the chamber has a considerable share in producing this extraordinary effect, is, however, very certain, as he has found by experiments made with a view merely to ascertain that fact.

The remaining pages of the Count's paper are occupied by a computation, tending to shew, that the force of the elastic fluid, generated in the combustion of gunpowder, may be satisfactorily accounted for upon the supposition that its force depends solely on the elasticity of watery vapour or steam. For this purpose he recurs to the experiments of Mr. Betancour, published in Paris, under the auspices of the Royal Academy of Sciences, in the year 1790, which shew that the elasticity of steam is doubled by every addition of temperature equal to 30° of Fahrenheit. From the Count's reference, it appears that the experiments were carried as far as 280 degrees of that scale, in which case the pressure was found to be equal to about four atmospheres. He affirms, that there does not appear to be any reason why the same law should not hold in higher temperatures, and has therefore extended his computations through thirteen more terms of the geometrical series, the last of which affords an elastic force equal to more than sixty-five thousand atmospheres. As an excuse for not giving this computation in detail, I must simply remark, that something more than a negative reason seems necessary to justify the extension of this law of increase from so limited a scale of experiments. For which reason I shall add no more of this speculative part, than merely that the water of crystallization in the nitre, and the moisture which the charcoal may be conceived to retain, appear to be fully sufficient to account for the explosive force by means of steam only, if the deductions from Mr. Betancour's experiments be admitted.

VIII.

Observations on Strontian. By *Citizen* PELLETIER. *Read to the National Institute 30th April,* 1796.*

STRONTIAN is at present considered by many foreign chemists as a peculiar earth. Its discovery seems to me to be due to Dr. Hope, professor of chemistry at Glasgow; he having first described its characters and chemical properties in a dissertation which he published 4th November, 1793, and which has since been printed in the Transactions of the Royal Society of Edinburgh. This memoir is nevertheless posterior to Dr. Crawford's dissertation on the internal use of muriate of barytes, in which he announces that he thinks it probable that the strontian mineral contains an earth different from barytes †.

M. Klaproth

* Translated from the Annales de Chimie, xxi. p. 113, by J. E—r.

† The salt obtained from the combination of strontian earth with muriatic acid is much more soluble in hot water than in cold, and consequently easily crystallizes by cooling: muriate of barytes, on the contrary, is nearly as soluble in cold water as in hot, and crystallizes by evaporation.

An ounce of distilled water at the temperature of 70 degrees, dissolves 9 drachms and 50 grains of muriate of strontian: the same quantity of water, at the same temperature, only dissolves 3 drachms and 35 grains of muriate of barytes.

The former produces at least 15 degrees of cold by its solution; the latter not more than 5 degrees at the most.

Muriate

M. Klaproth has alſo examined ſtrontian, but, as it appears, ſubſequently to Dr. Hope; or at leaſt, as he does not ſpeak in his work of Dr. Hope's experiments, there is reaſon to believe that they were not known to him.

Strontian is alſo mentioned in M. Schmeiſſer's mineralogical work, as different from the other known earths.

It is in the ſtate of carbonate that it is found in Argyleſhire in the weſtern part of the north of Scotland, accompanying a vein of lead-ore.

Klaproth, Blumenbach, and Sulzer of Ronnebourg, called it *ſtrontianite*; Hope called it *ſtrontite*. I conceive that the name of *ſtrontian*, taken from the place where it is found, may be properly aſſigned to it, as this word in itſelf has no ſignification, and cannot conſequently render it liable to be confounded with other ſubſtances.

The carbonate of ſtrontian has been for a long time conſidered as a variety of native carbonate of barytes; I myſelf looked upon it as ſuch in 1791, from ſome aſſays to which I had ſubjected a ſmall ſpecimen, which Mr. Greville of London had the goodneſs to procure me. Having aſſayed it comparatively with the carbonate of barytes from Anglezark, which was then called witherite, I thought I did not at that time perceive any remarkable difference between the two ſubſtances. Both of them when fuſed by the blow-pipe afforded white opaque vitreous globules, which, when expoſed for ſome time to the air, became reduced to powder. Expoſed to diſtillation in a moderate heat, carbonate of ſtrontian does not, any more than carbonate of barytes, emit any carbonic acid gas, though they are both diſſolved by the nitric and muriatic acids with efferveſcence and a diſengagement of this gas. The ſalts which reſulted from theſe combinations were by no means deliqueſcent, and I therefore took thoſe of the ſtrontian for nitrate and muriate of barytes, and ſtill rather becauſe their ſolutions were decompoſed by the alkaline, calcareous, and other ſulphates, as is the caſe with the barytic ſalts.

It is ſome months ſince we have been acquainted in France with the labours of M. Klaproth on the carbonate of ſtrontian; but thoſe of Dr. Hope, although prior, were unknown to us, and it is only within a few days that I have been apprized of them by M. Schmeiſſer.

In a letter which M. Hermbſtœdt wrote me ſix months ago, he announced that M. Klaproth had eſtabliſhed the properties of ſtrontian as a new earth, the diſcovery of which had been made ſeveral years ſince by M. Sulzer, and had been publiſhed by M. Blumenbach in his treatiſe on natural hiſtory. The diſtinctive characters which M. Klaproth had found in the carbonate of ſtrontian were:

Firſt, That it was ſpecifically lighter than native carbonate of barytes (witherite);

Secondly,

Muriate of barytes affords by evaporation flattened octagonal cryſtals, two of whoſe oppoſite ſides are always much longer than the others: muriate of ſtrontian, by rapidly cooling, cryſtallizes in elongated filaments, and, when ſlowly cooled, in hexagonal columns, of which three ſides are alternately wider, and the others narrower, and terminated in obtuſe triangular pyramids. Thus, though the carbonate of ſtrontian much reſembles that of barytes, theſe two ſubſtances have very different qualities. It is probable that the ſtrontian mineral has for its baſis a new ſpecies of earth which has not hitherto been examined, and which it is of importance not to ſubſtitute for barytes for medical uſes. *Extract from Dr. Crawford's Diſſertation.* P.

The above is a tranſlation of Citizen Pelletier's note. Not having Dr. Crawford's tract on the *terra ponderoſa ſalita*, or muriate of barytes, at preſent by me, I have not been able to examine the accuracy of the quotation, but have no reaſon to doubt it, except that I think he uſes a different nomenclature. T.

Secondly, That it produced with the nitric, muriatic, and other acids, salts which were more soluble than those of barytes;

Thirdly, that the salt which it formed with the muriatic acid, being dissolved in alcohol, gave it the property of burning with a red flame;

Fourthly and lastly, That it might be deprived of its carbonic acid by calcination; and that it became by this means soluble in water, and the most so in boiling water; so that by cooling a portion was separated in a crystalline form.

The specimen of carbonate of strontian, which I had in my collection, was not considerable enough to admit of a great number of experiments; it was however sufficiently so to enable me to repeat a part of those which are related by Messrs. Hope and Klaproth; and what mostly determined me to appropriate it to this purpose was, that Cit. Coquebert had published in No. 5 of the Journal des Mines, that from some assays which we had made of the carbonate of strontian, I had doubted whether it contained a peculiar earth. The following are the reasons on which my doubts were founded:

1. It is several years since I had succeeded in disengaging carbonic acid gas from carbonate of barytes by calcination; and, having then dissolved the calcined barytes in hot water, I had obtained a crystallization: I could not therefore consider this character as exclusively belonging to strontian.

2. I knew also that calcareous muriate dissolved in alcohol gave it the property of burning with a red flame. This consideration induced me therefore to suspect a mixture of calcareous carbonate in that of strontian, and the specimen which I had was in fact combined with this substance. I shall now describe the experiments which I have made to endeavour to detect this ingredient; and, as they were made comparatively with a similar set on the native carbonate of barytes (witherite), I have thought it proper to present them together, that it may be seen in what respects the carbonate of strontian resembles, and in what it differs from it.

Comparison of Strontian and Barytes.

1. CARBONATE of barytes is found with sulphate of barytes in a lead-mine at Anglezark, (near Chorley) in Lancashire*. Carbonate of strontian is found at Strontian in Argyleshire, also accompanying a lead-ore with sulphate of barytes.

2. The carbonate of barytes from Anglezark, taken internally, is poisonous, so that in that country it is known by the name of rat-stone [pierre contre les rats]. A little dog to which I had given fifteen grains was seized with vomiting, and died eight hours afterwards; and having given to another of apparently equal strength fifteen grains of carbonate of barytes obtained from the decomposition of the sulphate, it was also seized with vomiting, and died fifteen hours afterwards: the latter was opened by Cit. Chaussier. Another dog to which I had given a like quantity of carbonate of barytes, prepared from sulphate of barytes from the ci-devant province of Auvergne, had vomitings, but did not die, though he took it two days successively: he vomited each time. I purpose to repeat these experiments with carbonates of barytes procured from different barytic sulphates, and especially those which are not accompanied by any metallic ores. The carbonate of strontian, on the contrary, may be taken internally without danger. I gave 20 grains of it to a little dog, but he was

* See the Manchester Memoirs, iii. 598. T.

not

not feized with any vomiting, and twenty hours afterwards I did not at all perceive that he had felt the fmalleft inconvenience. It will be proper to repeat this experiment with ftronger dofes. Blumenbach was alfo convinced that the carbonate of ftrontian, taken internally, did not at all derange the animal œconomy. Thefe obfervations, therefore, point out a difference between ftrontian and barytes.

3. The colour of the carbonate of barytes from Anglezark is a grey white: it is fometimes found cryftallized, but more commonly in a ftriated mafs. Its fpecific gravity is from 4.2919 to 4.3710. The colour of carbonate of ftrontian is a light green, though it is fometimes found colourlefs and tranfparent: it is ftriated, and fometimes of a regular cryftalline form. Its fpecific gravity is from 3.6583 to 3.6750 *. This carbonate is confequently lighter than that of barytes.

4. Native carbonate of barytes, expofed to a fire which is not too violent, fcarcely lofes any thing of its weight: in a ftronger one it attacks the crucible, and paffes into fufion. Carbonate of ftrontian alfo retains the carbonic acid pretty ftrongly; but with caution, and a proper continuance of heat, the carbonic acid may be feparated in the proportion of five or fix hundredths of the falt, without its attacking the crucible: fome care muft, however, be taken not to have the fire too ftrong, as the earth would, in that cafe, attack the crucible, and run into a chryfolite-coloured glafs. The carbonic acid therefore is lefs ftrongly retained in carbonate of ftrontian than in that of barytes.

5. Meffrs. Hope and Klaproth had obferved, that calcined ftrontian was foluble in water, and that, when boiling, it diffolved a fufficient quantity to afford cryftals by cooling; fo that thefe two chemifts regarded this property in ftrontian as a diftinctive character. M. Klaproth efpecially never fucceeded in calcining the native carbonic of barytes fufficiently to try its folubility in that ftate. When he gave it but a fmall degree of heat, it was not deprived of its carbonic acid; and when he applied a greater, it became vitrified. Dr. Hope announced, in a fupplement to his memoir, that he had found means to calcine the native carbonate of barytes in a black-lead crucible, and that he found this earth, thus calcined, was, as well as ftrontian, foluble in boiling water, and fufceptible of cryftallization; which properties he has accordingly ceafed to confider, fince that time, as characters peculiar to ftrontian. I have alfo fucceeded in feparating with facility the carbonic acid from both the native and artificial carbonates of barytes, as well as that of ftrontian, without ufing black-lead crucibles; and I fhall here defcribe the method which has conftantly proved fuccefsful with me, and the comparative experiments which I have made on this fubject.

Procefs for feparating the Carbonic Acid from the Carbonates of Barytes and Strontian.

1. TO 100 grains of native carbonate of barytes in powder, I added 10 grains of powdered charcoal; and, the whole being well mixed, I made it into a ftiff pafte with boiled ftarch, and rolled it into a ball. Having put into a crucible a little frefh-burnt charcoal

* In the fpecific gravities in the original, that of diftilled water is fuppofed to be 10, the decimal point being put one place more forward than is here done. I have ventured, in conformity to the more ufual, and, as I think, more convenient, method of putting unity for this common meafure, to point them as above. T.

in powder, and laid the ball on it, I covered it with powdered charcoal, and luted on a cover with common loam. Things being thus diſpoſed, the crucible was kept in a very ſtrong fire for a full hour, which time was ſufficient to diſengage the carbonic acid. The crucible being cold, I opened it, and found the little ball perfectly compact; but it now weighed only 70 grains. I then triturated it in a glaſs-mortar with about nine ounces of boiling water, filtrated the liquor, and, in order to diſpoſe it the more to cryſtallization, put it into a glaſs retort, to ſeparate a portion of the water by diſtillation. By cooling, there were formed in the retort cryſtals ſeveral lines in length.

2. One hundred grains of carbonate of barytes prepared from the ſulphate by the ordinary proceſſes were treated with 10 grains of powdered charcoal, as above deſcribed, and the crucible equally expoſed to heat for only an hour. Having then treated the reſidue with boiling water, and concentrated the liquor in a retort, I obtained, on its cooling, cryſtals ſimilar to thoſe before mentioned.

3. One hundred grains of carbonate of ſtrontian being treated in the ſame manner, the reſidue, after calcination, weighed only 72 grains. Its ſolution in hot water appeared to me more ſaturated than that of barytes; and, without having recourſe to concentration, I obtained cryſtals on its cooling, though I had uſed about the ſame quantity of water as in the preceding experiments. I do not, however, believe that ſtrontian is much more ſoluble than barytes; for in ſeveral other experiments I have had ſolutions of barytes ſo fully ſaturated as to cryſtallize by cooling confuſedly and in a maſs. Thus it is very evident that the action of fire ſeparates carbonic acid from barytes and ſtrontian, and that theſe earths then become ſoluble in water, and in larger quantity if boiling, ſo as to give cryſtals by cooling. This character does not therefore belong excluſively to ſtrontian, as M. Klaproth ſuppoſes.

I have alſo remarked that the aqueous ſolutions, both of barytes and ſtrontian, when pure or calcined, have an odour ſomewhat ſimilar to that of cauſtic pot-aſh or ſoda, or what is commonly called a lixivious odour.

Habitudes of the Carbonates of Barytes and Strontian.

With nitric acid. NATIVE carbonate of barytes was totally diſſolved by diluted nitric acid, and the ſolution attended with a diſengagement of carbonic acid gas, in the proportion of twenty-two parts in the hundred. The concentrated liquor afforded cryſtals, the moſt ordinary figure of which was octahedral.

One hundred grains of carbonate of ſtrontian were alſo diſſolved in nitric acid; but the diſengagement of carbonic acid gas was more conſiderable than in the preceding experiment, the proportion being thirty hundredths. The ſalt which reſults from this combination alſo cryſtallizes in octahedrons.

[*To be continued.*]

PHILO-

PHILOSOPHICAL NEWS AND ACCOUNTS OF BOOKS.

ON the firſt of Brumaire (21 Oct. 1797.) Citizen Garnerin made the experiment of the parachute at the Garden de Mouſſeaux *. This experiment has not been before attempted at Paris. Blanchard had the notion ſoon after the diſcovery of balloons ; and at ſeveral different towns, particularly Liſle, he let fall from the veſſel of his balloon dogs and other animals. Some years ago he ventured to deſcend in perſon in an experiment he made at Bâle; but either from the bad conſtruction of his parachute, or by falling among trees, he had the misfortune to break one of his legs.

Citizen Garnerin was more fortunate, and has given the moſt ſatisfactory proofs of his ſkill, firmneſs, and intrepidity, which the impatient public had ſeemed to doubt at the Garden Biron. Notwithſtanding the haſte with which this philoſopher was obliged to make his preparations, and ſeveral accidents which happened to his apparatus, he aſcended from the Garden de Mouſſeaux at half paſt five in the evening. Between the balloon and the veſſel was placed the parachute, half opened, and forming a kind of tent over the aërial traveller. The wind, which through the courſe of the day had been violent, was now become calm, as if to favour his enterpriſe, and carried him to the northward, over the plain of Mouſſeaux. Full of that intereſt, that inſurmountable emotion which ſeizes us on beholding a man quit the earth and advance towards the clouds by an apparatus ſo majeſtic, the eyes of the ſpectators were fixed on the balloon, which roſe with rapidity. When it was at a conſiderable height, the parachute and veſſel were ſeen at once to ſeparate from the balloon; the latter of which burſt (celui-ci éclate), emptied itſelf, and floated down with the wind. The parachute unfolded itſelf, while the veſſel which ſerved as ballaſt drew it towards the earth. Its fall was at firſt ſlow and vertical; but ſoon afterwards it exhibited a kind of balancing or vibration, and a rotation gradually increaſing, which might be compared with that of a leaf falling from a tree. Cries expreſſive of the general terror and aſtoniſhment were heard on all ſides. The crowd ruſhed towards the place of his deſcent, where at length the aëronaut landed, and without injury. Emotions of joy and congratulation ſucceeded thoſe of alarm, and he was brought back in triumph to the Garden of Mouſſeaux.

Such was the experiment. The narrator then proceeds to give the following detail:—The parachute, which reſembles a vaſt umbrella, is of cloth, and its diameter, when unfolded, is 25 feet. Citizen Garnerin eſtimates the height from which he fell at 300 toiſes, but C. Say reckons it no more than one-third of that ſpace †. He had 75 pounds of ballaſt in his car at the moment of his fall, which he ſays he ſhould have thrown out, if he had not been apprehenſive of wounding the ſpectators below. C. Say thinks he would have done very wrong; becauſe the danger to which he was expoſed did not ariſe from the velocity of his fall, but the vibration of the car, which might have ſtruck him againſt the earth, or againſt walls, trees, or other prominences. Now, if he had thrown out his ballaſt, the remarker thinks he would have rendered the vibration much more rapid, extenſive, and dangerous, and perhaps even have cauſed his parachute to overſet, and the whole to fall in a maſs.

The author explains the theory of the parachute by obſerving that the reſiſtance of the air

* This account was drawn up by J. B. Say, Editor of La Decade philoſophique, literaire, et politique, No. 4.

† In the way of rough illuſtration, this may be ſtated at once and a half the height of St. Paul's Cathedral in London. N.

and gravitation are two forces which act at the fame time on the whole fyftem of the machine; that gravitation acts as if the whole mafs were united at its centre of gravity, while the refiftance acts as if the whole of this mafs was united at another centre little diftant from the centre of gravity of the cloth which forms the parachute, and very different from the former. The parachute may be confidered as fufpended in the air by this centre of refiftance.

If the centre of gravity be not vertically beneath the centre of refiftance when the parachute is properly placed, it is evident (fays he) that it will incline to one fide, defcend obliquely, ofcillate, and the fmalleft irregularity in its figure will caufe it to turn round its vertical axis. It is alfo important that thefe two centres fhould be at a diftance from each other, fince the ofcillations will more readily take place the nearer they are. If they were coincident, there would be no caufe why the whole apparatus fhould not overfet. C. Say therefore propofes that the car or part of the ballaft fhould be fufpended at a confiderable diftance below the parachute.

ACCOUNTS OF BOOKS.

Recherches expérimentales fur le Principe de la Communication latérale du Mouvement dans les Fluides, appliqué à l'Explication de différens Phénomènes hydrauliques. Par le Citoyen J. B. Venturi, Profeffeur de Phyfique expérimentale à Modène, Membre de la Societé Italienne, &c. &c. A Paris, chez Houel & Ducros, Rue du Bacq, No. 940—Théophile Barrois, Rue Haute-feuille, No. 22. Ann. VI. 1797.——Or, Experimental Refearches concerning the Principle of the lateral Communication of Motion in Fluids, applied to the Explanation of various Hydraulic Phenomena. By Citizen J. B. Venturi, Profeffor of Experimental Philofophy at Modena, &c. &c.

I HAVE been favoured with a copy of this curious and interefting work by the learned Profeffor, which I have read with much pleafure. The commiffion nominated for that purpofe by the National Inftitute of France have given a very correct analyfis, which I fhall chiefly follow in my account.

Citizen Venturi has introduced an horizontal current of water into a veffel filled with the fame fluid at reft. This ftream entering the veffel with a certain velocity, paffes through a portion of the fluid, and is then received in an inclined channel, the bottom of which gradually rifes, until it paffes over the border or rim of the veffel itfelf. The effect is found to be, not only that the ftream itfelf paffes out of the veffel through the channel, but carries along with it the fluid contained in the veffel; fo that after a fhort time no more of the fluid remains than was originally below the aperture at which the ftream enters. This fact is adopted as a principle or primitive phenomenon by the author, under the denomination of the lateral communication of motion in fluids, and to this he refers many important hydraulic facts. He does not undertake to give an explanation of this principle, but fhews, p. 37, that the mutual attraction of the particles of water is far from being a fufficient caufe to account for it.

The firft phenomenon which the author propofes to explain by this eftablifhed principle is the emiffion of a fluid through different adjutages applied to the refervoir which contains it. It is known that the vein of fluid which iffues from an orifice or perforation through a thin plate, becomes contracted, fo as to exhibit a fection equal to about 0,64 of the orifice itfelf, fuppofed to be circular; and that the place of the greateft contraction is ufually at the diftance

tance of one femi-diameter of the orifice itfelf. If a fmall adjutage be adapted to the orifice, having its internal cavity of the fame conoidal form as the fluid itfelf affects in that interval, the expenditure is the fame as by the fimple orifice. But if at the extremity of this adjutage a cylindric tube be affixed, of a greater diameter than that of the contracted vein, or a divergent conical tube, the expence of fluid increafes, and may exceed the double of that which paffes through the aperture in the thin plate, though the adjutage poffefs an horizontal or even afcending direction.

By the interpofition of a fmall adjutage, adapted to the form of the contracted vein, Citizen Venturi afcertained, in the firft place, that there is an increafe of velocity in the tubes he employed, though the velocity of emiffion itfelf be lefs than that of the ftream which iffues from an hole in a thin plate. He afterwards proves, by the fact, that the interior velocity and expenditure of fluid, which is increafed through tubes, even in the horizontal or afcending direction, is owing to the preffure of the atmofphere. If the fmalleft hole be made in the fide of the tube near the place of contraction of the vein, the increafed expenditure does not take place; and when a vertical tube is inferted in fuch a hole, the lower end of which tube is immerfed in water or mercury, it is found that afpiration takes place, and the water or mercury rifes; and this afpiration in conical tubes is lefs in proportion, as the place of infertion of the upright tube is more remote from the fection where the greateft contraction would have taken place. And, laftly, the difference between the expenditure of fluid, through an orifice made in a thin plate, and that which is obferved through an additional tube, does not take place in vacuo.

The influence of the weight of the atmofphere on the horizontal or afcending flux being thus eftablifhed, the author confiders it as a fecondary caufe, referable to, and explicable by, his principle of the lateral communication of motion in fluids. In conical divergent tubes, for example, the effect of this lateral communication is, that the central cylindrical jet, having for its bafis the fection of the contracted vein, carries with it the lateral fluid which would have remained ftagnant in the enlarged part of the cone. Hence a vacuum tends to be produced in this enlarged part which furrounds the central cylindric ftream; the preffure of the atmofphere becomes active to fupply the void, and is exerted on the furface of the refervoir, fo as to increafe the velocity of the fluid at the interior extremity of the tube.

The author proves, that the velocity or total expenditure of fluid through an aperture of given dimenfions, may be increafed by a proper adjutage in the proportion of 24 to 10: he applies this refult to the conftruction of the funnels of chimneys. He determines the lofs of emitted fluid, which may be fuftained by finuofity in pipes. He fhews by experiment, that a pipe which is enlarged in any part, affords a much lefs quantity of fluid than if it were throughout of a diameter equal to that of its fmalleft fection. This, as he remarks, is a circumftance to which fufficient attention has not been paid in the conftruction of hydraulic machines. It is not enough to avoid elbows and contractions; for it fometimes happens, that by an intermediate enlargement the whole of the advantage arifing from other judicious difpofitions of the parts of the machine is loft.

There are two caufes of the increafe of expenditure through defcending pipes. The firft is owing to the lateral communication of motion which takes place in defcending pipes, in the fame manner as in thofe which poffefs an horizontal fituation; the fecond arifes from the acceleration by gravity which takes place in the fluid while it falls through the defcending

ing tube. This second kind of augmentation was known to the ancients, though they possessed no good theory nor decisive experiments respecting it. The author endeavours to establish a theory on the principle of virtual ascension combined with the pressure of the atmosphere. His deductions are confirmed by experiment, in which he has succeeded so far as to separate the two causes of augmentation, and assigned to each their respective degree of influence.

Citizen Venturi then proceeds to different objects of enquiry, to which his principle seemed applicable. He gives the theory of the water-blowing * machine, and he determines by calculation the quantity of air which one of these machines can afford in a given time. He observes, that the natural falls of water in the mountains always produce a local wind; and he even thinks, that the falling streams in the internal parts of mountains are in some instances the cause of the winds which issue from caves. He proves, by the facts, that it is possible in certain instances to carry off, without any machinery, the waters from a spot of ground, though it may be situated on a lower level than that of the channel which is to receive the water.

The whirlpools, or circular eddies of water so frequent in rivers, are, according to the theory of our author, the effect of motion communicated from the parts of the current which are most rapid, to those lateral parts which are least so. In the application of this principle, he points out the circumstances adapted to produce such eddies at the surface or at the bottom of rivers. He concludes, that every movement of this kind destroys a part of the force of the current, and that in a channel through which water constantly flows, the height of this fluid will be greater than it would have been if the dimensions of the channel had been uniformly reduced to the measure of its smallest section.

There is another kind of whirling motion somewhat different in its nature from these last. It is produced in the water of a reservoir, when it is suffered to flow through an horizontal orifice. The author deduces the theory of these vortices from the doctrine of central forces. The form of the hollow funnel, which in this case opens through the fluid of the reservoir, is a curve of the 64th species of the lines of the third order, enumerated by Newton. Theory and experiment both unite here in proving, that it is not only possible, but that there really exists in nature a vortex, the concavity of which is convex towards the axis, and of which the revolutions of its different parts follow the ratio of the square of the distance from the centre. Daniel Bernoulli was in the wrong, in his Hydrodynamics, to reproach Newton for having supposed a vortex to be moved according to this law.

In the last place, the author considers that lateral communication of motion which takes place in the air as well as in the water. This is the cause of such local and partial

* *Soufflet d'eau.* It is also called *trompe*, but we have no appropriate name for this engine in English. The reader may consult Lewis's Philosophical Commerce of Arts, and Chaptal's Elements of Chemistry, for descriptions. It consists of an upright pipe, through which a shower of water is made to fall. This shower carries down a mass of air with it, which is received beneath a kind of tub, and conducted to the furnace by means of a pipe. The most remarkable natural phenomenon of this kind is the squall at sea. When a cloud suddenly falls in the form of rain, it drives down a portion of the atmosphere, which glides rapidly along the surface of the sea, and is capable of suddenly oversetting vessels, or carrying away their masts. It may easily be imagined, that seamen are very attentive to look out for this phenomenon, and to guard against it in time by lowering their sails, &c. N.

winds

winds as fometimes blow contrary to the direction of the general wind. It is by virtue of the fame principle, that the refonant vibration, excited laterally in the extremity of an organ-pipe, is communicated to the whole column of air contained in the pipe itfelf.

From the fame principle, the author deduces the augmentation of force which found receives in conical divergent tubes, compared with thofe of a cylindrical form. On this occafion, he points out the remarkable differences which appear to take place between the refonant vibrations of air contained in a tube, and the fonorous pulfations propagated through the open atmofphere.

In an Appendix, Citizen Venturi relates different experiments which he has made to determine the convergence and velocity of the fluid filaments which prefs forward to iffue out of a refervoir by an orifice through a thin plate. He proves, by a very clear experiment, that the contraction of the vein is made at a greater diftance from the orifice under ftrong than under weak preffures. He explains, why in a right-lined orifice, the fides of the contracted vein correfpond with the angles of the orifice and the angles with the fides. He examines the expenditure through a tube, the extremity of which is thruft into the refervoir itfelf, according to the method of Borda in the Memoirs of the Academy of Sciences for the year 1766.

The Commiffaries of the Inftitute appointed to examine this work of Citizen Venturi, without undertaking to decide refpecting all the applications of his principle, give him full credit for the acutenefs and fagacity he has difplayed in this curious experimental courfe. The author himfelf is indeed fufficiently candid to admit that every thing of this nature, which is not confirmed by direct experiment, ought to form a fubject of difcuffion and enquiry. "I have not," fays he, page 9, "infifted upon theoretical confiderations, excepting fo "far as they combine with the facts, and fo far as it was neceffary to unite thofe facts in a "fingle point of view. The reader may, if he pleafes, even omit the fmall portion of "theory, and confider my propofitions fimply as the refult of experiment."

There is no doubt but that the whole work will tend to confirm the reputation of its author as a fkilful experimentalift, and enlightened obferver.

Confiderations on the Doctrine of Phlogifton and the Decompofition of Water. By Jofeph Prieftley, LL. D. F. R. S. &c. Philadelphia printed, 1796.

Obfervations on the Doctrine of Phlogifton and the Compofition of Water. Part II. By Jofeph Prieftley, LL. D. F. R. S. Philadelphia printed, 1797.

Two Lectures on Combuftion. Supplementary to a Courfe of Lectures on Chemiftry, read at Naffau Hall, containing an Examination of Dr. Prieftley's Confiderations on the Doctrine of Phlogifton and the Decompofition of Water. By John Maclean, Profeffor of Mathematics and Natural Philofophy in the College of New Jerfey. Philadelphia printed, 1797.

As I hope to give a fuller account in future of Dr. Prieftley's obfervations in favour of the old chemical fyftem, I fhall for the prefent content myfelf with announcing the titles of the three laft works.

The

The Medical Repofitory, Vol. I. Nos. I. and II. octavo. The two numbers contain 287 pages, clofely printed. New-York, printed for T. and J. Swords.

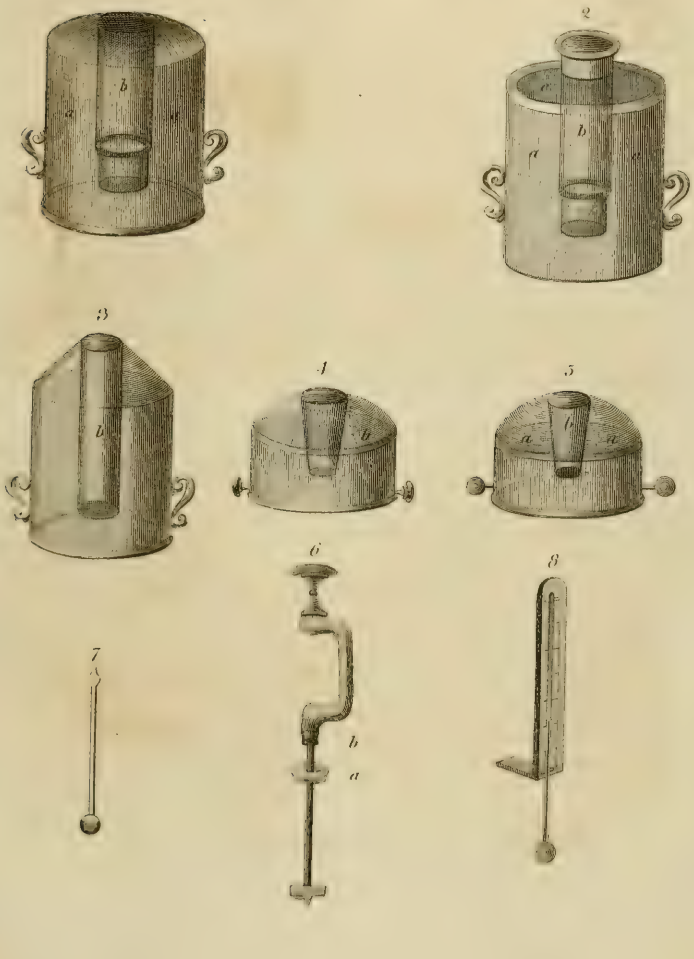
The authors and editors of this work are Doctors Samuel L. Mitchill, Member of the Legiflative Affembly of the State of New-York, F.R.S. Edin. Profeffor of Chemiftry in Columbia College, and Doctors Edward Miller, and E. H. Smith, of New-York. The firft Number appeared in July 1797, and the fecond in the month of November laft. It is intended to be publifhed in quarterly numbers of at leaft 100 octavo pages each, at the price of half a dollar per number. It confifts of medical effays or communications, a review of new publications, not only fuch as are ftrictly medical, but alfo thofe which relate to agriculture and other branches of natural hiftory, philofophy, &c. or may be in any manner related to the objects contemplated in the plan of a medical repofitory; and laftly, medical facts, hints, enquiries, and news.

It is a well known fact, that the public will not affift in any periodical work until they have received proof, for a certain length of time, of the accuracy, fpirit, and ability, with which the conductors are able from their own fources, whether original or derivative, to fupport it. A work of the kind before us is peculiarly calculated for the diffufion of knowledge in a country like America, where, from various circumftances, the written fources of intellectual acquifition are much more limited than in Europe. It is equally calculated to accelerate improvement in the whole of that confiderable part of the globe, wherein the language of England is current. The prefent numbers fhew no want either of ability or induftry on the part of the authors, and will, it is to be hoped, meet the fuccefs they merit.

MY young correfpondent from Liverpool has fhewn confiderable ability in his meditations and conjectures on the many important chemical facts he mentions. When facts can be fo difpofed as by mutual illuftration to prove an entire theory, the individual who makes the arrangement, may, perhaps, do more for the advancement of fcience, than the performer of many folitary experiments: but when this cannot be done, the chief ufe of imperfect conclufions or conjectures muft be to point out new and decifive experiments. W. S. may fupport the hope of making fuch experiments, by reflecting that the greateft difcoveries have been made by men who did not poffefs the means of acquiring much apparatus. Apothecaries' phials, Florence flafks, bafons, cups, faucers, a blow-pipe and charcoal, common tobacco-pipes and garden-pots, as fubftitutes for crucibles and a furnace, and the ordinary bellows, together with a few chemical materials, would conftitute no mean apparatus for philofophical experiment. When he fhall turn his thoughts towards the experimental refearches, to which his obfervations evidently lead, I am confident he will be of opinion, that the publication of his conjectures, in their prefent ftate, would not be defirable.

Mr. Walker's freezing Processes —

Fig. 1.



Barlow sculp.

Mr. Pearson's portable Electrical Machine.

Fig. 1.

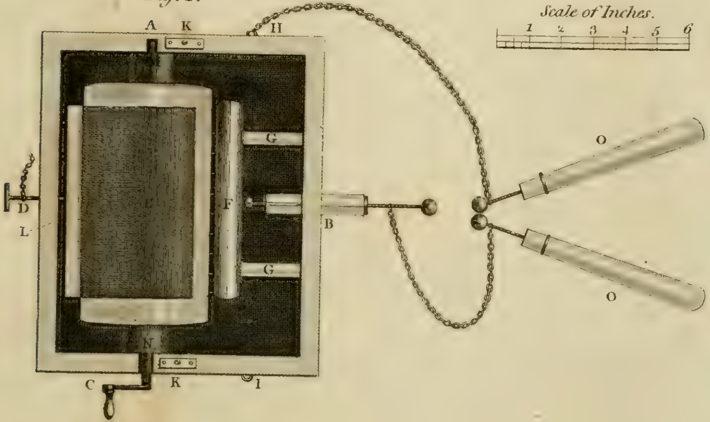


Fig. 2.

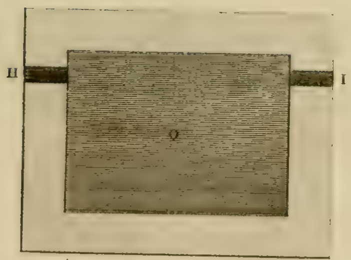


Fig. 3.

Barlow sculp.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH 1798.

ARTICLE I.

Obſervations on Strontian. By Citizen PELLETIER. *Read to the National Inſtitute 30th April,* 1796.

[Concluded from p. 222.]

Habitudes of the Carbonates of Barytes and Strontian.

With muriatic acid. I DISSOLVED 100 grains of native carbonate of barytes in muriatic acid, and obtained 22 grains of carbonic acid gas. The ſolution being evaporated afforded cryſtals in the form of ſhort flattened priſms, or hexagonal plates, the weight of which was 138 grains. I alſo diſſolved in the ſame acid 100 grains of carbonate of barytes, obtained by the decompoſition of barytic ſulphate: the diſengagement of carbonic acid gas alſo amounted to 22 grains, and the muriate cryſtallized like the preceding, ſo that there was not any difference in their appearance *.

It

* The ſucceſs which Dr. Crawford met with in the internal uſe of muriate of barytes, in the treatment of ſcrophulous diſeaſes, begins to be known in France; and ſeveral phyſicians already preſcribe this new medicine. As, however, it is very active, and ſometimes even dangerous, it cannot be too ſtrongly recommended that it be given at firſt only in a very ſmall doſe, and that the effects which it may produce during its adminiſtration be accurately obſerved. The Society of Health is at preſent engaged in a general work, in which it is intended to determine what advantages medicine may derive from the uſe of muriate of barytes. They have alſo appointed commiſſaries to obſerve its effects in the treatment of horſes; and Cit. Huſard and Biron, who are intruſted with theſe latter experiments, have already given both the muriate and carbonate of barytes to horſes ſeized with the farcy [farcin]. Both theſe medicines given to different horſes in the quantity of two drachms a day ſeemed for a while to have effected a perfect cure: they were even conſidered as cured at the end of a fortnight;

 when

It is not the fame with the carbonate of ftrontian: this was totally diffolved by muriatic acid, but the difengagement of carbonic acid gas amounted to thirty hundredths, and the falt obtained from this combination was in long needles or very flender rhomboidal cryftals, terminated by a pyramid with two faces: the prifms are fometimes hexahedral. This falt is alfo more foluble than muriate of barytes. I obtained 176 grains of the muriate from 100 grains of the carbonate. Dr. Crawford was one of the firft who obferved the great difference which there was in the form of the cryftals of the muriates of barytes and ftrontian, as well as their different degrees of folubility in water, and thence fufpected that thefe two fubftances could not be of the fame nature.

With fulphuric acid. Native carbonate of barytes reduced to powder is decompofed by fulphuric acid, with difengagement of carbonic acid gas. The refult of this combination is fulphate of barytes, infoluble in water.

Carbonate of ftrontian is alfo decompofed by fulphuric acid, with difengagement of carbonic acid gas; and the compound which is obtained is alfo but little foluble in water. Dr. Hope has obferved that four ounces of diftilled water only diffolved half a grain of it, and that if muriate of barytes be added to this folution, a precipitation of fulphate of barytes takes place: fulphate of ftrontian is therefore more foluble than that of barytes.

With acetous acid. The acetous acid difengages the carbonic acid from carbonate of barytes.

The fame acid alfo decompofes carbonate of ftrontian. The falts which refult from thefe combinations, viz. the acetites of barytes and ftrontian, are obtained in a cryftalline form, and are not deliquefcent.

Red Flame of Alcohol, holding in folution Nitrate or Muriate of Strontian or Lime.

AMONGST the characters by which Meffrs. Hope, Klaproth, and others have diftinguifhed ftrontian from barytes, there is one which they agree in confidering as peculiarly diftinctive. Chemifts have obferved that alcohol, in which either nitrate or muriate of barytes is diffolved, burns with a flame of a white yellow, whilft alcohol holding in folution nitrate or muriate of ftrontian burns with a flame of a carmine red. Dr. Hope relates in his memoir that Dr. Afh had fo long fince as 1787 obferved the particular colour which muriate of ftrontian gives to the flame of alcohol. The experiments repeated before the pupils of the polytechnic fchool prefented the fame refults; but as the nitrate and muriate of lime alfo communicate to alcohol the property of burning with a red flame, I thought it right to afcertain whether the nitrate and muriate of ftrontian did not contain lime. For this purpofe I tried the following experiments:

when the one to which the muriate of barytes had been given died without any apparent circumftance to indicate his death: the fecond, that which had taken the artificial carbonate of barytes, alfo died fuddenly fome days afterwards. The two horfes having been opened, all the vifcera were found in a found ftate, and exhibited no indication, either of the effect of the barytic preparations, or of the repercuffion of the difeafe. The experiments are continued on other horfes.

Since this article has been compiled, I have been informed, that a third horfe which was under a courfe of carbonate of barytes, alfo died fuddenly. It feems, therefore, more and more evident, that the carbonates of barytes, whether native or artificial, may prove mortal when taken internally. P.

Expe-

Experiments to aſcertain whether Strontian contains Lime.

1. TO a ſolution of nitrate of barytes I added fluoric acid, perfectly pure and free from ſulphuric acid: the mixture took place without precipitation.

2. To a ſolution of nitrate of ſtrontian I added the ſame fluoric acid: the mixture alſo took place without precipitation.

3. To ſolutions of the nitrates of barytes and ſtrontian put into ſeparate veſſels I added two or three drops of nitrate of lime, and afterwards ſome fluoric acid: this acid then very ſoon produced a white precipitate, which was fluate of lime.

It follows, from theſe comparative experiments, that the nitrate of ſtrontian did not contain lime; for, if it had contained ever ſo little, the fluoric acid would have occaſioned a precipitation. This acid has ſo ſtrong an affinity with lime that it takes it from the ſulphuric acid, when ſulphate of lime is holden in ſolution in water; ſo that when a few drops of fluoric acid are added to a ſelenitic water, a precipitation takes place as readily as when the oxalic acid is uſed. The fluoric acid preſents therefore an excellent means of detecting the preſence of lime. Very pure fluate of ammoniac may alſo be uſed; for, if this be mixed with the nitrate, muriate, or ſulphate of lime, a decompoſition takes place by double affinity, and the precipitate is fluate of lime.

Nitrate of Strontian is not precipitated, as Nitrate of Barytes is, by the Pruſſiates of Pot-aſh or Lime.

WE have ſeen, that the characters of ſtrontian in which it appears moſt ſimilar to barytes, exhibit, neverthelefs, differences ſufficiently well marked, when they are ſubjected to a rigorous examination. The following experiment, which is owing to the obſervations of Dr. Hope, preſents a more ſtriking diſtinction between the two earths. It was known that nitrate of barytes is totally precipitated by the pruſſiates of pot-aſh and lime. Dr. Hope having added pruſſiate of pot-aſh to a ſolution of nitrate of ſtrontian, perceived only a ſlight precipitation, occaſioned by the iron which accompanies the carbonate of ſtrontian. I had a mind to aſcertain this myſelf, and therefore prepared three ſolutions, viz. 1. A ſolution of native carbonate of barytes in nitric acid: 2. A ſolution in the ſame acid of carbonate of barytes, prepared by the decompoſition of barytic ſulphate: 3. A ſolution of carbonate of ſtrontian in the ſame acid. Theſe three ſolutions being placed in ſeparate veſſels, I added a ſufficient quantity of pruſſiate of pot-aſh, to decompoſe them entirely. There was in effect an abundant precipitate in the nitrates of barytes, and the ſupernatant liquors being tried with carbonate of pot-aſh gave no ſign of further precipitation. The nitrate of ſtrontian, on the contrary, gave only a weak blue precipitation, in conſequence of the iron which it contained; and an exceſs of pruſſiate of pot-aſh precipitated nothing more: but the ſupernatant liquor, tried with carbonate of pot-aſh, afforded a very abundant white precipitate, which was carbonate of ſtrontian.

Thus it appears manifeſt, that pruſſiate of pot-aſh does not at all decompoſe the nitrate of ſtrontian, whereas it totally decompoſes the nitrate of barytes. This method points out, therefore, a diſtinctive character between the two earths. I ſhall not here examine the nature of the precipitate which is obtained from the nitrate of barytes when decompoſed by pruſſiate of pot-aſh, nor whether the decompoſition takes place in conſequence of the formation of a trisule, or of a mixture of sulphuric acid, which the prussiate of pot-ash may sometimes contain. The comparative experiments which I have stated having been made with the same prussiate of pot-ash, the nitrate of strontian ought to have been decomposed, as well as that of barytes, if these earths had been of the same nature.

Constituent Parts of the Native Carbonates of Strontian and Barytes.

TO detail at large the different experiments which I have made to determine the constituent parts of the native carbonates of strontian and barytes, would be too prolix: I shall therefore only say, that it was by solution in muriatic acid that I determined the quantity of carbonic acid gas, and by calcination that of the earth. The mean result which I obtained was,

In 100 grains of native carbonate of barytes, or witherite,

Barytes, - - -	62
Carbonic acid, - -	22
Water, - - -	16
	100

In 100 grains of carbonate of strontian,

Strontian, - - -	62
Carbonic acid, - -	30
Water, - - -	8
	100

Conclusion.

I HAVE not been able to carry my researches further respecting strontian, not having any more to submit to new experiments: as soon as I can procure some, I shall continue them, as I perceive that it will be advantageous to establish the difference between it and barytes by more numerous and more striking characters. The results of my examination, and that of Messrs. Hope and Klaproth, are:

1st, That the carbonate of strontian is neither poisonous nor emetic, as both the native and artificial carbonates of barytes are;

2dly, That the carbonate of strontian is specifically lighter than that of barytes;

3dly, That it more easily parts with it's carbonic acid, and contains also a larger proportion of it than carbonate of barytes does;

4thly, That calcined strontian is soluble in cold and hot water, but more abundantly in the latter, so as to afford crystals by cooling;—a property, indeed, which it possesses in common with barytes;

5thly, That the muriate and other salts of strontian are more soluble than the homologous barytic salts; and the nitrate and muriate communicate to alcohol the property of burning with a red flame, whereas the same barytic salts tinge the flame of their alcoholic solution with a yellowish blue;

6thly, That strontian does not contain lime;

7thly and lastly, That the nitrate of strontian is not decomposed by prussiate of pot-ash, which decomposes the nitrate of barytes.

All these characters already establish a sufficiently distinguishable difference between strontian and barytes, and a still greater between it and all other known earths, so that it may be considered as a peculiar earth.

CON-

CONTINUATION.

IN the memoir which I read to the Inſtitute the 30th of April laſt, on the earth known by the name of ſtrontian, I remarked, that this earth was, when calcined, very ſoluble in boiling water, and that a portion became ſeparated in a cryſtalline form by cooling. I alſo remarked, that barytes acquired by calcination the property of becoming ſoluble in boiling water, and alſo affording cryſtals by cooling. It is in this ſtate of cryſtallization that ſtrontian and barytes muſt be found pure; and if they are of the ſame nature, as ſome chemiſts conceive, they ought to give ſimilar reſults when ſubmitted to comparative experiments. It has been thought that the properties which this earth exhibits in its combinations with the acids might be attributed to the preſence of carbonate of lime in the carbonate of ſtrontian; but calcined ſtrontian diſſolved in water cannot be ſuſpected to contain lime, and it is therefore in this ſtate that I have thought proper to procure ſome, in order to examine it comparatively with barytes, which I have alſo taken the pains to prepare in like manner in cryſtals, and perfectly deprived of carbonic acid. I ſhall here relate to the Inſtitute the experiments which I have made on this ſubject.

1. I firſt put into a ſmall capſule ſixteen grains of pure cryſtallized barytes, with thirty-ſix grains [demi-gros] of pure nitric acid of $12\frac{1}{2}°$. The combination took place with diſengagement of caloric, and the cryſtals of barytes without being diſſolved ſoon preſented opaque cryſtals, which were nitrate of barytes. I added 144 grains [deux gros] of diſtilled water, and the whole was diſſolved.

2. Sixteen grains of cryſtals of ſtrontian were treated with thirty-ſix grains of the ſame acid. The combination took place with diſengagement of caloric, but the cryſtals were diſſolved, and did not become opaque as in the preceding experiment. I added 144 grains of water, in order to have a ſolution ſimilar in ſtrength to the former.

3. A portion of the ſolution, No. 1, or that of nitrate of barytes prepared with pure barytes, being put into a glaſs, I added to it a pretty conſiderable quantity of pruſſiate of pot-aſh. A precipitation took place, and the liquor was not afterwards precipitated by the addition of carbonate of potaſh.

The other portion of the nitrate being evaporated, afforded cryſtals of barytic nitrate in octahedrons.

4. A portion of the ſolution, No. 2, or of the nitrate of pure ſtrontian, tried in like manner with pruſſiate of pot-aſh, afforded a ſlight precipitation; but, although I added an exceſs of the pruſſiate, the ſupernatant liquor ſtill yielded a pretty abundant precipitate with carbonate of pot-aſh.

The other portion of the ſolution of nitrate of ſtrontian being evaporated, afforded octahedral cryſtals, which were more ſoluble than thoſe of the barytic nitrate.

5. Fifty-four grains of barytes, pure and in cryſtals, being treated with 144 grains [deux gros] of weak nitric acid, the ſolution took place with heat, but without effervefcence. I added 144 grains of diſtilled water, and placed the ſolution to evaporate in a ſand-bath ſlightly heated; and when a portion of the water was evaporated, there were formed cryſtals in hexagonal plates ſimilar to thoſe of muriate of barytes. Their weight was thirty-two grains.

Twelve grains of this muriate, triturated in a glaſs mortar with half an ounce of alcohol, were

were entirely diffolved. I put the whole into a matrafs, and endeavoured to favour the folution by means of heat; a portion of the falt remained, however, undiffolved at the bottom of the matrafs.

Having fet the folution on fire, the alcohol burned with a yellowifh flame.

6. Fifty-four grains of cryftals of ftrontian being treated comparatively with the fame muriatic acid, the folution alfo took place with heat. The liquor being evaporated in an equal degree with that of the muriate of barytes (which, although upon the fire, had already cryftallized) ftill remained fluid; but, on being taken off the fire, it cryftallized in a mafs, and in needles like muriate of ftrontian. This falt is therefore more foluble in water than the muriate of barytes. The weight of the muriate of ftrontian obtained in this experiment was thirty-eight grains.

This muriate is alfo more foluble in alcohol; for, having triturated twelve grains in a glafs mortar with feventy-two grains of alcohol, they were entirely diffolved. I added, however, a fufficient quantity of alcohol to make the whole half an ounce, as in the preceding experiment; and having then fet it on fire, it burned with a flame of a beautiful red.

7. A faturated [aqueous] folution of pure barytes affords by the addition of a few drops of malic acid a white precipitate, which is malate of barytes; but the fame acid does not produce any precipitation in a faturated folution of ftrontian: it follows, therefore, that malate of ftrontian is more foluble than malate of barytes.

Thefe experiments continue, therefore, to eftablifh a difference between pure barytes and pure ftrontian.

Carbonate of ftrontian has hitherto been found only in one place; Strontian in Argylefhire: but M. Meyer, apothecary at Stettin, has lately announced that this earth is found in combination with fulphuric acid in the fulphate of barytes at Freyberg in Saxony. In confequence of this obfervation I analyfed a piece of barytic fulphate from Saxony, but the earth which I obtained from it was only barytes, and not ftrontian. I prefume that the fulphate which I affayed was different from that of which M. Meyer fpeaks; but I have, at all events, reafon to know that ftrontian exifts in other places as well as Argylefhire. Citizen Guyot, who travelled into Scotland with the Citizen Deleffere, fent me fome years fince feveral fpecimens of minerals which he had himfelf procured on the fpot, amongft which there is one which is labelled in the handwriting of Citizen Guyot "*Barytes from Leadhills*" in Scotland. I have examined this fpecimen, and find that it is carbonate of ftrontian. Its colour is a greenifh white, and it feems formed by the union of a number of prifms, which gives it a ftriated appearance. Its fpecific gravity is 3.6195. This carbonate, treated with muriatic acid, yields $31\frac{1}{2}$ per cent. of carbonic acid gas, and affords a muriate which cryftallizes in needles, and which, when diffolved in alcohol, communicates to it the property of burning with a red flame: in fhort, this muriate appears to me perfectly fimilar to muriate of ftrontian. I have alfo treated the ftone from Leadhills with the other acids, and the products which I have obtained demonftrate that the bafe of this carbonate differs in no refpect from ftrontian: here, therefore, we have this earth as well as at Strontian, and very probably the refearches of mineralogifts will difcover it in many other places.

II. *Abftract*

II.

Abſtract of Two Memoirs on a New Method of obtaining Barytes pure, and on the Properties of this Earth compared with thoſe of Strontian. By Citizens FOURCROY *and* VAUQUELIN. *Read to the National Inſtitute, 30th April and 21ſt September* 1796*.

THE difficulty of obtaining barytes pure, and the almoſt total impoſſibility of ſeparating this earth from the carbonic acid by calcination of its carbonate, are already ſufficiently known. In a courſe of Experiments undertaken jointly by Citizens Fourcroy and Vauquelin, to determine the characters and diſtinctive properties of the ſalts and compounds formed by barytes, one of the points which they were moſt deſirous of attaining, was of courſe to obtain this earth in a real ſtate of purity. Citizen Vauquelin having at length diſcovered a means of effecting this purpoſe by the decompoſition of nitrate of barytes by the action of fire, it became more eaſy to thoſe chemiſts to examine ſuch properties of this earth as had hitherto been unknown to them. The two memoirs of which we are here about to give an abſtract, ſhow the moſt remarkable properties which characterize both this earth and ſtrontian.

1. Nitrate of barytes in octahedral cryſtals, being expoſed to the action of fire in a porcelain retort, melts, ſwells, and gives out much oxygenous and azotic gas, with ſcarcely any nitrous vapour; and the retort being ſuffered to cool when no more elaſtic fluid is diſengaged, there is found on breaking it a grey, ſolid, but ſomewhat porous, maſs of a harſh taſte, and more cauſtic than quick-lime, which is pure barytes.

2. Expoſed to the blow-pipe on a piece of charcoal, this earth fuſes, bubbles up, and runs into globules which quickly penetrate into the charcoal.

3. In the air it effloreſces, cracks, burſts, ſwells up, heats and whitens, and, becoming thus rapidly flaked, it abſorbs 0.22 of its weight of water and carbonic acid.

4. It abſorbs water with extreme avidity, melts with hiſſing, heats conſiderably, ſolidifies the water, and cryſtallizes and hardens with it in ſuch a manner as to become a very tenacious cement, capable of adhering very ſtrongly to glaſs. A little more water changes it into a very bulky white powder. If it be entirely covered with water, it diſſolves in it with a violent hiſſing, and then cryſtallizes in tranſparent needles, which adhere together, and form compages like the particles of beaten plaſter.

5. Cold water diſſolves a twenty-fifth part of its weight; boiling water more than half: the latter by cooling depoſits very beautiful tranſparent priſms, which effloreſce and become pulverulent when expoſed to the air.

6. A ſolution of barytes has its ſurface more readily incruſted with a pellicle by expoſure to the air, and depoſits a more abundant precipitate by carbonic acid than lime-water.

7. The oxalic, citric, phoſphoric, and phoſphorous acids precipitate this ſolution, and the precipitates are re-diſſolved by the addition of an exceſs of the acids by which they are formed.

8. This ſolution decompoſes thoſe of ſoap, and of the nitrates of mercury, lead, and ſilver,

* Tranſlated from Annales de Chimie, XXI. p. 276, by J. F——r.

ſilver, precipitating the firſt black, the ſecond white, and the laſt of a fawn-colour. An exceſs of the barytic ſolution renders the oxides of lead and ſilver ſoluble.

9. Barytes thus prepared is ſoluble in alcohol. It is dreadfully poiſonous, and kills animals. Its moſt remarkable and moſt characteriſtic properties are its extreme cryſtallizability, which diſtinguiſhes it from all the other earths hitherto known in chemiſtry, and its great ſolubility in water.

The diſcovery by Meſſrs Hope and Klaproth of theſe two latter properties in ſtrontian induced us however to think for a while, with Citizens Pelletier and Coquebert, that barytes was ſo ſimilar to it as that they could not but be conſidered as very nearly allied, and perhaps even one and the ſame earth; but having received ſome fragments of ſtrontianite, of which we had not before been enabled to inveſtigate the properties ourſelves, we were eager to examine this new earth comparatively with barytes; and the reſult of this examination, as related in the ſecond memoir, which we are now abſtracting, ſhewed us that, notwithſtanding ſome very ſtrong analogies, theſe two earthy ſubſtances are really different, as Citizen Pelletier has concluded from his own reſearches relative to this ſubject. The following are the principal facts ſtated in this ſecond memoir.

Native carbonate of ſtrontian has a ſhade of light green, which that of barytes has not; it requires a little more muriatic acid for its ſolution, and contains more carbonic acid. Muriate of ſtrontian cryſtallizes by cooling, whilſt that of barytes cryſtallizes by evaporation: the former in hexagonal priſms, the latter in inclined plates. The ſolution of muriate of ſtrontian thickens into a jelly by evaporation, whilſt that of muriate of barytes dries in cryſtalline plates: the former, which is very ſoluble in alcohol, makes it burn with a flame of a beautiful purple; the latter, which is almoſt inſoluble in this liquid, gives it a yellow flame.

The greateſt and moſt remarkable difference which exiſts between ſtrontian and barytes is, that ſtrontian has leſs affinity with acids than barytes, and even than the fixed alkalies; ſo that a ſolution of barytes precipitates muriate of ſtrontian in white flocks.

Nitrate of ſtrontian differs from that of barytes; 1ſtly, in giving more nitrous vapour when decompoſed by fire, in conſequence of its retaining the nitric acid leſs ſtrongly than barytes; and, 2dly, in being three times as ſoluble in water.

Strontian obtained from its nitrate by the action of fire is leſs harſh than barytes; it does not fuſe in like manner by the blow-pipe, but glitters with a phoſphoric flame; it is almoſt ten times leſs ſoluble in water; and it ſeems to be leſs powerful in its attractions than lime, which it does not ſeparate from acids. When precipitated by the oxalic acid, it is not re-diſſolved by an exceſs of this acid, as barytes is; but the ſulphate of ſtrontian is on the contrary reſoluble by an exceſs of ſulphuric acid, which that of barytes is not.

The aqueous ſolution of ſtrontian is not precipitated by the gallic acid, whereas that of barytes affords with this acid a greeniſh precipitate.

The authors of theſe memoirs conclude from the Experiments (the reſults of which are here ſtated) that ſtrontian exhibits more properties which are different from thoſe of barytes than of ſuch as are analogous to them; and that, ſtriking as are the latter, it muſt nevertheleſs be concluded, from the whole of the phænomena, that theſe earths are really two different ſubſtances; a concluſion which is more particularly evident in reſpect of their different degrees of affinity, and of ſtrontian not being a poiſon, whereas barytes is a very active and violent one.

Several

Several of the properties which they have defcribed had been before obferved by Meffrs. Klaproth, Hope, and Pelletier; but feveral alfo are of their own difcovery. We have not here fpoken of fuch of the refults afforded by the two earths in their combinations, as were perfectly fimilar to each other; it having been our principal object to point out thofe fpecific characters by which they are to be diftinguifhed.

It is clear that we may reafonably entertain a hope, that more extenfive experiments fhall one day point out ufes both of barytes and ftrontian, as well as of their combinations, of great importance in refpect of the arts, when they fhall have been found more abundantly in nature, and when the progrefs of fcience fhall have perfected the means of obtaining them in a ftate of purity. The extreme folidity which barytes affumes when flaked, may lead us to prefume that it may be of ufe for making very hard and durable cements, or perhaps even mordants for the application of different matters on ftone, glafs, pottery and other fubftances of a like nature.

III.

Extract of a Letter from Count MUSSIN PUSCHKIN, Vice-Prefident of the Department of Mines at Peterfburg. On the Salts, Precipitates, and Amalgam of Platina; on Cobalt; on Antimonial Soap; and on the Decompofition of Soap by the Acid Extracts of Colouring Matters.*

I. *On the Salts and Precipitates of Platina.*

THE brick-coloured falt, which is obtained by pouring muriate of ammoniac into a folution of platina, is totally foluble in water, and depofits, when diffolved in boiling water, a black matter, which M. Fourcroy believes to be iron; but, in my opinion, it feems rather to be plumbago, though I have not yet made the requifite experiments to afcertain this fact. A fpecimen of this fubftance accompanies this letter.

In order that the whole of the falt may diffolve, it is neceffary to boil it for a long time with water; and when it is perceived that no more of the falt is taken up, the water muft be decanted, and frefh water poured into the veffel. An ounce of the falt required upon the whole between eight and nine pounds of water poured on and decanted at five different times. By this means the whole of the falt was not only diffolved, but its colour immediately became a beautiful orange; and by thefe cryftallizations, by evaporation on the water-bath and new folutions, after which the fluid was left to a very flow evaporation, the orange-colour of the cryftals became converted into a moft brilliant topaz-colour, and by new ebullitions was found no longer to depofit any black matter. I have not yet fucceeded in obtaining larger cryftals than thofe which Werner calls *very fmall*; but, by the affiftance of a good Englifh microfcope, all thofe which the topaz-coloured falt afforded were obferved to be perfectly tranfparent, and with fcarcely any exception affected the form either of an octahedral pyramid, or that of a polyhedron compofed of fix perfect hexagons united together by eight ifofceles triangles. There were fome lefs regular cryftals, which partook of the nature of both thefe figures.

* Annales de Chimie, XXIV. 205.

The alkalis precipitated (though with difficulty) from the aqueous folutions a fmall quantity of yellow powder, refembling Naples-yellow. I fuppofe, however, that this precipitate was compofed of cryftals, though none could be difcerned with the magnifier: and it did not appear to be foluble in water; for the nitrous acid, by remaining upon it for forty-eight hours, did not appear to have affected it in the leaft. The light yellow precipitate which is obtained by vegetable alkali, after fal-ammoniac has thrown down the whole of the brick-coloured precipitate, was feparated by the filter, and fubmitted without edulcoration to the action of nitrous acid in the proportion of nearly half an ounce of the acid to one dram of the precipitate, in conical veffels, fuch, for example, as a wine-glafs, in which the precipitate occupied the fmalleft-poffible fpace, and the acid covered it a finger's depth, and prefented a confiderable furface to the air. In three or four days the acid takes up more or lefs, according to the temperature, and affumes the confiftence of a jelly, at firft yellow, but afterwards of a chryfolite green more or lefs confiftent, but always in a certain degree moift. Before the blow-pipe this jelly becomes converted into a black fubftance, probably the imperfect oxide of platina. This experiment appeared furprifing to me at firft; but I recollected that Margraff, by detonating faltpetre upon platina, and then wafhing and filtering, had likewife obtained a jelly which may have been of the fame nature as mine. I muft obferve that I fatisfied myfelf in the moft convincing manner, that the acid and alkali I made ufe of were abfolutely pure, and that the platina alone, that is to fay in the metallic ftate, did not convert the nitrous acid into a jelly.

Urine, whether frefh or putrid, precipitates platina in the form of a fuper-compound falt, at the fame time that a grey yellow precipitate is formed, which is neither faline nor foluble in water. Part of this precipitate is formed a few moments after the urine is poured into the folution of platina: the falt is formed afterwards. In order to obtain this falt, it is neceffary that the folution of platina fhould be very concentrated, and carry with it an abundant portion of the firft precipitate. I have not yet attempted to diffolve this falt in boiling water, but I think it would exhibit a very beautiful topaz-yellow; for fome of the cryftals, when obferved in the microfcope, were femi-tranfparent, and had not the red colour of the falt precipitated by the muriate of ammoniac. With refpect to the grey-yellow precipitate, I fuppofe it to be phofphate of iron contaminated with fome foreign fubftance, and perhaps mixed with a fmall quantity of the precipitate of platina.

When urine is poured into a folution of the red falt in water, a beautiful lemon-yellow precipitate is formed, and the grey matter afterwards falls down flowly. This may perhaps afford a new method of feparating iron from platina, if this grey precipitate fhould in fact prove to be the phofphate of iron.

II. *On the Amalgam of Platina.*

REFLECTING on the facility with which the fuper-compound falts of platina are reduced in the fire, I thought that an amalgam of platina might perhaps be obtained by triturating thefe falts with mercury. To afcertain this, I took a dram of the orange-coloured falt of platina, and triturated it with an equal weight of mercury in a mortar of chalcedony.

 In

In a few minutes the ſalt loſt its colour, becoming at firſt brown, and afterwards greeniſh-brown; the matter was reduced to a very fine powder. Another dram of mercury was then added and the trituration continued, when the powder became grey; a third dram of mercury began to form an amalgam, and when the quantity amounted to ſix drams the amalgam was perfect. The whole operation ſcarcely employed twenty minutes. I added mercury to the quantity of nine times the weight of the ſalt, notwithſtanding which the amalgam was very tenacious; a fact which is indeed ſurpriſing, when it is conſidered that the ſalt contains no more than about 40 parts of platina in the hundred. In my Experiment therefore 24 grains of platina were ſufficient to give conſiſtence to 540 grains of mercury. This amalgam was eaſily ſpread out under the peſtle; it perfectly well received the impreſſions of the moſt delicate ſeals; its grain was very cloſe and brilliant, and in no reſpect inferior to that of the beſt amalgam of tin.

Though the ſight of a perfect amalgam of platina made in a few minutes, the production of which had coſt Lewis ſeveral weeks, and Sickingen ſeveral days, gave me pleaſure, I had not leſs ſatisfaction in obſerving the ſingular phenomenon I ſhall proceed to deſcribe:

Being deſirous of clearing my amalgam of its ſaline parts by waſhing, I triturated it in water in a glaſs mortar, and had ſcarcely given a few ſtrokes of the peſtle before I obſerved the ſurface of the amalgam to be covered with a black powder, mixed with ſome yellow particles. In leſs than ten minutes the whole of the amalgam diſappeared, inſtead of which I had this black powder. The yellow matter, which conſiſted of undecompoſed ſalt of platina, having likewiſe diſappeared by trituration; the black powder was ſeen, upon decanting the water, in extremely brilliant maſſes (*parcelles*), which probably were platina. The mercury had therefore paſſed with the greateſt facility to the ſtate of black oxide, nearly approaching the metallic ſtate. A portion of running mercury was found beneath, in the quantity of about two drams. Having taken a portion of the amalgam in the palm of my hand, and rubbed it with my finger, the ſame decompoſition took place in a few inſtants, and left a black powder interſperſed with brilliant particles. On the ſuppoſition that this oxide would eaſily paſs to the ſtate of cinnabar in the humid way, I poured the ſulphate of ammoniac (Beguin's volatile liver of ſulphur) upon it, and in leſs than 24 hours the powder became of a dull red, intermixed with metallic particles, which were evidently platina. I aſcertained afterwards by ſome experiments, that various metallic ſubſtances, and all the animal matters which I tried, decompoſe this amalgam by ſimple contact. For theſe reaſons it is abſolutely neceſſary to uſe a glaſs or ſiliceous peſtle and mortar in the compoſition of the amalgam.

In what manner are we to explain the ſpeedy reduction of the platina without oxidation of the mercury during the formation of the amalgam, as well as the much more ſpeedy oxidation of the mercury when brought into contact with water, or metallic or animal matters? This appears to me to be a very difficult queſtion, of which I expect the ſolution from more enlightened chemiſts. For the ſmall quantity of oxygene in the ſalt of platina is ſurely inſufficient to deprive ſo large a portion of mercury of its metallic ſtate; beſides which, in the dry trituration, ſuch as that which is made in the hollow of the hand, the acid is not diſſolved. Is it likely that there may be a decompoſition of atmoſpheric air, and

 a diſa difengagement of caloric from the vital air * ? This muft be determined by new experiments, which I have good reafon to expect from the illuftrious philofopher to whom I have the honour to addrefs myfelf. I fublimed the oxided powder of mercury in a fmall glafs retort. It afforded metallic mercury, and the muriate of mercury. The fubftance which remained at the bottom of the retort was friable, light, and of a colour inclining to a deep grey yellow. The fmall quantity of the powder of amalgam which I fubjected to experiment was not fufficient to admit of an examination of this refidue. I fhall not fail to repeat the experiment more at large, and acquaint you with the refults. In the mean time I requeft that the contents of this letter may ferve as a fupplement to the hiftory of platina, and beg that you will give it a place in your Annals under that title.

III. *Concerning Cobalt.*

WITH regard to the experiments I have made on cobalt, I muft fufpend my information until I fhall have been able to procure the regulus of cobalt in a quantity fomewhat confiderable, for the purpofe of repeating my experiments in the large way. Thofe which I have already made have afforded feveral interefting phenomena, particularly with regard to the effects of cold as a chemical agent. At prefent, I fhall only remark, that the oxides of cobalt, which have hitherto been fuppofed, after the methods of the beft chemifts, to be abfolutely pure, are neverthelefs contaminated with much foreign matters.

IV. *On the Antimonial Soap.*

PROFESSOR RUDOLPH has made an antimonial foap, after my method of preparing the mercurial foap. He finds it very active.

V. *On the Decompofition of Soaps by the Acid Extracts of Colouring Matters.*

I HAVE obferved, in decompofing foaps, that the acid extracts of colouring matter, fuch, for example, as that of Brazil wood by the diluted acetous acid, perfectly decompofe foaps, whether made with oil or wax. The oleo-colorant or cero-colorant compounds thus obtained are abfolutely infoluble in water, and afford extremely brilliant colours, which may perhaps be advantageoufly ufed in painting, particularly encauftic †. It would be likewife interefting

* The author thinks, and with reafon, that the oxygene united to the platina is far from being fufficient to [illegible] large a quantity of mercury; but he has not perhaps fufficiently attended to the facility with which the [illegible] metals pafs to the ftate of oxide, when they are rendered fluid, and their parts difengaged by their affinity with mercury. This has long been obferved with regard to the amalgam of gold, which foon becomes covered with a purple oxide by mere expofure to the air. Citizen Guyton, in a memoir on certain properties of platina, read to the National Inftitute the firft Meffidor, in the year 4, has exhibited a new proof of this principle in the oxidation of an amalgam of platina, obtained by the direct combination of the two metals, by means of an elevated temperature, and by the fimple effect of the affinity which was announced by the rank which platina occupies in the fcale of adhefions to mercury. *Note of the Editors of the Annales.*

† Citizen Chauffier has already communicated, in his leffons to the Polytechnic School, feveral experiments of this kind, which have perfectly fucceeded. The foap of copper afforded a very brilliant green, which it is prefume

intereſting to aſcertain preciſely whether the colouring matter has not more analogy with earths than has hitherto been thought, and whether it would not be advantageous, particularly in red dies, to employ the ſoaps of tin and of cochineal.

IV.

Obſervations on the Fundamental Property of the Lever; with a Proof of the Principle aſſumed by Archimedes in his Demonſtration. By the Rev. S. VINCE *, A.M. F.R.S.

THE want of a demonſtration of the property of the lever upon clear and ſelf-evident principles has juſtly been conſidered as a great deſideratum (defect) in the ſcience of mechanics, as the moſt important parts of that branch of natural philoſophy are founded upon it. Archimedes was, I believe, the firſt who attempted it. He ſuppoſes, that if two equal bodies be placed upon a lever, their effect to turn it about any point is the ſame as if they were placed in the middle point between them. This propoſition is by no means ſelf-evident, and therefore the inveſtigation which is founded upon it has been rejected as imperfect. Huygens obſerves, that ſome mathematicians, not ſatisfied with the principle here taken for granted, have, by altering the form of the demonſtration, endeavoured to render its defects leſs ſenſible, but without ſucceſs. He then attempts a demonſtration of his own, in which he takes for granted, that if the ſame weight be removed to a greater diſtance from the fulcrum, the effect to turn about the lever will be greater: this is a principle by no means to be admitted, when we are ſuppoſed to be totally ignorant of the effects of weights upon a lever at different diſtances from the fulcrum. Moreover, if it were ſelf-evident, his demonſtration only holds when the lengths of the arms are commenſurable. Sir I. Newton has given a demonſtration, in which it is ſuppoſed, that if a given weight act in any direction, and any radii be drawn from the fulcrum to the line of direction, the effect to turn the lever will be the ſame, on whichever of the radii it acts. But ſome of the moſt eminent mathematicians ſince his time have objected to this principle, as being far from ſelf-evident; and, in conſequence thereof, have attempted to demonſtrate the propoſition upon more clear and ſatisfactory principles. The demonſtration by M'Laurin, as far as it goes, is certainly very ſatisfactory; but as he collects the truth of the propoſition only from induction, and has not extended it to the caſe where the arms are incommenſurable, his demonſtration is imperfect. The demonſtration given by Dr. Hamilton, in his Eſſays, depends upon this propoſition, that when a body is at reſt, and acted upon by three forces, they will be as the three ſides of a triangle parallel to the directions of the forces. Now this is true when the three forces act at any point of a body; whereas, conſidering the lever as the body, the three forces act at different points, and

reſumed was known to the ancient maſters. On this ſubject, conſult the 3d cahier of the Journal of the Polytechnic School, p. 426 and 427.

* Phil. Tranſ. M.DCC.XCIV. 33.

2 therefore

therefore the principle, as applied by the author, is certainly not applicable. If in this demonstration we suppose a plane body, in which the three forces act, instead of simply a lever, then the three forces being actually directed to the same point of the body, the body would be at rest. But in reasoning from this to the case of the lever, the same difficulties would arise as in the proof of Sir I. Newton. But admitting that all other objections could be removed, the demonstration fails when any two of the forces are parallel. Another demonstration is founded upon this principle, that if two non-elastic bodies meet with equal quantities of motion, they will, after impact, continue at rest; and hence it is concluded, that if a lever which is in equilibrio be put in motion, the motions of the two bodies must be equal; and therefore the pressures of these bodies upon the lever at rest to put it in motion, must be as their motions. Now in the first place, this is comparing the effects of pressure and motion, the relation of the measures of which, or whether they admit of any relation, we are totally unacquainted with. Moreover, they act under very different circumstances; for in the former case the bodies acted immediately on each other, and in the latter they act by means of a lever, the properties of which we are supposed to be ignorant of. When forces act on a body considered as a point, or directly against the same point of any body, we only estimate the effect of these forces to move the body out of its place, and no rotatory motion is either generated, or any causes to produce it considered in the investigation. When we therefore apply the same proposition to investigate the effect of forces to generate a rotatory motion, we manifestly apply it to a case which is not contained in it, nor to which there is a single principle in the proposition applicable. The demonstration given by Mr. Landen, in his Memoirs, is founded upon self-evident principles; nor do I see any objections to his reasoning upon them. But as his investigation consists of several cases, and is besides very long and tedious, something more simple is still much to be wished for, proper to be introduced in an elementary treatise of mechanics, so as not to perplex the young student, either by the length of the demonstration or want of evidence in its principles. What I here propose to offer will, I hope, render the whole business not only very simple, but also perfectly satisfactory.

The demonstration given by Archimedes would be very satisfactory and elegant, provided the principle on which it is founded could be clearly proved, viz. that *two equal powers at the extremities, or their sum at the middle, of a lever, would have equal effects to move about any point.* Now, that the effects will be the same, so far as respects any progressive motion being communicated to the lever when at liberty to move freely, is sufficiently clear; but there is no evidence whatever that the effects will be the same to give the lever a rotatory motion about any point, because a very different motion is there produced, and we are supposed to know nothing about the efficacy of a force at different distances from the fulcrum to produce such a motion. Besides, the two motions are not only different, but the *same* forces are known to produce *different* effects in the two cases; so that in the former case the two equal powers, as the extremities of the arms, produce equal effects in generating a *progressive* motion; but in the latter case, they do *not* produce *equal* effects in generating a *rotatory* motion. We cannot, therefore, reason from one to the other. The principle, however, may be thus proved:

Let

Let A C, Pl. XXII. Fig. 1. be two equal bodies on a ſtraight lever, A P moveable about P. Biſect A C in B : produce P A to Q and take B Q = B P, and ſuppoſe the end Q to be ſuſtained by a prop. Then as A and C are ſimilarly ſituated in reſpect to each end of the lever, that is, A P = C Q, and A Q = C P, the prop and fulcrum muſt bear equal parts of the whole weight; and therefore the prop at Q will be preſſed with a weight equal to A. Now take away the weights A and C, and put a weight at B equal to their ſum; and then the weight at B being equally diſtant from Q and P, the prop and fulcrum muſt ſuſtain equal parts of the whole weight, and therefore the prop will now alſo ſuſtain a weight equal to A. Hence, if the prop Q be taken away, the moving force to turn the lever about P in both caſes muſt evidently be the ſame; therefore the effects of A and C upon the lever to turn it about any point, are the ſame as when they are both placed in the middle point between them. And the ſame is manifeſtly true if A and C be placed without the fulcrum and prop. If therefore A C be a cylindrical lever of uniform denſity, its effect to turn itſelf about any point will be the ſame as if the whole were collected into the middle point B; which follows from what has been already proved, by conceiving the whole cylinder to be divided into an infinite number of laminæ perpendicular to its axis of equal thickneſſes.

The principle therefore aſſumed by Archimedes is thus eſtabliſhed upon the moſt ſelf-evident principles, that is, that equal bodies at equal diſtances muſt produce equal effects; which is manifeſt from this conſideration, that when all the circumſtances in the cauſe are equal, the effects muſt be equal. Thus the whole demonſtration of Archimedes is rendered perfectly complete, and at the ſame time it is very ſhort and ſimple. The other part of the demonſtration we ſhall here inſert for the uſe of thoſe who may not be acquainted with it.

Let X Y, Fig. 2. be a cylinder, which biſect in A, on which point it would manifeſtly reſt. Take any point Z, and biſect Z X in B, and Z Y in C; then, from what has been proved, the effects of the two parts Z X, Z Y, to turn the lever about A, are the ſame as if the weight of each part were collected into B and C reſpectively, which weights are manifeſtly as Z X, Z Y, and which therefore conceive to be placed at B and C. Now $AB = AX - XB = \frac{1}{2}XY - \frac{1}{2}XZ = \frac{1}{2}YZ$; and $AC = AY - YC = \frac{1}{2}XY - \frac{1}{2}ZY = \frac{1}{2}XZ$; conſequently $AB : AC :: \frac{1}{2}YZ : \frac{1}{2}XZ :: YZ : XZ ::$ the weight at C : the weight at B.

The property of the ſtraight lever being thus eſtabliſhed, every thing relative to the bent lever immediately follows.

V.

Obſervations on the Acid of Tin, and the Analyſis of its Ores. Read at the Sitting of the Claſs of Mathematical and Philoſophical Sciences of the National Inſtitute of France, the firſt of Meſſidor, in the Year 5. By Citizen GUYTON *.

IT has long been obſerved that the nitric acid calcines tin inſtead of diſſolving it, and the phlogiſtic hypotheſis was incapable of explaining this phenomenon. It remained among

* Inſerted in the XXIVth volume of the Annales de Chimie.

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the number of facts for which the most fruitful imagination could not afford a probable solution. This difficulty has vanished since it has been discovered that the metals are capable of being converted into acids by a sufficient quantity of oxygene. Tin is one of the metals which must have most effectually contributed to fix the attention of chemists upon this combination. In this manner it is seen that the precipitation of gold by tin in the form of a purple oxide is simply, as I have announced in the Elements of Chemistry of the Academy of Dijon, the result of the action of the stannic acid upon gold. This oxigenation of tin is much more evidently seen in the operation which I have described in the first volume of the Chemical Dictionary of the Encyclopedia, p. 632 ; an operation which seems well calculated to exhibit at the same instant every fundamental principle of the pneumatic chemistry. For when the diluted nitric acid and tin are treated simply in the retort, the result is oxide of tin and nitrate of ammoniac, the latter of which is formed by the azote or basis of the acid, and the hydrogene of the water, without the appearance of nitrous gas at any period of the operation.

No further difficulty remained therefore with respect to the oxidation of tin by the nitric acid, as it is evidently effected by the decomposition of the acid and the affinity of the tin for oxygene. But no account has yet been given of the state in which this metal exists in its ores, particularly in the crystals of tin (zinn-graupen, zinnstein, of the Germans), nor of the causes which oppose their solution in acids.

This question presented itself to my consideration on reading the publication of Mr. Klaproth on the ores of tin in the second volume of Analyses with which he has enriched chemistry and mineralogy.

He begins by comparing the products of the same ore treated with alkaline reducing fluxes, and with charcoal alone. The difference convinced him, that in the first process the alkali retains a considerable portion of the metal in the state of oxide. He afterwards endeavoured to complete the analysis in the humid way. For this purpose he followed Bergman's process, which consists in digesting the powdered ore in concentrated sulphuric acid; adding muriatic acid after the cooling, and precipitating the solution by soda. By this treatment 131 parts of precipitate ought to be afforded for every 100 parts of metal contained in the ore. Mr. Klaproth, in fact, obtained a solution of about 0,19 of that rather scarce tin ore which the English call wood-tin ; but he could not succeed in charging the muriatic acid with any perceptible quantity of tin, by subjecting the other more common varieties to the same treatment.

Reflecting on the nature of the obstacle which might probably oppose the action of the muriatic acid on a substance which he estimated to contain 0,99 of oxide of tin, he imagined that it could be nothing but an excess of oxygene.

To remove this excess, he first attempted to treat the ore in the retort with a mixture of sulphur. Most of the sulphur was sublimed, and the residue, slightly agglutinated, had still the same grey whitish appearance as after the pulverization, excepting that some brilliant gold-coloured specks were seen; namely, sulphate of tin, or aurum-musivum. The mineral, however, was not more soluble in the muriatic acid than before.

After many other trials equally unsuccessful, the celebrated chemist of Berlin had recourse to that substance which in his hands has become so powerful an instrument of analysis.

lyfis. He treated his ores with fix parts of potafh in a crucible of filver, and the fuccefs was beyond his expectations. By the firft operation, 0,91 were rendered foluble in water, precipitated and afterwards taken up by the muriatic acid. The muriate of tin being then decompofed by carbonate of foda, a pure oxide was obtained, which was eafily re diffolved in the fame acid, and precipitated by zinc. This precipitate, fufed in a crucible with tallow, afforded a button of the fame weight as had been obtained from the fame kind of ore in the dry way.

By this means he was authorifed to conclude as follows, with regard to the brown ore of Schlackenwald;

In 100 parts	Tin - - -	75
	Iron - - -	0,5
	Silex - - -	0,75
	Oxygene - -	23,75
		100

I have treated in the fame manner a cryftal of tin ore of this kind, alfo from Schlackenwald; not becaufe I had any doubt of the fuccefs announced by a chemift who has fo long been in poffeffion of well-earned confidence; but this fuccefs feemed to be a confirmation of the conjecture on which Mr. Klaproth had founded his experiments, and I fuppofed that there was good reafon to doubt whether the complete faturation or fuperfaturation of tin by oxygene were the true caufe of its infolubility in the muriatic acid. For I could not at any period of the operation difcern either the fubftance which might feize this excefs of oxygene, or any trace of the phenomena which muft have accompanied its difengagement.

In order to obferve the circumftances with more facility, I operated in a fmall crucible of platina over a reverberatory lamp of Argand (fur une lampe d'Argand à réchaut*). Fifty-five centigrammes of the brown cryftals of tin, reduced into fine powder, were well mixed with fix times the quantity of potafh purified by alcohol, and dried. The mixture was moiftened with a few drops of water, then evaporated to drynefs, and afterwards heated to the commencement of fufion. After the firft operation, hot water was poured on the mafs, and took up more than half the mineral. Muriatic acid firft precipitated it, and afterwards diffolved it with the greateft facility. The precipitate or metallic oxide, reproduced by carbonate of potafh, was in fact completely foluble by the fame acid, as Mr. Klaproth had affirmed.

But after having thus witneffed the facts, my firft doubt concerning the direct folubility of this ore was rather ftrengthened than removed. For it cannot be affirmed that the excefs of oxygene was diffipated during the fufion with potafh, becaufe the metal could not form a foluble combination with the alkali, but in confequence of its oxidation to the laft degree, or, to fpeak more correctly, its acidification; fo that the lixivium of the filtered refidue is a true ftannate, or perhaps a ftannite †, of tin (potafh).

* The author intends foon to give a defcription of this lamp.

† Mr. Hatchett in his paper on the molybdate of lead and the molybdic acid, inferted in the Philofophical Tranfactions of London for 1796, compares precifely the action of the nitric acid on molybdena, to fuperoxygenate it, to that which produces the fame effect on tin. It may converfely be inferred that tin is acidified like molybdena. G.

In the mean time, as we are forced to admit that all the oxygene of the ore is found in the alkaline solution, the more effectual action of the acid upon the metal cannot be supposed to arise from the loss of a portion of this principle, because it continues still in the same state of saturation; neither is there any sign of oxygenated muriatic acid gas; and it would likewise be difficult to conceive, why in this circumstance there should rather be a disengagement of the gas than when the acid is digested on the ore, as is observed in the oxides of manganese and of lead.

To establish this point of theory on a more decisive experiment, I dissolved six grammes of tin in the nitric acid, and evaporated several times to dryness the new acid which was successively poured on. I suppose it cannot be doubted but that the tin in this state had combined with as much oxygene as it is capable of fixing. Nevertheless, the mass of white oxide, washed until the water which came off caused no further change in vegetable colours, was very soluble in muriatic acid.

What therefore is the cause of the insolubility of the ore, which consists also of tin and oxygene with scarce one hundredth part of foreign matter? This cause is to be found only in the state of aggregation of the latter. This assertion cannot be thought strange, except on account of the little regard which has hitherto been paid to its energy. If combinations be the result of affinity or elective attraction, is not this attraction itself a power which may be rendered ineffectual by the sum of the forces which act in the contrary direction? These truths surely cannot appear repulsive to Mr. Klaproth, who has rendered them so evident, by shewing that the ruby, the sapphire and the adamantine spar, of which the elements are in their own nature so easily soluble, do not resist the ordinary methods of analysis but in consequence of the aggregation of their integral parts.

VI.

On Fairy Rings.

THE appearance in the grass, commonly called Fairy Rings, is well known. It consists either of a ring of grass of more luxuriant vegetation than the rest, or a kind of circular path in which the vegetation is more defective than elsewhere. It appears to be pretty well ascertained, that the latter state precedes the former. Two causes are assigned for this phenomenon: the one, which cannot be controverted, is the running of a fungus; the other, which has been considered as an effusion of theory, is grounded on a supposition that the explosion of lightning may produce effects of the same kind on the ground as Dr. Priestley's battery was found to produce on the polished surface of a plate of metal, that is to say, a series of concentric rings. Some observations, which I find in my common-place book, appear to shew that this last effect may, in certain circumstances, take place.

On Tuesday the 19th of June, 1781, a very powerful thunder-storm passed over the western extremity of London. I was then at Battersea, and made no other remark on the phenomena than that the explosions, which were very marked and distinct, were in many instances forked at the lower end, but never at the top; whence it follows, that the clouds were in the positive state for the most part. On the following Sunday, namely the 24th, I happened

I happened to be in Kenfington Gardens, in every part of which extenfive piece of ground the lightning had left fome marks of its agency, chiefly by difcoloration of the grafs in zigzag ftreaks, fome of which were fifty or fixty yards in length. Inftances of this fuperficial courfe of the lightning along the ground before it enters the earth are fufficiently frequent. But the circumftance which attracted my attention the moft was feen in a fmall grove of trees at the angular point of one of the walks. Plate XXII. figure 3. exhibits a fketch of part of the gardens, in which the angular point is denoted by the letter A. Figure 4. reprefents the pofition of the trees. The numbers exprefs the diftances between the trees in feet.

Clofe to the ftem of the tree A was a hole in the ground four inches long and two wide, proceeding to the fouthward; and at two feet further to the fouth was another fimilar hole. Between thefe two holes the ground was torn away. This appearance is reprefented in figure 3, in which the letter A reprefents the trunk of the tree. The grafs was very much fcorched to the diftance of about three feet in every direction from the trunk, and in this burned fpace there were feveral other fmaller holes.

Clofe to the trunk of B, on the fouth fide, there was a hole in the ground.

Near the tree C there was alfo a hole in the ground, and it was furrounded with a faint ring of burnt grafs at a little diftance; but as the grafs was grown again, it feemed probable, that the ring was occafioned by fome earlier ftroke than that on Tuefday.

The tree D was furrounded by a ring of 6 feet radius, and 18 inches broad. Within the ring the grafs was frefh; but on the furface of the ring the grafs and the ground were much burned. To the eaftward of the tree upon the ring itfelf were two holes, in which the ground had the appearance of afhes. The tree E had half a faint ring to the weftward.

The tree F was furrounded by a faint ring of twoy ards radius. Within the ring the grafs was unhurt. To the weftward, at about three feet diftance from the inner ring, was part of another fimilar ring, of much the fame appearance; the verdure being unhurt in the interval between the rings.

I imagined the leaves of the trees were a little curled, but could obferve no blafted boughs; a circumftance which, together with the other facts, appears to indicate that thefe appearances were produced by the recent ftorm of the 19th.

VII.

Experimental Refearches to afcertain the Nature of the Procefs by which the Eye adapts itfelf to produce diftinct Vifion.

[Concluded from page 479.]

IN the eel there is a tranfparent horny convex covering at fome diftance before the eye, to defend it from external accidents. This covering to an eye fitted to fee in air, would entirely take off the effects arifing from change of figure in the cornea; but in water, where no fuch change could be attended with advantage, fuch a covering is employed as an external defence.

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In the eyes of fifhes the ciliary proceffes are entirely wanting. The cryftalline lens is fpherical and imbedded in the vitreous humour, which is inclofed in cells of a ftronger texture than in other animals.

The iris does not admit of motion; this is taken notice of by Haller; and the reafon probably is, that the light in water is never too ftrong for the eye to bear.

There is a mufcle fituated between the retina and the fclerotic coat, which is, as Mr. Home thinks, common to all fifh. This mufcle is particularly defcribed by Mr. Haller; and its ufe is ftated to be that of bringing the retina nearer the cryftalline lens for the purpofe of feeing objects at a greater diftance. Mr. Hunter called it the choroide mufcle, and has preferved feveral preparations of it.

This mufcle has a tendinous centre round the optic nerve, at which part it is attached to the fclerotic coat; the mufcular fibres are fhort, and go off from the central tendon in all directions; the fhape of the mufcle is nearly that of a horfe-fhoe; anteriorly it is attached to the choroide coats, and by means of that to the fclerotic. Its action tends evidently to bring the retina forwards; and in general the optic nerve in fifhes makes a bend where it enters the eye, to admit of this motion without the nerve being ftretched.

In thofe fifhes that have the fclerotic coat completely covered with bone, the whole adjuftment to great diftances muft be produced by the action of the choroide mufcle; but in the others, which are by far the greater number, this effect will be much affifted by the action of the ftrait mufcles pulling the eye-ball againft the focket, and compreffing the pofterior part, which, as it is the only membraneous part in many fifhes, would appear to be formed fo for that purpofe.

In fifhes, the eye in its natural eafy ftate appears to be adjufted to near objects, requiring fome change to adapt it to fee diftant ones; in this refpect differing entirely from the bird, the quadruped, and the human.

The preceding obfervations, on the ftructure of the eye, indicate two methods of adjufting the eye; one for feeing in air, the other for feeing in water; and that the cryftalline lens, as the moft confpicuous part, appears to have engroffed the whole attention of former enquirers, who in general do not feem to have paid fufficient attention to the elongation of the axis and change of curvature in the cornea. That the axis of vifion is really lengthened, and the lens moved forward, appears highly probable from the whole of the facts; and fince the elongation of the eye, the change of pofition of the cryftalline, or the alteration of curvature in the cornea, have feverally appeared to be the caufe of the requifite adjuftment, the combination of all three muft undoubtedly be fufficient to effect the purpofe.

In the Philofophical Tranfactions for 1797, page 1, Mr. Home explains certain morbid affections of the ftrait mufcles and cornea of the eye, and confiders their treatment. He ftates the ufes of the mufcles to be; firft, that of moving the eye-balls in different directions; fecondly, that of caufing the two eyes to correfpond; and thirdly, that of producing adjuftment by their lateral preffure. Imperfection in any one of thefe different actions muft be confidered as a difeafe. Three different cafes occur in practice: namely, indiftinct vifion, double vifion, and fquinting.

Indiftinct vifion is explained to confift of an inability in the mufcles to fupport that degree of tenfion which is requifite to adjuft the eye to near objects. In this morbid affection of the ftraight mufcles they may be capable of performing all the intermediate contractions

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as usual, but not the extreme degrees without considerable pain. As these symptoms have not been accounted for before in this way, the author adduces some curious instances of similar affections of other muscles, and concludes that bad effects must necessarily arise from every thing which irritates or weakens the parts themselves, or the general habit; and that such means ought to be adopted as may soothe the parts in their sensations, and quiet and strengthen their actions, since in that way only the muscular fibres can possibly recover their tone.

On the subject of double vision the author remarks, that many opinions have been advanced to account for the single appearance of objects contemplated by both eyes. The opinion of Dr. Reid of Glasgow, that vision is single when the impressions are made on correspondent parts of the retina in the two eyes, and double when this is not the case, appears to him to be strongly confirmed by the facts. This want of correspondence may be produced by some change in the refracting media of one of the eyes, or else by a want of similar actions in the muscles of both eyes respectively. The former takes place after the crystalline lens of one or both eyes has been extracted, and the convex lens made use of to produce the requisite focal adjustment is not duly placed. The latter is more particularly the object of medical treatment. Mr. Home advises repose, that is to say, that the disordered eye should be covered for a time. For, as he remarks, the first object of attention, with regard to strained or over-fatigued muscles, must be to put them into an easy state, and confine them from motion; and this practice is no less applicable to the muscles of the eye than those of other parts.

As double vision is produced by a moderate derangement of the optic axes, squinting is effected by a much greater derangement. In the case of squinting, the author proves that the object is not seen by both eyes; but that one eye, more or less perfect, is directed to the object, while the other, which in such cases is imperfect, is drawn aside by habit, in order that its operation may not disturb the perception received by the other eye. The greater strength, shortness and straitness of the adductor muscle causes the deviation to be made towards the nose. These doctrines are illustrated by apposite cases. Squinting takes place in three different circumstances; that is to say, where one eye has only an indistinct vision; where both eyes are capable of seeing objects, but the one is less perfect than the other; and where the muscles of one eye have, from practice, as in the case of frequently looking through telescopes, acquired a power of moving it independently of the other.

When squinting arises from absolute imperfection in the eye, there is no cure. Where it arises from weakness only in the sight of one eye, it may in some instances be got the better of by confining the person to the use of one eye by covering the other; in this way the muscles, from constant use, become perfect in the habit of directing the organ, and acquire strength and power of adjustment. The time required must depend upon the weakness of the sight, and length of time during which the muscles have been left to themselves.

The Author shews, that he cornea is not, like the cuticle, devoid of life; though, like tendons and ligaments, it is neither supplied with red blood nor possesses sensibility, but is made up of membraneous ligaments, which are continuations of the tunica conjunctiva and the tendons of the four strait muscles. When wounded, it commonly unites, like other living parts, by the first intention, and in some cases with inflammation exceeding the limits of adhesion; and the whole internal cavity of the eye proceeds to a state of suppuration. These stages of inflammation are only met with in parts possessed of life.

From the opinion of the cornea being void of life, the opacities which are found to take place on it have been supposed to arise from inanimate matter laid over its surface. And under that notion acrid and irritating applications have been used to remove it, such as powdered glass, powdered sugar, &c. Such applications being of service, have confirmed the opinion.

Dr. Home considers the cornea as a ligamentous part, and, as such, weak in its vital powers. This arises from such parts having no vessels carrying red blood. When they inflame, which is a state of increased action, they therefore require a different mode of treatment from the other parts of the body, whose vital powers are strong in consequence of being largely supplied with red blood.

The truly healthy inflammation requires an increased action in the parts affected; and if this, either from weakness or indolence, is not kept up, the inflammation does not go rapidly through its stages, but remains in a state between resolution and suppuration. In ligamentous structures the actions must therefore be roused and supported, when under inflammation, to promote resolution, and to prevent the parts from falling into an indolent diseased state. This is however attended with difficulty, and they too often become considerably thickened by a decomposition of coagulating lymph during the adhesive state of inflammation, which in the cornea renders it opake. The thickening of the parts remains after the inflammation is gone, and can only be removed by absorption, which is best effected by the application of very stimulating medicines.

He extends these principles to all ligamentous structures. These require a peculiar treatment, which may be illustrated both in inflammations of the joints, and of the cornea of the eye; the applications made use of with the greatest advantage in both cases being of a very stimulating kind.

The instance of the cure of an opacity of the cornea in the eyes of Tobit by the gall of a fish, as related in the apocryphal book which bears his name; with other circumstances of like practice among the Arabians; and also certain facts and observations which occurred in his own practice, are related as proofs of the advantage of stimulating applications to the cornea. In some old cases of opacity, he found gall the best application. He used it pure, and also diluted. The gall of quadrupeds, in these trials, gave more pain than the gall of fish. The painful sensation was very severe for an hour or two, and then went off, and the beneficial effects appeared to be in proportion to the local violence at the time of its application. The object of his observations having been to explain the principle upon which those effects depend, this knowledge may, as he observes, regulate the practice suitable to be adopted in such cases. It will shew the impropriety of using such medicines while the inflammatory action is increasing, and will point out their adoption the moment the inflammation appears to be at a stand, instead of postponing these remedies till an indolent unhealthy state takes place, which too often terminates in opacities not to be afterwards removed by any application.

VIII. *Expe-*

VIII.

Experiments and Obſervations on the Inflexion, Reflexion, and Colours of Light.

By HENRY BROUGHAM, *Jun. Eſq.**

IT has always appeared wonderful to me, ſince nature ſeems to delight in thoſe cloſe analogies which enable her to preſerve ſimplicity, and even uniformity in variety, that there ſhould be no diſpoſitions in the parts of light with reſpect to inflexion and reflexion analogous or ſimilar to their different refrangibility. In order to aſcertain the exiſtence of ſuch properties, I began a courſe of Experiments and Obſervations, a ſhort account of which forms the ſubſtance of this paper. For the ſake of perſpicuity I ſhall begin with the analytical branch of the ſubject, comprehending my obſervations under two parts: flexion, or the bending of the rays in their paſſage by bodies, and reflexion. And I ſhall conclude by applying the principles there eſtabliſhed to the explanation of phenomena, in the way of ſyntheſis.

As in every experimental enquiry much depends on the attention paid to the minuteſt circumſtances, in juſtice to myſelf I ought to mention that each experiment was ſet down as particularly as poſſible, immediately after it was made; that they were all repeated every favourable day for nearly a year, and before various perſons; and as any thing like a preconceived opinion with reſpect to matter of theory that is in diſpute, will, it is more than probable, influence us in the manner of drawing our concluſions, and even in the manner of recording the experiments that lead to theſe, I have endeavoured, as much as poſſible, to keep in view the ſaying of the Brahmin, "that he who obſtinately adheres to "any ſet of opinions, may bring himſelf at laſt to believe that the freſh *ſandal-wood* is a flame of fire †."

PART I. *Of Flexion.*

IN order to fix our ideas on a ſubject which has never been treated of with mathematical preciſion, we ſhall ſuppoſe, for the preſent, that all the parts of light are equally acted upon in their paſſage by bodies, and deduce ſeveral of the moſt important propoſitions which occur, without mentioning the demonſtrations.

Def. 1. If a ray paſſes within a certain diſtance of any body, it is bent inwards; this we ſhall call inflexion. 2. If it paſſes at a ſtill greater diſtance, it is turned away; this may be termed deflexion. 3. The angle of inflexion is that which the inflected ray makes with the line drawn parallel to the edge of the inflecting body; and the angle of incidence is that made by the ray before inflexion, at the point where it meets the parallel. And ſo of the angle of deflexion.

Propoſition I. The force by which bodies inflect and deflect the rays, acts in lines perpendicular to their ſurfaces.

Prop. II. The ſines of inflexion and deflexion are each of them to the ſine of incidence in a given ratio—(and what this ratio is, we ſhall afterwards ſhew).

Prop. III. The bending force is to the propelling force of light as the ſine of the difference between the angles of inflexion (or deflexion and incidence to the coſine of the angle of inflexion or deflexion).

* Philoſophical Tranſactions, M.DCCXCVI. I. † Aſiatic Reſearches, vol. i. p. 224.

 Prop.

Prop. IV. The rays of light may be made to revolve round a centre in a spiral orbit.

Prop. V. If the inflecting surface be of considerable extent, and a plane, then the curve described may be found by help of the Prop. XLI. Book I. of Principia; provided only the proportion of the force to the distance be given. Thus, if the bending force be inversely as the distance, the curve cannot be found; for, in order to obtain its equation, a curvilinear area must be squared, which, in this case, is a conic hyperbola; the relation, however, between its ordinates and abscissæ may be obtained in fluxions, thus: $y\dot{y} + b\dot{y} = a^2\dot{x}^2$.

If the force (which is most probable) be inversely as the square of the distance, the curve to be squared is the cubic hyperbola; species LXV. genus III. of Newton's Enumeration; and this being quadrable, the curve described by the light will be the *paraleia campaniformis pura*; Species LXIX. of Newton.

If the force be inversely as the cube of the distance, the curve is a circular arch, and that of deflexion is a conic hyperbola *. If the inflecting body be a globe or cylinder, and the force be inversely as the square of the distance from the surface; then, by Prop. LXXI. Book I. of Principia, the attraction to the centre is inversely as the square of the distance from that centre; and therefore, by Prop. XI. and XIII. of the same Book, the ray moves in an ellipse by the inflecting, and in an hyperbola by the deflecting force; each having one focus in the centre of the body. The truth of these things mathematicians will easily determine.

Prop. VI. If a ray fall on a specular surface, it will be bent before incidence into a curve having two points of contrary flexure, and then will be bent back the contrary way into an equal and similar curve, as in Fig. 1. Pl. XXIII.

Corollary to these propositions. If a pencil of rays fall converging on an interposed body, the shadow will be less than the body by twice the sine of inflexion.

And if a pencil fall diverging on the body, the shadow will be greater than the body by twice the sine of inflexion; but less than it should be, if the rays had passed without bending, by twice the sine of the difference between the angles of inflection and incidence. The sine or angle of incidence is greater than the sine or angle of inflexion, when the incident rays make an acute angle with the body; but, when they make an obtuse or right angle, then the sine or angle of inflexion is less than that of incidence. The sine of incidence is greater than that of deflexion, if the angle made by the incident ray with the body is obtuse; but less, if that angle be acute or right. If a globe or circle be held in a beam of light, the rays may be made to converge to a focus.

Hitherto it has been supposed that the parts of which light consists have all the same disposition to be acted upon by bodies which inflect and deflect them; but we shall now see that this is by no means the case.

Observation I. Into my darkened chamber I let a beam of the sun's light through a hole in a metal plate (fixed in the window-shut) of $\frac{1}{40}$ of an inch diameter; and all other light being absorbed by black cloth hung before the window and in the room, at the hole I placed a prism of glass, whose refracting angle was 45 degrees, and which was covered all over with black paper, except a small part on each side, which was free from impurities, and through which the light was refracted, so as to form a distinct and tolerably homogeneous spectrum on a chart at six feet from the window. In the rays, at two feet from the

* Principia, Lib. i. Prop. 8.

prism,

prifm, I placed a black unpolifhed pin (whofe diameter was everywhere one tenth of an inch) parallel to the chart and in a vertical pofition. Its fhadow was formed in the fpectrum on the chart, and had a confiderable penumbra, efpecially in the brighteft red, for it was by no means of the fame thicknefs in all its parts; that in violet was broadeft and moft diftinct; that in the red narroweft and moft confufed; and that in the intermediate colours was of an intermediate thicknefs and degree of diftinctnefs. It was not bounded by ftraight but by curvilinear fides, convex towards the axis, to which they approached as to an afymptote, and that neareft in the leaft refrangible rays, as is reprefented in Fig. 2, where A B is the axis, I K L M N A, and H G F E D A the two outlines. Nor could this be owing to any irregularity in the pin, for the fame thing happened in all forts of bodies that were ufed; and alfo if the prifm was moved on its axis, fo that the colours might afcend and defcend on thefe bodies; ftill, wherever the red fell, it made the leaft, and the violet the greateft fhadow.

Obf. II. In the place of the pin I fixed a fcreen, having in it a large hole, on which was a brafs plate pierced with a fmall hole $\frac{1}{12}$d of an inch in diameter. Then caufing an affiftant to move the prifm flowly on its axis, I obferved the round image made by the different rays paffing through the hole to the chart; that made by the red was greateft, by the violet leaft, and by the intermediate rays, of an intermediate fize. Alfo, when at the back of the hole, I held a fharp blade of a knife, fo as to produce the fringes mentioned by Grimaldo and Newton; thofe fringes in the red were broadeft and moft moved inwards towards the fhadow, and moft dilated when the knife was moved over the hole, and the hole itfelf on the chart was more dilated during the motion when illuminated by the red, than when illuminated by any other of the rays, and leaft of all when illuminated by the violet. Now in Obfervation I. the angle of incidence of the red rays was equal to that of the violet and all the reft, and yet the angle of inflexion was greateft and leaft in the violet; and indeed the difference between the two was greater than appears at firft from the experiment; for that part of the fhadow which was formed by the violet, fell at a greater diftance from the point of incidence than did that part which was formed by the red, from the divergency of the different rays upwards by the refraction, as appears in Fig. 3, where D E is the window, F G the beam propagated through the hole F, refracted by the prifm K I H and pointing on the chart O P q s; the fpectrum v r, being feparated in L r, the red rays incident on the pin C D at C; and M v, the violet incident at D; the fhadow of D C being formed in v r, fo that v being farther from D, than r is from C, therefore (by the propofitions formerly laid down) the fhadow in v fhould be confiderably lefs than in r, if the rays were equally inflected. Laftly, in Obf. II. the angle of the red's incidence was nearly equal to that of the violet's, by the motion of the prifm, and the confequent motion of the colours; only that if there was any difference it was on the fide of the violet: and yet the violet was leaft inflected, and the red moft inflected; and fo of the fecond inflexion by the knife-blade: wherefore I conclude that the rays of the fun's light differ in degree of inflexibility, and that thofe which are leaft refrangible are moft inflexible.

Obf. III. My room being darkened as before, and a conical beam propagated through the fmall hole in the window fhut; at this hole I placed a hollow prifm made of broken plates of mirror, and of fuch an angle that, when filled with diftilled water, it caft a fpectrum on an horizontal table, and was there received on a chart seven feet from the window. I then placed on the same table, and in the rays between the chart and the prism, at three inches from the chart, two sharp knife-blades, with even edges, and fixed to a board with wax, so as to make an angle with one another; moving them nearer and nearer, till I saw the fringes appear in the red light on the chart, and then in the orange and other colours successively. I then withdrew one, and the fringes became faint and narrow, and not all within the shadow of the remaining knife, but at its edge, and even in the light of the spectrum. Lastly, when I slowly approached the other, they moved into the shadow, and became broader and farther separated one from another; there being the like fringes in both shadows. This I repeated in all the rays, and plainly saw, that at the approach of the knife the fringes became broader and farther removed from one another, and from the light, in the red than in the violet, or any of the other rays.

Obs. IV. In repeating the foregoing Experiment, I observed a very curious phenomenon. When the angle of the knife-blades was so held in any of the rays, as to make the hyperbolic fringes described by Newton*, and these being always of the colour in which they were held; moving the angle a little, so as to make the fringes out of the light that went to the top of any one division of the spectrum, and also out of that which went near the bottom of the next, the fringes were made of two colours; one part was of the highest colour, and the other of the lowest; but both were on the ground of the highest. Thus, if held on the confine of the green and blue, the upper half of each fringe was blue, the under green, but both parts in the blue division of the spectrum; and trying the same in all the rays, it was evident that the red was moved farther into the orange, and the orange into the yellow, than the blue was into the indigo, or the indigo into the violet. Now in Obs. III. the fringes were formed by the inflexion of one knife, and were moved into its shadow, and separated and dilated by the deflexion of the other; and this most in the red, and least in the violet. Likewise in Obs. IV. the fringes of one colour were deflected into the region of the next, and this most in the red, and least in the violet; although in both observations the violet, from the position of the chart, was farthest from the angle, and consequently, had the rays been equally deflected, the violet should have been farthest moved, and most dilated by the deflexion: but, seeing that, at equal angles of incidence in the third, and at less in the fourth Observation, the red was most, and the violet less, deflected, it is evident that the most inflexible rays are also most deflexible.

Having thus found that the parts of light differ in flexibility, I wished next to learn two things; in what proportion the angle of inflexion is to that of deflexion at equal incidences; and secondly, what proportion the different flexibilities of the different rays bear to one another. But the nature of the coloured fringes must first be understood; so that I defer this enquiry till after I have made use of the principles now laid down for the explanation of natural phenomena, and proceed in the mean time to

PART II.—*Of Reflexion.*

THAT bodies reflect light by a repulsive power extending to some distance from their surfaces, has never been denied since the time of Sir Isaac Newton †. Now this power extends

* Optics, Book iii. Obs. 8. † Ibid. Book iii. Part III. Prop. 8.

tends to a diſtance much greater than that of apparent contact, at which an attraction again begins ſtill at a diſtance, though leſs than that at which before there was a repulſion; as will appear by the following demonſtration, which occurs to me, and which is general with reſpect to the theory of Boſcovich *. In Fig. 4, let the body A have for P an attraction which at the diſtance of A P is proportional to P M; then let P move towards A, ſo as to come to the ſituation P′, and let the attraction here be P′ M′; as it is continual during the motion of P to P′, M M′ is a curve line. Now in the caſe of the attraction of bodies for light, and for one another, P M is leſs than P′ M′; and conſequently M M′ does not ever return into itſelf, and therefore it muſt go *ad infinitum*, having its arc between A B and A C, to which it approaches as aſymptotes, the abſciſſa always repreſenting the diſtance, and the ordinate the attraction at that diſtance. Let P′ now continue its motion to P″, and M′ will move M″; and if P″ meets A, or the bodies come into perfect contact, P″ M″ will be infinite; ſo that the attraction being changed into coheſion will be infinite, and the bodies inſeparable, contrary to univerſal experience; ſo that P can never come nearer to A than a given diſtance. In the caſe of gravity P M is inverſely as the ſquare of A P, ſo that the curve N M M″ is the cubic hyperbola; but the demonſtrarion holds, whatever be the proportion of the force to the diſtance. It appears then, that flexion, refraction, and reflexion, are performed by a force acting at a definite diſtance; and it is reaſonable to think even *a priori*, that, as this ſame force in other circumſtances is exerted to a different degree on the different parts of light in refracting, inflecting, and deflecting them, it ſhould alſo be exerciſed with the like variations in reflecting them. Let us attend to the proof which enables us to change conjecture into conviction.

Obſ. I. The ſun ſhining into my darkened chamber through a ſmall hole $\frac{1}{40}$th of an inch in diameter, I placed a pin of $\frac{1}{37}$th of an inch diameter in the cone of light (one half inch from the hole) inclined to the rays at an angle of about 45°; and its ſhadow was received on a chart parallel to it, at the diſtance of two feet. The ſhadow was ſurrounded by the three fringes on each ſide, diſcovered by Grimaldo; beyond theſe there were two ſtreaks of white light diverging from the ſhadow, and mottled with bright colours, very irregularly ſcattered up and down; but on uſing another pin, whoſe ſurface was well poliſhed, and placing it nearer the hole than before, the colours in the ſtreaks became much brighter (and the ſtreaks themſelves narrower), being extended from one ſide to the other, ſo that, except in a very few points here and there, no white was now to be ſeen; and, on moving the pin, the colours moved alſo. But they diſappeared if the pin was deprived of its poliſh by being held in the flame of a candle, or if a roll of paper was uſed inſtead of the pin; alſo they were much brighter in direct than in reflected light, and in the light of the ſun at the focus of a lens, than in his direct unrefracted light. Placing a piece of paper round the hole in the window-ſhut, I obſerved the colours continued there; and inclining the chart to the point where they left off, I ſaw them continued on it and then proceed as before to the ſhadow. If the pin was held horizontally, or nearly ſo, they were ſeen of a great ſize on the floor the walls and roof of the room, forming a large circle; and if the chart was laid horizontally, and the pin held between the hole and it, in a vertical poſition, the circle was ſeen on the chart, and became an oval by inclining the pin a little to the horizon.

* Nova Theoria Philoſophiæ Naturalis.

Obf. II. Having produced a clear fet of colours, as in the laft Obfervation, I viewed them as attentively as poffible, and found that they were divided into fets, fometimes feparated by a gleam of white light, fometimes by a line of fhadow, and fometimes contiguous, or even running a little into one another. They were fpectra or images of the fun, for they varied with the luminous body by whofe rays they were formed, and with the fize of the beam in which the pin was held; and when by placing it between my eye and the candle a little to one fide I let the colours fall on my retina, I plainly faw that they refembled the candle in fhape and fize (though a little diftended), and alfo in motion, fince, if the flame was blown upon, they had the like agitation. The colours, therefore, which fell on the chart were images of the fun: they had parallel fides pretty diftinctly defined, but the ends were confufed and femicircular, like thofe of the prifmatic fpectrum. Like it, too, they were oblong, and in fome the length exceeded the breadth fix, even eight, times. The breadth was, as I found by meafurement, exactly equal to that of the fun's image received on a chart as far from the pin as the image was, and the length was always to the breadth at all diftances in the fame ratio, but not in all pofitions of the pin; for, if it was moved on its axis, the images moved towards the fhadow on one fide, and from it on the other, becoming longer and longer (the breadth remaining the fame) the nearer they came to the fhadow on the one fide, and fhorter in the fame proportion the farther they went from it on the other.

Obf. III. Having picked out an image that appeared very bright and well defined, I let it through a hole with moveable fides in the upper part of a fort of defk, which moved to any opening by hinges, and had a chart for its under fide, on which the image fell, and I fhut the hole fo clofe as to prevent any of the others from coming through. I then had a full opportunity of examining it in all refpects, and I counted in it diftinctly the feven prifmatic colours; the red was fartheft from the fhadow of the pin, and from the pin itfelf; then the orange; then the yellow, green, blue, and indigo; and the violet neareft of all: in fhort, it was exactly fimilar to a prifmatic fpectrum much diminifhed in length and breadth, and turned horizontally on the wall oppofite to the prifm, with the red fartheft away. In figure 5, *s e* is the pin reflecting the rays C P and C O, which pafs through P O the hole in the defk E D, to the chart or bottom of the defk R T S D; and from there the fpectrum I K divided into its colours, I being violet, and K red. On moving the hole in the defk, and letting through other images, the colours were not in all arranged the fame way: but I moved the pin on its axis, and obferved thofe where the order was inverted, to move, not only with refpect to the pin, but alfo with refpect to the contiguous images; and I was furprifed to fee them affume the order of colours firft mentioned, namely the red outermoft, and the violet innermoft. In like manner, the images which before the motion were regular, on moving into the places left by the others had always the order of their colours inverted, fo that the thing muft be owing to fome irregularities in the pin's furface; for thofe which were made by a fmall glafs tube filled with quickfilver, and freed from fcratches by a blow-pipe, preferved during the motion the proper order of colours. Another irregularity in the arrangement was alfo obfervable even in the glafs tube; for two contiguous images, by mixing one with another for two or three fucceffions, appeared each to have outermoft a dull colour between red and violet, and innermoft a green: but here

unlefs

unlefs the fucceffion continued through all the images, the outermoft of all was red, and the innermoft image had univerfally violet in the infide.

Obf. IV I placed at a hole in the window-fhut a prifm to refract the rays, and received the fpectrum at the diftance of fix feet from the window on a chart; then at the diftance of two feet I placed a fcreen with a hole in the middle of it, through which I let pafs fucceffively the different rays. At the diftance of one inch from the hole, between it and the chart, I placed the reflecting cylindrical body; the images were found on the chart and walls of the room round to the fides of the hole on the fcreen, and were always wholly of the colour in which they were formed, except in the confines of the green, where a fmall quantity of white light fell, and made them of all the feven colours; but this was almoft wholly prevented by ufing a prifm with a greater refracting angle, and holding the pin and fcreen farther from it. I then removed the fcreen, and left the reflector in its place, fo as it might reach through the rays; and thus there were formed images having in them, from top to bottom, the feven colours, one after another, the loweft divifion being red, the higheft violet. They were inclined confiderably towards their tops, and were much broader at the bottom or red parts than at the tops or violet parts. And laftly, the reflector being moved fo that the images might be difturbed (as in the former experiment made in the white light), the red was moft, the violet leaft dilated. In cafe thefe effects might be owing to any peculiarities in the fhape or pofition of the reflector, I placed at three feet from the prifm a lens of four inches breadth, to collect the rays to a focus, fix feet beyond which I held a chart, and there received the fpectrum inverted, the red being uppermoft, and the violet undermoft; holding the reflector at two feet from the focus and four from the chart, the images were formed juft as before, only inverted, inclining towards the violet, of greater breadth towards the red, and more diftended towards the fame quarter when the reflector was moved.

Obf. V. Things remaining as in the laft part of the laft Experiment; at the focus of the lens I placed a fecond prifm, which refracted the rays into a white beam *, and this I received on a fcreen with a hole in the middle, through which a fmall part of it paffed, and falling on the reflector placed behind was formed by it into images after the manner of the firft Experiment, each having in regular order the feven prifmatic colours. One of the brighteft and moft diftinct I let pafs through a hole in the fecond fcreen, and it fell on the chart. I then caufed an affiftant to intercept the red rays between the firft prifm and the lens, and immediately the red part of the image vanifhed; and when the violet was intercepted, the violet of the image vanifhed; and if the green was intercepted, the green was wanting in the image. In fhort, whatever colours were ftopped, the fame were miffing in the image. In Fig. 6. the rays paffing through the hole C of the window, A B are refracted by the prifm P M N, and feparated into D V, D G, and D R, violet, green and red; which, being collected into a focus F by the lens L, are there again refracted by a prifm P′ M′N′, and formed into a white beam a b m n, part of which is intercepted by the fcreen S S′, and part paffes through the hole h, as h H to H on the chart X Y Z W, and part is reflected by the body *o q* into a fet of images which are received on a fcreen T U, and one of them, r g v, let pafs to W X Y Z; but when an obftacle E ftops D R, r the red vanifhes;

* Optics, Book ii. Part II. Prop. 2.

nishes ; and if D G be ftopped, g the green vanishes ; and if D V be ftopped, v difappears. Laftly, if D R and D G be ftopped, g and r vanifh.

Obf. VI. Having produced a fet of bright images, I let one pafs through the defk defcribed in the third Experiment, and received it on a fmall lens $\frac{1}{2}$ inch broad to collect the rays into a focus, which I received on the chart by moving it a little on its hinge ; and by all the obfervations I could make, and all the tefts I could think of, it was white inclining to yellow, and of the fame nature and conftitution with the fun's direct light : but if any ray was ftopped before coming to the lens, the focus was a mixture of the remaining rays ; and the chart being moved a little farther round, the image was formed on it, the colours being in an inverted order. At the focus I held a reflector, and there were formed images of all the feven colours, as in the fun's direct light (Exp. 1.) ; if the light was fufficiently ftrong, and the defk near the window-fhut hole, one of thefe could even be collected by a fecond lens into a white focus. This experiment is rendered more uniform by fubftituting for the lens a concave metallic mirror, and placing at the focus another mirror to reduce the rays into a beam which may be made of any compofition we pleafe by ftopping one or more of the colours at the hole in the defk. I obferved in the courfe of thefe experiments a phenomenon worth mentioning; if a comb (as in Newton's experiment *) be very fwiftly moved before one of the images or more, a fenfation of white is produced : but this is ftill more evident if the pin be fwiftly moved round its axis ; for then the images move alfo, and, running into one another, caufe a fenfation of perfect whitenefs.

Obf. VII. I let an image through the hole in the defk, and viewed it through a glafs prifm, holding its axis parallel to the fides of the image, and its refracting angle upwards. I found that if the image was bright, and free from white light, the colours were not changed by the refraction ; but if it was mixed and diluted with white, the prifm, decompounding the white, caufed the image to appear violet at one fide, and red at the other ; yet ftill this only confufed the colours of the image without changing them. Farther, if the prifm was moved on its axis, the violet was lifted higher than the red or any of the other colours. Nor was the conftitution of the colours at all changed by reflexion from a pin or mirror, except in fo far as they were mixed by a concave one, as mentioned in the laft experiment. If a pin was held behind the hole to reflect the colours, it formed other images of the colour in which it was held, and as far as I could judge threw the red to the greateft diftance and breadth and inclination. Nor were the colours of the image changed by reflexion from natural bodies, for thefe were all of the colours in which they were held, but brighteft in that which they were difpofed to reflect moft copioufly. Likewife the rings of colour made by thin plates were broadeft in the red, and narroweft in the violet ; and the like happened to the fringes that furround the fhadows of bodies. Laftly, the fhadows of bodies were themfelves broadeft in the violet, and narroweft in the red.

Obf. VIII. I filled with water a glafs tube, whofe diameter was $\frac{1}{4}$th of an inch, and confequently the radius of curvature $\frac{1}{8}$th, and whofe fides were $\frac{1}{30}$th of an inch thick ; then ftanding at four feet from a candle I held the tube $\frac{1}{4}$th of an inch from my eye, fo that the light of the candle might be refracted through it, and moved my eye-lids clofe enough to prevent the extraneous fcattered light from entering along with that which was regularly

* Optics, Book i., Part II. Prop. 5.

refracted.

refracted. I ſaw ſeveral images of the candle, all highly coloured, and the colours were, in order from the candle outwards, red, orange, and ſo on to violet. I then filled the tube with clear diluted ſulphuric acid, and dropped a ſmall piece of chalk to the bottom, when immediately an effervеſcence took place by the eſcape of fixed air, which roſe in bubbles through the tube; and looking at the candle through one of theſe, I ſaw the images formed with the colours ſtill in the ſame order, but a little larger than before.

We are now to ſee to what concluſions theſe experiments lead us. The firſt experiment ſhews, that all ſorts of lights, whether direct or reflected or refracted, produce colours by reflexion from a curve ſurface. From the ſecond we learn that theſe colours are diſtinct images or ſpectra of the luminous body, much dilated in length, but not at all in breadth, and that the angle of incidence being changed, the dilatation of the images is alſo changed; and from the third experiment it appears, that each full image is compoſed of ſeven colours, red, orange, yellow, green, blue, indigo, and violet; and that the proper order is red outermoſt, and violet innermoſt, the reſt being in their order. The fourth experiment ſhews, that theſe images are produced, not by any accidental or new modification impreſſed on the rays, but by the white light being decompoſed by reflexion; that the mean rays, or thoſe at the confine of the green and blue, are reflected at an angle equal to that of incidence, and the red at a leſs, the violet at a greater angle. Experiments 5th and 6th prove, beyond a doubt, the decompoſition and ſeparation of the rays by reflexion; for in both we ſee that the colours in the images are thoſe, and thoſe only, which were mixed in the ray by reflexion or refraction before and at incidence, whilſt the 6th is (in addition) a proof that all the rays of any one image, if mixed together, compound a beam exactly ſimilar to the beam that was at firſt decompounded. The ſeventh experiment ſhews, that the colours into which the rays are ſeparated by reflexion are homogeneous and unchangeable; that they differ in flexibility and refrangibility; that they bear the ſame part in forming images by reflexion, and fringes by flexion, and colours from thin plates, which the rays ſeparated by the priſm do. And in the 8th experiment we ſee, that when the rays are placed in the ſame ſituation with reſpect to refraction, whether out of a rarer into a denſer, or a denſer into a rarer medium, in which they before were with reſpect to reflexion, the poſition of the colours produced is diametrically oppoſite in the two caſes. Seeing, then, that in all ſorts of light, direct, refracted, reflected, ſimple and homogeneous or heterogeneous, and compounded, and in whatever way the ſeparation and mixture may have been made, ſome of the rays, at equal or the ſame incidences, are conſtantly reflected nearer the perpendicular than the mean rays, and others not ſo near; and ſeeing that by ſuch reflexion the compound ray of whatever kind is ſeparated into parts ſo ſimple that they can never more be changed; and conſidering the different places to which theſe parts are reflected; it is evident, that the ſun's light conſiſts of parts different in reflexibility, and that thoſe which are leaſt refrangible are moſt reflexible. By reflexibility I here mean a diſpoſition to be reflected near to the perpendicular in any degree.

Although I have given what I take to be ſufficient proof of this property of light, yet I am aware that ſomething more is requiſite. It will be aſked, why does neither a plane, a common convex, nor a common concave mirror ſeparate the rays by reflexion? This is what has always hindered us from even ſuſpecting ſuch a thing as different reflexibility. I ſhall, however, take an opportunity of removing this obſtacle in the ſecond part of the plan,

when

when I come to explain the reafon of the colours made by the reflecting body, and the manner of their formation. At prefent I fhall only caution thofe who may wifh to repeat the above experiments, that the hole in the window-fhut muft be fmall, the room quite dark, the pin well polifhed, and the defk, chart, &c. placed at a diftance from the pin not greater than three feet, otherwife the images will be dilute and dim; nor, on the other hand, lefs than fix inches, otherwife they will be too fhort, and the colours not far enough feparated one from another.

My next object of enquiry was the different degrees of reflexibility belonging to each ray. It appears, not only from mathematical confiderations fufficiently obvious, but alfo from the experiments I have related, that though the different rays have, at the fame or equal incidences, different angles of reflexion, yet each ray is conftant to itfelf in degree of reflexibility, and that its fine of reflexion bears always the fame ratio to its fine of incidence. The queftion then is, what are the fines of reflexion of the different rays, the fine of incidence being the fame to all?

Obf. IX. In fummer, at noon, when the fun's light was exceedingly ftrong, and there was not the veftige of a cloud in the fky, I produced an uncommonly fine fet of images by fixing, at an inch from the fmall hole $\frac{1}{30}$th of an inch diameter, a pin $\frac{1}{25}$th of an inch diameter. One of the brighteft of thefe I let pafs through the defk to the chart below at $2\frac{1}{2}$ feet from the pin, and the image was three inches from the fhadow, in a ftraight line. I delineated it carefully by drawing two parallel lines for the fides, and marking the femicircular ends. Then with the point of a fmall needle I marked the confines of the contiguous colours on one of the parallel fides, and afterwards drew acrofs the image parallel lines. This operation I repeated with the fame and different images at many diftances from the pin, and on different days, with various forts of pins and fizes of holes, &c. &c. and all thefe repetitions were made before I once examined the refult of any one meafurement, that I might be unprejudiced in trying the thing over again. I then compared the fketches of divided images which I thus obtained, and found fufficient reafon to conclude, that the differences between the fines of reflexion in the different rays were in the harmonical order. For the divifions were nearly as $\frac{1}{9}, \frac{1}{18}, \frac{1}{12}, \frac{1}{12}, \frac{1}{15}, \frac{3}{80}, \frac{1}{18}$, which, when compounded with the fcale, gave $1, \frac{15}{16}, \frac{9}{10}, \frac{5}{6}, \frac{3}{4}, \frac{2}{3}, \frac{11}{18}, \frac{1}{2}$; and thefe are exactly the change of the notes in an octave, obtained by taking the fums of the octave, and a fecond major, a third major, a fourth, a fifth, a fixth major, a feventh major, and an eighth, inftead of the difference between a double octave and a fecond major, a third major, and fo on. Thus the fpectrum by reflexion is divided exactly as the fpectrum by refraction, only that the former is inverted, and the different rays have reflexibilities, that are inverfely as their refrangibilities. Having fettled this (I flatter myfelf) curious and important point, I proceeded next to enquire into the abfolute reflexibility of the extreme colours; for if this be known, the angle of incidence being given, the angle of reflexion of all the different rays may be found. For obtaining a folution of this problem I made the following experiment:

Obf. X. The fun fhining ftrongly through the fmall hole in the window-fhut, and the rays diverging into a cone whofe bafe fell on an horizontal chart $2\frac{1}{2}$ feet from the hole, between the hole and chart I placed a fcreen which had a plate and fmall hole in it. The rays paffing through this fell on a fmall pin, fo placed that the images formed might

3 be

be at right angles to the fhadow; one of thefe I meafured together with its diftance from the fhadow, the diftance of the fhadow from the hole, the breadth of the fhadow, and the diameter of the pin: thefe meafures were as follows. In fig. 7, C is the centre, and B *e n* the circumference of the pin, G M the chart, and G D a line in it, being the axis of all the images, at right angles to C D, the diftance of C from D the centre of the fhadow, and alfo to the fhadow itfelf; G E is the parallel fide of the image, G being red, E violet, and F the confine of the green and blue. C*e* is a radius parallel to E D, and C A another drawn through B, the point where O B is incident at the angle O B A, to which (by what was before fhown) A B F is equal. By meafurement G E is $\frac{1}{4}$th of an inch, C B $\frac{1}{50}$th, C D $4\frac{1}{2}$; now the fhadow being leffened by a penumbra, this added to half the fhadow, and their fum to the diftance between the penumbra and the violet, give E D $\frac{41}{7}$ths of an inch. From whence it is eafy to calculate, that the angle of incidence being 77° 20′, the angle of the red's reflection A B G is 75° 50′ and that of the violets 78° 51′. Now the natural fines of 77° 20′, 75° 50′, and 78° 51′, are as 9756, 9695, and 9811; or as 250, 248, and 251, which are very nearly as $77\frac{1}{2}$, 77, and 78; and making an allowance for the omiffions made in the reductions, the errors in the operations and meafurements, they may be accounted as accurately in the above proportion. Now their extremes 77 and 78 are the very proportions of the red's refrangibility to the violet's*. So that the reflexibility of the red is to that of the violets as the refrangibilities inverfely. But it is obvious that the fine of incidence is not the fame in the two cafes; for in the one it is equal to that of the mean rays reflection; while in the other none of the rays are refracted at an angle equal to that of incidence, otherwife they would not be refracted at all. This, however, being a confequence of the effential diftinction in the circumftances, does not impair the beautiful analogy which we have feen is preferved in the two operations, and which proves them to be different exertions of the fame power. Now we may find, from the data obtained, the fines of all the rays in the fpectrum by adding to 77, the lengths of the fpaces into which it is divided, and which are, without any fenfible error, as the differences of thofe fines. The fines of the red will be from 77 to $77\frac{1}{8}$; the orange from $77\frac{1}{8}$ to $77\frac{1}{5}$; the yellow from $77\frac{1}{5}$ to $77\frac{1}{3}$; the green from $77\frac{1}{3}$ to $77\frac{1}{2}$; the blue from $77\frac{1}{2}$ to $77\frac{2}{3}$; the indigo from $77\frac{2}{3}$ to $77\frac{7}{9}$; the violet from $77\frac{7}{9}$ to 78. So that, the fine of incidence being given, that of the reflexion of all the different rays may be found; and the angle of incidence being 50° 48′, the angles of reflexion are as follows: Of the extreme red 50° 21′; of the orange 50° 27′; of the yellow 50° 32′; of the green 50° 39′; of the blue 50° 48′; of the indigo 50° 57′; of the violet 51° 3′; and of the extreme violet 51° 15′.

I fhall conclude this part of the fubject with a few remarks on the phyfical caufe of reflexibility. As light is reflected by a power extending to fome diftance from the reflecting furface, the different reflexibility of its parts arifes from a conftitutional difpofition of thefe to be acted upon differently by the power. And as thefe parts are of different fizes, thofe which are largeft will be acted upon moft ftrongly. I fhall not hefitate to go a ftep farther. In fig. 8, let EC be the reflecting furface, DH the perpendicular, and AB a ray incident at B, and produced to F, and reflected into GB; draw GH parallel to FB, and GF to HB; then HB : (HG :) : BF : : fin. HGB : fin. HBG or : : fin. GBF : fin. HBG. But GBF is

* Optics, Book i. Part I. Prop. 7.

the

the fupplement of GBA, the fum of the angles of reflexion and incidence; wherefore HB : BF :: the fine of the fum of the angles of reflexion and incidence, to the fine of the angle of reflexion; fo that if I be the angle of incidence, R that of reflexion, V the velocity of light, and F the reflecting force; $F = \frac{V \times \text{fin.}\,(R+I)}{\text{fin. R.}}$. By accommodating this formula to the different cafes, we obtain F in all the rays; and the ratio of F in one fet to F in another being required, we have (by ftriking out V, which is conftant) $F : F' :: \frac{\text{fin.}\,(R+I)}{\text{fin. R}} : \frac{\text{fin.}\,(R'+I')}{\text{fin. R}'}$. Suppofe we would know F and F′ in the red and violet refpectively; $I = 50^\circ\ 48' - R = 50^\circ\ 21'$, and $R' = 51^\circ\ 15'$; then $F : F' :: \frac{\text{fin. } 101^\circ 9'}{\text{fin. } 50^\circ 21'} : \frac{\text{fin. } 102^\circ 3'}{\text{fin. } 51^\circ 15'}$ Performing the divifion in each by logarithms, and finding the natural fines correfponding to the quotients; F : F′:: 1275 : 1253. But the force exerted on the red is to that exerted on the violet as the fize of the red to the fize of the violet (by hypothefis); therefore the red particles are to the violet as 1275 to 1253. This may be extended to all the other colours by fimilar calculations; their fizes lying between 1275 and 1253, which are the extreme red and extreme violet; thus the red will be from 1275 to $1272\frac{1}{2}$; the orange from $1272\frac{1}{2}$ to 1270; the yellow from 1270 to 1267; the green from 1267 to 1264; the blue from 1264 to 1260; the indigo from 1260 to 1258; and the violet from 1258 to 1253.

All this follows mathematically, on the fuppofition that the parts of light are acted upon in proportion to their fizes: and to fay the truth, I fee no other proportion in which we can reafonably fuppofe them to be influenced; for fuch an action is not only conformable to the univerfal laws of attraction and repulfion, but alfo to the following arguments. If the action be not in the fimple ratio, it muft either be in a lower or in a higher. Let it be in a lower, as that of the fquare root, then the fize of the red would be to the fize of the violet as the fquares of the forces; that is, as 1625625 to 1572009: a difference evidently too great; and *à fortiori* of the cube or any other root. On the other hand, if the action were in a higher ratio, as that of the fquare, then the particles would be as the fquare roots of the forces, or nearly as 35.70 to 35.39, a difference evidently too fmall; for if the fize of the red particles were only $\frac{3}{10}$ths the greater than that of the violet, and the velocity of both were equal, the momentum and confequently the intenfity of the red could not fo much exceed that of the violet as we find it does; and as feems to me to be proved by the experiment of Buffon (on accidental colours), who found, that after looking at a white object, when he fhut his eyes, it firft became violet, then blue or a mixture of blue and the other colours, and laft of all red: fo in the impreffion of the white, compounded of the impreffions of all the other rays mixed together, the violet was firft obliterated or weakeft, and the red laft or ftrongeft. To this reafoning on the intenfity of the particles as owing to their fize, I fee only two objections that can be made. The one is, that the intenfity is increafed when the rays are thrown into a focus: but we muft recollect, that the rays in this cafe are mixed, and their particles fo blended as to be increafed in fize; for the number of feparate rays thrown into one place will not increafe their intenfity fenfibly. The other objection is that paffage in Newton, where he fays, " that the orange and yellow are the moft luminous of all the colours, affecting the senses most strongly *." Now, besides that this is an assertion opposed by the positive experiment just now quoted, I think an answer may be thus made to it: The white light, from which the spectrum is never free, which inclines to yellow, and which is composed also of red, abounds in the yellow and orange of the spectrum; so that both of these colours derive their superior lustre rather than intensity from this circumstance; or, if they have any degree of the latter more than the red, it is in fact owing to their mixture with the red and the other rays which are all in the white.

[*To be continued.*]

IX.

An Account of the Manner in which Heat is propagated in Fluids, and its general Consequences in the Economy of the Universe. By BENJAMIN *Count of* RUMFORD.

[Concluded from p. 348.]

IN order to ascertain the action of water upon ice retained beneath it, Count Rumford took a cylindrical glass jar, 4,7 inches in diameter, and 13,8 inches high. Into this he put 43,87 cubic inches, or 1 lb. $11\frac{1}{7}$ oz. troy of water. When the jar had been placed in a freezing mixture, and the water was congealed, the ice adhered firmly to the bottom and sides of the jar, and was just three inches high. The jar was then placed in a mixture of pounded ice and pure water, and kept in that situation for four hours, in order that the cake of ice might be brought to the temperatnre of 32 degrees.

The jar still standing in a shallow dish in the pounded ice and water, the surface of which cold mixture was just on a level with the surface of the ice in the jar, $73\frac{1}{4}$ oz. troy of boiling-hot water were gently poured in, which filled it to the height of eight inches above the surface of the jar.

In these experiments it was evidently of the greatest consequence to prevent those irregularities which would arise from the action of pouring. The expedient first adopted for this purpose was to cover the ice with a circular piece of strong paper, which was gently removed after the pouring. But this not being thought sufficiently effectual, a flat shallow dish of light wood, half an inch deep, and somewhat less externally than the diameter of the jar, was provided. Its bottom was about a quarter of an inch thick, and was perforated with a great number of small holes, which gave it the appearance of a sieve. This perforated wooden dish, having been previously made ice-cold, was placed on the surface of the ice in the jar, and the hot water was gently poured into the dish through a long wooden tube. As the perforated dish floated and remained constantly at the surface of the water, and as the water passing through many hundreds of small holes was not projected downwards with force, the violent motions in the mass of water in the jar were thus in a great measure prevented. The water was not suffered to issue from the wooden tube in a perpendicular stream; but, the bore being closed, it was made to issue horizontally through a number of small holes in the sides of the tube at its lower end.

* Optics, Book i. Part I. Prop. 7.

As ſoon as the operation of pouring the hot water into the jar was finiſhed, the perforated diſh was carefully removed, and the jar was covered with a circular wooden cover, from the centre of which a ſmall mercurial thermometer was ſuſpended. The irregularities of the experiments were greatly diminiſhed by the expedient of the wooden diſh. One more improvement however was added to the manipulation. It conſiſted in pouring $8\frac{1}{4}$ cubic inches of ice-cold water upon the ice in the jar, previous to the introduction of the wooden diſh. This water covered the ſurface to the height of 0,478 of an inch.

Much as I could have wiſhed to relate the whole of the intereſting experiments in this eſſay, the requiſite attention to brevity in this abridgment has made it neceſſary to omit every matter of ſubordinate detail. For the ſame reaſon I forbear to ſtate the reſults afforded by the two firſt methods of trial. Thoſe with the complete apparatus were as follows; the temperature of the room being 41 degrees.

No. of the Experiment.	Time the hot Water was on the Ice.	Temperature of the hot Water one inch below its ſurface.		Quantity of Ice melted.
		At the Beginning.	At the End.	
	Minutes.			Grains.
No. 25	10	192°	182°	580
26	30	190	165	914
27	180	190	95	3200

From the reſults of theſe three experiments, the Count proceeds to determine how much ice was melted *in the act of pouring the water into the jar*, and conſequently the rate at which it was melted in the ordinary courſe of the experiment; ſuppoſing equal quantities to be melted in equal times. I give the reaſoning in his own words.

As in the 27th experiment 3200 grains were melted in 180 minutes, and in the 25th experiment 580 grains were melted in 10 minutes, we may ſafely conclude that the ſame quantity muſt have been melted in the ſame time (10 minutes) in the 27th experiment. If therefore from 3200 grains, the quantity melted in 180 minutes in this laſt experiment, we deduct 580 grains for the quantity melted during the firſt ten minutes, there will remain 2620 grains for the quantity melted in the ſucceeding 170 minutes, when, the motions occaſioned in the water on its being poured into the jar having ſubſided, we may ſuppoſe the proceſs of melting the ice to have gone on regularly.

But if in the regular courſe of the experiment no more than 2620 grains were melted in 170 minutes, it is evident that not more than 154 grains could have been melted in the ordinary courſe of the proceſs in ten minutes; for 170 minutes : 2620 grains : : 10 minutes : 154 grains. If therefore from 580 grains, the quantity of ice actually melted in ten minutes in the 25th experiment, we deduct 154 grains, there remains 426 for the quantity melted in pouring the water into the jar.

Let us ſee now how far this agrees with the reſult of the 26th experiment. In this experiment 914 grains of ice were melted in 30 minutes. If from this quantity we deduct 426 grains, the quantity which, according to the foregoing computation, muſt have been melted *in pouring the hot water into the jar*, there will remain 478 grains for the quantity melted in the ordinary courfe of the procefs in 30 minutes, which gives 159 grains for the quantity melted in ten minutes; which differs very little from the refult of the foregoing computation, by which it appeared to be = 154 grains. This difference, however, fmall as it is, is fufficient to prove an important fact, namely, that the effects produced by the motion into which the hot water had been thrown on being poured into the jar, had not ceafed entirely in 10 minutes, or when an end was put to the 5th experiment. We fhall therefore come nearer the truth, if, in our endeavours to difcover the quantity of ice melted in any given time in the ordinary courfe of the experiment, we found our computation on the refults of the two experiments, No. 26 and No. 27.

In the latter of thefe experiments 3200 grains of ice were melted in 180 minutes, and in the former 914 grains were melted in 30 minutes. If, therefore, from 3200 grains, the quantity melted in 180 minutes, we take the quantity melted in the firft 30 minutes = 914 grains, there will remain 2286 grains for the quantity melted in the fucceeding 150 minutes, and this gives 152 grains for the quantity melted in 60 minutes. By the former computation it turned out to be 154 grains.

But if 152 grains of ice is the quantity melted in 10 minutes in the ordinary courfe of the procefs, three times that quantity or 456 grains only would have been melted in this manner in the 30 minutes during which the 26th experiment lafted; and deducting this quantity from 914 grains, the quantity actually melted in that experiment, the remainder 458 grains fhews how much muft have been melted in the pouring the hot water on the ice, or in confequence of the motion into which the water was thrown in the performance of that operation. By the preceding computation this quantity turned out to be 426 grains.

From the refult of thefe computations the Count thinks we may fafely conclude, that in the ordinary courfe of the experiments not more than 152 grains of ice were melted by the hot water in ten minutes.

He then proceeds to give an account of feveral experiments, in which the water employed to melt the ice was at a much lower temperature.

The previous congelation of water in the jar to the depth of four inches, the fubfequent reduction to the temperature of 32° by the external contact of ice and water, and the weighing of the whole jar and ice contained therein, were performed as in the former experiments. The jar, which had been wiped with a dry cold napkin before weighing, was then replaced in the ice and water to the depth of its internal contents of ice. The quantity of $73\frac{1}{4}$ ounces troy of water at the temperature of 41° (which was alfo that of the room) was then poured in with the fame precaution, of the wooden tube, difh, &c. as before. The refults of a fet of experiments were thefe :

Number of the Experiment.	Temperature of the Water in the jar one inch below its furface.		Temperature of the Air.	Time the Water remained on the Ice.	Quantity of Ice melted.
	Beginning of Experiment.	End of Experiment.		Minutes.	Grains.
No. 28	41°	40°	41°	10	203
29	41	40	41	10	220
30	41	40	41	10	237
31	41	40	41	10	228
32	41	38	41	30	617
33	41	38	41	30	585

By the refults of thefe experiments, and their correfpondence with each other, the extraordinary fact is eftablifhed, that boiling water does not fufe more water while ftanding on its furface in a given time, than water at the temperature of 41°, or only nine degrees above freezing.

Indeed, as the Count obferves, there is reafon to conclude that it does not thaw fo much. For the quantity melted in ten minutes by the hot water was 152 grains: and in the experiments with the cold water the quantity was 189½ grains. He was too much interefted in thefe refearches to leave this anomaly unexamined. It remained to be afcertained, whether the latter experiments were not affected, as well as thofe with hot water, by the agitation of the water in pouring in, and to what extent; and alfo how far the action of the external air, by cooling the outer part of the cylinder of heated water, and caufing it to defcend, might impede the rifing currents of cooled and expanded water from the furface of the ice. This laft object was numerically determined by clothing the jar with cotton wool in fome experiments, by furrounding it totally with ice and water in others, and comparing thefe refults with fuch as were afforded when all the part of the jar above the ice had been left expofed to the atmofphere. For thefe numbers, the judicious precautions of experiment, and the comparative remarks upon them, I muft again refer the philofopher to the Treatife itfelf, which every one who wifhes to inveftigate the fubject ftill farther, to repeat the experiments, or to draw more extenfive theoretical inferences from them, muft neceffarily confult. The concluding Table, drawn from the others which precede, is as follows:

		Ice melted in 30 min. Grains.
In the experiments in which the part of the jar occupied by the water was expofed uncovered to the air; then at 61°.	With boiling hot water - - -	558⅖
	With water at the temperature 61° -	646
	With water at the temperature 41° -	574
In the experiments in which the part of the jar occupied by the water was furrounded by pounded ice and water, and confequently was at 32°.	With boiling hot water - - -	399⅗
	With water at the temperature 61° -	661
	With water at the temperature 41° -	542

The Count confiders thefe experiments as the moft unqueftionable proof that water is a perfect *non-conductor of heat*, and that heat is propagated in it only in confequence of the motions which the heat occafions in the infulated and folitary particles of that fluid. He remarks, that this difcovery affords an infight into the nature of the mechanical procefs which takes place in chemical folutions; and he thinks that it will enable us to account in a fatisfactory manner for all the various phenomena of chemical affinity, vegetation, and perhaps all the other motions among the inanimate bodies on the furface of the globe.

But without dwelling upon thefe circumftances, lefs immediately deducible from the nature of the communication of heat through water, the author haftens to apply his difcoveries to the great operations which are regulated and performed on the furface of the globe by virtue of its moft imperfect conducting power, and the law of its expanfion and contraction, from the changes of temperature. This fubject occupies the third and concluding chapter of his effay. I fhall give the fubftance as nearly in the words of the author as the purpofes of abridgment will allow.

Though

Though ſummer and winter, ſpring and autumn, and all the variety of the ſeaſons, are produced by the ſimple and admirable contrivance of the inclination of the axis of the earth to the plane of the ecliptic ; yet this mechanical diſpoſition would not have been alone ſufficient to produce that gradual change of temperature which the neceſſities of animal and vegetable life appear to require. It ſeems neceſſary not only that the changes ſhould be gradual, but that the extremes of heat and cold ſhould be mitigated by ſome equalizing power.

The cold air from the northern and ſouthern regions of the earth ruſhes towards the heated parts in the regular winds which are known to prevail on its ſurface; and the return of theſe maſſes of air, which, when they become heated, riſe and flow back in the upper departments of the atmoſphere towards their former ſtation, muſt greatly tend to preſerve a more equal temperature of climates than would elſe have prevailed. But the agency of water in producing the ſame effect has been overlooked, though the remarkable law of its condenſation by cold renders it wonderfully ſuited to that purpoſe.

M. de Luc has ſhewn, that by cooling water from the boiling point 80° to the freezing point 0, if the whole contraction be divided into 80 equal portions, the condenſation or loſs of bulk for each 10° will be reſpectively, beginning at 80°, as follows : 18.0 ;—16.2 ;—13.8;—11.5;—9.3;—7.1;—3.9 ;—and 0.2. Whence it is ſeen, that the increaſe of ſpecific gravity, by cooling when near the mean temperature in England, is at leaſt ninety times leſs than when the water is near boiling hot.

The vaſt extent of the ocean, and its great depth, but ſtill more its numerous currents, and the power of water to abſorb a vaſt quantity of heat, render it peculiarly well adapted to ſerve as an equalizer of temperature.

On the retreat of the ſun after the ſolſtice, it is cloſely followed by the cold winds from the regions of eternal froſt, which are continually endeavouring to preſs on towards the equator. As the power of the ſun to warm the ſurface of the earth and the air diminiſhes very faſt in high latitudes on the days growing ſhorter, it ſoon becomes too weak to keep back the denſe atmoſphere which preſſes in from the polar regions, and the cold increaſes very faſt.

There is however a circumſtance by which theſe rapid advances of winter are in ſome meaſure moderated. The earth, but more eſpecially the water, having imbibed a vaſt quantity of heat during the long ſummer days, while they received the influence of the ſun's vivifying beams ; this heat being given off to the cold air which ruſhes in from the polar region, ſerves to warm it and ſoften it ; and conſequently to diminiſh the impetuoſity of its motion, and take off the keenneſs of its blaſt. But as the cold air ſtill continues to flow in as the ſun retires, the accumulated heat of ſummer is ſoon exhauſted, and all ſolid and fluid bodies are reduced to the temperature of freezing water. In this ſtage the cold in the atmoſphere increaſes very faſt, and would probably increaſe ſtill faſter, were it not for the vaſt quantity of heat which is communicated to the air by the watery vapours, which are firſt condenſed, and then congealed in the atmoſphere, and which afterwards fall upon the earth in the form of ſnow ; and by that ſtill larger quantity which is given off by the water in the rivers and lakes and in the ground, upon its being frozen.

But in very cold countries the ground is frozen and covered with ſnow, and all the lakes and rivers are frozen over in the very beginning of winter. The cold then firſt begins to be extreme,

extreme, and there appears to be no fource of heat left which is fufficient to moderate it in any fenfible degree.

It has been fhewn—the Count thinks he may venture to fay proved—in the moft fatisfactory manner, that liquids part with their heat only in confequence of their internal motions, and that the more rapid thefe motions are, the more rapid is the communication of the heat; that thefe motions are produced by the change in the fpecific gravity of the liquid occafioned by the change of temperature; and of courfe that they are more rapid as the fpecific gravity of the liquid is more changed by any given change of temperature.

But it has been fhown that the change in the fpecific gravity of water is extremely fmall, which takes place in any given change of temperature below the mean temperature of the atmofphere; and particularly when the temperature of the water is very near the freezing point; and hence it follows that water muft give off its heat very flowly when it is near freezing.

But this is not all. When water is cooled to within eight or nine degrees of the freezing point, it not only ceafes to be farther condenfed, but is actually expanded by farther diminutions of its heat; and this expanfion goes on, as the heat is diminifhed, as long as the water can be kept fluid; and when it is changed to ice it expands even ftill more, and the ice floats on the furface of the uncongealed part of the fluid.

It is well known that there is no communication of heat between two bodies, as long as they are both at the fame temperature; and it is likewife known that the tendency of heat to pafs from a hot body into one which is colder, with which it is in contact, is greater, as the difference is greater in the temperatures of the two bodies.

Suppofe now that a mafs of very cold air repofes on the quiet furface of a large lake of frefh water, at the temperature of 55° of Fahrenheit's thermometer. The particles of water at the furface, on giving off a part of their heat to the cold air with which they are in contact, and in confequence of this lofs of heat becoming fpecifically heavier than thofe hotter particles on which they repofe, muft of courfe defcend. This defcent of the particles which have been cooled, neceffarily forces other hotter particles to the furface; and thefe being cooled in their turns, bend their courfe downwards; and the whole mafs of water is put into motion, and continues in motion as long as the procefs of cooling goes on.

Before he proceeds to trace this operation through all its various ftages, the Count endeavours to remove an objection which may perhaps be made to his explanation of this phenomenon. As he fuppofes the mafs of air which refts on the furface of the water to be *very cold*, and as he has taken it for granted that there is no communication whatever of heat between the particles of water in contact with this very cold air, and the neighbouring warmer particles of water, it may be afked, how it happens that thefe particles at the furface are not fo much cooled as to be immediately changed to ice? To this he anfwers, that there are two caufes which confpire to prevent the *immediate* formation of ice at the furface of the water. Firft, the fpecific gravity of the particle of water at the furface being increafed at the fame moment when it parts with heat, it begins to defcend as foon as it begins to be cooled; and before the air has had time to rob it of all its heat, it efcapes and gets out of its reach: and, Secondly, air being a bad conductor of heat, it cannot receive and tranfmit, or *tranfport* it with fufficient celerity to cool the furface of water fo fuddenly as to embarrafs the motions of the particles of that liquid in the operation of giving it off.

But

But to return to our lake. As ſoon as the water in cooling has arrived at the temperature of about 40°, as at that temperature it ceaſes to be farther condenſed, its internal motion ceaſes, and thoſe of its particles which happen to be at its ſurface remain there; and after being cooled down to the freezing point they give off their latent heat, and ice begins to be formed.

As ſoon as the ſurface of the water is covered with ice, the communication of heat from the water to the atmoſphere is rendered extremely ſlow and difficult; for ice, being a bad conductor of heat, forms a very warm covering to the water; and moreover it prevents the water from being agitated by the wind. Farther, as the temperature of the ice at its lower ſurface is always very nearly the ſame as that of the particles of liquid water with which it is in contact (the warmer particles of this fluid, in conſequence of their greater ſpecific gravity, taking their places below), the communication of heat between the water and the ice is neceſſarily very ſlow on that account.

As ſoon as the upper ſurface of the ice is covered with ſnow (which commonly happens ſoon after the ice is formed), this is an additional and very powerful obſtacle to prevent the eſcape of the heat out of the water; and though the moſt intenſe cold may reign in the atmoſphere, the increaſe of the thickneſs of the ice will be very ſlow.

During this time the maſs of water which remains unfrozen will loſe *no part of its heat*; on the contrary, it will continually be receiving heat from the ground. This heat, which is accumulated in the earth during the ſummer, will not only ſerve in ſome meaſure to replace that which is communicated to the atmoſphere through the ice, and prevent its being furniſhed at the expence of the latent heat of the water in contact with its ſurface; but, when the temperature of the air is not much below that of freezing, this ſupply of heat from below will be quite ſufficient to replace that which the air carries off, and the thickneſs of the ice will not increaſe.

Whenever the temperature of the air is not actually colder than freezing water, the heat which riſes from the bottom of the lake will be all employed in melting the ice at its under ſurface, and diminiſhing its thickneſs.

It will indeed frequently happen, when the ice is very thick, and eſpecially when its upper ſurface is covered with deep ſnow, that the melting of the ice at its under ſurface will be going on, when the temperature of the atmoſphere is conſiderably below the freezing point.

As the particles of the water, which, receiving heat from the ground at the bottom of the lake, acquire a higher temperature than that of 40 degrees (and being expanded, and becoming ſpecifically lighter by this additional heat, riſe to the upper ſurface of the fluid water, and give off their ſenſible heat to the under ſurface of the ice), never return to the bottom, this communication of the heat which exhales from the earth produces very little motion in the maſs of the water; and this circumſtance is no doubt very favourable to the preſervation of the heat of the water.

When a ſtrong wind prevails, and the ſurface of the water is much agitated, ice is not formed, even though the whole maſs of water ſhould, by a long continuance of cold weather, have been previouſly cooled down to that point to which it is neceſſary that it ſhould be brought, in order that its internal motions may ceaſe, and that it may be diſpoſed to congeal. For though the particles at and near the ſurface may no longer have any tendency to descend, on being farther cooled, yet, as they have so considerable a quantity of sensible heat (eight or ten degrees) to dispose of, after their condensation by cold ceases; and as the agitation into which the water is thrown by the wind does not permit any particle to remain long enough in contact with the cold air to give off all its heat at once; there is a continual succession of fresh particles at the surface, all of which give off heat to the air, but none of them have time to be cooled sufficiently to form ice. The water will therefore lose a vast quantity of heat; and as soon as the wind ceases, if the cold should continue, ice will be formed very rapidly.

But it is not merely the agitation of the water which renders the communication of the heat very rapid; the agitation of the wind also tends to produce the same effect.

On the return of spring, the snow melting before the sun, as he advances and his rays become more powerful, all the heat which the earth exhales is employed in dissolving the ice at its under surface, while the sun on the other side acts still more powerfully to produce the same effect.

Though ice is transparent, yet it is not perfectly so; and as the light which is stopped, in its passage through it, cannot fail to generate heat *when* and *where* it is stopped or absorbed, it is by no means surprising that snow should be found to melt when exposed in the sun's rays, even when the temperature of the air in the shade is considerably below the point of freezing. Snow exposed to the sun melts long before the even surface of ice begins to be sensibly softened by its beams; and it is not till some time after all the hills are bare that the ice on the lakes and rivers breaks up.

The rays which penetrate a bank of snow, being often reflected and refracted, descend deep into it, and the heat is deposited in a place where it is not exposed to be carried off by the cold air of the atmosphere; but the rays which fall upon the horizontal and smooth surface of the ice are mostly reflected upwards into the atmosphere: and if any part of them are stopped at the surface of the ice, the heat generated by them *there* is instantaneously carried off by the cold air, and a particle of water is no sooner made fluid than it is again frozen.

Hence we see, that the snow which in cold countries covers the ice that is formed on the surface of fresh water, not only prevents the heat of the water from being carried off by the air during the winter, but also assists very powerfully in thawing the ice early in the spring.

Let us now see what the consequences would have been, had the condensation of water with cold followed the law which obtains in regard to all other fluids.

As the internal motion of the water could not have failed to continue as long as its specific gravity continued to be increased by parting with heat, ice would not have begun to be formed till the whole mass of water had arrived at the temperature of 32° of Fahrenheit's thermometer.

To see what an enormous quantity of heat would be lost when the water is deep, in consequence of its whole mass being cooled in this manner, we have only to compute how much ice this heat would melt, or how much water it would heat, from the point of freezing to that of boiling.

It has been shewn by experiments, that any given quantity of ice requires as much heat to melt it as an equal quantity of fluid water loses in cooling 140°; consequently the quantity of ice which might be melted by the heat given off by any given quantity of water in cooling any given number of degrees, is to the given quantity of water as the number of degrees which it is cooled to 140°.

Hence it follows, that when the temperature of the water is 8° above the freezing point, it gives off in cooling, down to that temperature, as much heat as would melt $\frac{8}{140}$ths or $\frac{2}{35}$ths of its weight of ice: the water therefore which is cooled from the temperature of 40° to that of 32°, if it be 35 feet deep, will give off as much heat, in being fo cooled, as would melt a covering of ice two feet thick.

But this even is not all; for, as the particles of water, on being cooled at the furface, would in confequence of the increafe of their fpecific gravity, on parting with a portion of their heat, immediately defcend to the bottom, the greateft part of the heat accumulated during the fummer in the earth on which the water repofes would be carried off and loft before the water began to freeze; and when ice was once formed, its thicknefs would increafe with great rapidity, and would continue increafing during the whole winter. And it feems very probable, that, in climates which are now temperate, the water in the large lakes would be frozen to fuch a depth in the courfe of a fevere winter, that the heat of the enfuing fummer would not be fufficient to thaw them; and fhould this once happen, the following winter could hardly fail to change the whole mafs of its waters to one folid body of ice, which never more could recover its liquid form, but muft remain immoveable till the end of time.

In the month of February, after a froft which had lafted a month, the temperature of the air being 38°, M. De Sauffure found the temperature of the water at the Lake of Geneva, at the furface, at 41°, and at the depth of 1000 feet, at 40°. Had the froft continued but a little longer, ice would have been formed; but had the conftitution of water been fuch, that the whole mafs of that fluid in the lake muft have been cooled down to the temperature of 32° before ice could have been formed, this event could not have happened till the water had given off as much heat as would be fufficient to melt a covering of ice above 57 feet thick.

This quantity of heat would be fufficient to heat to the point of boiling a quantity of ice-cold water as large as the lake, and 49 feet deep.

When we trace ftill further the aftonifhing effects which are produced in the world by the operations of that fimple law, which has been found to obtain in the condenfation of water on its being deprived of heat, we fhall find more and more reafon to admire the wifdom of the contrivance.

That high latitudes might be habitable, it was neceffary that vegetables fhould be protected from the effects of the chilling frofts of a long and fevere winter; but if it be true that watery liquids do not part with their heat but in confequence of their internal motion, and if thefe motions are occafioned merely by the change produced in the fpecific gravity of thofe particles of the liquid which receive heat, or which part with it, who does not fee how very powerfully the fudden diminution and final ceffation of the condenfation of water in cooling, as foon as its temperature approaches to the freezing point, operates to prevent the fap in vegetables from being frozen?

But if, for the purpofes of life and vegetation, it be neceffary that the ground, the rivers, the lakes, and the trees, be defended against the cold winds from the poles, it may be asked, how this inundation of cold air is to be warmed? The Count answers, By the waters of the ocean, which there is the greatest reason to think were not only designed principally for that use, but particularly prepared for it.

Sea water contains a large proportion of salt in solution; and the condensation of a saline solution on its being cooled, follows a law which is extremely different from that observed in regard to pure water, and which (as may easily be shown) renders it peculiarly well adapted for communicating heat to the cold winds which blow over its surface.

As sea water continues to be condensed as it goes on to cool, even after it has passed the point at which fresh water freezes, the particles at the surface, instead of remaining there, after the mass of the water had been cooled to about 40°, and preventing the other warmer particles below from coming in their turns, and giving off their heat to the cold air (as we have seen always when fresh or pure water is so cooled)—these cooled particles of salt water descend as soon as they have parted with their heat, and in moving downward force other warmer particles to move upwards; and in consequence of this continual succession of warm particles which come to the surface of the sea, a vast deal of heat is communicated to the air—incomparably more than could possibly be communicated to it by an equal quantity of fresh water at the same temperature, as will appear by the following computation:

Without taking into the account that very great advantage which sea water possesses over fresh water, considered as an equalizer of the temperature of the atmosphere, which arises from the comparative *lowness of the point of its congelation*; supposing even sea water to freeze at as high a temperature as fresh water, namely at 32°; and supposing (what is strictly true) that as soon as either sea water or fresh water is frozen at its surface, and this ice covered with snow, the communication of heat from the water to the atmosphere ceases almost entirely; the Count proceeds to determine how much more heat would, even on this supposition, be communicated to the air by salt water than by fresh water, after both have arrived at the temperature of 40°.

When fresh water, in cooling, has arrived at this temperature, it ceases to be farther condensed with cold, and its internal motions (which, as hath already more than once been observed, are caused solely by the changes produced in the specific gravity of its particles) ceace of course, and ice immediately begins to be formed on its surface: but as the condensation of salt water goes on as its heat goes on to be diminished, its internal motions will continue; and it is evidently impossible for ice to be formed at its surface, till the whole mass of the water has become ice cold, or till its temperature is brought down to the supposed point 32°. It would therefore give off a quantity of heat equal to 8 degrees at least of Fahrenheit's thermometer more than the fresh water would part with before ice could be formed on its surface.

To be able to form an idea of this enormous quantity of heat, we have only to recollect what has already been said, and we shall find reason to conclude that it would be sufficient to melt a covering of ice equal in thickness to $\frac{2}{35}$ths of the depth of the sea. It would therefore be sufficient in that part of the North sea (lat. 67) where Lord Mulgrave sounded at the depth of 4680 feet, to melt a cake of ice 265 feet thick.

But

But the heat evolved in the formation of each fuperficial foot of ice, would be fufficient to raife the temperature of a ftratum of incumbent air 2220 times as thick as the ice (confequently in the cafe in queftion 265 × 2220 feet or 869 miles thick) 28 degrees, or from the temperature of freezing water to that of 50° of Fahrenheit's thermometer, or to the mean annual temperature of the northern parts of Germany.

The heat given off to the air by each fuperficial foot of water in cooling *one degree*, is fufficient to heat an incumbent ftratum of air 44 times as thick as the depth of the water 10 degrees. Hence we fee how very powerfully the water of the ocean, which is never frozen over except in very high latitudes, muft contribute to warm the cold air which flows in from the polar regions.

But the ocean is not more ufeful in moderating the extreme cold of the polar regions than it is in tempering the exceffive heats of the torrid zone; and what is very remarkable, the fitnefs of the fea water to ferve this laft important purpofe, is owing to the very fame caufe which renders it fo peculiarly well adapted for communicating heat to the cold atmofphere in high latitudes, namely, *to the falt which it holds in folution.*

As the condenfation of falt water with cold continues to go on, even long after it has been cooled to the temperature at which frefh water freezes, thofe particles at the furface which are cooled by an immediate contact with the cold winds muft defcend, and take their places at the bottom of the fea, where they muft remain, till by acquiring an additional quantity of heat their fpecific gravity is again diminifhed. But this heat *they never can regain in the polar regions*; for innumerable experiments have proved beyond all poffibility of doubt, that there is no *principle of heat* in the interior parts of the globe, which by exhaling through the bottom of the ocean could communicate heat to the water which refts upon it.

It has been found that the temperature of the earth, at great depths under the furface, is different in different latitudes, and there is no doubt but this is alfo the cafe with refpect to the temperature at the bottom of the fea, in as far as it is not influenced by the currents which flow over it; and this proves to a demonftration, that the heat which we find to exift without any fenfible change during fummer and winter at great depths, is owing to the action of the fun, and not to central fires as fome have too haftily concluded.

But if the water of the ocean, which, on being deprived of a great part of its heat by cold winds, defcends to the bottom of the fea, cannot be warmed *where it defcends*, as its fpecific gravity is greater than that of water at the fame depth in warmer latitudes, it will immediately begin to fpread on the bottom of the fea, and to flow towards the equator; and this muft neceffarily produce a current at the furface in an oppofite direction. There are the moft indubitable proofs of the exiftence of both thefe currents.

The proof of the exiftence of one of them would, indeed, have been quite fufficient to have proved the exiftence of both; for one of them could not poffibly exift without the other: but they are feveral direct proofs of the exiftence of each of them.

What has been called the gulph ftream in the Atlantic Ocean, is no more than one of thefe currents, namely that at the furface, which moves from the equator towards the north pole, modified by the trade winds, and by the form of the continent of North America; and the progrefs of the lower current may be confidered as proved directly by the cold which has been found to exift in the fea at great depths in warm latitudes; a degree of temperature much below the mean annual temperature of the earth in the latitudes where it has been found, and which of courfe muft have been brought from colder latitudes.

The mean annual temperature in the latitude of 67° has been determined by Mr. Kirwan, in his excellent treatife on the temperature of different latitudes, to be 39°; but Lord Mulgrave found on the 20th of June, when the temperature of the air was 48½°, that the temperature of the fea at the depth of 4680 feet was 6 degrees below freezing, or 26° of Fahrenheit's thermometer.

On the 31ft of Auguft in the latitude of 69°, where the annual temperature is about 38°, the temperature of the fea at the depth of 4038 feet was 32°; the temperature of the atmofphere (and probably that of the water at the furface of the fea) being at the fame time at 59½.

But a ftill more ftriking and incontrovertible proof of the exiftence of currents of cold water at the bottom of the fea, fetting from the poles towards the equator, is the very remarkable difference that has been found to fubfift between the temperature of the fea at the furface, and at great depth at the tropic, though the temperature of the atmofphere there is fo conftant, that the greateft changes produced in it by the feafons feldom amount to more than five or fix degrees; yet the difference between the heat of the water at the furface of the fea, and that at the depth of 3600 feet, has been found to amount to no lefs than 31 degrees; the temperature above, or at the furface, being 84, and at the given depth below, no more than 53 *.

It appears to the Count to be extremely difficult, if not quite impoffible, to account for this degree of cold at the bottom of the fea in the torrid zone, on any other fuppofition than that of cold currents from the poles; and the utility of thefe currents in tempering the exceffive heats of thofe climates is too evident to require any illuftration.

Thefe currents are produced, as we have already feen, in confequence of the difference in the fpecific gravity of the fea water at different temperatures: their velocities muft therefore be in proportion to the change produced in the fpecific gravity of water by any given change of temperature; and hence we fee how much greater they muft be in falt water than they could poffibly have been had the ocean been compofed of frefh water.

It is not a little remarkable, that the water of all great lakes is frefh, and nearly fo in all inland feas (like the Baltic) in cold climates, which communicate with the ocean by narrow channels. We fhall find reafon to conclude, that this did not happen without defign, when we confider what confequences would probably enfue, fhould the waters of a large lake in an inland fituation in a cold country (fuch as the lake Superior for inftance in North America) become as falt as the fea.

Though the cold winds which blow over the lake in the beginning of winter would be more warmed, and the temperature of the air on the fide of the lake oppofite to the quarter from whence thefe winds arrive, would be rendered fomewhat milder than it now is; yet, as the water of the lake would give off an immenfe quantity of heat before a covering of ice could be formed on its furface for its protection, it would on the return of fpring be found to be extremely cold; and as it would require a long time to regain, from the influence of the returning fun, the enormous quantity of heat loft during the winter, it would

* Philofophical Tranfactions M.DCC.LII.

remain

remain very cold during the ſpring, and probably during the greateſt part of the ſummer; and this could not fail to chill the atmoſphere, and check vegetation in the ſurrounding country to a very conſiderable diſtance. And though a large lake of ſalt water in a cold country would tend to render the winter ſomewhat milder on one ſide of it, namely on the ſide oppoſite to the quarter from whence the cold winds came; yet this advantage would not only be confined to a ſmall tract of country, but would not any where be very important, and would by no means counterbalance the extenſive and fatal conſequences which would be produced in ſummer by ſo large a collection of very cold water.

When the winter is once fairly ſet in—when the earth is well covered with ſnow, and the rivers and lakes with ice, and more eſpecially when the ice as well as the land is covered with that warm winter garment, a few degrees more of cold in the air cannot produce any laſting bad conſequences. It may oblige the inhabitants to uſe additional precautions to guard themſelves, their domeſtic animals, and their proviſions, from the uncommon ſeverity of the weather; but it can have very little influence in the temperature of the enſuing ſummer; and it is even probable, if it influences it at all, that it tends rather to make it warmer than colder. Lakes of ſalt water could therefore be of no real uſe in winter in cold countries, and in ſummer they could not fail to be very hurtful; while freſh lakes, as they are frozen over almoſt as ſoon as the winter ſets in, and long before the whole maſs of their water is cooled down to the temperature of freezing, muſt preſerve the greater part of their heat through the winter; and if they are of no uſe during the cold ſeaſon, they probably do little or no harm in ſummer*.

X.

Uſeful Notices reſpecting various Objects.—Welding of Caſt-Steel—Flexure of Compound Metallic Bars by Change of Temperature.

1. *Welding of Caſt-Steel. By Sir* THOMAS FRANKLAND, *Bart.*†

THE uniting of ſteel to iron by welding is a well-known practice; in ſome caſes for the purpoſe of ſaving ſteel; in others, to render work leſs liable to break, by giving the ſteel a back or ſupport of a tougher material.

Ever ſince the invention of caſt-ſteel (or bar-ſteel refined by fuſion) it has generally been ſuppoſed impoſſible to weld it either to common ſteel or iron; and naturally—for the deſcription in Watſon's Chemical Eſſays (vol. iv. p. 148) is juſt, that in a welding heat it "runs away under the hammer like ſand." How far the Sheffield artiſts, who ſtamp much low-priced work with the title of caſt-ſteel, practiſe the welding it, I am ignorant; but though I have enquired of many ſmiths and cutlers in different parts of the kingdom, I have not yet found the workman who profeſſed himſelf able to accompliſh it. If therefore I ſhould deſcribe a ſimple proceſs for the purpoſe, I may be of uſe to the very many who are incredulous on the ſubject. If any one has made the diſcovery on principle, he has reaſoned thus: Caſt-ſteel in a welding heat is too ſoft to bear being hammered; but is there no

* Throughout this Eſſay, but more particularly in the laſt chapter, the author has very ſtrongly inſiſted on the final cauſes of the laws he has endeavoured to develope. I have no wiſh at preſent to diſcuſs the queſtion, how far the propagation of truth may be advanced by ſpeculations on thoſe cauſes; but have added this note, leſt I ſhould be thought to have mutilated his Eſſay by leaving out ſo ſtriking a part. My aim has been to communicate his phyſical diſcoveries only. N.

† Phil. Tranſ. M.DCC.XCV. 296.

lower

lower degree of heat in which it may be foft enough to unite with iron, yet without hazard of running under the hammer? A few experiments decided the queftion; for the fact is, that caft-fteel in a white heat, and iron in a welding heat, unite completely.

It muft not be denied that confiderable nicety is required in giving a proper heat to the fteel; for, on applying it to the iron, it receives an increafe of heat, and will fometimes run on that increafe, though it would have borne the hammer in that ftate in which it was taken from the fire.

I need fcarcely obferve, that when this procefs is intended, the fteel and iron muft be heated feparately, and the union of the parts propofed to be joined, effected at a fingle heat. In cafe of a confiderable length of work being required, a fuitable thicknefs muft be united, and afterwards drawn out, as is practifed in forging reap-hooks, &c.

The fteels on which my experiments have been made, are Walker's of Rotherham, and Huntfman's, between which I difcover no difference; and though there may be fome trifling variation in the flux ufed for melting, they are probably the fame in effentials.

2. *Flexure of Compound Metallic Bars by Change of Temperature.*

A BAR of fteel upwards of fix inches long, 0.56 inch broad, and 0.05 inch thick, was hard-foldered * to a bar of brafs of the fame dimenfions, but twice the thicknefs. This compound bar was fixed at one end to a fimple metallic bar, as in fig. 2. Pl.V. (facing p. 96) and at the other end there was a fliding piece to meafure the deviations by flexure. It muft be obferved, that a thin piece of metal was placed between the fimple and compound bars, at the pinned part A; fo that the bars were not permitted to come into contact in any temperature, but each pofition was determined by the fliding piece, by means of a micrometer-fcrew and magnifier applied to it whenever its place was required to be known. The clear part of the compound bar from the pin A, to the end D, was exactly fix inches.

By heating the apparatus from 66° to 212°, namely 146°, the catch was moved through 129 parts of the micrometer, each of which was $\frac{1}{1400}$th of an inch. The quantity 129 was therefore equal to 0.0921 inch.

Both fides of the compound bar were then carefully filed away till the metals were each (as near as could be afcertained) only half their former thicknefs. The flexure, by a like difference of temperature, as in the former trial, now proved 278 parts, or 0.199 inch.

The bar was then filed to about $\frac{3}{8}$ths of its original thicknefs, ftill preferving the relative thickneffes of the metals. Its flexure by the fame heat as before was 313 parts, or 0.224 inch; in which, however, from the fpring of the thin bar, it was doubted whether the catch had been moved to the fame diftance as the bar itfelf might have moved alone.

On confidering thefe deviations, the numbers prove to be inverfely as the thickneffes of the bars, with no greater error than might arife from the meafures by which the real thickneffes were afcertained. Thefe experiments may therefore be faid to agree with the theory †.

* For the information of thofe who may not be acquainted with the proceffes of foldering, I muft obferve, that this operation confifts in uniting the pieces by the fufion of a kind of brafs, which contains an extraordinary proportion of zinc, and flows into the joint before the heat is fufficient to melt the pieces. This kind of work will bear hammering and bending, becaufe the folder is not brittle. Soft-foldering is effected by plumbers' folder or tin, which forming a kind of bell metal with the copper is brittle in the joint, and therefore unfit for work intended to fuftain either blows or flexure.

† Philofophical Journal, I. 64.

Fig. 1.

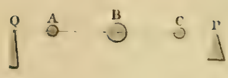


Fig. 2.

Fig. 3.

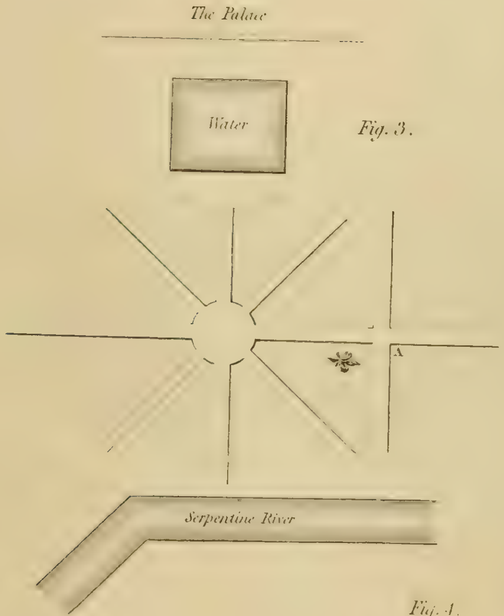


Fig. 4.

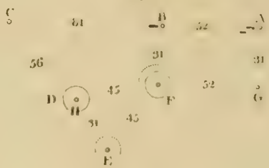
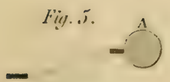


Fig. 5.



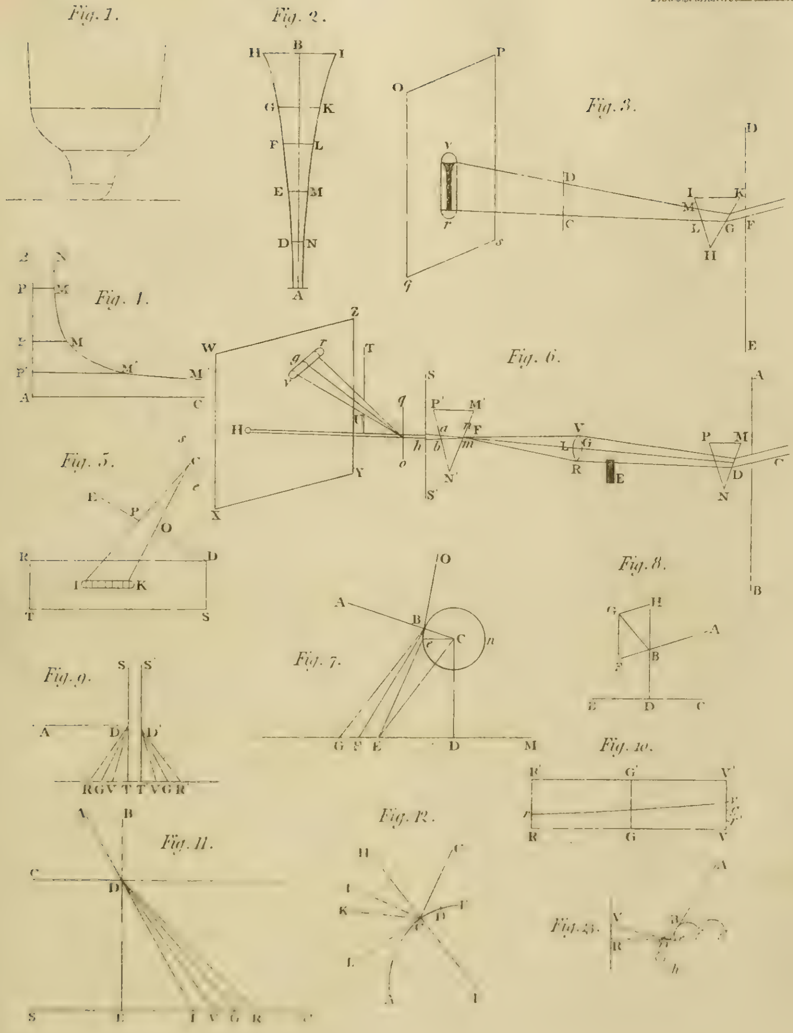


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH 1798.—*SUPPLEMENT.*

ARTICLE I.

Obſervations on Water-Spouts ſeen from Nice. By M. MICHAUD, Correſpondent with the Royal Academy of Sciences at Turin *.

WATER-ſpouts, as Mr. Senebier remarks, are phenomena which ariſe too ſeldom, and are too difficult of obſervation, to admit of our forming an accurate notion of their circumſtances, or pointing out the means by which their cauſes may be inveſtigated. Yet, as nature is never more diſpoſed to explain her ſecret operations than in ſuch as are performed on a great ſcale, there is no doubt but that theſe phenomena, if well explained, would lead to uſeful reſults †.

I do not flatter myſelf with the hope of giving a complete explanation of the cauſe of water-ſpouts, ſuch as M. Senebier appears to deſire; but as the true explanation can only be had by careful and frequent obſervation, I hope my work will not be totally uſeleſs if it ſhould afford an additional ſtep towards the deſired end.

My obſervations did not in ſtrictneſs commence till the 6th of January 1789, but it appears neceſſary, for the ſatisfaction and information of my readers, to mention ſome facts of an earlier date. I muſt therefore remark, that after a mild ſeaſon for the greateſt part of the month of December 1788, at Nice, where the winter is not in general ſevere, with clear weather in the day-time, our atmoſphere underwent a total change on the day of the new moon, which was the 27th of that month. On that day a very violent ſtorm of wind aroſe, attended with a degree of cold as acute as ever was known in the memory of man. The ſky became covered with clouds, and ſnow fell to the depth of more than eight inches.

* Turin Mem. VI.

† Senebier ſur les Moyens de perfectionner la Météorologie. Journal de Rozier, Mai 1787.

As the same effects are produced by the same causes, I have little doubt but that water-spouts were formed near us on the night between the 27th and 28th. But, as I did not see them, it becomes unnecessary for me to enlarge upon any which I did not myself clearly observe.

The severe cold had frozen the snow, and rendered it so compact that, notwithstanding the influence of several days of clear weather soon afterwards, in which the heat of the sun was very perceptible, there was not the least drop of water fell from the eaves of the house in which I dwell, which is exposed to the sun in winter for eight hours, and, being situated near the sea, is perfectly sheltered on the north side by the eminence of the rock of the castle. This fact appeared very surprising to me, after a residence of about forty years in this town. Several old persons remarked, that this snow would wait for another fall before it melted, and I found by the result that the observation was true.

On Sunday the fourth of January 178[illegible], at the phasis of the first quarter of the moon, the cold was again renewed, and continued severe on the Monday and Tuesday. At eight in the morning I first observed an immense mass of clouds towering upwards, and extending from north-east to south, which rose towards the zenith, by advancing to the westward. Accustomed as I am to consider these clouds according to the system of my old Professor of Natural Philosophy, Father Beccaria*, I concluded that they would proceed to desolate our fields, the fruits of which, particularly the oranges and lemons, had already perished by the antecedent cold. And as a strong wind then prevailed over the face of the sea, I foretold to my two eldest sons, that it was very probable we might discover some water-spout in the course of the day. In fact, about five minutes after ten in the morning, I observed on the sea, at the distance of not more than a musket-shot from the shore, a round space of ten or twelve toises in diameter, in which the water did not really boil, but seemed ready to boil. Plate XXIV. For there appeared all round, and sometimes within the circle, vapours in the form of mists, eight toises and more in height, having the appearance, though on a scale incomparably larger, of those vapours which rise from the surface of water beginning to simmer. I saw clearly that this was, if I may so express myself, the embryo of the foot of a water-spout driven along by the wind, while the clouds were not sufficiently advanced to afford the stem a body. It continued therefore to move before the wind from east to west, keeping, to my very great surprise, its surrounding vapours, elevated like sails, notwithstanding the extreme force of impulsion which drove it towards the shore. As soon as it came near the land, the circle was contracted, the mass of vapour became of less dimensions, and at the moment it touched the land it was at once overset by the wind, under the appearance of a long train of mist, fig. 1, b, which was speedily dissipated. I then perceived that the hope I had formed of seeing water-spouts during the day, was on the point of being realized: but as my occupations demanded my attendance elsewhere, I charged my two eldest sons to watch alternately at the window, in order that the phenomenon might not pass unobserved.

At last, about eight minutes before noon, my second son came to me, exclaiming, "Father, here is a very superb water-spout." His earnestness was equal to that of a sailor,

* Electricisme Atmospherique. We have a translation in English.

who,

who, after a long and tedious voyage, firft difcovers the land. I followed him to the window, and beheld an immenfe water-fpout paffing majeftically before Nice, as reprefented in fig. 2. The clouds had already occupied not only the upper and fouthern part of the atmofphere, but they had proceeded towards the weft, fo as to cover the whole extent within my view; with this circumftance neverthelefs, that they had left uncovered beneath and towards the fouth a part in the form of a fegment of a circle, through which, at an extreme diftance, fome clouds were difcerned, upon which the fun threw the colours of the morning.

The foot of this water-fpout, which was in every refpect infinitely fuperior to that which Meffrs. Papacin, Renaud, and myfelf, obferved in 1780, was fo ample, that a man of war of 100 guns, with all its fails, might have been enveloped and even concealed in it. For we are, from daily practice, fufficiently accuftomed to judge of the fize of objects at different diftances by that of the image formed upon the retina, affifted by our habitual reafoning. And hence, from the circular form of the foot of this water-fpout, fome judgment may be made of the volume of vapour it afforded.

Inftead of the tranquillity it exhibited at its firft appearance, this lower part affumed the refemblance of the crater of a volcano, with this exception, that it threw out nothing but large ftreams of cloud and fpouts of fea-water. But it threw thefe in parabolic ftreams from the centre to the circumference, and all around, with fuch impetuofity and violence as to render it very evident to us, that an inexpreffible effervefcence muft have prevailed in the interior bafon, though the great diftance and the opacity of the furrounding vapour prevented us from feeing the phenomena as diftinctly as we faw the ebullition of the water-fpout of 1780*.

The diameter of the water-fpout, and that of its expanded upper part, were large in proportion. Its colour was a very deep indigo, the fame as that of the clouds, which extended from eaft to weft. It was impoffible for us to obferve the afcent of the vapours of frefh water; but the obfervation of 1780, in which this was clearly feen, fupplies this defect. It will alfo be fhewn that the afcenfion was again feen in a moft complete manner.

While we were looking at this extraordinary appearance, which my fons beheld for the firft time, and which feemed to have concentrated all their fenfes in one, on a fudden an impetuous fhower of hail difcharged itfelf againft the windows in grains of the fize of piftol and mufket balls. We immediately fufpended our obfervations, in order to clofe the fhades of both ftories of the houfe, in which the whole family affifted, for fear of having the windows abfolutely broken to pieces, as happened a few years before. But I foon perceived that this precaution was abfolutely ufelefs, or at leaft unneceffary. For the hail, though in a few minutes it covered the ground to the height of four inches, did not in the leaft damage the trees in the garden behind our houfe. It confifted merely of large flakes of fnow rounded by the wind in their fall, and poffeffing neither the weight nor the hardnefs of hail. Upon opening fome of the pieces, I found them to confift of a thin compact fhell, nearly empty within, excepting a few rays from the centre to the circumference.

* See the Journal de Phyfique, XXX. Part I. 254. for Obfervation fur une Trombe de Mer, faite à Nice de Provence en 1780, et addreffée à M. Faujas de St. Fond, par M. Michaud. The facts are lefs minutely ftated, but nearly agree with the prefent memoir. N.

The

The degree of congelation in these balls was so slight that they began to melt the moment they touched the ground, and accelerated the fusion of the snow which had fallen before.

This frozen snow, which during its fall had obscured the air sufficiently to prevent our seeing the water-spout through the blinds, having ceased, we resumed our observations with all possible diligence, and beheld another water-spout somewhat inferior in magnitude to the former which had disappeared. It followed nearly the same course as the other. By the account of time employed by each in its successive passage, I estimated that the one before us must have been the third; nevertheless, by confining my narration to what I really saw, it must be considered as the second only. This water-spout having continued its course towards Antibes, we observed that it began to contract in all its dimensions, some time before it arrived at the shore, and that the foot was reduced to nothing when it touched the ground. It contracted insensibly upwards, the expanded conical part became broader and more rare, and the whole joined the mass of clouds in the same manner as one mist incorporates with another. There must consequently be some error in the account of the observation of 1780, where I say that the spout withdrew upwards as quick as lightning. This expression gives too precipitate an idea of the dissolution of the water-spout. Having thus kept sight of it until its total extinction, I returned towards the place where I had discovered the first water-spout, and was greatly surprised at discovering a new foot ready formed, without any descending spout a, fig. 3, Plate XXV. My astonishment was founded on these three circumstances:

1. The existence of the foot of the water-spout without its stem or body. For before this observation, and from the facts in 1780, I considered it as indubitable that the enveloping matter of the foot or recipient was a production of the body of the water-spout itself, or an expansion of its proper substance. Now I saw clearly enough in this phenomenon the identity of the substance which composes clouds and mists, and that it was not supplied by the water-spout. The embryo of the spout which I had seen at ten o'clock, appeared to show that it was probably produced by the sea.

2. I was surprised to see that this foot was stationary at the place of its formation, whereas those which I had before seen were carried swiftly along by the wind. For though it was not impossible but that this foot might be carried by a motion along the line of sight, and consequently not perceptible by me; it was at least certain that it gained nothing from east to west, that is to say, from my left to my right, the direction in which the sea, the clouds, and the other water-spouts which had travelled so far in so short a space of time, were carried.

3. I was astonished, that the body of the water-spout being wanting, which, according to my notions, might increase the intensity of the power by which this appearance is produced, it was nevertheless possible that this envelope should be capable of remaining upright and stationary. In this uncertainty I suspended my reflexions to observe the result. I remarked a kind of teat or protuberance, b, fig. 3, projecting obliquely from the lower part of the clouds which arrived from the east. The foot continued motionless, and the protuberance preserved its oblique direction, till the moment when by the action of the wind it arrived at the foot; at which instant we all three observed the protuberance direct itself perpendicularly towards the foot, and like an immense sack of gauze unroll itself

from the extremity c, fig. 3; when the folds of this fack difappeared, and the body of the water-fpout, which was grey and tranfparent, fixed itfelt in the bottom of the foot, affumed the vertical pofition, and became larger in diameter. My fecond fon, who, as well as his elder brother, poffeffes a very clear fight, immediately exclaimed, "See, father, how rapidly the vapours fly up through the bag." I faw, in fact, that they feemed to expand it with a kind of tenfion, at the fame time giving it a deep indigo colour, which was communicated to the cloud. At the fame inftant the colour of the whole water-fpout became fo deep that we could diftinguifh no motion in its expanded part. We obferved only that the whole phenomenon moved from eaft to weft, and was deftroyed on the coaft of Provence. Laftly, a fourth was formed, which was deftroyed in the fame manner, without any fuch reproduction, beyond the hills of Antibes, as I obferved in 1780, becaufe, their courfe being more oblique towards the north, they could not meet the gulph Jean, and the prolongation of their track was altogether over land. A fall of fnow fucceeded immediately afterwards, which was of the ufual denfity and configuration. It lafted all the reft of the afternoon and the following night, fo that on the following day there was as much fnow on the ground as before. It afterwards rained for a long time, which cleared the country of the fnow that had accumulated. As the impetuous wind of the preceding day continued with undiminifhed force through the whole night, and the other acceffary circumftances were likewife prefent, I think there is reafon to conclude that new water-fpouts muft have been formed in the afternoon, and perhaps in the night of the 6th; but the obfcurity of the atmofphere, from the fall of fnow, did not permit me to obferve them. I fhall therefore proceed to make fome remarks on the wind which caufed this phenomenon.

Though the velocity of this wind was nearly equal to that of the greateft ftorms in our feas, the waves were not proportionally deep. Two circumftances appeared to concur in producing this effect; the firft, that by the form of our coaft an eaft wind cannot have paffed over fo great an extent of fea as a wind from the fouth-weft, from which quarter our greateft ftorms come. This caufe is conftant with regard to our local fituation. The other circumftance was, that the wind did not blow obliquely downwards, but moved parallel to the furface of the fea. This fuppofition, which is the only one that requires proof, was confirmed at the time of obfervation by the appearance of a fmall Catalan veffel, which the wind of the 6th of January blew afhore near Nice. I faw her pafs before my windows, driven by a force fhe was incapable of refifting. She did not labour much, but came to an anchor at a little diftance, from which, however, the violence of the wind drove her on fhore, though without confiderable damage, fince fhe was got off a few days after, and purfued her voyage. The force of this wind was feen not only in the inftance of this veffel, but in a confiderable number of others which were loft on the neighbouring fhores.

It is an obfervation very agreeable to the opinion of Profeffor Toaldo, and at the fame time well eftablifhed by every obfervation I have made fince I directed my attention to the phenomena of water-fpouts, from the beginning of 1789 to the 19th of March in the fame year, on which day we obferved thofe which I fhall prefently defcribe: it is a confirmed obfervation, I fay, that the phafes of the moon are accompanied with a change of weather.

It

It is certain that the cold weather which fuddenly came on with the new moon of the 27th of December, recurred again exactly at each new phafis. I cannot be deceived in this refpect, becaufe the chimney of my apartment fmoked each time, and did not fmoke but during this accidental cold. Two days afterwards, when the weather became milder, this chimney acted as ufual, and did not fmoke again until the following change. I could urge many other circumftances in favour of the opinion of Mr. Toaldo; but I forbear, becaufe they are foreign to my prefent fubject.

Laftly, on the 19th of March the wind, which had begun the preceding evening, blew with a degree of impetuofity lefs than that of the 6th of January. The clouds were accumulated from the eaft towards the weft, but they were much lefs condenfed than at that time. At forty minutes after eleven in the morning, we obferved two water-fpouts, a, b, Plate XXV. fig. 4, which moved at the fame time, the one after the other. The moft remarkable circumftances in thefe water-fpouts were: 1. The prodigious enlargement of the protuberance d, fig. 4, from the extremity of which hung the effective fpout b, which was incomparably thinner; but the wonder difappears, when we reflect that the following fpout, which maintained itfelf in the fame ftate as thofe we had before obferved, robbed the preceding one in fome meafure of its fupport; fo that this enlargement was, as it were, a commencement of diffolution, and the thinnefs of b was a proof of the little intenfity of electric power then acting; a conclufion which is alfo confirmed by the following circumftances. 2. The incapacity in the feet of thefe two water-fpouts to elevate their furrounding plumes. It is feen, a b, fig. 4, that they were reverfed by a force which prevented their rifing in a perpendicular direction, like thofe of the preceding water-fpouts. At the extremities of the plumes here defcribed, as well as at the centre of the circle near the furface of the fea, there was formed a fmall atmofphere: but as it was not extenfive, the vapours were fo few that we had very little fnow, which continued for about half an hour; when the weather cleared up. During the tranfition of thefe water-fpouts very diftant thunder was heard five or fix times.

In the interval between the obfervations of the 6th of January and the 19th of March, other water-fpouts muft have been formed on the coafts of Provence. It is certain, at leaft, that I faw the appendices projecting from the clouds, and that the product of frozen fnow reached our firft hills on this fide of the Var; but as my profpect, being limited by the mountains of Provence, did not allow me to fee thefe water-fpouts in fuch a manner as to make any drawing or defcription, I fhall here conclude my obfervations, and attend to the refults.

I don't know whether I am mifled by partiality for my own obfervations, when I exprefs my opinion that the facts noted by me on the 12th of April 1780 are of great value in natural philofophy, as well becaufe of the vicinity of the water-fpout which appeared that day, as of the tranfparency of the furrounding vapours of the foot, which exhibited the interior ebullition with fcarcely any obfcurity. Whence it follows,

1. That there is a real ebullition in the fea, at the place circumfcribed by the foot of the water-fpout.

2. That the vapours of the water which muft arife, are the product of an evaporation which

which muſt ſeparate freſh water from the ſalt; it being aſcertained by experiment, that diſtillation is the only method by which ſea-water can be rendered completely freſh.

I ſhall take the liberty in this place to make uſe of a compariſon, by way of explaining more perfectly the phenomenon we at that time beheld. It will not probably be unacceptable to ſuch as have never ſeen a water-ſpout, at leaſt in ſo favourable a poſition. Suppoſe the chimney of a baker's oven, ſuch as we have at Nice, in which the fuel commonly burned conſiſts of branches of pine recently broken from the tree, and ſufficient to bake large quantities of bread; let this chimney be imagined to be in the ſtate of throwing out immenſe clouds of vapour and ſmoke. Suppoſe a funnel of glaſs to be adapted to the aperture at the lower part, and to have its diameter enlarged upwards, until it terminates in a very extended veſſel. It may eaſily be conceived that the aſcending vapours will be ſo preſſed together in the narrow neck of the glaſs, as to render it opake, without permitting the ſucceſſive undulations of the vapour to be diſcerned. But in proportion as they proceed further from the cauſe of motion, which is the fire of the oven, and arrive at a part of the glaſs where they have room to expand, the vapours, being leſs condenſed, will exhibit their peculiar motions, and the ſucceſſive eruptions of the ſmoke will indubitably be ſeen. In this imaginary experiment, let the foot be ſuppoſed to be abſtracted; let the aſcending vapour be ſimply that of boiling water, and the conical tube of glaſs here contemplated, will afford a natural repreſentation of the phenomenon obſerved in 1780, and confirmed on the 6th of January 1789.

It may perhaps be objected, that theſe facts do not at all agree with Profeſſor Muſchenbroek's theory of water-ſpouts, given in his Eſſai de Phyſique. This objection was ſtarted by all my philoſophical acquaintance againſt the report of our obſervation in 1780, and engaged me to ſuſpend the publication of my memoir for ſeveral years, becauſe it was not poſſible to reconcile the means employed by nature in the production of water-ſpouts, nor their uſe, with the ideas of the celebrated Dutch Profeſſor. Beſides which, having only one obſervation to offer, though well ſupported by the teſtimony of two reſpectable perſons, I flattered myſelf that I might remove all the difficulties by repreſenting a water-ſpout in the ſame manner as lightning and thunder are imitated by the electrical machine. For it ſeemed to me at that time, and ſtill appears to be, a thing very practicable. But at preſent, by a new ſeries of obſervations in confirmation of that of 1780, I ſee clearly that the proceſſes of nature are very different from thoſe pointed out by Profeſſor Muſchenbroek. That enlightened and moſt accurate obſerver had no opportunity of examining this phenomenon in a favourable poſition himſelf, and has been equally unfortunate in the explanation of the ſuppoſed deſcent of water, which really aſcends in water-ſpouts, as well as in the formation of the foot, which according to his theory is a maſs of ſea-water in its natural ſtate. I can affirm without fear of contradiction by experience, that this foot or atmoſphere as it may be called is nothing but the matter of clouds and miſts.

It muſt moreover be remarked, that in the time of Muſchenbroek the theory of electricity had made ſo little progreſs, that he did not avail himſelf of it in the account of fiery meteors. It is therefore leſs wonderful that he ſhould not have recurred to it in his theory of water-ſpouts.

What then is the agent, it may be aſked, which cauſes this ebullition in the ſea, and raiſes

raiſes the vapours through the water-ſpout to the cloud? Simply to affirm that this agent is electricity, without further proof, is in fact to ſay nothing. To this queſtion I muſt anſwer, that I have exhibited the products of obſervation: I have related what I have moſt clearly ſeen. I think I can diſcern the cauſe without being able to exhibit proof: but I ſhall be happy to be anticipated by philoſophers of greater ſkill in this theory, and think it better to ſuſpend my judgment than yield to the ſeductive pleaſure of explaining every thing by adding to the maſs of error in natural philoſophy.

The ſecond fact which preſents itſelf in our obſervations is, that two cauſes unite in the formation of water-ſpouts, or rather two different modifications of the ſame cauſe. When the foot appears without the water-ſpout, it is not the productive cauſe, but rather an effervefcence which prevails in the ſea at that place. But how many intereſting queſtions might be aſked reſpecting this part of the phenomenon! What cauſe is it ſo powerful as to retain the foot a, fig. 3, and keep it motionleſs, notwithſtanding an impetuous eaſterly wind, until the projection in the cloud which is to form the water-ſpout ſhall arrive directly over it? Was the apparent bag which developed itſelf from the cloud, pre-exiſtent in the projecting part? As I can make no ſatisfactory reply to theſe and other queſtions which might be propoſed, I ſhall proceed to the third remarkable fact.

3. When the foot of a water-ſpout begins to approach the earth, its diameter contracts, its height is diminiſhed, and its volume becomes leſs and leſs; ſo that the foot is reduced to nothing at the inſtant it touches the ſhore. From the attentive examination I have made, it has appeared that the foot even of the greateſt water-ſpouts began to diminiſh when the depth of the ſea beneath became leſs than the elevation of the foot itſelf above the ſurface. If this be true, as I think it is, it may be concluded that the effervefcence which ſupplies the ſpout with water, and forms the ſurrounding vapours of the foot, extends itſelf in depth nearly as much as the foot itſelf riſes above the ſea, and that materials for the ſupply of vapour become defective in quantity in proportion to the ſhallowneſs of the water.

Explanation of the Drawing.

FIG. 1. Plate XXIV. repreſents the imperfect foot a of the water-ſpout ſeen on the 6th of January 1789, at five minutes after ten in the morning. On the left hand are ſeen the clouds which riſe towards the zenith, but ſtill conſiderably diſtant. This foot had plumes elevated nearly like ſails, and was driven towards the ſhore by the wind. In proportion as it came near the land it contracted, and was reduced into a column of miſt, which the wind overſet on the land the moment the ſupply of water was wanting.

Fig. 2 repreſents, letter a, the enormous water-ſpout obſerved on the ſame day at eight minutes before noon. Nothing could more nearly reſemble a ſhip of war on fire than this phenomenon, excepting that no flames appeared. I have endeavoured to ſhew the continual jets of its ſurrounding vapour, and of the water which iſſued from the centre. At b are ſeen the remains of a water-ſpout after it has been deſtroyed by the foot having touched the ground.

Fig. 3. Plate XXV. a repreſents the foot of the ſecond water-ſpout ready formed, which was probably the third. It has yet no ſpout. At b is ſeen a protuberance tending obliquely towards the eaſt, and advancing to the weſt with the cloud to which it is ſuſpended. At the

the letter c it may be obferved how the protuberance b having arrived over the foot a became vertical, and unrolled itfelf inftantly in a kind of large bag, of the figure of an inverted cone, nearly tranfparent, like gauze. As foon as this bag compofed of the matter of the cloud had developed itfelf, which occupied the time of three or four feconds, and had fixed its fmall extremity in the bottom of the foot d, it became ftraight, without folds. A vapour like that feen in 1780 immediately rofe up the tube, extended it in the form of a water-fpout, deprived it of its tranfparency, gave it a deep indigo-colour, like that of the clouds, and at the fame inftant the foot and the fpout were moved from eaft to weft, and followed the courfe which the impulfe of the wind gave to the clouds to which the fpout was attached. This fpout being deftroyed, the following fpout exhibited the fame phenomena as the others. It muft be remarked, that the diftance of the protuberance b from the foot already formed at a could not be augmented in the figsre; but this protuberance, when firft obferved, was at more than a league diftant from a, which remained motionlefs till its arrival. It is probable that it may have arrived from a ftill greater diftance. It is alfo to be noted, that the fpouts 2 and 3 were fomewhat lefs in all their dimenfions than the firft.

Fig. 4. exhibits two water-fpouts, which were feen following each other on the 19th of March. The wind was lefs ftrong than on the 6th of January, the fea lefs agitated, the clouds lefs accumulated, and lefs deep in colour. The intenfity of the phenomenon was alfo proportionally lefs. It may be feen at a and b that the furrounding plumes of the foot had not the power to raife themfelves up, as in the preceding figures, but were kept down by the wind. The enlargement of the upper part of the fpout d appeared to be a commencement of diffolution, fuch as was obferved at b fig. 2. Thefe two water-fpouts proceeded to the point of N. Dame de la Garde, beyond Antibes. Thofe of the 6th of January reached the fhore between the town of Antibes and the mouth of the Var. The line they defcribed from the time we firft faw them to that of reaching the fhore may be eftimated at five or fix common leagues.

Advertifement refpecting the Figures.

THE magnitude of fig. 1. in thefe drawings muft not be confidered as proportional to thofe of the figures 2, 3, 4. The firft water-fpout paffed at no greater diftance than a mufket-fhot from our windows; the others were at the diftance of two or three leagues. It is neceffary to make allowance for what the firft gained by its nearnefs, and the others loft by their remote fituation, in order to form a judgment of the refpective fize of each fuppofed to be at the fame diftance.

II.

Experiments and Obfervations on the Inflexion, Reflexion, and Colours of Light. *By* HENRY BROUGHAM, *Jun. Efq.*

[Concluded from page 563.]

HAVING endeavoured to unfold the property of flexibility, as varied in inflexion, deflexion and reflexion; and alfo the phyfical caufe of this property; and having indulged in a fpeculation depending on this caufe, I flatter myfelf neither altogether ufelefs nor unimportant;

portant; I haften now to the natural phenomena, the explanation of which depends on the property whofe exiftence and nature we have juft now been inveftigating; and that we may treat this part of the fubject with concifenefs and order, we fhall rank the phenomena under a divifion fimilar to that under which we laid down the principles, beginning with thofe appearances which are explicable on the principles of flexion.

1. It is obfervable, that when a body is expofed in the fun's light fo as to caft a fhadow, and another body is approached to it, either between the fun and it, or the fhadow and it, or in the fame line with it, the fhadow of the one body comes out a confiderable way, and meets that of the other. Now it is evident, that when the bodies are held at a fufficient diftance from one another, a penumbra is formed round the fhadow of each, making it lefs than it fhould be were there no inflexion; but when the bodies are brought fo clofe to one another that the edge of the one is within the fphere of the other's inflexion, the light being already bent by this laft, the former can have none to bend, and confequently no penumbra in the part of the fhadow correfponding to that part of the body which is within the other's fphere of inflexion; and the reft of the fhadow having a penumbra, this part that has none will be larger than it, and increafe as the bodies approach, till at laft it meets the other fhadow: the like appearance happening when the fhadows are thrown on the eye. Mr. Melvill has endeavoured to fhew that it belongs fimply to a cafe of vifion*. However, we have now feen that it has no reference to the ftructure or pofition of the eye, but only to the common nature of all fhadows†.

2. If we fhut out all the light coming into a room from external objects, except what may pafs through a fmall hole of $\frac{1}{2}$ or $\frac{1}{4}$th of an inch in diameter, the images of the external objects, as clouds, houfes, trees, will be painted on the oppofite wall by the rays of light croffing at the hole; but if a piece of rough glafs or of very fine paper be held fo as to cover it all over, the light does not pafs through; then if the paper be wetted with oil, or the glafs with water, fo as to give either a fmall degree of tranfparency, the firft rays that come through are thofe from red and orange objects, and laft from blue and violet. Now it is evident that tranfparency in general, and this particular fact, are explicable by what was before laid down. It was found by Newton, that a body tranfmits the light incident on it more or lefs according to the continuity of its particles; and that a ftrong reflexion takes place on the confines of a vacuum‡: How does this happen? The initial velocity of light is fufficient to carry it through the firft furface or fet of particles; but it is fo much diminifhed that it is reflected by the repulfive power of the back fide of thefe particles, unlefs there be others behind at a certain diftance, namely, that at which inflexion or attraction acts, that is, apparent contact: this attraction renews the impetus of light, and tranfmits it to another fet, and fo on. Now this action being ftrongeft on the largeft and red particles, and weakeft on the blue and violet, if the continuity be diminifhed, the former will be tranfmitted, and not the latter; which is conformable to the experiment juft now mentioned.

3. The doctrine of flexibility furnifhes an eafy and fatisfactory explanation of the different colours which are affumed by flame. Whether we fuppofe the light to come from the burning body or the oxygenous gas, the largeft or red particles have the ftrongeft attrac-

* Edinburgh Literary Effays, Vol. II.

† See, however, Philof. Journ. I. 431.

‡ Optics, Book ii. Part III. Prop. 3.

 tion

tion for bodies, the violet the weakeſt; when therefore the gas and the body combine, the precipitation of light muſt be in the reverſe order of the affinity between the particles of light and thoſe of the bodies. If then the combination take place ſlowly, the violet and blue particles will be firſt emitted, and laſt of all the red: and this is conſiſtent with fact; for any inflammable body whatever, on being lighted, burns at firſt with a blue or violet flame, and afterwards has its flame of two or three diſtinct colours, blue, white, red, &c. as is ſeen remarkably in the caſe of a candle. Nay, I have obſerved in the flame of a blow-pipe, all the ſeven primary colours at once. When indeed a body is burnt in pure oxygenous gas, the combination is ſo rapid, that white light alone is precipitated undecompoſed; but in common air, where the azotic gas impedes the combuſtion, the above phenomena are obvious.

4. A curious phenomenon has often ſurpriſed philoſophers, namely, blue ſhadows. Theſe I have obſerved at all times when the paper on which I received them was illuminated by the ſky and any other light; and the reaſon of them I take to be this: that the ſhadow made by one light is illuminated by the blue rays from the ſky; for I have often obſerved purple, and even reddiſh ones, when the ſky or clouds happened to be of thoſe colours; and this account of the matter is confirmed by an experiment. Having received the coloured ſpectrum made by a priſm with a large refracting angle, on a ſheet of rough white paper, and held above it another ſheet; I ſtopped all the rays that illuminated the firſt, except the blue, and violet, and red; and if I held a body between the blue and the ſecond paper, its ſhadow was red; and if I held a body between the red and the paper, its ſhadow was blue; and ſo of other colours. This I take to amount to a demonſtration of the thing *.

5. Paſſing over other phenomena of leſs note, I come now to one that has divided opticians more than any other; I mean the coloured fringes that ſurround the ſhadows of bodies. I made ſeveral obſervations on theſe, which enable me to conclude that each fringe is an image of the luminous body; for, holding between my eye and a candle two knife-blades, as I approached the one to the other, the edge of the candle ſeemed multiplied, and ſoon became coloured, coming wholly away from the candle; and, as the knives approached ſtill nearer, became diſtinct dilated images, highly tinctured with the priſmatic colours; and juſt before the knives met, the candle, whoſe edges had been all along coloured with red and yellow, became much diſtended, till at laſt it was divided in the middle, one half ſeeming to be drawn away by each knife, and then it wholly diſappeared. I have obſerved three kinds of theſe images, two without, and one within the ſhadow: the firſt had its colours in the order from the ſhadow, red outermoſt, and violet innermoſt; the ſecond and third had the colours in the contrary order; but the ſecond was ſo very faint that I could never perceive it, unleſs when let fall on my eye. All this is eaſily explained by the different flexibility of the rays. In fig. 9, Plate XXIII. let AD be a body by which the rays SDT and S′D′T′ paſs, and let SD be within AD's ſphere of inflexion, and S′D′ within its ſphere of deflexion; then SD will be bent into DG: but becauſe of the different inflexibility of its parts, the red will be bent into DR, and the violet into DV, and the intermediate rays will fall between R and V; the whole forming an image RGV, ſeparated into the ſeven primary

* Since writing the above, I find the ſame explanation of the matter given by Mr. Melvill and ſome of the French academicians, particularly Meſſrs. Buffon and Biguelin; alſo Count Rumford (Philoſ. Journal I. 101); but I have thought fit to keep it in, on account of the experiment that occurred to me in illuſtration of it.

 colours;

colours; and in like manner by the different deflexibility of the parts whereof S′D′ confifts, an image without the fhadow, as V′G′R′, will be formed, fimilar to VGR, R′ being red, and V′ violet, all which is both theory and experience; and the fame explanation may be extended to the other cafes. Now in all thefe, the bending power ftretching to a very fmall definite diftance, and being of different degrees of ftrength at different diftances from the body, feveral pencils or fmall beams, paffing through different parts of the fpheres, will be acted upon by the power in its different ftates of ftrength, and each beam will be difpofed into an image in the way before defcribed. Of thefe images I have fometimes obferved four, and even, by ufing great care, the faint lineaments of a fifth. In forming them, the power acts ftrongeft at the fmalleft diftances, and of confequence bends the mean flexible rays that pafs near, farther inwards or outwards than thofe that pafs farther off; fo that the extreme rays will in the former cafe be more feparated from the mean than in the latter; and the nearer image will always be the largeft and moft highly coloured; which is confiftent with fact. This explains fully the celebrated experiment of Sir Ifaac Newton with the knives, and the explanation is confirmed by the experiments which I related above on flexibility, where the bending force acted moft ftrongly on thofe images formed out of red light, and leaft ftrongly on thofe formed out of violet and blue light. A number of other phenomena are explicable on the fame principles, being only particular cafes, as it were, of the coloured fringes or images: I fhall here mention a few of the moft remarkable.

6. When making fome of the experiments which I have related in the courfe of this paper, I obferved, that when the fun was furrounded, but not covered, by clear white clouds, the white image on the chart (the hole being $1\frac{1}{2}$ inch in diameter) was furrounded by two rainbows pretty broad and bright; in the colours were red on the outfide, and violet next the white of the image. Thefe bows muft not be confounded with one which fometimes appears wholly of a dull red and yellow, when the fun or moon fhines through a cloud, and which is owing to the direct tranfmiffion of the red rays, and reflexion of the others; for not only are the colours different in fpecies, in brightnefs, and in number, in the phenomena under difcuffion, but likewife they are formed by the hole in the window, as I knew by altering its fhape into an oblong; and the colours now were not difpofed in circles, but in broad lines of the fame breadth as the bows had been, running along the fhadow of the hole's fides, and in the fame pofition of colours as before. It is evident that their caufe is the inflexion of the light which comes from the clouds by the fides of the hole (for if the fky have no clouds the colours do not appear) which feparate the white light into the parts of which it is compofed.

7. It is obfervable, that when we look at any luminous body at a diftance greater than one or two feet, its flame appears furrounded by two bows of faint colours, the innermoft of them terminating in a white which continues to the flame; and the colours are red outermoft, and green and blue innermoft: the appearance is moft remarkable if we look at a fmall hole in the window-fhut, the room being otherwife dark; and if the eye be preffed upon, and then opened, the colours are more lively than before, as Defcartes obferved *; from which both he and Newton concluded that the appearance was owing entirely to wrinkles formed on the furface of the eye by the preffure †. But this could neither form

* De Meteoribus. † Lect. Opticæ. Sect. III. ad finem.

the

the bows with the regularity in which they always appear, nor could the colours be in the order above mentioned from the different refrangibility of the rays: it will alſo be obvious to any one who tries the thing, that the preſſure only increaſes the brightneſs and breadth of the bows, but does not form them. The true ſolution of the difficulty ſeems to be this: The rays which enter the pupil are inflected in their paſſage through the fibres which extend over the cornea, and which are very minute, but opake; by theſe they are decompounded into fringes, having the red outermoſt and the violet innermoſt; and the fringes formed by each fibre, being joined together, form the bow. How then does the preſſure enlarge and vivify them? The fibres are naturally extended over the ſurface of a ſpherical ſegment: when this ſurface is compreſſed into a plane circle, they are condenſed into a much leſs ſpace, and conſequently brought nearer to one another; the rays are therefore more inflected and ſeparated than before. If this explanation be true, it will follow that the like bows may be produced by ſmall hairs like fibres placed near one another; and this I found perfectly conſiſtent with fact: the bows are in this caſe brighter than in the other; and the ſmall hairs on a hat or the hand made them brighter than any other I have tried. A circumſtance which I obſerved in both caſes ſeems to ſhew clearly the identity of the cauſes; the white ſpace which reached from the interior bow to the flame, was ſpeckled or mottled in a manner which cannot be eaſily deſcribed; but which any one will perceive upon trying the experiment.

8. The laſt of theſe phenomena which I ſhall mention is the celebrated one obſerved by Sir Iſaac Newton, namely the rings of colours with which the focus of a concave glaſs mirror is ſurrounded. Sir Iſaac made ſeveral moſt ingenious and accurate experiments to inveſtigate their nature *; and finding their breadth to be in the inverſe ſubduplicate ratio of the mirror's thickneſs, he concluded that they were of the ſame nature and original with thoſe of thin plates deſcribed by him †. The Duc de Chaulnes purſued theſe experiments with conſiderable ſucceſs: he found that the rings were brighter, the nearer to the perpendicular the rays were incident; and that if, inſtead of a concave glaſs mirror, a metal one was uſed, with a ſmall piece of fine cambric, or reticulated ſilver wire, ſtretched before it, the colours were no longer diſpoſed in rings, but in ſtreaks of the ſame ſhape with the intervals between the threads: hence he concludes that they are owing to inflexion; that in paſſing through the firſt ſurface they are inflected, and condenſed by the ſecond ‡. I am not, I own, quite ſatisfied with this account of the matter: that they are produced by inflexion the Duke's experiments put beyond doubt; but that they ſhould be formed in paſſing through the firſt ſurface, and reflected by the ſecond, is quite inconſiſtent with the ratio obſerved by their breadth, this being greater in the thinneſt glaſs, and alſo with the order of the colours. Beſides, all the coloured images, which fall on the back ſide of the mirror, will be (by what we before found, when ſpeaking of flexibility §) reflected into a white focus; ſo that, upon the whole, there appears every reaſon to believe that the rings are formed by the firſt ſurface, out of the light which, after reflexion from the ſecond ſurface, is ſcattered, and paſſes on to the chart. It will follow, 1ſt, that a plane mirror makes them not; for the regularly reflected light, not being thrown to a focus, mixes with the

* Optics, Book ii. Part IV.

† Book ii. Parts I. and II.

‡ Mem. de l'Academie pour l'Année 1755.

§ Part II. Obſ. 6. of this paper.

decom-

decompounded scattered light, and dilutes it. 2d, That the nearer to the perpendicular the rays are incident, the more light will be reflected to the focus, and consequently the less will dilate and weaken the rings. 3d, That the thinner the mirror is, or the nearer the two surfaces are, the broader will the rings be. 4th, That the rings farther from the focus will be broader. And lastly, that when homogeneous light is reflected the fringes or images will be larger, and farther from one another in red than in any other primary colour. All which is perfectly consistent with the experiments of Newton and Chaulnes. There is only one difficulty that may be started to this explanation: How happens it that the colours (made by the mirror) are always circular? We answer, It is owing to the manner of polishing the concave mirror, which is laid between a convex and concave plate, and then turned round (with putty, or melted pitch) in the very direction in which the rings are. If it should be asked, Why does the thickness of the mirror influence the breadth of the rings exactly in the inverse subduplicate ratio? we answer, That to a certain distance from the point of incidence (and the rays are never scattered far from it) this is demonstrable to hold as a property of mathematical lines in general.

Having found that the fringes by flexion are images of the luminous body *, I thought that from this consideration a method of determining the different degrees of flexibility of the different rays might be deduced, similar to that which I had formerly used for determining their degrees of reflexibility †. I therefore made the following experiment:

Obs. 12.‡ Having let into my darkened chamber a strong beam of the sun's light, through a hole $\frac{1}{30}$th of an inch in diameter, I held a hair at four feet from the hole; and receiving the shadow at two feet from the hair, I drew a line across the middle of the coloured images, and pointed off in each the divisions of the colours, as nearly as I could observe; and repeating the observation several times, and at different distances, I found, by the same way I had formerly done in my experiment on reflexibility, that the axis, or line drawn through the middle of each, was divided inversely according to the intervals of the chords which sound the notes in an octave, *ut*, *re*, *mi*, *sol*, *la*, *fa*, *si*, *ut*. But as the measures in these experiments were very minute, and the operations of consequence liable to inaccuracy, I thought proper to try the thing by another test.

Obs. 13. The sun shining into the room, as before, I placed at the hole an hollow prism made of fine plate glass and filled with pure water, its refracting angle being 55 degrees. The spectrum was thrown on an horizontal chart eight feet from the window; and at four feet from the prism there was placed in the rays a rough black pin $\frac{1}{20}$th of an inch in diameter. The shadow in the spectrum was bounded by hyperbolic sides, as before described; and drawing a line, which might be the axis of the shadow, and pass precisely through its middle, I marked on one side six or eight points of the shadow's outline in each set of rays; and this being often repeated at different distances and in different shadows, the position of the axis remaining the same, the curves formed by joining the points were all parallel; which shows that each sine of inflexion taken apart has a given ratio to the sine of incidence. I afterwards divided the axis according to the musical intervals, and thus found where each colour of the spectrum had terminated, in what colour each part of the shadows had been,

* Page 588. † Page 560. ‡ So numbered in the original. N.

and

and by what rays formed. Then I joined the parts that I had marked, and obtained a curve, which I took to be, either nearly or accurately, an hyperbola of the fourth order. I next meafured the ordinates (the axis of the fpectrum and fhadow being the axis of the curve) at the confines of each colour; firft, the ordinate at the extremity of the rectilinear red, then that at the confine of the red and orange, and fo on to that at the extreme rectilinear violet. To each of thefe ordinates I added the greateft one, or that in the violet, which (in fig. 10) is VV′; that is, I produced vV to V′, fo that vV′ is equal to vV; and through V′ I drew V′R′ parallel to the axis VR, and produced gG to G′, and rR to R′; then from V′ I fet off V′g′ equal to G′g, and V′r′ equal to R′r, and the other ordinates in like manner; and I found, according to the method before defcribed*, that VV′ was divided inverfely, after the manner of the mufical intervals. It is therefore evident that the inflexibilities of the rays are directly as their deflexibilities and reflexibilities, but inverfely as their refrangibilities. The fame may be proved by meafuring and dividing the images made in the infide of the fhadows. Thefe I have found to be at equal incidences and diftances, equal to the images on the outfide, both in breadth, in diftance from the edge of the fhadow, and in the relation which their divifions bear to one another; wherefore whatever be the ratio of the angle of inflexion to that of incidence, the fame is the ratio of the angle of deflexion to that of incidence; fo that the angle of deflexion is equal to the angle of inflexion. If farther proof of this propofition be defired, the following experiment and obfervations, which from the importance of the thing I do not fcruple to add, may be fufficient.

Obf. 14. When two knife blades were placed by one another in a beam of light which entered the dark room, fo that the one might form and the other diftend the images, I made in one of the blades (with a file) a fmall dent, which, on the chart, caft an elliptic or femicircular outline; then I obferved that the images of both blades were difturbed by it, and wound round the edges of the femicircle; and they were all affected in precifely the fame manner and degree. So then the firft knife deflected the images formed by the fecond in precifely the fame degree that it inflected thofe images which itfelf formed, and fo of the other knife; otherwife the effect of the dent would have been different upon the two fets of images. We may therefore conclude that the angles, or fines of inflexion and deflexion, bear the fame ratio to the angle or fine of incidence, and that they are equal to one another. My next object was to determine this ratio in one of thefe cafes, and confequently in both; and it was very agreeable to find data for the folution of this problem in Newton's meafurement of the images and fhadow; fince this philofopher's well-known accuracy in fuch matters, befides the fingular ingenuity of the methods he employed, made me more fatisfied with thefe than any experiment I could make on the fubject. In fig. 11. CS is the line perpendicular to the chart SU, and paffing through the centre of the body, whofe half is CD or SE. EB is parallel to CS, and AI a ray incident at D. ADI or EDI is the angle of incidence, EDR that of the red's deflexion, EDV that of the violet's, and EDG that of the intermediates. According to Newton †, CD was [illegible]th of an inch, DE fix inches, SI $\frac{1}{108}$th of an inch, RV $\frac{1}{170}$th, and confequently RG $\frac{1}{340}$th; GS was [illegible]th; whence the angles IDE, EDV, EDG, and EDR, will be found to be 4′, 30″; 5′; 7′, and 9′, refpec-

* Page 560.

† Optics, Book III. Obf. 3.

9′, respectively. Now the natural sines of 4′, 30″; 5′; 7′ and 9′, are as the numbers 1309, 1454, $2035\frac{1}{2}$, and 2617, which are as the sines of incidence, deflexion, and inflexion, of the violet, green, and red. Thus the angles of flexion of the extreme and mean rays being given, those of the other rays are found by dividing the difference between 1454 and 2617 in the harmonical ratio; for then the red will be equal to $145\frac{3}{8}$, the orange $87\frac{2}{14}$, the yellow $155\frac{7}{15}$, the green $193\frac{5}{6}$, the blue $193\frac{5}{7}$, the indigo $129\frac{2}{5}$, and the violet $258\frac{1}{3}$; and by adding to the number 1454 the violet, and to their sum the indigo, and so on, we get the flexibility of the red from 2617 to $2471\frac{5}{8}$, of the orange from $2471\frac{5}{8}$ to $2384\frac{2}{3}$, of the yellow from $2384\frac{2}{3}$ to $2229\frac{1}{5}$, of the green from $2229\frac{1}{5}$ to $2035\frac{1}{2}$, of the blue from $2035\frac{1}{2}$ to $1841\frac{2}{3}$, of the indigo from $1841\frac{2}{3}$ to $1712\frac{4}{9}$, and of the violet from $1712\frac{4}{9}$ to 1454, the common sine of incidence being 1309. It is therefore evident that the flexibility of the red is not to that of the violet as the refrangibility of the violet to that of the red; and a little attention will convince us that we had no reason to expect the analogy should be kept up in this respect; for the refrangibility of the rays depends on the species of the refracting medium, and follows no general rule; whereas our calculation has been made concerning the action of the bending power at a certain distance, greater than that whereat the particles of media act on the rays in refracting them. It was observed in the mathematical propositions prefixed to this paper, that the angle of flexion is less than that of incidence, when in the case of inflexion the angle made by the ray and the body is acute, and when in the case of deflexion that angle is obtuse; and when the ray is perpendicular, or parallel, the angle of incidence vanishes in both cases. It is evident therefore, that in both these situations of things the ratio of 1309 to 2036 being that of a less to a greater, will not enable us to find the angle of flexion, although it serves very well when the ray before inflexion makes an obtuse, and before the deflexion an acute angle. I have therefore mentioned the angle made by the bent ray with the incident, which gives a general formula; for let the angle of incidence be I, and that which the bent ray makes with the incident B, then F being the angle of flexion, we have $F = B \pm I$; so that if $I = O$, $F = B$, or if the incident makes an obtuse angle with the body in the case of deflexion, and an acute in that of inflexion, then $F = I - B$, and in the remaining case $F = I + B$.

These observations enable us to give a very short summary of optical science. When particles of light pass at a certain distance from any body, a repulsive power drives them off; at a distance a little less this power becomes attractive: at a still less distance it again becomes repulsive; and at the least distance it becomes attractive, as before, always acting in the same direction. These things hold, whatever be the direction of the particles; but if, when produced, it passes through the body, then the nearest repulsive force drives the particles back, and the nearest attractive force either transmits them or turns them out of their course during transmission. Farther, the particles differ in their dispositions to be acted upon by this power in all these varieties of exertion; and those which are most strongly affected by its exertion in one case, are also most strongly affected by that exertion when varied, except in the cases of refraction, of which we before spoke; and these dispositions of the parts are in all the cases in the same harmonical ratio. Lastly, the cause of these different dispositions is the magnitude of the particles being various.

All that remains now to be done on this part of the subject, is to explain one or two phenomena relating to reflexibility.

 1. It

1. It has been remarked, that if we look at a candle, or other luminous body, with our eyes almoſt ſhut, bright ſtreaks ſeem to dart upwards and downwards from it. Newton* explains this by refraction through the humours adhering to the eyelids; Rohault † and Mr. Young ‡ aſcribe them to reflexions; Deſcartes makes them ariſe from wrinkles on the eye's ſurface; De la Hire from refraction through the moiſture on the eyelids, as through a concave lens; and Prieſtley ‖ from inflexion through the laſhes. The truth of Sir Iſaac's obſervation is obvious, becauſe the ſtreaks which dart from the top of the luminous body are formed by the under eyelid, or at leaſt by the moiſture adhering to the under ciliary proceſs, and thoſe which appear from the bottom of the body by the upper eyelid; which could not be, either if they were formed by reflexion from the proceſſes, or by inflexion through the laſhes.

I have however obſerved another kind of ſtreaks, mottled with broken colours of all kinds, and formed by reflexion from the moiſture on the proceſſes. In theſe the under ſtreak correſponds to the under proceſs, and *vice verſa.* They may be formed by any poliſhed body held in the proper poſition between the pupil and luminous body. The colours are very beautiful when made by the ſun, and reſemble, in form and irregularity of arrangement, ſome of the ſtreaks made by large half-poliſhed bodies, as deſcribed in Part II. of this paper.

2. The next object of attention is one of the greateſt importance to our theory, namely, the formation of images by reflexion. Three things here require explanation: the number of the images, their colours, and their variations in point of ſize.

Obſ. 15. I have uniformly found that no reflecting ſurface forms them except it be curve, and (its ſurface) of a ſtructure ſomewhat fibrous. A plain mirror, nor a concave, nor a convex one, do not make them; unleſs they are of that ſtructure; and for the ſame reaſon quickſilver, when held ſo as to reflect the light incident upon it, forms them not; but by triturating it, ſo as to divide it into ſmall particles, and by placing theſe in the beam of the ſun's light, each particle formed an image with the colours in the regular order, and very bright. On holding a cylinder in the rays, and obſerving the lengths of the images, I found that if the curvature was increaſed, the images were alſo increaſed in ſize, being more diſtended and highly coloured. Theſe things immediately ſuggeſt the explanation. Each of the ſmall fibres forms an image, which, from the different reflexibility of the rays, is divided into the ſeven primary colours. But why does not a plain mirror form one of theſe upon the ſame principles? In fig. 12. let A E be the curve ſurface of a very convex mirror, that is of a ſmall fibre, GC a ray reflected by the ſmall ſurface DC. It will be ſeparated into CI red, and CK violet, by the unequal action of FC on its parts. But if DC is continued to L in a ſtraight line, then LC's ſphere of reflexion extending a little way beyond it to KC, the part neareſt to C, and not to IC, will drive KC, and alſo the indigo and part of the blue, nearer to the perpendicular; then IC being within LC's ſphere of inflexion, will, together with the orange, yellow, and part of the green, be brought nearer to KC; ſo that IC and KC will both be brought to an angle equal to that of incidence, and will be reflected in a parallel white beam. If LC is removed a little, or the ſurface becomes more convex,

* Lect. Opt. Sect. III. ad finem.
† Phyſica, p. 249. Clark's edit.
‡ Phil. Tranſ. 1793.
‖ On Viſion, vol. ii.

IC is attracted, and KC repelled, but not ſo much as to reduce them to parallelifm and whitenefs; an image being formed narrower, and lefs coloured, than when LC is moved ſo far round that KC is attracted, and IC deflected or repelled. If LC is moved round, ſo that the mirror is concave, then KC is repelled and IC attracted, as before, unlefs the curvature be conſiderable; and then KC and IC are both repelled, and an image formed in the *cauſtic* by reflexion. In *Obſ.* 3. we found that certain irregularities in the ſurface of the reflector cauſed the images to be in the inverted order of colours. How does this happen? In fig. 13. let *f*, *fe*, *er*, *ri*, and *ih*, repreſent the ſections of the convex fibres on the ſurface of the reflector, and let the ray AB be reflected from *ef*, ſeparated into B*r* red and B*v* violet; then if AB was ſo inclined to *ef*, that B*r* and B*v* fell upon *er*, the ſide of the fibre next to *ef*, and a little larger than *ef*, it is evident that B*v* will be reflected into *v*V, and B*r* into *r*R, and an image VR will be formed, having the violet outermoſt, and the red innermoſt, the intermediate colours being in their order from V to R. Laſtly, it is evident that the greater the angle of incidence is, the longer will be the image, and the farther ſeparated its colours; for which reaſon the farther the images are from the ſhadow, the lefs dilated and coloured will they be. Nor will they have the ſame appearance at all diſtances from the point of incidence. Very near it they will be all in the form of fringes acroſs the ſtreak, the breadth being greater than the length (if I may uſe the expreſſion); but as we recede from it they will become diſtended, as before deſcribed, the length increaſing faſter than the breadth, and at one point or diſtance they will be juſt as long as broad; all which agrees with experiment. And it is needlefs to ſhow by particular demonſtration, the manner in which one image is divided from another, the reaſon obviouſly being the manner in which the fibres on the reflecting ſurface are arranged and inclined to one another.

3. A number of phenomena involved in that of the images are explicable by what has been ſaid on them. If a piece of metal be ſcratched, and then expoſed in the ſunſhine, a number of broken colours will be formed by the ſcratches, as may be ſeen either by letting them fall on the eye, or by receiving them on a white object. This is evidently owing to the different reflexibility of the rays incident on the ſcratches, which are ſo many irregular ſpecula, of great curvature; the images are therefore diſtorted and broken, juſt as a candle, &c. appears broken and coloured when viewed through a piece of irregular cryſtal, ſuch as the bottom of a wine glaſs. If we look attentively at any object expoſed in the light of the ſun, provided it be not poliſhed, we ſhall ſee its ſurface mottled with various points of colours from the ſpecular nature of its minute particles. If we look towards the ſun with a hat on our head, held down, ſo that the ſun's direct light may not fall on our eyes, but on the hairs of the hat, and be reflected, we ſhall ſee a variety of lively colours darting in all directions from thoſe hairs; and we may eaſily ſatisfy ourſelves that they are not the conſequence of flexion, by trying the ſame thing with unpoliſhed threads; in which caſe they do not appear, provided the threads be not very ſmall. In the ſame manner we may account for the colours of ſpider's webs, of different cloths which change their colours when their poſition is altered, and of ſome foſſils which appear of different ſtreaks of colours when held in the light; ſuch as the fire marble of Saxony, &c. All theſe bodies having ſurfaces of a fibrous ſtructure, each fibre reflects and decompounds the rays.

4. The conſideration of the foregoing phenomena inclined me to think that, upon the

principles which have been laid down, the colours of natural bodies may be explained. The celebrated discovery of Newton, that these depend on the thickness of their parts, is degraded by a comparison with his hypothesis of the fits of rays and waves of ether. Delighted and astonished by the former, we gladly turn from the latter; and unwilling to involve in the smoke of unintelligible theory so fair a fabric, founded on strict induction, we wish to find some continuation of experiments and observations, which may relieve us from the necessity of the supposition. My speculations on this subject have by no means been completed, as I have not yet finished the demonstrations and experiments into which it has engaged me to enter; but in order to complete my plan I shall offer a few hints on the subject: -The parts of light are affirmed in Prop. III. Book I. Part I. of the Optics to be different in reflexibility; that is, according to the author's definition, in disposition to be turned back, and not transmitted at the confines of two transparent media. That the demonstration involves a logical error, appears pretty evident. When the rays, by refraction through the base of the prism used in the experiment, are separated into their parts, these become divergent, the violet and red emerging at very different angles, and these were also incident on the base, at different angles from the refraction of the side at which they entered: when therefore the prism is moved round on its axis, as described in the proposition, the base is nearest the violet, from the position of the rays by refraction, and meets it first; so that the violet being reflected as soon as it meets the base, it is reflected before any of the other rays, not from a different disposition to be so, but merely from its different refrangibility. Although then this experiment is a complete proof of the different refrangibility of the rays, it proves nothing else; and indeed an experiment will convince us that the rays all have the same disposition to be reflected, provided the angle of incidence be the same. For I held a prism vertically, and let the spectrum of another prism be reflected by the base of the former, so that the rays had all the same angle of incidence; then turning round the vertical prism on its axis when one sort of rays was transmitted or reflected, all were transmitted or reflected. We cannot therefore apply the different reflexibility of light to the explanation of the colours of bodies, since this property has no existence. But we have shewn that the rays differ in reflexibility, taking the word in the new sense, as explained above. Let us see whether this principle will not solve the important problem. It is evident that the particles of bodies are specular. Now, I take the colours of bodies to depend not on the size, but on the position, of these particles, or at least on only the size in as far as it influences their position; an idea perfectly familiar to mathematicians.

Obs. 16. In making some of the experiments which I related above, on the reflexibility of light, I observed, among the regular images made by most of the pins which I used, one or two all of the same colour, as red, blue, &c.; and when the pin was moved these moved also, becoming of other colours in regular order like the rest; which shows plainly that their being of one colour at first was owing to some fibre in the surface jutting out, or rather to several of these, which stopped the red and all the rest but the blue of several images, or the blue and all the rest but the red. Farther, I produced several regular images by two or three very small pins, and with considerable trouble. I at last contrived to place them in such a position, as that one blue colour of considerable size might be produced, then a red, and so on, by altering the posture of the pins. Now whether the posture or the size be altered it matters not, for the one affects the other. Is it not evident that this experiment,

 and

and the conclufion to which it evidently leads, may be transferred to the colours of natural bodies, as feen by reflexion? For, the parts being fpecular and fpherical, each will form an image of the luminous body; and by the pofition of the fides of the neighbouring ones, any fix of the colours may be ftopped while the feventh emerges; and if this happens in one part it will happen in all, fince, that the texture and fize of the parts is the fame throughout has never been called in queftion. But it will be afked, How are the particles to reflect a mixture of different colours? We anfwer, That a particle having its fide concave and front convex will produce the effect; for the colours will be thus mixed in a proportion determined by the pofition of the others. How can whitenefs and blacknefs be produced? If the particles be large, then the whole light incident on each will be reflected and feparated; and all the images being compounded and mixed together, a confufed fenfation, or a fenfation of white, will be the refult. For, the parts being tranfparent, and the images formed by the convex furface of the fecond row of particles, thefe will be larger in proportion to the thicknefs of the particles or plates through which they have to pafs before they meet with obftruction, and confequently will not be ftopped by other particles; and in like manner the colour will be red if the particles are a little lefs, and fo on. If the particles be very fmall, the light will be feparated into images alfo fmall; with which and with one another the particles interfering, the light by many reflexions and obftructions will be totally loft. How do bodies appear of their proper colours, though no luminous body be fhining whofe image may be formed by a reflexion? They reflect images of the clouds which reflect the fun's white light; for if we hold between our eye and a hole in the window illuminated by the light of the clouds a reflecting body, as a pin, &c. coloured images are formed of the hole diftended, like thofe of the fun, as I have often found; and the fame holds of inflexion. Why does cutting a body to pieces not alter its colours? This only changes the pofition of maffes of particles, not of the particles themfelves; but if by bruifing them we change their magnitude and pofition, we change alfo their colonr: thus the leaves of vegetables bruifed in a mortar, many paints powdered, &c. Why do many bodies change colours when viewed in different pofitions? Becaufe they reflect two colours or more of each image to different quarters; and it matters not whether their pofition with refpect to us, or our pofition with refpect to them, be changed. How do bodies become coloured by tranfmitted light? Becaufe the foregoing reafonings apply alfo to the flexion of the rays in their paffage through the parts of bodies. Thefe obfervations appear to me to furnifh a very fimple folution of the problem. I fhall endeavour in a future communication to confirm what has been faid by other remarks and experiments; for it would be tedious, and perhaps fuperfluous, to illuftrate what has been faid by figures and demonftrarions *.

Purfuant to thefe remarks, it will not be difficult to account for the rings of colours of thin plates by reflexion, as we before did thofe of thick plates by flexion †. Indeed, thofe formed in the experiment of the two lenfes, fuppofed by Newton to be owing to the plates

* It is obvious that the different refrangibility of the rays will not account for the bright and diftinct colour of bodies. If the refracting angle of a prifm be continually diminifhed, till, for example, it is equal to one of a minute, the refraction will produce no fenfible colour. Indeed, almoft every piece of plane glafs has its fides in a fmall degree inclined to one another, and yet no colours are formed. Much lefs then will refraction through the infinitely fmaller parts of bodies produce feparation of the rays.

† Page 589.

of

of air between them, appear to have a different caufe, as may be without much reafoning gathered from the curious experiments of the Abbé Mazeas *, and even from one or two of Sir Ifaac's own, in which he fuppofes fome medium more fubtle than air to be between the glaffes †. But at prefent I forbear entering into the fubject any farther. This paper has already been extended to a greater length than was at firft intended. And I haften to conclude by a fhort fummary of propofitions, containing the principal things which have been demonftrated in the courfe of it.

Prop. I. The angles of inflexion and deflexion are equal at equal incidences.

Prop. II. The fine of inflexion is to that of incidence in a given ratio (which is determined in the paper).

Prop. III. The fun's light confifts of parts which differ in degree of inflexibility and deflexibility, thofe which are moft refrangible being leaft flexible.

Prop. IV. The flexibilities of the rays are inverfely as their refrangibilities; and the fpectrum by flexion is divided by the harmonical ratio, like the fpectrum by refraction.

Prop. V. The angle of reflexion is not equal to that of incidence, except in particular (though common) combinations of circumftances, and in the mean rays of the fpectrum.

Prop. VI. The rays which are moft refrangible are leaft reflexible, or make the leaft angle of reflexion.

Prop. VII. The reflexibilities of the different rays are inverfely as their refrangibilities, and the fpectrum by reflexion is divided in the harmonical ratio, like that by refraction.

Prop. VIII. The fines of reflexion of the different rays are in given ratios to thofe of incidence (which are determined in the paper).

Prop. IX. The ratio of the fizes of the different parts of light are found.

Prop. X. The colours of natural bodies are found to depend on the different reflexibilities of the rays, and fometimes on their flexibilities.

Prop. XI. The rays of light are reflected, refracted, inflected, and deflected, by one and the fame power varioufly exerted in different circumftances.

III.

On certain Points of Nomenclature. By a Correfpondent.

To Mr. Nicholson.

SIR,

IT appears to me, that the new name *tartarin*, propofed to defignate the fubftance commonly called vegetable alkali, is more objectionable than fome of the number of names which are in ufe.

1. Becaufe the word *tartar* is familiarly known to always denote a falt of acid properties, which confifts of nearly three parts of tartareous acid, and one of vegetable alkali.

2. The word *tartareous acid*, which is already employed to denote the acid contained in tartar (and for which fubftance there is no other name), fo nearly refembles the word *tartarin* as to make this name an improper one for the alkali of tartar and of vegetables in general.

* Mem. de l'Academie pour l'Année 1738. † Optics, Book ii. Part I. Obf. x. and xi.

3. But

3. But a very fmall proportion of alkali is obtained from tartar comparatively with that got from other fources; and it feems at leaft as improper to call it *tartarin* becaufe it is gotten from tartar, as it would be to call it fern alkali, wormwood alkali, beech alkali, &c.

I agree that the name *pot-afh* is very objectionable, and I wifh that another could be thought of that is appropriate, efpecially becaufe this word is commonly underftood in commerce and in the arts to denote the vegetable alkali in its ordinary mild but impure ftate, as ufed in manufactories; in which ftate the alkali is only partially faturated with carbonic acid, and which therefore fhould be called, according to the new nomenclature, *fub-carbonate of pot-afh*; whereas, in the new fyftem, the name pot-afh is intended to fignify the alkali in its pure or cauftic ftate.

Confidering, 1ft. That the alkali called pot-afh is very generally diffufed through the whole vegetable kingdom, which neither of the other fpecies of alkali are—2. That the other fixed alkali, commonly called foffil alkali, or foda, exifts principally in the mineral kingdom—3. That the volatile alkali or ammoniac is obtained almoft entirely from the animal kingdom—4. That the denomination *vegetable alkali* is ftill very generally employed, and has been fo for a very long time—5. That the three alkaline falts refemble one another in fo many properties as to form a natural genus which may continue to be denominated *alkali*:—I fay, from thefe confiderations it feems beft to retain the former fpecific terms vegetable, foffil, and volatile alkàli; but abbreviated, as propofed fome years ago by Mr. Chriftie, and accordingly to employ the names *vegalkali*, *fofkali*, and *volkali*, till more appofite names be fuggefted.

Give me leave to remark, that Englifh writers are not uniform in their orthography of feveral terms of the new fyftem, and I apprehend fome of them write improperly; for inftance:

Ox*i*gen and oxygen*e* for *oxygen*.
H*i*drogen and hidrogen*e* for *hydrogen*.
Ox*i*d and ox*y*de for *oxide*.
Sulph*at* and fulfate for *fulphate*.
Sulph*it* and fulfite for *fulphite*.
Muri*at*, &c. &c. &c. for *muriate*.
Calor*ique* for *caloric*.

We hear alfo fometimes erroneous profody, for inftance *călŏric* for *cālōric*.

The French writers uniformly write gaz, and the Englifh generally ga*s*. In the Englifh, either mode may be employed; but the French probably prefer their orthography *gaz* on account of the word *gas* founding too much like another word in their language which excites an indelicate idea.

I am, Sir, your fincere friend,

A VERY HUMBLE PHILOLOGER.

Feb. 17, 1798.

NEW

NEW PUBLICATIONS.

Effai fur les Ouvrages Phyfico-mathématiques de Léonard de Vinci, avec de Fragmens tirés de fes Manufcrits apportés de l'Italie. Lu à la première Claffe de l'Inftitut National des Sciences et Arts. Par J. B. Venturi, Profeffeur de Phyfique à Modène, de l'Inftitut de Bologne, &c. A Paris, chez Duprat, 1797.—Or, An Effay on the Phyfico-mathematical Works of Leonardo da Vinci, with Extracts from his Manufcripts brought from Italy. Read before the Firft Clafs of the National Inftitute of Sciences and Arts. By J. B. Venturi, Profeffor of Natural Philofophy at Modena, Member of the Inftitute at Bologna, &c. Quarto, 56 pages, with one plate.

AMONG the treafures of fcience and art which the French have lately brought from Italy, are thirteen manufcript volumes written by Leonardo da Vinci, whofe extraordinary powers as a painter, fculptor, mufician, geometer, philofopher, and engineer, are well known. Citizen Venturi, who refided in France during the war in his own country, has obtained the communication of thefe manufcripts, and has felected fuch parts as appear deferving of publication, which he purpofes to print in three feparate and complete treatifes, on mechanics, hydraulics, and optics. The fragments in the prefent work are for the moft part diftinct and feparate from thofe principal matters, and are enriched with notes by the editor.

Leonardo was born in 1452. He was the natural fon of a notary, whofe family poffeffes at this day at Vinci in Tufcany the fituation of refpectable mediocrity. Nature, which, as C. Venturi obferves, is not actuated by confiderations of birth, was prodigal of her gifts to this man. His perfon was beautiful; his difpofition animated and lively, and the powers of his mind wonderful. He applied to geometry, mufic, and painting, and in each of thefe purfuits he foon excelled his teachers. He was called to Milan to caft an equeftrian ftatue in bronze, which Louis Sforza confecrated to the memory of his father. He offered his fervices to the Duke in every undertaking relating to military machines, water-works, fculpture, mechanics, and painting, with a defiance to any one who fhould pretend to excel him; a boaft which indeed he had the means of fupporting. When France took poffeffion of the Milanefe at the end of the 15th century, he paffed feveral years at Florence; not becaufe he was in any refpect prejudiced againft the French, as is reported; on the contrary, Louis the XIIth gave him a penfion, together with certain duties on the Milanefe canals, where Leonardo was employed under the French government. While he was at Florence he felected two of the moft beautiful ladies of the country, with the intention of drawing their portraits, and prefented them to Louis the XIIth. He left Milan, and repaired to Rome in 1513, after Sforza had again entered the Milanefe; after which he went to France on the invitation of Francis I. where he died, as it is faid, in the arms of that prince.

I fhall forbear at prefent to enter into any detail refpecting the contents of this interefting work, in which this great genius, who preceded Chancellor Bacon in the true method of philofophifing near a century, has the peculiar felicity of an editor and commentator, whofe valuable annotations befpeak him a complete mafter of the fubjects of fcience and refearch he has undertaken to publifh and elucidate. On a future occafion I fhall prefent my readers with fome extracts of the moft curious parts.

Phyfiology;

Physiology; or, An Attempt to explain the Functions and Laws of the Nervous System; the Contraction of Muscular Fibres, and the constant and involuntary Actions of the Heart, the Stomach and Organs of Respiration, by Means of simple, universal, and unvarying Principles. To which are added, Observations on the intellectual Operations of the Brain; and on the Diversity of Sensations; with Remarks on the Effects of Poisons, and an Explanation of the Experiments of Galvani and others on Animal Electricity. By E. Peart, M. D. &c. 8vo. 327 pages. W. Miller, 1798.

I have just received this work; for which reason I can at present only announce its publication, and say that the author's explanation of the phenomena mentioned in the title appears to be founded upon the system which he offered to the world in his Elementary Principles of Nature published in 1789.

Travels in the Two Sicilies, and some Parts of the Apennines. Translated from the original Italian of the Abbé Lazzaro Spallanzani, Professor Royal of Natural History in the University of Pavia, F. R. S. &c. &c. In Four Volumes 8vo. 1500 pages, with 11 Plates. Robinsons, 1798.

This celebrated philosopher had the good fortune to obtain a clear and distinct view of three out of the four craters which are still burning in the volcanized countries through which he travelled, namely, Etna, Stromboli, and Vulcano, of which he has given designs; but at Vesuvius his wish could not be gratified. The Lipari Islands have been the subject of his assiduous examination, as well with regard to their igneous productions, as the state, character, and occupations of the inhabitants; and the environs of the unfortunate city of Messina afforded much information, from the variety of natural objects they presented. Scylla and Charybdis; the fisheries in the Straits of Messina for the sword-fish, the ravenous shark, and for coral, with various other interesting objects, also engaged his attention during his excursion in the year 1788-89.

To say that these volumes contain a treasure of geological and chemical information, is scarcely necessary to those who are acquainted with the former labours of the Professor. I must therefore more particularly observe, that he has traversed these interesting districts, contemplated and compared the volcanic products with a particular regard to local circumstances, and the inferences to which those circumstances lead; examined the several substances with every precaution which the difficulties of fixing the external characters and obvious properties could suggest; submitted them to the action of fire before the blow-pipe, and in the furnace, with the pyrometer of Wedgwood and pneumatic apparatus; and lastly, that in such specimens as could not from their external characters be referred to known earths already analysed by chemists of reputation, he has had recourse to the methods of humid analysis. Where his experimental enquiries have proved concise, he has frequently incorporated them with the account of his journeys; but where otherwise, he has placed them with such subjects of general discussion as could not form a part of his narrative in the order of time.

From this rapid sketch of his method of proceeding, the reader, who may be acquainted with the advantages which the modern improvements of Chemistry must afford in the hands of an enlightened observer, will be able to form some estimate of the value of the work.

END OF THE FIRST VOLUME.

Dutau-16e

July. 1881

Fig. 1.

The Phenomena of Water Spouts.

Fig. 2.

Fig. 3.

The Phenomena of Water Spouts.

Fig. 4.